DAVID NEUMARK AND
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MINIMUM WAGES

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For Noey and Eitan—
Did you ask a good question today? (DN)
For Marilyn (WW)

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Chapter 3 is an abbreviated and updated version of our much lengthier review:

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The views expressed in this book are our own, and do not necessarily reflect the views of the Federal Reserve Board, the Public Policy Institute of California, or other organizations with which we are affiliated or that have provided support.

1 Introduction

1.1 The Minimum Wage Controversy

The minimum wage has been a core element of public policy for more than a century. Originating in the 1890s in New Zealand and Australia, minimum wages spread to the United Kingdom in 1909 and to nearly one-third of U.S. states during the next two decades. In 1938, the U.S. Congress passed a federal minimum wage law as part of the Fair Labor Standards Act. Since that time, minimum wages have been introduced in some form or another in numerous other industrialized countries, as well as in some developing countries. As a result, by the 1990s, minimum wages existed in well over one hundred countries from all parts of the world, and the International Labour Organization (ILO) has designated the minimum wage as an international labor standard (International Labour Organization 2006).

Minimum wages were originally proposed as a means to combat the proliferation of so-called sweatshops in manufacturing industries. These sweatshops employed considerable numbers of women and youths and paid them what were generally regarded as substandard wages. In particular, employers were viewed as having disproportionate bargaining power over such workers, and the minimum wage was intended to ensure that they received a "fair" wage for their work. Over time, however, proponents of minimum wages increasingly advocated them as a way to help individuals or families achieve self-sufficiency, and, as a result, coverage of minimum wage laws was extended to men and to workers in most low-paid occupations.

The goals associated with the minimum wage are widely accepted as right and proper. However, there is much less agreement about whether the minimum wage is effective at attaining these goals. Although overwhelmingly popular with the public in the United States, the minimum wage has, from the time of its introduction, been highly controversial in the political arena. In addition, minimum wages have typically received less support from economists, who from the very beginning of the minimum wage debate pointed to the potential loss of jobs stemming from a wage floor. Despite decades of economic research, policy debates about the costs and benefits of minimum wages continue to the present day.

1.2 Our Research on Minimum Wages

Research on the economic effects of the minimum wage had largely come to a halt in the 1980s, following the publication of the extensive report of the Minimum Wage Study Commission (1981). This landmark report included studies by a virtual "Who's Who?" of labor economists working in the United States at that time. During that decade, when we were completing our graduate education, the common advice we received was to find some other topic to research, as it seemed that virtually everything there was to be known about minimum wages was covered in the commission's report or in other research published prior to the report.

However, in our jobs at the Federal Reserve Board in the late 1980s, where we followed labor market developments particularly closely, we were struck by the increasing number of state legislatures that were setting minimum wages above the federal level.[1] In particular, we saw this state-level variation in minimum wages as an opportunity to improve upon the earlier evidence on how minimum wages affected employment. That earlier evidence was largely based on aggregate time-series data, which posed difficulties for reliably estimating the employment effects of minimum wages because of the limited variation in the federal minimum wage and the absence of valid counterfactual information on what would have happened to the economy in the absence of higher minimum wages. Taking advantage of this opportunity, in our first study of the employment effects of the minimum wage, we constructed a cross-section time-series panel of state minimum wages and labor market conditions, which effectively used states without a minimum wage increase as a comparison group for states that raised their minimum wage.

Not surprisingly, perhaps, we were not the only economists who had this idea, and an early set of papers published in a symposium in the *Industrial and Labor Relations Review* (*ILRR*) kick-started a new wave of research into the employment effects of minimum wages. This wave of research, which is often referred to as the "new minimum wage research," has continued unabated, with well over one hundred papers on this topic published since then, both for the United States—where state minimum wage variation has remained a prominent feature of the policy landscape—and for other industrialized and developing countries.

In the mid-1990s, we began to branch out from our original focus on the employment effects of minimum wages to consider their other consequences. This broadening of our research was prompted by our view that evidence on the employment effects of minimum wages is, taken alone, insufficient to inform the policy debate over the effects of minimum wages. In particular, even if minimum wages result in lower employment for low-skilled groups, there may be other benefits of minimum wages that trade off favorably against the disemployment effects, as well as other costs.

This inquiry led us in two directions. First, given the evidence that minimum wages play an important role in youth labor markets, it seemed natural to ask what other consequences they might have for youths. For example, if minimum wages reduce employment, do they also encourage young people to stay in school because of the decrease in job opportunities or to acquire more skills to raise their productivity to a level at which they would not be priced out of the labor market by a minimum wage? Conversely, does a higher minimum wage induce some teenagers or young adults to leave school in the hope of landing a job paying a higher wage *because of* the higher minimum? Similarly, do minimum wages deter firms from providing training to young workers, hence curtailing an important source of future wage growth? And finally, what are the longer-run effects of the minimum wage on the acquisition of skills? These questions had received some attention in earlier research, but as in the case of employment effects, the emergence of the greater variation in minimum wages across states in the late 1980s provided an opportunity to obtain new—and arguably superior—evidence on the effects of minimum wages.

The second direction our research took is one that we regard as especially important from a policy perspective. In particular, we see the principal intent of the minimum wage as helping to raise incomes of low-income families. This implies that the principal criterion for decisions about raising the minimum wage is whether doing so has beneficial effects for the distribution of family incomes—reducing poverty or increasing incomes at the bottom of the distribution. Again, this question had been considered in research predating the new minimum wage research, but the available evidence was limited and ambiguous.

1.3 Plan of the Book

This book describes the findings of nearly two decades of research on minimum wages. It synthesizes and presents the evidence we have accumulated across nearly thirty research papers and provides a comprehensive discussion of other research that touches on the same questions we have considered, as well as on other topics central to the debate over minimum wages. In much of the book, we emphasize evidence for the United States. However, where possible, we also discuss research for other industrialized and developing countries.

We begin in chapter 2 with a brief history of the minimum wage. Although we touch briefly on its genesis in Australia and New Zealand in the late 1800s, our discussion focuses mostly on the development and evolution of minimum wage policies in the United States, and on the legal and political challenges that accompanied the efforts to enact these policies. In addition, this chapter summarizes the early debate and research among economists regarding the effects of minimum wages, a debate that to a remarkable degree foreshadowed the issues and questions raised in contemporary research.

Chapter 3 focuses on the issue that has dominated research and debate on the minimum wage—its effects on employment. After a brief discussion of the theory of the minimum wage and employment, we summarize the extensive body of contemporary research launched by the 1992 *ILRR* symposium. This literature is so extensive that we limit our discussion to our own research and to what we view as the most important or compelling evidence from the larger body of research.[2]

In chapter 4, we turn to the distributional effects of minimum wages, focusing first on the relationship between minimum wages and the distribution of individual wages. The substantial increase in wage inequality in the United States in recent decades has brought this issue to the forefront, with researchers asking whether minimum wage policies—in particular, the general stagnation of the federal minimum wage—have contributed to the rise in inequality. In addition, research on the effects of minimum wages on the wage distribution contributes to our understanding of how minimum wages affect the labor market, and is a precursor to considering the effects of minimum wages on the distribution of family incomes.

Chapter 5 examines the effects of the minimum wage on the distribution of family incomes. We first consider the relationship between low-wage workers and low-income families. Although it has long been recognized that the two are not synonymous, we discuss updated evidence on the extent to which minimum-wage workers are concentrated in low-income families. We then turn to evidence that parallels much of the minimum wage employment literature in terms of empirical methods, but focuses instead on how minimum wages affect family incomes. This analysis considers—although is not limited to—the question of whether higher minimum wages reduce poverty.

The possible effects of minimum wages on schooling and training, and other potential effects of minimum wages on the acquisition of skills, are the topic of chapter 6. We begin by studying the direct effects of minimum wages on schooling, focusing both on their overall effects and on differences in the effects of minimum wages across skill groups. For example, we ask whether minimum wage increases disproportionately reduce employment opportunities for teenagers who have already dropped out of school, and whether higher-skill workers who may be enticed to leave school by a higher minimum wage displace high school dropouts from the labor market. We then turn to the effects of minimum wages on training, a topic that is relevant to individuals whose employment status is not influenced by the minimum wage, but whose terms of employment may be affected. Because the effects of minimum wages on both training and schooling (as well as foregone labor market experience stemming from reduced employment opportunities) may have longer-lasting consequences, we also present evidence on the longer-run effects of minimum wages on workers' wages and earnings even after their wages have risen above the minimum wage.

Our work—and indeed of most of the research on the minimum wage—emphasizes the effects of minimum wages on individual workers and families. But the broader economic effects of minimum wages on profits, prices, and inflation have also been the subject of some research, and chapter 7 discusses this evidence.

In chapter 8, we turn from studying questions about *how* minimum wages affect workers and the economy to questions about the political economy of minimum wages. That is, rather than viewing minimum wages as mandated wage floors that fall from the sky, we examine the factors that influence the decisions of politicians (and others) to support or oppose higher minimum wages. As part of this inquiry, we also consider why minimum wages remain so popular even though many economists oppose them (although they certainly do not oppose them unanimously). Finally, we also discuss the newest manifestation of mandated wage floors—"living wages," which are enacted at the city level and cover a narrower set of workers. After a brief overview of evidence on the effects of living wages, we consider similar political economy questions, asking why living wage laws have arisen and why they take on particular forms.

Finally, in chapter 9, we provide a summary of our evidence, offer some concluding thoughts, and discuss implications for public policy.

1.4 Our Approach and Key Conclusions

As we emphasize throughout this book, we believe that questions about the economic effects of the minimum wage need to be resolved empirically. Economic theory is valuable in suggesting testable hypotheses about the potential influences of minimum wages, and in that sense, it helps guide the empirical analysis. But economic theory rarely makes firm predictions about the effects of minimum wages on the outcomes we consider, and in many cases makes no prediction at all. And, even when the theoretical predictions are unambiguous, they rarely tell us about the magnitude of the effect. As a result, in this book we emphasize what the data have to say about various effects of minimum wages, and the corresponding implications both for public policy and for our understanding of how labor markets work.

Based on the extensive research we have done, and our reading of the research done by others, we arrive at the following four main conclusions regarding the outcomes that are central to policy debate about minimum wages. First, minimum wages reduce employment opportunities for less-skilled workers, especially those who are most directly affected by the minimum wage. Second, although minimum wages compress the wage distribution, because of employment and hours declines among those whose wages are most affected by minimum wage increases, a higher minimum wage tends to reduce rather than to increase the earnings of the lowest-skilled individuals. Third, minimum wages do not, on net, reduce poverty or otherwise help low-income families, but primarily redistribute income among low-income families and may increase poverty. Fourth, minimum wages appear to have adverse longer-run effects on wages and earnings, in part because they hinder the acquisition of human capital. The latter two sets of conclusions, relating to the effects of minimum wages on the income distribution and on skills, come largely from U.S. evidence; correspondingly, our conclusions apply most strongly to the evaluation of minimum wage policies in the United States.

We state these conclusions bluntly here because we believe they are justified based on the evidence. Nonetheless, as will become clear during the reading of this book, research on the many facets of minimum wages is characterized by continuing disagreement and controversy. As a result, we are under no illusion that all readers of this book will agree fully with our conclusions. However, regardless of whether the reader is convinced by our analysis, we hope that our book will be viewed as providing a thorough and dispassionate study of what has become a remarkably extensive body of research.

2 The History of the Minimum Wage in the United States

2.1 Introduction

The minimum wage has a lengthy and extensive history in the United States, in terms of both its implementation and its interaction with economic research. In this chapter, we review the political, legal, and economic developments over the last century that resulted in today's minimum wage laws. We also discuss how previous generations of economists approached questions about the economic effects of minimum wages.

The purpose of this chapter is twofold. The first is to present a brief history of the minimum wage over the past century. In this regard, we trace the origins of the minimum wage back to its roots in the late nineteenth century, and describe how it evolved in the United States from a weak set of state-specific laws targeted at women in the early 1900s to the combination of federal, state, and local minimum wage laws with broad coverage that we have today. In particular, we highlight the political and legal challenges faced by early supporters of minimum wages, the events leading up to the eventual introduction of a national minimum wage as part of the Fair Labor Standards Act (FLSA) of 1938, and the subsequent political debates about further increasing the federal minimum and expanding its coverage. In addition, we examine the political and economic conditions that led to the proliferation of state and local minimum wages during the past two decades.

Second, we track the ongoing discourse among economists about the merits of a minimum wage law. As will become evident upon reading the rest of this book, many of the key issues that are debated by economists today have a long history in the economics literature. For example, the potential for the minimum wage to cause job losses was a central theme discussed by economists both in the early 1900s and during the period surrounding the passage of the FLSA, and, we argue, was as overemphasized in those earlier debates as it is today. Similarly—although typically presented less formally—in the earlier literature one can see strands of the various theoretical arguments regarding the implications of minimum wages for labor markets that we present in chapter 3. These include not only the competitive model advocated by John Bates Clark and other classical economists, but also efficiency-wage models, monopsony models, and other non-neoclassical formulations of labor market behavior.

Many of the empirical approaches used currently to analyze the minimum wage, and the concerns that attend these approaches, also are echoed in the early research literature on the effects of minimum wages. Perhaps most obviously, the case study approach that constitutes a key branch of the new minimum wage research was used as early as 1915 by economists at the Bureau of Labor Statistics to study the effects of the Oregon minimum wage (Obenauer and von der Nienburg 1915); similarly, a number of difference-in-differences estimates of minimum wage effects for particular industries were published in the early 1940s, following the introduction of the federal minimum wage in 1938. In addition, the subsequent concerns expressed about the quality of the empirical studies—such as the reliability of the surveys used to collect data, the adequacy of the control groups used in the experiments, possible lagged effects of minimum wages, and other issues—parallel many of those we raise in subsequent chapters.

As a result, we think that it is useful to set the stage for the remainder of this book by including this brief historical summary of the politics and economics of the minimum wage. Our hope is that in subsequent chapters, readers will have an historical context within which to view the more recent research.

2.2 The Origins of the Modern Minimum Wage

Although the majority of research on the effects of the minimum wage has focused on the United States, the first minimum wage laws were enacted elsewhere—notably, in New Zealand in 1894 and in Australia in 1896. In New Zealand, the minimum wage was a by-product of the Industrial Conciliation and Arbitration Act, which established District Conciliation Boards to arbitrate industrial labor disputes. Although the main role of the boards was to facilitate settlements between employer associations and worker unions, the boards were also given the authority to fix conditions of employment, including minimum wage levels, if necessary, and to extend these conditions to other employers in the same industry who were not members of the associations.[1] Subsequently, in 1899 New Zealand set a nationwide minimum wage, which was primarily intended to prevent employers from hiring children, or apprentices at no pay.

In contrast, minimum wages in Australia were determined at the state level and took two basic forms. In 1896, the state of Victoria established a set of "wages boards," each of which consisted of equal numbers of employee and employer representatives from a particular trade, and was chaired by an impartial third party. These boards would meet to determine the appropriate minimum wage rate for that trade, and the agreed-upon minimum wage would then become binding for all employers in that trade in the state. This model was subsequently followed by South Australia (1900), Queensland (1908), and Tasmania (1910). Other states, such as New South Wales (1901) and Western Australia (1902), copied the original New Zealand model, in which minimum wages could be set as part of compulsory arbitration between employer associations and unions. Although the initially proposed legislation in Victoria covered only women and children, it was amended before its passage to include adult men. Similarly, in all of the other Australian states, both sexes were from the beginning covered by the minimum wage laws.

England adopted the Victorian wages boards model in 1909, but initially applied it to only four industries for which "sweating"—defined by the British Board of Trade as conditions of employment characterized by unusually low wage rates, excessive hours of labor, and/or unsanitary workplaces—was believed to be a particularly severe problem: chain making, lace finishing, paper and cardboard box making, and tailoring.[2] In 1913, trade boards (as wages boards were referred to in England) were established for four additional industries (sugar confectionary and food preserving, shirt-making, hollow ware, and linen and cotton embroidery); attempts to include a board for power laundries were rejected by Parliament. As in most parts of Australia, the trade boards consisted of equal numbers of representatives from employers and workers, as well as appointees not associated with either group. Similarly, minimum wages in England covered both men and women, and varied by industry, sex, age, and experience.

There were also widespread concerns in the early 1900s about sweatshops in the United States and, more broadly, about the working conditions of women and children. As a result, interest in the minimum wage quickly spread to this side of the Atlantic. Prasch (1999), Waltman (2000), Leonard (2005), and others trace the advent of minimum wage policy in the United States to the Progressive movement of the early twentieth century, and, more specifically, to efforts by the American Association for Labor Legislation and the women-led National Consumers' League, whose board of directors explicitly endorsed a legal minimum wage for women in 1909.[3] Although Progressive-era reformers generally were supportive of the free market system, they also believed that the government should step in when the market's functioning could be improved by intervention. In the case of the minimum wage, the Progressive view was that an advantage in bargaining power held by employers over low-wage workers—especially women and children—enabled firms to set wage rates below what was warranted by the productivity of these employees. Moreover, many Progressives felt that individuals were entitled to a "living wage" that ensured a decent standard of living, and some even believed that imposing a minimum wage would raise the productivity of labor and increase the efficiency of firms (Prasch 1999).[4]

Despite the efforts of the Progressive movement, there seemed to be little interest among politicians in implementing such reforms at the federal level, in part because of a view among legislators that minimum wages would violate the freedom of contract provisions of the Fourteenth Amendment.[5] Instead, the states took the first steps toward introducing the minimum wage to the United States, beginning with legislation enacted by Massachusetts in 1912. Other states soon followed suit, and by 1923 fifteen states, the District of Columbia, and Puerto Rico had minimum wage laws on their books (table 2.1).

In many respects, these U.S. state minimum wage laws followed the broad outlines of those enacted earlier in Australia and England. However, state lawmakers also attempted to craft laws that were less likely to run afoul of the U.S. legal system. In most cases, for example, the minimum wage laws applied only to women and children, because it was believed that laws regulating the wages of adult men were more likely to be rejected by the courts. Likewise, the states generally established a central minimum wage commission rather than the independent industry-specific wages boards used abroad, because of constitutional concerns about delegating legislative authority to administrative agencies (Douglas 1919).[6]

Table 2.1
Minimum wage legislation prior to enactment of the FLSA

State	Year enacted	Key provisions
Massachusetts	1912	Varying minimum wage rates determined by commissions. Enforcement by public opinion rather than by legal sanction.
Oregon	1913	Mandatory minimum wage set by commissions. Violators subject to fines and imprisonment.
Utah	1913	Single minimum wage set by statute. Violators subject to fines and imprisonment.
Minnesota	1913	Oregon model
Nebraska	1913	Massachusetts model
Wisconsin	1913	Oregon model
California	1913	Oregon model
Colorado	1913	Massachusetts model (1913–1916); Oregon model thereafter
Washington	1913	Oregon model
Arkansas	1915	Utah model
Kansas	1915	Oregon model
Arizona	1917	Utah model
District of Columbia	1918	Oregon model
Texas	1919	Oregon model
North Dakota	1919	Oregon model
Puerto Rico	1919	Utah model
South Dakota	1923	Utah model
New York and seven others	1933–1936	Utah model

Sources: Douglas 1919; Nordlund 1997; Thies 1991.

However, there were also some important differences across states in minimum wage legislation. In particular, Douglas (1919) and Nordlund (1997) classify these laws into three broad models: the Massachusetts model, the Oregon model, and the Utah model. In most respects, the Massachusetts model, which was also adopted by Nebraska and Colorado, was the weakest of the three models. Compliance with the law was largely voluntary (and hence low), and the only consequence to firms of paying a wage less than the minimum wage was adverse publicity, either through publication of a blacklist of employers that paid below the minimum or through omission from a white list of firms that complied with the law (Broda 1928, 34). In addition, the boards and commissions that determined the appropriate minimum wage rates were instructed to take into consideration both employee needs and the financial condition of the industry. Lucas (1924) notes that, as a result, minimum wage rates tended to be low, and firms often ignored the law unless market wages were already above the minimum pay levels.[7]

The Oregon model, which quickly became the most prevalent model used by the states with minimum wages, was similar to the Massachusetts model, in that it relied on wage boards and an oversight commission to set minimum wages by industry. However, it differed from the Massachusetts model in two important respects. First, the sole criterion that was provided to the boards and commissions as guidance for determining the appropriate level of the minimum was the "necessary cost of living" (Douglas 1919, 707). Second, compliance with the minimum wage law was mandatory, with enforcement backed up by penalties that included fines and imprisonment for employers who violated the statutes.

The third approach to minimum wage legislation—the Utah model—bears the greatest similarity to today's laws. The most notable aspect of this model is that the minimum wage was set by fiat rather than by wage boards and commissions. In addition, this model used "flat-rate laws," which meant that the same minimum wage rate applied to all industries in the state, although different wage rates were often set for children and trainees. As Douglas (1919) notes, this model avoided the constitutional concerns about the delegation of authority to administrative agencies. But it was also criticized for its lack of flexibility in setting minimum wage rates.

Regardless of the particular model they followed, the state minimum wage laws quickly met with opposition from employers and thus were regularly challenged in the courts. The essential argument made by opponents of the minimum wage was that it violated employers' constitutional rights to enter freely into contracts and deprived them of their private property (i.e., their profits) without due process of law. In contrast, proponents of the laws argued that the public interest in protecting the health and welfare of citizens outweighed these constitutional concerns and thus gave the states the power to enact minimum wage laws.

There was little conformity in how the courts responded to the initial legal challenges brought against the early minimum wage laws. In some states, the lower courts upheld the statutes, while in other states, courts declared them unconstitutional. In addition, the lower court rulings were frequently overturned on appeal. For the most part, however, the state minimum wage laws survived into the early 1920s, bolstered by the U.S. Supreme Court's 1917 decision in *Settler v. O'Hara* to let Oregon's minimum wage law stand on a 4–4 vote.

The situation changed in 1923, when the U.S. Supreme Court ruled in *Adkins v. Children's Hospital* that the District of Columbia minimum wage law was unconstitutional because it violated the due process clause of the Fifth Amendment. This ruling was decided on a close (5–3) vote and was widely criticized by proponents of minimum wages. Nevertheless, the *Adkins* decision was followed by the overturning of several other state minimum wage laws, and it led administrators in other states with minimum wages to weaken enforcement provisions in order to avoid further court appeals. Thies (1991) notes that by the end of the 1920s, seven of the original seventeen minimum wage laws were declared unconstitutional, five others were either repealed or not enforced, and the remainder were rendered largely ineffective by the adjustments made to them in response to the possibility of legal action.

2.3 Early Views among Economists

The appropriateness of minimum wage legislation was also contentious among economists of that time. Taking a neoclassical position consistent with his theoretical approach to economic analysis, John Bates Clark argued "We can be sure . . . that raising the rate of wages will, of itself and in the absence of any new demand for labor, lessen the number of workers employed" (1913, 290), although he also acknowledged, "A certain low minimum rate may be clearly and wholly legitimate . . . in ruling out some of the hardest practices that now prevail" if minimum wages could be set "at the level fixed by the productive power of the individual workers" (292). However, according to Leonard (2003), it is clear that Clark believed that higher productivity was the only effective means of raising the incomes of low-skilled workers in the longer run and viewed minimum wages as possibly appropriate only when workers are exploited in non-competitive labor markets.[8] Other well-known economists at the time, including Lees Smith (1907) and Taussig (1916), similarly emphasized that economic theory suggested that minimum wages would reduce the employment of lower-skilled workers.[9]

Progressive economists disputed this characterization of the labor market and, in particular, the assumption that wages were determined by a worker's productivity. Sidney Webb (1912) argued, for example, that employers set wages based on the subsistence needs of their workers and thus had an incentive to hire the least able workers with fewer needs (e.g., dependent women and children). In his view, a minimum wage was necessary to prevent the widespread exploitation of lower-skilled workers. Moreover, he believed that a minimum wage would encourage those workers who could do so to increase their efforts and abilities and would prompt employers to reduce other inefficiencies in their business practices.[10] According to Prasch (1999), other prominent Progressive economists in the early twentieth century, including Henry Rogers Seager and John Commons, took a similar view, and a few supporters—for example, Filene (1923)—even argued that raising the minimum wage would increase consumers' purchasing power, thereby boosting aggregate demand.[11] Such arguments about minimum wages are still prominent today—for example, in the context of efficiency wage models of labor markets (Rebitzer and Taylor 1995), as well as in less well-formulated claims that a higher minimum wage will stimulate the economy by raising the purchasing power of the household sector.

Although many economists expressed their opinions about the advisability of the new minimum wage laws based on theoretical reasoning, little empirical evidence was brought to bear in this literature, which is perhaps unsurprising, given that minimum wages had been introduced only recently. That is not to say that there was no interest in assessing their actual effects. For example, the New York State Factory Investigating Commission, which was studying the advisability of introducing minimum wage laws in that state, sent a questionnaire to the British Board of Trades asking about the effects of the minimum wage in England.[12] In addition, several studies, including some conducted by state and federal agencies, examined data on the distribution of women's wages before and after the effective date of the minimum wage laws, and reported that the new minimum wages raised women's wages in some states; however, in a number of other states the evidence was less definitive and often indicated that many women were still earning less than the minimum wage. Many of these studies also concluded that the new laws had little effect on the employment of women, although Thies (1991) argues that most of these conclusions either were not based on any statistical evidence or were seemingly contradicted by the employment patterns reported by the authors.[13]

In this regard, a Bureau of Labor Statistics (BLS) report on the effects of the Oregon minimum wage law by Obenauer and von der Nienburg (1915) is often considered to be the most significant early empirical study of minimum wages.[14] For this study, the BLS collected data on employment, wages, and sales from forty retail stores in Oregon for March and April of 1913, about five months prior to the introduction of the minimum wage in the state, and for March and April of 1914, about five months after the minimum wage first took effect.[15] The authors of the study cautioned that "the number of women affected . . . were too small and the time for adjustment . . . was too short to allow the results of the study to do more than show tendencies" (7); in addition, the analysis was complicated by a recession in 1914 and by a legislated reduction in legal working hours for women. Nonetheless, the authors concluded that the minimum wage had a positive effect on women's wages and led to little or no decline in women's employment (which they instead attributed to the economic recession), although they did find evidence that stores substituted teenagers (who were subject to a lower minimum wage) for adult women in lesser-skilled jobs.[16]

2.4 The Fair Labor Standards Act

Absent decisive evidence that minimum wages led to substantial employment losses among women, the states continued to look for ways to implement minimum wages in a way that would avoid successful legal challenges. New York enacted a "fair" wage (rather than a "living" wage) law in 1933, and over the next two years a number of other states followed suit (Nordlund 1997, 25–26). However, these laws were subsequently interpreted by the courts as effectively equivalent to the D.C. law that had been struck down in 1923, and most were left ineffective by the U.S. Supreme Court's 1936 decision to declare the New York law unconstitutional.[17]

At the same time, however, the economic problems of the Great Depression were stimulating interest in minimum wage legislation at the federal level. In 1933, President Roosevelt, as part of the New Deal, signed the National Industrial Recovery Act (NIRA), which, among other things, pressured employers to agree "to a workweek between 35 and 40 hours and a minimum wage of $12 to $15 a week"

(Grossman 1978, 23). In addition, the Department of Labor, led by Labor Secretary Frances Perkins, organized "conferences" of state minimum wage administrators and representatives from groups supporting the minimum wage in an effort to encourage more consistent minimum wage provisions across states. In 1935, however, the Supreme Court declared the NIRA unconstitutional. This decision, coupled with the Supreme Court's refusal to uphold state minimum wage laws, led many employers to retreat from the wage and hours practices that they had agreed to under the NIRA.

Following his reelection in November 1936, Roosevelt renewed his efforts to implement a federal minimum wage. To address more generally the issue of Supreme Court opposition to the New Deal programs, he threatened to "pack" the Court with six additional judges. Shortly thereafter, the Court upheld the state of Washington's minimum wage law by a 5–4 vote. Whether there is a causal link between Roosevelt's threat to pack the court and the change in the Supreme Court's view on minimum wages has been debated extensively by historians. As a factual matter, however, Justice Owen Roberts, who had voted to strike down New York's minimum wage law in 1936, voted to uphold the Washington (state) minimum wage, thereby swinging the Court's view on minimum wages in the other direction.[18] And, in the end, Roosevelt withdrew his court-packing proposal.

In any event, with the Supreme Court now seemingly less likely to stand in the way, the Congress began in 1937 to consider legislative proposals for a labor standards bill that included a federal minimum wage of 40 cents per hour and a five-person board that could raise the minimum wage in particular industries after review.[19] Three aspects of the initially proposed bill were especially controversial. First, many southern congressmen wanted to set a lower minimum wage in the South to reflect the lower cost of living and to offset the higher freight costs in that region. Second, many union leaders, especially those in the American Federation of Labor (AFL), wanted to exclude from the bill workers covered by collective bargaining, fearing that the proposed wage board would interfere in contract negotiations. Third, the AFL and some in Congress opposed the five-person wage board and instead argued for a single administrator and a statutorily set minimum wage for all industries.[20]

After several failed legislative efforts, extensive pressure from President Roosevelt, and numerous rounds of drafting and redrafting, a compromise bill was approved by the Congress and signed by Roosevelt in June 1938. The FLSA provided for an initial minimum wage of 25 cents per hour, with an increase to 30 cents in the second year and a minimum wage of at least 40 cents per hour by 1945.[21] In addition, the act created a Wage and Hour division in the Department of Labor to administer the program. No regional differentials were included in the act, although minimum wages could differ by industry after review by industry committees that were appointed by the administrator. Finally, only firms engaged in interstate commerce (broadly defined) were subject to the act, and many industries—most notably, retailing and agriculture—were exempted from the law altogether; indeed, by some estimates, only about 20 percent of the U.S. workforce was initially covered by the FLSA, and only about three hundred thousand workers benefited from the new 25 cent minimum wage (Grossman 1978, 29).[22]

The minimum wage provisions of the FLSA differed in several important ways from those of the earlier state laws. First, they applied to both sexes rather than to just women. Second, employers who violated the FLSA were subject to fines, imprisonment, and damages for unpaid wages, a sharp contrast to the weaker state laws that had been implemented in an effort to avoid legal challenge. And third, the establishment of a uniform national minimum wage eliminated much of the variation in minimum wage rates across states that had resulted as a by-product of the earlier state legislation. That said, with so many workers exempted from the federal law, both President Roosevelt and Secretary of Labor Perkins believed that a role would remain for state minimum wage laws (Nordlund 1997, 49).

Not surprisingly, the constitutionality of the FLSA was almost immediately challenged in the courts. Recognizing that any decision they rendered would eventually be appealed to the Supreme Court, judges in the lower courts wasted little time debating the FLSA, and by 1940, the Supreme Court itself turned to the judicial arguments raised by opponents of the act (Nordlund 1997, 52). The question in the first case brought to the Court (*United States v. Darby*) was whether the Congress could apply the FLSA to manufacturing employees who produced goods for interstate commerce but who were not engaged in the actual shipment of the goods across state borders. The key question raised in the second case that reached the Supreme Court (*Opp Cotton Mills, Inc., et al. v. Administrator of Wage and Hour Division of Department of Labor*) was whether Congress was permitted by the Constitution to delegate authority for setting minimum wages to an administrator. In both cases, the Court refused to overturn the FLSA, which effectively "laid to rest the constitutionality" of the act (55).

2.5 The Marginalist Debate

Echoing the early 1900s, the passage of the FLSA led to a renewed debate among economists about the likely effects of the minimum wage, most notably in the context of the "marginalist" controversy of the 1940s. Similar to the dialogue between Clark, Webb, and others, this debate primarily revolved around the appropriate theoretical model of the labor market. In particular, George Stigler (1946) and Fritz Machlup (1946) defended the neoclassical (or marginal) model, in which minimum wages reduce employment of low-skilled workers. Stigler acknowledged the possibility that minimum wages might not reduce employment if the minimum wage induces the firm's employees to work harder and could even increase employment in the monopsony case where "an employer has a significant degree of control over the wage rate he pays for a given quality of labor" (1946, 360). However, he dismissed the former effect as "not very probable" (359) and a national minimum wage as "wholly unsuited to [the] diversities of conditions" associated with the monopsony model of the labor market. Moreover, he viewed industry-level data as suggesting that "(1) the low-wage industries are competitive, and (2) the ratio of wages to total-processing-cost-plus-profit is higher than in high-wage industries" (359). As a result, he concluded that "there is a presumption that a minimum wage will have adverse effects upon aggregate employment" (362).

In contrast, Lester argued that the competitive model of the low-wage labor market was incorrect. In particular, he asserted that "market demand is far more important than wage rates in determining a firm's volume of employment" (1946, 81), and that "it would be utterly impractical under present conditions for the manager of a multi-process plant to attempt . . . to work out and equate marginal costs and marginal returns for each productive factor" (75). As evidence, he pointed to a 1945 survey that he conducted of southern manufacturing firms, in which only 15 percent of respondents mentioned wages as a factor in determining the level of employment, which he argued "raises grave doubts as to the validity of conventional marginal theory and the assumptions on which it rests" (81).

In the ensuing debate, both sides stood their ground. Machlup (1946) criticized Lester's survey, arguing that the low response rate and vagueness and other inadequacies in the survey instrument meant that "even the most complete, reliable, and intelligent answers could not have yielded significant findings" (548–549). More fundamentally, Machlup asserted that whether or not individual employers actually understood and made the marginal calculations implicit in profit maximization was irrelevant to the question of the adequacy of the neoclassical model as long as the aggregate outcomes of their behavior conformed to the predictions of the theory, an argument later expounded in more general terms by Friedman (1953).[23]

Lester, however, refused to back down. He asserted that "Machlup's admissions and inclusions leave the doctrine [of marginalism] weak and distended" and that "Stigler's strict application of 'pecuniary' marginalism to the labor market . . . exposes it to further discredit" (1947, 135). In his view, it was simply incorrect to model "labor markets as though they were commodity markets" (146). Rather, "a firm may increase its wage scale for a variety of nonmarket reasons, such as notions of 'fairness' and 'rightness,' increases in the cost of living, custom and tradition, maintenance of historic relationships, desire for the security from criticism provided by conformance to an industry pattern, public sentiment, etc." (147–148). And, as a result, "wage-employment relationships for individual firms cannot be adequately explained if we confine our thinking within the mental ruts of marginalists" (148).

One reason for the ferocity of this debate was the limited amount of serious empirical research on the effects of the minimum wage. In addition to his survey of individual businessmen, Lester (1946) pointed out that a Department of Labor study in 1941 found that employment increased faster in low-wage southern industries (men's cotton garments and wood furniture) between October 1937 and February 1941 than in similar industries in the North, despite the larger effect of the minimum wage on average hourly earnings in the South; in addition, he reported that within the wood furniture manufacturing industry, the southern plants paying below 35 cents per hour in 1937 had larger increases in employment between 1937 and 1941 than did plants initially paying at or above 35 cents per hour. However, Machlup dismissed these comparisons as "nearly useless because we have no way of eliminating the simultaneous effects of several other significant variables" (1946, 548).[24] Moreover, as Kennan (1995) notes, other studies found results more consistent with negative effects on employment from the introduction of the federal minimum wage. For example, Moloney (1942) studied the southern cottonseed industry and reported that the minimum wage had led to greater adoption of labor-saving machinery and reduced employment levels. Similarly, Hinrichs (1940) and Douty (1941) found relative declines in hours and employment in the lowest-wage seamless hosiery plants following the introduction of the minimum wage, reflecting the greater use of automatic knitting machines and elastic top attachments at such firms. However, these studies were also subject to the criticisms leveled by Machlup.

2.6 The Evolution of the Minimum Wage under the FLSA

Although the FLSA called for a gradual increase in the minimum wage to 40 cents per hour by October 1945, the recommendations of industry committees and their acceptance by the administrator had raised the minimum to that level in all industries covered by the FLSA well before that date (Nordlund 1997, 52). However, there was no provision in the original FLSA to raise the minimum wage above 40 cents per hour, and, absent new legislation to amend the act, the minimum wage declined in real terms throughout most of the 1940s.

Following the end of World War II, the attention of the public and lawmakers turned once again to the domestic economy. The Truman administration and congressional Democrats began pushing for both a higher minimum wage level and an expansion in the number of workers covered by the FLSA, while industry representatives again highlighted the potential costs of a minimum wage increase. After considerable debate about both the appropriate level and coverage of the minimum wage, the first major amendments to the FLSA were signed into law in late 1949. These amendments increased the minimum wage to 75 cents per hour, but made only minor modifications to the coverage of the act to include workers in the air transport industry.

Subsequent changes to the level of the minimum wage in the 1950s and 1960s tended to follow a similar pattern (table 2.2 and figure 2.1). The nominal minimum wage would remain unchanged for several years and thus fall in real terms because of inflation. Then, after considerable debate, Congress would pass new legislation to amend the act to increase the minimum wage. For example, the 1955 amendments raised the minimum wage to $1.00 per hour in 1956, and the 1961

Table 2.2
History of federal minimum wage legislation

Effective date	Minimum wage	Coverage	Other changes
Oct. 24, 1938	$0.25		
Oct. 24, 1939	$0.30		
Oct. 24, 1945	$0.40		
Jan. 25, 1950	$0.75	Coverage expanded to air transport industry	
Mar. 1, 1956	$1.00		
Sept. 3, 1961	$1.15	Coverage extended to retail and service enterprises with annual sales exceeding $1 million	$1.00 minimum wage for workers newly covered by 1961 amendments
Sept. 3, 1963	$1.25		
Sept. 3, 1964			$1.15 minimum wage for workers newly covered by 1961 amendments
Sept. 3, 1965			$1.25 minimum wage for workers newly covered by 1961 amendments
Feb. 1, 1967	$1.40	Coverage extended to public schools, nursing homes, laundries, construction, and farms employing more than 100 man-days of labor annually; reduced sales volume exemption to $500,000 or less	$1.00 minimum wage for workers newly covered by 1966 amendments (effective in 1967)
Feb. 1, 1968	$1.60		$1.15 minimum wage for workers newly covered by 1966 amendments
Feb. 1, 1969		Reduced sales volume exemption to $250,000 or less	$1.30 minimum wage for workers newly covered by 1966 amendments
Feb. 1, 1970			$1.45 minimum wage for nonfarm workers newly covered by 1966 amendments
Feb. 1, 1971			$1.60 minimum wage for nonfarm workers newly covered by 1966 amendments

Table 2.2
(continued)

Effective date	Minimum wage	Coverage	Other changes
May 1, 1974	$2.00	Coverage extended to federal and state and local government workers (coverage of state and local workers overturned by U.S. Supreme Court in 1976)	$1.90 minimum wage for nonfarm workers newly covered by 1966 amendments; $1.60 for farm workers
Jan. 1, 1975	$2.10		$2.00 minimum wage for nonfarm workers newly covered by 1966 amendments; $1.80 for farm workers
Jan. 1, 1976	$2.30		$2.20 minimum wage for nonfarm workers newly covered by 1966 amendments; $2.00 for farm workers
Jan. 1, 1977			$2.30 minimum wage for nonfarm workers newly covered by 1966 amendments; $2.20 for farm workers
Jan. 1, 1978	$2.65		Applicable to all covered, nonexempt workers
Jan. 1, 1979	$2.90		
Jan. 1, 1980	$3.10		
Jan. 1, 1981	$3.35	Increased sales volume exemption to $365,500 or less	
Jan. 1, 1986		Coverage extended to state and local government workers after U.S. Supreme Court reversal	
Apr. 1, 1990	$3.80	Increased sales volume exemption to $500,000 or less	Established ninety-day training 85 percent subminimum wage for workers under age twenty (this provision expired in 1993 and was not renewed)

Table 2.2
(continued)

Effective date	Minimum wage	Coverage	Other changes
Apr. 1, 1991	$4.25		
Oct. 1, 1996	$4.75		
Sept. 1, 1997	$5.15		Established ninety-day youth subminimum wage for workers under age twenty set at $4.25
Jul. 24, 2007	$5.85		
Jul. 24, 2008	$6.55		
Jul. 24, 2009	$7.25		

Sources: United States Department of Labor:
http://www.dol.gov/esa/minwage/chart.pdf;
http://www.dol.gov/esa/minwage/coverage.htm.

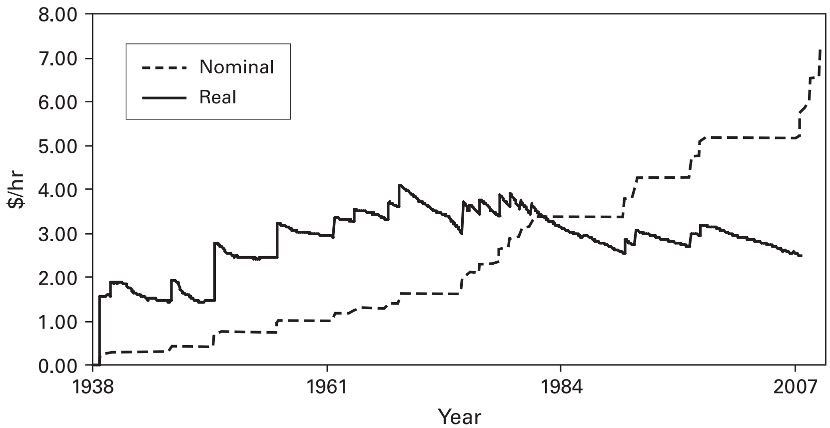


Figure 2.1
History of the U.S. federal minimum wage
Note: The real series is constructed by deflating the nominal minimum wage by a combination of the consumer price index for all urban consumers (CPI-U) from 1938–1966, the experimental CPI (CPI-U-X1) from 1967–1977, and the CPI current-methods series (CPI-U-RS) from 1978–2007. The deflator is indexed so that 1983 = 1.0.

amendments raised the minimum to $1.15 per hour (for workers already covered by the law) in September of that year and to $1.25 per hour in September 1963. New legislation in 1966 increased the wage floor to $1.40 per hour in 1967 and to $1.60 per hour in 1968, where it remained until 1974.

Changes in the coverage of the law were less common, but equally or even more important to employers. As noted earlier, coverage was expanded only slightly in 1949, and the 1955 amendments did not make any additional changes to that aspect of the law. In 1961, however, coverage was increased to include nearly six million workers in the retail trade and services industries, although the minimum wage for such workers was initially set below the level for previously covered workers and was only gradually raised to the full minimum wage by 1965 (U.S. Department of Labor 2007). Policymakers again expanded coverage significantly in 1966 by reducing the small business exemption from a sales volume of $1 million to $500,000 in 1967 and to $250,000 in 1969, and by extending coverage to public schools, nursing homes, laundries, construction firms, and larger agricultural employers; by some estimates, these amendments made another nine million workers subject to minimum wage laws (Nordlund 1997, 115). Again, the minimum wage for newly covered workers was phased in gradually, and in the case of newly covered farm workers, remained below the level pertaining to other covered workers. Nonetheless, the 1960s were characterized by the sharpest increase to date in the percentage of the private non-supervisory workforce subject to the minimum wage, from about 60 percent in 1960 to nearly 80 percent in 1970 (Brown 1999, 2112; U.S. Department of Labor 2007).[25]

Policymakers took a different approach in the mid-1970s, raising the minimum wage in nearly every year between 1974 and 1981. In particular, the 1974 amendments to the FLSA increased the hourly minimum in three steps from $1.60 to $2.30 by January 1976.[26] Subsequently, the 1977 amendments boosted the minimum wage in four steps to $3.35 per hour in 1981 and eliminated the lower minimum wage differential for large agricultural employers.

2.7 The Beginning of Modern Research on the Effects of the Minimum Wage

Throughout the 1940s and 1950s, establishment-level surveys conducted by the U.S. Department of Labor (DOL) formed the basis for much of the empirical work on the minimum wage (U.S. Department of Labor 1942, 1954, 1959). In addition to the studies of specific state minimum wage laws cited earlier and the surveys that followed the implementation of the FLSA in 1938, the DOL also conducted surveys of wages and employment in selected low-wage industries before and after the January 1950 minimum wage increase to 75 cents per hour, and before and after the January 1956 increase to $1.00 per hour. The 1950 study concluded that there were no significant employment losses in low-wage industries following that minimum wage increase; the 1956 studies found some evidence of disemployment effects, but argued that "experience thus far under the Fair Labor Standards Act indicates that a uniform minimum wage can be used to improve wage standards in relatively low-wage industries where labour markets are such that wage adjustments tend to lag behind those in the economy more generally" (Douty 1960, 147–148).

However, the appropriate interpretation of the data from these studies remained the subject of considerable debate. For example, John Peterson published two articles on minimum wages that argued that the earlier government surveys did produce evidence of disemployment effects. The first paper (Peterson 1957) was a response to the 1938–1940 and 1949–1950 DOL studies that concluded there was no evidence of employment losses in various low-wage manufacturing in response to increases in the federal minimum wage. Peterson noted that the DOL studies focused on the overall employment changes in these industries, which he argued were "not . . . adequate to isolate the net employment effects of the minimum wage" (413).[27] Instead, he examined differences in employment changes across firms in three industries—southern sawmills, men's cotton garments, and seamless hosiery—and consistently found relative employment declines for those firms for which the minimum wage induced a larger wage increase. Peterson's second paper reexamined the evidence from three well-known Women's Bureau studies on the effects of state minimum wages on employment and argued that "the generally accepted conclusion—that the minimums had *no effects* on women's employment—is based on a misreading of the facts presented" (1959, 406). Instead, he concluded that, in each case, the data were more consistent with "the orthodox hypothesis that employment is inversely related to the wage level" (422).

These two papers elicited a strong response from Richard Lester (1960), who asserted that Peterson inadequately controlled for other factors (such as macroeconomic conditions) influencing employment in some of his analyses and that the evidence from other analyses was much more mixed than Peterson claimed. In reply, Peterson disputed these assertions on a point-by-point basis, and concluded that "when Lester flatly concludes that the hypothesis 'flunks the test of prediction,' he seems to be in another realm of discourse" (1960, 271). Peterson further pointed out that the DOL study of the 1956 increase in the minimum wage, the results of which were cited by Lester for three industries, found that the wage increase was associated with disemployment effects in nine of the twelve low-wage industries included in that study.

Irrespective of this particular debate, the center of economic thinking was moving toward the view that minimum wages would reduce employment along the lines suggested by the competitive model of the labor market. Nordlund (1997, 117) highlights speeches given in 1964 by Jules Backman and by George Stigler, who at the time was president of the American Economic Association, as an indication of this shift. In any event, for most economists (though not all), the questions of interest were increasingly centered on the precise nature of the distortionary effects of minimum wages predicted by the theory and whether they were large enough to present adverse outcomes for the economy.

This shift in thinking also stimulated a considerable amount of new empirical research focused on estimating the magnitude of the effects of the minimum wage on employment and unemployment. In particular, researchers began to take advantage of the longer history of the minimum wage by turning to time-series methods, which allowed them to make greater use of the variation in the federal minimum wage and to better control for other factors that potentially influence labor market outcomes. Of course, even this research had its limitations—most notably, the time-series variation in the federal minimum wage was not especially great; but, over the course of the 1970s, the time-series approach came to be the predominant means of estimating the employment effects of minimum wages.[28]

The literature on minimum wages in the 1960s and 1970s is summarized extensively in Brown, Gilroy, and Kohen (1982), which we discuss shortly. However, two studies are worth singling out here for their contributions to that literature. The first is a paper by Kaitz, who examined the effects of the minimum wage on youth employment and unemployment using quarterly and annual time-series data from 1954 to 1968.[29] Kaitz reported some evidence of a negative effect of the minimum wage on teenage employment in specifications that included controls for the business cycle and other factors, but he emphasized that these findings were quite sensitive to which controls were included and concluded that his analyses "do not permit confident conclusions about the effect of minimum wage laws upon the employment experience of teenagers" (1970, 45). However, the most important contribution of this paper was Kaitz's development of a new minimum wage variable—subsequently referred to as the "Kaitz index"—which took account of both changes in the minimum wage relative to market-determined wages and changes in the proportion of workers covered by the law. As Brown, Gilroy, and Kohen noted, "the Kaitz index has the advantage of summarizing a great deal of information about the minimum wage law in a single variable" (1982, 500), and it subsequently became the standard minimum wage variable used in the time-series research.

Kaitz also pointed out that the effects of the minimum wage on employment and unemployment could be quite different because of the lack of a sharp behavioral distinction between unemployed individuals and those who are out of the labor force. That is, if workers displaced from their job due to the higher minimum wage left the labor force rather than searching for work, or if nonworking individuals who could not find a job because of the minimum wage became discouraged and stopped looking for work, they would not be counted in the official measures of the unemployment rate. As a result, he argued that "equations using [unemployment] rates as dependent variables . . . will be somewhat more difficult to interpret" (1970, 35), and that the effects of the minimum wage on labor demand are better measured by changes in the employment-to-population ratio. Over time, researchers increasingly focused on this metric.

The second study we highlight for its contribution to this literature is by Gramlich, who estimated minimum wage effects on a variety of outcomes, including employment and wages. However, the main contribution of this study, in our view, is that it pointed out that the employment effects of minimum wages, which were the focus of the bulk of the literature in existence at that time, are only part of the story. In particular, Gramlich wrote: "Minimum wages do, of course, distort relative prices, and hence compromise economic efficiency, but so do all other attempts to redistribute income through the tax and transfer system. The important question is not whether minimum wages distort, but whether the benefits of any income redistribution they bring about are in some political sense sufficient to outweigh the efficiency costs" (1976, 410). He then proceeded to assess the effects of minimum wages on the distribution of family incomes, concluding that "as long as minimum wages are kept low relative to other wages, they are not terribly harmful and in fact even have slightly beneficial effects both on low-wage workers and on the overall distribution of incomes. They are far from the *best* way of redistributing income, however, and there are definite limits to how high the minimum can be raised" (450–451).[30]

This new research did little to quell the debate over minimum wages. For some, the sharp expansion of coverage in the 1960s and the steady rise in the level of the wage floor in the second half of the 1970s intensified concerns about the potential negative consequences of minimum wage policy. For example, in his discussion of the debate leading up to the 1977 amendments, Nordlund pointed to an editorial in the *New York Times* that argued that "the minimum wage, as a policy tool, no longer made economic sense . . . [because] there would be general displacement effects, there would be North-South competitive shifts, and . . . an increase would not address the vagaries of poverty" (1997, 140–141).[31] In contrast, many politicians and union leaders continued to see the minimum wage as a means of reducing poverty and thus considered increasing the federal minimum wage to be a primary legislative goal.[32]

In light of the considerable discord about the appropriateness of raising the minimum wage, and recognizing the need for a wholesale assessment of minimum wage policy, as part of the 1977 amendments to the FLSA, Congress established the Minimum Wage Study Commission "to help it resolve the many controversial issues that have surrounded the federal minimum wage and overtime requirements since their origin in the Fair Labor Standards Act of 1938" (Minimum Wage Study Commission 1981, xiii). The commission consisted of eight members and a staff of more than twenty persons, including six economists, and contracted with more than fifty additional economists to produce forty-four studies in six research areas.[33] The commission published its report in May 1981, calling it "the most exhaustive inquiry ever undertaken into the issues surrounding the Act since its inception" (Minimum Wage Study Commission 1981, n.p.).

Although the focus of much of the research on the minimum wage continued to be on its effects on employment, an important feature of the commission's report was its consideration of the influence of minimum wages on other economic outcomes. For example, studies included in the report examined the influence of minimum wages on inflation, poverty and the income distribution, and human capital accumulation. In addition, some sections of the report examined compliance with the minimum wage and the role of specific exemptions to the FLSA.

In the end, however, the report itself had little impact on policymakers. According to Eccles and Freeman, "The MWSC final report and recommendations . . . did not attract much attention. Press coverage was minimal . . . [and] . . . there were no briefings on the findings for press or Capitol Hill" (1982, 230). These authors attributed the lack of interest in the report to "a lack of connection between the research findings and the Commission recommendations, lack of unanimity in the Commission recommendations, and the changed political climate" (232).

Instead, the most notable outgrowth of the Minimum Wage Study Commission's report (1981) was a review of the existing literature on the effects of the minimum wage on employment by three of the commission's senior staff economists, which was subsequently published in the *Journal of Economic Literature* (Brown, Gilroy, and Kohen 1982). These authors recognized that the primary contribution of the recent literature was empirical in nature, noting that "although arguments for and against the minimum wage are the same today as when the Fair Labor Standards Act was passed forty years ago, they are now accompanied by more sophisticated approaches to the measurement of the law's impact" (487). In summarizing the literature, Brown, Gilroy, and Kohen concluded that "time-series studies typically find that a 10 percent increase in the minimum wage reduces teenage employment by one to three percent" and that "the effect of the minimum wage on young adult (20–24 years) employment is negative and smaller than that for teenagers" (524). They also noted that "the direction of the effect on adult employment is uncertain in the empirical work, as it is in the theory" (524).

This conclusion soon came to be viewed as the "consensus" of economists on the effects of the minimum wage on employment. Indeed, in the wake of the comprehensive report of the Minimum Wage Study Commission (1981), researchers spent little of their time over the remainder of the 1980s on research related to the minimum wage. Perhaps their view was similar to our own academic advisers, who cautioned us against working in an area where nearly all of the interesting questions had been answered. In any event, labor economists turned their attention to other issues for the next decade.

2.8 Recent Changes in the Federal Minimum Wage and the Expansion of State Minimum Wages

There were few substantive changes in federal minimum wage policy after the increase in the hourly minimum to $3.35 in January 1981. Although proposals for additional increases in the minimum wage were regularly introduced by members of Congress, they made little headway in the face of a conservative administration and Republican control of the Senate, and indeed, there were nearly as many calls for a subminimum wage for youths as there were for a higher minimum wage for the private sector as a whole. As a result of the absence of additional increases in the nominal minimum wage for the longest period since the inception of the FLSA, by the end of 1989 the real value of the federal minimum wage had declined by roughly 30 percent.

As it became increasingly apparent during the 1980s that a federal minimum wage increase would not be enacted, state legislatures began to raise their states' minimum wage levels above the federal level.[34] A few states had consistently set a relatively high minimum wage—most notably Alaska, which pegged its wage floor at 50 cents above the federal level to account for its higher cost of living, and Washington, D.C., which had an array of minimum wage rates for different occupations, most of which were above the federal level.[35] However, beginning in 1985, several New England states raised their minimum wages to slightly above the $3.35 per hour federal level, and by early 1990, sixteen states and Washington, D.C., had a minimum wage higher than the national minimum. The highest state minimum wage at the time was $4.25 per hour, effective in California, Connecticut, Oregon, Rhode Island, and Washington[36] (Nelson 1990, 35–36).[37]

During this expansion of state minimum wages, efforts to raise the federal minimum wage began to regain momentum. After President Bush took office in January 1989, Congress passed a bill that would have raised the minimum wage to $4.55 per hour over the next three years. However, the president vetoed the bill and instead proposed a smaller increase to $4.25 per hour along with a lower training wage for youths (Nordlund 1997, 205). After much additional debate, the Congress passed and the president signed an amendment to the FLSA that raised the minimum wage to $3.80 per hour on April 1, 1990, and to $4.25 per hour the following April. The amendment also included a ninety-day training wage for youths and increased the sales exemption for small businesses. After another five years, and with the political balance of power shifted more heavily toward the Democrats, the minimum wage was raised to $4.75 per hour in September 1996 and to $5.15 per hour one year later. The federal minimum wage then remained at $5.15 for the next decade. But, in 2007, with the Democrats back in control of Congress, the minimum wage was raised to $5.85 on July 24, and will increase further, to $6.55 in July 2008 and to $7.25 in July 2009.

Although the increases in the federal hourly minimum wage to $3.80 in 1990 and to $4.25 in 1991 temporarily stemmed the tide of state minimum wage hikes, actions by state legislatures soon resumed. New Jersey raised its minimum wage to $5.05 in April 1992, and several other states followed suit over the next few years. Similarly, the number of state-specific increases in minimum wages dropped off briefly after the 1996–1997 increases in the federal minimum but picked up again as prospects for further increases diminished after the 2000 election. As of January 2008, thirty-two states and Washington, D.C., had a minimum wage higher than the federal level (table 2.3). In addition, nineteen states and Washington, D.C., had a minimum wage of at least $7.00 per hour, with the highest wage floor set at $8.07 in Washington.[38]

Moreover, in recent years, minimum wage increases have become more common in larger states (such as Florida, Illinois, and New York), whereas—with the exception of California—the states with high minimum wages in previous years had typically been relatively small. As a result, the share of the population aged sixteen to sixty-four residing in states with a minimum wage higher than the federal level rose from about 15 percent in 1998 to about 70 percent in early 2008. Finally, a number of local jurisdictions have recently enacted either their own minimum wage or a living wage, boosting the percentage of the workforce covered by a non-federal minimum wage law even further.

2.9 Conclusions

The federal minimum wage has now been in effect for seventy years, and state minimum wage laws have been around in some form or another for nearly a century. Our brief narrative history illustrates how

Table 2.3
State minimum wages above the federal level as of January 2008

State	Minimum wage	Comments
Maryland, Minnesota, North Carolina	$6.15	Minnesota: $5.25 for employers with receipts less than $625,000
Arkansas, Montana	$6.25	Arkansas: Four or more employees Montana: $4.00 for employers with sales of $110,000 or less; indexed to CPI-U
Nevada	$6.33	$5.30 when employer offers health insurance
New Hampshire, New Mexico, Wisconsin	$6.50	New Hampshire: $7.25 on Sept. 1, 2008 New Mexico: $7.50 on Jan. 1, 2009
West Virginia	$6.55	Employers with six or more employees
Missouri	$6.65	Retail or service businesses with sales less than $500,000 exempt; indexed to CPI-W
Florida	$6.79	Indexed to CPI-W (South region)
Arizona	$6.90	Indexed to CPI-U
District of Columbia, Maine, Ohio	$7.00	D.C.: Minimum of $1.00 over federal minimum
Colorado	$7.02	Selected industries; indexed to CPI-U
Alaska, Delaware, Michigan, New Jersey, New York, Pennsylvania	$7.15	Michigan: $7.40 on Jul. 1, 2008
Hawaii, Iowa	$7.25	
Rhode Island	$7.40	
Illinois	$7.50	Four or more employees; increments of $0.25 each Jul. 1 through 2010
Connecticut	$7.65	
Vermont	$7.68	Increased each year by greater of 5 percent or increase in CPI-U
Oregon	$7.95	Indexed to CPI-U
California, Massachusetts	$8.00	
Washington	$8.07	Indexed to CPI-W

Source: http://www.dol.gov/esa/minwage/america.htm (viewed March 6, 2008).

controversial minimum wages have been throughout this period—controversy that is reflected not only in the policy debate, but also in how legal scholars interpret the U.S. Constitution. Moreover, the need for Congress to pass legislation to raise the nominal minimum wage has meant that this issue regularly reemerges at the forefront of economic policy debate.

Economic research has also played a prominent role in the ongoing debate about minimum wages. This research was mostly theoretical in nature in the early 1900s, but has increasingly turned to empirical methods to assess the economic effects of minimum wages. In addition, although much of the research has been aimed at informing policymakers, it has also had an important influence in discussions about the adequacy of alternative theories of the labor market proposed by economists. Indeed, it is striking how much of the earlier research on minimum wages, both in terms of the channels through which the minimum wage could influence labor market outcomes and in terms of the empirical methods used to assess those effects, foreshadows the issues that arise in more recent work.

Although further increases in the federal minimum wage will undoubtedly occur, it now seems increasingly likely that state and local government officials will have a larger role to play in determining minimum wage policy. As noted previously, state legislatures have increasingly taken it upon themselves to set minimum wages for their constituents; moreover, local jurisdictions are exhibiting an increasing tendency to legislate minimum wages at the city or county level. Such developments have shifted the relevant questions facing policymakers and economists and have broadened the debate over the efficacy of using minimum wage policy as a redistributional tool. Moreover, as we highlight throughout this book, the proliferation of state and local minimum wages set above the federal minimum wage has enabled researchers to develop and use a new set of empirical strategies for investigating the effects of minimum wages, and has been the genesis for an exciting new set of results. In the remaining chapters of this book, we provide a critical review of the latest research on these issues and attempt to synthesize this research into a digestible whole.

3 The Effects of Minimum Wages on Employment

3.1 Introduction

How minimum wages affect employment has been the most prominent issue with respect to the evaluation of minimum wage policies, and indeed one of the most researched topics in economics. The question is clearly important from a policy perspective, as any potential benefits of the minimum wage in terms of higher earnings are offset by adverse employment effects that may result. But the employment effects of minimum wages are also significant to economists because they provide a means of testing alternative models of the labor market. For example, Stigler (1946), while acknowledging that a higher minimum wage could theoretically raise employment in a labor market characterized by monopsony, argues that the competitive nature of low-wage industries suggested that the displacement of low-wage labor was a more likely outcome. In contrast, more recent work (e.g., Burdett and Mortensen 1998 and Manning 2003), drawing on some evidence suggesting that minimum wages could increase low-wage employment, tends to emphasize monopsony in low-wage labor markets, with monopsony power stemming from labor market frictions rather than from the traditional "company town" model described in economics textbooks.

As noted in the previous chapter, the most ambitious effort to study the employment effects of the minimum wage was launched in 1977, with the creation of the Minimum Wage Study Commission. A central product stemming from the commission's report was the review of the literature by Charles Brown, Curtis Gilroy, and Andrew Kohen (1982). This review largely established the "consensus" view of economists on the employment effects of the minimum wage—that the elasticity of teenage employment with respect to the minimum wage ranges from −0.1 to −0.3.

This review was followed by a lull in research on the employment effects of the minimum wage until the late 1980s, when a number of state legislatures began to raise state-specific minimum wages above the federal level in response to the lack of action by Congress. The new state variation in minimum wages, along with the federal increases in 1990 and 1991, spurred researchers to reexamine the effects of the minimum wage on employment. A narrow line of this research simply extended the earlier studies by adding more recent time-series data to the sample period, employing in some cases new techniques to improve upon the methods used in earlier research. However, a second, larger, and ultimately more important line of research attempted to use state-level variation in minimum wages and economic conditions to estimate the employment effects of the minimum wage. This research strategy addressed concerns about the lack of variation in the federal minimum wage, and also provided a better counterfactual for estimating the effects of a higher minimum wage. Moreover, the proliferation of state minimum wages set above the federal minimum wage was rendering the aggregate time-series approach increasingly obsolete, both from the perspective of correctly measuring the effective minimum wage and from the perspective of the relevant question facing policymakers, which had shifted toward the advisability of raising minimum wages at the state (or even local) level; this is even more true currently, when a record number of states have minimum wages above the federal level.[1]

This chapter reviews this more recent literature (including our own research on the employment effects of minimum wages), which has become known as the "new minimum wage research."[2] We begin with a brief treatment of the alternative theoretical models that form the foundation of the recent empirical work on minimum wages. We then review the set of four papers that constituted the initial round of the new minimum wage research, discuss the major conceptual and empirical issues that arose out of that initial research, and summarize what we view as the best and most important research that followed. Results and key features of the literature are summarized in table 3.1, more or less in the order in which the studies are discussed in this chapter.

The review in this chapter is not exhaustive. In other work (Neumark and Wascher 2007a), we review the entire recent body of literature on the employment effects of minimum wages, encompassing more than one hundred papers written since the early 1990s. Here, we focus on what we view as the most compelling studies from that larger body of research, as well as on a few other studies that have received significant attention in the literature.[3] In our lengthier review of employment effects, we conclude that, overall, about two-thirds of the hundred or so studies that we discuss yield relatively consistent (although by no means always statistically significant) evidence of negative employment effects of minimum wages—while only eight give a relatively consistent indication of positive employment effects. In contrast, of the thirty-three studies we identify as providing the most reliable evidence, more than 80 percent point to negative employment effects.[4] As a result, the evidence we review in this chapter is more indicative of negative employment effects than is a simple description of the distribution of estimated effects reported in the wider literature. However, our purpose in this chapter is to inform the reader about what the best research—in our judgment—has to say about the employment effects of minimum wages.

3.2 The Theory of the Minimum Wage

3.2.1 The Neoclassical Model

The textbook neoclassical model of the effects of minimum wages on employment is straightforward. In its simplest form, the model assumes that labor and product markets are competitive (so that firms take wages, interest rates, and prices as given), there is only one type of labor, output is produced with a mix of labor and capital, and all workers are covered by the minimum wage. Setting the minimum wage above the equilibrium wage raises each firm's marginal cost of production and induces two economy-wide effects. First, the price of output rises and the demand for it falls, leading to a decline in production (the "scale effect"). Second, the higher wage rate causes firms to substitute capital for labor in the production process (the "substitution effect"). As a result, the demand for labor falls, with the magnitude of the decline determined by the responsiveness of product demand to the change in price, labor's share of total production costs, the ease of substitutability between labor and capital, and the difference between the minimum wage and the equilibrium competitive wage. In general, for a given minimum wage change, the decline in the demand for labor will be greater the higher labor's share of costs is, the more responsive product demand is to price changes, and the easier it is to substitute

Table 3.1
Studies of the employment effects of minimum wages

Study	Minimum wage variation	Group	Data	Estimated elasticities
The first wave of the new minimum wage research				
Card 1992a	1990 federal minimum wage increase	Teenagers	CPS, 1989–1990	−0.06 to 0.19; not significant
Neumark and Wascher 1992	Federal and state	Teenagers and youths	CPS, 1973–1989	Teenagers: −0.1 to −0.2 Youths: −0.15 to −0.2
Katz and Krueger 1992	1991 federal minimum wage increase	Fast-food employment in Texas	Survey of restaurants in December 1990 and July/August 1991	1.7 to 2.65; significant
Card 1992b	1988 California minimum wage increase	Teen employment and retail trade employment	CPS; QCEW	Teens: 0.35; significant Retail trade: 0.04; not significant Eating and drinking: −0.07; not significant
Card and Krueger 1994	1992 New Jersey minimum wage increase	Fast-food employment in New Jersey and Pennsylvania	Survey of restaurants in February 1992 and November 1992	FTEs: 0.63 to 0.73; some estimates significant
Studies addressing issues from the first wave of research				
Baker, Benjamin, and Stanger 1999	Across provinces and over time	Teenagers (aged fifteen to nineteen)	Special tabulations from Statistics Canada, 1979–1993	Panel data analysis, with attention to frequency domain: Within-group: −0.27 (−0.47 with one lag) First difference: 0.07 (−0.23 with one lag) Second difference: −0.13 Third difference: −0.31 Fourth difference: −0.40 Within-group estimates and longer-difference estimates significant; similar result reflected in lower-frequency filters

Keil, Robertson, and Symons 2001	Federal and state	Aggregate and youth employment (not defined)	CPS, 1977–1995	Dynamic model: Aggregate: −0.11 (short run); −0.19 (long run) Youths: −0.37 (short run); −0.69 (long run)
Neumark and Wascher 1994	Federal and state	Teenagers and sixteen- to twenty-four-year-olds	CPS, 1973–1989	IV for enrollment in some specifications: Teenagers: −0.17 to −0.39 Youth: −0.12 to −0.16
Deere, Murphy, and Welch 1995	1990 and 1991 federal minimum wage increases	Teens and adult high school dropouts by sex and race	CPS, 1985–1992	Male teens: −0.27 to −0.36 Female teens: −0.42 to −0.49 Black teens: −0.37 to −0.56 Adult high school dropouts: −0.11 to −0.33
Burkhauser, Couch, and Wittenburg 2000a	Federal and state	Teenagers	CPS, 1979–1997; monthly data	1979–1997: 0 to −0.35 1979–1992: −0.24 to −0.57 1996–1997: −0.17 to −0.27 Estimates generally significant in specifications excluding year effects
Sabia 2006a	Federal and state	Teenagers	CPS ORGs, 1979–2004	Teen employment: −0.18 to −0.33 Average hours worked by teens: −0.37 to −0.45 Average hours worked by employed teens: −0.01 to −0.29; almost all estimates significant
Kim and Taylor 1995	1988 California minimum wage increase	Retail trade employment	QCEW	−0.15 to −0.2; some estimates significant
Neumark and Wascher 2000	1992 New Jersey minimum wage increase	Fast-food employment in New Jersey and Pennsylvania	Payroll data collected from establishments	FTEs: −0.1 to −0.25; many estimates significant
Card and Krueger 2000	1992 New Jersey minimum wage increase	Fast-food employment in New Jersey and Pennsylvania	BLS establishment-level data	FTEs: 0.005 to 0.15; not significant

Table 3.1
(continued)

Study	Minimum wage variation	Group	Data	Estimated elasticities
Powers, Baiman, and Persky 2007	2004–2005 Illinois minimum wage increases	Fast-food employment in Illinois and Indiana	Survey of restaurants in 2003, 2004, and 2005	0 or slightly positive to −0.14 for all, full-time, or part-time FTE employment −0.85 to −0.92 for hours −0.75 for FTE employment for 2004–2005
Zavodny 2000	Federal and state	Teenagers	CPS, 1979–1993 Matched CPS, 1979–1980 to 1992–1993	*Aggregate results* Employment: −0.02 to −0.12; Total hours: 0.24 to −0.11 *Individual results* Employment: −0.08 to −0.10 Total hours: positive but not significant
Couch and Wittenburg 2001	Federal and state	Teenagers	CPS, 1979–1992	Employment: −0.41 to −0.58 Total hours: −0.48 to −0.77 Estimates significant
More recent U.S. evidence				
Wellington 1991	Federal	Teenagers and twenty- to twenty-four-year-olds	1954–1986	Teenagers: −0.05 to −0.09 Twenty- to twenty-four-year-olds: 0.002 to −0.02
Williams and Mills 2001	Federal	Teenagers	Data from Card and Krueger 1995a, 1954–1993	Teenagers: −0.3 to −0.5 after two years
Bazen and Marimoutou 2002	Federal	Teenagers	Data from Card and Krueger 1995a, 1954–1993, updated through 1999:Q2	Teenagers: −0.11 (short run), −0.27 (long-run), significant, and similar estimates for subperiods

Neumark 2001	Cross-state variation generated by 1996 and 1997 federal minimum wage increases	Teens, youths (aged sixteen to twenty-four), nonenrolled youths and twenty- to twenty-four-year-olds with high school education or less, and with less than a high school education	October–December CPS files, 1995–1998	Teenagers: centered on 0 Sixteen- to twenty-four-year-olds: −0.02 to −0.22; insignificant Nonenrolled, sixteen to twenty-four, high school or less: −0.11 to −0.53; significant Nonenrolled, twenty to twenty-four, high school or less: −0.09 to −0.15; sometimes significant Nonenrolled, sixteen to twenty-four, less than high school: −0.21; significant Nonenrolled, twenty to twenty-four, less than high school: −0.11 to −0.12; insignificant
Singell and Terborg 2007	Oregon and Washington minimum wage increases	Eating and drinking workers; hotel and lodging workers	BLS monthly employment data, 1997–2001; help-wanted ads, 1994–2001	Eating and drinking employment: −0.2; significant Hotel and lodging employment: 0.15 to 0.16; significant Want-ads: negative and significant for all restaurant jobs except cooks, and for hotel housekeepers
Studies focused on directly affected workers				
Currie and Fallick 1996	1980–1981 increases in federal minimum wage	Workers with initial wage between old and new minimum	NLSY, 1979–1987	−0.19 to −0.24; significant
Abowd, Kramarz, Margolis, and Phillipon 2000	Change in real federal and state minimum wages	Low-wage workers affected by a change in the real minimum wage relative to those marginally above them	Matched CPS, 1981–1982 to 1990–1991	Many results reported for exit and entry elasticities; generally small (of both signs) and not significant

Table 3.1
(continued)

Study	Minimum wage variation	Group	Data	Estimated elasticities
Neumark, Schweitzer, and Wascher 2004	Federal and state	Workers at different points in the wage distribution	Matched CPS, 1979–1980 to 1996–1997	Employment: −0.06 to −0.15 for workers between 1 and 1.3 times the old minimum wage Hours: −0.3 for workers between 1 and 1.3 times the old minimum wage
Neumark and Wascher 2002a	Federal and state, based on probability minimum wage is binding	Youths	CPS, 1973–1989	−0.13 to −0.21 on binding regime
Cross-country evidence for OECD countries				
Neumark and Wascher 2004	Across countries and over time	Teens (fifteen to nineteen) and youths (fifteen to twenty-four)	OECD and various sources, mid-1970s through 2000 (varies by country)	Standard models: teens, −0.18 to −0.24; youths, −0.13 to −0.16 Less negative with youth subminimum, with bargained minimum, with greater employment protection, and with more active labor market policies; more negative with stronger labor standards (working time rules, less flexible contracts), and higher union density
Canada				
Campolieti, Gunderson, and Riddell 2006	Across provinces and over time	Youths aged sixteen to nineteen, twenty to twenty-four, and sixteen to twenty-four, including full-time versus part-time and nonenrolled	April Labor Force Surveys, 1981–1997	Teens: −0.17 to −0.44 Twenty- to twenty-four-year-olds: −0.14 to −0.43 Sixteen- to twenty-four-year-olds: −0.17 to −0.44

Campolieti, Fang, and Gunderson 2005a	Across provinces and over time	Youths (sixteen to twenty-four)	Survey of Labour and Income Dynamics (1993–1999)	−0.33 to −0.54
United Kingdom				
Machin and Manning 1994; Dickens, Machin, and Manning 1999	Wages Councils	All workers in covered (low-wage) industries	New Earnings Survey, Employment Gazette, 1978–1992	0.05 to 0.43
Dolado et al. 1996	Abolition of Wages Councils	Workers in Council and non-Council sectors	Quarterly Labour Force Survey Micro Data	Relative increases in hiring rate and employment in Council sector after Councils abolished
Machin, Manning, and Rahman 2003	Introduction of national minimum wage in 1999	Workers in residential care homes	Labor Force Survey, 1994–2001, and authors' survey of residential care homes	Employment and hours fell more where initial proportion of minimum wage workers or wage gap higher; implied elasticities for employment −0.08 to −0.38; for hours −0.15 to −0.39
Stewart and Swaffield 2006	Variation across workers at different points of the wage distribution	Adult men and women	Matched Labor Force Survey, March 1997–September 2000; New Earnings Survey, April 1994–April 2000	Weekly hours of employed workers declined by 1 to 2 hours, with the reduction occurring at a lag of approximately one year
Galinda-Rueda and Pereira 2004	1. Variation across region-sector cells in fraction below new minimum wage, as of 1998	All	Annual Business Inquiry and New Earnings Survey, 1997–2001	No disemployment effects in manufacturing; in services, 1 percentage point higher fraction affected leads to 0.06 to 0.12 percent lower employment

Table 3.1
(continued)

Study	Minimum wage variation	Group	Data	Estimated elasticities
Galinda-Rueda and Pereira 2004	2. Variation across regions in fraction below by region	All	Office of National Statistics, 1998–2001	Significant disemployment effects in four of eight low-wage sectors, negative estimates in seven of eight; evidence that effects stem in part from slower job creation through firm entry in low-wage sectors
France				
Abowd, Kramarz, Lemieux, and Margolis 2000	Differences between workers caught by national minimum wage increases and workers with slightly higher wages	Various ages	Enquête Emploi, 1982–1989	Large disemployment effects for workers newly constrained by minimum relative to those with marginally higher wages, especially those just above age twenty-four not protected by employment promotion contracts permitting subminimum wages: Men, twenty-five to thirty: −4.6 Women, twenty-five to thirty: −1.38 Men, twenty to twenty-four: −0.77 Women, twenty to twenty-four: −1.21 Men, sixteen to nineteen: −0.08 Women, sixteen to nineteen: 0.46
Spain				
Dolado et al. 1996	Sharp increase in minimum for sixteen-year-olds and more modest increase for seventeen-year-olds in 1990	Teens (16–19)	Contalilidad Nacional Sectorial	Negative relationship across regions between change in teenage employment rate and share initially low-paid, but not for twenty- to twenty-four-year-olds

Portugal				
Pereira 2003	Abolition of teenage subminimum wage in 1987	Eighteen- to nineteen- and twenty- to twenty-five-year-olds	Quadros de Pessoal, 1986–1989	Teen employment (and hours) declined relative to employment of thirty- to thirty-five-year-olds, with elasticity of −0.2 to −0.4; substitution toward twenty- to twenty-five-year-olds
Mexico/Colombia				
Bell 1997	Mexico: minimum wages set by region, consolidated into fewer regions over time Colombia: minimum wages by large/small cities until 1984, then a national minimum wage implemented	Firms in the formal/informal sectors with information broken down by skilled/unskilled workers	Mexico: Annual Industrial Survey (1984–1990); National Minimum Wage Commission Statistical Reports (1984–1990); Mexican Ecuesta Nationale de Empleo (1988 only); time-series data source not specified Colombia: Annual Industrial Survey (1980–1987); National Minimum Wage Commission (1980–1987)	Time series: Mexico, −0.18 (insignificant); Colombia, −0.34 (significant) Panel data, fixed effects: Mexico skilled, −0.01 to 0.05, and unskilled, −0.03 to 0.03 (insignificant); Colombia skilled, −0.03 to −0.24, and unskilled, −0.15 to −0.33 (significant)

Table 3.1
(continued)

Study	Minimum wage variation	Group	Data	Estimated elasticities
Mexico				
Feliciano 1998	Sharp consolidation of regional minimum wages and decline in average minimum wage	Males and females, all ages	Mexican Census of Population, 1970, 1980, and 1990; National Minimum Wage Commission Statistical Reports; Encuesta Nacional de Empleo Urbana	Males: 0.005 to 0.01 (−0.002 to 0.04 by age group) Females: −0.43 to −0.58 (−0.41 to −0.76 by age group)
Costa Rica				
Gindling and Terrell 2007a	Sharp consolidation of occupation-skill-specific minimum wages	All	Household Surveys for Multiple Purposes, industry data from Costa Rican Central Bank, 1988–2000	Covered-sector employment: −0.11 Covered-sector hours of employed: −0.06
Honduras				
Gindling and Terrell 2007b	Minimum wage decrees varying by industry, firm size, region, and time	Large-firm and small-firm covered sectors, and uncovered public and self-employment sectors	Permanent Household Surveys for Multiple Persons, 1990–2004	Fixed effects: 0.14 for large firms, 0.51 (significant) for small firms; Arellano-Bond estimator: −0.46 (significant) for large firms, 0.39 (significant) for small firms; all estimates insignificant for uncovered public and self-employment sectors; Arellano-Bond estimator, by schooling: −1.85 (significant) for primary incomplete, −0.86 (significant) for primary complete, smaller and insignificant for higher education levels

Indonesia				
Harrison and Scorse 2005	District level differences in the minimum wage, within the same province	Manufacturing firms overall and subgroup of textiles, apparel, and footwear factories	Indonesia's Annual Survey of Manufacturing Firms, 1990–1996	Difference-in-differences, elasticity for manufacturing employment: −0.05 for all firms; −0.05 for balanced panel Other specifications: −0.12 to −0.18 (all significant); insignificant only when done separately for small firms, −0.02
Alatas and Cameron 2003	Differences in minimum wage changes between a province, Jakarta, and a grouping of districts, Botabek, across the border in an adjacent province	Manufacturing sector, Greater Jakarta area	Indonesia's Annual Survey of Manufacturing Firms, 1990–1996; Indonesian Labor Force Survey, 1990–1996	Significant negative employment effect only for small domestic firms, 41 percent (16 percent) relative employment loss from 1991 (1992) to 1996 in Botabek, which experienced sharper minimum wage increase; effect no longer significant when restricted to a narrow strip along the border; large foreign firms show insignificant negative effects; large domestic firm estimates are insignificant and inconclusive

Note: In some cases we report, in the last column, the set of estimates that seem to be preferred by the authors. See Neumark and Wascher 2007a for an expanded table with additional comments and details regarding these estimates.

capital for labor.[5] The overall effect on employment or total hours is referred to as the "unconditional elasticity of labor demand with respect to the minimum wage."

One obvious simplification in this model is that it assumes that all workers are covered by the minimum wage. Such an assumption is probably not too far removed from the current situation in the United States, where the combined coverage of federal and state minimum wage laws exceeds 90 percent of hourly workers and most employers comply with the law. But it may be more problematic for thinking about minimum wages in less-developed countries, where coverage is often low and noncompliance high, or for living wage laws in U.S. cities, which typically cover only a subset of the workforce.

Extending the model to allow for partial coverage does not change the basic conclusion that employment in the covered sector falls when the minimum wage is set above the competitive wage. However, the uncovered sector may provide alternative opportunities to workers who cannot find jobs in the covered sector and thus can potentially mitigate the overall employment losses associated with the minimum wage. As Brown (1999) notes, the particular implications from these two-sector models depend on assumptions about how jobs in the covered sector are allocated and how the rationing of employment affects labor supply. In some versions (Welch 1976), covered jobs are allocated randomly among all workers, and individuals who do not get these jobs end up working in the uncovered sector if the wage in that sector is higher than their reservation wage. Under this assumption, the minimum wage raises the supply of labor to the uncovered sector, which lowers the wage and increases employment in that sector, thus offsetting some of the job loss in the covered sector. In another version of the model (Gramlich 1976; Mincer 1976), workers are assumed to choose a sector based on the expected wage in each, and those who do not get a job in the covered sector remain unemployed. The expected wage in the covered sector is the product of the minimum wage and the probability of employment, while the expected wage in the uncovered sector is just the equilibrium wage (because everyone who chooses that sector gets a job). Because expected wages equilibrate across the two sectors, with some workers queuing for jobs in the covered sector, overall employment falls by more than in Welch's model; that is, the uncovered sector provides less of an offset to the job losses in the covered sector. Indeed, employment in the uncovered sector could actually decline if the minimum wage induced workers previously employed in the uncovered sector to queue for jobs in the covered sector.

An important implication of these two-sector models is that the predictions from the neoclassical model for the effects of minimum wages are clearer for employment than for unemployment. For example, in Welch's version of the model, individuals who cannot find employment in the covered sector either take a job in the uncovered sector or, if the uncovered wage is too low, drop out of the labor force. Thus, there is no unemployment in that model, regardless of the effects of the minimum wage on employment. In contrast, there is unemployment in the Gramlich and Mincer versions of the model, because individuals who cannot find employment in the covered sector are assumed to queue for jobs in that sector. More generally, whether an increase in the minimum wage leaves individuals unemployed or causes them to drop out of the labor force seems like a relatively minor distinction, and so economists have tended to focus on the effects of the minimum wage on employment rather than on unemployment.

A second useful extension of the neoclassical model is to relax the assumption of a homogenous labor force. In particular, including workers whose productivity leads them to be paid above the minimum wage allows for substitution by employers across different types of labor and alters the predictions from the model. Using a model with two skill groups (and capital) as an example, if skilled and less-skilled labor are substitutes in production, a higher minimum wage will lead to a reduction in the demand for less-skilled labor and an increase in the demand for skilled labor. As a result, the overall employment decline will be smaller than the decline in less-skilled jobs, although total employment will still fall, as long as less-skilled labor is a substitute for capital. On the other hand, in the case where less-skilled labor and capital are complements in production, the rise in skilled labor could be large enough to offset the decline in less-skilled labor.[6]

The lesson from this extension is that the neoclassical model's prediction of a reduction in labor demand applies unambiguously only to less-skilled workers whose wages are directly raised by the minimum wage. The effects on other workers depend on the nature of the production process and, indeed, the minimum wage can generally be expected to lead to an increase in the employment of slightly higher-skilled workers who are good substitutes for minimum wage workers. As a result, the effect of the minimum wage on total employment will generally be considerably smaller than for less-skilled workers, and it could be much smaller—or, in principle, it could even be positive. Consequently, much of the empirical research we discuss later in this chapter attempts to focus on subgroups of the population for which disemployment effects are more likely.

Thus far, we have presented the neoclassical model in the context of an economy with only one industry. If we extend the model to include more than one industry, we also need to consider possible general equilibrium cross-industry effects. For example, if a minimum wage increase pushes up costs for one product (X) more than for another product (Y) that is substitutable for X, then the demand for Y can increase, even though the minimum wage has increased its price. That is, when there are multiple products that are viewed as close substitutes by consumers, the scale effect can operate in the opposite direction for products that are produced with a smaller share (in costs) of minimum wage labor. As a result, the neoclassical model does not make firm predictions about the effects of minimum wage increases on an industry-by-industry basis, and thus a failure by researchers to find a decline in less-skilled employment in a narrow industry should not necessarily be viewed as inconsistent with the theory. This may be relevant for the fast-food studies reviewed later in this chapter, because, as Card and Krueger (1995a) note, labor's cost share in the fast-food industry is not especially high (around 30 percent). For example, if the cost share of minimum wage labor is lower in the fast-food sector than in other restaurants, a minimum wage increase could shift demand *toward* fast food, and hence increase labor demand in that industry.[7] Despite the controversy over studies of this industry, the bottom line that is missed by many people is that studies of a specific industry are probably not especially informative about the validity of the neoclassical model or about the overall effects of a higher minimum wage on less-skilled workers.

The neoclassical model can also be extended to distinguish between short-run and long-run effects of a minimum wage increase. It is sometimes claimed that short-run and long-run adjustments to a change in the minimum wage are quite similar because the rapid turnover of workers in labor markets that employ minimum wage workers renders hiring and firing costs insignificant (e.g., Card and Krueger 1995a). However, as Hamermesh notes, even if there are minimal costs to adjusting labor input, sizable adjustment costs for capital or other inputs can result in lags in the adjustment of labor (1995, 836).

Although there is no explicit definition of "short run" and "long run," for expository purposes and along the lines of Hamermesh's argument, we can take the short-run effects to be the change in employment over a period during which the capital stock is fixed. In this case, the firm cannot substitute capital for less-skilled labor in response to a rise in the minimum wage, and so the decline in less-skilled labor is mitigated until that adjustment can occur. This suggests that empirical studies may need to allow sufficient time for minimum wage effects to show up in the data, even in high-turnover industries.

Thus far, we have not concerned ourselves with the distinction between labor input and employment, but instead have referred interchangeably to the two. However, labor input is actually the product of employment and average hours worked per employee, and some researchers have recently attempted to model the effect of the minimum wage on each. For example, Michl (2000) proposes a neoclassical model in which hours and employment have different effects on effective labor services—perhaps because fatigue or diminished motivation begins to set in for individuals working longer hours—and in which there are fixed costs per worker (e.g., benefits). In this model, an increase in the hourly minimum wage will lead employers to add workers and shorten the average work week, because the marginal cost of an additional hour rises relative to the marginal cost of hiring an additional employee. Michl also notes that in his model, any scale effects associated with a reduction in output affect only the number of workers, so that the implications for employment are ambiguous. Strobl and Walsh (2007) develop a more general model along these lines by allowing firms to differ in the desired intensity of hours and employment in their production functions. In this case, the effects of the minimum wage on employment and average hours, as well on total hours, are ambiguous and may differ significantly across industries or occupations.

3.2.2 Monopsony Models

In the neoclassical model, each individual employer is assumed to take the market wage as given, and all firms can hire as many workers as they need at the existing wage; that is, no firm is large enough to have any direct say in determining the market wage for less-skilled employees. An alternative set of models that have gained favor in recent years assumes instead that individual firms do have some market power over wages. In the standard textbook form of such models, a single firm—called a "monopsonist"—constitutes the employers' side of the labor market, while the labor force consists of homogenous workers, all of whom are paid the same wage. Because this firm is the only employer, it faces an upward-sloping labor supply curve and thus must pay a higher wage to attract more workers.

To maximize profits, the monopsonist sets the wage at the point where the marginal cost of labor equals the marginal revenue product of labor. But because the marginal cost of labor rises more steeply than the wage (because the firm has to pay all its workers the higher wage), this equilibrium wage results in a lower level of employment than in the competitive model. In the context of the minimum wage, Robinson (1933, 295) showed that when a firm has monopsony power in the labor market, a minimum wage can, if not set too high, lead to an *increase* in employment. Intuitively, this occurs because the minimum wage reduces the marginal cost of hiring an additional worker (although it does raise the firm's average cost).[8] In particular, increases in the minimum wage raise employment up to the point at which the wage floor reaches the wage that would obtain in a competitive labor market; beyond that point, any further rise in the minimum begins to reduce employment, as in the neoclassical model.[9]

As noted in chapter 2, Stigler acknowledged this theoretical possibility, but did not view it as "very relevant to the question of a national minimum wage" (1946, 360) because differences in the optimal minimum wage would vary across occupations, firms, and over time, and because the government would need to know the relevant labor supply and demand schedules for each firm in order to set the minimum wage at a level that would increase employment. More generally, the textbook monopsony model is not viewed by most economists as a sensible model of low-wage labor markets, which in most industries and geographic areas consist of numerous small firms.

In an effort to make the monopsony model more applicable to low-wage labor markets, economic theorists have developed more complicated models that lead to monopsony-like implications in markets with a large number of employers. In general, the common thread of these models is a mechanism that results in an upward-sloping relationship between employment and the wage paid by firms and a marginal cost of labor curve that rises more steeply than the labor supply schedule. In some cases, this is accomplished by introducing more complex wage mechanisms. For example, Wessels (1997) develops a model for the restaurant industry in which workers receive both an hourly wage and tips. In this model, tips are shared among workers, and thus the average tip received by a worker is inversely related to the number of workers employed by the restaurant. In order to hire more workers, the restaurant must offset the decline in average tip income by increasing the hourly wage paid to all of its existing employees. This pay structure leads to a gap between the wage and the marginal cost of labor similar to that in the textbook monopsony model, so that an increase in the minimum wage will, over some range, lead to an increase in employment.

In a different vein, Rebitzer and Taylor (1995) posit an “efficiency wage” model, in which firms can pay a higher wage to induce greater effort on the part of employees and threaten to dismiss employees who shirk.[10] However, firms need to monitor that effort, and the effectiveness of monitoring is assumed to be inversely related to establishment size—so that shirking employees in a larger firm are less likely to be caught than they would be in a smaller firm. In such a model, a firm that wishes to raise its level of employment must pay a higher wage to its existing workforce in order to discourage shirking, which again leads to a gap between the wage and the marginal cost of labor. Rebitzer and Taylor show that, under these conditions, imposing a minimum wage slightly above the equilibrium wage raises the cost of dismissal to the worker, so that the firm can devote less resources to monitoring work effort and hire additional workers instead.[11]

Other researchers have shown that monopsony behavior can result from models that assume heterogeneity across firms. For example, Bhaskar and To (1999) propose a model in which there is a large number of firms competing for workers, but where each employer gains some monopsony power over workers as a result of non-pecuniary differences in the jobs offered at different firms. Bhaskar and To cite work schedules, geographic preferences, and job specification as potential sources of such differences, but the general idea in their model is that a firm will need to raise its relative wage to attract additional workers for whom the non-pecuniary aspects of the job are not valued as highly as they are by its existing workforce. This leads to “monopsonistic competition” in the labor market and to the familiar result that the marginal cost of labor curve lies above the labor supply schedule. Consequently, the equilibrium level of employment for the firm is less than what would have occurred in a perfectly competitive labor market, and a small increase in the minimum wage induces firms to hire additional workers. Bhaskar and To emphasize, however, that the higher minimum wage also reduces profits and causes firm exit, and thus the change in employment at the industry level could be either positive or negative beyond the short run.[12]

Another theoretical approach used in recent years has been to induce monopsony-like behavior by incorporating search-related frictions into the labor market. The idea underlying this framework, which was first embodied in a formal equilibrium search model developed by Burdett and Mortensen (1998) and is typically referred to as "dynamic monopsony," is that workers do not have perfect information about the full range of potential job opportunities in the labor market. Instead, in each period they receive a limited number of job offers from which to choose, with the offers drawn randomly from the distribution of wages across firms. As a result, a firm that pays a relatively low wage tends to lose workers over time and finds it difficult to recruit new employees, while a firm that pays a relatively high wage has a lower quit rate and finds it easier to attract new workers. In addition, unemployed workers take a job only if they get a wage offer that exceeds their reservation wage. These search frictions generate a positive relationship between employment and wages at the firm level and an equilibrium level of employment below that found in the competitive model. A minimum wage leads to an increase in the level of employment, both because the minimum wage raises the probability that some workers will receive a wage offer that exceeds their reservation wage and because it induces firms already paying above the minimum to raise their wage offers as well.[13]

Other models of minimum wages based on search-theoretic frameworks have also been proposed in recent years, with differing implications for the effects of minimum wages on employment. For example, Ahn, Arcidiacono, and Wessels (2005) develop an endogenous search model in which the minimum wage raises overall employment by increasing the number of searching workers, but reduces the probability of employment for the lowest-productivity workers. Flinn (2006) presents a matching model of the labor market in which the minimum wage increases the "effective" bargaining power of workers and induces more of them to enter the labor market to search for jobs, thus boosting employment (and perhaps unemployment as well). Finally, van den Berg and Ridder (1998) extend the Burdett-Mortensen model by assuming heterogeneity in firm and worker productivity; under these assumptions, the minimum wage acts as a selection device that forces some firms out of business and leads some individuals in low productivity industries to become permanently unemployed.[14]

3.2.3 Summary

A key lesson from this summary of theoretical models of the effects of minimum wages on employment is that care is needed in interpreting empirical results as evidence for or against alternative models. As we have shown, the neoclassical model does not predict that an increase in the minimum wage will reduce employment in every instance. Moreover, as Manning emphasizes, "the simple result from the model of a single monopsonist, that a suitably chosen minimum wage must raise employment, does not carry over to a labor market in which one models interactions between firms and heterogeneity among them" (2003, 27). As a result, a careful empirical approach is needed to understand how minimum wages affect employment. In the remainder of this chapter we pull together and interpret what we regard as the best and most important evidence on this issue.

3.3 Findings on Employment Effects on Less-Skilled U.S. Workers from the First Wave of the New Minimum Wage Research

The new minimum wage research began with the publication of a new and innovative set of four studies on the employment effects of the minimum wage in a symposium in the October 1992 issue of the *Industrial and Labor Relations Review* (*ILRR*). Even sixteen years later, this symposium provides a good representation of both the range of analyses that have characterized the new minimum wage research and the mix of empirical estimates generated by some of this research. In particular, the studies in the symposium include the use of both state and time-series variation over relatively long sample periods (Neumark and Wascher 1992), the use of regional and state-level variation in employment and wage changes surrounding a particular increase in the federal minimum wage (Card 1992a), an analysis of an increase in a particular state's minimum wage (Card 1992b), and a survey of fast-food restaurants before and after an increase in the minimum wage (Katz and Krueger 1992). The findings from this research range from disemployment effects similar to the earlier consensus (Neumark and Wascher 1992) to no effect on employment (Card 1992a) to a positive effect of the minimum wage on employment (Card 1992b; Katz and Krueger 1992).

We first summarize the findings from these original studies, as well as from a related series of follow-up studies (in section 3.4). Sections 3.5 and 3.6 turn to more recent evidence from the United States and other countries.

Two papers in the *ILRR* symposium extended the traditional empirical specification used in the earlier time-series literature to an analysis of panel data on states, estimating models of the form

$$Y_{it} = \alpha MW_{it} + R_{it}\beta + \varepsilon_{it}. \tag{3.1}$$

In this model, i indexes states and t indexes years, and Y denotes an employment rate. The model includes a minimum wage variable (MW) and a vector of control variables (R) that includes state and year fixed effects. The state effects control for persistent differences across states in the levels of the employment rate and minimum wages, which may both be driven by other factors, and the year effects control for aggregate changes. Either type of influence could generate spurious evidence of minimum wage effects on employment—for example, if minimum wages tended to increase during national recessions when employment rates fell for other reasons. Instead, equation (3.1) identifies the effects of minimum wages from differences in employment changes across states in which the minimum wage rose by different amounts (or not at all)—a difference-in-differences estimator.

The specification of the minimum wage variable differs across studies, as does the set of control variables included in the model and the method of estimation, issues to which we will return shortly. However, so long as changes in the minimum wage are viewed as exogenous to the model, α can be interpreted as the effect of the minimum wage on employment. In the broader literature, this model is typically estimated using data for workers in demographic groups or industries for which the minimum wage is more likely to be binding. In addition, this framework has sometimes been applied to time-series/cross-section or longitudinal data on individuals.

Card's (1992a) study of the employment effects of the April 1990 increase in the federal minimum wage makes use of the fact that differences in the distribution of wages across states (in part due to differences in state minimum wage laws) meant that the effects of the federal increase should be more apparent in low-wage states than in high-wage states. Card shows that average teen wages rose more in states where a greater fraction of teenagers was affected by the minimum wage increase. But when Card regresses the change in state teen employment-to-population ratios on the fraction-affected variable (and a control for aggregate state labor market conditions), he finds no evidence that the 1990 minimum wage increase affected teen employment.[15]

In our paper (Neumark and Wascher 1992), we use equation (3.1) to estimate the effects of changes in the minimum wage on the employment-to-population ratio of teenagers (aged sixteen to nineteen) and the broader youth population (aged sixteen to twenty-four), for an annual panel of state-specific observations from 1973 to 1989 for large states and from 1977 to 1989 for smaller states. We include a coverage-adjusted minimum wage for each state-year observation—the higher of the federal or state minimum wage, multiplied by federal coverage in the state, and divided by the average wage in the state, paralleling the Kaitz index used in the earlier time-series literature. In contrast to Card's paper, our findings generally support the earlier consensus that increases in the minimum wage reduce employment among young workers, with employment elasticities with respect to the minimum wage ranging from −0.1 to −0.2 for teenagers and from −0.15 to −0.2 for youths. We also find that state youth subminimums tended to reduce the impact of the minimum wage, as the standard neoclassical model would predict.

The other two papers in the *ILRR* symposium were case studies of minimum wage increases in particular states: Katz and Krueger's (1992) study of the effects of the 1991 increase in the federal minimum wage on fast-food restaurants in Texas, and Card's (1992b) study of the 1988 increase in California's minimum wage. Studies that limit the analysis to a particular state have often been interpreted as "natural experiments" for studying the effects of minimum wage increases (Card and Krueger 1995a, 20–22). However, we do not view these types of studies as different in principle from the panel data studies discussed previously. The experimental design is the same, and they differ mainly in the construction of the control group, which seems no more compelling than those used in the state-level panel analyses. Nonetheless, this strand of the literature has received considerable attention both within the economics profession and in the public discussion about the merits of raising the minimum wage, and it has pushed researchers to think carefully about how to construct appropriate control groups in their studies of the effects of minimum wages.

Katz and Krueger (1992) study the effects of the 1991 increase in the federal minimum wage on employment in the fast-food industry in Texas, based on telephone surveys of managers or assistant managers at fast-food establishments conducted in December 1990 and July/August 1991. Katz and Krueger define the effective change in the minimum wage at each restaurant as the log difference between the firm's average starting wage in December 1990 and the new federal minimum wage of $4.25 per hour in April 1991, so that the difference in employment changes between restaurants initially paying relatively lower wages and those initially paying relatively higher wages identifies the effect of the minimum wage on employment. Their regression results indicate a large positive and statistically significant effect of the minimum wage on employment, with the elasticities ranging from 1.70 to 2.65. Katz and Krueger conclude that their findings are inconsistent with the competitive model, but consistent with "a model in which the employers of low-wage workers are assumed to have market power and act as monopsonistic buyers of labor" (17). On the other hand, they also note that a monopsony story is "somewhat implausible in the high-turnover labor market of the fast-food industry" (18).

Card (1992b) uses data from the Current Population Survey (CPS) to assess the effects on low-skilled employment of California's increase in the minimum wage to $4.25 per hour in July 1988. He compares the change in teen employment from 1987 to 1989 in California to the change in the control areas over the same period and finds that teen employment increased more rapidly in California, despite the minimum wage increase.[16] The difference was statistically significant, with an implied elasticity of about 0.35. Card also finds a relative increase in employment in retail trade in California between 1987 and 1989, and a small relative decline in the eating and drinking industry in California; however, he interprets the latter result as more likely stemming from differences in longer-run trends than the effect of the minimum wage increase. As did Katz and Krueger, Card concludes that the teen employment results are inconsistent with the competitive model and might indicate the presence of monopsony power in the low-wage labor market.

Subsequent to the *ILRR* symposium, the best known and most influential case study of a specific minimum wage increase is Card and Krueger's (1994) investigation of the effects of the 1992 increase in New Jersey's minimum wage. Following the methodology used in the Katz and Krueger study, Card and Krueger survey fast-food restaurants in February 1992, roughly two months before the April 1992 increase in the New Jersey minimum wage to $5.05 per hour, and then again in November of that year, about seven months after the increase. For New Jersey stores, they construct a wage gap variable equivalent to that used by Katz and Krueger in their study of fast-food restaurants in Texas. But they also attempt to improve on the identification in this earlier research by including in the sample a control group of restaurants in eastern Pennsylvania, where the minimum wage did not change.

Their results consistently imply that the increase in New Jersey's minimum wage raised employment (as measured by full-time equivalents, or FTEs) in that state. For example, stores that initially paid low starting wages showed significantly more employment growth between February and November than did stores that paid higher starting wages. Similarly, employment in the New Jersey sample rose over this period, while employment in the Pennsylvania sample fell. Putting this information together, Card and Krueger construct a wage gap measure equal to the difference between the initial starting wage and $5.05 for stores in New Jersey, and zero for stores in Pennsylvania (and stores in New Jersey with an initial starting wage exceeding $5.05). Again, the results show a positive and statistically significant effect of the minimum wage increase on employment, with an estimated elasticity of about 0.7. They interpret their empirical results as "inconsistent with the predictions of a conventional competitive model of the fast-food industry" (1994, 790).[17]

3.4 Issues Raised in Subsequent Research

Much ensuing empirical literature has been aimed at understanding and reconciling the divergent results reported in the first wave of the new minimum wage research. In particular, this literature has focused on issues related to the measurement of variables, the specification of the underlying model, and the adequacy of the comparison groups used in the studies.

3.4.1 The Minimum Wage Variable

In their comment on our 1992 paper, Card, Katz, and Krueger (1994) criticize our use of a Kaitz-style relative minimum wage variable. They assert that if the minimum wage variable is intended to measure the relative price of teen labor, it should be positively correlated with teen wages. They then show that in contrast to this expectation, the actual correlation was negative, because the denominator of the index (the average wage of adults in the state) was positively correlated with teen wages.

However, the premise of their argument is incorrect. An appropriate minimum wage variable should be positively correlated with changes in the *relative* teen wage, and as we show in our reply (Neumark and Wascher 1994), the minimum wage variable we used meets this criterion and thus correctly captures increases in the nominal minimum wage as increases in the relative price of teen labor. Moreover, in the absence of an increase in the nominal minimum wage, the negative correlation described by Card, Katz, and Krueger correctly captures the relative decline in the price of minimum wage labor associated with an increase in the general wage level. Measuring such changes in the price of minimum wage workers relative to higher-wage, higher-skilled workers is important, because the substitution by employers away from lower-skilled minimum wage workers toward higher-skilled, higher-wage workers is likely the principal source of disemployment effects of minimum wages.[18]

Nonetheless, the relative minimum wage variable does constrain nominal increases in the minimum wage and increases in the adult wage to affect employment in opposite directions, and it is valid to ask whether this constraint is appropriate.[19] In our data, this constraint was not rejected (Neumark and Wascher 1994). However, as Card and Krueger (1995a) point out, a theoretical specification of the labor demand function for youths would include prices of all factor inputs (including the adult wage) as well as the price of output. And, in this context, implementing the standard homogeneity assumption would lead to a model that includes both the relative minimum wage and the real adult wage (and perhaps the real interest rate or relative energy costs as well).

In subsequent research, the constraint has been relaxed in a variety of ways (Burkhauser, Couch, and Wittenburg 2000a, 2000b; Keil, Robertson, and Symons 2001). As it turns out, however, the results in these later studies, which generally use longer sample periods as well, do not appear very sensitive to the alternative specifications. In addition, a couple of studies have reduced the parameterization of minimum wage effects even further by freely estimating the effects of each observed change in the minimum wage, whether federal (Deere, Murphy, and Welch 1995) or state (Burkhauser, Couch, and Wittenburg 2000a).

Both of these studies find that increases in the minimum wage significantly reduced teenage employment rates, although the minimum wage effects in these specifications are more difficult to separate from other influences that vary over time.

3.4.2 Lagged Effects of the Minimum Wage

The new minimum wage literature also raises questions about how long it should take for minimum wages to have their full effect on employment. Brown, Gilroy, and Kohen argue that "lagged adjustments to minimum wage increases are probably less plausible than in most other contexts where such lags are routinely assumed" (1982, 496), because the higher turnover rates of minimum wage workers imply that desired adjustments in employment can be accomplished quickly, and because minimum wage increases are typically announced several months in advance of becoming effective. Card and Krueger also argue that the industries that typically employ minimum wage workers can "easily vary their staffing levels by cutting back on off-peak or store hours, and by allowing longer queues" (1995a, 67), so that any disemployment effects should be evident shortly after the minimum wage is raised. As we noted previously, however, Hamermesh (1995) points out that although factors such as hiring, firing, or training costs may be less important for workers with normally high quit rates, firms may adjust nonlabor inputs (e.g., capital) slowly, which will tend to slow the adjustment of other inputs, including labor, as well. Thus, the omission of lagged effects may inappropriately exclude longer-run substitution between labor and capital, as well as scale effects, which also may evolve more slowly.

As a result, the question of how quickly minimum wages affect employment is an empirical one. And the potential for lagged effects did arise as a significant issue in interpreting the results from the papers in the *ILRR* symposium. We found statistically significant employment effects from lagged values of the minimum wage with our time-series panel of state-level data, and we also argued that one reason for the discrepancy between our results and those reported in Card's study of the 1991 federal increase was his omission of lagged effects. As evidence, we show that using our sample, a one-year first-difference estimator equivalent to that used by Card produces minimum wage effects close to zero, similar to what was reported in his paper. But adding a lagged minimum wage effect to the model results in a negative and statistically significant employment effect in both the levels and first-differenced versions of the basic model. We also show that inappropriately omitting lagged effects would generate biases leading to precisely this result.[20]

A more compelling analysis of this issue is contained in a paper by Baker, Benjamin, and Stanger (1999), who study the dynamics of minimum wage effects on employment in Canada. They first replicate some of the U.S. panel data estimates for teenagers with Canadian data, reporting that one-year first-difference estimates of minimum wage effects in Canada are positive, whereas longer differences and specifications with lags of the minimum wage tend to show negative employment effects that are statistically significant—similar to what we found for the United States. For example, in their preferred specification, the first-difference elasticity is 0.07, while the within-group elasticity is −0.27. With lagged minimum wages, the estimates are more similar (−0.23 and −0.47, respectively).[21]

Baker, Benjamin, and Stanger then show that the alternative differencing operators, as well as the inclusion of lagged minimum wages, can be interpreted as applying different filters to the data, with the longer differences or inclusion of lags corresponding to filters that emphasize the low-frequency variation in the data. Building on this analysis, they show that filtering the variables to separate their high-frequency and low-frequency movements, and estimating the minimum wage elasticities separately at high and low frequencies, yields a positive effect of the minimum wage on employment at high frequencies and a negative effect of the minimum wage on employment at low frequencies. Overall, the authors report an employment elasticity of −0.25 in Canada, noting that "this result is driven by low-frequency variation in the data" (1999, 345). In addition, although they do not analyze U.S. data directly, they use the U.S. literature to demonstrate—through equations that relate estimated coefficients for alternative estimators to the implied elasticities at different frequencies—that their analysis can explain the different findings for the United States. More broadly, the results in this paper indicate that the disemployment effects of minimum wages tend to show up as longer-run responses to more evolutionary changes in the level of the minimum wage, rather than as a short-term response to a particular change in the minimum wage.

Subsequent research has tended to confirm evidence of adverse longer-run effects of minimum wages on employment. For example, Keil, Robertson, and Symons (2001) allow for lagged minimum wage effects by estimating a dynamic version of the employment equation that includes a lag of the dependent variable rather than by entering a lagged minimum wage term directly. In their preferred specification, they find a short-run employment elasticity of −0.37 for youths and a long-run elasticity of −0.69. Related evidence from studies that include lagged minimum wage variables is reported in Burkhauser, Couch, and Wittenburg 2000a and Partridge and Partridge 1999, as well as in many other studies.

In our view, the research on this issue suggests that studies claiming to find no minimum wage effect on employment should be discounted unless the evidence points to no effects in both the short run and the longer run. Indeed, this issue turns out to figure prominently in our assessment of the research literature, as the studies that fail to detect disemployment effects typically do not allow for a longer-run impact (although there are also studies that do not include lagged effects and still find disemployment effects).

3.4.3 Employment and School Enrollment

The subsequent exchanges over the papers in the *ILRR* symposium also highlighted the role of school enrollment in studies of the employment effects of minimum wages. Card, Katz, and Krueger (1994) criticize both the specific measure of school enrollment that we used, as well as the inclusion of the school enrollment control in the employment model in our 1992 paper. From a measurement perspective, they point out that our school enrollment variable included only individuals who were enrolled in school and not employed, which they argue would lead to a negative bias in our estimated employment effects. More broadly, they argue that it was inappropriate to include school enrollment in the employment equation because that equation is essentially a labor demand function. These criticisms are significant because a statistically significant disemployment effect for teenagers was evident only in our specifications that included the school enrollment rate.[22]

The definition of schooling used in our original paper was indeed too narrow. However, substituting broader measures of enrollment that do not exclude employed teenagers led to only minor differences in the results. For example, when we reestimated the model using an alternative measure of the enrollment rate that counts individuals as enrolled if they report schooling as their major activity (Neumark and

Wascher 1994), the resulting employment elasticity for teenagers was −0.11. Using an even broader definition of enrollment that is calculated independently of employment, we found a statistically significant employment elasticity of −0.22 (Neumark and Wascher 1996a).[23]

Regarding the second criticism, the aggregate employment equation is designed to estimate an average effect using both observations for which the minimum wage is binding and observations for which it is not binding. Although employment for the first group is determined solely by the labor demand curve in the standard competitive model, employment for the second group is influenced by both demand and supply factors. As a result, the specification of a model for the employment of all teenagers should also include variables that capture exogenous shifts in the labor supply curve, including exogenous changes in the school enrollment rate.

Nonetheless, because the decision to enroll in school may not be independent of the decision to work, the estimates from a version of equation (3.1) that includes the enrollment rate may be subject to endogeneity bias. To address this question, we computed instrumental variable (IV) estimates of the equation, using school expenditures, student-teacher ratios, and compulsory schooling laws as instruments (Neumark and Wascher 1994). The estimated employment elasticities for teenagers ranged from −0.17 to −0.39. Thus, the IV estimates support the view that minimum wages reduce employment among teenagers.

We have also explored more fully the effects of minimum wages on employment and school enrollment. Because this research addresses the effects of minimum wages on schooling, it is discussed more fully in chapter 6. Briefly, though, the evidence indicates that the disemployment effects of the minimum wage fall largely on the least-skilled teenagers, who find it more difficult to find a job following an increase in the wage floor. At the same time, a higher minimum wage induces some labor-labor substitution, with the increased demand for more-skilled teenagers inducing some teenagers to leave school for employment, displacing those teenagers already out of school and working, and raising the share of teenagers who are idle (neither employed nor enrolled in school). Thus, employment equations that do not condition on enrollment pick up the *net* employment effect on teenagers, which can be quite small, whereas those that condition on enrollment or otherwise take account of changes in both employment and enrollment detect the larger *gross* employment effects.[24]

3.4.4 Aggregate Effects and Trend Differences in the State-Level Panel Data Approach

Researchers have also raised several other concerns about model specification in state-level panel data analyses in reaction to the first round of the new minimum wage research. For example, Deere, Murphy, and Welch (1995) highlight the possibility that differences in underlying trends in employment growth across states could bias estimated employment effects in short state-year panels. In the case of the federal increase studied by Card (1992a), they note that the low-wage states where a large fraction of teenagers was affected by the minimum wage increase also tended to be states where trend employment growth was faster, creating a positive bias in the estimated employment effect. In particular, they show that rates of employment growth for well-educated adult men were also higher in low-wage states than in high-wage states between 1989 and 1992, and that controlling for the 1985–1992 trend in employment and for business cycle developments results in statistically significant negative estimates of the effects of the 1990 and 1991 minimum wage increases on the employment rates of teenagers and high-school dropouts, with relatively large implied elasticities of −0.27 to −0.36 for teenage males and from −0.42 to −0.49 for teenage females.

Meanwhile, Burkhauser, Couch, and Wittenburg note that the inclusion of year effects in panel data analyses effectively eliminates the identification associated with variation in the federal minimum wage, so that "the minimum wage effects can be identified only by using the relatively small number of observations in which the state minimum wage is higher than the federal minimum wage" (2000a, 655). They then show that equations estimated without year effects produce negative and statistically significant coefficients on the minimum wage variable across a variety of specifications, with elasticities in the range of −0.3 to −0.35, while specifications that include year effects produce small and insignificant coefficients. Their paper raises the question of how to balance the loss of identification associated with including year effects with the potential bias caused by omitting macroeconomic or other aggregate influences that would be captured by the year dummies. The authors attempt to solve the problem by omitting year effects and including instead the unemployment rate and the average adult wage, as well as dummy variables corresponding to recessions.

Our preference is to include year effects, for several reasons. First, including a relative minimum wage measure (in levels, but not logs) will permit some identification from variation in the federal minimum wage, because of differential movements in state average wages. Second, it is impractical to include (or even specify) all of the potentially relevant macroeconomic or other aggregate-level variables—including those affecting only the narrow age or skill groups that are often studied. And third, the diminution of federal variation in the minimum wage has become less of a problem over time, because samples that include more recent data have significantly more variation in state minimum wages than did the samples that ended in the early or mid-1990s. Along these lines, when Sabia (2006a) reestimates the Burkhauser, Couch, and Wittenburg (2000a) specifications using data through 2004, he finds a negative and statistically significant effect of the minimum wage on employment regardless of whether year effects are included in the regressions, with estimated elasticities of −0.18 without year dummies and around −0.3 when year dummies are included.

Related concerns have been raised more recently by other researchers. In particular, Dube, Lester, and Reich (2007) also address the issue of how to account for underlying trends in employment that may be spuriously associated with minimum wage changes, by implementing a research design that incorporates period effects in a more flexible way. Their preferred specifications, which are estimated on restaurant employment (as well as on earnings per worker in the industry) for either differences between counties in metropolitan areas that cross state lines, or differences across states between pairs of counties that are contiguous across state borders, include interactions between period dummy variables and dummy variables for the metropolitan area or the county pairs. These interactions introduce arbitrary time effects for subsets of counties that are arguably in the same labor market but exposed to different minimum wages.

Dube, Lester, and Reich argue that allowing for labor-market specific period effects has a large impact on estimated minimum wage employment effects. When they estimate a baseline model that includes the minimum wage along with period (quarter) and county fixed effects, they find a significant negative employment elasticity. In contrast, their specifications that include the interactions between the period dummies and the metropolitan area or county pair dummy variables yield estimates near zero.[25] Furthermore, simply adding interactions between census division (of which there are nine) dummy variables and the period dummy variables also reduces the effect to zero. Finally, they show that a so-called placebo specification, in which the authors restrict the sample to counties in which the state minimum wage was never above the federal level in the sample period, but assign to these counties the average minimum wage for their census division, yields spurious estimated employment effects similar to those in the baseline model.

It appears to us, however, that the importance that Dube, Lester, and Reich ascribe to area-specific trends is significantly overstated. They use the log of the employment level in the restaurant industry as the dependent variable in their specification and do not include any controls for population or overall employment. As the authors point out, the census divisions with the most rapid growth in *total* employment were the regions where fewer state minimum wages above the federal level were imposed (the three southern regions, and the Mountain region). But these divisions were also among the fastest growing in terms of population; for example, over the last decade they constituted four of the five fastest-growing divisions.[26] Because the models do not control for population growth and population growth is correlated with the changes in the minimum wage, it is not at all surprising that the results from their baseline model are contaminated by important area-specific trends. The same phenomenon can explain the authors' placebo results: the census divisions with the smallest minimum wage increases had higher population growth and therefore also higher employment growth—likely accounting for the apparent negative effect found in the authors' regressions.

Nevertheless, the Deere, Murphy, and Welch (1995) and Dube, Lester, and Reich (2007) studies highlight the potential importance of accounting for underlying trends that may bias the estimated effects of minimum wages. Of course, many other studies have done this in one of two ways (and sometimes both): (1) by using an employment-to-population ratio as the dependent variable; and (2) by controlling either for overall employment levels or for employment levels (or other outcomes) for groups not affected by minimum wages (e.g., Neumark 2001; Neumark, Schweitzer, and Wascher 2004). An alternative way that researchers often account for area-specific effects or trends is to compare results for those who *should* be affected by a minimum wage to results for those who should *not* be affected, on the assumption that the latter group serves to capture labor market-specific effects attributable to sources other than the minimum wage. This approach has been used in research on nearly all types of effects of minimum wages.[27]

A related issue discussed by Dube, Lester, and Reich (2007) is the importance of accounting for the non-independence of the error terms among observations in the same geographic area (most often states, in the existing research on minimum wages).[28] Dube, Lester, and Reich assert that because this adjustment was not made in many of the earlier minimum wage panel studies, the standard errors reported in those studies are biased downward by a large amount (and hence the precision of the estimates is strongly overstated). This claim appears to be based on two pieces of evidence. First, for their baseline estimates, they show that the standard errors computed from a procedure that does not account for non-independence of observations on the same geographic area are understated by a factor of between 5 and 12 (29).[29] Second, they note that in our recent study (Neumark and Wascher 2007b), we report standard errors clustering on state (the same procedure they use), and we find many insignificant employment effects; this leads Dube, Lester, and Reich to conclude that many of our previous findings "would not pass tests of significance using clustered standard errors" (2007, footnote 12).

However, this conclusion does not follow from their analysis. Although clustering the standard errors is needed to account for possible non-independence, doing so does not always result in much larger standard errors. Indeed, when we recalculate the standard errors with and without clustering for some of the key estimates in our recent study (Neumark and Wascher 2007b), we find that clustering sometimes increases the standard errors and sometimes lowers them. However, they rarely differ much, and never by more than a factor of 2. Instead, the prevalence of insignificant effects in our recent paper likely reflects the much shorter sample period used in that study.

In addition, we suspect that the large impact of clustering in their own estimates reflects their use of an underspecified model. In particular, they exclude state-specific controls, such as unemployment rates or demographic shares of young workers, which are included in most minimum wage employment studies, and the absence of these controls likely induces a positive correlation in the residual terms for counties in the same state, implying that standard errors computed without clustering are understated. And more important, we suspect, is that the absence of controls for population or overall employment in their baseline specifications probably contributed to strong positive serial correlation in the residuals because of variation across areas in trend population growth.

Although the tendency for researchers to calculate and report standard errors robust to non-independence of error terms clearly represents an improvement over the previous literature, we see little basis for concluding that the earlier specifications consistently led to incorrect inferences that minimum wages had significant effects because of downward-biased standard errors. This may well have occurred in some studies, and may in fact be the more prevalent outcome for studies that did not estimate richer specifications designed to capture some of the non-independence.[30] But we do not think it is appropriate to make stronger statements about the existing body of research as a whole. In addition, most studies conducted since the results from Bertrand, Duflo, and Mullainathan (2004) were circulated report standard errors robust to this non-independence (e.g., Burkhauser, Couch, and Wittenburg 2000a; Sabia 2006a; Neumark and Wascher 2007a), and these studies tend to find significant effects of minimum wages.

3.4.5 Reactions to the State Case Studies

The case studies that constitute the other strand of the new minimum wage literature soon generated considerable controversy. Some labor economists embraced the studies as praiseworthy examples of the usefulness of the natural experiment approach to studying the economic effects of policy changes (e.g., Freeman 1995). Others, however, were more critical of these studies. For example, referring to the descriptions of these studies in *Myth and Measurement*, Welch writes: "I am convinced that the book's long-run impact will instead be to spur, by negative example, a much-needed consideration of standards we should institute for the collection, analysis, and release of primary data" (1995, 842). Likewise, Hamermesh concludes that "even on its own grounds, CK's [Card and Krueger's] strongest evidence is fatally flawed" (1995, 838).

The criticisms of the case study approach focus on three main issues. The first concerns the adequacy of the control groups used in the studies. On its face, for example, it seems reasonable to question the use of Georgia, Florida, and Dallas/Ft. Worth as appropriate control groups in Card's (1992b) study of the California minimum wage increase, given that these places are far from California and likely influenced by very different demand conditions. But even for states in close geographic proximity, using one state as a control for analyzing a policy change in another state can sometimes be problematic. For example, Deere, Murphy, and Welch (1995) point out that teenage employment rates in New Jersey diverged significantly from those in Pennsylvania beginning in 1988, casting doubt on Card and Krueger's claim that restaurants in Pennsylvania provided a sensible control group with which to compare restaurants in New Jersey. More broadly, Hamermesh notes that the variance in employment seems to be dominated by demand shocks, which suggests that "any changes in the relative demand shocks" affecting two geographic areas will easily "swamp the effect of a higher minimum wage" (1995, 837). In our view, this issue highlights a potential advantage of a larger panel with many minimum wage increases, over which these demand shocks would be much more likely to be, on average, uncorrelated with minimum wage changes.

A second criticism concerns the timing of the surveys used in the case study analyses. In each of the fast-food case studies, the post-treatment observation comes less than a year after the relevant minimum wage increase. As we noted earlier, however, there is substantial empirical evidence that the disemployment effects of an increase in the minimum wage may occur with a lag of one year or more. For the same reason, both Brown (1995) and Freeman (1995) suggest that these studies are more appropriate for examining the short-run effects of minimum wage changes than for estimating their long-run effects.

A third concern involves questions about the reliability of the data used in these case studies. In each study, the researchers conducted their own telephone surveys of fast-food restaurants, which were not subject to the same rigorous standards as those employed in developing the surveys used in government statistical programs. Welch (1995) expresses significant doubts about the quality of the data, noting in particular some puzzling features of the sample collected for the analysis in Card and Krueger (1994). In Neumark and Wascher (2000), we document what seems to us to be an unusually high degree of volatility in the employment changes measured with Card and Krueger's survey data.

In light of these concerns, a number of researchers subsequently reexamined the results reported in the initial round of state-specific case studies. For example, Kim and Taylor (1995) revisit Card's study of the effects of California's 1988 minimum wage increase on employment in the low-wage retail sector. Using data for the retail trade sector as a whole, Kim and Taylor first replicate Card's finding that employment growth in California around the time of the minimum wage increase was not statistically different from retail employment growth for the United States as a whole. However, they also point out that the volume of retail sales in California rose much more rapidly during that period than in the United States, which raises questions about the validity of this experiment. Kim and Taylor then turn to more-detailed industry data within the retail sector and examine whether differences across industries in wage growth in California relative to the United States as a whole were negatively correlated with differences across industries in California versus U.S. employment growth in various years. The results show a negative and statistically significant correlation for the changes from March 1988 to March 1989, the period that included the minimum wage increase, but not for the changes in earlier years; they interpret this result as consistent with a negative employment effect of the minimum wage. A similar result emerges from their analysis of county-level employment growth and wage growth. The implied minimum wage elasticities that they calculate from their estimates range from -0.15 to -0.2.[31]

In Neumark and Wascher 2000, we revisit Card and Krueger's analysis of New Jersey's minimum wage increase, paying particular attention to data quality issues. In particular, we collected administrative payroll records on hours worked from 235 fast-food establishments that were in the universe from which Card and Krueger drew their sample, and compare the two data sources. The Card-Krueger data were elicited from a survey that asked managers or assistant managers "How many full-time and part-time workers are employed in your restaurant, excluding managers and assistant managers?" This question is highly ambiguous, as it could refer to the current shift, the day, or perhaps the payroll period, and the respondents' interpretation of it could differ in the observations covering the periods before and after the minimum wage increase. In contrast, the payroll data referred unambiguously to the payroll period used by the restaurant. Reflecting this difference, the data collected by Card and Krueger had much greater variability across the two observations than did the payroll data, with changes that were sometimes implausible.[32]

We then replicate Card and Krueger's difference-in-differences test after replacing their survey-based data with observations taken from the payroll records. In contrast to Card and Krueger's results, the results from our replication indicate that the minimum wage increase in New Jersey led to a decline in employment (FTEs) in the New Jersey sample of restaurants relative to the Pennsylvania sample. The elasticities from our direct replication analysis were a little larger than -0.2, while additional sensitivity analyses suggested a range of elasticities from −0.1 to −0.25, with many (but not all) of the estimates statistically significant at conventional levels.

In their reply, Card and Krueger (2000) present several additional analyses of the effects of New Jersey's minimum wage increase using both their original data and our payroll records. In addition, they report results from a separate longitudinal sample of fast-food restaurants obtained from BLS records. In contrast both to their original study and to our replication, their reanalysis generally finds small and statistically insignificant effects of the increase in New Jersey's minimum wage on employment, and they conclude that "the increase in New Jersey's minimum wage probably had no effect on total employment in New Jersey's fast-food industry, and possibly had a small positive effect" (1419). Of course, had this been the conclusion from Card and Krueger's original analysis, there would have been much less scope for casting doubt on the standard competitive model of labor markets.

A more recent case study is contained in a paper by Powers, Baiman, and Persky (2007), who revisit the question of the effects of the minimum wage on employment and hours in the fast-food industry based on an increase in the minimum wage in Illinois. Their research design parallels very closely the original design of the Card and Krueger 1994 study, using survey data to examine employment changes in counties along the Illinois-Indiana border between the fall of 2003 and the fall of 2005, when the Indiana minimum wage was unchanged and the Illinois minimum wage rose from $5.15 (the federal minimum) to $6.50 in two steps.

As in Card and Krueger, Powers, Baiman, and Persky (2007) use two estimators: (1) a simple difference-in-differences comparison of employment changes in Illinois to those in Indiana, and (2) a regression of employment changes on the wage gap between the average starting wage before the minimum wage increase and the new minimum wage. As dependent variables, they look at the change in FTE employees (weighting part-time workers at 0.5), changes in the numbers of full-time and part-time employees separately, and the change in weekly hours. In all cases, they examine both absolute and percentage changes.

One significant improvement in this study is the use of a more precise employment question that asks "How many people . . . were on your restaurant's payroll during the last pay period?" The responses to this question should correspond much more closely to the type of information we collected from payroll records. Unfortunately, however, Powers, Baiman, and Persky do not provide any information on the distribution of employment changes to confirm that they obtained far fewer of the implausibly large employment changes that we documented in Card and Krueger's data (Neumark and Wascher, 2000).

For the entire 2003–2005 period, the state difference-in-differences specifications for FTE employment yield an estimate of zero for the absolute changes, and a negative but insignificant (and imprecise) estimate for the relative changes, with an implied elasticity in the latter case of −0.14. For the gap specification, they also obtain negative but insignificant estimates. When they estimate specifications for part-time and full-time employees separately, the evidence for part-time employment points to negative effects, while the evidence for full-time employment points to positive (and generally much smaller) effects. However, the only significant estimate is a negative effect for the absolute change in part-time employment in the state difference-in-differences specification. When they break the sample into 2003–2004 and 2004–2005 changes, they find significant negative effects on FTE employment from the second minimum wage change using both the difference-in-differences and gap specifications, but little evidence of any effect from the first minimum wage change. For the 2004–2005 period, the estimated elasticity for the difference-in-differences specification is −0.75, which is very large. The stronger evidence of negative effects for the latter change could reflect the fact that the minimum wage only rose by 35 cents in January 2004, while it rose by $1.00 in January 2005, as well as the possibility that the 2004–2005 change includes some lagged effects from the first increase.[33]

Powers, Baiman, and Persky conclude from this research: "While we can reasonably conclude that the Illinois-Indiana comparison shows no positive response to minimum wages (the most striking claim of the original Card-Krueger study), we cannot yet confidently assert that the overall response is negative (the conventional hypothesis)" (2007, 26). This statement closely parallels our conclusion based on our reevaluation of the Card and Krueger study.[34]

Regardless of what the case studies of the fast-food industry show, we think that their importance is overstated. For one thing, there is no reason to expect the predicted negative employment effect to show up in studies of a particular state minimum wage increase—especially in light of Hamermesh's point about the importance of relative demand shocks in generating fluctuations in employment. In addition, as we discussed in section 3.2, the interpretation of evidence from case studies of a specific industry is unclear, given that the neoclassical model does not predict that employment in a particular sub-sector of the economy will decline in response to a general increase in the minimum wage. For example, it is possible that fast-food restaurant chains are less intensive in low-wage labor than are their competitors, in which case the effect of the higher wage floor on prices at the low-wage-intensive establishments could induce greater consumer demand for fast-food output and an increase in fast-food employment. As a consequence, the absence of an employment decline for a narrow industry should not be viewed as a contradiction of that model.

Finally, two recent studies attempt, in one way or another, to explore some of the explanations of the differences in results between the industry-specific case studies and the panel data analysis of broader affected groups. Dube, Lester, and Reich attempt to broaden the analysis by studying the restaurant industry as a whole, rather than the fast-food sector in isolation, and by computing difference-in-differences estimates for a large number of minimum wage increases over many geographic areas. Their preferred specifications yield estimated minimum wage effects that are near zero. The authors suggest that because they look at the entire restaurant industry, for which substitution in consumption between the output of subsectors of the industry is not problematic, their study can help to reconcile the findings of the fast-food studies and the state-level panel data studies of groups of low-skill workers (2007, 3 and 39).

In general, it seems preferable to estimate minimum wage effects from a large set of increases over many regions, in order to avoid the undue influence of idiosyncratic shocks that may plague a case study of a single minimum wage increase in an isolated region; and certainly, in that sense, their study is more like the state-level panel analyses. However, their focus on the restaurant industry is complicated by two factors. First, tip credits—which are important for non-fast-food restaurants—vary across states, making measurement of the effective minimum wage complicated.[35] Second, as discussed earlier with reference to the paper by Wessels (1997), monopsony-like effects can arise in an industry with tipped workers, raising questions about whether these results can be generalized to other industries (aside from other industry differences). As a result, the implications of these results for more aggregate state-level panel data studies are unclear.[36]

Hoffman and Trace (2007) attempt to bridge the gap between the state-level panel data analyses and the fast-food case studies in a different way. They focus on teenagers and other low-skill groups, as in the state-level panel data analyses, but they restrict their attention to New Jersey and Pennsylvania in the 1991–1993 period surrounding the minimum wage increase in New Jersey that Card and Krueger (1994) studied. In addition, they examine a "reverse" experiment for these two states for the 1995–1998 period, when Pennsylvania's minimum wage went up faster than New Jersey's in 1996 and 1997 because of the federal minimum wage increases in those years (coupled with New Jersey's higher minimum wage prior to the federal increases).

Their results are mixed. For the 1991–1993 period, they find a negative but insignificant effect on the employment of teenagers, but a positive and significant effect for non-teenage dropouts. In contrast, in the 1995–1998 period, they find that employment of sixteen- to nineteen-year-olds and sixteen- to twenty-four-year-olds and of non-teenage high-school dropouts declined in Pennsylvania relative to New Jersey, with the results especially strong in a triple difference estimate that compares employment changes for these groups with changes in the employment of thirty- to forty-nine-year-olds. They also examine changes in the shares of teenagers in the restaurant industry in each state and find weak evidence suggesting that minimum wages reduced this share in the 1995–1998 period, but evidence of a positive effect in the 1991–1993 period. Thus, this study also does not successfully reconcile the fast-food case studies and state-level panel data analyses, although it does help to emphasize the fragility of results from studies that estimate minimum wage effects from the impact of an isolated minimum wage increase in a single pair of nearby regions.

3.4.6 Hours versus Employment Effects

The predictions of the various theoretical models of the minimum wage typically refer to labor input rather than to employment specifically, and some authors have suggested that one potential reason for a small (or even positive) employment effect is that employers can also adjust hours worked by their employees. For example, in the case of the New Jersey study, Card and Krueger measured employment, whereas we measured payroll hours, a difference that could potentially explain the different results. Because Card and Krueger's data showed a shift toward full-time workers in response to New Jersey's minimum wage increase, it seems unlikely that the rise in employment they found was accompanied by a decline in average hours. On the other hand, for a subset of fifty-two restaurants for which we (unintentionally) received data on both hours and employment, the payroll data showed a positive effect on employment and a negative effect on total hours, although given the small number of restaurants reporting both employment and hours, we cautioned against making much out of the estimates from the subsample. Regardless, it seems to us that the effect of minimum wages on total hours is the most relevant statistic for testing the validity of the competitive model of labor demand, although perhaps not necessarily the most important statistic from a policy perspective.

The more general question of the effect of the minimum wage on hours of teenagers has been examined in longer sample periods for the United States, using the state-level panel data framework (Zavodny 2000; Couch and Wittenburg 2001). In a model with state and year fixed effects, and using data from 1979 through 1993, Zavodny finds a negative and significant elasticity for employment (−0.12) using a relative minimum wage measure, but a smaller negative and insignificant effect using the real minimum wage. However, the evidence does not point to reductions in average hours per worker, and the elasticity for total hours worked (unconditional on employment) for all teenagers is 0.24 using the real minimum wage and −0.11 using the relative minimum wage.

In sharp contrast, Couch and Wittenburg (2001), using the specifications suggested by Burkhauser, Couch, and Wittenburg (2000a) that exclude year effects, estimate total hours elasticities ranging from −0.48 to −0.77, which are 25 percent to 30 percent larger than those estimated from identical specifications for employment. The authors interpret these results as suggesting that employers respond to a minimum wage increase by reducing both teen employment and average hours of those teenagers who remain employed. As we cautioned earlier, we are reluctant to place too much weight on estimates from specifications that exclude year effects. Nevertheless, the differences in the results reported by Zavodny and by Couch and Wittenburg indicate that the question of how employers adjust average hours in response to a minimum wage increase is not yet resolved.

3.5 An Overview of More Recent U.S. Evidence on Employment Effects

In this section, we selectively review the more recent literature for the United States. This literature is less explicitly focused on issues that arose out of the first round of the new minimum wage research and more focused on providing new evidence on the employment effects of the minimum wage using new data or alternative empirical methods.

3.5.1 Revisiting Aggregate Time-Series Estimates of the Effects of the Federal Minimum Wage

Although the new minimum wage research has generally shifted away from aggregate time-series studies of the effects of the federal minimum wage, there is a small body of research over the past fifteen years that updates and reassesses the time-series evidence. This segment of the literature has its genesis with Wellington (1991) and Card and Krueger (1995a), who reported that extending the time-series data beyond the period covered by Brown, Gilroy, and Kohen (1983) led to smaller employment elasticities that were generally insignificant. However, Bazen and Marimoutou (2002) note that Wellington and Card and Krueger enter some variables in levels that Brown, Gilroy, and Kohen entered in logs. When they estimate the original specification with data through 1993 (the same period as in Card and Krueger 1995a), they find an elasticity of −0.08 that is significant at the 5 percent level, although this estimate is still below those based on data through 1979.[37]

The reasons for a decline over time in the estimated minimum wage effect from such models have been the subject of some debate. Card and Krueger (1995b) argue that this decline suggests that the time-series studies published in the 1970s and early 1980s were contaminated by publication bias. Using meta-analysis methods, they find that the reported t-ratios in such studies were clustered around two, and that estimated effects declined (toward zero) over time. Because smaller estimated effects would tend to become significant as the sample size grew longer, they argue that the declining estimates constitute evidence that researchers were more likely to choose and report specifications that produced statistically significant negative estimates corresponding to their theoretical priors. However, in Neumark and Wascher (1998), we report successive estimates with increasingly longer time series from a benchmark specification that is arguably uncontaminated by publication bias. These estimates produce a pattern of results not materially different from those in the studies included in Card and Krueger's meta-analysis, indicating that the decline in reported estimates is likely due to a weakening effect rather than to publication bias.

We offered two possible reasons for the decline in the coefficient on the minimum wage variable (the Kaitz index). First, if changes in coverage, which dominate movements in the Kaitz index early in the sample period, have a larger effect on employment than changes in the relative value of the minimum wage, the fact that coverage has been essentially unchanged since the early 1970s would lead to a lower estimated effect over time. Second, given the widening in the wage distribution during the 1980s, the Kaitz index, which uses the average wage in the denominator, may overstate the decline in the bite of the minimum wage that took place during the 1980s, leading to a growing bias (toward zero) in estimates of the effect of minimum wages on employment.[38]

However, more recent research finds no evidence of a declining minimum wage effect in the aggregate time-series data. In particular, Williams and Mills (2001) argue that previous time-series studies of the effects of the minimum wage on employment did not adequately account for serial correlation and non-stationarity in the data, rendering estimates from the standard specifications inconsistent.[39] To address this issue, Williams and Mills estimate a vector autoregression model with separate equations for employment, the change in the Kaitz index, and each of the control variables (transformed as needed to ensure stationarity). The results indicate that changes in the minimum wage "Granger-cause" teenage employment and can account for between 7 and 10 percent of the variation in teen employment rates over the 1954–1993 sample period. In addition, impulse response functions from the VAR model suggest that raising the minimum wage has an immediate negative effect on employment and that the employment elasticity rises to roughly −0.4 over a two-year period.

Bazen and Marimoutou (2002) also argue that the specifications used in the earlier time-series literature were dynamically misspecified, but they address this issue in a different manner than Williams and Mills. In particular, they extend the standard model by implementing an approach that specifies stochastic structures for the trend, seasonal, and cyclical components, rather than the deterministic components used in past time-series models, but that nests those models as well. In addition, they include the minimum wage and average manufacturing earnings in the model along with the Kaitz index to relax the constraint imposed by the Kaitz index that the effects of changes in the minimum wage and the average wage are of equal but opposite sign. In general, the data reject the deterministic specification in favor of the stochastic specification: the estimates indicate that many of the unobserved components have stochastic elements (a key exception is the cyclical component) and that the stochastic model exhibits greater parameter stability and better forecasting performance than does the standard model.[40] In addition, although the coefficient on the Kaitz index is not statistically significant, the coefficients on both the minimum wage and average manufacturing wage are significant, and the restriction that the minimum wage and average wage enter with equal but opposite-signed effects is rejected. Finally, they find that the effect of the minimum wage on employment has been fairly constant over time and, extending the sample through the second quarter of 1999, they report statistically significant negative effects of the minimum wage on teenage employment, with elasticities of -0.11 in the short run and -0.27 in the long run.

We have doubts about the relevance of time-series studies to the present context, given the proliferation of state minimum wages. Nonetheless, the new time-series results pose a clear challenge to claims that the time-series evidence for the United States does not show a detectable adverse effect of minimum wages on teenage employment.[41]

3.5.2 Studies Focused on Specific Federal or State Minimum Wage Increases

In the mid-1990s, David Levine (editor of *Industrial Relations*) asked various researchers who had studied minimum wages to precommit to a research design for studying the 1996 and 1997 increases in the federal minimum wage. The journal would review the design and accept it (with revisions) or not, after which the authors, when the data were released, would simply follow their "recipe" and report the results.[42] The motivation for this approach is to avoid specification searches that might lead to results consistent with authors' priors—a charge that Card and Krueger had earlier leveled at economists who tended to find negative effects of minimum wages on employment (1995b, 242). The journal's project would have been more valuable had more researchers involved in the minimum wage debate who were invited to participate agreed to do so, but only one prespecified research design—Neumark (2001)—was submitted and published.

This paper reports results from a variety of standard panel data models estimated with two different minimum wage variables—the minimum wage relative to the average wage in the state, and the fraction below variable described earlier. The data set extends from roughly one year before the first federal minimum wage increase in October 1996 to one year after the second increase in September 1997. The identifying information is the state-specific change in the effective minimum wage, which is driven in large part by the federal increases; unfortunately, a general lack of state minimum wage variation during this period makes it less than ideal for studying the effects of minimum wages.

The estimates of the employment effects for teenagers are generally near zero; for youths (aged sixteen to twenty-four), the estimates are frequently negative and larger, with elasticities of approximately −0.15 (although they are again insignificant). In addition, the results are not particularly robust for either teenagers or sixteen- to twenty-four-year-olds as a whole. In contrast, evidence of disemployment effects is stronger when the sample is restricted to less-skilled individuals. For example, using the relative minimum wage variable, the estimated elasticity for nonenrolled youths aged sixteen to twenty-four with no more than a high school education is −0.3, and for nonenrolled twenty- to twenty-four-year-olds with no more than a high school education the elasticity is around −0.15; these estimates are often significant, but not always. Thus, the evidence points to disemployment effects of minimum wages for young, unskilled workers.

At the state level, Singell and Terborg (2007) examine the effects of minimum wage increases in Oregon and Washington on the eating and drinking sector and the hotel and lodging industry. They use data from 1994–2001, a period that includes three increases in the minimum wage in Oregon and three increases in the minimum wage in Washington. Although this study is subject to the criticism of sector-specific studies discussed earlier, the large size of the sectors used in the analysis (as opposed to, for example, fast-food restaurants) arguably makes the problem less severe. On the other hand, the issue of tipped workers in these industries remains a potential concern.[43]

Singell and Terborg first use BLS wage survey data to establish that minimum wages are strongly binding in the eating and drinking sector, but considerably less binding in the hotel and lodging sector, where average wages are higher. They then use monthly BLS employment data for these two sectors to estimate flexible regression models that identify the effects of minimum wage changes from the relative changes in state-level employment growth following minimum wage increases. The results for the eating and drinking sector consistently indicate that increases in the minimum wage reduced employment, with an employment elasticity of −0.2. In contrast, the estimates for hotel and lodging are positive and significant, with elasticities of about 0.15. The higher wages in this sector may explain the absence of a negative effect, although it is not clear why this factor would contribute to a positive effect.

Singell and Terborg also analyze help-wanted ads for various jobs in these two industries. The help-wanted data are a potentially valuable addition to the study, because the BLS employment data do not provide the breakdown into the specific jobs for which the wage results were reported. Using the help-wanted ads, the authors classify the data by the jobs for which wage distributions are reported, and focus on those jobs for which minimum wages were binding. The want-ad regressions are presented for five jobs in eating and drinking (wait staff, bus staff, dishwasher staff, hosts, and cooks) and one job in hotels and lodging (housekeepers) for which wages are low. In five out of six cases—*including* housekeepers—the estimated effect of the minimum wage on the number of want-ads is negative and significant, with a 10 percent increase in the minimum wage reducing the number of ads by 10 to 47 percent. Among the restaurant jobs, the only insignificant result is for cooks, who hold the highest-paying job in the eating and drinking industry and thus are less likely to be affected by minimum wage changes.[44] Thus, the general conclusion the authors draw from this study is that the minimum wage increases in Oregon and Washington had an adverse effect on employment in the low-wage eating and drinking sector and on low-wage workers in the hotel and lodging sector.

3.5.3 Efforts to Identify the Effects of the Minimum Wage on Directly Affected Workers

Much of the literature discussed thus far has focused on the effects of the minimum wage on the aggregate employment rates of teenagers (or young adults). Teenagers are typically the age group studied because a large share of them work at or near the minimum wage, so that the effects of minimum wages are more likely to be evident for this group than for other broad demographic groups.[45] However, many teenage workers earn significantly more than the minimum wage, and, as a result, the reported elasticities from studies of teenagers tend to understate the elasticity of demand with respect to the minimum wage for the least-skilled workers among them.

To illustrate this intuition, we can write the minimum wage elasticity for all teenagers as a weighted average of the elasticity for workers directly affected by a change in the minimum wage and the elasticity for workers currently earning above the minimum wage, or $e = e^A \cdot p^A + e^{NA} \cdot (1 - p^A)$, where e is the estimated elasticity for teenagers as a whole, e^A and e^{NA} are the minimum wage elasticities for affected and unaffected workers, and p^A is the proportion directly affected by the change in the minimum wage. If we simplify and assume that the elasticity for unaffected workers is zero, then the minimum wage elasticity for affected workers (e^A) can be written $e^A = e/p^A$. It follows that the minimum wage elasticity for affected teenage workers is greater than the elasticity estimated for teenagers as a whole.

The same argument applies with even greater force to the broader adult population. Because p^A is much smaller for this population, the disemployment effect for adults as a whole may be trivial, even if there are sizable disemployment effects for low-wage adults directly affected by the minimum wage. And from a policy perspective, the effect of a minimum wage increase on low-skill adults is arguably of much greater interest than the effect on teenagers, both because low-skill, low-wage adults are more likely than teenagers to be permanent low-wage workers, and because such adults are much more likely to be primary earners in poor families. This line of reasoning provides a motivation for focusing more explicitly on the effects of minimum wages on the employment of workers directly affected by the minimum wage, and in particular those workers whose wages were at or near the old minimum prior to an increase in the wage floor.

The estimated elasticity from the usual minimum wage study tends to understate the elasticity of demand for affected workers for a second reason, regardless of the age group studied. Specifically, because some affected workers are already earning more than the old minimum wage (but less than the new minimum wage), the size of the average wage increase associated with a higher minimum wage will be smaller than the minimum wage increase itself. Letting ΔW^A denote the average percent wage change of those workers whose wages are directly affected by the change in the minimum wage, and ΔMW the legislated increase, the demand elasticity for affected workers can be written as

$$e^A = (e/p^A) \cdot (\Delta MW/\Delta W^A), \tag{3.2}$$

where $(\Delta MW/\Delta W^A)$ exceeds 1.

To the extent that we are interested simply in the employment effects of a legislated increase in the minimum wage, the adjustment for the actual versus legislated wage change is not important. But it becomes highly relevant when we consider the implications of a change in the minimum wage for earnings (see chapter 4), which is more closely related to the elasticity of demand for affected workers. It is often asserted that because estimated employment elasticities in the range -0.1 to -0.2 are much smaller than -1 (in absolute value), the earnings of affected workers, on the whole, will rise substantially when the minimum wage is raised (e.g., Freeman 1996). However, equation (3.2) shows that this argument is too simplistic, both because the elasticity should be adjusted for the proportion affected and because the average wage increase for affected workers may fall short of the legislated increase in the minimum wage.

To illustrate this point, consider the 1996–1997 legislation that raised the minimum wage from \$4.25 per hour to \$5.15 per hour, a 21.2 percent increase. Data from the 1995 CPS indicate that 6.2 percent of workers aged sixteen to twenty-four were paid the old minimum wage in that year and another 15.1 percent were paid a wage between the old and new minimums, implying that a total of 21.3 percent of the youth workforce was directly affected by the minimum wage increase. If everyone in these categories who retained a job saw their new wage rise to exactly \$5.15 per hour as a result of the increase in the minimum, the average wage increase received by a worker in this affected group would be 10.8 percent. If we further assume that all of the job loss resulting from the minimum wage increase occurred among these affected workers, then using an elasticity of -0.1 for the age group as a whole, we can calculate the demand elasticity for young minimum wage workers as

$(-0.1/0.213)/(10.8/21.2) = -0.92.$[46]

This calculation is only illustrative. But it suggests that appropriately adjusting the estimates taken from studies of the employment effects of minimum wages in order to obtain an elasticity of demand for minimum wage workers can easily produce an elasticity that is close to -1, the level at which minimum wage increases have essentially no effect on the average earnings of the low-wage workforce. And, if we start with a larger "baseline" disemployment elasticity (e.g., an "elasticity" of -0.2), then the effective elasticity of demand would be closer to -2, clearly within the range in which an increase in the minimum wage would lead to a reduction in the average earnings of low-wage workers. And of course, similar considerations apply to estimated effects of minimum wages on hours.[47]

A number of studies have looked for ways to explicitly estimate the effects of minimum wages on employment of directly affected workers. For example, Currie and Fallick (1996) use longitudinal data from the National Longitudinal Survey of Youth (NLSY) to study the increases in the federal minimum wage in 1980 and 1981. They calculate a wage gap for each employed individual as the difference between the individual's wage in year t and the minimum wage in year $t+1$ for workers whose wage in year t was between the old and new minimum wage, and zero otherwise. They then estimate a linear probability model to compare subsequent employment rates for individuals who were directly affected by increases in the nominal minimum wage with individuals who were not directly affected; the model is also estimated with fixed individual effects to control for persistent differences in turnover between low-wage and high-wage individuals. The results show clear signs of a negative and statistically significant disemployment effect even after controlling for other unobservable individual differences, with an implied employment elasticity of about -0.2.[48]

Abowd et al. ("The Tail of Two Countries," 2000) attempt to improve on this approach by using data over a longer period, which introduces identifying information from comparisons between workers in periods when the minimum wage rose with similarly paid workers in periods when the minimum wage stayed the same. In particular, when the nominal minimum wage rises, they count an observation as affected if the individual's real wage in year t is between the real value of the minimum wage in year t and the real value of the minimum wage in year $t+1$. In contrast, when the nominal minimum wage is unchanged between year t and year $t+1$, they count an observation as affected (in the opposite direction) if the individual's real wage in year $t+1$ is above the real value of the minimum wage in year $t+1$ but below the real value of the minimum wage in year t; this construct captures individuals who are no longer bound by the minimum wage because of its erosion in real terms. Using data from 1981 to 1991 and incorporating information on both federal and state minimum wages, they find little evidence of statistically significant effects of the minimum wage on either exit rates from or entry rates into employment.[49]

In Neumark, Schweitzer, and Wascher (2004), we use individual-level matched observations from the CPS Outgoing Rotation Group

(ORG) files for the years 1979–1997, also incorporating state-specific increases in minimum wages. Our approach is similar to that used by Currie and Fallick (1996), as well as by Abowd et al. ("The Tail of Two Countries," 2000), but is more general, in that it enables us to estimate the effects of minimum wages at various points throughout the wage distribution. In particular, we specify an employment model that allows the change in the effective minimum wage for each state-month observation to interact with a set of indicator variables that describe where each individual's wage stands in relation to the minimum wage:

$$E^2_{isym} = \alpha + \sum_j \beta_j \frac{MW^2_{sym} - MW^1_{sym}}{MW^1_{sym}} \cdot R(w^1_{isym}, MW^1_{sym})^j$$
$$+ \sum_j \gamma_j R(w^1_{isym}, MW^1_{sym})^j + X^1_{isym}\delta + M_m\lambda + S_s \cdot Y_y\pi + \varepsilon_{isym}. \quad (3.3)$$

In this model, i indexes the individual, s the state, y the year, and m the month. The 1 and 2 superscripts indicate the year 1 and year 2 observations on each individual, and the model is estimated for those employed in year 1 (because we need to observe a year 1 wage). The dependent variable is employment in year 2. We estimate a similar model for the percent change in hours, as well as for wages and earnings (these results are discussed in the next chapter). The first term, involving β_j, is a sum of the percentage change in the minimum wage multiplied by a vector of dummy variables (R) that divide up the initial wage distribution, with very finely divided categories near the minimum wage, and broader categories higher up in the wage distribution. The model also includes these dummy variables, separately, to allow for a flexible relationship between employment and wages irrespective of the change in the minimum wage.[50] Finally, the model includes individual demographic and skill-related controls, as well as a full set of state-year interactions. In this specification, the minimum wage effects are identified from differential changes in employment for workers at similar points in the wage distribution who experience different minimum wage changes.[51]

The results indicate that workers whose wages are initially close to the minimum wage are most likely to be affected by changes in the wage floor. For workers initially earning the minimum wage or slightly more, the estimated employment elasticities range from about −0.06 to −0.15 and are often statistically significant. Note that these results are for workers of all ages, and not just teenagers. The effect on hours (conditional on employment) is particularly noticeable at the low end of the wage distribution, with an elasticity around −0.3 for workers initially earning between 1 and 1.3 times the old minimum wage—suggesting that employers also respond to minimum wages by shortening the workweeks of their lowest-paid employees.

A related approach focuses on identifying employment effects of minimum wages on workers more likely to be affected by the minimum wage because they reside in a state where the minimum wage is high relative to the equilibrium wage (Neumark and Wascher 2002a). In this approach, we use data aggregated to the state-by-year level (as in the panel data analyses). For any state-year observation, however, the minimum wage is on average more or less likely to be binding, depending on the minimum wage and other determinants of labor supply and labor demand. Only for those observations for which minimum wages are more binding would we expect—according to the competitive model—to find much evidence of disemployment effects. We therefore classify state-year observations (in a probabilistic sense) into one of two categories: observations for which the minimum wage is binding, so that employment is determined by minimum wages along the labor demand curve; and observations for which the minimum wage is not binding, so that employment is determined by variables affecting labor supply and labor demand, but not by the minimum wage. We then specify a switching regression model, with the switch points defined as the intersection of the labor demand and labor supply curves (to differentiate the binding and nonbinding regimes).[52] Estimates from the model point to negative and significant effects of the minimum wage on sixteen- to twenty-four-year-olds in the binding regime, with estimated elasticities in the range of −0.13 to −0.21; of course, the effect of the minimum wage is zero in the nonbinding regime.

The estimates from this approach can be used to calculate the probability that minimum wages are binding for any particular state-year observation, and hence these estimates are informative about what we might expect from single-equation reduced-form regression estimates of minimum wage effects—especially from studies of isolated minimum wage increases. For example, in the late 1980s and for the Pacific states, the estimated probabilities that an observation is on the binding regime are quite low, which could help to explain the weak evidence of negative minimum wage effects in Card's 1992 studies (although this cannot explain a positive effect).

3.6 International Evidence

The international evidence on minimum wages is large and growing, and covers both industrialized and developing countries. In this chapter, where we discuss employment effects of minimum wages, as well as in subsequent chapters, when we discuss other topics, our review of the international evidence is likely less comprehensive than our discussion of research for the United States, if for no other reason than that some of the studies are written in languages other than English.[53] Our review of the international evidence may therefore provide a less reliable description of the distribution of estimated minimum wage effects across studies. In this section, we begin with a review of the evidence for the industrialized countries, and then turn to studies of developing countries, focusing in both cases on the studies that we regard as the most interesting and compelling.[54]

3.6.1 Industrialized Countries

3.6.1.1 Panel of OECD Countries We begin this section with a discussion of our study of the effects of minimum wages in seventeen OECD countries, taking account of variation in a number of other labor market policies and institutions (Neumark and Wascher 2004). We first estimate a standard panel data specification for teen and youth employment rates, including as explanatory variables the current and lagged minimum wage relative to the average wage, aggregate labor market and demographic controls, fixed country and year effects, and country-specific time trends. The models are estimated for teenagers (aged fifteen to nineteen) and youths (aged fifteen to twenty-four), with data extending from the mid-1970s through about 2000. The results consistently point to negative effects of the minimum wage on employment for the sample as a whole. For our baseline specification, the estimated short-run elasticities range from −0.18 to −0.24 for teenagers and from −0.13 to −0.16 for youths, with all of these estimates statistically significant. The estimated long-run elasticities from a dynamic specification with a lagged employment rate are somewhat larger: roughly −0.40 for teenagers and −0.23 for youths.

We then augment the models to explore how differences in the characteristics of minimum wage policies in each country, as well as differences in other labor market policies and institutions, influence the effects of minimum wages.[55] The policy and institutional differences we consider include the rigidity of labor standards (e.g., legislated working time rules, worker representation rights, and restrictions on the use of contract workers), the strength of employment protection regulations, the use of active labor market policies by the government, union density, and the generosity of unemployment insurance.

With regard to minimum wage systems, we find that the negative effect of the minimum wage on teenage or youth employment is diminished in countries with a youth subminimum, consistent with the hypothesis that a higher minimum wage induces substitution toward workers eligible for the subminimum.[56] We also find evidence, although it is somewhat weaker, that minimum wages do not result in employment losses in countries in which minimum wages are set by some type of national collective bargaining process, suggesting that collective bargaining takes more explicit account of (and hence avoids) potential disemployment effects in setting minimum wages.

Turning to the influence of other labor market policies and institutions on the effects of minimum wages, in accordance with expectations we find that minimum wages have more adverse effects when labor standards (such as working hours limits) are more restrictive, presumably because the presence of rigidities causes firms to make more of the adjustment to the higher minimum through the employment channel (although the differences are typically not significant). Conversely, we find strong evidence that a high degree of employment protection mutes the disemployment effects of minimum wages. The same is true when active labor market policies are more prevalent, most likely because some of those who would otherwise be considered nonemployed are instead participating in these programs. Finally, minimum wages are estimated to have more adverse employment effects when union density is high, possibly reflecting greater power of incumbent workers; we will return to this issue in chapter 8.

Finally, we use these characteristics of labor market policies to classify countries along two dimensions: high versus low labor standards, and high versus low employment protection or active labor market policies. For example, the United States, the United Kingdom, Japan, and Canada fall into the quadrant with low standards and low protection, and Germany, Italy, Sweden, Spain, and France fall into the quadrant where both are high. The estimates implied by the interactive specifications and by models fit for separate sets of countries indicate that negative employment effects are strongest for the least-regulated economies, although the disemployment effects also show up to some extent in countries with high labor standards but low employment protection and active labor market policies. For the other countries in the sample, the estimated effects are zero or positive. These results indicate that the effects of minimum wages can vary considerably depending on the presence of other labor market institutions, and they suggest—perhaps not surprisingly—that the neoclassical prediction about disemployment effects of minimum wages holds most strongly for the economies in which labor markets are less regulated. Of course, evidence from individual countries is likely to provide more compelling information on how minimum wages affect employment in that country, and the remainder of this section discusses such evidence. As will be apparent, however, such analyses often create a challenge for researchers in identifying an appropriate control group because of the lack of within-country variation in minimum wages for most countries.

3.6.1.2 Canada One exception to the lack of within-country variation is Canada, where minimum wages vary across provinces and over time. Perhaps the most compelling study for this country is a recent paper by Campolieti, Gunderson, and Riddell (2006), which has a similar flavor to the prespecified research design in Neumark 2001. These authors did not precommit to a research design before obtaining and studying data on minimum wage increases in Canada, and hence avoid discarding useful data. Instead, they simply apply the specifications proposed in the Neumark paper, as well as other modifications proposed by others, to the Canadian data, so as to avoid specification search that could introduce biases. The results from these standard reduced-form specifications consistently show disemployment effects of minimum wages, with elasticities ranging from about -0.14 to -0.44, and centered on about -0.3.[57]

Research for Canada has also attempted to isolate the effects of minimum wages on the employment of the least skilled. Campolieti, Fang, and Gunderson (2005a) use longitudinal data for 1993–1999 to examine the effects of provincial changes in minimum wages on the transitions from employment to nonemployment among low-wage youths. They define youths as at-risk if they resided in a province that experienced an increase in the minimum wage between year t and year $t+1$

and if their initial wage was between the old and new minimum wage. They then compare the transition probabilities for these individuals with a variety of control groups consisting of subsets of young workers who resided in provinces in which the minimum wage did not rise during that year; these subsets range from workers with a wage between the minimum wage and 25 cents above the minimum wage to all workers in the control set of province-year observations. In addition, they present evidence from a specification that uses the gap between the previous wage and the new minimum wage as the minimum wage variable, as well as a specification that attempts to control for within-group heterogeneity by including as an additional control variable the gap between an individual's wage and the upper bound of the control group wage for individuals in the control group. In general, Campolieti, Fang, and Gunderson find large negative effects from the minimum wage, with implied overall employment elasticities for youths between -0.33 and -0.54.[58]

3.6.1.3 United Kingdom Prior to the early 1990s, the United Kingdom had a system of Wages Councils, which consisted of employer and worker representatives and independent members appointed by the government, and which set minimum wages in low-wage sectors. The Wages Councils were abolished in 1993, and from 1993 to 1998 there was no minimum wage in the United Kingdom. In 1999, a national minimum wage was introduced. Thus, although there is no regional variation in minimum wages in the United Kingdom, the sharp changes in policy suggest that analyses of this country could be potentially valuable in studying the effects of the minimum wage.

We begin with a study by Machin and Manning (1994) and a later paper coauthored with Richard Dickens (Dickens, Machin, and Manning 1999), which focus on the late 1970s to the early 1990s, during which minimum wages declined relative to average wages in industries covered by the Wages Councils.[59] The authors report one-year first difference regressions of the change in log employment on the change in the log of the minimum wage relative to the average wage in each sector. For the low-wage sector as a whole, these models consistently yield positive estimated employment effects, which are in some cases statistically significant. In addition, the estimates are often quite large (with elasticities as high as 0.43), and they remain positive when lags are included.

The evidence of positive effects leads Dickens, Machin, and Manning (and others citing this work) to suggest that the data are more consistent with monopsony in the low-wage labor market than with a competitive model. However, the specifications with the largest positive effects include a control for sales in the industry covered by the Wages Council, which is problematic, because an important channel through which the minimum wage is thought to reduce employment is by raising labor costs and prices, which reduces product demand.[60] Their reduced-form estimates, which are generally smaller and not statistically significant, strike us as more sensible estimates of policy effects.

In addition, these results are potentially contaminated by endogeneity bias. More specifically, if committees of workers and owners increase minimum wages relatively more when demand conditions for low-wage workers in the industry are (or are projected to be) good, estimates of the employment effects will have a positive bias. Of course the potential problem of the endogeneity of minimum wage increases is not unique to this study, and it is one that we regard as an important unanswered question more generally. However, we suspect that this problem is more likely to arise in the context of the U.K. Wages Councils than in cases where minimum wages are enacted by legislatures (for which there often seems to be much more regard for political than economic timing).[61] Dickens, Machin, and Manning acknowledge the endogeneity problem but downplay it, citing discussions with independent members of Wages Councils (although in a footnote they only mention one) indicating that "the method of minimum-wage fixing was generally rather crude, using only recent pay settlements and inflation figures and making no attempt to forecast future market conditions" (1999, 8). More systematic evidence on what influenced the minimum pay rates set by Wages Councils clearly would be helpful in this regard. But at a minimum, one should be cautious in presuming that these results carry over to the effects of legislated minimum wage changes.

Along with other coauthors, Machin and Manning then present evidence associated with the abolition of the Wages Councils in 1993 (Dolado et al. 1996). In particular, they report data on the share of total employment accounted for by the Wages Councils industries before and after their abolition and compare hiring and exit rates in industries covered by Wages Councils with the equivalent rates in industries not covered by the councils. Based on these data, they conclude that "there is no noticeable change in the behaviour of the Wages Council sector relative to the rest of the economy" (1996, 355). However, we read their evidence differently. Using average employment totals for the three quarters before and two quarters after the abolition of the Wages Councils, employment grew more rapidly in the Wages Councils industries following the elimination of the minimum wage. In other words, a simple difference-in-differences estimate suggests that the abolition of the Wages Councils led to a relative increase in employment in the Wages Council sector, consistent with disemployment effects of minimum wages.[62]

Several recent studies have examined the effects of the introduction of a national minimum wage in April 1999. One is a study by Machin, Manning, and Rahman (2003), who survey low-wage residential care homes (nursing homes) in the period from nine months before to nine months after the minimum wage was implemented. Using the share of workers initially paid less than the minimum and the average wage gap (hours weighted) to identify the minimum wage effect, they report adverse effects of the minimum wage on both hours and employment. The estimated employment effects are mostly statistically significant, with elasticities ranging from −0.08 to −0.38; the hours elasticities are of similar magnitudes. Reiterating the earlier point we made about studies of specific industries, these estimates are not necessarily informative about the overall employment effects of minimum wages on low-skill individuals. Moreover, it is unclear why these findings differ from those reported in the studies of the Wages Councils, although the focus of this study on a single sector and the low response rate to the survey (about 20 percent) limits its comparability to the earlier research. Arguably, though, this is a better research design for a policy change that the authors describe as a "very good testing ground for evaluating the economic effects of minimum wages" (155).

Stewart and Swaffield (2006) also study the introduction of the national minimum wage, using a standard difference-in-differences approach to compare hours changes for those initially paid less than the new minimum wage and those initially paid just above the new minimum wage. One limitation of this identification strategy is the possibility that aggregate influences on hours changes may differ across the groups. However, because the minimum wage variation is national, there is no way to control for this with year effects, in contrast to what one can do when there is regional minimum wage variation. The estimates, which also allow for lagged effects, show a small and insignificant contemporaneous effect of the minimum wage on hours, but more adverse longer-run effects; the lagged effect on hours is always negative, larger in absolute value, and generally (although not always) statistically significant.[63] Summarizing the results, Stewart and Swaffield conclude that the minimum wage led to reductions of one to two hours per week for affected workers.[64]

Finally, Galindo-Rueda and Pereira (2004) use firm-level data to study the introduction of the national minimum wage in the United Kingdom. This study employs data from the Annual Business Inquiry and the New Earnings Survey for the period 1997–2001 to compare outcomes at firms more affected by the minimum wage relative to firms less affected. In particular, they impute exposure to the minimum wage based on the distribution of wages at the regional and sectoral levels, and then estimate models for changes in employment and other outcomes using a specification that interacts this exposure variable with year dummy variables and that includes sector- and region-specific trends; the interactions capture the minimum wage effect. The regression results point to significant disemployment effects for services but not for manufacturing.[65]

Galindo-Rueda and Pereira also study employment changes in low-wage sectors using data from the Office of National Statistics. In this analysis, they identify minimum wage effects from variation in wage levels across regions. The estimated effects are negative and significant in four of the eight low-wage sectors they study, negative and insignificant in three of the eight, and positive and insignificant in the eighth. Based on other information on entry and exit, the authors interpret the combined evidence as suggesting that the introduction of the minimum wage had relatively little effect on workers already employed, but exerted more impact through its effect on job creation in low-wage sectors.

Aside from the United States, the effect of minimum wages on employment has been studied more extensively in the United Kingdom than in any other country. The research for the United Kingdom is particularly significant, in our view, because some of it has been widely cited as providing evidence that an increase in the minimum wage does not reduce employment and in some cases increases it. However, based on the broader and more recent evidence, it seems incorrect to point to the evidence from the United Kingdom as making a strong case that the minimum wage does *not* reduce labor demand for unskilled workers; indeed, the weight of the evidence points more toward disemployment effects.

3.6.1.4 France The minimum wage in France is set by the federal government and is generally increased each year in line with prices and average wages for blue-collar workers. Moreover, the level of the minimum wage in France has tended to be high relative to the average wage and the minimum wage is often blamed for the high level of youth unemployment in that country.

There have been a number of studies of the effects of minimum wages in France, but in our view, the most compelling is a paper by Abowd et al. ("Minimum Wages," 2000) that compares the effects of minimum wages on low-wage workers in the United States and in France. As discussed earlier, the authors identify the effects of minimum wages by identifying individuals who were either newly bound by an increase in the real minimum wage or "freed" by a decrease in the real wage floor. For France, which generally had a rising nominal minimum wage over the 1982–1989 period used in the study, the authors consistently find considerably higher transitions to nonemployment for workers newly bound by the minimum wage than for workers with marginally higher wages; these results are especially strong for workers just above age twenty-four, who are the youngest workers not covered by employment promotion contracts that permit paying subminimum wages.[66] For example, for men aged twenty-five to thirty who were caught by the rise in the minimum wage, the elasticity of employment with respect to the minimum wage is −4.6, relative to similar men just above the minimum. For women, the results are weaker and insignificant, but the elasticity is still large (−1.38). For those aged twenty to twenty-four, the elasticities are smaller and insignificant, and the elasticities are smaller still, and insignificant, for males and females aged sixteen to nineteen. Although the elasticities for those above age twenty-five are large, the authors point out that these are elasticities that apply to a very small share of the population in the age group. Thus, these results reinforce our earlier argument that the effects of minimum wages are quite different when one focuses on directly affected workers rather than on a broader group.

Our paper on the OECD countries, discussed previously, suggests that France may have a combination of labor market institutions that makes it less likely that minimum wages will have detectable disemployment effects on young workers, and the results in Abowd et al. ("Minimum Wages," 2000) for those under age twenty-five appear to confirm this. At the same, however, the results reported by Abowd et al. point to disemployment effects of the minimum wage among low-skilled workers less protected by these institutions.

3.6.1.5 Spain Although the national minimum wage in Spain is also set by statute, it is determined by the Council of Ministers after consultation with trade unions and employer organizations. As in the United States, the ratio of the national minimum wage to the average wage in Spain has gradually declined over time, limiting the extent to which time variation can be used to identify the effects of minimum wages on employment. However, Dolado et al. (1996) report on what is more likely a fairly clean experiment—an 83 percent increase in 1990 in the minimum wage for sixteen-year-olds; there was also a more modest increase of 15 percent for seventeen-year-olds. In particular, they regress region-specific changes in youth employment between 1990 and 1994 on the fraction of workers in each region that were low-paid prior to the implementation of the higher minimum wage. The results provide strong evidence that employers substituted away from sixteen- to nineteen-year-olds after the increase in the minimum wage for sixteen- to seventeen-year-olds. Moreover, the coefficient on the minimum wage variable is positive for twenty- to twenty-four-year-olds, which suggests that the results for teenagers reflect the change in the minimum wage rather than other changes in labor demand. The authors conclude from their study that minimum wage increases in Spain reduce teen employment.

3.6.1.6 Portugal A similar quasi experiment took place in Portugal in January 1987. In this case, the government eliminated the 75 percent subminimum wage for workers aged eighteen to nineteen, making them eligible instead for the adult minimum wage. In effect, this legislative change resulted in a 49.3 percent increase in the nominal minimum wage for this age group, as compared with a minimum wage increase of only 12 percent for workers aged twenty and over. Pereira (2003) exploits this policy change to study the effects of the minimum wage on teenage employment in Portugal, using a firm-level panel data set for the period 1986–1989.[67] She first compares changes in employment and hours across the three age groups for intervals of one, two, and three years after the minimum wage increase. She also estimates models that separate out the differences in changes in employment by age for firms whose average wage for teenagers in March 1986 was between the old and the new minimum, identifying the minimum wage effect from those firms that were most likely to be directly affected by the minimum wage increase. All of the models include controls for initial firm size, industry, and region; she also presents evidence from analyses that account for firm entry and exit, which is substantial.

The evidence indicates that teen employment fell relative to employment for workers aged thirty to thirty-five, with the difference statistically significant; implied elasticities from her preferred estimates range from −0.2 to −0.4. In contrast, employment of workers aged twenty to twenty-five increased relative to the older group, consistent with substitution away from teenagers and toward this group in response to the increase in the price of teen workers relative to their close substitutes. In addition, the estimates imply that overall youth employment (ages eighteen to twenty-five) declined slightly. The evidence is particularly strong for the specifications that identify the minimum wage effects from the most-affected firms, and indicates that the effects are stronger one or two years after the minimum wage increase than in the first year of the increase, suggesting the presence of lagged effects. Pereira also estimates similar models for total hours and finds even larger effects, suggesting that employers reduced the average work weeks of their teenage employees as well.[68]

3.6.1.7 New Zealand A recent paper by Hyslop and Stillman (2007) similarly exploits changes in youth subminimum wages in New Zealand. A 2001 reform eliminated (in two steps) a 60 percent subminimum for eighteen- to nineteen-year-olds, resulting in a 69 percent increase in their minimum wage, most of which occurred in 2001. The same reform raised the subminimum for sixteen- to seventeen-year-olds from 60 to 80 percent in two roughly equal steps in 2001 and 2002, adding up to a 41 percent increase in the minimum wage for this group; in this same period, the adult minimum wage increased only slightly.

Using data from the Household Labour Force survey for the period 1998–2003, Hyslop and Stillman estimate difference-in-differences models for sixteen- to seventeen-year-olds, eighteen- to nineteen-year-olds, and twenty- to twenty-one-year-olds relative to twenty-two- to twenty-five-year-olds, for employment, hours conditional on employment, and earnings (as well as other outcomes discussed in chapter 6). The employment models with the fullest set of controls point to insignificant but negative effects for sixteen- to seventeen-year-olds and eighteen- to nineteen-year-olds immediately after the reforms, but a significant negative effect for sixteen- to seventeen-year-olds in 2003 (as well as a larger negative effect, albeit still insignificant, for eighteen- to nineteen-year-olds for the same time span); the implied elasticities are -0.06 for eighteen- to nineteen-year-olds and -0.23 for sixteen- to seventeen-year-olds. However, the point estimates for twenty- to twenty-one-year-olds are also negative, although insignificant, suggesting that employers did not substitute towards these slightly older individuals.[69] We view the evidence for New Zealand as more consistent with minimum wages reducing labor demand, although the evidence is fairly weak.[70]

3.6.2 Developing Countries

The analysis of minimum wage effects on employment in developing countries is complicated by a number of factors. First, labor markets in developing countries often have a large informal sector in which minimum wages (and other labor laws) do not apply or are not enforced, and to which there can be substantial spillovers from the formal sector. Second, even in the formal sector, enforcement of and compliance with minimum wage laws is often erratic. And third, for some countries confounding factors such as antisweatshop campaigns have also created upward pressure on wages of low-skilled workers. These factors make the analysis of minimum wage effects on employment in developing countries more challenging, and require researchers to consider potentially complicating factors that may be specific to each country.

3.6.2.1 Mexico and Colombia These two countries provide an interesting contrast in minimum wage policy, with the minimum wage in Mexico falling sharply in real terms between 1981 and 1987, and the minimum wage in Colombia increasing sharply over this period. As a result, the relative value of the minimum wage was quite low in Mexico at the end of the 1980s and relatively high in Colombia. Bell (1997) exploits this divergence in minimum wage policy to examine whether the employment effects of the minimum wage show through more clearly in the country where the minimum wage is higher. She first presents standard time-series regressions using annual manufacturing data for Mexico and Colombia over a relatively long sample period. For Mexico, she reports a negative but insignificant employment elasticity

(−0.18). For Colombia, in contrast, the estimated effect of the minimum wage is larger, with a statistically significant employment elasticity of −0.34.

She then turns to firm-level panel data sets that allow her to focus on the 1980s, when the divergence between the minimum wage changes in the two countries was especially large. For Mexico, specifications that include firm fixed effects yield employment elasticities ranging from −0.03 to 0.03 for unskilled workers and from −0.01 to 0.05 for skilled workers, with all of the estimated minimum wage effects insignificant. Similar models estimated for Colombia generate different results, with statistically significant elasticities ranging from −0.15 to −0.33 for unskilled workers and from −0.03 to −0.24 for skilled workers. Bell attributes the differing results to the minimum wage being binding on firms in Colombia but not in Mexico, as suggested by distributions of average firm-level wages in both countries.

Feliciano (1998) studies minimum wage effects for Mexico using data on all workers taken from the 1970, 1980, and 1990 Mexican Census of Population. As just noted, the minimum wage in Mexico declined sharply in the 1980s. But minimum wages also fell noticeably over the longer period that she studies, and they became much more uniform across states and regions within states as well. Feliciano uses a standard state-level panel data specification with a relative minimum wage variable, controls for the business cycle, and state and year fixed effects.[71] She generally finds small and insignificant effects for males. For females, however, there is consistent evidence of disemployment effects for all age groups, with elasticities ranging from −0.41 to −0.76. Feliciano speculates that the results differ because Bell focuses only on the manufacturing sector, although she did not check this with her data. Regardless, the evidence that the reductions in the minimum wage in Mexico increased employment of women and had little impact on men is, on net, consistent with overall disemployment effects.

3.6.2.2 Costa Rica Costa Rica also provides fertile ground for studying minimum wages in a developing country context. Between 1988 and 2000, the country moved from a system of more than five hundred minimum wages based on occupation and skill categories to a system of only nineteen different levels, a consolidation that generated a great deal of exogenous variation in minimum wages by occupation and skill category. Gindling and Terrell (2007a) use this variation to create a pooled time-series cross-sectional data set on about ten thousand individuals per year, and estimate models for employment in the covered sector and for hours worked by workers in each sector. The models include the real value of the minimum wage applicable to each individual, a set of human capital controls, and dummy variables for year and for each occupation-skill category that was used in the determination of minimum wages in 1988. The sample for the employment analysis is restricted to those who have worked before, in order to identify their occupation. The estimates from the employment and hours regressions indicate significant negative effects in the covered sector. The employment elasticity is −0.11, and the hours elasticity is −0.06, with the effects concentrated toward the bottom deciles of the skill distribution. There is also a reduction in uncovered sector hours, although this estimate is not significant.

3.6.2.3 Honduras Gindling and Terrell (2007b) also study minimum wage employment effects in Honduras, using a panel data set by industry covering the period 1990–2004. They estimate models for the large-firm sector, the small-firm sector, the public sector, and the self-employed. Formally, the first two, but not the last two, are covered by minimum wages. However, information on wages suggests that minimum wages are enforced only in the large-firm sector, although there also appears to be a modest effect of the minimum wage on wages in the public sector, even though wages in that sector are higher.

Minimum wages varied by industry, firm size, and location, as well as over time. The authors report that minimum wage adjustments could be set by commissions comprised of representatives from unions, businesses, and the government (like the U.K. Wages Councils), or by presidential decree, but that most changes during their sample period were set by the latter mechanism, because the commissions could not agree. Nonetheless, with sector-specific minimum wages, in particular, there is reason to be concerned about endogenous minimum wage setting that would tend to impart an upward bias to estimates of the effect of minimum wages on employment. Gindling and Terrell use the dynamic fixed-effects IV estimator of Arellano and Bond (1991), which uses lagged values as instruments. However, related to our earlier comment on Dickens, Machin, and Manning (1999), we are somewhat uneasy that this estimator effectively identifies the model off of assumptions about the lag structure.

In the authors' preferred specification (using this estimator), they find significant negative disemployment effects in the large-firm sector, with an elasticity of −0.46. The estimate is weakly positive for the small-firm sector, which might reflect a movement of workers out of the large-firm sector into small firms. The effects in the other two sectors, which are not formally covered by minimum wages, are insignificant. When Gindling and Terrell disaggregate workers by educational level, they find the strongest evidence of disemployment effects in the large-firm sector for lower-educated workers, with significant elasticities of −1.85 and −0.86 for those with incomplete and complete primary education, respectively; the estimates are negative but insignificant for the higher education groups in this sector. Thus, although we have some reservations about the estimator, the evidence for Honduras does point to significant disemployment effects precisely in the sectors and for the groups of workers for which they are most likely to occur.

3.6.2.4 Indonesia Two studies for Indonesia take advantage of the variation in minimum wages in that country. Harrison and Scorse (2005) analyze both the effects of sharp increases in the minimum wage and the effects of the United States–driven antisweatshop campaigns on wages and employment, using firm-level data from the Annual Manufacturing Survey of Indonesia for the years 1990–1996. They exploit variation in minimum wages by districts within provinces, and separate the effects of the minimum wage from wage increases attributable to antisweatshop activism by recognizing that the latter should be limited to the textiles, apparel, and footwear industries that were the target of this activism and that these industries are located in a narrower geographic area.

Using long-difference regressions for the change in log employment from 1990 to 1996, the authors report significant and robust estimates of the elasticity of employment with respect to the minimum wage, ranging from −0.12 to −0.18. Only in small firms are the estimated effects insignificant (and smaller), which the authors suggest may result from lower compliance at these firms. Using annual differences instead, they find smaller elasticities of −0.05. They also report weak evidence that the minimum wage increased the probability of firm exit, although they note that the effect of the minimum wage on exit rates could be larger over a longer time period.

Alatas and Cameron (2003) focus on manufacturing firms in Greater Jakarta, which includes the province of Jakarta and three districts of the province of West Java; this three-district area is known as Botabek.

Although Jakarta and Botabek are adjacent and both urban, the provincial minimum wage was considerably higher in Jakarta than in West Java, resulting in a 36 percent differential in the legal minimum wage between Jakarta and Botabek in 1990. Recognizing this discrepancy, the provincial government of West Java subsequently legislated separate minimum wages for Botabek and the rest of West Java, resulting in a convergence of minimum wages in Jakarta and Botabek by 1994.

Using a panel of all Indonesian manufacturing firms with twenty or more employees, the authors implement a matched difference-in-differences approach to estimate employment effects for production workers, who are typically less skilled. The minimum wage effect is identified from changes in otherwise similar firms in Botabek relative to Jakarta. They estimate the model separately for small (20–150 workers) domestic, large domestic, and large foreign firms, arguing that different cost structures across these categories can result in different minimum wage effects. For large firms, all of the estimates are insignificant. The point estimate for large foreign firms is negative, while the evidence for large domestic firms is inconclusive, with some negative and some positive estimates. For small firms, the estimated employment effect is negative overall, indicating significantly faster employment growth in Jakarta in this period, but it becomes insignificant when the control group is narrowed to a small strip just along the border (to hold economic conditions more similar) or when a higher-wage control group from Botabek is used. The authors conclude that the disemployment effects of the minimum wage increases in Indonesia were, overall, not very large.

Based on these two papers (as well as other work), the evidence for Indonesia is mixed, with the results dependent upon research design and firm size. However, the Harrison and Scorse study seems to us to provide the most compelling evidence, both because of its careful research design and because the data cover a wider swath of employment.

3.7 Conclusions

The range of estimates of the employment effects of minimum wages is clearly much wider than was the case in the earlier literature reviewed by Brown, Gilroy, and Kohen (1982). Few of the studies in the Brown, Gilroy, and Kohen survey were outside of the consensus range of −0.1

to -0.3 for the elasticity of teenage employment with respect to the minimum wage. In contrast, even limiting the sample of studies to those focused on the effects of the minimum wage on teenagers in the United States, the range of elasticities reported in studies comprising the new minimum wage research extends from near -1 to above zero. This wider range for the United States undoubtedly reflects both the new sources of variation used to identify minimum wage effects—notably the greater state-level variation in minimum wages—and the new approaches and methods used to estimate these effects; in contrast, the earlier literature was based mainly on time-series evidence, with the methods differing only slightly across studies and data being added incrementally as the literature progressed.

Nonetheless, our overall sense of the literature is that the preponderance of evidence supports the view that minimum wages reduce the employment of low-wage workers. In this chapter, we have identified and emphasized studies that we regard as providing the most credible evidence, and of these, most point to negative employment effects. Moreover, when researchers focus on the least-skilled groups that are most likely to be directly affected by minimum wage increases, the evidence for disemployment effects seems especially strong. In contrast, we see very few—if any—cases where a study provides convincing evidence of positive employment effects of minimum wages, especially among the studies that focus on broader groups for which the competitive model generally predicts disemployment effects. This conclusion contrasts with the assertion made by some researchers that the empirical evidence coming out of the new minimum wage research is most consistent with the conclusion that minimum wages do not reduce employment or even increase it. Examples of such assertions include: "The latest studies of the experience of the USA and the UK in general find no evidence of negative effects on youth employment" (Bazen 2000, 64); and "Under close scrutiny, the bulk of the empirical evidence on the employment effects of the minimum wage is shown to be consistent with our findings . . . which suggest that increases in the minimum wage have had, if anything, a small, positive effect on employment, rather than an adverse effect" (Card and Krueger 1995a, 236).[72] We believe that the evidence discussed in this chapter clearly refutes such assertions.

Some other general themes also emerge from the literature. First, the majority of the U.S. studies that found zero or positive effects of the minimum wage on low-skill employment were either short panel data studies or case studies of the effects of a state-specific change in the minimum wage on a particular industry. In contrast, longer panel studies that incorporate both state and time variation in minimum wages tend, on the whole, to find negative and statistically significant employment effects from minimum wage increases. This difference raises the question, highlighted in the reviews of *Myth and Measurement* by both Brown (1995) and Hamermesh (1995), of whether the former analyses encompass too short of a time period to capture the full effects of minimum wage changes, given the time that is often needed to adjust the production process to economize on low-skilled labor. Indeed, the inclusion of lagged effects seems to help in reconciling alternative estimates of minimum wage effects.

Second, the concerns raised in the literature about the case study approach seem especially cogent. Even aside from the question of whether the surveys conducted by the authors of these studies provide accurate estimates of employment and other indicators, the doubts expressed about the adequacy of the so-called natural experiments used in the case study approach, along with the fact that the standard competitive model provides little guidance as to the expected sign of the employment effects of the minimum wage in the narrow industries usually considered in these studies, makes the results from them difficult to interpret. As a result, it is not clear to us that these studies have much to say either about the adequacy of the neoclassical model or about the broader implications of changes in either the federal minimum wage or state minimum wages.

Third, even aside from the estimates of the effects of the minimum wage on low-skilled individuals as a whole, there seems to be substantial evidence of labor-labor substitution within low-skill groups. Although the choice of the aggregate teenage employment rate as the dependent variable in much of the literature is due to the fact that a sizable portion of this group consists of low-wage workers, not all teenagers are low-wage workers and not all low-wage workers are teenagers. Moreover, from a policy standpoint, the effect of the minimum wage on teenage employment is probably of less interest than its effect on other less-skilled individuals. Some of the more recent literature has attempted to identify these substitution effects more directly or has focused more specifically on those individuals whose wages and employment opportunities are most likely to be affected by the minimum wage, and the estimates from this line of research tend to support the notion that employers replace their lowest-skilled labor with close substitutes in response to an increase in the wage floor. As a result, minimum wages may harm the least-skilled workers more than is suggested by the net disemployment effects estimated in many studies.

In sum, we view the literature—when read broadly and critically—as largely solidifying the view that minimum wages reduce employment of low-skilled workers, and as suggesting that the low-wage labor market can be reasonably approximated by the neoclassical competitive model. As we argue in subsequent chapters, however, the effect of the minimum wage on employment represents only one piece of the analysis necessary to assess whether minimum wages are a useful policy tool for improving the economic position of those at the bottom of the income distribution—which we believe is the ultimate goal of minimum wage policy and the criterion by which it should be assessed.

4 Minimum Wage Effects on the Distribution of Wages and Earnings

4.1 Introduction

The previous chapter reviewed the extensive literature on the employment effects of minimum wages, highlighting along the way research that focuses on the workers most directly affected by changes in the wage floor. In this chapter, we examine how minimum wage changes affect wages and earnings, both for low-wage workers and for workers higher in the wage distribution.

We have two major goals in this chapter. The first is to describe how minimum wages affect the wage distribution. Assuming reasonable levels of enforcement and compliance, the most obvious effect is to truncate or thin out the lower tail of the wage distribution (below the minimum) and to create a spike at the minimum. However, an increase in the minimum wage can also lead to changes in wages higher up in the wage distribution. These "spillovers" or "ripple effects" may arise for two main reasons.[1] First, if employers substitute away from the lowest-skilled workers and toward workers with somewhat higher skills in response to an increase in the wage floor—as is suggested by some of the evidence discussed in the chapter 3—then wages of workers earning above the minimum wage may be pushed up by the increase in demand for their services. Alternatively, if employers maintain wage differentials between their lowest-skilled workers and higher-skilled workers in order to create behavioral incentives, then a minimum wage increase may raise wages for higher-skilled workers as well.[2] The potential for such ripple effects is often cited by proponents of minimum wage increases as a way to raise the incomes of low-wage workers earning a little more than the minimum.[3] In addition, the influence of minimum wages on the wage distribution, coupled with the longer-run decline in the real value of the minimum wage, has spurred debate about the contribution of the minimum wage to the rise in wage inequality in the past few decades.

The effect of the minimum wage on the distribution of earnings is not as straightforward as its effect on the distribution of wages, and thus the second goal of the analysis in this chapter is to provide a more complete description of how the minimum wage influences labor income—both for workers directly affected by the minimum wage and for workers who might be indirectly affected via spillover effects. To this end, we present evidence on how minimum wages affect workers at different points in the wage distribution. In particular, this evidence examines a broader set of margins along which workers at different points in the wage distribution may be affected, including wages, employment, hours, and ultimately labor income.

Evidence on this broader set of minimum wage effects is potentially quite important in considering whether minimum wages achieve their policy goals. For example, even if some of the lowest-skilled workers most directly affected by minimum wages are hurt because they bear the brunt of the disemployment effects, spillovers to wages for slightly higher-skilled workers could still help poor families because, as we show in the next chapter, many minimum wage workers are not in poor families, and many somewhat higher-wage workers are in poor families. Thus, the evidence in this chapter is a prerequisite to our discussion of the effects of minimum wages on the distribution of family incomes, to which we turn in chapter 5.

4.2 Theory

The simplest neoclassical setting is one in which there are two labor inputs—for example, skilled and unskilled labor—and the minimum wage is the price of unskilled labor. As we showed in the previous chapter, in this setting a higher minimum wage reduces employment of the unskilled group. In contrast, the implication for employment of skilled labor is ambiguous. On the one hand, the scale effect implies lower use of all inputs, which would put downward pressure on the demand for skilled labor. However, as long as the two types of labor are substitutes, employers will substitute toward skilled labor, putting upward pressure on demand. If this substitution effect dominates, the overall demand for skilled labor will increase, resulting in a higher wage and higher employment of skilled labor. Thus, this simple setting indicates why an increase in the minimum wage can boost wages for workers already paid above the wage floor.[4]

Another version of this model treats skill as continuous rather than discrete, so as to better capture the fact that wage distributions are relatively smooth.[5] Assuming that there is only one type of skill—say, "human capital"—and that workers are distinguished only by how much human capital they have, the relevant price of labor is the price of a unit of human capital. In this model, the minimum wage simply truncates the wage distribution at the minimum wage, as any worker whose "hourly" human capital is below the minimum wage will not find employment. As before, employers substitute toward workers with more skill, which bids up the price of human capital. As a result, this model also predicts wage increases for workers above the minimum wage. However, in contrast to the observed distribution of wages, the model does not necessarily suggest that these increases will be more pronounced for workers earning only a little more than the minimum, nor does it predict the observed spike in the wage distribution at the minimum wage (e.g., Card and Krueger 1995a; DiNardo, Fortin, and Lemieux 1996; Dolado et al. 1996; Lemos 2004a).

In order to address these weaknesses, Pettengill (1981) develops a model in which there is a continuous distribution of worker skills and a labor market equilibrium characterized by an upward-sloping "wage curve" that relates wages to skill. Firms are assumed to hire workers into a variety of jobs that differ in the relationship between worker productivity and skill, with higher-skilled workers relatively more productive than lower-skilled workers in some jobs. The jobs for which productivity is more "sensitive" to skill will be filled by higher-skilled workers who command higher wages. This sensitivity of productivity to skill results in a greater degree of substitutability between workers whose skill levels are relatively close than between workers whose skill levels are quite different; for example, a high-skilled worker costs a lot more than a low-skilled worker, but is only marginally more productive in the tasks otherwise performed by low-skilled workers. As a result, the minimum wage raises the wages of workers who are close substitutes for the lowest-skilled workers by more than the wages of "farther" substitutes.

Pettengill also shows that it is straightforward to extend the model to generate a spike in the wage distribution at the minimum. In models in which there are a number of distinct skill groups, the spike arises naturally, as employment of workers directly affected by the minimum falls until the marginal revenue product of workers in that group rises enough that it is equated to their wage. Alternatively, with a skill continuum, if worker effort is endogenous, workers of low quality who would otherwise be nonemployed after the minimum wage increase may choose to adjust their effort upwards, resulting in workers of different initial quality having the same productivity and hence generating a spike at the minimum wage (Pettengill 1981).[6] In support of this possibility, Converse et al. (1981) report survey evidence indicating that about one-eighth of employers with minimum wage workers increased responsibilities of low-wage workers to offset the effects of a higher minimum—potentially consistent with increased effort.[7]

Finally, the spike in the wage distribution could be an artifact of downward adjustments of nonwage components of compensation for some workers such that the distribution of compensation does not have a spike but the wage distribution does. In general, however, the limited research on this question suggests that such offsets are not very important. For example, Simon and Kaestner (2004) report evidence from the 1979 NLSY indicating little or no effect of minimum wages on the provision of health or pension benefits. Royalty (2000) presents results that suggest different effects for small versus large minimum wage changes, although her estimates indicate that for the small minimum wage increases that constitute most of the changes in her sample period, a higher minimum is associated with greater health and pension benefits.[8] Simon and Kaestner discuss some of the potential impediments to adjusting benefits, including federal regulations that require some nontaxable benefits to be similar for low- and high-wage workers, and the high fixed costs of changing benefits. One possible exception to these findings is training, which we discuss in chapter 6.

Turning to models that embody monopsony behavior, Manning (2003) shows that in equilibrium search models in which monopsony power is generated by non-pecuniary firm characteristics, the spillover effects of a minimum wage increase should be concentrated among jobs paying just above the minimum. Intuitively, the higher minimum wage reduces the likelihood that workers in firms that previously paid below the new minimum wage will accept a job offer from the higher-paying firm and increases the likelihood that workers at the higher-paying firm will move to the lower-paying firm. This effect is stronger for employers paying wages nearer the minimum wage than for higher-wage firms. Thus, the labor supply curve facing firms paying above the minimum wage shifts in, but more so at lower wages, and hence becomes more elastic. This increase in the elasticity of the labor supply curve facing the higher-paying firm will induce that firm to raise its offer wage, as in the standard monopsony model.

Manning also illustrates how such models can be modified to generate spikes in the wage distribution. In the standard Burdett-Mortensen model (1998), spikes cannot arise, because a spike in the equilibrium wage distribution would induce employers to offer a slightly higher wage. However, Manning shows that this feature of the model is caused by the sharp discontinuities in the labor supply functions facing each firm, and that allowing for some mobility costs is sufficient to eliminate these discontinuities and generate a spike in the wage distribution.

Flinn (2002) develops a structural search model that incorporates wage bargaining, and also generates both a spike in the wage distribution at the minimum and spillover effects to wages above the minimum. Flinn then uses the model to derive implications for the effects of minimum wages on welfare that are testable from evidence on changes in the wage distribution induced by minimum wage increases. Not surprisingly, the tests are highly reliant on the assumed model structure. Nonetheless, even in the highly stylized labor market assumed by Flinn, observed changes in wage distributions that might be viewed as beneficial are not necessarily welfare-enhancing. For example, based on the welfare criterion he uses (the value of unemployed search), even if the wage distribution after the minimum wage increase first-order dominates the prior wage distribution, the minimum wage increase does not necessarily increase welfare, because looking at effects on the wage distribution and ignoring employment effects is misleading. At the same time, the model emphasizes that disemployment effects do not necessarily imply that welfare falls, because shifts in the wage distribution can also affect the value of unemployed search; in addition, however, an absence of employment effects does not necessarily imply that welfare increases.

We do not necessarily put much store in the real-world welfare implications that one can draw from these models and the tests to which they lead. But this type of model helps to emphasize the point that one must look at effects both on the wage distribution and on employment to learn how workers are affected by minimum wages. Similar effects on the wage distribution can be associated with quite different employment effects, and hence have different implications for whether workers are helped or hurt by the minimum (in terms of total earnings, if not welfare). For example, in the neoclassical model, workers earning above the minimum wage experience wage increases because of increased demand for them, and hence are made better off as long as the substitution effects dominate the scale effects. But spillover effects can also arise in non-neoclassical models in which wages influence motivation or effort, and in these models the effects of a minimum wage increase on skilled workers are more complicated and potentially more adverse.

An early and well-known version of the latter type of model is developed in Grossman (1983). In the simplest version of this model, there are two types of labor—skilled and unskilled. The minimum wage is binding only for unskilled labor, and unskilled labor has no ability to vary its effort. Thus, unskilled labor is treated as in the neoclassical model. Skilled labor, however, can choose the amount of effort supplied, and this effort depends both on the real wage earned by skilled labor and on the wage for skilled labor relative to the wage for unskilled labor. In her model, output is fixed at Q (the implications of varying output are discussed shortly), so that the firm chooses the number of unskilled workers, the number of skilled workers, and the wage of the latter to minimize the cost of producing Q.[9] This is effectively an efficiency wage model in which firms set the wage for skilled workers to minimize the cost per efficiency unit of skilled labor.

What happens in this model when the minimum wage is increased? First, there is a standard substitution effect that leads firms to substitute skilled for unskilled labor. However, because the higher minimum wage also reduces the relative wage of skilled labor, firms must raise the wage for skilled labor to elicit the same level of effort as before. As a result, the wage for skilled labor rises by more than in the standard neoclassical model, reflecting both the standard increase in relative demand for skilled labor and the lower supply of efficiency units of skilled labor. In addition, the sharper wage increase for skilled labor leads the firm to choose an input combination more intensive in low-skilled labor, so that unskilled employment falls by less than in the neoclassical model and skilled employment increases by less. If we also allow firms to adjust production, it becomes more likely that skilled employment will fall as a result of the minimum wage increase, because the negative scale effect on skilled worker employment is augmented by the relative wage effect.

This model illustrates that spillovers from the minimum wage to the wages of workers higher up in the wage distribution do not necessarily make those workers better off. Intuitively, if the minimum wage pushes up wages of workers higher in the wage distribution for reasons other than the standard substitution effect, employment declines among the latter workers become more likely.[10] This is yet another reason why evidence only on what minimum wages do to the *wage* distribution can be uninformative about whether minimum wage increases yield benefits for workers higher up in the wage distribution.

4.3 Effects of Minimum Wages on the Wage Distribution

4.3.1 Effects on Low-Wage Workers, and Spikes in the Wage Distribution

Studies of the employment effects of minimum wages often first verify that minimum wages are binding by showing that the same research design used to test for employment effects also detects positive effects on wages of affected workers. If no such wage effects are detected, then it is likely either that the research design is flawed or that the minimum wage is not binding. However, most of the studies we considered in our lengthy review of the employment effects of minimum wages (Neumark and Wascher 2007a) report positive effects on wages, suggesting that the minimum wage has a noticeable effect on the lower tail of the wage distribution.[11]

Other papers test more explicitly for the types of effects discussed previously—namely, the creation of a spike at the minimum wage, and the bidding up of wages of workers earning above the minimum. Focusing first on the existence of spikes, a very simple type of evidence is provided by detailed histograms of the wage distribution. For example, in Neumark and Wascher 1992, we present evidence of spikes corresponding to state minimum wages and subminimum wages for teenagers in the United States. Similarly, Card and Krueger (1995a, chapter 9) illustrate how spikes in the wage distribution followed increases in the federal minimum wage in 1990 and 1991, while Baker, Benjamin, and Stanger (1999) document the existence of spikes at provincial minimum wages in Canada for fifteen- to nineteen-year-olds. More formally, DiNardo, Fortin, and Lemieux (1996) present nonparametric kernel estimates of the density of hourly wages for men and women in the United States from 1973 to 1992, a period in which there were numerous increases in the federal minimum wage. The concentration of observations at or near the minimum wage is particularly apparent in the earlier years of the sample, when the minimum wage was higher in real terms.[12] Lee (1999) presents similar evidence for the 1980s, noting that the decline in the value of the minimum during that decade was associated with an increase in the dispersion of wages.

In contrast, Dickens and Manning (2004a) examine Labour Force Survey (LFS) data on adults aged twenty-two and older subject to the minimum wage introduced in the United Kingdom in 1999 (a lower minimum wage was introduced for eighteen- to twenty-one-year-olds). They report evidence showing that many workers whose wages were below the new minimum wage prior to 1999 did not see their wages adjusted all the way up to the new minimum after it was introduced. Although this result might be read as suggesting low compliance with the new minimum wage law in the United Kingdom, it more likely reflects measurement problems. In particular, when wages are constructed from data on earnings and hours rather than observed directly, errors may obscure evidence of a spike in the wage distribution at the minimum wage if, as is typically the case in developed countries, it is specified as an hourly wage. This is not much of a problem in the United States, because a high fraction of teenagers report earnings on an hourly basis in the CPS (96 percent in 2005, versus 60 percent of all workers). In contrast, Dickens and Manning (2004b) report that only about 40 percent of U.K. workers report an hourly wage in the LFS. As confirmation that use of a constructed wage can obscure evidence of a spike in the wage distribution at the minimum wage, they find much greater evidence of such a spike in a dataset that elicited direct information on hourly wages in the low-wage residential homes sector. In particular, the proportion paid below the minimum wage in this sector dropped sharply when the minimum wage was imposed (by about 0.27, from a level of about 0.31 to 0.04), and the proportion paid exactly the minimum rose sharply (by 0.24, from 0.06 to a level of about 0.3).

Stewart and Swaffield (2002) examine data from the British Household Panel Survey (BHPS) and also find that the introduction of the minimum wage in the United Kingdom resulted in a spike at that point in the wage distribution. In particular, based on a direct question on the basic hourly rate for hourly workers that was added to the BHPS just after the minimum wage was implemented, they find that 12 percent of workers were paid the minimum.[13] The same wave of the BHPS also asked workers some direct questions about wages. With reference to spikes in the wage distribution, workers were asked whether their "pay or hourly wage rate . . . has been *increased* to bring you up to the National Minimum Wage." Of those who responded, 4.6 percent responded "yes," and of these, 60 percent reported an hourly wage of exactly the new minimum.

Thus, the evidence clearly indicates that in industrialized countries the minimum wage raises wages of the lowest-skilled workers and creates a spike in the wage distribution at the minimum. In contrast, labor markets in developing countries are often characterized by serious noncompliance or have a large informal sector in which the minimum wage does not apply. For example, even though the Harrison and Scorse (2005) study of Indonesia finds that minimum wages boost wages of the unskilled, the authors also report that increases in the statutory minimum wage during the mid-1980s through the 1990s were accompanied by declines in the proportion of plants paying at least the minimum wage. In addition, they present results indicating that minimum wages did more to raise wages in industries and sectors that were targets of antisweatshop activism and U.S. government pressure (export-oriented or foreign-owned plants producing textiles, footwear, and apparel), which suggests that noncompliance was a problem in industries not subject to such pressure.

Among other developing countries, Gindling and Terrell (2005, 2007b) generally find spikes in the wage distribution generated by minimum wages in the covered sector in both Costa Rica and Honduras, but not in the uncovered sector.[14] In a more extensive survey, kernel density plots for the late 1990s show a spike at the minimum wage in the formal sector in many Latin American and Caribbean countries, but not for all of them—including, for example, Argentina, Jamaica, Mexico, and Uruguay (World Bank 2006).[15] To some extent, the absence of a spike may reflect the very low level of the minimum wage in these countries. But the World Bank report also suggests that enforcement and compliance in these countries is quite low.

Another interesting feature of the relationship between the minimum wage and the wage distribution in developing countries—illustrated for many countries in the World Bank report and in a variety of studies of Brazil (most recently Lemos 2006a)—is that the minimum wage also leads to a spike in the wage distribution in the informal sector. The existence of a spike in the informal sector is hard to explain in a neoclassical model, but the World Bank report suggests that employers in the informal sector who compete for workers with employers in the formal sector tend to keep their wages at the minimum wage that applies to the formal sector. The report characterizes this tendency as either reflecting fairness considerations or as an efficiency wage effect in which firms pay a wage above the market-clearing wage so that workers do not leave their jobs to seek employment in the covered sector (see, e.g., Akerlof 1982, and Shapiro and Stiglitz 1984). Of course, another possibility is that workers are misclassified by sector, although given that the spike is sometimes as pronounced in the informal as in the formal sector, this seems unlikely to be the full explanation. Explaining the spike in the wage distribution in the informal sector in developing countries strikes us as an interesting unexplained puzzle that may ultimately prove informative about both minimum wage effects and labor market behavior more generally.

4.3.2 Spillovers

Evidence of a spike in the wage distribution at the minimum wage is not really surprising, nor is it a topic of much controversy in the policy debate. Workers whose wages are bound by the minimum, and who retain their jobs, no doubt experience a wage increase. However, as we emphasized earlier in this chapter, the potential for minimum wage increases to affect wages higher up in the wage distribution is also important in assessing the effects of minimum wage policy.

Grossman's (1983) paper appears to have been the first to attempt to directly estimate the spillover effects of minimum wages.[16] In particular, she uses data from BLS Area Wage Surveys (AWS) to examine the influence of increases in the federal minimum wage on wages in low-wage occupations that nonetheless pay above the minimum wage. Her data cover sixteen Standard Metropolitan Statistical Areas and nine low-wage non-manufacturing occupations from 1960 to 1975.[17] The AWS data include information on wage ranges within occupations, and in order to focus on spillover effects, Grossman does not include occupations for which some wages fell in the same range as the minimum wage. For each occupation, she then regresses average occupational wages on the contemporaneous and lagged minimum wage (as well as leads, which turn out to be insignificant), to obtain estimates of effects of minimum wages on occupational wages in both the short run and the longer run. In the short run (out to one quarter), the effects of the minimum wage are positive for some occupations, and generally more so for the lower-paying occupations, consistent with minimum wages increasing wages more for workers paid just above the minimum. The longer-run effects are less clear, growing for some occupations and dissipating for others. In general, though, the estimates are quite imprecise.

Most of the subsequent research on spillover effects uses a more conventional analysis of the effects of minimum wages on the wages of workers at various points in the wage distribution other than the minimum wage. For example, Card and Krueger (1995a, chapter 9) focus on the effects of the 1990 and 1991 increases in the federal minimum wage on the 5th and 10th percentiles of the wage distribution. Recognizing that the effects may differ across states due to differences in the level of wages prevailing prior to the increases (in part because of higher state minimum wages), they regress the change in the 5th or 10th percentile wage from 1989 to 1991 on the fraction affected by the minimum wage increase (i.e., the fraction in 1989 who were below the minimum prevailing in 1991), plus other controls. These regression estimates indicate positive and significant effects of the fraction affected variable that are larger at the 5th than at the 10th percentile. This analysis, however, does not directly address the spillover question, because workers at the 5th percentile (and perhaps even at the 10th percentile in low-wage states) can be minimum wage workers,[18] in which case these estimates would include both the effects of the minimum wage on the spike in the wage distribution at the minimum and spillover effects above it. Card and Krueger also present results at the 25th percentile (as well as the 50th and 90th percentiles). They find no evidence of an effect at the 25th percentile, with an estimated coefficient of zero in the specification with controls.[19] This evidence suggests that spillover effects did not extend that high up in the wage distribution, but it does not rule out the possibility of spillovers to wages a little lower in the distribution.

Another means of estimating spillover effects is to examine how minimum wage increases influence differences between various points in the wage distribution. Lee (1999) presents such a method using the gap between the 10th and 50th percentile of the wage distribution, and Manning (2003) shows how Lee's model, with additional assumptions, can be parameterized to characterize spillover effects more generally.[20] In particular, if we assume that $w^*(F)$ is the unobserved or latent log wage distribution and $w(F)$ is the observed log wage distribution—meaning that these functions give the log wage at percentile F of the wage distribution—then the spillover effects of the minimum wage can be estimated by comparing the densities of the two distributions for intervals above the minimum wage. For example, if there are no spillover effects and full compliance (and no disemployment effects), then wages that would be below the minimum in the latent wage distribution will simply pile up at the minimum, so that

$$w(F) = w^*(F) + \max(w_m - w^*(F), 0), \tag{4.1}$$

where w_m is the log of the minimum wage. Rewriting this expression as $w(F) - w^*(F) = \max(w_m - w^*(F), 0)$ yields the direct effect of the minimum wage on the wage distribution (paralleling the spike created by the minimum, discussed earlier).

To extend the model to capture indirect effects of the minimum wage on wages, Manning (2003) considers the expression for the spillover at percentile F:

$$s(F) = \frac{w_m - w^*(F)}{1 - e^{-\beta[w_m - w^*(F)]}} - \max(w_m - w^*(F), 0), \quad \beta > 0. \tag{4.2}$$

In this equation spillover effects are proportional to $(w_m - w^*(F))$ on either side of the minimum wage. The largest spillovers are assumed to occur in the neighborhood of the minimum wage; in particular, when $(w_m - w^*(F))$ is very small, $s(F)$ is close to $1/\beta$, so that $1/\beta$ is the maximum spillover.[21] The model can be estimated if a parameterization of the latent wage distribution is chosen.

Manning chooses a standard log normal distribution for the latent wage distribution, with a scale parameter set equal to the median log wage and a dispersion parameter that is estimated. He then estimates the model using observed percentiles (below the median) of the U.S. wage distribution by state and year. For the 1979–2000 period, the model estimate of β is 8.88, implying a maximum spillover of about 11 percent (for those whose wage, absent the minimum, would be right near the minimum).

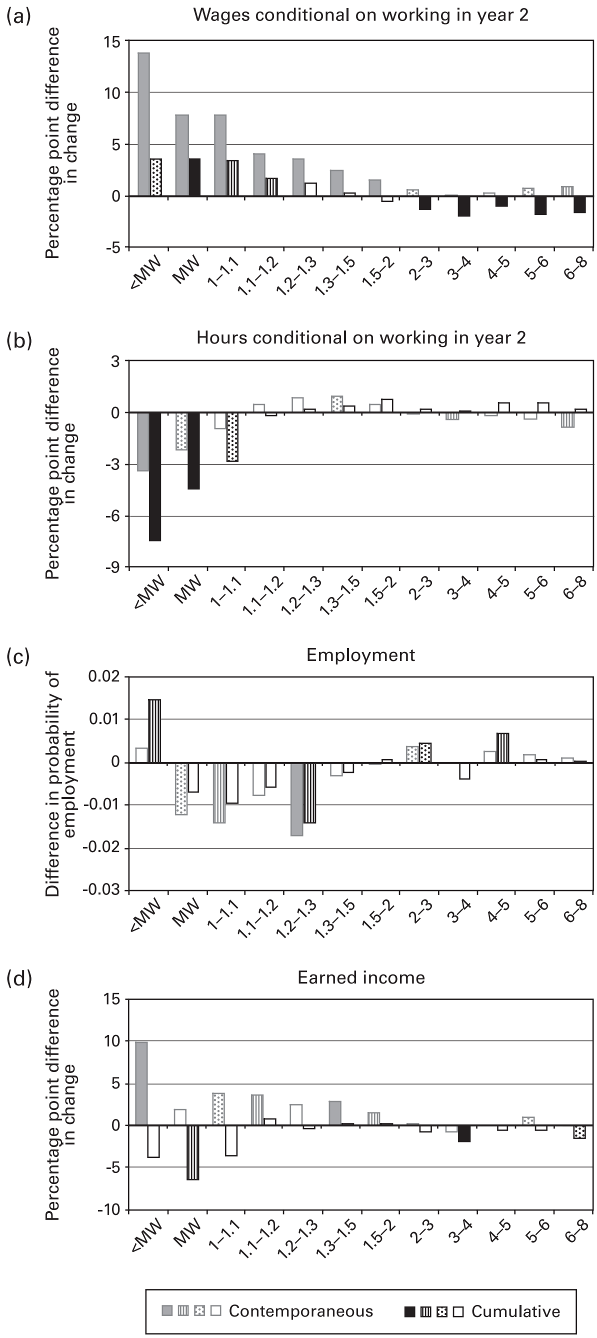
Based on the model estimates for the latent wage distribution, Manning can also compute the implied spillover effect at each point of the wage distribution as the difference between the observed wage and the latent wage. Both the direct model estimates and these calculations suggest that spillovers decline from about 11 percent of the minimum wage for workers whose wages would be very near the minimum, to about 3 percent for wages about 25 percent above the minimum. In addition, the estimates indicate that wages further up the wage distribution (beginning at wages only 50 percent higher than the minimum) may decline; although it may seem unlikely that wages this distance from the minimum would decline, some evidence for the United States (discussed later in this chapter) also points to slight negative spillover effects higher up in the distribution.

One potential shortcoming of this procedure is that spillover effects cannot always be distinguished from disemployment effects. In particular, if some very low-wage workers lose their jobs in response to a minimum wage increase, then the wage at any given percentile higher up in the wage distribution will increase even if no employed worker experiences a wage change. In that sense, estimates of spillover effects based on changes in the percentiles of the wage distribution should be viewed as upper-bound estimates of spillover effects, as these percentiles would be lower if the disemployed were still working.

An alternative, and arguably more informative, approach is to directly estimate the effects of increases in the minimum wage on the wages of workers who were already earning more than the minimum wage. Our paper with Mark Schweitzer (Neumark, Schweitzer, and Wascher 2004), discussed earlier in the context of the estimation of employment effects, takes this approach. In particular, rather than parameterizing the latent wage distribution, we instead use workers in states where the minimum wage did not increase as controls for workers at the same position in the wage distribution (relative to minimum wages) in states where the minimum wage did rise. As we noted in chapter 3, this approaches enables us to estimate the effects of minimum wage increases on outcomes at various points of the wage distribution, allowing for both contemporaneous and lagged effects.

The estimates for wages are summarized in graphical form in figure 4.1a, with the coefficient estimates transformed to show the effects of a 10 percent increase in the minimum wage.[22] In particular, the graph displays the estimated differential between the percentage change in the wage experienced by workers in states with a 10 percent minimum wage increase and workers in states without an increase, at comparable points of the initial wage distribution.[23] The gray bars are the contemporaneous effects, and the black bars incorporate the lagged effects of minimum wages; the bars are filled in differently to indicate statistical significance, as explained in the notes to the figure.

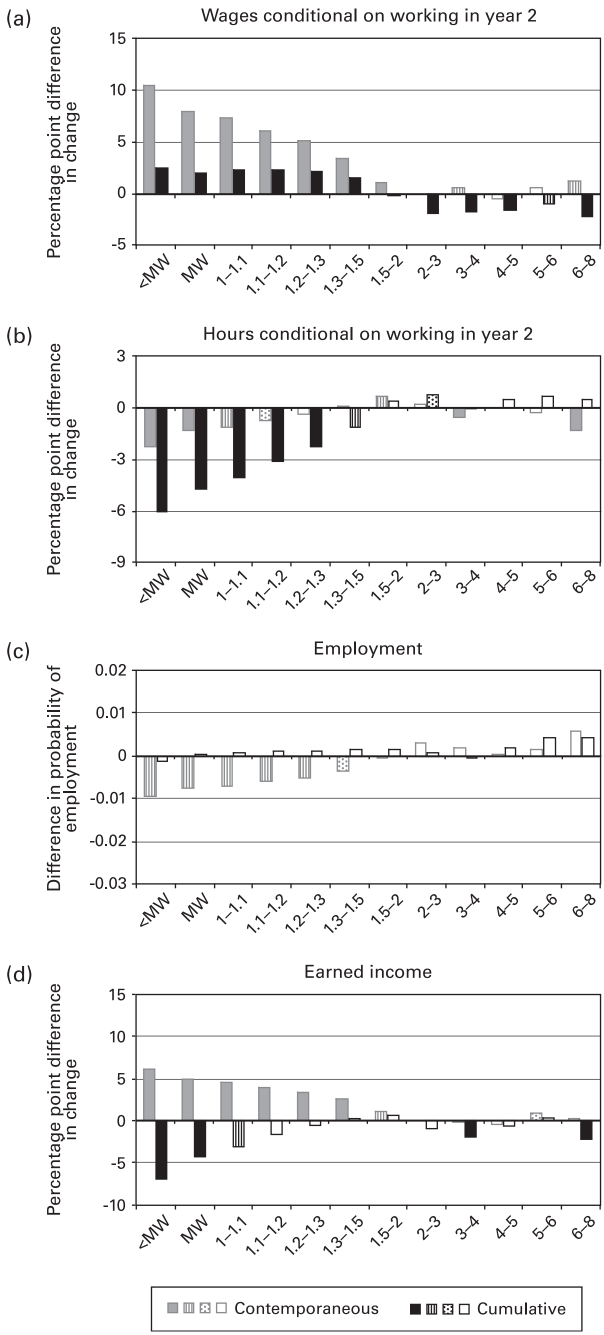
The contemporaneous estimates reveal pronounced, statistically significant positive spillover effects on wages near the minimum wage. In particular, for workers with a wage less than 10 percent above the minimum, the elasticity of wages with respect to the minimum is about


(a)
Wages conditional on working in year 2
Percentage point difference in change
15
10
5
0
-5
<MW
MW
1–1.1
1.1–1.2
1.2–1.3
1.3–1.5
1.5–2
2–3
3–4
4–5
5–6
6–8
(b)
Hours conditional on working in year 2
Percentage point difference in change
3
0
-3
-6
-9
(c)
Employment
Difference in probability of employment
0.02
0.01
0
-0.01
-0.02
-0.03
(d)
Earned income
Percentage point difference in change
15
10
5
0
-5
-10
Contemporaneous
Cumulative

0.8.[24] The elasticity falls to about 0.4 for workers with a wage between 10 and 30 percent above the minimum, to about 0.25 for workers with a wage between 30 and 50 percent higher than the minimum, and 0.15 for workers earnings between 1.5 and 2 times the minimum wage. These contemporaneous spillovers appear to peter out higher in the wage distribution.

The black bars, which incorporate lagged effects, tell a somewhat different story. Most important, for workers with an initial wage only a little above the minimum wage, the elasticity with respect to the minimum falls to about 0.4. The estimated elasticities then decline for the wage cells that are slightly higher in the wage distribution, and become negative, albeit small, for workers paid more than twice the minimum; note that this latter result has some parallels to Manning's findings, although the estimates here set in more strongly at points higher in the wage distribution. The differences between the estimated contemporaneous effects and the full effects that include lags suggest that a substantial part of the wage gains associated with an increase in the minimum wage are "given back" in the following year. These give-backs have generally not been noted in the previous literature on the effects of minimum wages on the wage distribution, which have not incorporated lags. However, they probably should not be surprising. Employers who adjust their wage structure in response to a minimum wage increase may elect to reduce the usual nominal wage increase for workers earning near the minimum the following year, while employers in states where the minimum wage did not increase may continue to raise pay at a steadier pace.[25]

Figure 4.1
Effects of a 10 percent minimum wage increase, dummy/spline specification
Source: Neumark, Schweitzer, and Wascher 2004.
Note: The data used in this analysis are taken from matched monthly CPS ORG files, from 1979–1997. The estimates come from regression models of the form of equation (3.3), which specify the percentage change in the dependent variable (except for employment rates, where the model is for subsequent employment conditional on initial employment) as a function of the percentage change in the minimum wage interacted with indicator variables for a worker's wage relative to the minimum wage prior to the current period's increase in the minimum. The estimated effects in the ranges captured by these indicators are reported in the graphs. The indicators themselves are included, as are other controls intended to capture changes in wages or other outcomes at different points of the wage distribution, as well as demographic and skill-related controls, month dummy variables, and state-by-year interactions. Significance levels for two-sided tests are as follows: 1 percent = solid; 5 percent = striped; and 10 percent = dotted. These are computed from bootstrapped standard errors.


(a)
Wages conditional on working in year 2
Percentage point difference in change
15
10
5
0
-5
<MW
MW
1–1.1
1.1–1.2
1.2–1.3
1.3–1.5
1.5–2
2–3
3–4
4–5
5–6
6–8
(b)
Hours conditional on working in year 2
Percentage point difference in change
3
0
-3
-6
-9
(c)
Employment
Difference in probability of employment
0.02
0.01
0
-0.01
-0.02
-0.03
(d)
Earned income
Percentage point difference in change
15
10
5
0
-5
-10
Contemporaneous
Cumulative

We have also computed estimates of a modified version of equation (3.3) that imposes more smoothness in how the minimum wage effects vary across the wage distribution. We do this by allowing the effects of minimum wages to vary along a higher-order (seventh-order) polynomial in the ratio of the minimum wage relative to the wage. We then take the average effects implied by this specification, in the same ranges used in figure 4.1. These estimates are reported in figure 4.2, and the conclusions are qualitatively similar, although, as shown in figure 4.2a, with less drop-off in the wage elasticity up to 150 percent of the minimum wage for the longer-run effects.

Taken as a whole, this evidence suggests that minimum wages do generate some spillover effects on the wage distribution. However, the estimates that account for longer-term adjustments indicate that these effects probably extend only to those previously earning 20 or 30 percent above the minimum. Roughly speaking, these estimates are in line with those calculated by Manning using a quite different approach.

Some research for the United Kingdom has also looked for evidence of spillovers to wages above the wage floor. For example, in their analysis of wage data from their survey of residential care homes, Dickens and Manning (2004b) implement a procedure very similar to Manning's analysis of the U.S. data described earlier. A key difference, however, is that instead of using a parameterized latent wage distribution, they assume that $w^*(F)$ matches the observed wage distribution in the period just prior to the introduction of the new national minimum wage and compare it to the observed wage distribution five months after the new minimum wage took effect. Because changes in the observed wage distribution may have been affected by general wage growth as well as by spillovers associated with the introduction of the new minimum wage, their more reliable estimates are probably those that are calculated relative to across-the board wage growth (i.e.,

Figure 4.2
Effects of a 10 percent minimum wage increase, polynomial specification
Source: Neumark, Schweitzer, and Wascher 2004.
Note: The data and models are as described in the notes to figure 4.1, except that the effects of minimum wages are constrained to vary along a seventh-order polynomial in the ratio of the minimum wage relative to the earned wage; we then take the average effects implied by this specification for the same ranges shown in figure 4.1. Significance levels for two-sided tests are as follows: 1 percent = solid; 5 percent = striped; and 10 percent = dotted. These are computed from bootstrapped standard errors.

the wage growth that was common to all percentiles of the wage distribution). Dickens and Manning also report estimates that allow an additional four months to pass (for a total of nine months) after the introduction of the minimum wage, to see whether spillover effects strengthened or weakened over time.

They generally find small spillover effects in these data. In a specification that does not allow for general wage growth and that uses the observed distribution in the five months after the minimum wage was introduced, the model estimates a spillover effect of 6 percent for wages just above the minimum wage (this compares to the 11 percent estimate of $1/\beta$ discussed earlier). After subtracting an estimate of general wage growth, the estimated spillover effect falls to just 2.4 percent. When the period of analysis is lengthened to nine months, the first estimate rises to 7 percent, and the second falls to only 1 percent. A related analysis of changes in average wages at the firm level shows that average hourly wages in the industry rose by no more than was necessary to bring workers below the minimum wage into compliance. Together, the evidence leads them to conclude that there were "virtually no spillover effects"[26] in the residential care sector following the introduction of the new minimum wage (Dickens and Manning 2004b, C100).[27]

The research literature on spillovers in developing countries—which is limited primarily to Latin America—includes some puzzling results. In particular, much of the empirical evidence suggests that minimum wages have a noticeable effect on wages very high up in the wage distribution. Evidence for a number of countries is summarized in the 2006 World Bank report cited earlier; in addition, several papers explore this issue for Brazil. For example, Lemos (2004a) reports positive effects of the minimum wage on wages at the median of the distribution. In addition, when she tests for spillovers separately in the formal and informal sectors (Lemos 2006a), she finds positive effects up to the 90th percentile in the informal sector and up to the median in the formal sector, even though the minimum wage is between the 5th and 10th percentile of the wage distribution in the formal sector and between the 15th and 30th percentiles in the informal sector (2006a, 6). Furthermore, the magnitudes are not small. For example, the estimates imply that a 10 percent increase in the minimum wage raises wages in the formal sector by 3.3 percent at the 25th percentile and by 1 percent at the median; similarly, the estimates for the informal sector are 3.1 percent at the 25th percentile, 4.8 percent at the median, and 0.8 percent at the 90th percentile. Fajnzylber (2001) uses the approach of Neumark, Schweitzer, and Wascher 2004 and reports even larger effects, with significant spillover effects from minimum wage increases evident for those earning as much as *forty times* the minimum wage.[28] However, we view this result with a great deal of skepticism.

Part of the problem with this evidence, we suspect, is that much of it comes from highly inflationary periods (especially for Brazil).[29] For example, because the nominal minimum wage was raised so frequently during this period, it effectively became the "numeraire" for wages, with other workers' wages set as a given number of multiples of the minimum wage (Fajnzylber 2001; World Bank 2006). Such numeraire effects are not spillovers in the traditional sense, but rather a means of indexing wages to inflation. The inflationary environment is also problematic because of the use of real wage and real minimum wage measures in this study. In particular, high and variable inflation can lead to a strong but spurious positive correlation between the real minimum wage variable and the real wage variable because of the extraordinary amount of nominal variation in the denominator.[30] For this reason, Lemos (2004a) explores a number of minimum wage variables and argues that those measuring the fraction of workers below the minimum wage are more appropriate because they do not suffer from this problem. Finally, consistent with our suspicion that the estimates of spillovers in Brazil are contaminated by high inflation, Neumark, Cunningham, and Siga (2006) revisit the Brazilian case using data beginning only in 1996—after the country's hyperinflation ended—and find no evidence of positive effects on wages above the 10th percentile in the formal sector or above the 20th percentile when the two sectors are combined.[31] In any event, considerably more work is needed to obtain reliable evidence of spillover effects in developing countries.

4.3.3 The Contribution of Minimum Wages to Increases in Wage Inequality in the United States

Given the evidence that minimum wages create spikes in the wage distribution at the minimum, as well as the evidence of spillover effects on wages higher up in the distribution, it was only natural for economists to explore the role of minimum wages in the trend towards greater inequality in U.S. wages that has been the subject of so much research in labor economics in recent years. The initial burst of research on wage inequality focused on periods that extended through much of the 1980s, when the real value of the federal minimum wage declined sharply, and included studies by Blackburn, Bloom, and Freeman

(1990), Bound and Johnson (1992), Katz and Murphy (1992), Juhn, Murphy, and Pierce (1993), and Levy and Murnane (1992).

The consensus in most of this early literature was that the changes in wage inequality were driven primarily by increased demand for skilled workers (on both observed dimensions of skill, such as schooling, as well as on unobserved dimensions of skill). This increased demand for skills was thought to be driven by technological change, although this interpretation was, to a large extent, a "residual" explanation for the widening in the wage structure that remained after accounting for changes in the supply of workers at different skill levels. The skill-biased technical change (SBTC) hypothesis, as it is known, was bolstered by studies that looked more explicitly at the relationship between wages and the use of technology in the workplace (e.g., Berman, Bound, and Griliches 1994; Krueger 1993).[32]

However, this consensus subsequently came under attack. Although earlier research by Blackburn, Bloom, and Freeman (1990) identified institutional changes—including the decline in the real value of the minimum wage and the decline in unionization—as contributing to the rise in inequality in the 1980s, these institutional changes were generally considered less important than the changes in the demand for skills. By the mid-1990s, however, some researchers were claiming that the increase in wage inequality was instead *primarily* attributable to institutional changes in labor markets, including the declining real minimum wage.

Two influential studies, in particular, claimed that the minimum wage was an especially important factor in the rise in wage inequality during the 1980s. The first, by DiNardo, Fortin, and Lemieux (1996), used non-parametric density estimation of wage distributions to decompose changes in various measures of between-group and within-group wage inequality into the portions associated with changes in the minimum wage, changes in unionization, changes in individual attributes, supply and demand influences, and a residual category not explained by any of these factors.

The basic strategy of DiNardo, Fortin, and Lemieux requires constructing counterfactual, unobserved wage densities that capture how the distribution of wages would have evolved absent a particular change—such as the decline in the value of the real minimum.[33] They construct these counterfactual distributions by reweighting the data to ask what the distribution would look like if, for example, the share of workers who were unionized in 1988 was the same as in 1979, but the

1988 wage structure for union and nonunion workers prevailed. With respect to demand and supply changes, the approach is a bit more complicated and requires positing a model of how changes in the supply of and demand for workers in particular education/experience/gender categories would have affected wages. Thus, we leave the details regarding this part of the decomposition to the paper.

The part of the decomposition pertaining to the contribution of minimum wage effects also depends on some specific assumptions. First, DiNardo, Fortin, and Lemieux assume that there are no spillover effects, so that the effects of a higher minimum wage (which is estimated from the counterfactual that the value of the minimum wage in 1988 is equal to its real value in 1979) are limited to bringing more workers up to the higher minimum. To the extent that there are spillover effects in the United States, this assumption leads to an underestimate of the contribution of the falling minimum wage to the rise in wage inequality.

Second, the authors assume that an increase in the minimum wage has no effects on employment. They note that allowing for disemployment effects would cause there to be fewer workers at low wage levels, so that, again, this assumption is conservative—that is, with disemployment, minimum wages would have more of an equalizing effect. However, when measuring wage inequality, individuals who are left without a job because of a higher minimum wage should arguably be assigned a zero wage (or some measure of the value of their nonworking time) rather than ignored.[34] Indeed, the general tendency for this line of research to ignore those who do not work because of the minimum wage suggests that evidence on the effects of minimum wages on wage inequality says very little about their effect on economic well-being.[35]

Putting this shortcoming aside for the moment, their results indicate that the minimum wage played a major role in the increase in inequality over the 1980s, especially among women. For example, for men they estimate that of the 0.195 increase in the 90/10 difference in log wages (i.e., the difference between the 90th and 10th percentiles), 25.3 percent was due to the fall in the minimum wage. Of the 0.076 increase in the 50/10 differential, 65.7 percent is attributable to the minimum wage, while the minimum wage did not contribute at all to the 0.119 increase in the 90/50 differential. That is, these estimates indicate that the minimum wage played a major role in the increase in "lower-tail" wage inequality. For women, the 90/10 and 50/10 differentials rose much more over this period—by 0.328 and 0.243, respectively—and DiNardo, Fortin, and Lemieux (1996) attribute 45.1 percent of the increase in the 90/10 differential to the minimum wage, and 61.7 percent of the increase in the 50/10 differential.[36] As for men, the minimum wage did not contribute to the rise in inequality among women as measured by the 90/50 differential. The conclusions are much the same for between-group inequality measures based on schooling or experience, indicating that for both men and women the decline in the real value of the minimum wage contributed to growing wage gaps between more- and less-educated workers and more- and less-experienced workers.

A second study arguing that minimum wages were central to the changes in wage inequality over the 1980s is Lee (1999), who also takes account of regional variation in minimum wages. Building on the framework in equations (4.1) and (4.2), which link the latent wage distribution to the observed one, Lee estimates regression models for the 50/10 differential of the form

$$\log(w_{jt}^{50}) - \log(w_{jt}^{10}) = \alpha_t + \beta \cdot \{\log(w_{jt}^{\min}) - \log(w_{jt}^{med})\} + \lambda \cdot \{\log(w_{jt}^{\min}) - \log(w_{jt}^{med})\}^2 + \varepsilon_{jt}, \tag{4.3}$$

where the 10 and 50 subscripts refer to the 10th and 50th percentiles, $w^{\min}$ is the minimum wage (higher of state and federal), and w^{med} is the median wage; and j and t denote state and year, respectively.

Lee shows that the linear and quadratic minimum wage terms in this equation capture in a simple way the effects of the minimum wage on the wage distribution in the first term of equation (4.2). The year dummy variables, then, capture changes in unobserved, or latent, inequality—in this case, in the 50/10 differential. Based on estimates of this equation, Lee concludes that the observed increase in the 50/10 differential from 1979 to 1988 was largely due to the decline in the real value of the minimum wage. Indeed, performing the analysis for men and women separately, and for the over-sixteen, eighteen to sixty-four, and twenty-five to sixty-four age groups, Lee concludes that with the exception of men aged twenty-five to sixty-four, there is *no* evidence of an increase in latent inequality; that is, the entire increase in the 50/10 differential is attributed to the declining minimum wage. Thus, these results are a sharp contrast to the SBTC hypothesis, which viewed much of the increase in inequality in the bottom half of the wage distribution as related to increased relative demand for more-skilled workers.

Lee explores a good deal of other evidence as well. First, he looks at the 1989–1991 period, when the federal minimum wage increased twice. Following the strategy used by Card and Krueger (1995a), he exploits the differential variation across states created by the fact that some states already had a minimum wage that was above the federal minimum, so that the model is identified from variation in the minimum wage that is primarily produced by legislated minimum wage changes rather than by the erosion of the nominal minimum wage. For women, he reports that the roughly 16 percent increase in the minimum wage resulted in a 0.032 log point drop in the 50/10 differential, largely consistent with his core analysis over the longer sample period. In contrast, he finds little evidence of an effect for men.

In addition, Lee applies his core analysis to upper-tail wage inequality (such as the 90/50 or 80/50 differential), and for most permutations of the sample and estimating equation, finds curious evidence that an increase in the effective minimum wage appears to increase upper-tail wage inequality, despite the fact that "we are reasonably confident that the minimum has no effect" (1999, 1002) in the upper tail. His take on this evidence is that only the findings from the permutations that do not show an effect on upper-tail inequality are compelling. One could argue, instead, that the apparent evidence of effects of minimum wages on upper-tail inequality casts doubt on whether he is identifying minimum wage effects. Nonetheless, despite these potential problems, Lee attributes most of the increase in lower-tail inequality in the period to changes in the minimum wage, suggesting that its real decline explains about 70 percent and 25 percent, respectively, of the growth in the 50/10 and 50/25 differentials over this period for men, and nearly all of the growth in lower-tail inequality for women.

The findings of the DiNardo, Fortin, and Lemieux (1996) and Lee (1999) studies have fueled what Autor, Katz, and Kearney (2005) term a "revisionist" perspective on the sources of rising wage inequality over recent decades. This perspective, which downplays the importance of increased demand for skilled workers and emphasizes to a greater extent the role of institutions, is best captured in a paper by Card and DiNardo (2002). In particular, Card and DiNardo suggest that the rise in inequality was an "episodic event" largely concentrated in the period 1979 to about 1985 and thus inconsistent with the relatively steady increase in demand for skilled workers suggested by the SBTC hypothesis. Instead, they argue that a "primary candidate" for the rise in inequality during this period was the fall in the real value of the minimum wage (774). The evidence they present shows that the actual time-series of the 90/10 wage gap is predicted very well by a simple regression of this differential on the log of the real minimum wage—capturing 90 percent of the variation and predicting some of the turning points in the series as well. The authors do not, of course, argue that the minimum wage explains all changes in the wage structure; they also suggest that changes in unionization are important, and note that the declining gender wage gap must be attributable to other sources, because women's wages are more heavily influenced by the minimum wage than are men's wages. Nonetheless, they clearly favor the importance of the minimum wage over changes in relative demand for skilled workers, arguing that, based on their analysis, "the evidence linking rising wage inequality to SBTC is surprisingly weak" (776).[37]

Autor, Katz, and Kearney (2005) take strong issue both with Card and DiNardo's claims (2002) about the weakness of the evidence in favor of the SBTC hypothesis and with their emphasis on the importance of the minimum wage. Regarding the first issue, they note that although the growth of overall wage inequality (as well as schooling-related earnings differentials) did slow after the mid-1980s, inequality in the upper half of the wage distribution (i.e., the 90/50 differential) continued to grow just as fast in the 1990s as in the 1980s. They also address an hypothesis advanced by Lemieux (2006), which attributes much of the rise in inequality to changing workforce composition—in particular, more unobserved variance in human capital as the workforce became more educated and more experienced. They argue instead that the data are much more suggestive of an important role for changing relative demands, especially with a modified version of the SBTC hypothesis that leads to polarization of skill demands.

In addition to bolstering the SBTC hypothesis, Autor, Katz, and Kearney also cast serious doubt on previous research that emphasized the importance of minimum wages for changes in wage inequality. The most striking evidence they present is that the minimum wage is strongly correlated with upper-tail wage inequality as well as with lower-tail wage inequality. Indeed, in simple regressions of the 90/50 or 50/10 wage gaps on the real minimum wage, the estimated coefficient on the real minimum wage is larger for the 90/50 gap than for the 50/10 gap (−0.44 vs. −0.27, with both significant).[38] In more complete regression models that account for a time trend, the relative supply of more- and less-educated workers, and aggregate economic conditions, a significant relationship between these gaps and the real minimum wage persists, although the estimated coefficient on the minimum wage is about two-thirds lower for the 90/50 gap than for the 50/10 gap. Autor, Katz, and Kearney conclude that correlations between the minimum wage and both upper- and lower-tail inequality measures suggest that the "time series correlation between minimum wages and inequality is unlikely to provide an accurate account of the causal effect of the minimum wage on earnings inequality. Indeed, we view the relationship between the minimum wage and upper tail inequality as potential evidence of spurious causation" (2005, 18). This evidence does not, of course, mean that the falling real minimum wage was not responsible for the increase in lower-tail wage inequality in the 1980s—it is conceivable that the evidence of an effect on upper-tail inequality is spurious, while the effect on lower-tail inequality is real. However, the upper-tail evidence constitutes what is often referred to as a *falsification exercise*. That is, if theory predicts an effect of x (the minimum wage) on y (lower-tail inequality), and such evidence is found, researchers often also explore whether there is an effect of x on another variable, z (upper-tail inequality), which is conceptually related to y but for which theory does not predict an effect on z. If no evidence suggesting an effect of x on z is found, the evidence of an effect of x on y is viewed as more convincing, and vice versa. The point of the Autor, Katz, and Kearney analysis is that, in this case, the falsification exercise fails.

Aside from the failure of this falsification exercise, is there reason to be skeptical that the minimum wage was the primary factor increasing lower-tail wage inequality in the 1980s? The graphs in the Autor, Katz, and Kearney paper that plot the two wage gap measures against the predicted value from the simple regression described previously (their figures 10a and 10b) indicate that after the late-1980s the relationship between the minimum wage and the 50/10 wage gap appears to have become quite a bit weaker.[39] This leads them to conclude, in a later paper describing these results, that "the minimum wage explanation fits only lower-tail inequality trends well to 1987" (Autor, Katz, and Kearney 2006). In this later paper, they also argue that even for the period during the 1980s when the minimum wage predicts lower-tail wage inequality well, the evidence is potentially more consistent with demand shifts against low-wage workers. As they note, the minimum wage-inequality hypothesis "does not explain why relative employment in low-wage jobs *fell* as the minimum wage dropped" (193). That is, if the minimum wage became less binding, and this was the predominant influence on low-wage workers, then the employment of low-wage workers should have risen. In contrast, the analysis in the authors' 2006 paper suggests that the 1980s were a period of declining employment shares for the least-skilled workers (their figure 3).

One question left unanswered is what might have generated the spurious correlation between the minimum wage and upper-tail wage inequality, and, by extension, some of the correlation between the minimum wage and lower-tail wage inequality. Here, Autor, Katz, and Kearney speculate that the federal minimum wage may respond to macroeconomic shocks that affect earnings inequality, with policymakers letting the real minimum wage decline during periods when shocks increase wage inequality (to reduce disemployment of less-skilled individuals), and vice versa. The authors present no evidence to support this conjecture, but it is an interesting hypothesis that—along with the effect of endogeneity of minimum wages on estimated employment effects—bears further exploration.

4.4 Effects of Minimum Wages on Earnings

We now turn our attention away from how minimum wages affect the wage distribution and instead consider the evidence on how minimum wages affect earnings. The material covered in the previous sections addresses this point to some extent, showing that workers below the minimum are brought up to the minimum and that there are limited spillovers to the wages of workers above the minimum. These statements, however, pertain to *workers*—that is, those individuals who are still employed after a change in the minimum wage. None of the preceding discussion takes account of the lower earnings of individuals who do not have a job because of a higher minimum wage or whose hours decline because of an increase in the minimum. That is, to this point, we have discussed the effects of minimum wages on the wage distribution without considering the *consequences* of the changes in wages that are induced by changes in the minimum for the quantities of labor employed and hours worked or the combined consequences of the wage, employment, and hours changes for labor income.

In this section, we describe evidence on the effects of minimum wages on these various margins of adjustment to minimum wages. We regard this evidence as moving us closer to the type of information we need to evaluate minimum wage policies, as it provides information that is more pertinent to evaluating the consequences of minimum wages than does the employment effects literature. In particular, in chapter 3 we noted that employment elasticities estimated from studies of teenagers that were in the −0.1 to −0.2 range, and hence well below 1 in absolute value, did not necessarily imply that the earnings of affected workers rose, because such estimates are weighted averages of individuals who are directly affected by the minimum wage with those who are not. In addition, employment studies do not capture potential effects of the minimum wage on average hours nor—as this chapter has emphasized—spillover effects to wages a little higher in the wage distribution. The evidence we present here captures all of these effects and thus provides a much clearer sense of how minimum wages affect the economic well-being of low-wage individuals. In addition, as explained in chapter 3, our approach focuses on low-wage workers regardless of age. As a result, the evidence is likely more relevant to policy than is evidence that pertains only to teenagers (like much of the literature on employment effects), because low-wage adults are more likely to be permanent low-wage workers and primary earners.

Grossman's (1983) study presents indirect evidence on wage spillovers. In particular, she was interested in testing for the "equity" effects on wages suggested by her efficiency wage model, as opposed to neoclassical substitution effects. As she admits, however, she does not have a sharp test, but suggests that equity effects should occur quite soon after a minimum wage increase, while substitution effects should occur more slowly. She finds evidence of a rapid short-run response for some of the occupations she studies—in general, white-collar occupations. On the other hand, she also finds evidence of longer-run effects for some of these occupations, which she attributes to substitution. If there are equity-type effects, then the implications for whether workers above the minimum are made better off are unclear, as there may be disemployment effects for these workers.

An earlier attempt to estimate the effects of minimum wages on wages, employment, hours, and income is a study by Linneman (1982) using data from the Panel Study of Income Dynamics (PSID) for the mid-1970s. His findings indicated hours (and to a lesser extent employment) reductions among workers directly constrained by minimum wage increases, and employment reductions but hours increases for those just above the minimum (in all cases relative to those well above the minimum). His wage, employment, and hours effects imply a negative effect on incomes of workers whose wages are constrained by the minimum wage.

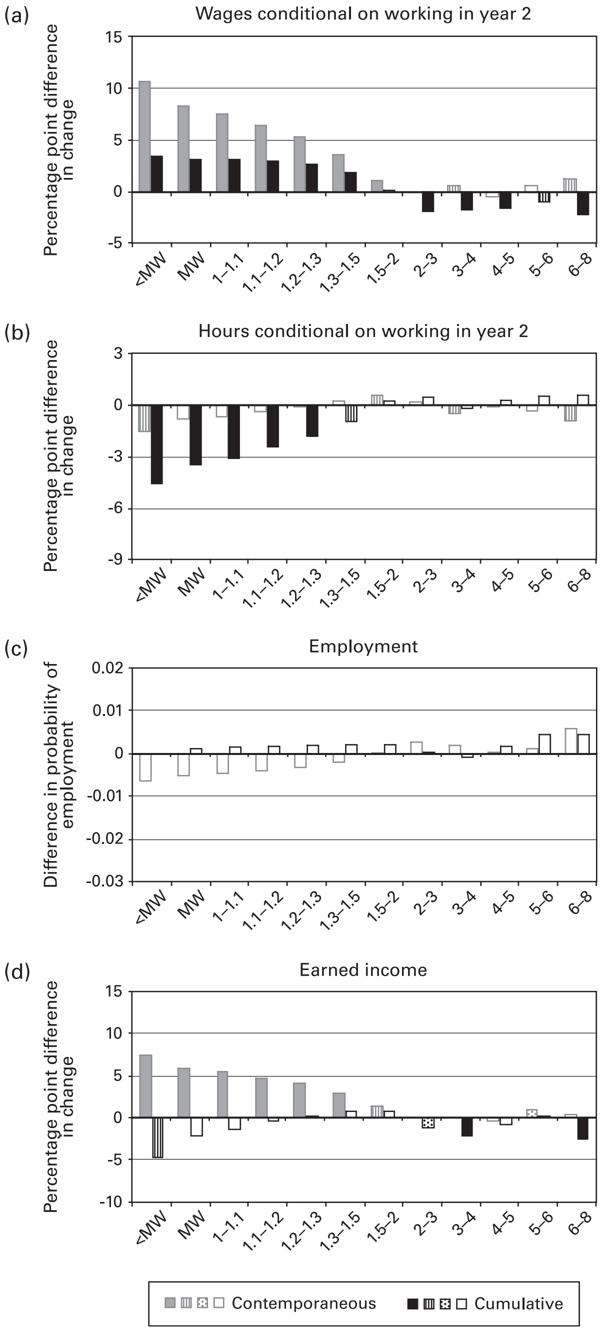
However, Linneman's analysis has two important shortcomings. First, his estimates of the effects of the minimum wage on income are not based on observed income, but are rather imputed from the estimated hours and employment effects; as a result, they do not incorporate potential spillovers to wages of workers earning more than the minimum. In addition, the imputation method does not take account of the joint distribution of wage and hours (and employment) effects across individuals. For example, if a higher minimum wage increases wages more for individuals who work fewer hours, the average rise in earnings will be smaller than what is calculated using his imputation method. Second, his approach does not provide a credible counterfactual for the experiences of the group affected by the minimum wage increase because he studies only federal minimum wage increases.

To address these shortcomings, in Neumark, Schweitzer, and Wascher 2004 we estimate wage, hours, employment, and total earnings effects independently, using state variation in minimum wages to obtain treatment and control groups. Our basic approach was explained in chapter 3, where we presented evidence on employment effects from this study. And, earlier in this chapter we described the evidence on wage effects. Figures 4.1b and 4.2b (pages 120 and 122) report the estimated effects for hours (conditional on remaining employed). The estimates point to a moderate contemporaneous decline in hours for workers paid at or below the minimum, but little evidence of statistically significant effects on hours worked by those paid more than 20 percent above the minimum wage, whose wages rise substantially. That said, the estimated effects are positive and non-negligible, with elasticities of about 0.1 for those between 1.2 and 1.5 times the minimum wage.

The full contemporaneous plus lagged effects are indicated by the dark bars in the graphs. For individuals below the minimum, the estimated total effect on hours is more negative than the contemporaneous effect alone. The more negative total effect occurs because the wage gains experienced by these workers (see panel a in each figure) put them into higher cells in the wage distribution, where there are lagged hours reductions.[40] More important, the graphs reveals hours reductions for workers initially paid at or just above the minimum wage, with elasticities near −0.3; the estimates for both cells are strongly significant. In the dummy/spline specification in figure 4.1, the hours effects are present (and significant) only up to 1.1 times the minimum wage, but in the polynomial specification in figure 4.2, significant negative effects are evident for workers earning up to 1.5 times the minimum wage, which is also (as seen in figures 4.1a and 4.2a) the highest wage at which the minimum wage induces a significant positive wage effect. Similarly, in figure 4.1, where the negative hours effects do not extend as high in the wage distribution, neither do the wage effects.

Although we previously discussed the results for employment in chapter 3, they are repeated in figures 4.1c and 4.2c. In particular, a contemporaneous disemployment effect is evident in figure 4.1 for individuals paid at or just above the minimum (up to 1.3 times the minimum), with the estimated elasticities ranging from −0.12 to −0.17 (with the exception of the cell for workers with wages 1.1 to 1.2 times the minimum).[41] However, as suggested by the combined contemporaneous and lagged estimates, the total disemployment effect becomes smaller and statistically weaker, although it remains significant for workers with initial wages between 1.2 and 1.3 times the minimum. The pattern of stronger employment effects initially and stronger hours effects later is consistent with the possibility that employers first reduce their reliance on part-time workers, perhaps reflecting fixed costs per employee, and only later adjust downward hours of the remaining low-wage workers. Alternatively, the time pattern of effects could arise from adjustments to benefits that take place more slowly. In the polynomial specification, this contrast is sharper, with significant contemporaneous disemployment effects up to 1.5 times the minimum, but with combined effects that are near zero and statistically insignificant.

In terms of earned income, the results reported previously are insufficient to tell whether a higher minimum wage would have a positive or negative effect on the earned income of low-wage workers. Low-wage workers experience wage gains as a result of minimum wage increases, but they also experience employment and hours declines. The results for observed earnings—shown in figures 4.1d and 4.2d—indicate that the contemporaneous effects are positive (and significant for most cells) for workers initially earning up to twice the minimum wage. However, the total effects—shown by the dark bars—tell a much different story. In figure 4.1d, the total effects indicate that workers initially below the minimum, at the minimum, and up to 1.1 times the minimum experience a reduction in earned income; the estimated decline for those paid at the minimum is on the order of 6 percent and is statistically significant at the 5 percent level. In figure 4.2d, the negative effect for workers paid between the minimum and 1.1 times the minimum is also significant (at the 5 percent level). The source of


(a)
Wages conditional on working in year 2
Percentage point difference in change
15
10
5
0
-5
<MW
MW
1–1.1
1.1–1.2
1.2–1.3
1.3–1.5
1.5–2
2–3
3–4
4–5
5–6
6–8
(b)
Hours conditional on working in year 2
Percentage point difference in change
3
0
-3
-6
-9
(c)
Employment
Difference in probability of employment
0.02
0.01
0
-0.01
-0.02
-0.03
(d)
Earned income
Percentage point difference in change
15
10
5
0
-5
-10
Contemporaneous
Cumulative

the reversal is clear from the other panels of the figures. Some of the wage gains are given back after a year, and the reductions in hours are larger.

Overall, our analysis suggests that low-wage workers experience earnings declines, on average, following an increase in the minimum wage, despite the initial wage increase received by those who stay employed. Although minimum wages bump up wages of these workers, hours reductions, in particular, interact with changes in wages in such a way that earned income declines. The negative earnings and hours effects for workers paid above the minimum are consistent with Grossman's view that wage spillovers are attributable to increases in wages needed to preserve wage differentials, rather than to neoclassical substitution effects, as the former constitute cost increases for somewhat higher-skilled labor, and the latter reflect increased demand for this labor.

Finally, note that these results are limited to workers who are initially employed. If individuals who do not have a job find it more difficult to find one after a minimum wage increase, the adverse effects of the minimum wage on earned income would be even stronger. However, it is also possible that some workers with higher reservation wages will choose to enter the labor market after the minimum wage increases, with the result that their earned income rises.

As we noted earlier, estimating the effects of minimum wages conditional on workers' positions in the initial wage distribution allows us to test whether there is evidence of labor demand reductions for low-skilled workers. In this sense, the focus of our analysis on low-wage workers provides a cleaner estimate of the effects of the minimum wage on those who are affected by it than does past work studying specific age groups (e.g., teens or young adults) that include many—but not exclusively—low-skilled workers. Indeed, we can use our methods to estimate effects for low-skilled adults, who are often ignored in the minimum wage literature. Figure 4.3 reports estimates using a sample restricted to individuals aged twenty and over.[42]

Figure 4.3
Effects of a 10 percent minimum wage increase, polynomial specification, adults only
Source: Neumark, Schweitzer, and Wascher 2004.
Note: See the note to figure 4.2. The only difference is that the estimates are based on a sample that excludes individuals younger than age twenty. Significance levels for two-sided tests are as follows: 1 percent = solid; 5 percent = striped; and 10 percent = dotted. These are computed from bootstrapped standard errors.

Overall, these estimates are quite similar to those in figure 4.2, with evidence of significant positive wage effects in the lower range of the wage distribution, but negative and significant hours and earned income effects for the lowest-wage workers. Thus, the negative consequences of minimum wages are not restricted to teenagers, but appear more generally for low-wage workers.[43]

4.5 Conclusions

The evidence presented in this chapter clearly indicates that minimum wages affect the wage distribution. For industrialized countries, the minimum wage creates a spike in the wage distribution and appears to provide some boost to wages for workers who previously earned somewhat more than the minimum wage. Our preferred estimates suggest that spillovers extend to wages about 20 percent above the minimum, with elasticities around 0.4 near the minimum and 0.2 above it. In addition, shorter-run spillover effects tend to dissipate over time as firms reduce nominal wage growth for these workers in subsequent years. The results for developing countries pose some puzzles, including the creation of a spike at the minimum in the uncovered sector and some evidence that minimum wage spillovers extend very high into the wage distribution, although we believe the latter evidence is likely spuriously generated by evidence from high inflation periods.

Based on the estimated effects of minimum wages on the wage distribution, most economists believe that it was a factor contributing to the rise in wage inequality in the United States over the last few decades—especially in the 1980s, when the real value of the federal minimum declined sharply. However, the most compelling research suggests that this is only part of the story, and that changes in the relative demand for skilled workers were at least as important if not more so.

Finally, although there is obvious interest in the effects of minimum wages on the wage distribution per se, in our view the more important question is how the minimum wage affects the economic wellbeing of workers at different points in the wage distribution. This question is undoubtedly one reason for the interest in wage spillovers, which many argue should be counted as a benefit of minimum wage increases. And it is clearly related to the more general interest in the effect of the minimum wage on wage inequality. Indeed, it is often argued that if (1) higher wage inequality implies that low-skill workers are worse off, and (2) a lower minimum wage is responsible for higher wage inequality, then raising the minimum wage would help low-skill workers.

In contrast to this view, the evidence suggests that higher minimum wages tend, on average, to reduce the economic well-being of affected workers. Evidence regarding the effects on workers initially paid at or just above the minimum suggests that their labor income declines as a result of minimum wage increases, reflecting negative effects of minimum wages on employment and hours. For workers above the minimum, such effects do not accord well with the simple neoclassical model, in which a higher minimum wage increases demand for more-skilled workers. Instead, they may reflect a desire among employers to maintain wage differentials between workers, so that a higher minimum wage puts upward pressure on other wages, amounting to cost increases.

We have argued that a focus on the disemployment effects of minimum wages is overly narrow from a policy perspective. Evidence on how minimum wages affect low-wage or low-skill workers more generally helps to fill out the picture of how well minimum wages achieve their goals. But we contend that a far more important policy question is how minimum wages affect the distribution of family incomes, to which we turn in the next chapter.

5 The Effects of Minimum Wages on the Distribution of Incomes

5.1 Introduction

There is something of a disjuncture in debates over the minimum wage. As the massive number of studies on the employment effects of minimum wages indicates, much of the debate about minimum wages centers on their employment effects. In contrast, policymakers tend to focus much more on distributional goals in advocating minimum wages. For example, President Bill Clinton, in calling for an increase in the minimum wage in 1998, argued that minimum wages will "raise the living standards of 12 million hardworking Americans."[1] And even more explicitly, Senator Edward Kennedy, a perennial sponsor of legislation to raise the federal minimum wage, has argued that "the minimum wage was one of the first—and is still one of the best—anti-poverty programs we have" (quoted in Clymer 1999, 449).[2]

Of course, the two issues are not unrelated. If an increase in the minimum wage does not lead to a reduction in labor demand, then low-wage workers will benefit from it. In that case, perhaps the worst that can happen from a distributional perspective (ignoring who pays the cost of the higher minimum) is that some of the gains may go to low-wage workers in high-income families, so that both low-income and high-income families benefit from the increase.[3] However, if there are disemployment effects, then the policy debate becomes more complicated. Opponents of minimum wage increases frequently cite the evidence on disemployment effects as the justification for their opposition. But the existence of disemployment effects does not necessarily imply that minimum wages constitute bad social policy. As with many government rules and regulations, a higher minimum wage entails both benefits and costs. Thus, the question is not whether there are *any* costs to a higher minimum wage, but instead whether the tradeoffs between the costs and the benefits are acceptable, as captured in the quote by Gramlich (1976) in chapter 2. Our view of the evidence, summarized in chapter 3, is that minimum wages unambiguously reduce employment of less-skilled workers, implying distortionary effects of minimum wages.[4] This brings Gramlich's question to the fore.

In this chapter, we address the evidence on the distributional effects of minimum wages. In light of the policy arguments in favor of a higher minimum wage, and the evidence of disemployment effects, the distributional effects should be at the center of the policy debate about minimum wages. The importance of these distributional effects, however, is clearly not reflected in the literature. Gramlich (1976) noted that "even though the main appeal of the minimum wage appears to be its effect on income distribution, its impact on those with low family incomes has received almost no discussion" (443–444). Although written more than thirty years ago, this statement is still a relatively accurate characterization of the existing research on minimum wages. Nonetheless, research literature on the distributional effects of minimum wages has begun to develop, and in this chapter we discuss what this research shows.

Even if we agree on the importance of the distributional effects of minimum wages, how to interpret evidence on those effects is not entirely straightforward. For example, assume that society believes that redistributing income towards families at the bottom of the income distribution is a desirable goal, consistent with Dalton's (1920) "principle of transfers" (also sometimes referred to as the "Pigou-Dalton transfer principle"). Evidence of distributional effects in this direction would not necessarily imply that these benefits outweigh the costs—in particular, the loss of output from a less efficient economy as a consequence of a higher minimum wage could, in principle, outweigh the benefits associated with the accompanying redistribution of income—leaving as an open question whether the equity versus efficiency tradeoff is worthwhile. In addition, there may be more efficient ways to achieve the same equity goals. Strictly speaking, of course, in the simple neoclassical model with no other market imperfections, minimum wages create a distortion that reduces welfare, and theory implies that it would be optimal to avoid this distortion by achieving the desired distributional goals through lump-sum transfers instead. However, the assumptions embodied in the neoclassical model do not necessarily match the economic and political realities that underlie the policy debate about the minimum wage. And, in models that relax the assumptions of the neoclassical model, the implications for the effects of minimum wages on welfare are more ambiguous (e.g., Agell and Lommerud 1997; Flinn 2002; Rebitzer and Taylor 1995).[5]

Even aside from efficiency considerations, there remains the question of how to evaluate the effects of minimum wages on economic well-being. The messy reality is that minimum wages create both winners and losers. In particular, workers who keep their jobs, maintain their hours, and see their wages rise will likely gain from minimum wages,[6] while those who lose their jobs, have more difficulty finding a new one, or have their hours reduced may lose as a result of a higher minimum wage. Moreover, it seems clear that, ultimately, we should be more concerned with family incomes than with individual incomes. This notion is well-ensconced in other distributive policies—such as the Earned Income Tax Credit (EITC) and the tax code more generally—as well as in how we typically use the income distribution to measure economic well-being; for example, the poverty line is a family income concept. Even so, evaluating changes in the distribution of family incomes when different families can win or lose from a higher minimum wage requires an agreed-upon social welfare function (e.g., Formby, Bishop, and Kim 2005; Wu, Perloff, and Golan 2006).

In this chapter, we largely put aside a full consideration of the equity/efficiency tradeoff and welfare analysis, and instead pursue the simpler goal of establishing the empirical evidence on how minimum wages affect the income distribution. One simple and unambiguous metric—whatever its flaws—is provided by the effects of minimum wages on the poverty rate; this metric ignores issues of changes elsewhere in the distribution but is clearly related to the goals that policymakers frequently invoke. However, it is also of value to understand how raising the minimum wage affects other parts of the income distribution, as there may be interest in helping lower-income but nonpoor families, as well as in determining whose incomes might be reduced as a consequence of minimum wage increases. Regardless of exactly how we assess the distributional consequences, we suspect that policymakers would find employment losses associated with a higher minimum wage to be acceptable if the minimum wage raises the incomes of poor or near-poor families without generating sizable adverse consequences for families in other parts of the income distribution.

Two empirical questions underlie the redistributive effects of minimum wages. The first question is how minimum wages affect the total earnings of the low-wage workforce; that is, do the wage gains received by employed workers more than offset the earnings lost by those who lose or cannot find jobs? The previous chapter provided evidence on this question. Although minimum wages raise wages of low-skill workers, the evidence presented there suggested that, on average, the earned incomes of low-wage workers decline in response to a minimum wage hike. This type of evidence makes it less likely that minimum wages have beneficial distributional effects, but ultimately the distributional effects depend on the family incomes of workers differentially affected by minimum wages.

Thus, the second and more central question—which is the focus of this chapter—is how minimum wages affect the distribution of family incomes. We begin by presenting some statistics on the family incomes of workers most likely to be affected by a higher minimum wage. We then present evidence from studies that directly estimate the effects of minimum wages on the distribution of family incomes, emphasizing some of our own research, and also other recent research that considers the same question. Finally, we present and discuss evidence that compares the distributional effects of minimum wages to the distributional effects of other policies intended to help low-income families, and which may—by design—do so more directly.

5.2 Minimum Wage Workers, the Family Income Distribution, and Poverty

5.2.1 Descriptive Statistics

Because poverty is defined based on family income, minimum wage workers need not be in poor families. The first study to explore the link between low-wage work and poverty was by Gramlich (1976), who documented, in data from the early 1970s, that many low-wage workers were members of higher-income families.[7] This tendency is particularly true for teenage workers, who are strongly overrepresented among minimum wage workers but are distributed widely throughout the family income distribution. For example, Gramlich reported that about 25 percent of adult low-wage workers (earning less than $2.00 per hour in 1972 or 1973, when the minimum wage was $1.60) were in families with above-median income, but that 52 percent of low-wage teenagers were in such families. Based on these data, he concluded that "the generally loose correlation between wages and family incomes implies that minimum wages will never have strong redistributive effects" (445).

Although Gramlich's work is dated, its basic conclusion has generally held up over time. For example, recent research by Burkhauser and Sabia illustrates that there continue to be many minimum wage workers in nonpoor and even relatively high-income families. Based on March 2003 CPS data, they report that only 13.2 percent of workers earning a wage less than $7.25 were in poor families, while 46.3 percent were in families with incomes at or above three times the poverty line.[8] Moreover, the effectiveness of the minimum wage in targeting low-income families has declined dramatically since the passage of the FLSA. In 1939, according to their calculations, 94 percent of household heads who were low-wage workers (defined as those with wages less than half the average private sector wage) were in poor families, as were 85 percent of all low-wage workers. By 1969 these numbers had declined to 45 and 23 percent respectively, and by 2003 to 31 and 17 percent. Viewed differently, 31 percent of low-wage workers were heads of poor households in 1939; this percentage fell to 11 percent in 1969 and to 9 percent in 2003. These declines reflect two key factors: (1) the average number of workers per family has risen over time, and (2) there have been sizable increases in other sources of household income[9] (which is measured pretax, posttransfer).[10] Thus, as Burkhauser and Sabia conclude, "the majority of low-wage workers are not household heads . . . and an even greater share are not poor household heads" (2007, 266).[11]

The fact that many minimum wage workers are in nonpoor families reinforces Gramlich's earlier conclusion, and most economists agree that minimum wages target the poor badly. Even Card and Krueger, whose work on employment effects of minimum wages is frequently cited by minimum wage advocates, acknowledge that "the minimum wage is evidently a 'blunt instrument' for redistributing income to the poorest families" (1995a, 285).[12] That said, it is still the case that minimum wage workers (or workers who would be affected by a plausible increase in the minimum wage) are overrepresented among poor families, and these summary statistics calculations do not tell us which low-wage workers are helped by minimum wage increases and which are hurt. We turn to this question in the following discussion.

5.2.2 Simulating the Effects of a Minimum Wage Increase

Gramlich took his analysis a step further, using the descriptive statistics on the distribution of low-wage workers by family income to simulate the share of benefits from a higher minimum wage that would accrue to families in various parts of the income distribution (assuming no employment or hours changes). He concluded that although low-income families gain, there is what he termed significant "leakage" of the benefits of minimum wages to higher-income families, which sharply reduces the efficiency of the minimum wage as a redistributive tool. In particular, he estimated that 25 percent of the income gain goes to families with incomes above the median. Of course, of the 75 percent estimated to go to families with incomes below the median, only part would go to poor or near-poor families, implying even greater leakage with respect to these families.[13]

A number of other papers presented more sophisticated analyses based on simulation methods (e.g., Johnson and Browning 1983; Burkhauser and Finegan 1989; and Horrigan and Mincy 1993). As in Gramlich's paper, these simulations begin with descriptive statistics on the distribution of minimum wage workers across different parts of the family income distribution, and then make assumptions about employment effects and other relevant parameters (regarding changes in hours, for example) for these workers. Some studies (e.g., Johnson and Browning 1983; Horrigan and Mincy 1993) allow for possible disemployment effects; the first study also takes account of effects on taxes and transfers. The Johnson and Browning study suggests a modest equalizing effect of the minimum wage, while the Horrigan and Mincy study finds no equalizing effect.

Horrigan and Mincy attribute their conclusion to the fact that "minimum-wage workers live in families that are more or less evenly placed along the entire distribution of family incomes" (1993, 252). Card and Krueger (1995a) question this conclusion, arguing that because Horrigan and Mincy restrict attention to family incomes of hourly workers only, they underrepresent upper-income families. However, in a later comment on Card and Krueger's analysis, Burkhauser, Couch, and Wittenburg (1996) show that workers affected by the minimum wage are in fact rather evenly distributed across the family income distribution, consistent with the assumption used by Horrigan and Mincy. In particular, Burkhauser, Couch, and Wittenburg note that Card and Krueger report the number of affected workers in each decile as a proportion of working individuals. However, employment rates are much higher in higher-income deciles, and if we instead calculate the share of people (instead of workers) in each decile who are affected, a much more even distribution results.[14] To be sure, the question that Card and Krueger's calculation answers is not irrelevant—as it tells us where the affected workers are in the income distribution, but it is the share of people in each decile who are affected, rather than the share of workers, that is more relevant to the question of how the minimum wage might affect the income distribution.

The most recent study of the effects of minimum wages on the income distribution using simulation methods is by Burkhauser and Sabia (2007), who compare simulations of the effect of the federal minimum wage increases in 1996 and 1997 (based on income data for 1995) with more recent simulations of an increase to $7.25 (based on income data for 2003). The results of this exercise suggest that the targeting of the recent minimum wage legislation may be slightly worse than in the mid-1990s because the percentage of affected workers in poor and near-poor families was lower in 2003 than in 1995 (24.2 percent versus 28.9 percent) and the percentage of affected workers in families with incomes at least three times the poverty line was higher (46.3 percent versus 40 percent in 1995).[15] The calculations are not strictly comparable, because the minimum wage increases are of different magnitudes. Nonetheless, their evidence certainly does not point to any obvious improvement in the targeting of the benefits of a higher minimum wage to poor (or near-poor) families.

5.2.3 Problems with Simulation Studies of the Distributional Effects of Minimum Wages

The calculations from these simulation exercises are subject to a number of criticisms. First, the assumptions about employment effects may be incorrect or overly simplistic. For example, the simulation studies tend to calibrate their models with estimated employment elasticities from studies of all teenagers or all young adults, as opposed to elasticities for the most-affected workers—which, as chapter 3 suggested, could be considerably larger (in absolute value). In addition, no study appears to allow for the possibility that the employment effects may be different for low-wage workers at different parts of the family income distribution—in part because, to the best of our knowledge, no such estimates exist. Instead, the simulation studies typically apportion the predicted disemployment effect across all low-wage workers, assigning to everyone the equivalent average effect via a reduction in hours; because the employment elasticities used in these simulations are less than one in absolute value, this assumption guarantees that the simulated earnings of all affected workers will increase in response to a higher minimum wage.[16] Finally, as Addison and Blackburn (1999) point out, a number of other possible responses that are ignored in the simulations could also influence how minimum wages affect the distribution of family incomes. For example, the labor supply of other family members could respond to changes in the hours or employment opportunities of individuals affected by the minimum wage. Similarly, living arrangements can change, affecting measures of family income relative to needs. And, in response to changes in earned income or family structure, government transfers can change.

As a result, we believe that more reliable evidence can be obtained from the empirical approach we have emphasized throughout this book—the "before-and-after" estimates that are the standard used in social science research to study the effects of policy changes. In this particular case, we want to measure changes in outcomes, such as the poverty rate, in states where minimum wages increased, and compare these changes to what happened in states without minimum wage increases, or across states with increases of different sizes. Such studies have the advantage of eliminating the necessity of specifying all of the assumptions needed to do a simulation study of the type described earlier, and, by extension, also have the advantage of capturing all the possible sources of changes in income—including both earned income and transfers. We turn to such estimates in the next section.

5.3 The Effects of Minimum Wages on the Distribution of Family Incomes

As noted previously, the fact that many minimum wage workers are not in poor families makes it more difficult for minimum wages to have beneficial distributional effects (which we take as redistribution of income toward lower-income families). The evidence summarized in the previous chapter (from Neumark, Schweitzer, and Wascher 2004), indicating that minimum wages tend to lower earnings of low-wage workers rather than raising them, also militates against beneficial distributional effects. However, neither of these findings is decisive, as the actual distributional effects of minimum wages depend on the incidence of gains and losses to low-wage workers in different parts of the family income distribution. Thus, conclusive evidence on the distributional effects of minimum wages can be obtained only from direct estimates of the effects of minimum wages on family incomes.

5.3.1 Regression Estimates of Effects of Minimum Wages on Poverty

A number of studies have used the poverty rate as a metric on which to judge the extent to which minimum wages redistribute income toward lower-income families. For example, Card and Krueger (1995a) estimate the effect of the minimum wage on state poverty rates, using regressions of changes in state poverty rates from 1989 to 1991 on the fraction of workers in the state affected by the 1990 and 1991 increases in the federal minimum wage and various other controls. Although limited in scope to a very short sample period, this work is of note as perhaps the first before-and-after analysis of the effects of minimum wages on poverty. The results consistently show a negative relationship between the fraction affected and the poverty rate—so that a higher fraction affected by the increases in the minimum wage reduces poverty. But when controls are added for either changes in the state employment or unemployment rate, the estimated effects on the poverty rate are small and insignificant.[17]

The authors report slightly stronger effects of minimum wages in reducing poverty among a sample limited to workers, although the evidence from what seems to be their preferred specification (with the employment and regional controls) is still not statistically significant. Moreover, evidence on how minimum wages influence the poverty rate among workers does not tell us how minimum wages affect poverty overall,[18] and by conditioning on workers, disemployment effects are downplayed.[19] At the same time, given that disemployment effects *are* downplayed, the absence of a significant poverty-reducing effect of minimum wages is quite striking.

Finally, Card and Krueger present evidence on the effect of the minimum wage on the 10th percentile of the family earnings distribution, and on the gap between this percentile and both the 50th and 90th percentiles. In this case, the evidence is somewhat stronger, as the minimum wage has a significant positive effect on the 10th percentile. However, it also appears to have a significant positive (although smaller) effect on the 50th percentile, which is a bit hard to explain. In addition, the critique of Burkhauser, Couch, and Wittenburg (1996) regarding the use of family earnings without any relation to needs applies here, and, depending on the question, it may be preferable to study total family income (which includes transfers). As a result, we view Card and Krueger's results for poverty to be more compelling.

Burkhauser and Sabia (2007) update the Card and Krueger analysis to include data from 1988 through 2003. Their conclusions are similar. The point estimates are consistent with minimum wages reducing poverty, but the evidence is never statistically significant once controls for the prime-age male unemployment rate (and adult wages) are added.[20] One difference that does emerge is that this analysis finds little evidence of an effect of the minimum wage on poverty among workers.

Addison and Blackburn (1999) used a similar state-level panel data regression analysis to estimate the effect of the minimum wage on state-level poverty rates, using March CPS data from 1983 to 1996. They use the log of the real minimum wage (or the level), which is more relevant for asking whether a minimum wage puts a family above the federal poverty line than is the relative minimum wage measure that we prefer for employment equations. An unusual feature of this study is its focus on narrow groups. Specifically, the authors estimate these poverty regressions for teenagers, young adults (aged twenty to twenty-four), and junior high school dropouts—defined as those with at most nine years of education and aged twenty-five and over. Poverty, then, is measured using the level of income for the families of individuals in each of these categories.

The point estimates from their regressions are nearly always negative, implying that a higher minimum wage reduces poverty. However, the results are not particularly robust. For young adults, the estimates are never close to statistically significant. For teenagers, the results are significant only when state-year trends are excluded, and the estimate is positive for the 1980s part of the sample (results are not shown only for the 1990s). There is also no significant effect on the share of teenagers below 1.25 times the poverty line (and the estimated effect falls by 82 percent). For junior high school dropouts, the evidence that the minimum wage reduces poverty is stronger and more robust, although for this group, as well, there is no evidence of a reduction in poverty from minimum wage increases in the 1980s part of the sample.

The rationale provided by Addison and Blackburn for focusing on the three particular subgroups they study is that these groups are most likely to be affected by the minimum wage. As chapter 3 indicated, in estimating employment effects, much of the literature has focused on groups more likely to be affected by minimum wages—although we have suggested that a general focus on low-wage workers, rather than groups with a high share of low-wage workers, may be preferable. But from the point of view of evaluating the distributional effects of minimum wages, one has to wonder how informative this analysis is. It is true that minimum wages should affect family incomes in families that have minimum wage workers. But there are many minimum wage workers who are not teenagers, young adults, or junior high school dropouts, and so many other families are also potentially affected by minimum wages. Moreover, there is no obvious reason to care more about poverty rates among families with members in the groups studied by Addison and Blackburn. To give one concrete example, female-headed households with children may be noticeably affected by minimum wage increases, and we may, from a policy perspective, be most concerned about children living in poverty.[21]

Indeed, if any group has been singled out for special attention with regard to the effects of minimum wages on their poverty, it is single mothers. Sabia (2006b) notes that with the advent of welfare reform in 1996, which created strong incentives for single mothers to work (and/or to leave the welfare rolls), policymakers have frequently invoked the goal of helping single mothers escape poverty in arguing for a higher minimum wage. To explore whether minimum wages help to achieve this goal, Sabia uses March CPS data from 1990 to 2005 to estimate the effects of minimum wages on employment of single mothers, and on hours worked, wage income, and poverty of employed single mothers.[22] Results from a standard individual-level regression analysis, with fixed state and year effects, as well as state-specific trends, provide no evidence of an effect of minimum wages on poverty of these women. Depending on the specification and sample (and also on varying the income threshold to be 75 percent or 125 percent of the poverty line), the estimates Sabia reports are sometimes positive and sometimes negative, but never statistically significant.

Burkhauser and Sabia (2007) extend this analysis to include all single female heads of household aged eighteen to sixty-four, using a state-level analysis covering a slightly different period (1988–2003). Their results for working women parallel Sabia's earlier study in that they find no significant effects on poverty for working single mothers. Their results for all single mothers hint at negative effects; however, none of the estimates from specifications that include controls are statistically significant. At first glance, the stronger antipoverty effects when the sample includes nonworkers may appear anomalous, as this sample presumably includes women who might have become disemployed because of a higher minimum wage. But as we point out in Neumark and Wascher 2007b, even if, on net, minimum wages reduce employment of less-skilled workers, they may increase employment (and earnings) for individuals for whom the wage is initially below their reservation wage, but rises above their reservation wage as a result of a minimum wage increase.[23] The bottom line, however, is that these papers do not find any statistically significant evidence that minimum wages reduce poverty among a set of families in which policymakers might be particularly interested.

Finally, Gunderson and Ziliak (2004) study the determinants of poverty rates for all families as well as for a number of subgroups (female-headed, married couples, and white and black families), using state-level poverty measures for 1981 to 2000 calculated from March CPS files. Although the main emphasis of the study is on the effects of macroeconomic changes on poverty, the authors report evidence of minimum wage effects as part of their analysis. In addition, they look at both the poverty count and what is called the "squared poverty gap," which captures not only the number of poor families, but how far families are below the poverty line (the "depth" of poverty) and inequality among the poor. Finally, this study looks at results for both before-tax income (paralleling the other work discussed in this chapter) as well as estimated after-tax income. The after-tax income results are potentially valuable—subject to the quality of the estimates of tax payments and EITC receipt—because they are more relevant to families' economic well-being. The authors specify the minimum wage variable as the difference between the logs of the state and federal minimum wage.

The evidence is mixed. Looking at either before-tax or after-tax poverty rates, the estimated effects of the minimum wage are negative, implying that minimum wages reduce poverty. For example, for the after-tax measure the estimated coefficient for all families is −0.025, implying that a 10 percent increase in the minimum wage reduces the proportion of families in poverty by 0.0025. Interestingly, in light of the earlier discussion about female-headed households, the estimated effects of minimum wages are *smallest* for this group, and insignificant for both the after- and before-tax analysis. And, in what appears to be their preferred analysis—using the squared poverty gap measure[24] and after-tax income—the effect of minimum wages on poverty is small and insignificant in the aggregate (−0.006), sometimes positive and sometimes negative across the other subgroups, and never statistically significant. Thus, this study also provides no compelling evidence of beneficial distributional effects of minimum wages, and indeed no compelling evidence of effects one way or the other.

5.3.2 The Effects of Minimum Wages on Transitions into and out of Poverty

In our first study of the distributional effects of minimum wages (Neumark and Wascher 2002b), we use matched March CPS files from 1986 to 1995 to study how changes in minimum wages affect families' transitions into and out of different parts of the income-to-needs distribution. We study the overall population, and follow other research in this area in looking at total family income from all sources (although pretax, and therefore also excluding the EITC). Given the family income data, each family is classified in terms of its income-to-needs ratio (the ratio of total family income to the poverty line for that family). We focus on the effects of minimum wages on the probabilities that poor families either escape poverty or remain poor, and on the probabilities that nonpoor families remain nonpoor or fall into poverty. We also look at transitions among other segments of the income-to-needs distribution. Transitions into and out of poverty, and elsewhere in the income-to-needs distribution, should reflect all of the effects of minimum wages, including changes in wages, employment, and hours of directly affected workers and other family members, as well as any induced changes in government transfers and living arrangements.

Studying these transitions rather than the poverty rate provides additional information that can aid our understanding of the sources of changes in poverty. For example, the same decrease in the share of families in poverty can come about via *only* a small increase in the probability of a transition out of poverty, or from large increases in the probabilities of transitions in both directions, with the flow out of poverty being slightly larger. Which of these two scenarios holds is likely to be of interest to policymakers, as the second scenario implies that far more families gain and lose as a result of a minimum wage increase. Evidence of significant flows both into and out of poverty as a result of minimum wage increases might, for example, motivate an inquiry into which types of families gain or lose, and the answer to this question might influence the desirability of a minimum wage increase.

We begin by estimating logit models for the probability that a family is poor in the second year in which they are observed (year 2); we estimate these separately for families that are poor or nonpoor in year 1.[25] Our minimum wage variable is the real value of the minimum wage, and we always include both contemporaneous and lagged effects. Our model incorporates a wide variety of control variables, including the adult male unemployment rate in each state, the 25th and 50th percentiles of the wage distribution, benefits from Aid to Families with Dependent Children (AFDC), and indicators for welfare reform, as well as state and year fixed effects. The state effects capture cross-state differences in mobility between poverty and nonpoverty and the year effects and other controls capture other aggregate influences—such as the business cycle or policy changes—on the likelihood that families experience changes in income-to-needs. The estimates are reported in panel a of table 5.1 as partial derivatives of the probabilities of being poor in year 2 with respect to a $1.00 increase in the real minimum wage measured in 1982–1984 dollars; this represents an increase of a bit more than $2.00 in current dollars, or very close to the recently legislated increase in the federal minimum from $5.15 to $7.25.

The first row of panel a reports estimates of the effects of the minimum wage on the probability that a poor family remains in poverty. The contemporaneous effect points to a reduction in this probability from a higher minimum wage. The lagged effect is in the opposite direction, although insignificant. The combined effect implies that a $1.00 real increase in the minimum wage reduces the probability of remaining poor by 0.056 (which equivalently is the increase in the probability of escaping poverty). The second row shows the effects on the probability of becoming poor, for initially nonpoor families. Here the contemporaneous effect is zero, while the lagged (and total) effect is a statistically significant 0.02 increase in this probability. Note that the signs of the estimated effects in both rows of panel a are generally consistent with what we would expect in light of the contemporaneous and lagged effects of minimum wages on low-wage workers reported in chapter 4. Recall that there was a relatively strong contemporaneous increase in wages, followed by a lagged reduction in employment or hours. Those findings likely help to explain why the beneficial effects occur contemporaneously and the adverse effects occur with a lag.

Panel b reinterprets these estimates in terms of their implications for poverty rates. Although the estimated minimum wage effect on the probability of escaping poverty is nearly three times larger than the estimated effect on the probability of falling into poverty, the latter estimate is applied to a much larger share of the population (83.9 percent). Thus, the implied change in the proportion of families that is poor is positive—an increase of 0.008, although this estimated change is not statistically significant.[26] Thus, the results are best interpreted as providing no evidence that minimum wages affect the poverty rate. At the same time, however, the evidence suggests that minimum wages do have significant effects on transition rates into and out of poverty.

More detailed evidence reported in the paper comes from multinomial logit models of the effects of minimum wages on the probability of transitions between different parts of the income-to-needs distribution. This evidence suggests that a higher minimum wage is associated

Table 5.1
Estimated effects of minimum wages on transitions between poverty and nonpoverty

	(1)	(2)	(3)	(4)	(5)
a. Estimated effects on probability poor in year 2	Contempora-neous effect	Lagged effect	Total effect	Observations	
Poor in year 1	−0.094*	0.038	−0.056	24,763	
	(0.036)	(0.038)	(0.042)		
Nonpoor in year 1	−0.000	0.020**	0.020**	171,506	
	(0.007)	(0.006)	(0.007)		
b. Implied change in proportion poor	Proportion initially poor	Change in proportion poor, initially poor	Proportion initially nonpoor	Change in proportion poor, initially nonpoor	Change in proportion poor
	0.161	−0.056	0.839	0.020**	0.008
		(0.042)		(0.007)	(0.009)

Source: Neumark and Wascher 2002b.
Note: The data come from matched CPS March files, from 1986 to 1995. Data are weighted to account for nonmatching. In panel a, estimates come from logit models estimated separately for those initially poor and those initially nonpoor. Estimates reported are partial derivatives of probabilities with respect to a $1 increase in the real minimum wage measured in 1982–1984 dollars, which is a bit more than a $2 increase in current dollars. Approximate standard errors are reported in parentheses. The model includes controls for the prime-age male unemployment rate, AFDC benefits and waivers, and the 25th and 50th percentiles of the wage distribution, as well as state and year effects. In panel b, the standard error in column (5) is computed treating the proportions in the two samples as known and the samples as independent. * and ** indicate that estimate is statistically significant at the 5 percent or 1 percent level.

with increases in the probabilities that a family falls from 1 to 1.5 or 1.5 to 2 times the poverty line into poverty.[27] Combining these estimates with the initial shares in each category suggests that minimum wages increase the proportion of families in poverty by 0.006, reduce the proportion between 1 and 1.5 or 1.5 and 2 times the poverty line by 0.006 and 0.004, respectively, and increase the proportion with incomes more than 2 times the poverty line by 0.004. Other evidence suggests that the effects we find are real and not spurious, and provides additional information on how these effects arise. In particular, the evidence indicates that for initially nonpoor families, minimum wages lead to a decrease in the number of workers per family, which is presumably the source of the income decline.[28] Assuming that many of these families have secondary workers, this may help to explain the increase in the probability that families higher in the income distribution fall into poverty. Finally, we find some evidence that minimum wages tend to increase income-to-needs for families that remain poor. We do not report these results in detail, because later in this chapter we describe research that provides a much more revealing look at how minimum wages affect the entire distribution of family incomes (relative to needs).

How do we interpret this evidence? First, it emphasizes what we said at the outset of this chapter—that minimum wages create both winners and losers. Second, the evidence does not support the conclusion that minimum wages reduce poverty. The point estimates are, if anything, consistent with the opposite conclusion—that a higher minimum wage increases poverty—but this overall increase is not statistically significant. Indeed, the evidence suggests that the main distributional effects of minimum wages are to redistribute income among different low-income families. Absent a compelling argument for why the winners are more deserving than the losers, the evidence from this study provides no support for the claim that minimum wages have beneficial distributional effects.

5.3.3 A Richer Description of the Effects of Minimum Wages on the Distribution of Family Incomes Relative to Needs

In order to provide a richer description of how minimum wages affect the distribution of family incomes relative to needs, in a follow-up study (Neumark, Schweitzer, and Wascher 2005), we used a non-parametric approach that provides a full picture—literally—of the effects of minimum wages on the family income-to-needs distribution and on changes in incomes of families at different points of the income-to-needs distribution. With this approach, we can still estimate the effects of minimum wages on the proportions of families that are poor or near-poor, and we can also examine the extent to which minimum wages push families initially near-poor into poverty, or lift initially poor families out of poverty. However, the non-parametric approach provides a far more detailed description of the effects of minimum wages on family incomes than a regression-based approach that arbitrarily specifies particular points of the income distribution—such as the poverty line—and asks whether the proportions of families above or below those points increase or decrease.[29]

5.3.3.1 Data and Methods The data we use in this study are essentially the same as we used in the research described in the previous subsection (Neumark and Wascher 2002b)—matched March CPS data from 1986 through 1995. The estimation is conducted for families with nonnegative incomes, up to a maximum income-to-needs ratio of six. (Recall that an income-to-needs ratio of one represents the poverty line.) Our basic approach is to construct a non-parametric difference-in-differences estimator of the effects of minimum wage increases on the distribution of family incomes relative to needs. We infer the effects of minimum wage increases by comparing changes in income-to-needs for families in states in which the minimum wage rose between years 1 and 2 (the treatment group), and families in states in which the minimum wage remained constant between years 1 and 2 (the control group).

Letting $MW = 1$ and $MW = 0$ denote the treatment and control groups, and numbers in the subscripts denote years, $f_{1,MW=1}(I)$ denotes the density of income-to-needs in year 1 in the treatment group and $f_{2,MW=1}(I)$ denotes the density in year 2 in the treatment group. The difference $f_{2,MW=1}(I) - f_{1,MW=1}(I)$ measures the change in the density at each point I for this group. Because the density of income-to-needs may be changing for reasons other than minimum wage increases, we subtract the corresponding quantity for the control group. This generates a difference-in-differences estimator of the effect of minimum wage increases on the density at each income-to-needs ratio I:

$$\{f_{2,MW=1}(I) - f_{1,MW=1}(I)\} - \{f_{2,MW=0}(I) - f_{1,MW=0}(I)\}. \tag{5.1}$$

Note that although this approach does not provide explicit estimates of the influences of various regression controls such as changes in welfare benefits or state economic conditions (as in Neumark and Wascher 2002b), it potentially accounts for a wide range of factors that might alter the distribution of incomes. For example, business cycles and the corresponding changes in unemployment rates, rising earnings inequality stemming from other sources, and demographic trends (all national phenomena) are controlled for if these effects are equally evident in the treatment and control groups. That said, a high fraction of the minimum wage increases in our sample period stemmed from changes in the federal law in 1990 and 1991, which affected states differently because of the proliferation of state minimum wages in the late 1980s. Because these increases in the federal minimum coincided with sharp increases in overall unemployment rates (in 1991, in particular) that were not uniform across states, we need to account for the relationship between minimum wages and the business cycle to draw valid conclusions regarding the effects of minimum wages on family incomes.

To estimate each of the four densities in equation (5.1), we use a kernel estimator. In particular, given a kernel $K(z)$, the estimated density function for I is

$$f^e(I) = \frac{1}{n}\sum_{j=1}^{n}\frac{\theta_j}{h}K\left[\frac{I - I_j}{h}\right], \tag{5.2}$$

where n is the number of observations in the sample, h is the bandwidth, and θ_j is a sampling weight that has been normalized to sum to 1. The points at which the density is estimated are indicated by I, and the data by I_j.[30] We use a Gaussian kernel. This procedure can best be thought of as generating smoothed histograms of the densities of income-to-needs.

A variety of issues that come up in regression analysis are also of concern here, although handling some of these issues is more complicated in the non-parametric setting. First, we need to consider the possibility that there are other variables affecting the treatment and control groups that may be correlated with minimum wage changes. As noted above, one such variable is the unemployment rate (or the business cycle more generally). Our strategy for controlling for state-specific changes in the unemployment rate follows DiNardo, Fortin, and Lemieux 1996. Specifically, the conditioning on observables that is normally captured in a regression context is converted to a reweighting problem in which we define a number of cells for the conditioning variable (the change in the unemployment rate), and then reweight the observations in the treatment group so that the distribution of observations across unemployment rate change cells is the same as in the control group.[31] The densities in equation (5.1) are estimated using these reweighted data, which removes the influence of the correlation between minimum wage increases and changes in the unemployment rate.

An alternative, and simpler, strategy to remove the influence of the relationship between minimum wage changes and changes in unemployment rates is simply to exclude from the analysis the minimum wage increases that took effect in 1991 or 1992, years in which unemployment rates rose sharply as a result of the recession. Although we also present these results, we note that this procedure comes at considerable cost in terms of the efficiency and accuracy of the estimates, because it ignores a substantial portion of the variation in minimum wages used in the analysis.[32]

Second, we also implement a procedure that mimics the inclusion of state and year fixed effects in a regression model, to control for state-specific or year-specific shifts in the income-to-needs distribution that are potentially correlated with minimum wage increases. To do this, we first estimate the median proportional change in income-to-needs by state (across all years). We then adjust each family's income-to-needs in year 2 so that the common state shift is taken out of the change in the family's income-to-needs from year 1 to year 2. We make a parallel adjustment for the median proportional change by year (across all states).[33] The difference between the adjusted data on income-to-needs for each family in year 2 and the year 1 income-to-needs ratio is the deviation around the average state change over all years in the sample and the average year change over all states in the sample. We then perform the same analysis described previously using these adjusted data.

Third, we are interested in estimating both contemporaneous and lagged minimum wage effects on the densities of family income-to-needs. This estimation creates complications, because the observations for the treatment group (or the control group) may be contaminated by the effects of minimum wage increases not directly captured by the difference-in-differences estimator. For example, when we estimate $f_{2,MW=1}(I)$ for the treatment group for the contemporaneous effect, there could also be a lagged effect from a minimum wage increase in the previous year, so that there are really two different types of treatment groups consisting of families in states with and without a lagged increase. The same problem arises in defining both the treatment group for estimating lagged effects and in defining the corresponding control groups. We could drop all of the observations in which the treatment or control group is contaminated, maintaining only three types of observations—contemporaneous increases only, lagged increases only, and neither a lagged nor a contemporaneous increase—and from these observations, estimate contemporaneous and lagged effects. But that would entail the loss of many observations. Instead, we employ a procedure that uses all of the observations and distributes the overall effects into "pure" contemporaneous and "pure" lagged effects by correcting for the incidence of contaminated treatment and control groups. This procedure, which is explained in detail in Neumark, Schweitzer, and Wascher 2005, recovers the equivalent of contemporaneous and lagged effects of minimum wages on the distribution of family incomes.

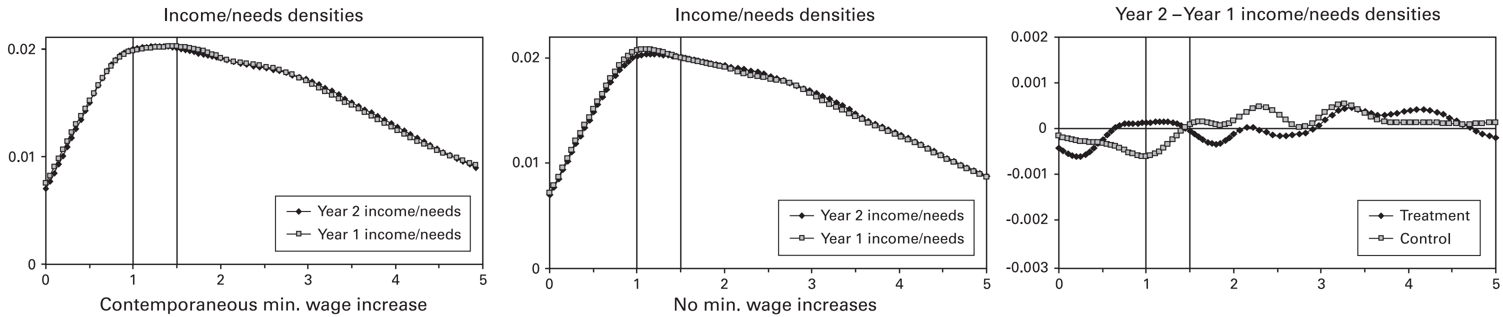
5.3.3.2 Results Panels a through c of figure 5.1 display the entire set of density estimations that we use to infer the effects of minimum wage increases on the distribution of income-to-needs. These are the baseline estimates for the full sample period and do not include any adjustments for unemployment rate changes or fixed state and year effects (these are discussed shortly). Panel a presents evidence on changes in the income-to-needs distribution in states with contemporaneous minimum wage increases as compared to states with no contemporaneous minimum wage increases. The left-hand graph presents estimates of the densities in year 1 and year 2 for the treatment group (observations with increases), and the middle graph presents the corresponding densities for the control group. The vertical axis shows the proportion of families at each income-to-needs level. Because the differences between the densities in each panel are small relative to the scale (and therefore hard to distinguish visually), the right-hand graph summarizes the information by plotting the vertical distance between the year 1 and year 2 densities, for both the treatment and control groups, using a different scale from the left-hand and middle graphs.

The difference-in-differences estimate of the effects of contemporaneous minimum wage increases on income-to-needs is the vertical distance between the two lines in the right-hand graph of panel a. This vertical distance is displayed in the left-hand graph of panel c, after an adjustment based on the methods alluded to above to obtain the "pure" contemporaneous effects of a minimum wage increase. The results indicate that the effects of contemporaneous minimum wage increases are to reduce the proportion of families with a ratio of income-to-needs between zero and about 0.6, to increase the proportion with income-to-needs between 0.6 and 1.5, and to reduce the proportion with income-to-needs from 1.5 to about 2.7. These results are consistent with minimum wages helping the poorest families, but they also suggest that some families with an initial income-to-needs ratio in the range from 1.5 to about 2.7 experience income losses.

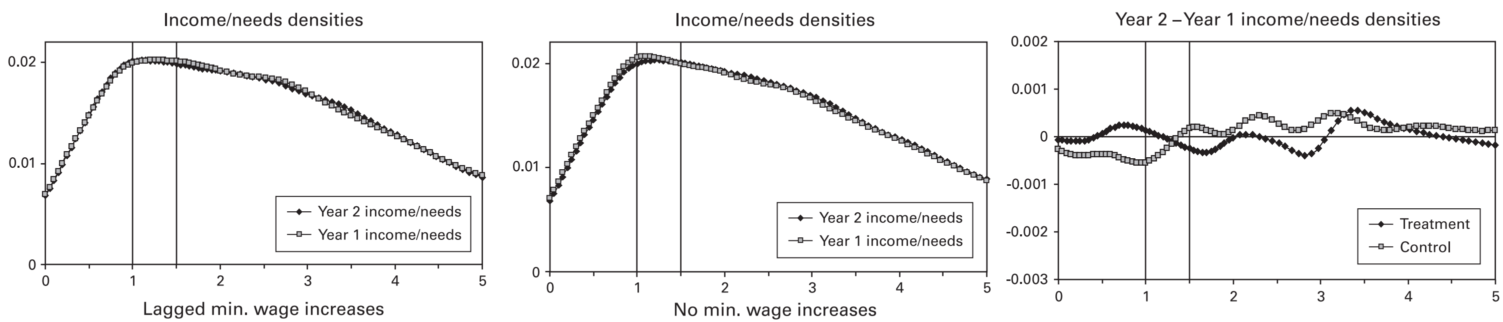
Panel b reports results when the treatment group is defined as those observations for which there was a lagged minimum wage increase, and the difference-in-differences estimate of the pure lagged minimum wage effect is reported in the middle graph of panel c. In contrast to the estimated effects of contemporaneous minimum wage increases, lagged increases unambiguously raise the proportion of families below about 1.3 times the poverty line, with a corresponding decrease in the proportion of families with income-to-needs between 1.3 and 3.2. This evidence, and the contrast with contemporaneous effects, is consistent with disemployment effects (or hours reductions) occurring with a lag, while the contemporaneous effects reflect more of the impact of immediate wage increases—which, according to the results in chapter 4, tend to diminish over time. These qualitative differences between contemporaneous and lagged effects are consistent with what we found in our regression analysis described earlier (Neumark and Wascher 2002b).

The total effects of minimum wage increases, shown in the right-hand graph of panel c, are the sums of the pure contemporaneous and lagged effects. The estimated effect at each particular point of the income-to-needs distribution is given by the middle curve, while the upper and lower curves are the bounds of the 95 percent confidence intervals, calculated using a bootstrap procedure for the non-parametric estimation. The results are quite striking. They show essentially no net change in the proportion of families with income-to-needs below 0.3, as the benefit associated with the contemporaneous increase is offset by the cost of the lagged increase. There is a marked increase in the proportion of families with income-to-needs between about 0.3 and 1.4, and a marked decrease in the proportion of families with income-to-needs between about 1.4 and 3.3. These results suggest that the overall effect of minimum wage increases is to push some families that are initially low-income but above the near-poverty line into poverty or near-poverty. On a point-by-point basis, the estimated increases in

(a) Contemporaneous effects



(b) Lagged effects



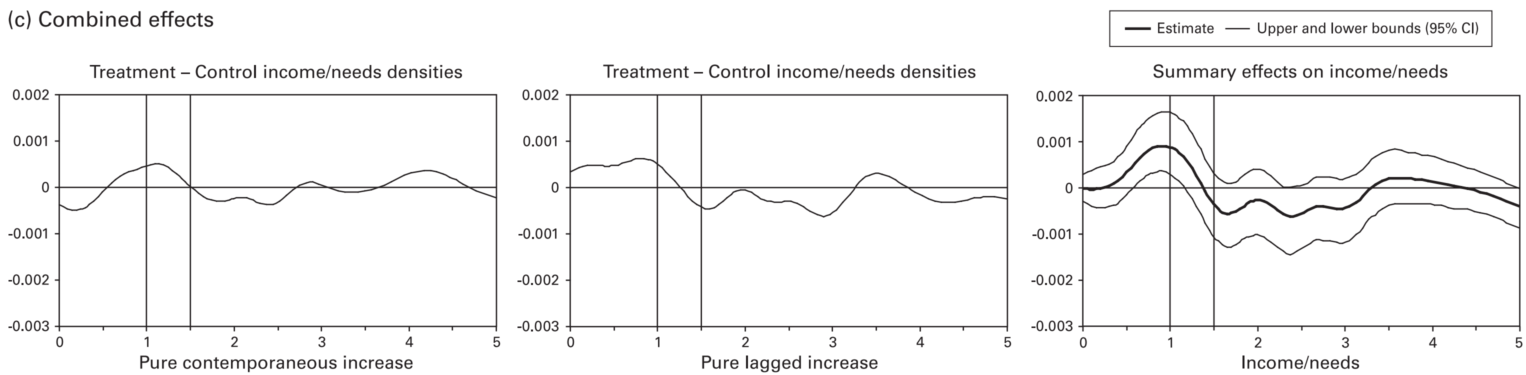


Figure 5.1
Estimated effects of minimum wages on the distribution of family income/needs, 1986–1995
Source: Neumark, Schweitzer, and Wascher 2005.
Note: The estimates are based on data taken from matched CPS March files from 1986 to 1995. The first two graphs in panels a and b display non-parametric estimates of the income-to-needs densities in the first and second year for observations with minimum wage increases (the treatment group) and without minimum wage increases (the controls); panel a shows this for contemporaneous increases, and panel b for lagged increases. The right-hand graphs in panels a and b show on a different scale the differences between the densities in the two years for the treatments and controls. The first two graphs in panel c show the difference between the change in the densities for the treatments and controls, first for contemporaneous and then for lagged minimum wage increases, with the correction described in the text to account for contaminated treatment groups. The right-hand graph in panel c reports the sum of the other two panels, or the combined minimum wage effect, at each point of the income-to-needs density; the 95 percent confidence band is also displayed.

the proportions of families with income-to-needs from about 0.6 to 1.2 are statistically significant.

Policymakers may be more interested in knowing whether minimum wage increases lead to a statistically significant increase in the proportion of families below a certain level, such as the poverty line, than in the change in the proportion of families at a particular point of the income-to-needs distribution. Thus, the first row of table 5.2 reports the estimated changes based on the density estimation (and corresponding standard errors) shown in figure 5.1 for some potentially interesting ranges of income-to-needs. As indicated in column (1), an increase in the minimum wage has essentially no effect on the proportion of families with income-to-needs between 0 and 0.5. In contrast, as shown in columns (2) and (3), minimum wage hikes lead to an estimated increase of 0.0079 in the proportion of families with income-to-needs between 0.5 and 1 and an increase of 0.0083 for the zero-to-one category as a whole. The proportion of poor families in the sample is approximately 0.18, so the change in the proportion poor corresponds to a 4.6 percent increase in the number of families with incomes below the poverty line. As indicated by the standard errors, the change in the proportion of families between 0 and 0.5 is not statistically significant, and the changes in the proportion between 0.5 and 1 and the proportion below 1 are statistically significant. As was apparent in figure 5.1, column (4) shows a sizable increase in the proportion of near-poor families (0.0046, or 3.6 percent) following minimum wage increases, an estimate that is statistically significant at the 10 percent level. Column (5) aggregates over the preceding categories and shows that minimum wage increases are estimated to raise the proportion of poor or near-poor families by 0.013, an estimate that is again statistically significant. Columns (6) through (8) indicate that minimum wage increases lead to declines in the proportion of families with income-to-needs in the 1.5 to 2 or 2 to 3 category of 0.0049 and 0.0071, respectively, while the overall decline in the proportion of families with income-to-needs between 1.5 and 3 is 0.012 (3.4 percent); the latter two estimates are statistically significant at the 5 percent level, and the first at the 10 percent level. To interpret the magnitudes in table 5.2, the average minimum wage increase in our sample is 43 cents, or about 10 percent. Thus, the elasticity of changes in the proportion poor or near-poor with respect to the minimum wage is approximately 0.41, and the elasticity of the proportion with income-to-needs in the 1.5 to 3 range is about −0.34.[34]

Table 5.2
Estimated effects of minimum wage increases on proportions in income-to-needs ranges

	Income-to-needs ranges							
	0–0.5	0.5–1	0–1, in poverty	1–1.5, near-poor	0–1.5, poor/ near-poor	1.5–2	2–3	1.5–3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No controls	0.0005	0.0079**	0.0083*	0.0046+	0.0130**	−0.0049+	−0.0071*	−0.0120**
	(0.0018)	(0.0025)	(0.0035)	(0.0027)	(0.0040)	(0.0028)	(0.0031)	(0.0040)
Fixed state and year effects (proportional shifts)	0.0002	0.0069*	0.0071+	0.0033	0.0104*	−0.0072*	−0.0074*	−0.0146**
	(0.0022)	(0.0028)	(0.0039)	(0.0034)	(0.0046)	(0.0033)	(0.0037)	(0.0048)

Source: Neumark, Schweitzer, and Wascher 2005.
Note: The data come from matched CPS March files, from 1986 to 1995. Data are weighted to account for nonmatching. Estimates are constructed by integrating under the changes in densities reported in the right-hand graphs in panel c of figures 5.1 and 5.2. The total sample size for the analysis, including families with income-to-needs up to 6, is 196,270. Standard errors are bootstrapped, based on five hundred repetitions, with implied t-statistics asymptotically normally distributed. +, *, and ** indicate that estimate is statistically significant at the 10, 5, or 1 percent level.

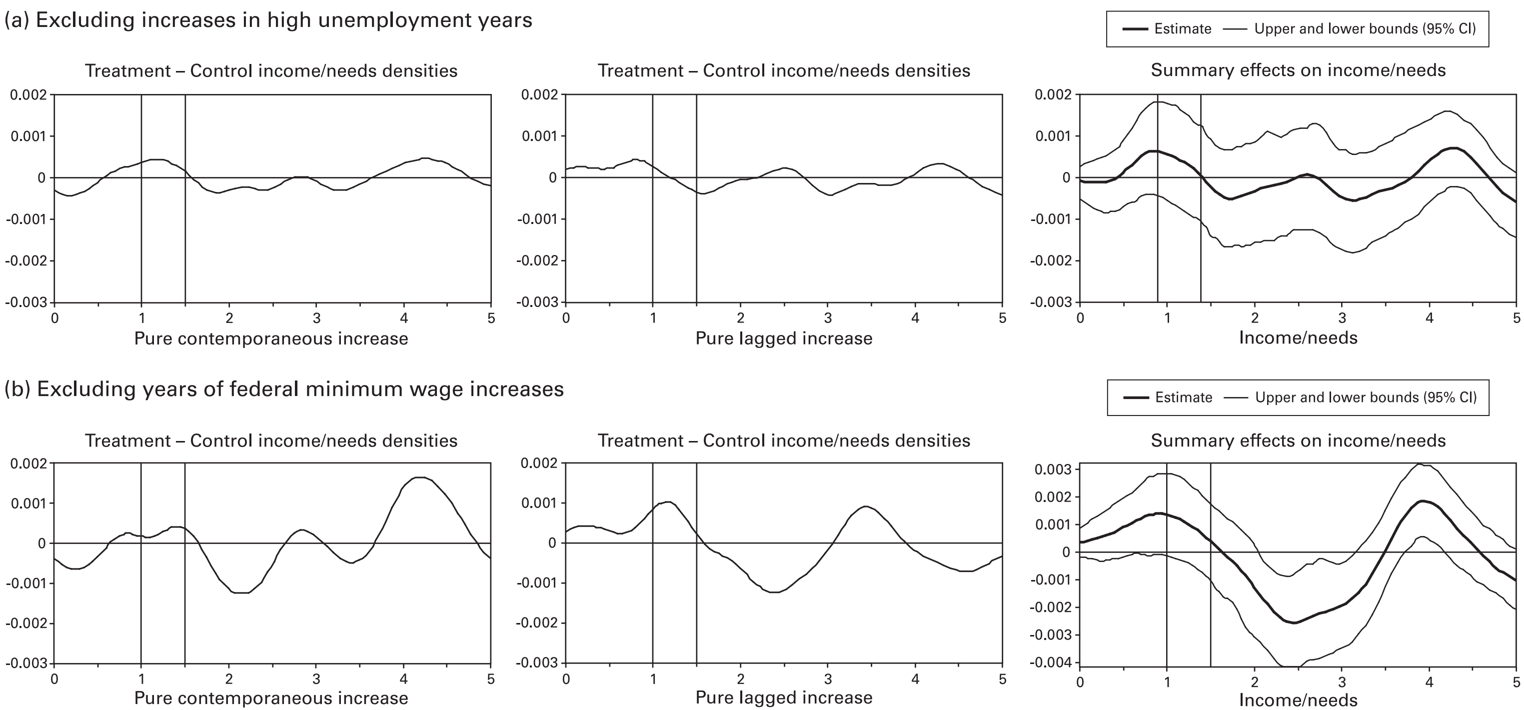
By looking at changes in the income-to-needs density between those state/year pairs with minimum wage increases and those without such increases, the difference-in-differences estimates account for fixed state differences in the density of the income-to-needs distribution as well as for changes in the density over time that are common across all states. However, the analysis to this point does not take explicit account of the possibility that minimum wage increases are correlated with other changes in economic conditions that may have influenced the distribution of family incomes yet varied by state.

Estimates that take account of this possibility are reported in figure 5.2. We first explore alternative methods of accounting for the relationship between minimum wage changes and changes in unemployment rates. In particular, we begin by excluding from the analysis minimum wage increases that took effect in 1991 or 1992, years in which the aggregate unemployment rate rose sharply as a result of the 1990–1991 recession.[35] As the federal minimum wage rose in both 1990 and 1991, this exclusion drops the 1991 increase from the analysis of contemporaneous effects of minimum wages, and *all* federal increases from the analysis of lagged effects (panel a). As a more extreme version of this exclusion rule, we also eliminated all years for which there was a contemporaneous or lagged federal minimum wage increase, which means that we drop 1992 as well (panel b). This procedure throws out all common variation across states where the federal minimum wage is binding, but also throws out variation from differences in minimum wage changes that result from the federal minimum catching up to state minimums in high minimum wage states. It thus does more than simply the equivalent of including year fixed effects in a regression framework. Second, we use the reweighting method described previously to account for differences in the change in the unemployment rate across states and years; as noted previously, we prefer this method because it accounts for the relationship between changes in the minimum wage and unemployment rates without excluding observations altogether (panel c). Finally, we also control more generically for factors generating state-specific or year-specific shifts in the income-to-needs distribution, removing state and year effects in the proportional shifts in income-to-needs distributions, as described previously (panel d).[36]

For each of these analyses, figure 5.2 shows the difference-in-differences estimate that is conceptually equivalent to panel c of figure 5.1. The first graph in each panel shows the pure contemporaneous effect on the income-to-needs density, the second the pure lagged effect, and the third the total effect; as before, the confidence intervals surrounding the estimates of the total effects are shown as well. As can be seen in panel b of figure 5.2, excluding all years with contemporaneous or lagged federal minimum wage increases widens the confidence intervals considerably (note that the scale in the right-hand side panel is more condensed) and leads to much larger point estimates of the changes in the income-to-needs distribution, which are probably unreasonable. In each of the other analyses reported in figure 5.2 (and even to some extent in panel b), the qualitative conclusions are similar to the results reported in figure 5.1. The estimated contemporaneous effect of a minimum wage increase—displayed in the graphs in the left-hand column—is always beneficial for the families at the very bottom of the income-to-needs distribution. In addition, although the exact shape of the difference-in-differences estimate of contemporaneous effects on the density varies, there generally is an increase in the proportion of families with income-to-needs ranging from about 0.6 or 0.7 to about 1.5 or 1.6, and a decline in the proportion of families with income-to-needs in at least some part of the 1.5 to 3 range.

On the other hand, the estimated lagged effects—displayed in the graphs in the middle column—systematically show a net increase in the proportion of families with income-to-needs in the 0 to 0.5 range in response to a higher minimum wage, and, more broadly, a net increase in the proportion of families below the poverty line. In addition, estimates of the lagged effects indicate a net reduction in the proportion of families in the 1.5 to 3 range.

The total effects, displayed in the right-hand graphs in each panel, lead to conclusions that parallel our initial analysis; indeed, the estimated total effects appear more similar across panels a, c, and d of figure 5.2 than do the estimated contemporaneous or lagged effects considered separately. In particular, raising the minimum wage appears to have little net effect on the proportion of families in the lowest income-to-needs range (approximately 0 to 0.5) and raises the proportion of families in the 0.5 to 1.5 range; together, these effects imply that a higher minimum wage results in a net increase in the proportion of families that are poor or near-poor. In panels c and d, which show results that use all of the data but that either reweight the data or subtract out fixed effects, the point estimates of the increases in the proportion of families are statistically significant in a range surrounding the poverty line. Finally, all of the graphs show a reduction in the


(a) Excluding increases in high unemployment years
Estimate
Upper and lower bounds (95% CI)
Treatment – Control income/needs densities
Pure contemporaneous increase
Treatment – Control income/needs densities
Pure lagged increase
Summary effects on income/needs
Income/needs
(b) Excluding years of federal minimum wage increases
Estimate
Upper and lower bounds (95% CI)
Treatment – Control income/needs densities
Pure contemporaneous increase
Treatment – Control income/needs densities
Pure lagged increase
Summary effects on income/needs
Income/needs

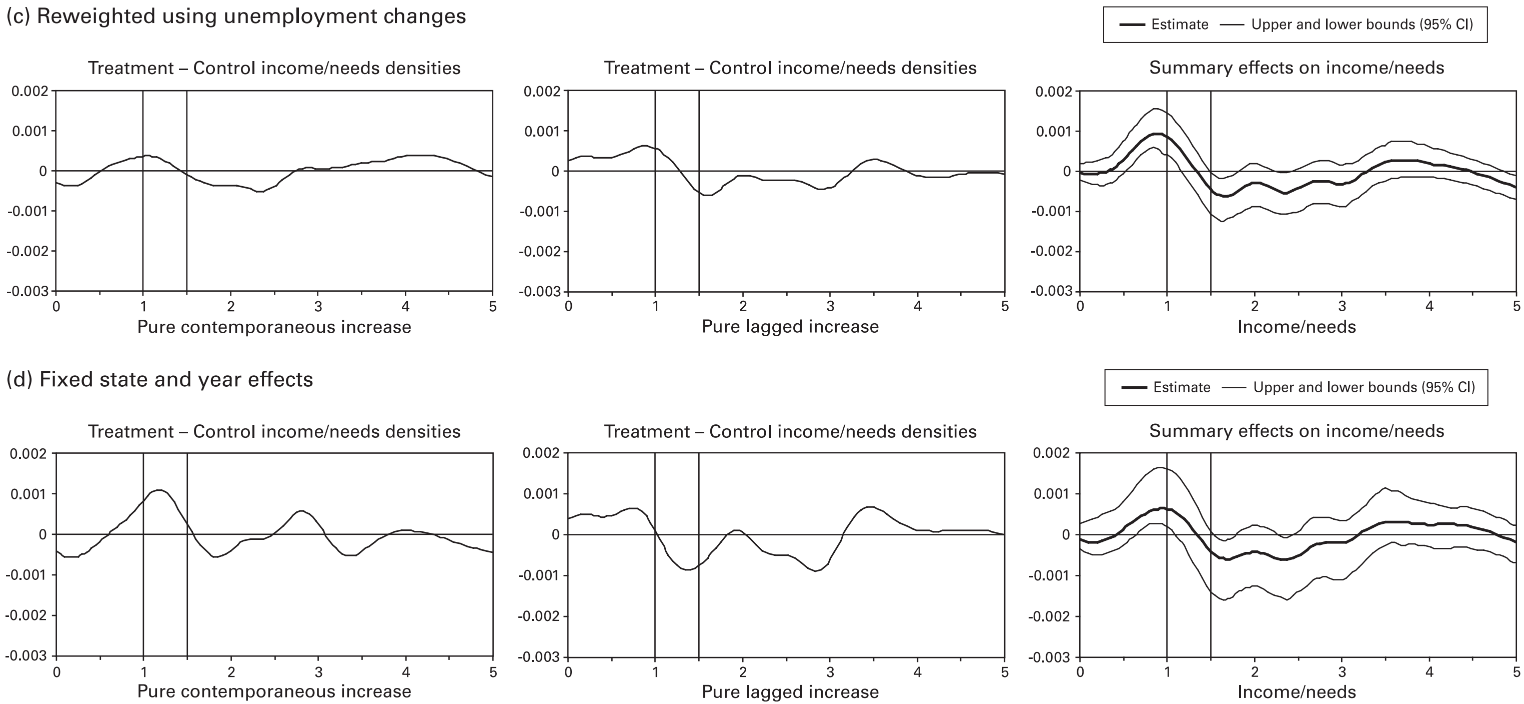


Figure 5.2
Alternative estimates of effects of minimum wages on the distribution of family income/needs, 1986–1995
Source: Neumark, Schweitzer, and Wascher 2005.
Note: Each panel shows estimates paralleling panel c of figure 5.1, with the differences in sample or estimation method as indicated in the panel heading.

proportion of families in the range from about 1.5 to 3. Thus, the evidence suggests that minimum wages increase the number of poor and near-poor families, and that these newly poor families come from the ranks of lower-income, nonpoor (and non-near-poor) families.[37] The second row of table 5.2 summarizes the results from the last panel in figure 5.2 for broader income-to-needs categories. The estimates are very similar to those in the first row, with estimated increases in the proportion of families poor and near-poor, and reductions in the proportions with incomes between 1.5 and 3 times the poverty line.

5.3.3.3 Robustness and Verification We have performed a number of additional analyses to verify that these results actually reflect effects of minimum wages, and that our estimates are plausible. First, we checked for minimum wage effects in parts of the income-to-needs distribution where there should be no effects. As can be seen in figure 5.1, the estimated changes in the density from 3 to 5 times the poverty line are small and, as depicted by the confidence intervals, not statistically significant. Furthermore, although not reported in table 5.2, the estimated minimum wage effects on the density defined over income-to-needs ratios between 3 and 5 (as well as over the 3 to 4 and 4 to 5 ranges separately) are always small and statistically insignificant. The fact that we do not find an effect of the minimum wage on higher-income families suggests that the changes in the income-to-needs distribution that we find for lower-income families can be attributed to increases in minimum wages.[38]

Second, one potential problem with any difference-in-differences estimator is that different trends in the treatment and control groups can lead to incorrect conclusions about the treatment effect. One way to see if this is a problem in our analysis is to test for "lead" effects of minimum wages. If the difference-in-differences procedure shows that future minimum wage increases are associated with the same types of effects on the income-to-needs density that we obtain from the contemporaneous and lagged effects, we might conclude that our estimates are picking up differential changes over time in the treatment and control groups that are not truly attributable to minimum wage increases.[39] The results using one-year leads are displayed in figure 5.3; the figure shows the equivalent information to that in panel b of figure 5.1 (except for showing the "leading" effects) and the middle graph of panel c of figure 5.1. The estimates reveal no "effect" of future minimum wage increases on the income-to-needs density over the part of

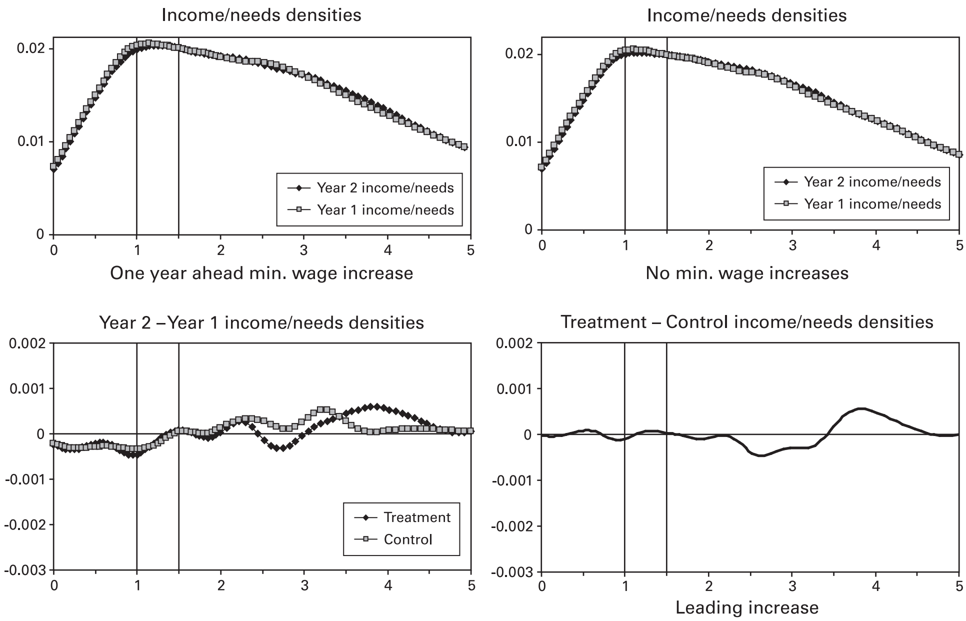


Figure 5.3
Estimated leading effects of minimum wages on the distribution of family income/needs, 1986–1995
Source: Authors' calculations.
Note: The first three graphs (moving left to right across each row) correspond to the graphs in panel b of figure 5.1 (except showing the "leading" rather than the lagged effects), and the bottom right-hand graph corresponds to the middle graph of panel c of figure 5.1.

the distribution that is less than twice the poverty line.[40] Interestingly, the results do hint at small leading effects for higher ranges (roughly 3.5 to 4.5 times the poverty line), matching those depicted in figures 5.1 and 5.2 (although these are nearly always insignificant, as noted previously). This result suggests that the indication of contemporaneous and lagged effects at these higher ranges may reflect common trends in states where minimum wages increased, rather than causal effects of minimum wages. If so, this evidence further reinforces our conclusion that minimum wage effects are concentrated in the lower parts of the income-to-needs distribution.

Third, we investigated whether states with larger minimum wage increases experienced bigger changes in their income-to-needs distributions. Although our non-parametric procedure is not designed to take explicit account of continuous variation in the size of the minimum wage increase, some minimum wage increases in our sample are quite small (e.g., 10 cents in Minnesota in 1990), and others are relatively large (e.g., 80 cents in New Jersey in 1992). We divided the sample of state-year observations with minimum wage increases into those with small increases (less than the median increase of 45 cents) and those with larger increases (greater than or equal to 45 cents), and then recomputed our estimates for these two treatment samples, relative to the sample of state-year observations with no minimum wage increases. As would be expected if we are capturing real effects of minimum wages, the estimated effects are stronger in the subsample with larger minimum wage increases. For example, when we restrict the treatment group to those observations with above-median changes in the minimum wage, the estimated effect of an increase in the minimum wage is to raise the proportion of families in poverty by 0.0120, well above the estimated effect for the entire sample (0.0083) shown in the first row of table 5.2. Correspondingly, when we use only the subsample of observations with below-median minimum wage increases for the treatment group, this estimate falls to −0.0006. Similar patterns are evident for the estimated changes in the proportion poor or near-poor; the estimate in table 5.2 is 0.0130, compared with 0.0157 for large minimum wage increases and 0.0076 for small minimum wage increases.

Fourth, the methods described in this subsection are something of a "black box," in that they provide estimates of the net effects of minimum wages on income-to-needs ratios without tracing out all of the channels of influence. The credibility of our finding that minimum wages lead to increases in the proportions of poor or near-poor families is strengthened by the evidence discussed in the previous chapter (based on Neumark, Schweitzer, and Wascher 2004), which indicates that low-wage workers suffer earnings declines when minimum wages increase.

Table 5.3 illustrates more clearly how families with incomes initially above the poverty or near-poverty line might be adversely affected by an increase in the minimum wage. In particular, although minimum wage workers (those earning less than 110 percent of the minimum) account for a very small share of primary earners in families above 1.5 times the poverty line (the second panel), it is not unusual for the lowest-paid worker in these families to be paid at or below the minimum wage (the third panel). And as shown in the fourth panel, which presents the distribution of workers in each wage category across

Table 5.3
Wages and family income-to-needs

	Income-to-needs ranges				
	0–0.5	0.5–1	1–1.5	1.5–2	2–3
	(1)	(2)	(3)	(4)	(5)
Share of families with at least one worker earning less than 110 percent of minimum wage that are exposed to minimum wage increase	0.19	0.13	0.10	0.09	0.06
Distributions of primary earners in family income-to-needs category by hourly earnings:					
Less than 90 percent of minimum	0.49	0.27	0.12	0.06	0.03
90–110 percent of minimum	0.17	0.18	0.12	0.05	0.02
110–200 percent of minimum	0.25	0.43	0.53	0.50	0.29
More than 200 percent of minimum	0.09	0.12	0.23	0.39	0.66
Distributions of lowest earner in family income-to-needs category by hourly earnings:					
Less than 90 percent of minimum	0.57	0.52	0.41	0.34	0.25
90–110 percent of minimum	0.20	0.16	0.18	0.17	0.14
110–200 percent of minimum	0.17	0.26	0.32	0.40	0.45
More than 200 percent of minimum	0.06	0.06	0.08	0.10	0.16
Distributions of workers by family income-to-needs:					
Less than 90 percent of minimum	0.13	0.15	0.12	0.11	0.18
90–110 percent of minimum	0.08	0.14	0.15	0.11	0.19
110–200 percent of minimum	0.03	0.08	0.12	0.14	0.23
More than 200 percent of minimum	0.01	0.01	0.03	0.05	0.16
N	2,979	5,980	8,852	10,741	24,420

Source: Neumark, Schweitzer, and Wascher 2005.
Note: Income-to-needs categories and income measures are reported for year 1 for each family. Hourly earnings are calculated using annual wage and salary income/{(weeks worked last year) × (usual hours worked last year)}; this way, we use the full March files, rather than only the ORG files. In the second and third panels the columns sum to 1; in the fourth panel the rows sum to 1, but entries are not shown for income-to-needs greater than 3. The third panel is restricted to families with at least 2 earners.

income-to-needs categories, the proportion of minimum wage workers (including those below the minimum) in families with incomes between 1.5 and 3 times the poverty line is nearly as large as in families between 0 and 1.5 times the poverty line, and is actually larger than the proportion of such workers in families below the poverty line.

Thus, the evidence that minimum wage increases cause families with somewhat higher incomes to fall below the near-poverty line could easily reflect job losses among low-wage secondary workers in these families. Moreover, the numbers of such secondary workers suggest that the magnitudes of the estimated minimum wage effects that we report are quite plausible. For example, about 22 percent of families with income-to-needs between 1.5 and 2 had at least two workers in our sample. In addition, as indicated in the third panel of table 5.3, 51 percent of the lowest earners in this set of families earned less than 110 percent of the minimum. Thus, about 11 percent (22 percent times 0.51) of families in the 1.5 to 2 income-to-needs range had a worker who was paid close to the minimum wage, and only a small share of these workers would have had to become nonemployed to generate, for example, the estimated 0.46 percentage point increase in the share of families with income-to-needs between 1 and 1.5 that is reported in table 5.2.

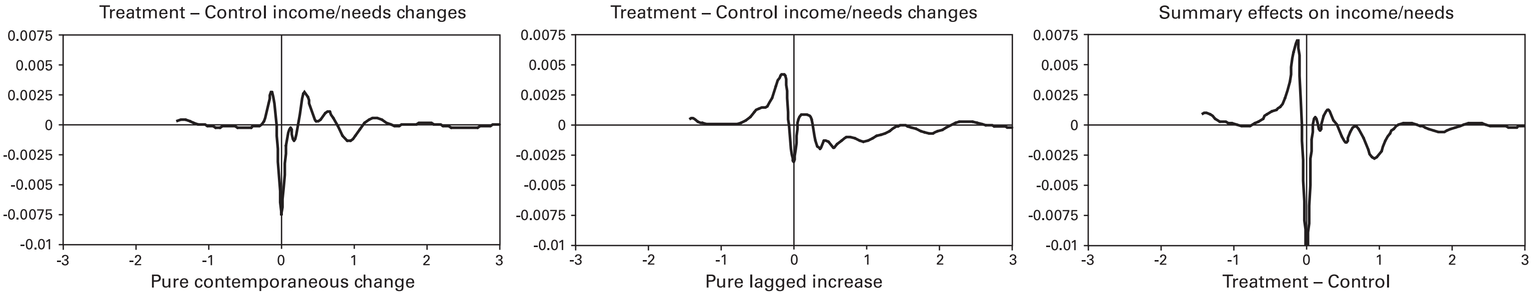
To verify that the changes in the income-to-needs distribution are associated with relatively small declines in the incomes of families with low-wage workers, we also applied our difference-in-differences procedure for estimating the effects of minimum wages to the distributions of *changes* in income-to-needs at different parts of the initial income-to-needs distribution. The analysis is performed separately for families initially in each of the following four (not mutually exclusive) income-to-needs categories: 0 to 1.5, 1.5 to 3, 1.5 to 2, and 2 to 3.[41]

Figure 5.4 reports the estimates for the densities of changes in income-to-needs for each of these categories.[42] As before, we estimate the densities separately for contemporaneous and lagged increases, versus the control group of no increases. Panel a of figure 5.4 shows the results for families initially in the 0 to 1.5 income-to-needs category. The left-hand graph shows the estimated effects of a contemporaneous minimum wage increase. Consistent with our previous finding that the wage increase is the dominant contemporaneous effect, the most notable feature of this graph is the positive mass to the right of zero. This indicates that the contemporaneous effect of a minimum wage increase is to raise the proportion of families that experience an increase in their income-to-needs ratio. The middle graph displays the estimated lagged effects. This graph is more suggestive of disemployment effects or hours reductions, with the positive mass to the left of zero indicating an increase in the proportion of families that experience a decline in their income-to-needs ratios, and the trough to the right of zero indicating a decline in the proportion of families that experience an increase in income-to-needs. Finally, the right-hand graph displays the total effects of minimum wage increases. The picture is relatively unambiguous, with its most prominent feature being the positive mass to the left of zero. This implies that the net effect of minimum wage increases on poor and near-poor families is a decline in income-to-needs.

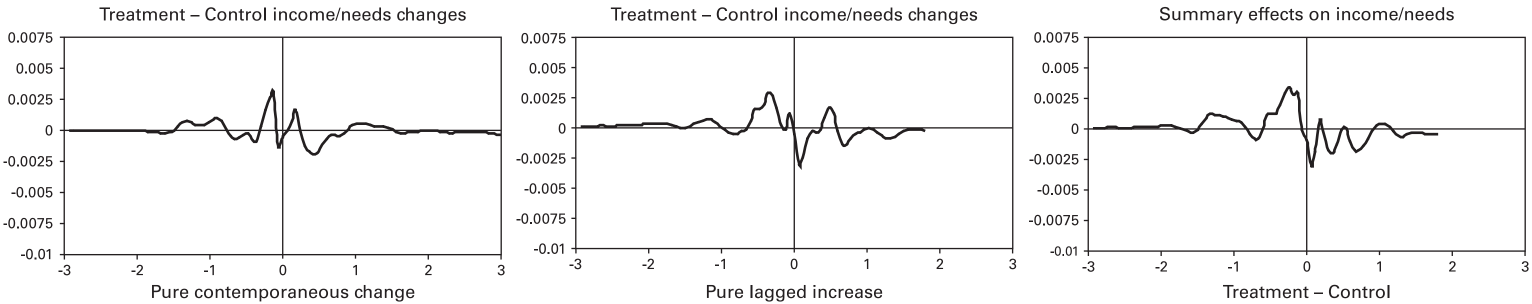
The graphs in panel b of figure 5.4 report a similar analysis for families initially in the 1.5 to 3 income-to-needs range. Focusing on the total effects displayed in the right-hand graph, we again see that minimum wage increases result in a net increase in the proportion of families that experience a decline in income-to-needs, and a net decrease in the proportion that experience an increase in income-to-needs. Note that the bulk of the positive mass to the left of zero in this graph is between 0 and −1, which suggests that relatively few families with income-to-needs initially above 2 are falling into poverty. Instead, we suspect that some families with income-to-needs of about 2 are falling to 1.5 or so, while others with income-to-needs of about 1.5 are falling into poverty. Thus, what we ultimately observe can be thought of as the cumulative effect of many families making small movements to the left in the income-to-needs distribution. To explore this further, panels c and d in figure 5.4 break out the results for those with initial income-to-needs of 1.5 to 2, and 2 to 3. The same qualitative pattern of a positive mass corresponding to small declines in income-to-needs and a trough at small increases appears for both groups. The declines for those with income-to-needs initially in the 2 to 3 range tend to be less than 0.5, suggesting that the declines into poverty or near-poverty are generally coming from families that are initially in the 1.5 to 2 range, or just above the near-poor cutoff.

Overall, the results from this study offer no empirical support for the view that minimum wage increases reduce the proportions of poor and low-income families. In this regard, the results reinforce the other evidence reviewed in this chapter, virtually none of which detects beneficial distributional effects of minimum wages. Moreover, the evidence indicates that the net effect of higher minimum wages is, if anything, to increase the proportions of families that are poor and near-poor. This more negative conclusion about the effects of minimum

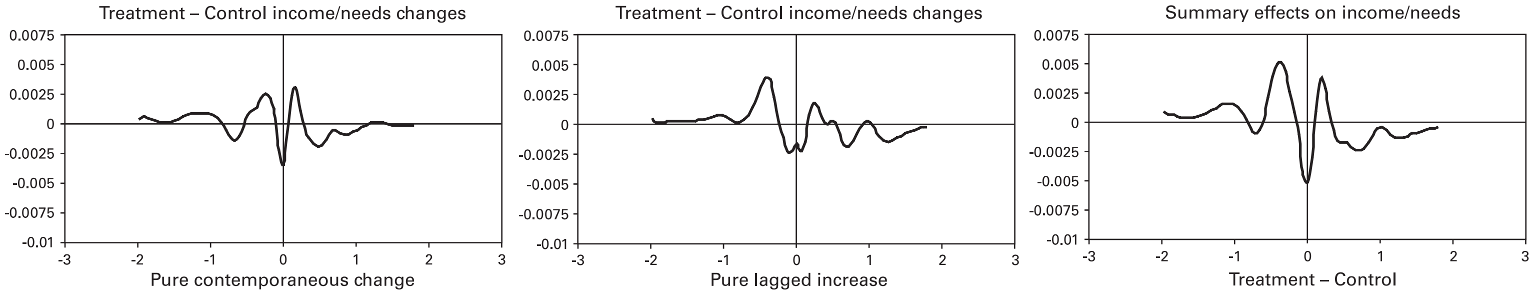
(a) Initial income/needs 0–1.5



(b) Initial income/needs 1.5–3



(c) Initial income/needs 1.5–2



(d) Initial income/needs 2–3

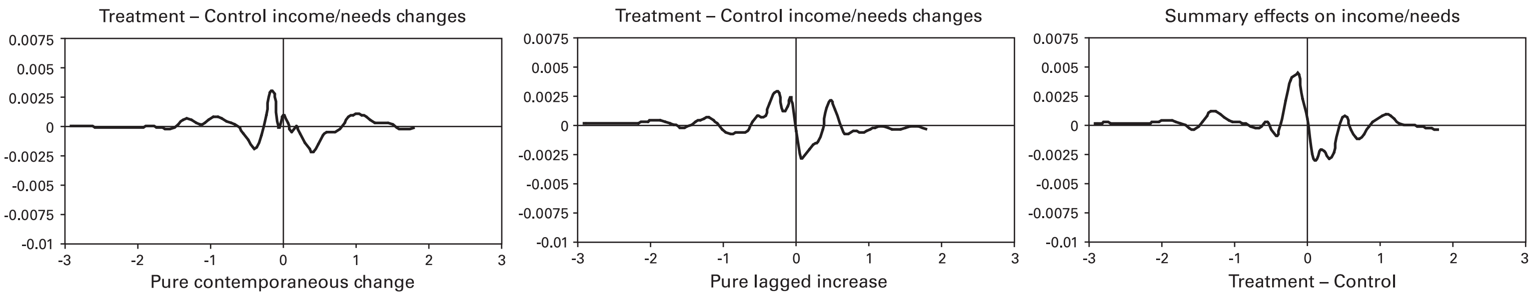


Figure 5.4
Estimated effects of minimum wages on the distribution of changes in family income/needs, 1986–1995
Source: Authors' calculations.
Note: Each panel shows graphs corresponding to panel c of figure 5.1, but for densities of changes in income-to-needs.

wages is consistent with the findings in Neumark and Wascher 2002b, although the approach discussed in this section yields some findings that are statistically stronger.

5.3.3.4 Other Evidence Similar conclusions about the distributional effects of minimum wages—pointing to no beneficial and possibly adverse effects—are reported by Wu, Perloff, and Golan (2006), using similar data and a conceptually similar, albeit mechanically different, approach. In particular, Wu, Perloff, and Golan construct a variety of family income inequality measures by state and year, including the coefficient of variation, the relative mean deviation, the Gini index, and the standard deviation of logarithms.[43] In addition, they estimate versions of the Atkinson index that range from very egalitarian to very non-egalitarian in terms of the index's correspondence to a social welfare function; the Atkinson index can be transformed into a monetary measure that is interpreted as the level of income per unit (families, in this case) that, if income were equally distributed, would give the same level of social welfare as the actual income distribution.

In addition to data on minimum wages by state and year (using the highest of the state and federal minimum wage), the authors assemble data on parameters describing a large set of policies that may affect family income (discussed in greater detail in the next section). They then estimate a regression model relating their state-by-year inequality measures to all of these policies, plus controls for economic conditions, education, and family structure. The model also includes state dummy variables. It does not include year dummy variables or time trends, which is unfortunate, given the evidence of trend increases in wage inequality over this period (discussed in chapter 4).[44]

For most of the measures of family income inequality, the evidence suggests that minimum wage increases tend to raise inequality.[45] The only exceptions are for the Gini index and the coefficient of variation; however, for these measures the estimated effect is statistically insignificant (*t*-statistics below 1). In addition, the estimated positive effect on inequality is statistically significant for the standard deviation of logarithms, which places more weight on transfers at the lower end of the income distribution; in contrast, the coefficient of variation weights transfers anywhere in the distribution equally, and the Gini index attaches more weight to transfers in the middle. Thus, these results imply that when we focus more on whether those at the bottom of the income distribution are helped or hurt by a higher minimum wage, minimum wage effects are more adverse. Using the Atkinson index, the evidence always points to the minimum wage increasing inequality (and hence decreasing welfare, as the authors do not estimate this model for the limiting case when the distribution of incomes is irrelevant to welfare). In addition, the evidence is statistically significant and the estimate is larger for versions of the index that place more weight on transfers at the low end of the income distribution, thus echoing the results for the standard deviation of logarithms measure.[46]

Taken as a whole, the evidence from studies that directly estimate the effects of minimum wages on the distribution of family incomes can be viewed as leading to one of two conclusions, depending on exactly what specifications and approaches one prefers. Either the minimum wage has no distributional effects, or it harms those at the bottom of the income distribution. In no case, though, is there evidence that minimum wages help poor or low-income families. One important qualification worth emphasizing, though, is that the research we have discussed is based on data that ends in the mid- to late-1990s. Given the considerable changes in recent years in labor market incentives for low-income households, especially those headed by single females, there is a clear need for more research that revisits this type of evidence in the post-welfare reform era.

5.3.4 International Evidence

There is very little evidence on the distributional effects of minimum wages from other countries. And what evidence there is comes from developing countries, where the distributional effects may be quite different than in the United States. Although the theoretical prediction of disemployment effects (at least in the covered sector) should carry over to developing countries—and based on the evidence in chapter 3, it largely does—there is a much greater degree of wage and income inequality in those countries, a much higher share of workers affected by the minimum, and numerous other institutional differences that could lead to quite different distributional effects.

The World Bank report (2006) reviews a few studies, along with some evidence of its own, and concludes that the distributional effects are ambiguous.[47] In particular, the report concludes that minimum wages tend to have no effect on the poverty rate, but effects on incomes of the poor that vary by country.

Neumark, Cunningham, and Siga (2006) study the case of Brazil, which is of interest because it has one of the highest levels of inequality in the world, has historically used the minimum wage as a tool of social policy, and raised its minimum wage substantially in recent years. Unlike other work for Brazil, this study uses the same kind of before-and-after analysis of minimum wage changes that has been applied to U.S. data. However, because of data limitations, the authors could only estimate the effects of minimum wages on the per capita income distribution, and the paper focuses on the 10th, 20th, and 30th percentiles of that distribution. Although the minimum wage is national, wage and income levels vary sharply across regions of Brazil, and so a fraction-affected variable is used to capture minimum wage effects. The results largely parallel those for the United States. The contemporaneous effects of minimum wages on these percentiles of the income distribution are close to zero at the 10th and 30th percentiles, but positive and significant at the 20th percentile. But once lagged effects up to a year are allowed, the evidence generally points more strongly to adverse effects. Moreover, the evidence points to disemployment effects among household heads, which can help explain these income declines. But the evidence is not always robust across different specifications (especially with regard to statistical significance), and thus might largely be viewed as providing no support for beneficial distributional effects of minimum wages.

Gindling and Terrell (2007c) study the distributional effects of the minimum wage in Honduras. They use the same data source discussed in chapter 3, but for a shorter sample period (2001–2004) for which data on family poverty status is available. As in their research on employment effects, they find that minimum wage effects are concentrated in the large-firm sector. For workers in this sector, they estimate that minimum wages reduce the likelihood that a worker is in a poor or extremely poor family, with elasticities of −0.1 and −0.18, respectively.[48] However, their estimates focus only on workers and unemployed individuals who can be assigned to a particular industry and sector (because they previously worked in that industry and sector). As a result, the estimates ignore the effects on poverty arising from changes in job opportunities for new entrants to the labor force. In addition, unemployed individuals who previously worked in a particular industry and sector are assigned the current minimum wage in that industry and sector; however, it is not clear that this is the relevant minimum for those individuals.

Most important, however, recall that the Honduran minimum wage varies by industry (and along other dimensions). Although an industry-level analysis may be appropriate for studying employment effects, using this approach to estimate distributional effects seems problematic. The standard neoclassical model, for example, predicts that an increase in the minimum wage in an industry will lead employers to substitute towards higher-skilled workers. (The results of Gindling and Terrell 2007b are consistent with these employment effects.) These higher-skilled workers are probably less likely to be poor, but one would not want to claim that an increase in earnings in an industry due to substitution toward higher-skilled workers reflects poverty-reducing effects of the higher minimum wage in the industry.[49] Similarly, if workers are displaced from an industry where the minimum wage has risen and find work in other industries, the outward supply shift in the destination industries can lead to lower average earnings there, also generating spurious evidence suggesting that the relative decline in the minimum wage in those industries led to higher poverty (or conversely, that a higher minimum wage reduces poverty). As a consequence, we do not believe that an industry-level analysis appropriately addresses the distributional effects of minimum wages. It may be possible to estimate distributional effects of minimum wages when minimum wages vary by industry, but not by studying the cross-industry variation in minimum wages.

Research for Colombia (Arango and Pachón 2004) uses an identification strategy relatively similar to that used for Brazil by Neumark, Cunningham, and Siga 2006. The authors report a number of dynamic panel specifications that require use of lagged minimum wages as instruments. We have already criticized this approach, and the authors do not offer a strong rationale for these specifications relative to the "long-run" specifications that are typical in the literature. However, in the one table that does report estimates from long-run specifications, the authors find that higher minimum wages reduce family earnings at the 5th and 10th percentiles (the estimates are insignificant), but increase earnings significantly at the 20th percentile and above (all the way up to the 90th percentile, with the estimates rather similar from the 30th to the 90th percentiles). Other estimates in the paper also point to positive effects of minimum wages at relatively high percentiles of the per capita family earnings distribution, but adverse effects on those at the bottom of the distribution. Paralleling some of the earlier discussion for Brazil, we are skeptical of studies that find positive effects from minimum wages at the 80th and 90th percentiles of the family earnings distribution when there are few minimum wage workers in such families.[50]

Clearly, the evidence to date on the distributional effects of minimum wages in developing countries is somewhat mixed. Given the arguments in this chapter, that may not be surprising, as the answer may well differ from country to country. At any rate, it is clear that much more evidence is needed on distributional effects of minimum wages in other countries—both developed and developing countries. Nonetheless, it is important to emphasize the near-complete absence of compelling evidence of beneficial distributional effects of minimum wages in other countries.

5.4 Alternatives to the Minimum Wage

Because the Wu, Perloff, and Golan (2006) study of the effects of minimum wages on the distribution of incomes also includes estimates of the effects of many other policies, it provides a nice segue into a question that follows naturally from our analysis of the distributional effects of minimum wages. In light of our conclusion that minimum wages may have, if anything, adverse distributional effects on poor and low-income families, are there other policies that we should pursue *instead of* higher minimum wages to redistribute income to low-income families?

In most research studying this question, the minimum wage is contrasted with EITC. This comparison is natural, because the EITC subsidizes earnings for low-income working families and creates incentives for employment among families with no workers—pursuing much the same goals as suggested by the rhetoric, if not the reality, of minimum wages. The EITC payment is determined by four policy parameters. First, for those with the lowest earnings, there is an earnings subsidy—a percentage of earnings determined by the phase-in rate. Currently, the federal phase-in rate for a family with two or more qualifying children is 40 percent. Second, there is a maximum benefit level, which for the same type of family was $4,536 in 2006 (this level is indexed and thus rises each year). Third, there is a "plateau," or an income range over which the maximum benefit remains fixed (in 2006, from $11,340 to $14,810). And fourth, there is a phase-out rate at which the credit is reduced as income rises (currently 21.05 percent).[51] In addition to the federal EITC program, many states have their own EITCs that supplement the federal program (see Neumark and Wascher 2007b).

The EITC is a complex program that introduces a variety of incentives regarding labor market (and other) behavior, and as a result has attracted a significant amount of research attention in its own right. Good surveys of this research literature are provided by Hoffman and Seidman (2003) and Hotz and Scholz (2003), and thus we include only a brief discussion here. Regarding labor market behavior, for families with only one adult and small children—typically the female-headed households for which poverty might be most problematic—theory predicts that the EITC will have a positive influence on employment, because the EITC raises the effective wage; for those previously nonemployed, there is only a substitution effect and no income effect, and someone who chose to work prior to the EITC will still do so.[52] Hours effects are more ambiguous. In the phase-in range, there are offsetting income and substitution effects. On the plateau, there is only a negative income effect. And in the phase-out range, both income and substitution effects create incentives for reduced hours. The research summarized in the surveys mentioned previously is fairly unambiguous in indicating that the EITC boosts employment of single mothers. Hours effects for those already working but with a somewhat higher income (perhaps a working spouse) appear to be modestly negative.

The implication of these findings is that the EITC is likely to boost the incomes of the poorest families, in particular. Moreover, as this discussion makes clear, the EITC targets *low-income families*. This contrasts sharply with the minimum wage, which of course targets *low-wage individuals*. Given what we have said in this chapter about the weak link between the two, we would expect the EITC to be more effective at reducing poverty and helping low-income families than is the minimum wage.[53]

At the same time, it is important to note that the EITC does not target poor families perfectly. Most important, the break-even point (at which benefits in the phase-out range have fallen to zero) occurs well above the poverty line. Part of the reason for this is that if benefits were phased out too quickly, strong labor supply disincentive effects (in terms of hours) would be created. In addition, because it is based on income rather than wages, it is possible that the EITC sometimes subsidizes workers who have high skills but work low hours. The EITC may not reach families that earn such low incomes that they do not file income tax returns, and it may sometimes subsidize higher-income families in which the adults are unmarried but cohabiting, and whose joint income if they married would make them ineligible. Nonetheless, calculations suggest that the EITC targets reasonably well, with very few dollars going to families in excess of twice the poverty line (Liebman 1998) and nearly one-half of payments going to poor families (Scholz 1994); these calculations are based on the types of simulated policy effects that we criticized in the minimum wage literature, rather than direct estimates of effects of policy changes. However, other research provides more compelling evidence of the better distributional impact of the EITC.

For example, in Neumark and Wascher 2001a, we use the same CPS data as in our studies of the distributional effects of minimum wages discussed earlier in this chapter to estimate and compare the effects of changes in the minimum wage and the EITC on transitions into and out of poverty (and on changes in income-to-needs). We use state variation in both of these policies; for the EITC, this is captured by the phase-in rate, which, because of the interaction between state and federal EITCs, varies by year and state and with the number of children. It would be unsurprising if the EITC lifts families out of poverty when we include both earnings *and* EITC payments in our measure of income. However, we forego using estimated EITC payments (and other transfers), and instead just study earned income. This can be viewed as providing a particularly strong test of the effectiveness of the EITC, in that we ask whether increased generosity of the EITC or the minimum wage raises the probability that a family *earns its way* out of poverty. Such evidence would suggest that the incentives created (and not just the checks written) by the EITC are prowork and antipoverty.

The evidence suggests that greater EITC generosity increases the probability that families' earnings rise above the poverty level, and, more generally, increases the earnings of families that are initially below the poverty line. In contrast, there is no evidence that the EITC pushes near-poor families into poverty (based on earnings) because of adverse effects on hours or on earnings of those initially near-poor.[54] The implied effects on escaping poverty are large, suggesting that the average increase in the federal credit rate over the sample period (4 percent) reduces the poverty rate by about 0.029, or nearly 3 percentage points.[55] Thus, it is clear that the EITC has better beneficial distributional effects than the minimum wage.

Some of the other research on the distributional effects of minimum wages discussed earlier in this chapter also includes a comparison to the EITC. In contrast to our results, Gunderson and Ziliak (2004) find that the EITC is associated with an increase in the poverty rate using either the before- or after-tax measure (the latter incorporates the credit), although only some of the estimates are statistically significant.[56] For the squared poverty gap, they find no significant effects of the EITC with the before-tax measure, but some evidence consistent with a reduction in the gap using after-tax income. However, this reduction in the gap is only evident for married-couple families. A potential problem with their specification is that it does not include year effects and does not allow the effects of the EITC and other policies to vary with the number of children in the family, which in our analysis (Neumark and Wascher 2001a) appeared to be quite important. Overall, however, the evidence from this research is less favorable to the EITC than is our own research.

As noted previously, the Wu, Perloff, and Golan (2006) study also estimates the effects of some redistributive policies other than the minimum wage. These policies, and the way they are measured, are as follows: income tax rates (the federal marginal tax rate for the top and bottom brackets); unemployment insurance (the maximum weekly benefit); Supplemental Security Income (using the maximum benefit for individuals living independently); AFDC (a maximum benefit measure, the maximum income at which a family remains eligible, and a dummy variable equal to one for the year in which welfare reform began and thereafter); disability insurance (the annual benefit); food stamps (the maximum monthly benefit); and the Earned Income Tax Credit (the maximum benefit and the phase-out rate). Their evidence generally indicates that the maximum EITC benefit reduces inequality.[57] Interestingly, they find statistically significant negative effects of the EITC benefit on inequality for all specifications *except* variants of the Atkinson index that place relatively more weight on the low end of the income distribution, which the authors suggest occurs because most of the benefits of the EITC accrue to families on the plateau and in the phase-out range.[58] They also find that the phase-out rate increases inequality for nearly every inequality measure, presumably because, all else the same, a higher phase-out rate reduces hours and earnings among moderate-income families.

In addition to these empirical analyses, it is possible—paralleling the earlier simulations of the effects of minimum wages—to simulate the effects of the EITC, and to contrast its distributional effects with those of minimum wages. Formby, Bishop, and Kim (2005) present an ambitious analysis of this type that also includes a third redistributional policy—rebating payroll taxes to workers in families with incomes below twice the poverty line. They begin by considering a $1.00 increase in the minimum wage. Using March CPS data for 2001, they make assumptions about how the minimum wage increase affects the wage distribution and how it affects employment (using a range of estimated employment effects, including one simulation with no employment effects). They then attempt to consider "equal cost" expansions of the EITC or payroll tax rebates by taking the change in the wage bill associated with the minimum wage increase and instead (1) distributing that amount to current (estimated) EITC recipients, in an amount proportional to their current credit, or (2) distributing it to workers in low-income families, in proportion to their estimated payroll taxes.[59] They calculate the implications for income, allowing for the possibility that increased income from the minimum wage leads to a reduction in EITC payments (for families in the phase-out range), as well as for negative effects of the EITC and tax rebate on the labor supply of married women.

Based on these simulations, they conduct an applied welfare economics analysis to see what can be established about the effects of these three policies on the distribution of incomes, other than identifying winners and losers. The first criterion they consider is first-order dominance, in which income distribution A first-order dominates income distribution B only if no family's income is lower in A, and at least one family's income is higher. That is, first-order dominance is solely a Pareto efficiency criterion. Because the different policies deliver gains in different parts of the income distribution, there is no way to establish first-order dominance of any policy. Second-order dominance is a weaker criterion based on the Pigou-Dalton transfer principle, which builds in a preference for equity by assuming that a transfer from a higher-income to a lower-income family increases welfare. Their results show that the EITC second-order dominates the minimum wage because the EITC better targets low-income families. In contrast, the payroll tax does not second-order dominate the minimum wage.[60] Based on these conclusions, they argue that the EITC is the superior redistributive policy, especially in comparison with the minimum wage.

Although the Formby, Bishop, and Kim analysis is interesting, it ultimately suffers from the same generic criticism that we leveled at other simulation exercises—that it rests on numerous assumptions and simplifications that influence the results. A few other problems stand out in this study as well. First, the authors simulate the disemployment effects of minimum wages as proportional hours reductions across all workers in groups for which they assume disemployment effects, even though these effects are estimated for groups that include both affected and unaffected individuals. As we noted earlier, this approach guarantees that the earnings of all affected workers will increase in response to a higher minimum wage. In reality, we would expect some workers to lose their jobs as a result of a minimum wage increase, and these workers could end up with lower income. The problem, of course, is that studies of the employment effects of minimum wages give us no guidance as to *which* workers in a particular group will end up nonemployed, nor how they are distributed across the family income distribution. As a result, we are skeptical of any conclusions reached from these simulations about how a minimum wage increase affects the income distribution. It is likely that some families gain more, and some lose considerably more, than in the authors' calculations.

Second, Formby, Bishop, and Kim allow for negative hours effects of the EITC on married women's labor supply but not for positive employment effects for single adult heads of households, despite clear evidence of such positive employment effects in the literature. Allowing for this effect would presumably increase the attractiveness of the EITC relative to the minimum wage even more. In addition, the authors do not consider who pays for the different policies and how this burden is distributed throughout the income distribution. There is little work on this topic, with the exception of research that attempts to address the issue of increased prices from a higher minimum wage and who pays them (O'Brien-Strain and MaCurdy 2000). Other important cost-related issues might include, for example, the relative income of business owners who bear the burden of a higher minimum wage as compared to the taxpayers who would finance the EITC (or payroll tax rebates). Moreover, the effects of these policies on incomes would have implications for other government transfer programs that, as best we can tell, are ignored in the analysis.

Finally, when Formby, Bishop, and Kim consider the effects of expanding the EITC or reducing the payroll tax, they assume that all of the gains accrue to workers. However, the actual incidence of these policy changes will likely differ from that assumption. For example, as discussed earlier, both Leigh (2007) and Rothstein (2007) report that a more generous EITC lowers market wages because of the supply response. With a payroll tax reduction, the standard textbook analysis suggests that the wage net of payroll taxes rises by less than the tax reduction. To be sure, Hamermesh (1993) concludes that workers bear most of the burden of the payroll tax in the longer run. However, the available estimates of payroll tax incidence do not come from studies that explicitly consider payroll tax rebates for workers in low-income families, and thus the effects assumed by Formby, Bishop, and Kim in their simulations are subject to considerable uncertainty.

We do not mean to suggest that it is easy to take account of all of these influences. And even if this were done, there would still be concerns about the validity of the assumptions used; for example, based on the available evidence, the authors seem to use too large an implied wage spillover effect when they assume that all workers with wages up to 125 percent of the original minimum wage receive a wage increase. Our general point is that the difficulty of this type of exercise, the innumerable assumptions that have to be made, and the possibility of behavioral responses that are ignored, all suggest that empirical before-and-after analyses of the distributional effects of minimum wages and other policies are likely to provide more reliable evidence. Indeed, we do not even have to agree on baseline disemployment effects of minimum wages to conduct this type of study.[61]

There is good reason, on a priori grounds, to believe that the EITC is a superior policy to the minimum wage for helping poor or low-income families. And, despite some of the criticisms mentioned previously, the simulation studies are helpful in formalizing the a priori argument about the superiority of the EITC. However, as with evidence on minimum wages, in establishing the superiority of the EITC we put much more store in the before-and-after studies that compare the distributional effects of alternative policies. Most of the evidence from these latter studies tends to show that the EITC does serve to help low-income and poor families—in contrast to the minimum wage. This conclusion does not hold across all of the existing research, but it does hold for what we regard as the more compelling evidence. Clearly, though, more research on this question is needed.

Finally, even the evidence that finds beneficial distributional effects of the EITC points out that the EITC may not be effective for families at the very bottom of the family income distribution, although that is almost surely true of the minimum wage as well. Both of these policies are aimed at raising incomes of those who work. However, other policies—for example, those focused on disability—are needed to address the income needs of families with no workers. Finally, the EITC offers virtually no benefits to unrelated individuals, who may also be the intended beneficiaries of redistributional policies.

5.5 Conclusions

For many economists, the prediction that minimum wages reduce employment is viewed as unassailable, a consequence of the law of demand. In contrast, there is no theoretical prediction about the distributional effects of minimum wages. Even an economist with 100 percent certitude that minimum wages reduce employment has to admit the possibility that minimum wages may, on the whole, benefit poor or low-income families; indeed, we would argue that this has been the prevailing view. It is perhaps ironic, then, that with respect to one of the questions regarding minimum wages about which there is *no* theoretical prediction, the evidence appears *least* ambiguous. In contrast to the case for disemployment effects, where some evidence points to no effect or positive effects (although we are critical of this evidence), there is essentially no empirical evidence indicating that minimum wages have beneficial distributional effects. Instead, the research tends to find either no evidence of distributional effects or evidence that minimum wages increase poverty.

In our view, the combined evidence is best summarized as indicating that an increase in the minimum wage largely results in a redistribution of income *among* low-income families, with some gaining as a result of the higher minimum wage and others losing as a result of diminished employment opportunities or reduced hours, and some likelihood that, on net, poor or low-income families are made worse off. Were there compelling evidence that the families that gain are disproportionately those to which we might want to redistribute income—such as poor households with children—then it would be possible to imagine that the evidence masks some beneficial distributional effects. But there is, as yet, no evidence to support this contention either.

We began this chapter by referring to the question originally posed by Gramlich—whether the negative distortionary effects of minimum wages reflected in disemployment effects are outweighed by beneficial distributional effects. If the evidence pointed to beneficial distributional effects, we would face the challenging question of trying to determine how to balance the efficiency and equity effects. But in the absence of beneficial distributional effects, the tradeoff simply does not arise. In our view, this makes it difficult to justify the policy arguments often made in support of a higher minimum wage.

However, the evidence on the distributional effects of minimum wages is nearly entirely from the United States. Because these distributional effects depend on so many factors—including the magnitudes of gains and losses to many different types of workers, the location of these workers in the family income distribution, the response of other family members, and government policies—there is little, if any, basis for assuming that the evidence for the United States carries over to other economies, especially those that are more dissimilar. Indeed, even in the United States, the distributional effects could be influenced by changes in the policy environment.

Put somewhat differently, an economist asked to predict the effect of a higher minimum wage (or a new minimum wage) in a country for which there is no direct evidence might, for two reasons, confidently predict employment declines. First, there is a fairly strong theoretical prediction that a higher minimum wage will reduce employment of the least-skilled individuals. And second (and more important, in our view), there is evidence of disemployment effects from many countries, as was made clear in chapter 3. But neither of these statements holds for the distributional effects of minimum wages, and hence there is little basis for making a firm prediction about the distributional effects of minimum wages in countries other than the United States. Empirical evidence on distributional effects from more countries is necessary before generalizations can be drawn about the distributional effects of minimum wages or about the factors that might influence these distributional effects. On the other hand, there is virtually no basis—drawing on the existing research—for concluding that minimum wages *do* have beneficial distributional effects in other countries.

One feature that is common to the evidence on the employment, wage, hours, earnings, and distributional effects of minimum wages that we have considered in this and the previous two chapters is the focus on relatively short-run effects. Although studies often allow for lags of up to a year or so, we have not yet explored whether minimum wages deliver some longer-term benefits, or impose some longer-term costs, on affected individuals. In the next chapter, we turn to evidence on longer-run effects of minimum wages, focusing specifically on how minimum wages influence the accumulation of skills. In the following chapter, we broaden the analysis in another direction, shifting from a focus on affected workers and their families to consider evidence of the effects of minimum wages on firms and on the broader macroeconomy.

6 The Effects of Minimum Wages on Skills

6.1 Introduction

Most research on minimum wages, and much of the research we have discussed up to this point, focuses on the employment and wage effects of minimum wages. However, as we have emphasized, this focus provides too narrow a basis for policy evaluation. The previous two chapters discussed this limitation with respect to distributional effects, arguing that from a policy standpoint, the distributional consequences of minimum wages—especially for family incomes—are more important than its effects on employment and wages. But even this broader focus misses another potentially important dimension of the effects of minimum wages. In particular, minimum wages may affect the acquisition of skills and hence earnings for workers who do *not* experience disemployment effects, as well as for those who do experience these effects. And because the effects on skill acquisition are likely to be manifested over the longer term, they likely continue to influence earnings beyond the typical age ranges (sixteen to twenty-four) for which economists have studied the employment effects of minimum wages. The more widespread and longer-run nature of the potential effects of minimum wages on skill acquisition implies that these effects will not be fully reflected in the empirical analyses we have described thus far.[1]

In this chapter, therefore, we consider the effects of minimum wages on skill formation. We first emphasize the effects of minimum wages on on-the-job training and schooling. Lost opportunities for on-the-job training are, of course, a cost of the disemployment effects of minimum wages. But if there are also reductions in training for those who remain employed, then the overall negative effect of minimum wages on skill formation could be larger. On the other hand, as discussed in the next section, a higher minimum wage could lead to greater training for some workers, in which case there may be benefits from a minimum wage in addition to the wage increases received by workers who remain employed. In either case, because skill acquisition affects wage growth (Mincer 1974; Brown 1989; Becker 1993), there may be both contemporaneous and significant longer-term consequences for earnings from the effects of minimum wages on training.

Minimum wages can also affect schooling decisions. A higher minimum could induce some individuals to leave school for work, but it could also induce others to stay in school to increase their human capital in order to raise their productivity to a level that exceeds the higher minimum. In either case, there are again implications for skills (and wages) that extend beyond the usual employment and wage effects of minimum wages.

Reflecting the existing research, this chapter focuses primarily on the contemporaneous effects of minimum wages on training and schooling. In addition, however, we highlight recent evidence on the longer-run effects of minimum wages on wages and earnings that arise through exposure to a high minimum wage as a youth, effects that can arise through the influence of minimum wages on training, schooling, lost work opportunities, or other avenues.

The research on the effects of minimum wages on skill acquisition focuses on workers, asking whether minimum wages increase or decrease training or schooling, and how they affect future wages and earnings. This type of evidence has been important in the policy debate about minimum wages, because it is related to policymakers' stated goal of encouraging economic self-sufficiency.[2]

However, the effects of minimum wages on skill acquisition may also have implications for economic welfare, as there may be existing market distortions that are either exacerbated or mitigated by the effects of minimum wages on skills. One possibility is that private and social returns and costs of acquiring skills differ. For example, a lower-skilled worker is more likely to be eligible for various public support programs, in which case the social benefits of skill acquisition exceed the private benefits, and a higher minimum wage might induce too little schooling as youths leave school for a higher current wage. Conversely, the next section includes a discussion of a theoretical model of labor markets in which market imperfections lead to underinvestment in training that can be corrected by a higher minimum wage. Recent research has also embedded minimum wage effects on skill acquisition in aggregate models to study their welfare implications.[3] As in the other chapters of this book, our discussion—like nearly all of the literature—focuses primarily on the effects of minimum wages that have figured prominently in the policy debate. However, this evidence is also useful as background for more general welfare analyses of the minimum wage because those analyses hinge rather critically on how minimum wage changes affect skill acquisition.

6.2 Minimum Wages and Training

6.2.1 Theory

The potential adverse effects of minimum wages on on-the-job training were originally discussed by Rosen (1972), Feldstein (1973), and Welch (1978). We first consider the standard model of general human capital (Becker 1993), in which training is financed by the worker because it raises skills that are valued by multiple employers, so that an employer who does not pay the worker's opportunity wage after the training has occurred will lose the worker to another employer.

Suppose first that training is financed out of worker's wages, simply reducing the wage to cover the cost of training. Because the FLSA specifies that the minimum wage applies to the wage net of any deducted training costs, a higher minimum wage raises the wage floor below which the wage net of training costs cannot fall, and can therefore deter training. Alternatively, although perhaps less likely, the worker could be paid a wage above the minimum but pay the employer for training. Even in this case, however, the employer still must pay the worker for the time spent in training required for the job,[4] so that the higher minimum wage still raises the cost of training without raising its benefit to the worker. Thus, under either financing arrangement, a higher minimum wage is likely to reduce general training paid for by the worker.[5] If instead training provides human capital that is specific to a particular employer, then the standard model predicts that the employer will bear some of the cost, which mitigates the adverse effect of minimum wages on training.[6]

The discussion to this point focuses on on-the-job training, which can be thought of as training to improve skills on the current job. Broadening the inquiry to include other types of training generates more ambiguous predictions about the effects of minimum wages on training. For example, Leighton and Mincer (1981) point out that even in the neoclassical framework, a higher minimum wage may encourage low-skilled individuals to acquire more skills in order to raise their marginal product above the minimum wage floor.[7] In the context of training, the implication is that training undertaken by workers (and presumably financed by them) in order to qualify for jobs may increase in response to a higher minimum wage. This type of effect is considered more formally by Agell and Lommerud (1997), in the context of investment in schooling.

Some other recent critiques of the standard model are presented in Barron, Berger, and Black 1999 and Loewenstein and Spletzer 1999, based on evidence that employers pay for general training.[8] Acemoglu and Pischke (1999) appeal to this evidence (and related evidence from other studies they discuss) to motivate a model of why employers pay for general training. They begin by considering the competitive model, and note that there are two potential barriers that can prevent workers from financing their own training: liquidity constraints (which can reduce investment below the optimal level because workers prefer to smooth consumption), and contracting difficulties, which arise because workers who take a lower wage to finance general training cannot always discern whether that training is being provided (i.e., in the form of learning by doing) or if the firm is simply using them in regular production while paying a lower wage. Nonetheless, in a competitive labor market, it is still the case that employers will not pay for general training, for the reasons originally elucidated by Becker.

Acemoglu and Pischke then extend their model to show that firms may be induced to pay for general training when labor markets are not perfectly competitive. In particular, they assume that the presence of some monopsony power in labor markets causes two conditions to hold: workers are paid less than their marginal product, and this gap rises with the level of skill.[9] In this case, firms profit from each worker hired, but more so from higher-skilled workers. As a result, a firm has an incentive to invest in its employees' training even if that training is general and raises workers' productivity equally at other employers, because even at the wage paid to workers with those additional skills, the firm earns more from the employment relationship than it does from employing an untrained worker.[10]

In a second paper (Acemoglu and Pischke 2003), the authors show that minimum wages can create an extreme form of this wage compression that may induce employers to invest in general training. In particular, because of the noncompetitive feature of labor markets that creates the gap between the marginal product and the wage, if a minimum wage is imposed (or, equivalently, is increased), there will be some workers whose marginal product is below the minimum wage but whom the firm still finds it profitable to retain.[11] And, for some of these workers, the firm can restore a portion of the lost rents associated with the minimum wage by training the worker and reaping the entire gain in productivity, which induces firms to increase training for a subset of affected workers.

Two additional points that emerge from this analysis are worth noting. First, although the Acemoglu and Pischke model implies that minimum wages increase training for some workers, it also implies that employment falls. Second, although the increase in training in this model is socially optimal, the workers whose training is increased when the minimum wage rises do not earn higher wages because of this training. Indeed, it is precisely because the firm is the residual claimant to the increase in productivity created by the training that the minimum wage can lead to more training. Thus, even if its conditions hold, the model developed by Acemoglu and Pischke cannot be used to argue that a higher minimum wage will help low-wage workers via its impact on training.

6.2.2 Evidence

A few papers from the early 1980s attempted to assess the effects of minimum wages on training. Broadly speaking, this research tends to suggest that minimum wages reduce training, although much of this evidence is fairly weak. In particular, Hashimoto (1982) presents some indirect evidence indicating that time-series increases in the minimum wage are associated with flatter wage profiles in panel data. Leighton and Mincer (1981) present similar indirect evidence, as do Grossberg and Sicilian (1999), somewhat later. However, this evidence is potentially problematic. As pointed out by Lazear and Miller (1981), lower wage growth associated with minimum wage effects need not reflect reductions in training generated by a higher minimum wage. Instead, in a model like that in Lazear 1979, a higher minimum wage can increase the value of the job to a worker, reducing the need for a rising wage profile, which has the same effect. Furthermore, minimum wage increases could affect relative demands for workers of different skills, and hence influence wages paid to workers of different ages or experience levels.[12] Moreover, the reliance on time-series data (in Hashimoto 1982) makes it difficult to rule out other changes in wage profiles occurring over time.

For these reasons, the more compelling evidence from the earlier research comes from studies that use data on the actual incidence of training.[13] Leighton and Mincer (1981) study evidence on direct training measures from the PSID and the National Longitudinal Survey of Young Men for black and white men. All of the evidence points to negative effects of a higher minimum wage, but the evidence is statistically significant only for an on-the-job training measure from the PSID. The authors use some cross-state variation in minimum wages, but that variation is entirely driven by differences in coverage and average state wages, and an appendix table shows that much of the effect they identify comes from the variation in wage levels. This is problematic, because wages may be affected by training. In particular, wages are likely to be higher in states where there is more training, so that the Leighton-Mincer minimum wage variable is likely to be negatively correlated with training for reasons unrelated to the minimum wage. As a result, their evidence that minimum wages reduce training is potentially spurious.

Two subsequent papers focus on the relationship between training and minimum wages based on individual-level variation in wages. Schiller (1994) studies individuals entering the labor market (in the NLSY79) in 1980, classified by whether their wage was above the federal minimum. He reports that the incidence of training is lower among workers earning the minimum wage than among higher-wage workers, conditional on demographic and other controls. Acemoglu and Pischke (2003) rightly criticize this study as probably uninformative about the effects of minimum wages on training, as the identifying variation comes from individual-specific variation in wages, and lower-wage workers may have lower unmeasured returns to training (although the model includes Armed Forces Qualification Test scores). At the same time, Schiller's study makes no claims regarding the effects of minimum wages on training, but instead is meant to simply characterize minimum wage versus non–minimum wage jobs.[14]

Grossberg and Sicilian (1999) revisit the earlier studies, using data from the 1982 Employment Opportunities Pilot Project (EOPP) to study the relationship between the minimum wage, wage growth, and training. The data are cross-sectional, so that the effects are identified from variation in individual-level wages and from variation in state wage levels. In that sense, this approach is subject to the same criticism as was leveled at the Leighton and Mincer study. To address this concern, the authors use detailed controls and restrict their attention to comparisons between workers at the minimum wage, workers below the minimum wage, and workers above the minimum wage by 25 cents or less. They find that both male and female minimum wage workers have lower wage growth (relative to the comparison groups), with the differences larger and statistically significant only for men. With regard to training, the point estimates suggest less training for men in minimum wage jobs, although the differences are insignificant; there is no evidence of lower levels of training for women in minimum wage jobs. On the other hand, they also report that for women, their training measure is not associated with higher wage growth, which strikes us as problematic.

In addition, we have questions about the adequacy of their comparison groups. Workers earning below the minimum wage are likely in different types of firms or jobs than workers earning the minimum wage or higher. And workers earning just above the minimum wage may also have had their training reduced because of a minimum wage. Moreover, despite the inclusion of detailed controls, workers in these comparison groups may also differ in unobserved ways from workers at the minimum wage. In our view, it is preferable to have policy variation in the minimum wage, and to estimate the effects of this variation on training in a manner that does not require comparing workers at different wage levels. Otherwise, it is difficult to distinguish the effects of minimum wages on training from a simple description of the different training experiences of workers at differing wage levels, conditional on a number of control variables.

In Neumark and Wascher 2001b, we tried to improve on the earlier analyses in two ways. First, we used state variation in minimum wages to identify the effects of minimum wages, avoiding the problem just discussed. Second, we controlled for state-level differences in training that may be driven by factors other than minimum wages, but are nonetheless correlated with minimum wages.

The data used in this research come from special supplements to the CPS conducted in January 1983 and January 1991. These supplements included questions about two types of training: training to improve skills on the current job, and training to obtain one's current job; in both cases, respondents were asked about both formal and informal training.[15] These questions are convenient for addressing the theoretical issues outlined previously. The first type of training (to improve skills) corresponds closely to the type of training that might be expected to decline as a result of a higher minimum wage; in addition, we might expect minimum wage effects to be stronger for formal training than for informal training, as the latter is likely less expensive. In contrast, the second type of training (to obtain one's job) is closer to the type that could conceivably increase in response to a higher minimum wage, as individuals attempt to raise their skills to qualify for a job.

We identify the effects of minimum wages on training in two ways. First, we use only the 1991 data, which cover a period shortly after many states had increased their minimum wages above the federal level. For each state, we define the minimum wage variable as the average percentage by which the state minimum wage exceeded the federal minimum wage in the previous three years. We use this three-year "window" to capture the effect of recent high minimum wages, but also verify that the results are qualitatively similar using both shorter and longer windows. We expect the effects of minimum wages to be most apparent for young workers, but we also want to allow for state-specific differences in training that may be attributable to other sources of variation, such as the strength of the community college system, its relationship with business training, and differences in industry structure.[16] We do this by estimating a model for training that includes, for example, both sixteen- to twenty-four-year-olds and thirty-five- to fifty-four-year-olds, and that allows the training of both groups to be related to the minimum wage variable, but that also includes a differential effect for the younger group, as in the following regression model:

$$T_{ij} = \alpha + S_j\beta + \gamma Y_{ij} + \delta MW_j \cdot Y_{ij} + \varepsilon_{ij}, \tag{6.1}$$

where T denotes training, S is a vector of state dummy variables, Y is a dummy variable for the younger group, and MW is the minimum wage variable; the i subscript denotes individuals, and the j subscript denotes states.[17] In this model γ captures the average difference in training between the two age groups, and δ is the difference in the incidence of training between the younger and older age groups associated with variation in the minimum wage. This is a difference-in-differences estimator that identifies the effect of minimum wages from the difference between the relationship between minimum wages and training for an age group believed to be strongly affected by the minimum wage (the young), and an older age group unlikely to be affected. The older age group serves as a control group for the relationship between minimum wages and training attributable to factors other than a causal effect of minimum wages on training.

As an alternative identification strategy, we use only the younger age group, but add the data from 1983 to provide an alternative control group. This more standard difference-in-differences approach uses a regression of the form

$$T_{ijt} = \alpha + S_j\beta + \gamma Z_t + \delta MW_{jt} + \varepsilon_{ijt}, \tag{6.2}$$

where there is now a t subscript indicating year, and Z is a year dummy variable. In this model, δ captures changes in training of the young associated with changes in the minimum wage variable in the same state. There is very little variation in effective state minimum wages in 1983 or the immediately preceding years, so the statistical experiment here can be thought of as changing the minimum wage in some states but not others, and seeing how training of young workers changes.

Some of the key estimates are reported in table 6.1. The first three columns show results for training to improve skills on the current job, for any training, and then for formal and informal training separately. Nearly all of the estimated effects are negative, and they tend to be significant for twenty- to twenty-four-year-olds, both for any training and especially for formal training (for which we find a significant negative effect for both regressions). Thus, this evidence suggests that minimum wages do reduce training to improve skills on the job, as the conventional theory suggests.[18]

There are two potential explanations for our finding that the minimum wage significantly reduces this type of training for twenty- to twenty-four-year-olds, but less so for sixteen- to nineteen-year-olds. First, because the incidence of training for sixteen- to nineteen-year-olds is very low, there may be little scope for reducing training, and what reductions there are may be hard to detect. For example, the raw data for 1991 indicate that only 2.5 percent of teenagers receive formal training, versus 10 percent of twenty- to twenty-four-year-olds. In addition, the data suggest that the training that teenagers receive is less intensive and thus of lower cost. Among teenagers reporting formal training in 1991, 58 percent reported receiving one week of training or less, compared with 42 percent of twenty- to twenty-four-year-olds.

In order to interpret the estimates for training to improve skills on the current job, consider the last estimate in column (1). The estimate (-0.243) implies that a 10 percent higher minimum wage reduces the proportion of twenty- to twenty-four-year-olds receiving training by 0.024, or 2.4 percentage points. Because the raw data indicate that 30.4

Table 6.1
Estimated effects of minimum wages on training

	Training to improve skills on current job			Any training to obtain current job
	Any	Formal	Informal	
	(1)	(2)	(3)	(4)
a. Thirty-five- to fifty-four-year-olds in 1991 as control sample				
Ages sixteen to twenty-four	−0.083	−0.095	−0.029	−0.001
	(0.094)	(0.063)	(0.077)	(0.098)
Ages sixteen to nineteen	−0.059	0.022	−0.043	0.046
	(0.147)	(0.078)	(0.126)	(0.158)
Ages twenty to twenty-four	−0.091	−0.145*	−0.025	−0.026
	(0.110)	(0.073)	(0.088)	(0.114)
b. Workers of same age in 1983 as control sample				
Ages sixteen to twenty-four	−0.211*	−0.120*	−0.048	0.012
	(0.101)	(0.058)	(0.083)	(0.109)
Ages sixteen to nineteen	−0.126	0.053	−0.107	0.136
	(0.168)	(0.075)	(0.148)	(0.187)
Ages twenty to twenty-four	−0.243*	−0.183*	−0.028	−0.016
	(0.124)	(0.075)	(0.100)	(0.133)

Source: Neumark and Wascher 2001b, table 3, columns (1′), (2′), and (3′), and table 5, column (1′).
Note: The data come from the 1983 and 1991 CPS supplements. Standard errors robust to heteroskedasticity are reported in parentheses. All regressions contain controls for age (single-year age dummy variables), race, sex, schooling, and marital status. The training questions are asked of those currently working. The specification in panel a is estimated using the data for 1991, using thirty-five- to fifty-four-year-old workers and workers in the indicated age group. The specification includes state dummy variables, an indicator for belonging to the younger age group, and an interaction of this indicator with the average percentage difference between the state and federal minimum wage over the previous three years. The table reports this interactive effect, which measures how the difference between training reported by younger and older workers varies with the minimum wage. The specification in panel b is estimated using the data for 1983 and 1991, for younger individuals only. It includes state dummy variables, a year dummy variable, and the minimum wage variable; the coefficient of the minimum wage variable is reported. For 1991, there are 6,745 sixteen- to twenty-four-year-olds, of whom 2,057 are aged sixteen to nineteen. * indicates that estimate is statistically significant at the 5 percent level.

percent of twenty- to twenty-four-year-olds report any training of this type, this amounts to a 7.8 percent reduction in the incidence of training, or an elasticity of −0.78, which is sizable. Across a variety of other estimates, the elasticities are as high as −2, admittedly quite large.[19]

What about training to qualify for the job? The estimates in column (4) tend to be negative rather than positive, are mostly near zero, and are never significant. Although not reported in the table, the estimates point to no effect or negative effects for formal, informal, and in-school training to qualify for the job. Thus, there appears to be no evidence that minimum wages lead to increased skill acquisition through this channel.

Based on the evidence, we conclude from this research that minimum wages reduce training aimed at improving skills on the current job, in particular for formal training, and in particular for workers in their early twenties. The estimated elasticities are large, implying substantial negative effects of minimum wages on training for these workers. Although there could, in principle, be offsetting positive effects on training to qualify for jobs, we find no such evidence, and so, on net, minimum wages appear to reduce training. On the other hand, we do not find compelling evidence that minimum wages reduce the training of teenagers (aged sixteen to nineteen), which may reflect, at least in part, the low incidence of training for these workers in the first place and its lower cost when it does occur. This reinforces a more general point that the effects of minimum wages on training will not necessarily be most pronounced for those workers whose wages are most likely to be bound by the minimum wage (unlike the case for employment effects). Rather, the effect will also depend on the cost of training the worker in the absence of the minimum wage, which could be higher for higher-wage workers; more specifically, the relevant factor is whether the minimum wage is higher than the gross wage minus the training cost that would otherwise be incurred for a particular worker. Unfortunately, these quantities are not observed.

Acemoglu and Pischke (2003) also present evidence on the effect of minimum wages on training. Using data from the NLSY79 for the years 1987–1992, they report estimates for a sample restricted to those with twelve years of education or less. Two different specifications are used. The first parallels much of the other work on minimum wages and training by estimating regressions of the incidence of formal training on a state-specific relative minimum wage measure (the ratio of the minimum wage in the state to a measure of the median wage for older workers, sometimes differentiated by urban/rural residence and sex).

The second parallels the specifications used in the minimum wage employment literature that identify workers “caught” by a minimum wage increase. In this specification, the authors regress individual-level changes in the incidence of training on an indicator for whether the worker’s wage was between the old and new minimum wage. One potential shortcoming of this specification is that minimum wages may also increase wages for higher-wage workers (as discussed in chapter 4), which can reduce training of these workers as well. This would make the treatment and control groups more alike, and hence bias the estimates towards zero. Partly because of this, the authors also consider higher ranges—for example, whether a worker’s wage in the prior year was below 130 or 150 percent of the current minimum wage.

The evidence from these regressions yields no evidence of an effect of minimum wages on training. The estimates are sometimes positive and sometimes negative, almost always small, and almost never statistically significant. These results differ substantially from our results using the CPS data, described previously. There are, however, some important differences in the data sets that may explain the differences in the estimates. First, the training questions are different. In the CPS (in 1991), the question asks, “Since you obtained your present job, did you take any training to improve your skills?” In contrast, the NLSY79 asks, “Since [date of last interview] did you attend any training program or any on-the-job training designed to help people find a job, improve skills, or learn a new job?” As discussed already, the predictions of the theory differ for training to improve skills on the job and training to qualify for a new job (perhaps at the higher wages induced by the minimum wage), and we saw that the evidence based on the CPS data suggested that the minimum wage reduces the first type of training but not the second. There is, of course, no way to determine the breakdown of the two types of training in the NLSY79 data, but in the CPS data the incidence of training to obtain the current job is about 50 percent higher. Clearly, then, this could help explain the absence of a negative effect in the NLSY79 data.[20]

A second major difference is that the NLSY79 sample that Acemoglu and Pischke study consists of older individuals than are in our 1991 CPS sample. In particular, NLSY79 respondents were aged fourteen to twenty-one years in 1979, and hence their ages range from twenty-two to thirty-four in the data these authors analyze. To see whether this matters, we have reestimated our specifications using workers in these age ranges in the CPS, with the observations weighted to reflect the representation of each age group that would be expected in the NLSY79 given the age ranges and years covered. For example, observations in the middle of the age range receive more weight than those at either end of the age range because ages in the middle of the range can be observed in all the years of the NLSY79 that the authors use, and those at the ends of the age range cannot. Differences in the age group studied appear to make a large difference. As an example, the estimate in the last row of table 6.1 for twenty- to twenty-four-year-olds' receipt of formal training is −0.183 and significant at the 5 percent level. In contrast, using the same age range for the CPS data as in Acemoglu and Pischke's NLSY97 sample, the estimate is 0.030 and insignificant, similar to what they find with the NLSY97.[21] Moreover, from a policy perspective, we would argue that much of the policy concern has been with whether minimum wages reduce training opportunities for young labor market entrants.

Overall, then, we do not think the Acemoglu and Pischke results contradict ours. Instead, they at best establish that at older ages than those we studied, negative effects of minimum wages on training are less apparent. In addition, the training variable they use mixes training that the minimum wage is predicted to reduce, and training that it may increase. Thus, from a conceptual standpoint, the results are really not comparable, and their evidence does little to refute the standard theory.

Acemoglu and Pischke's model is actually richer than suggested by the empirical analysis described previously, in that it predicts that minimum wages will increase training in cases where the employment relationship creates more rents, whereas minimum wages will reduce training in the absence of rents. Thus, the authors also present evidence from an augmented version of the second specification described previously, which allows different effects of the minimum wage on workers in different industries, differentiated by whether the wage differential in each worker's industry (based on a standard wage regression estimated for eighteen- to sixty-five-year-olds) is above or below the industry wage differential for the median worker. They find some evidence of negative effects of minimum wages on training in the lower-wage industries and positive effects in the higher-wage industries. They interpret these results as consistent with a hybrid model in which lower-wage industries are competitive, and hence conform to the standard prediction, while higher-wage industries are less competitive, so that positive training effects are possible. We regard this evidence as intriguing and as suggesting that—similar to other effects of minimum wages—the effects may be heterogeneous. However, the potential problems with the NLSY79 data still apply.

In still another study, Fairris and Pedace (2004) exploit state minimum wage variation in 1996 to examine the effect of minimum wages on training reported by establishments in the 1997 National Establishment Survey (NES). This survey includes questions about the percentage of workers that receive training, and about the average hours of training.[22] The survey results are reported for all workers combined, as well as for workers in five broad occupational categories: front line, support staff, technical, supervisory, and managerial.

Based on these data, the authors estimate regression models for both the incidence and average intensity of training, using either the simple difference between the state and federal minimum wage or the minimum wage relative to the average wage. In the latter case, concerns about endogeneity between the average wage and training (as mentioned earlier) lead them to instrument for the minimum wage variable using the percentage of the workforce unionized and total sales as instruments. Because the question in the NES refers to training provided by firms, the data may primarily measure training to improve workers' skills on their current jobs, although no direct information on the nature of training is available.

Both specifications reveal significant negative effects for the data on all workers combined. The implied effects are quite large, suggesting that a 50 cent higher state minimum wage (when the federal minimum was $4.25 for part of the year and $4.75 for the rest) reduces the fraction receiving training by 15 percentage points (or 25 percent). In the relative minimum wage specification, the authors also find significant negative effects for support staff and for supervisory workers; the first result makes sense, but the second is harder to explain. In the simple minimum wage specification, they find significant negative effects for front line, technical, and managerial workers. However, they suggest that the results for managers are tainted by omitted variable bias that creates a spurious relationship between training for managers and minimum wages, given that an actual effect of minimum wages on training of managers seems implausible.

Fairris and Pedace also focus on the differences between the estimated effects across occupations, arguing that the managerial group can be used as a baseline control, because minimum wages should not affect their training. In particular, they suggest that the true minimum wage effect for each occupation can be calculated as the difference in the estimates for other occupations relative to that for managers because this calculation subtracts out the omitted variable bias. We agree that the estimated training effects for managers are suspect, but we do not see why this occupation should be viewed as a reasonable baseline control group absent an explanation of the large estimate for this category and a cogent argument as to why the omitted variable bias is constant across occupations. Indeed, it seems to us just as likely that any spurious correlation between state minimum wages and training of managers is idiosyncratic to that occupation, in which case subtracting the estimated training effect for managers from the training effect for other occupations may well provide a worse estimate of the effects of minimum wages on training in other occupations.

Overall, we view these results as providing some evidence consistent with negative effects of training. However, the evidence is not consistent with expectations across occupations and should therefore be viewed with some suspicion. One potentially serious problem is that nothing in this empirical analysis attempts to identify the training effects for workers whose wages were affected by minimum wages, other than the across-occupation analysis, which delivers suspect results. Although this cannot explain the finding of significant effects, it raises the likelihood—as is suggested by the results for managers—that the estimates are picking up correlations between minimum wages and training that have little to do with the causal effects of minimum wages on training.

There is only limited evidence from other countries on the effects of minimum wages on training. Arulampalam, Booth, and Bryan (2004a) study the effect on training of the imposition of the new minimum wage in the United Kingdom in 1999, using data from the BHPS. They estimate regressions for the change in both incidence and intensity of training to improve skills on the current job, with two alternative treatment and control groups. One approach closely parallels Acemoglu and Pischke, using as a treatment group workers whose wages in 1998 were below the minimum wage subsequently imposed, and using as a control group those workers whose 1998 wages were between the minimum and 115 percent of the minimum. Again, a potential problem is that the minimum wage could push up wages of the latter group, and if that group receives more training (which, as indicated in table 1 of the paper, is clearly the case, especially for intensity), then their training could potentially fall by more. The second approach uses as the treatment group those who said in 1999 that their wage was increased to bring them up to the new minimum wage; the control group consisted of those who did not so indicate. This approach suffers from a related problem: the new minimum wage could have boosted a worker's wage from below to above the minimum wage, or it could have led to a wage increase for a worker whose wage was already above the new minimum. In either case, the worker would end up in the control group even though the minimum wage would potentially have reduced the amount of training received.[23]

The estimates in the paper indicate that training rose more in the treatment than in the control group, suggesting that the minimum wage increased training. But given the potential problems with the treatment and control groups, we do not find this evidence convincing. And, as in the case of research on the employment effects of the new minimum wage in the United Kingdom, trends in training may have been different for workers at different wage levels—a problem that is mitigated when there is regional variation in minimum wages.[24]

Finally, Baker (2003) reports evidence on the effects of training in Canada, using cross-province variation in the minimum wage and data on seventeen- to twenty-four-year-olds from three years of the Adult Education and Training Surveys (AETS). He focuses on training that, according to the respondent, was supported by the employer (via provision, paying for it, time off, and so on) and excludes training in pursuit of educational degrees. Baker uses a conventional pooled time-series cross-section logit specification for training receipt, with year and province dummy variables and the minimum wage relative to an average wage measure included in the model (the results are insensitive to using other minimum wage variables). A number of results point to negative effects of the minimum wage on training, although the implied elasticities are sometimes very large (in the -6 to -8 range—much larger than in our 2001b paper). Furthermore, the pattern of differences in the estimates is indicative of real effects; for example, the evidence of negative effects is strongest for training "suggested" by the employer, which presumably isolates training related to one's work. On the other hand, the results are not robust to which years are included in the sample. Finally, to account for a potential spurious association between minimum wages and training, Baker uses a specification that, as in our cross-sectional specification in equation (6.1), introduces older workers aged thirty-five to forty-four as a control group, identifying the effect of the minimum wage from the difference in the estimated minimum wage coefficient for the younger group. This interaction, however, is imprecisely estimated and of varying sign across samples and specifications; in addition, the implied effect for older workers is large and strongly negative, which Baker regards as implausible (correctly, it seems to us).[25] Overall, Baker concludes that he cannot establish any reliable sense of the effect of the minimum wage on training in Canada using these data.

Summing up all of the evidence on training, we can only conclude that the evidence is mixed. Our own research tends to find negative effects of minimum wages on training, but most of the other recent research finds little evidence of an effect in either direction. Whether this is because there is in fact no effect, because the effects are quite heterogeneous for the reasons discussed in Acemoglu and Pischke's work, or because the effects are hard to detect given difficulties in measuring training, is hard to say at this point, and remains an important question for future research. In the next two sections, we turn to other evidence on the effects of minimum wages on skill acquisition. Our reading is that this other evidence points more strongly to adverse effects of minimum wages on the acquisition of skills.

6.3 Minimum Wages and Schooling

6.3.1 Theory

Theoretical models of how minimum wages might affect schooling decisions have quite a few layers of complexity and provide no clear predictions. Starting from research on this subject by Cunningham (1981) and Ehrenberg and Marcus (1980), it is useful to first consider a simple model in which an individual faces only two options—full-time schooling or full-time labor market participation. In addition, we ignore changes in labor demand for workers of different types of skill (i.e., different amounts of schooling) and assume that work at the minimum wage is the only option for those who leave school early. In this simple model, the effect of an increase in the minimum wage on the schooling decisions of young individuals is determined by its effect on expected earnings. In particular, if expected earnings decline because the impact of fewer jobs at the minimum wage outweighs the impact of a higher wage, then we would expect more individuals to remain in school, for two reasons. First, as discussed in the last section in the context of training, some individuals may choose to remain in school to become better qualified for jobs at the now-higher minimum wage.

Second, the opportunity cost of schooling, which might result in an above-minimum wage job, is lower. On the other hand, if the higher minimum wage raises expected earnings because the disemployment effects are small, then the opportunity cost of schooling rises and enrollments may fall.[26]

Of course, the minimum wage can also affect the wage structure, which can have varying influences on schooling decisions. If—as the neoclassical model predicts and as the employment evidence suggests is the case—a higher minimum wage results in a relative increase in the demand for more-skilled labor, then the price of more-educated labor should rise. In and of itself, this shift in demand will increase the return to schooling, which might be expected to encourage some youths to stay in school. But the higher minimum wage will also raise the wages of employed youths who have left school, so that the net effect of a minimum wage increase could be to lower the return to an extra year of schooling, at the relevant margin. (Again, this will depend on the probability of finding a job at the minimum wage.)

Finally, as both the Cunningham and Ehrenberg paper and the Marcus paper emphasize, modeling the schooling decision is complicated by the fact that youths can be in school while working part-time.[27] Cunningham suggests that a higher minimum wage will reduce part-time employment more than full-time employment. For example, the higher fixed costs associated with part-time workers may lead to an increase in the relative demand for full-time workers in response to a higher minimum wage, and the expected wage (wage times the probability of employment) in the part-time sector may fall, which can induce some people to leave part-time work coupled with school for full-time work. Based on this reasoning, Cunningham suggests that once we take account of part-time workers, it becomes more likely that students will tend to leave school in response to a higher minimum wage.

However, as Ehrenberg and Marcus (1982) point out, this implication is not so clear, because the effects are likely to be heterogeneous across individuals with different income levels. In particular, they point out that youths in low-income families often need to work part-time to finance their education. That is, there is a minimum earnings constraint to stay in school, and if a higher minimum wage reduces job opportunities in the part-time sector, then youths from low-income families may be induced to leave school. The effect on youths from higher-income families is ambiguous. More of them could choose to stay in school if employment opportunities decline, depending on some of the issues discussed previously. In light of all of these considerations, then, there is no firm prediction regarding the effects of minimum wages on schooling.

6.3.2 Evidence

The evidence from the papers just discussed, along with that reported in earlier work by Matilla (1978), is mixed. For example, in time-series data extending through the 1970s, Matilla (1978, 1982) tended to find positive effects of the minimum wage on school enrollment—in particular, for eighteen- to twenty-one-year-olds. In contrast, Ehrenberg and Marcus looked at cross-sectional data on white male and female teenagers from 1970 and found little, if any, effect of the minimum wage on school enrollment. However, they also analyzed data for the late 1960s from the National Longitudinal Survey of Young Men and found that for white male teenagers, minimum wages reduced enrollments for low-income teenagers but increased enrollments for high-income teenagers; the results were consistent with their argument that teenagers in low-income families shifted from being in school and employed part-time to full-time employment, while teenagers in high-income families shifted towards full-time schooling. For nonwhite male teenagers, the evidence also points to enrollment declines for those from low-income families, but in this case, the drop in enrollment occurs because individuals who were in school and not employed left school for full-time employment, which is inconsistent with their conjecture.[28] Finally, Cunningham (1981), using data from the 1960 and 1970 Census, reports a negative enrollment effect for male and female white teenagers, but the opposite result for black female teenagers and black male teenagers and young adults.

There was little subsequent work on this topic until the 1990s. One of the first new studies was Card's paper in the 1992 *ILRR* symposium (1992b). In this paper, he reports a significant negative difference-in-differences estimate of the California minimum wage increase in 1988 on the teenage enrollment rate, both with and without demographic controls. This result is replicated for different measures of enrollment using both CPS data and administrative data. Card also reports that the decline in enrollment was not directly associated with the higher employment growth that he finds in California (as discussed in chapter 3), as the increase in employment rates that he finds is similar for enrolled students and the overall teen population.

Our own interest in the effects of minimum wages on school enrollment originally stemmed from an effort to better understand the employment effects of the minimum wage. In particular, our findings (Neumark and Wascher 1992, 1994) indicated that the estimated effects of minimum wages on employment of teenagers were stronger in regressions that conditioned on enrollment.[29] In Neumark and Wascher 1995a, we estimated a model of minimum wage effects on employment as well as enrollment. In particular, we modeled the effects of the minimum wage and other controls (X) on four possible states of employment/enrollment status (j), as in

$$U_{ijst} = \alpha_j MW_{st} + X_{ist}\beta_j + S_s\lambda_j + Y_t\theta_j + \varepsilon_{ijst}, \quad j = 1, \ldots, 4. \tag{6.3}$$

In this equation, which is based on a specification originally proposed by Wachter and Kim (1982), i indexes individuals, s the state, and t the year. The assumption that ε_{ijst} follows an extreme value distribution gives rise to a conditional logit model for the four mutually exclusive categories of activities that we use: enrolled and employed, enrolled and not employed, not enrolled and employed, and not enrolled and not employed (or idle). This amounts to estimating models for the log of the odds ratio for three of the categories relative to the fourth, from which one can compute the implied effect of the minimum wage on the probability of being in any of the four categories. The minimum wage variable is the coverage-adjusted relative minimum wage used in much of our work on employment, and we enter it both contemporaneously and lagged. The model also includes controls for the prime-age male unemployment rate, the population share of teenagers, dummy variables for the compulsory schooling age, average teacher salaries by state and year, and year and state dummy variables (S and Y, respectively). The year effects control for changes over time, common to all states, in the share of teens in each of the employment and enrollment categories, and the state effects control for persistent differences across states in these shares. Thus, the specification largely parallels those we have used to study employment effects, with the addition of control variables that might affect schooling.

As reported in panel a of table 6.2, estimating the model using state-year data from 1977 to 1989, we find that a higher minimum wage led to little change in the proportion enrolled but not employed, a significant negative effect on the proportion enrolled and employed (with an elasticity of −0.47), a weak positive effect on the proportion not enrolled but employed (with an elasticity of 0.14), and a significant positive effect on the proportion idle (with an elasticity of 0.64).[30] These estimates indicate that a higher minimum wage reduces both the proportion employed and the proportion enrolled; the latter effect is reported in column (5). The net employment effect is rather weak, as there are larger and partly offsetting employment effects for different groups based on enrollment status.

In updated estimates for a later sample period and using a better enrollment measure (reported in panel b of table 6.2), the effects are more muted, but there is still evidence that a higher minimum wage increases the proportion employed but nonenrolled, and the proportion idle (Neumark and Wascher 2003). This updated study also reports robust evidence of negative effects of minimum wages on teenage enrollments, with elasticities in the range of -0.06 to -0.33, depending on the exact data used, the measure of enrollment, and the estimator. As confirmatory evidence, we found that this negative effect was present for observations in states with compulsory schooling ages less than eighteen (where teenagers have more choice about leaving school), but not in states with a compulsory schooling age of eighteen (for which the estimates were smaller and insignificant, although still negative). Similar results are reported in Chaplin, Turner, and Pape 2003, based on data on the entire population of public schools in the United States.

The evidence indicates that the effects of minimum wages on teenagers are more important and more complicated than is suggested by the employment effects or the enrollment effects alone. In terms of the underlying economics, the results are consistent with a higher minimum wage causing employers to substitute away from lower-skilled teenagers (who are less likely to be in school) and toward higher-skilled teenagers (who are more likely to be in school), with the resulting increase in the relative wages of higher-skilled teenagers inducing some of them to leave school for employment. However, more direct evidence on this hypothesis requires information on the flows of teenagers across enrollment/employment states. Thus, in subsequent work (Neumark and Wascher 1995b, 1996b), we turned to individual panel-level data from matched CPS surveys, using data from 1979 to 1992. We applied the same basic modeling approach as in the state-level analysis and estimated multinomial logit models on the individual data. In addition, we augmented the model by adding indicators for each individual's school/work activity in the previous year, which together with the parameter estimates enables us to calculate the implied effect of the minimum wage on the probability of being in each of the

Table 6.2
Estimated effects of minimum wages on enrollment and employment

	(1)	(2)	(3)	(4)	(5)
a. Teenagers, 1977–1989	Enrolled, not employed	Enrolled, employed	Not enrolled, employed	Not enrolled, not employed	Enrolled
Proportion	0.45	0.21	0.23	0.12	0.66
Minimum wage elasticity	−0.02	−0.47*	0.14	0.64**	−0.26**
	(0.18)	(0.29)	(0.26)	(0.15)	(0.12)
b. Teenagers, 1980–1998					
Minimum wage elasticity	−0.11	−0.09	0.41**	0.18*	−0.21**
	(0.12)	(0.18)	(0.20)	(0.10)	(0.06)
c. Teenagers, 1977–1992					
Minimum wage effect on probability	−0.11	−0.39**	0.26	0.23**	−0.30[a]
	(0.21)	(0.12)	(0.20)	(0.06)	
Elasticity	−0.10	−0.70**	0.57	1.16**	
d. Teenagers, 1977–1992, minimum wage effects on probability of transition	Enrolled, not employed, year 1	Enrolled, employed, year 1	Not enrolled, employed, year 1	Not enrolled, not employed, year 1	
Enrolled, not employed, year 2	−0.09	0.05	−0.12	−0.28**	
	(0.20)	(0.18)	(0.13)	(0.14)	
Enrolled, employed, year 2	−0.29**	−0.56**	−0.33**	−0.19**	
	(0.10)	(0.18)	(0.10)	(0.05)	
Not enrolled, employed, year 2	0.17	0.39[+]	0.25	−0.02	
	(0.14)	(0.20)	(0.23)	(0.20)	
Not enrolled, not employed, year 2	0.21**	0.13**	0.19**	0.49**	
	(0.06)	(0.03)	(0.08)	(0.18)	

e. Twenty-five- to twenty-nine-year-olds	Percentage with high school degree	Years of schooling
Effect of log average state minimum wage exposed to, ages sixteen to nineteen	−7.03** (2.22)	−0.81** (0.21)
Effect of representative higher state minimum	−0.42	−0.05
Effect of log average state minimum wage exposed to, ages twenty to twenty-four	−7.48** (2.46)	−1.19** (0.24)
Effect of representative higher state minimum	−0.45	−0.07

Source: Panel a: Neumark and Wascher 2003, table 1, and table 4, row 1; panel b: Neumark and Wascher 2003, table 3, row 11; panels c and d: Neumark and Wascher 1996, table 3, column 6, and table 6, panel A; and panel e: Neumark and Nizalova 2007, table 6.
Note: The data come from various CPS surveys. Standard errors are reported in parentheses. Estimation and sample details are reported in the referenced papers. +, *, and ** indicate that estimate is statistically significant at the 10, 5, or 1 percent level.
[a] Standard error not available.

four distinct enrollment/employment states in the current year, conditional on an individual's enrollment/employment status in the previous year; in other words, we obtain estimates of the effects of the minimum wage on transitions from any enrollment/employment status in the previous year to any enrollment/employment status in the current year.

Consistent with the previous hypothesis, the results suggest that the employment effects of the minimum wage fall largely on the least-skilled teenagers (those who are out of school); selected results are shown in panels c and d of table 6.2. As reported in panel c, the individual-level data confirm that a higher minimum wage increases the probability that a teenager leaves school, presumably to look for a job or to work, and increases the probability that teenagers become idle. The implied overall enrollment elasticity is quite large (−0.30), and the implied overall employment elasticity (not reported in the table) is smaller (−0.13).

The estimates that take account of enrollment/employment status in the previous year, and hence allow us to compute the effects of minimum wages on transitions across the years, are shown in panel d. These estimates help us understand the results just described. First, for those originally in school and employed (the second column), a higher minimum wage reduces the probability of remaining in this state, and increases the probability of leaving school either for employment or for nonemployment (the latter could reflect leaving school to queue for minimum wage jobs). Second, for those originally employed but not in school (the third column), there is a decreased probability of returning to school, and an increased probability of becoming idle. And finally, among those originally idle (the fourth column), there is an increased probability of remaining idle, and a reduced probability of returning to school.

These results are consistent with displacement of lower-skilled workers (those originally not enrolled but employed) by higher-skilled workers (those originally in school and employed, presumably part-time). We would expect this situation to occur if a higher minimum wage increases the demand for more-skilled teenagers, inducing some of them to leave school and take up full-time employment,[31] and reducing the demand for less-skilled teenagers. As it turns out, there is additional evidence consistent with this type of response. First, when we disaggregate by age, looking separately at sixteen- to seventeen-year-olds and eighteen- to nineteen-year-olds, among the older teens—

who we assume are more skilled—the largest response is the increased probability of moving from enrolled and employed to nonenrolled and employed. On the other hand, among sixteen- to seventeen-year-olds we find larger effects on the probability of moving from nonenrolled and employed to idle, and on the probability of remaining idle. Second, we find similar results when we compare nonblack, non-Hispanic teens to black or Hispanic teens, with the nonminority teens exhibiting a larger response of moving from in school and employed to nonenrolled and employed, while for minorities the largest effect is on the transition from nonenrolled and employed to idle.[32]

Turner and Demiralp (2001) use an approach similar to that of Neumark and Wascher (1995b) with data from the 1991 and 1992 waves of the Survey of Income and Program Participation (SIPP).[33] In particular, they examine employment/enrollment transitions between January–April 1991 and January–April 1992, thus capturing the effects of both the April 1991 increase in the federal minimum wage and some state minimum wage increases.[34]

Their results tend to confirm some of our enrollment results, although they find that the adverse effects are concentrated among inner-city minorities, for whom idleness increases. However, we do not have great confidence in these results. The authors use a very limited sample period, with the consequence that they have relatively little minimum wage variation and very small sample sizes, especially for their disaggregated analyses.[35] In addition, their models exclude fixed state effects (including only dummy variables for census divisions), and therefore their estimates may confound minimum wage effects with state differences in transition rates. Finally, it is not clear why the authors did not use additional years of SIPP data.

Finally, evidence reported in Neumark and Nizalova 2007, which is described in more detail in the next section, also points to negative effects of minimum wages on schooling, as shown in panel e of table 6.2. The nature of this evidence is somewhat different, as it refers to schooling of twenty-five- to twenty-nine-year-olds (and hence, more likely, completed schooling), and relates their level of schooling to the level of the minimum wage they were exposed to at ages sixteen to nineteen and twenty to twenty-four, when they presumably made their decisions about schooling. The panel shows the regression estimates, as well as the implied effect of the minimum wage applicable, on average, to individuals in states with minimum wages above the federal level (termed the "representative higher minimum wage"); the latter estimates imply that in states with higher average minimum wages (conditioning on fixed state effects), years of schooling are lower by about 0.12 years, and the percent with a high school degree is lower by about 0.86 percentage point.

In sum, our evidence points to negative effects of minimum wages on enrollment over a number of different periods, and estimated a number of different ways. In addition, our findings emphasize that the estimates of relatively modest teenage employment effects likely understate the size of the gross disemployment effects on the lowest-skilled workers, because they measure net employment changes among a broader group of teenagers. Put another way, the net disemployment effects mask a significant amount of labor/labor substitution, with some teenagers leaving school and displacing others in the labor market.

The international evidence, while scant, is less indicative of negative effects of minimum wages on the schooling of young individuals than is the evidence for the United States. For example, Campolieti, Fang, and Gunderson (2005b) examined longitudinal data for Canada from 1993 to 1999 using an approach similar to that in our 1995b paper, and found little evidence of an effect of the minimum wage on school enrollment.[36] In a pooled cross-section time-series analysis of Canadian provinces covering a longer period (1983–2000), Baker (2003) finds similarly weak evidence. He does find some evidence of negative enrollment effects for fifteen- to sixteen-year-olds and seventeen- to nineteen-year-olds (the young group is largely required to be in school owing to compulsory schooling ages), but these findings are not consistent across different choices about which years of data to use and in which month to measure enrollment. Similarly, he reports some evidence that points to positive effects on twenty- to twenty-four-year-olds, but this evidence is again not robust. In addition, Baker's findings appear to overturn earlier results reported by Landon (1997), who used data for a subset of provinces over an earlier period (1975–1989) and found rather strong evidence of negative enrollment effects for sixteen- to seventeen-year-olds (with the exception of sixteen-year-old females).[37] The reasons for the differences are not clear, but from a policy perspective, the results from the more recent data are likely of greater interest.

Hyslop and Stillman (2007) use the same analysis described in chapter 3 to estimate the effects of increases in youth subminimum wages in New Zealand on school enrollment rates and rates of idleness. Some of their estimates suggest that the minimum wage increases reduced schooling and increased inactivity. But these effects are not present in the models that include business cycle controls, with the exception of evidence of some shorter-run adverse effects on schooling among sixteen- to seventeen-year-olds. Pacheco and Cruickshank (2007) conduct a similar analysis of sixteen- to twenty-four-year-olds and sixteen- to nineteen-year-olds using a longer sample period, and more important, using an enrollment measure that is independent of employment status—whereas the measure used by Hyslop and Stillman cannot capture any school enrollment of those who work more than two hours per week and are out of secondary school. Pacheco and Cruickshank find that the introduction of the teen minimum wage in 1994 appears to have increased enrollment of sixteen- to nineteen-year-olds, but that subsequent increases in the teen minimum over their sample period reduced enrollment. They suggest that this difference may indicate that very low-skilled teenagers were induced to return to school as the introduction of the minimum wage reduced the number of job opportunities available to them, while the subsequent increases tended to lure higher-skilled teens out of school and into the labor market. Taken literally, the estimates imply that once introduced, increases in the minimum wage reduced enrollment, although identification of minimum wage effects is not strong in these data because of the lack of regional variation.

Finally, Rice considers the effect of the extension of the national minimum wage in the United Kingdom to sixteen- to seventeen-year-olds in 2004, based on a model relating wages to education and employment choices. Her results suggest that higher wages reduce participation in full-time education, and increase idleness. However, the variation in this study does not come from minimum wages, but from variation in market wages across regions, presumably reflecting local labor market conditions (2006, 19). It is unclear, then, how these results would carry over to variation in wages generated by a higher minimum wage.[38]

Overall, we read the evidence for the United States as indicating that higher minimum wages lead to lower school enrollment rates and lower completed schooling, although the limited evidence for other countries is clearly weaker. In any event, the evidence that minimum wages reduce skill acquisition along this dimension is considerably stronger than it is with respect to training.

6.4 The Effects of Youth Exposure to Minimum Wages on Adult Labor Market Outcomes

To this point, we have considered explicit evidence on the effects of minimum wages on training and schooling. There is some evidence—stronger for schooling than for training—that minimum wages reduce the acquisition of skills. These effects, coupled with the reduced accumulation of labor market experience stemming from disemployment effects of minimum wages for teens and young adults, suggest that minimum wages may have longer-run adverse effects on labor market outcomes. Such longer-run impacts could be exacerbated by the scarring effects of early nonemployment (Ellwood 1982), which might deter the formation of good work habits, a reputation as a good worker, and labor market networks.[39]

Of course, the evidence on training and schooling effects considered in the previous two sections of this chapter indirectly addresses some of the longer-run effects of minimum wages, in that changes in training or schooling should influence a worker's longer-run wage profile. However, that evidence does not focus explicitly on whether these longer-run effects are present. Thus, in this section, we summarize some recent research on the longer-run effects of minimum wages on wages and earnings.

In particular, recent research by Neumark and Nizalova (2007) attempts to estimate the effects of exposure to higher minimum wages at younger ages—when minimum wages were most binding—on outcomes for somewhat older individuals (twenty-five- to twenty-nine-year-olds). To do this, the authors compute the log of the average effective minimum wage to which an individual in a state-year-age cell was exposed in each of three age ranges—ages sixteen to nineteen, twenty to twenty-four, and twenty-five to twenty-nine—and then estimate, for twenty-five- to twenty-nine-year-olds, the following specification:

$$\begin{aligned} Z_{ijt} = \alpha &+ \gamma_1 MWEXP^{1619}_{ijt} + \gamma_2 MWEXP^{2024}_{ijt} + \gamma_3 MWEXP^{2529}_{ijt} \\ &+ S_i\theta_S + Y_t\theta_Y + A_j\theta_A + \varepsilon_{ijt}, \end{aligned} \tag{6.4}$$

where i indexes states, j indexes single-year age groups, and t indexes years.[40] The dependent variable Z is alternatively: the log of average wages of workers in the state-year-age cell; the percentage employed in the cell; average hours worked in the cell; and the log of average weekly earnings in the cell. S, Y, and A are vectors of state, year, and single-year age dummy variables, respectively. The key variables are $MWEXP^{1619}$, $MWEXP^{2024}$, and $MWEXP^{2529}$, which measure exposure to the minimum wage in the indicated age range, hence capturing the minimum wage "history" to which individuals were exposed.[41] The inclusion of year effects removes the influence of common movements in the exposure variables generated by variation in the federal minimum (but also perhaps attributable to other aggregate changes, such as the business cycle). The inclusion of state effects implies that the effects of exposure to a higher minimum are identified from differences in this exposure across cohorts within the same state; for example, a state that pursued high minimum wages and other bad economic policies leading to lower employment over a long period would not generate a spurious effect of exposure to high minimum wages.

The motivation for the regression model, equation (6.4), is straightforward. Any consequences of minimum wages—reducing employment directly, lowering training, and so on—are likely to be more severe at young ages when the minimum wage is more binding. Equation (6.4) tests whether exposure to higher minimum wages when individuals were young generates longer-run effects. Note that the equation does not include controls for productivity-related characteristics that are potentially endogenous, such as schooling, to avoid controlling for variation in characteristics that may be influenced by minimum wages; instead, the γ's capture both direct effects on the dependent variables, as well as indirect effects via the accumulation of skills.

Estimates of the model for the full sample are reported in panel a of table 6.3. For wages, the effects of exposure at ages sixteen to nineteen and twenty to twenty-four are negative and statistically significant. To interpret these magnitudes, it is necessary to multiply them by 0.06, which is the approximate difference (in log points) in the average effective log minimum wage between states with and without contemporaneous minimum wages that exceed the federal minimum; this figure thus provides an approximation of the difference in minimum wage "histories" between representative high minimum wage and low minimum wage states. For example, the estimates for wages imply that exposure to the average higher minimum wage at ages sixteen to nineteen reduces adult wages by 1.3 percent (0.06 times 0.215). The results for employment and hours also point to adverse longer-run effects of exposure to a high minimum wage when younger. And, finally, as

Table 6.3
Estimated effects of average effective log state minimum wage by age of exposure, twenty-five- to twenty-nine-year-olds

	Log (wage)	Percent employed	Hours	Log (weekly earnings)
	(1)	(2)	(3)	(4)
a. All observations				
Log average effective state minimum wage, ages sixteen to nineteen	−0.215** (0.049)	−5.748* (2.709)	−2.799* (1.306)	−0.302** (0.068)
Log average effective state minimum wage, ages twenty to twenty-four	−0.189** (0.046)	−11.052** (2.662)	−6.630** (1.343)	−0.381** (0.073)
Log average effective state minimum wage, ages twenty-five to twenty-nine	0.035 (0.045)	−1.649 (2.238)	−1.042 (1.149)	0.001 (0.069)
R^2	0.77	0.48	0.53	0.71
b. Panel a specification, whites				
Log average effective state minimum wage, ages sixteen to nineteen	−0.218** (0.051)	−0.387 (2.733)	−0.321 (1.356)	−0.238** (0.070)
Log average effective state minimum wage, ages twenty to twenty-four	−0.187** (0.049)	−7.070* (2.754)	−4.867** (1.421)	−0.327** (0.076)
Log average effective state minimum wage, ages twenty-five to twenty-nine	−0.001 (0.052)	−0.972 (2.316)	−0.526 (1.177)	−0.019 (0.075)
c. Panel a specification, blacks				
Log average effective state minimum wage, ages sixteen to nineteen	−0.535** (0.123)	−32.174** (8.994)	−16.473** (3.762)	−1.083** (0.204)
Log average effective state minimum wage, ages twenty to twenty-four	−0.486** (0.111)	−26.847** (7.790)	−15.176** (3.365)	−0.981** (0.179)
Log average effective state minimum wage, ages twenty-five to twenty-nine	0.005 (0.098)	−4.116 (7.133)	−1.509 (2.971)	−0.044 (0.154)
d. Including unemployment rate exposure				
Log average effective state minimum wage, ages sixteen to nineteen	−0.083+ (0.046)	−1.043 (2.821)	−0.422 (1.312)	−0.101* (0.062)
Log average effective state minimum wage, ages twenty to twenty-four	−0.147** (0.046)	−7.670** (2.759)	−4.838** (1.353)	−0.291** (0.072)
Log average effective state minimum wage, ages twenty-five to twenty-nine	−0.034 (0.043)	−4.283+ (2.273)	−2.379* (1.128)	−0.107 (0.066)
Average unemployment rate, ages sixteen to nineteen	−0.006** (0.001)	0.090 (0.073)	0.059+ (0.035)	−0.005** (0.002)
Average unemployment rate, ages twenty to twenty-four	−0.016** (0.002)	−0.441** (0.076)	−0.217** (0.035)	−0.023** (0.002)

Source: Neumark and Nizalova 2007.
Note: The data come from CPS ORG files from 1979 to 2001. Standard errors, clustered by state, are reported in parentheses. This allows for non-independence of observations over time and across single-year age groups (which could stem from serial correlation, overlapping samples, and common shocks). All regressions contain controls for age (single-year age dummy variables), year, and state. State-age-year observations are weighted by the number of observations in the cell, multiplied by the average CPS earnings weight of individuals in the state-year-age cell to correct for oversampling of individuals in small states. The minimum wage exposure variables are estimated up to the current age; for example, for a seventeen-year-old, the minimum wage faced at ages sixteen and seventeen is used. There are 4,590 observations for the full sample in panel a. +, *, and ** indicate that estimate is statistically significant at the 10, 5, or 1 percent level.

shown in column (4), the estimated longer-run effects on earnings for exposure both as a teenager and a young adult are negative and statistically significant for twenty-five- to twenty-nine-year-olds. In this case, exposure to the average higher minimum wage as a teenager is estimated to reduce adult earnings by 1.8 percent, while similar exposure as a twenty- to twenty-four-year-old reduces earnings by an estimated 2.3 percent.

The general pattern in these estimates is that exposure to higher minimum wages at younger ages has adverse longer-run effects on labor market outcomes. Moreover, estimates of *contemporaneous* employment effects for sixteen- to nineteen-year-olds and twenty- to twenty-four-year-olds reveal significant negative employment and hours effects only for teens (and not for young adults).[42] This suggests that the longer-run adverse effects of exposure to minimum wages as a young adult reported in table 6.3 may be more attributable to the lasting impact of effects of minimum wages on training and schooling than to a residual effect from earlier employment or hours reductions.

Panels b and c of table 6.3 report results for whites and blacks separately. The estimates quite clearly indicate that the longer-run effects of minimum wages are more adverse for blacks. In particular, the estimates for whites are often about one-quarter to one-third as large as for blacks, although the estimates are generally statistically significant for both racial groups. The effects on blacks are likely stronger because their wage levels are lower, so that the minimum wage was more binding when they were teenagers.[43]

The results by race are inherently interesting, given worse labor market outcomes for blacks. In addition, though, by identifying two groups that should be differentially affected by longer-run exposure to high minimum wages, and finding evidence of stronger effects on the group for whom this would be expected (i.e., blacks), the race results bolster a causal interpretation of the evidence on the longer-run effects of minimum wages. Essentially, the race differences provide a third level of differencing, relative to the difference-in-differences identification strategy that relies solely on the variation in exposure to minimum wages.

One potential problem with the estimates reported thus far is that the history of economic conditions to which one was exposed as a youth may also affect subsequent labor market outcomes, and this history may be correlated with the minimum wage history, biasing the estimates. Neumark and Nizalova address this issue by adding controls for exposure to unemployment rates, constructed in the same way as the minimum wage history.[44] As reported in panel d of table 6.3, the history of unemployment rates to which individuals were exposed does impact contemporaneous outcomes; in six out of eight cases, higher past unemployment rates have negative and significant estimated effects on the dependent variables. The estimated effects of exposure to a higher minimum wage fall somewhat as a result of the inclusion of the unemployment history, but the qualitative results remain the same, especially with regard to exposure at ages twenty to twenty-four.[45]

The stronger longer-run effects from exposure to a high minimum wage at ages twenty to twenty-four may make sense. In our study of the effects of minimum wages on training discussed earlier (Neumark and Wascher 2001b), we found stronger adverse effects of minimum wages on training of twenty- to twenty-four-year-olds than on teenagers. Furthermore, evidence from the Neumark and Nizalova study reported in the last panel of table 6.2 suggests that facing a higher minimum wage in the older age range reduces completed schooling. These effects on training and schooling of those in their early twenties are not surprising, as these are ages at which jobs are more likely to entail training (Neumark and Wascher 2001b) and at which many individuals are still on the margin between staying in or leaving school.

In sum, this evidence points to longer-run negative effects on earnings from exposure to a higher minimum wage at earlier ages. Furthermore, if one applies the estimated training effects from Neumark and Wascher 2001b and the estimated effects on schooling and on foregone labor market experience reported in Neumark and Nizalova, along with estimated returns to these types of human capital investment, then these effects can account for a sizable share of the shortfall in earnings experienced by adults aged twenty-five to twenty-nine because of their exposure to a higher minimum wage when young—about 46 percent of the shortfall.[46] (Contemporaneous labor supply effects stemming from lower wages, as well as other scarring effects of early nonemployment, may account for the rest.) These calculations are only suggestive, but they indicate that it is at least plausible that reductions in the acquisition of skills at younger ages when minimum wages are more binding can contribute to the longer-run adverse effects of minimum wages.[47]

6.5 Conclusions

This chapter provides an extensive review of evidence on the question of how minimum wages affect skill acquisition, from three perspectives: the effects of minimum wages on training among youths, the effects of minimum wages on school enrollments of youths, and the longer-run effects of exposure to a high minimum wage at young ages on wages and earnings as an adult (as well as completed schooling). Taken separately, the research on training points to some evidence of negative effects, but it would be difficult to argue that this evidence is conclusive. With respect to schooling, the evidence is stronger, with most of the research for the United States pointing to negative effects and the limited international evidence (mainly for Canada) less robust. Finally, recent research that studies the question more indirectly finds that teens and youths exposed to higher minimum wages have lower wages and earnings when they are in their late twenties, consistent with reduced skill acquisition; the same regression framework points to lower schooling as well, but the strength of this approach is that it does not require measurement of training, which can be quite problematic.

Our motivation in considering the effects of minimum wages on skills is that there could be negative (or, in principle, positive) short-run or long-run effects even in the absence of detectable employment effects on teens and young adults. In particular, reductions in schooling or training among teens or young adults could have adverse longer-run effects that are independent of any minimum-wage-induced changes in employment, and, as a result, could be more widespread than the disemployment effects. As a consequence, the exclusive focus of much of the existing research—and of the policy debate—on the short-run effects of minimum wages on youths in general, and their employment in particular, fails to capture a potentially harmful effect of minimum wages. Moreover, the effects of minimum wages on skills may be especially important from a policy perspective, because the effects are persistent and therefore subsequently fall on older individuals who are more likely to be primary breadwinners in their families.

7 The Effects of Minimum Wages on Prices and Profits

7.1 Introduction

In comparison with the voluminous literature on the effects of the minimum wage on employment and wages, there has been relatively little research on the influence of minimum wages on prices or profits. In part, this may reflect a lack of available data on prices and profits at the firm level. However, it also reemphasizes our earlier criticism about the disproportionate attention that researchers have given to the employment effects of minimum wages at the expense of other considerations.

There are reasons to be interested in the price effects of minimum wages. First, although the direct effect of a minimum wage increase would be to raise the *relative* prices of goods produced with minimum wage labor, opponents of minimum wages have frequently pointed to the potential consequences for *aggregate* inflation from an increase in the wage floor. The basic argument is that minimum wages can be viewed as an exogenous cost shock that, like an increase in oil prices, would raise inflationary pressures in the economy. In addition, in some countries and (more recently) U.S. states, minimum wages are indexed to prices or average wages. Such a direct link between minimum wages and other wages or prices will tend to exacerbate the effects of shocks to wages or prices on unit labor costs and thus could increase the extent to which such a shock would be passed through to future inflation.[1]

Second, even if minimum wages do not result in a persistent increase in inflation, one-time changes in prices and profits have potential welfare consequences for society. For example, an increase in the minimum wage that raises the aggregate price level will reduce real incomes and thus the purchasing power of the household sector. If, instead, minimum wages largely influence relative prices, the welfare implications will depend on how different segments of the household sector are influenced by these relative price changes. And, if higher minimum wages reduce firm profitability, the costs will be borne by the shareholders of the affected companies and perhaps induce firms to exit the market, which may lead to negative effects on employment. Of course, some proponents of minimum wages argue that an increase in the minimum will stimulate the economy by raising the purchasing power of households, so that no one is made worse off from a higher minimum wage.[2] However, such arguments, which date back to the "high-wage doctrine" popular during the Great Depression, have little basis in economic theory, and moreover, would seem to be contradicted by the evidence presented in chapter 3 that a higher minimum wage reduces the employment of less-skilled workers.[3]

Finally, estimates of the effects of the minimum wage on prices and profits can help to provide a more complete picture of how firms behave than is provided by studying only the employment effects of minimum wages. Alternative theoretical models of the labor market not only have different implications for how minimum wages affect employment, but they also differ in their predictions for how prices and profits would respond to an increase in the wage floor. Taken together, the full set of minimum wage effects can help to distinguish among some of the models of the low-wage labor market that have been proposed in the literature.

In this chapter, we review the available evidence on the effects of minimum wages on prices and profits. In a recent survey, Lemos (2008) distinguishes two branches of this literature. The first branch focuses on the relationship between minimum wages and prices or profits at the firm or industry level. In much of this research, the intent is to demonstrate a link (or lack thereof) between minimum wages and prices or profits in cases where, at either the firm or industry level, one would expect to find such an effect. In essence, this approach is similar to the emphasis of much of the research on employment effects on low-wage workers or industries. In some of the more ambitious papers, however, researchers attempt to use their empirical results as a way to test the applicability of some of the alternative models of the labor market to low-wage industries.

The second branch of the literature examines the effects of minimum wages on the aggregate price level and inflation. Although higher prices were always viewed as a potential effect of minimum wages, much of this literature has its roots in the 1970s, when inflation was especially high and researchers spent considerable time studying its causes. Given the lower and more stable inflation environment in the United States and Europe during the past two decades, the newer research on industrialized countries has focused less on the potential effects of minimum wage increases on aggregate inflation. However, there has been some research in recent years that analyzes the relationship between minimum wages and inflation in Latin American countries.

7.2 Theoretical Considerations

The effect of the minimum wage on prices in the standard competitive model of the labor market is relatively straightforward and has been described in considerable detail elsewhere in the literature.[4] In particular, the higher minimum wage increases the cost of employing a low-skilled worker and forces the employer off of the mix of inputs that would be optimal in the absence of a wage floor. As an example, consider a very stylized model that assumes that the supplies of all factors of production are perfectly elastic, that firms can adjust the mix of their factor inputs immediately, that the production function exhibits constant returns to scale, and that product markets are perfectly competitive. In such a model, a higher minimum wage leads to an increase in prices and a decline in both output and employment. Moreover, as long as the elasticities of substitution between all of the inputs in the production function are constant, the extent of the passthrough from a higher minimum wage into prices depends only on the share of total costs accounted for by low-skilled labor, and the decline in output is determined by the price elasticity of the demand for output.[5] In addition, reflecting the decline in product demand, industry profits fall as a result of the higher minimum wage. As both Card and Krueger (1995a) and Aaronson and French (2007) note, the details differ in the case of monopolistic competition in product markets, but the basic results are similar under the assumption that firms face a constant elasticity of output demand.

There is a lengthy set of restrictive assumptions embedded in this stylized model, and, in practice, the size of the response of prices to a minimum wage increase in the competitive model can depend on a number of other factors. First, the degree of substitutability of capital and skilled labor for unskilled labor will affect the extent of the added cost pressures resulting from the higher minimum wage. In particular, the easier it is for firms to shift away from employing minimum wage workers, the smaller will be the increases in marginal costs and prices. Second, a more elastic demand curve for the product will reduce the overall price response by limiting how much of the cost increase can be passed through to prices. In addition, rigidities in the short run may affect the speed with which firms adjust their factor inputs and/or their output prices in response to a higher minimum wage, leading to differences in the price response as compared with the flexible adjustment model described previously. Finally, the extent to which minimum wages influence prices for other inputs of production can affect the overall increase in production costs. For example, if a minimum wage increase leads to higher wages for skilled workers, either because the supply curve for skilled labor is not completely elastic or because the wage structure embodies a direct linkage between the minimum wage and other wages, the increase in marginal costs will be greater than would otherwise be generated by the higher price for unskilled labor. Note, however, that prices increase in all of these cases, so that the potential differences in the effect of minimum wages on prices implied by variations in the competitive model are in size rather than direction.

However, other models of the labor market can lead to a very different relationship between minimum wages and prices. For example, in the textbook monopsony model suggested by Stigler (1946), an increase in the minimum wage can cause a decline in prices if the size of the minimum wage hike is not too large. In particular, firms with monopsony power over low-wage workers will set their employment levels at the point along the supply curve where the marginal cost of labor equals the marginal revenue product of labor, which will result in levels of employment and wages that are below those associated with the competitive equilibrium wage. In such an environment, raising the minimum wage will, over the range between the monopsony wage and the competitive wage, lead the monopsonist to increase employment and output, and thus lead to lower output prices.

In response to widespread skepticism about the relevance of the pure monopsony model to present-day low-wage labor markets, economic theorists developed dynamic monopsony models that had similar outcomes to those in the pure monopsony model. For example, Burdett and Mortensen (1998) develop a search model in which there are many firms but workers have incomplete information about the full range of job opportunities available to them. Because an increase in the minimum wage raises the probability that unemployed workers will receive an offer above their reservation wage, employment and output rise, and prices fall. Still other researchers, such as Bhaskar and To (1999) and Dickens, Machin, and Manning (1999), have shown that employment and price effects similar to those derived from the pure monopsony model also hold in the short run under monopsonistic competition in the labor market.[6]

Other models of the labor market can also have implications for price behavior that differ from those embodied in the competitive labor market model. For example, efficiency wage models that relate effort to wages imply that an increase in the minimum wage can influence worker productivity, which would tend to mitigate the increases in marginal costs and prices associated with the higher minimum wage. Moreover, in some versions of this model (e.g., Rebitzer and Taylor 1995), employment does not fall or can even rise, which, given the increase in productivity, leads to an increase in output and a decline in prices. The "shock" theories proposed in the 1930s, in which it was hypothesized that employers would be induced to remove inefficiencies in their production processes as a result of a minimum wage increase, would lead to similar results.[7] Finally, in the "hungry teenagers" model resurrected by Kennan (1995), a higher minimum wage leads to changes in the distribution of consumer demand toward products produced with minimum-wage labor. In this model, employment can either increase or decrease, but prices rise because of the outward shift in product demand.

At the aggregate level, researchers differ considerably on the specifics of the appropriate model of inflation dynamics, but from a macroeconomic standpoint, we can loosely characterize price inflation as a function of resource utilization rates, expectations of future inflation, and other exogenous price shocks (including changes in the minimum wage). In this context, the initial effect of an increase in the minimum wage on inflation is the aggregation across markets of the effects described previously. That is, an increase in the cost of employing minimum wage workers will directly raise the prices of the goods produced by these workers, which leads to an increase in the aggregate inflation rate in the short run. In the medium- to longer-run span, however, the effect of a minimum wage increase on inflation also depends importantly on how the wage hike influences inflation expectations and on the extent to which monetary policy accommodates the initial inflationary shock generated by the higher minimum wage.

As a result, research on the macroeconomics of minimum wages has tended to distinguish between the price effects of the initial cost shock associated with a minimum wage increase and the follow-on inflationary implications associated with changes in expectations. In some earlier models of inflation dynamics, inflation expectations were often specified as adaptive, so that the initial boost to prices from the higher minimum wage could have a persistent influence on inflation (Gramlich 1976). In contrast, more recent models, such as the New Keynesian sticky-price model espoused by Gali and Gertler (1999), have tended to emphasize the forward-looking nature of the inflation process. As a result, although an increase in the minimum wage initially boosts the price level through its effect on marginal costs, an understanding by economic agents that monetary policy will not tolerate higher inflation will severely limit its longer-run inflationary impact by minimizing the effects on expectations.

In sum, the theoretical link between the minimum wage and prices is ambiguous and depends crucially on the degree of competition in labor and product markets. Moreover, even in the case of competitive labor markets, for which the theory predicts that minimum wages will raise prices, the size of the cost shock associated with a rise in the minimum wage depends on a number of factors, and the propagation of that shock depends on how expectations are formed. As a result, the effect of the minimum wage on prices and inflation is largely an empirical question. In the remainder of this chapter, we review the evidence provided by the relevant research on this issue.

7.3 Macroeconomic Studies of the Effects of Minimum Wages on Prices

Although opponents of minimum wages have often highlighted the potential adverse effects on prices and inflation as arguments against increasing the wage floor, there was, for a long time, little empirical evidence to support or refute these arguments. To be sure, researchers in the 1950s and 1960s sometimes included estimates of price effects in their analyses of specific industries.[8] But the main focus of the more comprehensive studies was on the effects of the minimum wage on employment and wages.

As inflation emerged as a greater concern in the late 1960s and early 1970s, however, macroeconomists began to consider whether minimum wages, among other factors, might be a potential source of inflationary pressures. This concern may also have been encouraged by the numerous increases in the federal minimum wage that occurred in the late 1960s and the 1970s, documented in chapter 2 (see table 2.2).

The predominant working models of inflation dynamics at that time were variants of the wage-price Phillips curve, a simultaneous two-equation system for wage and price determination that, in later versions, included inflation expectations. The minimum wage was hypothesized to raise unit labor costs, with a passthrough into prices through a markup equation. In addition, the model allowed the additional inflation generated by the minimum wage increase to feed back into wages through its effects on inflation expectations. Finally, the minimum wage was seen as possibly raising the non-inflationary rate of unemployment (or NAIRU) by eliminating job opportunities for individuals with very low skills.[9]

There are numerous examples of aggregate models of the inflation process that include a minimum wage variable, but three studies stand out because of their particular focus on the role of minimum wages. The first, by Gramlich (1976), is noteworthy because it finds that a 10 percent increase in the minimum wage raised average wage growth by about 0.3 percentage point, twice as much as would be expected from the direct impact of the higher minimum on those workers whose wages were initially below the new minimum wage, suggesting that there were spillovers from the minimum wage into wages of higher-paid workers. Although Gramlich did not fully trace out the effects on price inflation, he noted that "the adaptive-expectations terms in the wage-setting equations . . . are high enough that this supposed one-shot change in the overall price level would be converted into a nearly permanent one-shot change in the *rate* of price inflation" (1976, 430). Sellekaerts, in a paper included in the Minimum Wage Study Commission's report (1981), simulated a two-equation model of wage and price inflation to derive estimates of the effects of a 10 percent rise in the minimum wage. She found a somewhat larger effect on average wage inflation—about 0.76 percentage point after six quarters—and reported an increase in consumer price inflation of about 0.15 percentage point.[10] Finally, Frye and Gordon (1981) included a minimum wage variable in a reduced-form model of price inflation and reported that a 10 percent rise in the minimum would boost price inflation by 0.2 percentage point.[11]

Two studies in the commission report also attempted to build up estimates of the effect of minimum wages on prices using more detailed structural models. Cox and Oaxaca (1981a) derived and estimated a general equilibrium model of demand and supply for nine major sectors of the economy and reported, among other results, that the 1974–1978 increases in the federal minimum wage (which amounted to a cumulative increase of 65 percent) boosted prices by about 1.5 percent on average, with industry-specific effects ranging from less than 0.1 percent in mining to more than 3 percent in agriculture and services. Wolff and Nadiri (1981) used a modified input-output framework at the industry level that allowed for substitution among inputs in response to a minimum wage increase. Simulating the model, they found that a 25 percent increase in the minimum wage would raise overall consumer prices by as much as 0.7 percent, with the increase concentrated in prices of household services. The effects in both papers seem roughly comparable (when scaled to a 10 percent increase in the minimum wage) to those estimated in the aggregate time-series specifications, although their conclusions were generally dismissed by the commission because of the stylized models and strong assumptions underlying the simulations.[12]

In general, the impact of the increases in the minimum wage on prices in the late 1970s and early 1980s was judged to be small relative to those stemming from other supply shocks at the time, such as the spikes in oil prices or the slowdown in productivity growth.[13] Moreover, as inflation fell during the 1980s and 1990s, macroeconomists modified their models to include a greater role for forward-looking expectations, recognizing the important role that credible monetary policy aimed at low inflation could play in mitigating the overall inflationary effects of cost shocks.[14] Thus, while more recent models of the aggregate inflation process in the United States sometimes still include a variable to capture the short-run impact of changes in the minimum wage,[15] Freeman notes that "the wage inflation argument has disappeared from discourse" (1996, 645). As we note shortly, however, linkages between minimum wage policy and inflation remain a concern in some lesser-developed countries where minimum wages have traditionally played a larger role in the wage-setting process.

7.4 Evidence from the New Minimum Wage Research

Although concerns about the inflationary consequences of minimum wage policy in the United States diminished over time, growing interest emerged regarding the potential for using the estimated effects of minimum wages on prices to help distinguish among alternative models of the low-wage labor market. With this in mind, some of the early studies constituting the new minimum wage research conducted rudimentary analyses of the effects of the minimum wage on prices, although this aspect of the research received much less attention than the accompanying analyses of employment effects. Like the employment analyses, the research on price effects tended to focus on the restaurant industry, reflecting the sizable share of minimum wage workers employed in establishments in this industry, and were in some cases based on the same surveys that were conducted to look for employment effects.

7.4.1 Case Studies

In his paper in the *ILRR* symposium, for example, Card (1992b) provides a very simple comparison of city-level data on changes in the CPI for food away from home from 1987 to 1989, the period surrounding the increase in California's minimum wage from $3.35 per hour to $4.25 per hour. The analysis is based on twenty-four cities nationwide, including three in California (Los Angeles, San Diego, and San Francisco) where restaurant costs should have been adversely affected by the state minimum wage hike. The results indicate that prices in San Diego rose more over that period than the average increase for all of the cities included in the sample, consistent with the relatively large increase in wages Card finds for teenagers in that city. In contrast, neither Los Angeles nor San Francisco experienced an especially large price increase, despite the increase in the minimum wage. Card then repeats this analysis using data on quarter-pound hamburger prices for 250 cities taken from surveys conducted by the American Chamber of Commerce Researchers Association (ACCRA). Of the seven California cities included in the sample, price increases over the 1987–1989 period were slightly greater than the all-city average in three (Riverside, Sacramento, and San Diego), but about the same as the average in the other four (Bakersfield, Fresno, Los Angeles, and San Jose). From this mixed evidence, Card concludes that there is no compelling link between the California increase in the minimum wage and restaurant prices in that state.

In their paper in the same symposium, Katz and Krueger (1992) include an analysis of the effect of the April 1991 federal minimum wage increase on prices at fast-food restaurants in Texas. More specifically, these authors asked survey respondents for the prices of a set of standard items that constituted a full meal at these restaurants in both January 1991 and July/August 1991 (before and after the minimum wage increase). They then regress the change in the price of a meal at each restaurant on the gap between the new minimum wage and the starting wage in December 1990 to see how the price increases differed at establishments for which the minimum wage led to larger or smaller increases in the starting wage. The results show a relative decline in prices for restaurants more affected by the minimum wage increase, but the point estimates are not statistically different from zero. However, they interpret the absence of clear evidence of a positive relationship between the minimum wage and prices, coupled with their finding of a relative increase in employment at establishments more affected by the minimum wage increase, as evidence against the competitive model of labor markets.[16]

Card and Krueger (1994) also look for price effects in their study of the increase in New Jersey's minimum wage in April 1992. Following the same basic procedure as Katz and Krueger, they collected survey responses on the price of a standard meal at establishments in New Jersey and Pennsylvania and compare price changes for establishments for which the minimum wage increase was more important with those for which the minimum wage change was less important. Defining a wage gap variable that measures the increase in the starting wage required to bring it into line with the higher minimum, they find a positive, albeit insignificant, relationship between the bite of the minimum wage and prices for the sample as a whole, and essentially no relationship for stores in New Jersey. However, when they simply compare restaurants in New Jersey with those in Pennsylvania (where the minimum wage did not change), the results are much stronger, with prices in New Jersey rising more than 3 percent faster than in Pennsylvania—enough to fully pass through the labor cost increase associated with the rise in New Jersey's minimum wage.

Finally, Powers, Baiman, and Persky (2007) include some estimates of price effects in their study of minimum wage increases in Illinois. As noted in chapter 3, they use a difference-in-differences approach that—for border counties—compares fast-food restaurants in Illinois with those in Indiana (where the minimum wage did not change); they also estimate the model using the same wage gap variable that was used in the earlier case studies. For the simple comparison of restaurants in the two states, they find no evidence that meal prices rose faster in Illinois than in Indiana following the minimum wage increases. Using the gap measure, the evidence is somewhat stronger, with prices rising faster at restaurants that needed to raise their starting wage more to bring it into line with the higher minimum wage.

We highlighted in chapter 3 some important criticisms leveled at the case study approach used in these papers; specifically, the comparability of the treatment and control groups, the difficulties in capturing lagged effects in these studies, and questions about the reliability of the data. Of these, the first seems most relevant to their investigation of price effects of minimum wages. Given the substantial variation in prices across local markets, the absence of controls for demand shocks across markets seems like an important omission. In contrast, other research (e.g., Aaronson 2001) suggests that prices are less rigid than employment, and thus that the lags in adjusting prices in response to a minimum wage increase would be shorter. In addition, the definition of prices used in these studies seems reasonably well-defined, especially when compared with the vague definitions of employment levels used in the surveys conducted for the Katz-Krueger and Card-Krueger studies. Nevertheless, the first concern, as well as small sample issues, makes the results from these studies difficult to interpret.

7.4.2 Panel Studies

Card and Krueger (1995a) take an approach similar to that used in Card (1992b) to analyze the effects on prices of the 1990 and 1991 increases in the federal minimum wage. In particular, they first regress the city-specific change in the CPI for food away from home from 1989 to 1992 on the fraction of workers in the city's retail trade industry who earned between \$3.35 and \$4.25 per hour in 1989 and thus were directly affected by the federal minimum wage increases in that period.[17] The results indicate that restaurant prices rose more rapidly in cities with a higher fraction of affected workers; moreover, the coefficient is statistically significant in two out of the three specifications, and the magnitude of the estimate is broadly consistent with the prediction of the competitive model that the price increase associated with the boost to wages from minimum wage increases should be proportional to the cost share of minimum-wage labor. They then turn to the ACCRA price data and construct statewide averages of price increases for hamburgers between the first quarter of 1990 and the first quarter of 1992. The estimates with these data are also positive, but generally are not statistically significant.

Aaronson (2001) extends this analysis from 1978 to 1995, using a metropolitan-level panel data approach similar to the state-level approach we used in our original study of the employment effects of minimum wages (Neumark and Wascher 1992). In particular, using both the city-specific CPI indexes for food away from home and ACCRA price data for three fast-food products, he regresses the change in prices on the change in the effective minimum wage in the city, changes in prices of inputs common to the restaurant industry, overall city-specific inflation and employment conditions, and city fixed effects. The estimates from the CPI specifications suggest that minimum wage increases boost restaurant prices, with the price response concentrated in the three-month period surrounding the increase in the wage floor. The estimated effects are statistically significant, with the elasticity of the price response ranging from 0.037 to 0.072. Regressions using the ACCRA data show a wider band of price responses (with elasticities ranging from 0 for pizza to as high as 0.12 to 0.16 for hamburgers and chicken), but most of the estimates are positive and the price response again occurs quite quickly.[18]

Aaronson notes that a shortcoming of using the U.S. CPI data to study minimum wage effects is that only seven of the twenty-seven cities sampled by the BLS are in states that experienced a minimum wage increase over the 1978–1995 sample period, so that most of the identification stems from changes in the federal minimum wage over this period. In addition, the ACCRA data are less than ideal, because the individual city observations are often based on small samples and are collected quarterly rather than monthly. As a result, Aaronson augments his study of the United States with CPI data from Canada, where there is a separate minimum wage for each province. In particular, ninety-seven province-specific minimum wage increases took place over Aaronson's sample, as opposed to about twenty (federal and state) in the U.S. sample. The estimated passthrough elasticities from regressions using the Canadian minimum wage changes and province-specific price indexes for food purchased at restaurants range from 0.048 to 0.080, quite similar to the results using the U.S. CPI data. Aaronson notes that the size of the estimated passthrough is sensitive to the specification used, but the evidence that minimum wage increases led restaurants to raise their prices over this sample period is quite compelling.

One question raised by this research is whether the higher labor costs associated with minimum wage increases are fully passed through to consumer prices.[19] As noted previously, complete passthrough of a minimum wage increase would be consistent with the competitive model of labor and product markets, in which firms reduce output to a level at which price is equal to the higher marginal cost of production generated by the minimum wage hike. In contrast, evidence of over- or undershifting would be less supportive of this model.[20] Because the effect of a minimum wage increase on marginal costs depends on the degree of substitutability across factors of production, wage spillovers, and other influences, precise estimates of what constitutes "full passthrough" can be difficult to measure.[21] Using a detailed input-output model for the retail food sector and assuming fixed factor inputs, Lee and O'Roark (1999) calculate that full passthrough of an increase in the minimum wage would imply an elasticity for prices at eating and drinking establishments of between 0.075 and 0.114; as they note, however, this estimate should be interpreted as an upper bound. Thus, most of Aaronson's estimated price elasticities for the U.S. and Canadian restaurant industries appear to be in a range that would be consistent with complete or nearly complete passthrough of minimum wage increases into retail prices, especially if employers are able to substitute away from low-skilled labor in response to a higher minimum wage.[22]

In subsequent work, MacDonald and Aaronson (2006) look more closely at how restaurants adjust prices in response to minimum wage increases. The authors use the microlevel data that are collected by the BLS to form the CPI for food away from home. The sample includes price quotes for 7,500 food items taken at more than 1,000 different establishments in 88 different geographic areas (mostly Metropolitan Statistical Areas, or MSAs) for the three-year period from January 1995 through December 1997. Although minimum wage legislation during this time period was dominated by the 1996 and 1997 increases in the federal minimum wage, the authors argue that the variation in state minimum wage levels just prior to the federal increase, combined with a few explicit changes in state laws, provides sufficient variation to identify the effects of minimum wage increases on prices.

For overall restaurant prices, the authors report a statistically significant price elasticity with respect to the minimum wage of 0.073 for the full sample of establishments, similar to the elasticity reported by Aaronson (2001) using aggregate data. However, the price responses are much larger for limited-service outlets than for full-service restaurants, which the authors suggest reflects the likelihood that the proportion of workers who are paid at or near the minimum wage is higher at limited-service establishments.[23] In addition, the estimated price effects are larger in low-wage areas, where the increase in the federal minimum wage would be expected to have a larger effect on wages at the low end of the distribution. More interestingly, however, they show that restaurants are quite selective in choosing which prices to raise in response to a minimum wage increase. Rather than boosting all prices a little, limited-service establishments were more likely to raise prices on subsets of items for which prices had recently been cut and were less likely to raise prices on items that had been set at psychological pricing points (e.g., prices ending in 99 cents). Thus, while the results suggest that restaurants adjust their average prices to reflect changes in labor costs, they also appear consistent with menu cost rigidities in individual prices.

Although most of the research discussed thus far has been for the United States, there is one study for the United Kingdom by Draca, Machin, and Van Reenan (2005) that weighs in on the effect on prices of that country's reintroduction of a national minimum wage set to £3.60 per hour in April 1999.[24] The authors first attempt to identify the effects of the minimum wage on prices using a cross-section dataset of producer prices for 240 manufacturing industries. In particular, they regress the change in log prices between 1998 and 1999 on a set of industry control variables and the proportion of workers in each industry who earned less than the new minimum wage in 1998. A reduced form version of the model yields a coefficient on the minimum wage variable of 0.034, but that estimate is not statistically significant. An instrumental variables estimate yields a smaller effect (0.020), which is also not significant. Moreover, both estimates are quite small relative to the estimated coefficient (1.72) on the minimum wage variable in a similar equation for average industry wages.

7.4.3 Using Price Responses to Test Alternative Labor Market Models

Finally, a couple of recent papers by Aaronson and his coauthors attempt to use the estimated price and employment effects in combination to identify more precisely the appropriate model of low-wage labor markets. In particular, Aaronson and French (2007) combine the estimates of positive price effects for the restaurant industry from Aaronson 2001 and MacDonald and Aaronson 2006 with the negative employment effects reported in the literature and compare them to predictions from calibrated versions of alternative models of the labor market. They find that with their calibrated substitution elasticities, a benchmark competitive model that yields a 0.07 price elasticity in the restaurant industry (as reported in the earlier papers) predicts an employment elasticity for low-skilled workers with respect to the minimum wage of around −0.35. They then augment the model so that employers have some monopsony power in the labor market. Given the estimated price responses, however, this augmented model predicts that only a few employers will increase employment in response to the minimum wage, so that the implied employment elasticity is only slightly smaller (about −0.3). As a result, the authors conclude that monopsony power cannot explain the findings of employment effects near zero (or even positive) in some studies of the restaurant industry. This, in turn, either casts further doubt on such findings, or, if such findings are correct, suggests that other explanations need to be considered instead.

Aaronson, French, and MacDonald (2006) extend this methodology to examine alternative versions of the monopsony model, as well as several other models that have been proposed in the literature. They begin by using the same micro dataset as do MacDonald and Aaronson (2006) to document that prices generally rise in response to an increase in the minimum wage. Specifically, they report that prices increased for 22.6 percent of items sold at limited-service restaurants in the two-month period immediately following a minimum wage hike—more than twice the percentage that experienced price increases in other months. In addition, prices were no more likely to decline after a minimum wage increase than in other months. Similarly, at the store level, 38 percent of limited-service establishments raised average prices in the two months following a minimum wage increase, while 24 percent of these establishments raised average prices in other months. In contrast, there was no evidence that limited-service establishments were more likely to lower average prices following an increase in the minimum wage. These tendencies were also apparent for full-service restaurants, although the differences were smaller and sometimes not statistically significant. On average, the authors estimate that a 10 percent increase in the minimum wage raises restaurant prices by about 0.7 percent, similar to their estimate of what would be implied by full passthrough. They also reemphasize that the effects are larger for limited-service establishments and in lower-wage areas, hypothesizing that the minimum wage is likely to be more binding for these subsamples.

Aaronson, French, and MacDonald then complement these results with an updated analysis using city-level data on changes in the CPI for food away from home for the period 1979–1995. In particular, using a sample covering each instance of a minimum wage change in a city (induced either by a change in the federal minimum or a state minimum), they regress the ratio of the log change in prices to the change in the minimum on the proportion of restaurant workers in each city over the past nine months who earned less than 120 percent of the old minimum wage; a set of fixed city effects is included in the regression as well. The estimated coefficient is positive and, at 0.36, is close to estimates of labor's share in the restaurant industry derived from 10-K company reports, economic censuses, and IRS data, and consistent with essentially complete passthrough of minimum wage increases into restaurant prices.[25] The authors include two robustness checks. They first test whether unobserved demand shocks might account for their results by replacing the fixed year effects in the model with the change in the city-specific total CPI. The intercept in this specification is not statistically different from zero, indicating that prices did not tend to accelerate after minimum wage increases in cities in which the minimum wage was not binding. Second, they replace the "food away from home" CPI with the CPIs for housing services and medical care (two components that should be relatively unaffected by the minimum wage), and find no evidence that price increases for these items are correlated with the share of workers earning less than the new minimum wage.

Armed with these results, Aaronson, French, and MacDonald compare them to the predictions derived from some proposed theoretical models of the low-wage labor market. As in Aaronson and French (2007), they argue that the large short-run price effects they find are inconsistent with a monopsony model of the labor market, and they further argue that this conclusion extends to models of monopsonistic competition proposed by Bhaskar and To (1999) and Dickens, Machin, and Manning (1999). Although they acknowledge that firm exit could generate a positive price response in a model of monopsonistic competition, they assert that observed exit rates are too small to generate this effect. Moreover, they argue that the observed price increases in their empirical analysis occur too quickly to be consistent with the firm exit needed to produce the rise in prices in the Bhaskar and To model.

Aaronson, French, and MacDonald also briefly consider several other models that have been proposed in the literature. First, they compare their findings to the implications of efficiency wage models in which work effort is endogenous to the wage. As noted previously, the employment and price responses in this model should be muted by the higher productivity induced by the higher minimum wage.[26] However, Aaronson, French, and MacDonald argue that if such efficiency wage effects were important, the estimated price responses should be smaller than predicted by the competitive model, a prediction that runs counter to their empirical results. Another model they look at is Kennan's "hungry teenager theory" (1995), in which a rise in the minimum wage alters the distributions of income and consumer spending in a way that shifts demand towards products produced with minimum wage labor. In the case of the fast-food industry, the idea is that an increase in the minimum wage might raise the incomes of a subgroup of low-wage workers (i.e., teenagers) who have a particular penchant for fast food. Moreover, other workers whose income is adversely affected by the higher minimum wage might shift their food consumption patterns toward lower-price options like fast food. As a result, the disemployment effects of the minimum wage will be smaller and prices will rise by more than in the baseline competitive model. Indeed, if the outward shift in the product demand curve is sufficiently great, employment may even rise.[27] Aaronson, French, and MacDonald observe that their price responses are consistent with the hungry teenager hypothesis, but they estimate that this effect could, at most, offset only 10 to 30 percent of the employment losses predicted by the competitive model.[28] The final set of models they reference are search models of the labor market. As they note, however, many versions of these models (e.g., Burdett and Mortensen 1998) have implications for employment and prices that are similar to those in monopsony models and thus inconsistent with the observed price increases they report.

Thus, in the end, the more recent and more thorough research on the price effects of minimum wages in the United States most strongly supports the competitive model of low-wage labor markets. Indeed, the prevalence of positive estimates and the near-absence of any finding of a negative effect on prices would seem to argue strongly against noncompetitive interpretations of firm behavior in these markets.

7.5 Minimum Wages and Prices and Inflation in Developing Countries

As noted previously, the potential link between minimum wages and inflation has remained a concern for many developing countries. In part, this concern reflects the high rates of inflation that plagued some developing countries in recent decades, together with the fact that minimum wage changes tend to have an especially noticeable effect on the wage structure in such countries, both because they tend to be binding for a sizable fraction of the labor force and because they have sometimes been used as a basis for determining wage changes in the public sector and for higher-skilled workers. In particular, Maloney and Mendez (2004) note that the use of the minimum wage as a numeraire for other wages and for pensions was common in Latin America and that, in Colombia, wages well above the minimum wage seem to be influenced by changes in the wage floor. Similarly, as we noted in chapter 4, Fajnzylber (2001) estimates that increases in the minimum wage in Brazil have had a significant influence on wages for formal-sector workers earning as much as forty times the minimum wage, as well as for self-employed workers and those working in the informal sector. Although we are skeptical about some of these results, Lemos (2004b) also finds a positive relationship between minimum wage increases in Brazil and wage changes well up into the wage distribution.

Despite these apparent sizable wage spillovers, the only research on minimum-wage-related price effects of which we are aware consists of a set of studies on Brazil by Lemos (2004b, 2006b). In particular, Lemos (2006b) focuses on the effects of changes in that country's minimum wage on consumer prices from 1982 to 2000, a sample period that included sharp increases in the nominal minimum wage in a high inflation environment. Because the simultaneity of inflation and minimum wage changes complicates identification of the minimum wage effect, Lemos employs a regional panel of monthly observations and includes lags of both the minimum wage and inflation in her estimating equation as well as fixed time and regional effects. For most of her sample, there is no regional variation in the minimum wage, and so Lemos measures the bite of the minimum wage with two relative minimum wage variables (one that uses the average wage as the denominator, and another that uses the 90th percentile wage), as well as with a variable measuring the fraction of workers paid the minimum wage for each region/month observation.

Her results generally indicate that minimum wage increases had a sizable impact on prices in Brazil.[29] Her preferred specification models price changes as a function of changes in the average wage and uses leads and lags of the fraction paid the minimum as instruments to capture the variation in the average wage due solely to changes in the minimum wage. Based on this specification, she finds that a 10 percent increase in the minimum wage results in an increase of 3.5 percent in the overall price level within a two-month window surrounding the minimum wage increase. However, this result is driven largely by the high-inflation period prior to 1994; for the 1994–2000 subperiod, the estimated price increase from a 10 percent increase in the minimum is only 1.3 percent and is not statistically significant. Although she does not supply enough information on the dynamics of inflation to say for sure, the implication is that minimum wage policies in effect in Brazil during the 1980s and early 1990s contributed to the high inflation experienced at that time.

Lemos (2004b) examines the welfare implications of these minimum-wage-induced price increases in Brazil. In particular, she estimates the same specification as above for three separate price indexes—a broad index based on a consumption bundle for all households, an index based on a consumption bundle purchased by middle-class households, and an index intended to measure prices faced by households earning the minimum wage. The regression estimates indicate that, initially, the minimum wage has the largest effect on prices of goods purchased disproportionately by poor households. Over time, however, the effects on prices are borne more equally across households, perhaps reflecting the sizable spillovers to other wages.

7.6 Minimum Wages and Profits

Given that most of the theoretical models of the minimum wage start from the assumption that firms operate in a way that maximizes profits, they also predict that an increase in the minimum wage will reduce profits. In particular, in the standard neoclassical model of the labor market, prices rise to match the increase in marginal costs associated with a higher minimum wage, but, as a result, output and profits decline. However, the extent to which other changes employers make in response to a minimum wage increase offset the associated higher labor costs will influence the magnitude of the profit decline. As we have discussed, such adjustments might include shifting the mix of factor inputs away from minimum wage labor or reducing nonwage benefits.

As Card and Krueger (1995a) emphasize, the situation is more complicated in some of the other models that have been proposed in the literature. For example, they show that in efficiency wage models in which productivity depends on the level of the wage, an increase in the minimum wage relative to the wage consistent with profit maximization would, to a first-order approximation, leave profits unchanged; this result also holds for the standard monopsony model in which employment and output rise, but prices fall. More generally, models in which employers set both employment and wages to maximize profits yield this type of result, although Card and Krueger also note that the second-order effects associated with more than a marginal increase in the minimum wage would be negative. In addition, monopsony models that allow for entry and exit will eventually restore firm-level profits to their original levels (even if industry-level profits fall) as unprofitable firms exit the market. Finally, in models in which firms do not maximize profits, firms may be induced to operate more efficiently following an increase in the minimum wage. If so, and if the cost savings achieved by moving toward the production-function frontier are sufficient to offset the higher labor costs associated with the minimum wage hike, profits may not decline.

There are only a few empirical studies that examine the effects of the minimum wage on profits. Card and Krueger (1995a) include one such study in their book, in which they combine data on stock prices with news stories about the minimum wage to conduct an event study of the effects of changing expectations about future minimum wage increases on expected profits. The authors first estimate the daily excess returns on stock prices for two subsamples of firms.[30] The first subsample consists of 110 firms in industries that have the highest proportions of minimum-wage workers, and the second subsample consists of 28 firms (mostly in the restaurant industry) that referred to the 1990–1991 minimum wage increases in their annual reports. They then identify 20 newsworthy events related to the progress of the bill introduced in Congress that eventually resulted in the 1990–1991 increases in the federal minimum wage. A few events show some statistically significant evidence of an expected effect; most notably, estimates of excess returns in the first subsample were positive for the ten-day interval surrounding the Republican filibuster of the minimum wage bill in September 1989. But excess returns appear to be of the "wrong" sign for intervals surrounding several other news events, and in most cases there was no significant movement in either direction for both subsamples of firms.

Card and Krueger also examine a subsequent set of events related to a proposal by Labor Secretary Robert Reich to boost the minimum wage to $4.75 per hour. In this case, there is some evidence that the excess returns were negative at the time the specific proposal was reported and were positive when Secretary Reich recommended postponing the increase. In general, however, the evidence of a link between the minimum wage and expected profitability from this analysis is pretty weak, although, as Card and Krueger note, this result could simply mean that the events included in the analysis were not viewed by market participants as providing much news about the likelihood of future minimum wage changes.

One other recent study that addresses this question is by Draca, Machin, and Van Reenan (2006). An advantage of the approach used by these authors relative to the Card-Krueger study is that they directly estimate the link between profits and the re-introduction of the minimum wage in the United Kingdom using firm-level data on profit margins, rather than inferring the effect from investor returns. In the first part of the study, Draca, Machin, and Van Reenan focus on the residential home care sector, in which about 40 percent of workers were paid below the new minimum wage of £3.60 per hour prior to its introduction in April 1999. A key feature of this industry that limits the generalizability of the results is that government regulations restricted the extent to which firms could raise prices, thus forcing more of any adjustment to the minimum wage onto profits or employment. Surveys were conducted of firms in this industry for a variety of years, including years both before and after the new minimum wage was put in place.

To examine the effect of the minimum wage on profitability, the authors construct a wage gap variable, defined as the percentage increase in the firm's wage bill that would be required to bring the wages of all of its workers up to the minimum. For the year prior to the introduction of the minimum wage, the wage gap averaged about 4 percent, and it fell to about 0 once the minimum wage was in effect. They then regress the change in the average wage from 1998 to 1999 for each firm on the measure of the wage gap in 1998, and compare the coefficient on the wage gap variable to a similar regression for 1992 to 1993. The coefficient on the wage gap variable is considerably larger for the period that includes the minimum wage, which they interpret as evidence that the new minimum wage significantly raised wage costs for residential care homes.[31]

The authors then repeat this analysis using a measure of profitability, which they define as the ratio of profits to total revenue.[32] In particular, they regress profitability of each firm on the initial wage gap and a set of controls that includes demographic characteristics of the home's population and workforce, as well as county-level fixed effects. The estimated coefficient on the wage gap variable is -0.59 and statistically significant, which indicates that a firm with a 10 percent wage gap prior to the minimum wage saw its profit margin decline by 0.059. Based on the sample statistics, which indicate that the average firm had a wage gap of about 4 percent and an initial profit margin of 0.102, the authors calculate that the new minimum wage led to a 23 percent decline in the average firm's profit margin. Despite what seems to be a large effect, the authors find no evidence that the minimum wage increased the probability that these firms exited the market.

In the second part of the study, the authors use accounting data from a subsample of all firms registered in the United Kingdom that reported end of year accounts on March 31, and define profitability in terms of the profit-to-sales ratio. In particular, they compare the change in average profit margins for the three-year periods before and after the date that the new minimum wage was introduced (April 1, 1999) for a "treatment" group of firms whose labor costs were more heavily affected by the minimum wage and for a "control" group of firms whose labor costs were less affected by the minimum wage. Because they do not have data on the distribution of wages within each firm, they define the treatment group in two different ways. The first definition consists of all firms for which the average annual wage was less than £12,000 in the accounting year preceding the introduction of the minimum wage. The second definition further restricts the treatment group to those low-wage firms that were in industry-region cells where 10 percent or more of workers earned less than the new minimum wage in the preceding year.[33]

Draca, Machin, and Van Reenan first check whether the minimum wage had a substantially larger impact on labor costs in the treatment groups than in the control groups. Using a simple difference-in-differences approach, they estimate that wages for the first treatment group of firms rose by 21 percent on average in the three-year period following the introduction of the minimum wage, whereas wages rose 12 percent in the control group of firms, for a difference-in-differences estimate of 9 percentage points (which is statistically significant).[34] They find similar results for the second definition of the treatment group. The differences are somewhat smaller in an annual panel regression analysis in which a variety of industry-level controls are included, and indicate that average wage growth was 5 to 6 percentage points faster in the treatment groups than in the control groups after the introduction of the minimum wage. They also estimate a version of the regression model that uses the firm's average wage prior to the introduction of the minimum wage as the treatment variable. In this case, firms with a lower initial average wage experienced faster wage growth after the wage floor was put in place, consistent with the results for the more discretely-defined treatment groups.

The authors then repeat this analysis for their measure of profitability. In the simple difference-in-differences model, the profit-to-sales ratio fell by between 0.02 and 0.03 in the two treatment groups following the introduction of the minimum wage and rose about 0.01 in the associated control groups. As a result, the difference-in-differences estimate of the effect of minimum wages on profitability is about −0.03 and statistically significant. The regression-based framework that includes controls yields very similar results, with the estimated effect ranging from −0.031 to −0.042. Finally, the results using the initial wage to define treatment intensity show that the minimum wage had a negative effect on profit margins in lower-wage firms.[35] Given the average level of the profit-to-sales ratio for the sample, these estimates translate into declines in profit margins ranging from 7.8 to 10.7 percent.

The results from this study are unsurprising. The hypothesis that the introduction of the minimum wage had a negative effect on the profitability of low-wage employers in the United Kingdom seems reasonably well supported by the data, a result consistent with most theoretical models of the low-wage labor market. In their conclusions, the authors suggest that the ability of firms to absorb minimum wage increases through reduced profits may help to explain the absence of large disemployment effects from the minimum wage. Although the drop in profitability in the residential home care market does seem large, the high rate of exit and entry in this industry in general, and the fact that the authors do not have time-series data on entry rates, make it difficult to gauge the overall size of the employment effects.

7.7 Conclusions

Theoretical predictions for the effects of minimum wages on prices are ambiguous. The conventional competitive model of the labor market predicts that an increase in the minimum wage will boost prices of products produced with low-skilled labor, while monopsony-style models mostly predict a decline in prices in response to a hike in the wage floor, at least over some range of the minimum. In contrast to these latter models, the limited empirical evidence consistently indicates that increases in the minimum wage lead to increases in prices of goods and services produced with low-skilled labor and thus poses a challenge to the assertions that the standard competitive model of low-wage labor markets is incorrect.

We would caution, however, that the research in this area lags behind that discussed in most of the previous chapters, both in terms of its quantity and (in some cases) its quality. Many of the studies reviewed here are quite simplistic in nature, while others are based on very short samples or suffer from data quality issues or other problems. As a result, much of this research should be read as suggestive rather than authoritative. That said, the most reliable studies, which in our opinion are those by Aaronson and his coauthors, do clearly point to a positive effect of minimum wages on prices and cast considerable doubt on the relevance of noncompetitive models of the low-wage labor market.

Finally, even if minimum wages boost prices in low-wage industries, the inflationary impact of modest minimum wage increases in the aggregate economy is unlikely to be important in industrialized countries. Both because of the relatively small share of production costs accounted for by minimum wage labor and because of the limited spillovers from a minimum wage increase to wages of other workers, the effect of a minimum wage increase on the overall price level is likely to be small. And, as a result, minimum wage increases probably have little, if any, measurable impact on inflation expectations. In contrast, for developing countries, where inflation expectations are often less stable and the minimum wage is binding for many more workers, minimum wages could potentially have more adverse inflationary consequences, especially if they are indexed to prices or other wages.

8 The Political Economy of Minimum Wages

8.1 Introduction

We have spent much of this book presenting evidence that minimum wages are a relatively ineffective social policy for aiding the poor. They entail disemployment effects that are felt most heavily by low-skilled workers. They discourage human capital formation. They lead to price increases on products frequently consumed by low-income families. And, on balance, they seem to do little, if anything, to raise the incomes of poor and near-poor families, and more likely have adverse effects on these families.

Despite these findings, minimum wages are extraordinarily popular. According to a 2006 survey conducted by the Pew Research Center, 83 percent of Americans favored raising the minimum wage to $7.15 per hour, and nearly half of the respondents to the survey said that they would strongly support this increase (Dimock 2006). Minimum wage increases are also strongly supported by labor unions, liberal advocacy groups, and even some large corporations. And in the political arena, even conservative politicians generally opposed to government intervention find it difficult to publicly oppose minimum wage legislation when it is put to the vote, and instead often attempt to derail it through procedural channels or to link it to tax incentives that favor the small businesses they believe will be harmed the most by the additional costs associated with a higher minimum wage (or which they want to help for other reasons).

Why are minimum wages so popular? In part, their popularity undoubtedly reflects a genuine and well-intended desire by Americans to help less well-off families and, more generally, a related discomfort with the degree of economic inequality in modern day society. And, in this context, the strong support for minimum wages may simply reflect a lack of clear understanding about their effects. But there may be other motivations for supporting minimum wage laws as well. For example, as we have highlighted in earlier chapters, an increase in the minimum wage effectively leads to a redistribution of income across members of society, so that there are both winners and losers from a higher wage floor. Moreover, members of particular institutions or policy-relevant groups may have reasons to support or oppose minimum wages for noneconomic reasons. In this chapter, we explore some of these issues by reviewing the existing literature on the political economy of minimum wages.

Although much of the discussion in this chapter focuses on the federal minimum wage, inaction at the federal level has, over time, increasingly shifted the debate to the state level. As we already noted, as of the beginning of 2008, thirty-two states and the District of Columbia had a minimum wage that was higher than the federal rate. Moreover, in the mid-1990s, political support for minimum wage floors emerged in a new arena—local governments. In cities and other local jurisdictions across the country, campaigns arose in support of "living wages," and governments adopted them by the score. At latest count, there were more than 140 living wage laws on the books.[1] Living wages share with minimum wages the specification of a minimum wage floor, although the coverage is typically much narrower and the wage level much higher.

Thus, in the final part of this chapter, we provide a brief discussion of the burgeoning evidence on the effects of living wage laws. The background and evidence that this discussion establishes sets the stage for then considering the same questions that this chapter addresses with respect to federal and state minimum wage laws. That is, we attempt to describe some of the motivations of various actors to support living wage laws, with an emphasis on the political economy of such laws.

8.2 Why Are Minimum Wages So Popular?

Public support of minimum wage laws is not a recent development. According to Waltman, in a Gallup poll taken in 1936, "70 percent of Americans said they favored a constitutional amendment to regulate minimum wages" and "especially since the 1970s, the percentage favoring an increase has hovered around 75–80 percent" (2000, 49). In contrast, until very recently, the bulk of the economics profession was decidedly opposed to minimum wage laws, as were the editorial boards of some influential newspapers, including the *New York Times*.[2] And, even as recently as 2005, nearly half of economists responding to a survey believed that the minimum wage should be eliminated (Whaples 2006). That said, as evidenced by the signed endorsements that are frequently circulated by proponents of minimum wage increases, many economists do, in fact, support minimum wages.[3]

It has sometimes been argued that popular support for the minimum wage stems from a lack of understanding about its effects.[4] Waltman provides some evidence for this, noting that a variety of survey results suggest that public views of the effects of the minimum wage on employment have, in the past, differed markedly from the opinions of economists (2000, 59–62). For example, in a 1979 survey, 71 percent of respondents believed that a higher minimum wage would *not* mean fewer jobs for young people, a sharp contrast to the 90 percent of economists who believed that higher minimum wages led to higher unemployment in a survey taken just a year earlier. Similarly, 70 percent of the public responding to a 1994 survey believed that raising the minimum wage would help the "overall job situation," while few thought that keeping the minimum wage constant was a good idea. Of course, one might argue that the greater diversity of opinions among economists since the emergence of the new minimum wage research about the effects of the minimum wage might be a source of increasing public confusion. However, the persistent differences between the views of economists and the public stemming back to the 1970s run counter to that claim.[5]

There are several reasons why the public might have a more positive view of the minimum wage than seems to be warranted by the empirical evidence gathered from economic research. First, the influence of modest changes in the minimum wage on the national economy is undoubtedly small relative to business cycle fluctuations and other macroeconomic shocks. As a result, even starting from the premise that minimum wages do reduce employment, it may be difficult for the average American to observe the aggregate economic costs of a minimum wage increase. For example, even the larger estimates of disemployment effects pale in comparison with movements in employment over the business cycle or with the monthly gross rates of job flows in the U.S. economy. Indeed, minimum wage proponents often argue that aggregate employment rose noticeably in a particular year despite an increase in the minimum wage that year, which may be true but does not speak to the effects of minimum wages. In addition, despite the research-based evidence of price passthrough in the fast-food restaurant industry, the effects of the minimum wage on the aggregate price level are small enough that they are far outweighed by fluctuations in prices for products such as gasoline and apparel.

In an environment where the effects of minimum wages are difficult to observe at the aggregate level, we might expect individuals instead to rely on their own experiences with respect to the minimum wage. But even at the individual level, there is a significant potential for incorrect inference. At any particular time, it is relatively easy to identify the beneficiaries of a minimum wage increase, and most affected workers will, in fact, see their earnings rise as a result of a higher minimum wage (i.e., there are likely more winners than losers). In contrast, much of the negative effect that minimum wages have on low-skilled employment may reflect a reduction in hiring rather than an increase in separations.[6] As a result, it is often difficult to explicitly identify those individuals who would have been employed in the absence of a minimum wage increase. In this regard, Brown notes that "an absence of evidence of widespread discharges following minimum wage increases had led some supporters of the minimum to doubt that it was causing any significant loss of employment," a view that he characterizes as a "logical error" (1988, 143).

A third misperception may stem from outdated views about the connection between the minimum wage and the poor. Burkhauser, Couch, and Glenn (1996) estimate that in 1939, following the introduction of the federal minimum wage the previous year, 85 percent of low-wage workers (defined as a worker whose wage was less than 50 percent of the average private sector wage) lived in a household with an income level that was below the poverty line. As a result, it seemed likely that most of the wage gains stemming directly from increases in the minimum wage at that time would flow to the poor. However, that relationship gradually weakened in subsequent decades, and, by the late 1970s, the percentage of low-wage workers who lived in poor households had fallen to just 20 percent. In large part, of course, the weakening of this link reflected the entrance of the baby boom into the teenage labor market and the sharp rise in labor force participation among adult women. But the basic point is that, contrary to public perceptions and to the beliefs of some economists, it is no longer the case that the beneficiaries of a minimum wage increase are disproportionately from poor households.

8.3 The Political Economy of Minimum Wages

One might expect that, with popular support of minimum wages consistently at such a high level, passage of minimum wage laws at the federal and state level would occur on a regular basis essentially without objection. Instead, vigorous debate on the merits of the minimum wage occurs nearly every time new legislation is introduced, and in some instances considerable periods of time have elapsed between increases in the nominal wage floor. Moreover, there are significant disparities in the level and the existence of minimum wages across states (and countries).

What accounts for this inconsistency? Waltman (2000) cites an array of statistics suggesting that while the vast majority of Americans support the minimum wage, most also do not feel strongly enough about it to actually change their voting behavior, perhaps because the minimum wage has little or no direct effect on their own economic circumstances, and at most slight indirect effects. Along the same lines, West (1974) argues that the group of individuals largely unaffected by the minimum wage constitutes the median group of voters, and thus differences across time and space in the success of proposed legislation must stem from another source.

As a result, some political scientists and economists have instead hypothesized that minimum wage policy has been driven by the efforts of organized constituent groups that support or oppose the minimum wage in their own self interest. That such groups could exert disproportionate pressures on legislators could stem both from their ability to organize large blocs of voters on particular issues and from their potential to provide substantial financial support in the form of campaign contributions. With regard to the types of groups that may have a particular interest in minimum wage policy, it has long been recognized that labor unions have an incentive to support the minimum wage because it shifts labor demand toward higher-skilled unionized workers (Tyler 1959).[7] Similarly, Simons (1944) points to the support for the FLSA from the northern textile industry as a way to reduce competition from southern states as evidence of such incentives. More recently, the National Restaurant Association and the National Federation of Independent Businesses, organizations that advocate for smaller firms employing significant numbers of low-wage workers, have routinely opposed minimum wage increases, while some larger firms, including Wal-Mart, have expressed support for a higher minimum wage.[8] More generally, Keech (1977) classifies potential constituent groups other than minimum wage workers into three broad categories: (1) workers earning above the minimum wage who support increases in it to reduce competition from low-wage workers; (2) employers of higher-wage workers who support increases in the minimum wage to reduce competition from low-wage competitors; and (3) employers of minimum wage workers who oppose an increase in the minimum.

A good example of the theory underlying the political economy of minimum wages is contained in Cox and Oaxaca (1982). These authors formalize the intuition about competing interest groups in the context of a general equilibrium model with three factors of production (unskilled labor, skilled labor, and capital) and a unionized and nonunionized sector of the economy whose products are gross substitutes in consumption. The unionized sector is assumed to be more skilled-labor-intensive than the nonunionized sector, and the wage rate of unionized skilled labor is assumed to be greater than the wage rate of nonunionized skilled labor by a fixed percentage. In the model, an increase in the nominal wage that raises the wage rate of unskilled labor not only induces substitution of skilled for unskilled labor, it also leads to substitution of unionized skilled labor for nonunionized skilled labor because the union sector expands and the nonunion sector contracts. In addition, the higher minimum wage reduces the real rental rate of capital. Thus, acting in their own self interest, labor unions have an incentive to support a minimum wage increase, as do owners and corporate executives in the unionized sector. In contrast, owners and corporate executives in the nonunion sector have an incentive to oppose minimum wage legislation.[9]

Because these different interest groups cannot directly set the minimum wage, they demand an intermediate product from their legislators in the form of political support for or against an increase in the minimum wage. Building on this framework, politicians can be viewed as utility maximizers who, regardless of their own personal views, respond in a predictable way to the influence of minimum wage legislation on the probability of their reelection. The equilibrium in such a model implies that legislators' support for the minimum wage will depend on both the preferences of their constituent groups and on the political power that each group offers in aiding their reelection.

Although support for the idea that labor unions and higher-wage employers have an economic incentive to support the minimum wage, and that employers of lower-wage workers have an incentive to oppose it, often comes from theoretical models or anecdotal information, there is also some indirect statistical evidence in support of this intuition. The evidence discussed in chapter 3 that points to disemployment effects on the lowest-skilled workers speaks to this to some extent, suggesting that firms that employ such workers are more adversely affected by increases in the minimum wage. Similarly, Sabia (2006a) finds that teenage job losses are especially large at small businesses and in the retail industry—segments of the market that tend to employ a higher percentage of low-wage workers.

In addition, a couple of papers include, as part of their analysis, an estimate of the effect of minimum wage increases on unionized workers. In particular, Linneman's study (1982) imputes the effect of the 1974 and 1975 increases in the minimum wage on the expected earnings of unionized workers by combining his estimates of the effects on hours and employment of these workers with the wage structure in those years. He claims that about 85 percent of union members would be expected to have higher earnings as a result of the minimum wage increases, with an average expected increase in annual earnings of more than $400. As noted in chapter 4, we have some concerns about the assumptions that underlie his estimates. Nevertheless, in an earlier version of Neumark, Schweitzer, and Wascher (2004), we estimated the effects of minimum wages on earnings of union and nonunion workers separately and found that union workers benefited, on average, from increases in the wage floor.[10] In particular, we found that the wage gains for low-wage union workers were more than twice as large as for their nonunion counterparts, and that total hours rose among low-wage union workers but fell for nonunion workers.[11] As a result, minimum wage increases tend to boost the total earnings of union workers, partly at the expense of the lowest-wage nonunion workers.

Finally, we would note that the strength of the overall economy may also influence the voting behavior of legislators with respect to minimum wage legislation. In particular, constituents without strong opinions about the minimum wage itself may care about the economy as a whole, and thus good economic performance is likely to be positively correlated with the probability of reelection (Kiewiet 1983). As a result, legislators who believe that minimum wages reduce employment (but who nonetheless prefer a higher minimum, or have other constituents who do) may be more inclined to vote in favor of an increase in the wage floor when the economy is strong (and unemployment is low) than when the economy is weak (and unemployment is high). Such considerations could result from economic conditions at either the national or state level.

There is also empirical research that directly assesses the determinants of political decisions about minimum wages. This research tends to focus on the linkages between the economic interests of constituent groups and voting patterns for federal legislation—in some cases studying the legislative events leading up to the initial passage of the FLSA in 1938 and in others focusing on subsequent amendments to the FLSA. A good example of this line of research is a study by Silberman and Durden (1976) on the determinants of legislators' votes for and against a bill passed by the House (HR 7935) in 1973 to increase the level of the minimum wage from $1.60 to $2.20 in two steps and to extend its coverage to about six million additional workers, including domestic workers and employees in federal, state, and local government.[12] These authors construct a variable for the level of political support for the minimum wage provided by each legislator based on two roll call votes associated with the proposed legislation: a vote for or against HR 7935 itself, and an earlier vote for or against a weaker substitute bill that had the support of the Republican administration. A vote for the substitute bill and against HR 7935 is defined as the maximum observed opposition to the minimum wage, and a vote against the substitute bill and for the House bill is defined as providing maximum support to the minimum wage; a vote for both bills is viewed as indicative of a moderately supportive stance on the minimum wage.[13]

Silberman and Durden then estimate a multivariate probit model for these voting patterns on a set of proxy variables designed to capture the preferences of particular special interest groups—including labor unions, high- and low-wage industries, small businesses, low-wage workers, and teenagers. The results indicate that labor unions exerted the largest influence on voting behavior, a result the authors viewed as "not surprising" given that they "felt intensely about passage of a strong minimum-wage bill and contributed significant amounts of campaign funds to representatives" (1976, 326). A regional indicator for southern states was also important, as was a variable measuring campaign contributions from small business organizations; these variables indicate opposition to the minimum wage from lower-wage firms in the South and from small businesses. The preferences of low-wage workers and teenagers appear to have been unimportant, suggesting that these groups either had less intense preferences about the minimum wage or less ability to influence legislators.

Kau and Rubin (1978) extended this line of analysis to examine six legislated changes in the minimum wage, ranging from the initial passage of the FSLA in 1938 to the amendment passed in 1974. These authors estimate a simple probit model for each legislator's final vote on the minimum wage legislation on the degree of unionization in the legislator's state, the percentage of black individuals in the state's population, the average manufacturing wage in the state, the party affiliation of the legislator, and a measure of each legislator's ideology. The basic results were fairly consistent across voting episodes and indicate that legislators from higher-wage states were relatively more likely to vote for the minimum wage increase, while those from states with larger black populations, who tend to be overrepresented among low-wage workers, were less likely to vote for it, perhaps because they recognized that it would adversely affect this group.[14] In contrast to Silberman and Durden, they find that the coefficient on unionization, while positive, was not statistically significant, although they note that the correlation between average hourly earnings and unionization rates was high in their sample. Finally, they also report a significant positive correlation between a legislator's liberalness and his or her tendency to vote for a minimum wage increase, suggesting that ideology, as well as pressure from interest groups, influences support for the minimum wage.[15]

In a short paper, Bloch (1993) examines votes by senators on the 1977 amendment to the FLSA as well as the vote in 1989 that resulted in the April 1990 and 1991 increases in the minimum wage; he also replicates his previous analyses for the 1966 and 1974 amendments.[16] In his specification, Bloch includes only two variables: the average wage in the state and the proportion of manufacturing workers in each state that are unionized. In all instances, the coefficient on the degree of unionization is positive and statistically significant. In contrast, the coefficient on the average wage changes sign and is never significant. Bloch also estimates his model on samples limited first to Democratic senators and then to Republican senators. He finds similar results for Republicans, but no effect of either unionization or average wages when the sample is limited to Democrats.

As Waltman and Pittman note (2002), one shortcoming of these studies is that they typically focus on the vote that led to the final passage of the relevant minimum wage legislation. By that time, however, the legislation has typically undergone significant changes, many of which have been made to attract the votes of lawmakers who were opposed to the initially proposed legislation. Seltzer (1995) also criticizes previous studies of the 1938 FLSA (e.g., Kau and Rubin 1978) on these grounds, noting in particular the convoluted path taken by the bill on the way to the final vote.[17]

To test these hypotheses, Seltzer models congressional voting behavior for three separate votes on the FLSA: the Senate's vote to pass the bill in July 1937; the vote by the House of Representatives to send the bill back to the Rules and Labor Committees in December 1937, after it had been brought to the floor; and the House vote to pass the bill in May 1938.[18] He includes a number of variables in the model to account for the presence of special interest groups at the state level, as well as variables designed to capture the party affiliation and ideology of the legislators.

In some respects, the results conform to the research described previously. In particular, southern legislators were relatively more likely to vote against the minimum wage on all three occasions, primarily because southern states were less unionized, more heavily agricultural, and had lower wages prior to the FLSA. That is, unions and higher-wage industries in the North were important sources of support for the minimum wage. More broadly, legislators in states with a greater percentage of employment in small businesses and that had a greater percentage of workers likely to be adversely affected by a higher minimum wage were more likely to vote against it. However, Seltzer also finds a role for ideology. In particular, controlling for constituent characteristics, Democrats and legislators who were more liberal were more likely to support the FLSA; Republicans and legislators who were more conservative were more likely to oppose it.

Krehbiel and Rivers (1988) use a similar approach to study two Senate votes leading up to the 1977 amendment to the FLSA, which boosted the minimum wage in four steps to \$3.35 per hour by 1983. The first vote they consider was a proposal to raise the minimum wage only 20 cents per year to \$2.90 in 1980, and the second was a proposal to raise the minimum to \$3.05 in 1980; both proposals were made in an effort to reach a compromise increase in the minimum that was below the level in the bill that had reached the Senate floor. In their estimation, which is based on both votes, they find that party affiliation, region, and the level of unionization in the senators' states had the largest effect on legislators' preferences. In particular, Democratic and Republican senators from northern states with high rates of unionization were more likely than other senators to prefer a higher minimum wage. Thus, depending on how one interprets the coefficient on unionization, these results provide evidence that both economic interests and political ideology influence minimum wage policy. Finally, in contrast to many other studies, they also include state-specific economic controls, but find them largely unimportant.

Poole and Rosenthal (1991) emphasize the role of ideology even more strongly. In particular, they raise concerns about identification in the economic models of voting behavior on specific issues, noting that it is often difficult to distinguish economic interests associated with the minimum wage from the broader interests of labor unions or particular regions. Instead, they propose a "spatial model" of voting behavior based on one or two liberal/conservative dimensions that they construct using a principal component analysis of every roll call vote from 1789 to 1985. Their two dimensions, which they loosely refer to as "pro-labor vs. pro-management" and "support vs. opposition to civil rights," are thus constructed mostly from votes on issues unrelated to the minimum wage.

These authors then show that their spatial ideology model consistently does a better job of predicting actual voting behavior on the 1973 and 1977 minimum wage legislation than did the economic models estimated by Krehbiel and Rivers, Bloch, or Silberman and Durden. Moreover, they argue that the largest residuals from their analysis do not appear to have a political economy interpretation, in that they were not obviously correlated with union membership or economic variables. Finally, they use the spatial approach to analyze the votes that led up to the legislation that raised the minimum wage in 1990 and 1991 and find that this model correctly predicted voting behavior about 90 percent of the time. Thus, although the authors do not entirely dismiss the hypothesis that economic interest groups play some role in determining minimum wage policy, they conclude that, by and large, the minimum wage debate is better characterized as a highly partisan battle waged between liberals and conservatives rather than as a response by different legislators to their particular constituent groups.

In a similar vein, Bartels (2006) points out that despite the overwhelming popularity of minimum wages, the real value of the federal minimum stands well below its peak in the late 1960s, largely because of opposition from conservative Republicans. In related regression analyses, he finds that a Democratic president and greater Democratic strength in Congress are associated with a higher real value of the minimum wage, and that Democratic senators were more likely to vote for the 1989 minimum wage legislation even after controlling for constituency opinion on the issue. He then contrasts this with the EITC, which was significantly expanded by Congress over the 1990s despite lukewarm support by the public.

Sobel (1999) takes a different approach and asks whether Congress has historically set the minimum wage at a level consistent with the stated goals of that policy. First, he compares the level of the minimum wage over time with the level that would be needed to lift a minimum-wage worker's family out of poverty. Second, he examines whether the minimum wage has been set at a level designed to maximize the transfer of earnings to minimum-wage workers. His estimates for the amount needed to push family income above the poverty threshold are fairly straightforward, as they are simply based on assumptions about family size and the number of earners in the family. In particular, he calculates that the required minimum wage would range from $3.89 per hour (1996 dollars) for a four-person family with two earners to $6.07 per hour for a single earner with two children; the average value from the various combinations that he considers is $5.17 per hour. His estimates of the minimum wage consistent with maximizing the earnings of minimum-wage workers are calculated from a time-series regression of total earnings of minimum wage workers on the real minimum wage and its square, and some control variables.[19] The intuition is that a higher wage floor will raise total income of minimum-wage workers along the inelastic portion of the labor demand curve and then reduce total income once the elasticity rises above unity. His point estimate of the optimal minimum wage is $5.39 per hour, which is toward the lower end of the poverty-threshold target band.

Sobel then compares the real value of the minimum wage to these implicit targets over time and finds that nearly half of the time, the minimum wage was outside of a two-standard-error band around the targets. He interprets these results as suggesting that the minimum wage is not set to meet some overarching policy goals, but rather that its level is importantly influenced by interest group pressure. To test this hypothesis, he regresses the average change in the real minimum wage over ten-year periods against a measure of the relative power of labor unions and business groups: the ratio of union membership as a share of employment to one minus the top corporate income tax rate. A high value of this ratio is taken to be a signal of greater union strength and a low value is taken to be a signal of greater business strength. He finds a positive and significant relationship between changes in the real minimum wage and this ratio, consistent with the hypothesis that greater union strength relative to the political power of business groups is associated with a rising minimum wage in real terms.

Although we are sympathetic to the possibility that economic interest groups influence minimum wage policy, we do not see how Sobel's analysis adds much to the debate. One obvious problem is with the construction of the target wage floors. Sobel's calculation of the poverty-threshold minimum wage ignores potential disemployment effects, which as we discussed in chapter 5, tend to offset the benefits of a higher minimum wage on poor households. Similarly, his estimates of the minimum wage needed to maximize the earnings of minimum wage workers contrasts with the evidence on this issue that we summarized in chapter 4, probably because he uses all teenagers to proxy for minimum-wage workers and thus likely understates the disemployment (and negative hours) effect for the lowest-wage workers. In particular, in Neumark, Schweitzer, and Wascher (2004) we found that minimum wage increases over the 1979–1997 period tended to reduce the earnings of minimum wage workers; in contrast, Sobel estimates the minimum wage to be below its target level for maximizing earnings during that period. Finally, the likelihood that legislators had different views about these parameters complicates Sobel's interpretation of the deviations of the actual minimum wage from his constructed targets, and thus we question whether that portion of his analysis is particularly relevant for understanding the political economy of minimum wages.

A few other researchers have examined hypotheses regarding the political economy of minimum wages by looking at the relationship between variation in policy and the relative power of competing interest groups across states, rather than studying the determinants of voting on federal minimum wage legislation. For example, Cox and Oaxaca (1982) specify a model that relates the value of the state minimum wage (both above and below the federal minimum) to two variables: the ratio of the number of union members in the state to nonagricultural employment, and the ratio of capital income in the state (measured either as rents, dividends, and interest income, or as proprietors' income) to total state personal income. In addition, they include the average manufacturing wage in the state in their specification on the argument that legislators will frequently target either the real value of the minimum wage or the value of the minimum wage relative to prevailing wages in the state.

Because some states have no minimum wage, the authors estimate this model using a Tobit specification for state cross-sections in 1970 and 1975. The results for both years are quite similar and indicate that a larger union presence raises the expected value of the minimum wage in the state, whereas states with a higher capital income share tend to have lower minimum wages. The authors also estimate the effects of these variables on the probability of a state having a minimum wage at all, and find that states with a larger share of unionized workers are more likely to have a minimum wage, and that states with a higher capital income share are less likely to have a minimum wage. They interpret these results as supportive of their hypothesis that the relative strength of groups with competing economic self-interests is an important determinant of minimum wage policy.

Given that most state minimum wages were below the federal level in the samples used by Cox and Oaxaca, it is natural to ask whether the levels of state minimum wages at that time were of much consequence to firms and workers and thus whether the results are informative about the authors' hypotheses.[20] The authors note, however, that only about 60 percent of the workforce was covered by the federal minimum wage in 1970, while state-only coverage rates averaged 36 percent, suggesting that state laws were likely a relevant factor affecting low-skilled labor costs in many states. Moreover, in a companion paper (Cox and Oaxaca 1981b), the authors estimate similar models for the percentage of workers in a state covered only by the state minimum wage, as well as for an "effective" minimum wage variable defined as the product of the state level and state coverage rate.[21] The results from these models indicate that the level of coverage is not as responsive as the level of the minimum wage to the relative interests of organized labor and business, but the direction of the effects are the same and most of the estimates are statistically significant. However, in neither study do they attempt to assess the extent to which differences in economic conditions or ideology across states might influence state minimum wage laws, and thus it is difficult to compare their results to the studies of voting behavior.

Three more recent studies also attempt to take advantage of the variation in state minimum wage levels. First, Besley and Case (1995) estimate a model that relates the real value of each state's minimum wage from 1950 to 1986 to state real income per capita, the party affiliation of that state's governor, and a variable indicating whether the incumbent could stand for reelection. They find no evidence that the average level of income influenced the minimum wage, but they do find evidence of a link between the minimum wage and their political variables. Most important, they report that Republican governors who cannot stand for reelection are much more likely to allow the state minimum wage to fall in real terms. They interpret this result, in conjunction with similar results for other state-specific economic policies, as suggesting that term limits can have an important effect on economic policy at the state level by reducing the incentives for incumbents to respond to pressures from special interest groups.

Second, Zavodny (1996) includes both economic and political variables in a model for state-specific changes in the minimum wage from 1979 to 1993. Her dependent variable is an indicator variable set to one if the state's minimum wage was above the federal level, and zero otherwise; she regresses this variable on state-specific total and teenage unemployment rates, the average real manufacturing wage in the state, and political variables representing the proportion of Democrats from the state in the two houses of Congress and the political party of the governor. Her results indicate that states were more likely to raise their minimum wage above the federal level when economic conditions were favorable but that the effect of political affiliation was small. However, the analysis did not explicitly allow for an influence from particular constituent groups, and thus the evidence does not really speak to that question.[22]

Zavodny also examines voting behavior on a 1988 ballot initiative in Washington to raise the minimum wage to $3.80 in 1989 and to $4.25 in 1990.[23] In this analysis, she regresses county-specific percentages of votes in favor of the initiative on a variety of county-specific economic and political ideology indicators. Similar to her earlier results, the estimates from this regression suggest that economic factors are important determinants of voting behavior, with higher-income, nonrural counties, and rapidly growing counties all voting more strongly in favor of the minimum wage increase. In addition, she found that, controlling for economic factors, counties that exhibited support for Democratic candidates also voted more heavily in favor of the ballot initiative.

Third, Waltman and Pittman (2002) use data from 1998 to examine whether state minimum wages are related mostly to societal wealth in a state (consistent with the idea that a more affluent population is more willing to support a welfare state), the political power of the Democratic party, or the relative liberalism of a state's population. In particular, they group states into four categories based on their minimum wage in 1998: states with no minimum wage, states with a minimum wage below the federal level, states with a minimum wage at the federal level, and states with a minimum wage higher than the federal level. Using non-parametric methods, they find that the most significant determinant of state minimum wage rates was the relative liberalism of its residents, although wealth (measured as median household income) also played a role. In contrast, there was essentially no relationship between the strength of the Democratic party and minimum wage rates. Because they view the political power of the Democratic party as a proxy for special interests that would support the minimum wage (especially unions), they interpret these results as evidence against the special interests model. As discussed previously, however, some businesses also have an incentive to oppose minimum wage increases, and thus Waltman and Pittman's conclusions would seem to be based on an overly restrictive version of the political economy model.

Finally, a few researchers have conducted cross-jurisdictional studies of minimum wage determination in Canada, which naturally lends itself to this type of analysis because the minimum wage is determined solely at the provincial level. For example, Blais, Cousineau, and McRoberts (1989) employ a cross-section time-series study of minimum wages in nine Canadian provinces from 1975 to 1982.[24] They use the minimum wage relative to the manufacturing wage as their dependent variable, on the argument that interest groups and legislators are likely targeting an unskilled wage relative to a skilled wage level. They include a variety of explanatory variables to capture the influences of economic factors, special interest groups, and political leanings of the provinces; in addition, they include a "convergence" variable that measures the ratio of a province's average wage to the national average wage on the notion that a tendency for provinces to adopt fairly similar nominal minimum wage rates will cause the relative minimum wage variable to be higher in lower-wage provinces. Their results indicate that provinces with larger percentages of small businesses, women, and youths—groups more likely to be harmed by the minimum wage—tended to have lower minimum wages, suggesting that legislators in those states recognize that minimum wages may harm such groups. In addition, provinces in which the government was formed by the Conservative Party tended to have lower minimum wages. In contrast to the findings of many U.S. studies, the results do not indicate any effect on minimum wage levels from the degree of unionization. However, regional economic conditions were important, with higher rates of unemployment and inflation associated with lower minimum wages.

Dickson and Myatt (2002) update this study to 1996 and improve upon it. For one thing, they add fixed effects for both year and province, which were excluded in the Blais, Cousineau, and McRoberts study. In addition, the authors augment the list of explanatory variables to include a larger number of ideological measures, a variable intended to capture the political power of large business, and a variable that measures differences in the level of unemployment insurance benefits across provinces and time. Many of their results have unexpected (to them) signs, although some are quite similar to what Blais, Cousineau, and McRoberts found. In particular, a greater percentage of women or youth was associated with a lower minimum wage, as was a higher unemployment rate. Also similar to the earlier study, Dickson and Myatt find no effect on minimum wages from unionization. On the other hand, both of the business interest variables have a positive coefficient. In addition, they find that ideology matters, with a negative influence when the conservative Social Credit Party was in power and a small positive influence when the prolabor New Democratic Party was in power.

A third paper that focuses on Canada is by St-Arnaud (2005). In many respects, this study is similar to those just discussed except that it updates the dataset to 2003. However, there are a couple of important differences. First, St-Arnaud criticizes the use of a relative minimum wage variable on the grounds that the denominator in that variable (the average manufacturing wage) is correlated with many of the other covariates. For example, he demonstrates that unionization has a strong positive correlation with manufacturing wages, which could potentially mask the effect of labor unions on legislated minimum wage increases, and thus he instead suggests using the real value of the minimum wage as the dependent variable. Second, he is much more careful to explore the appropriate econometric specification of the model. In the end, however, the results are broadly similar to those reported in the earlier studies. Conservative governments are less likely to implement a higher minimum wage, and legislators are less likely to increase the minimum wage when the unemployment rate is high. Similarly, St-Arnaud finds no evidence that union density leads to higher minimum wages, in contrast to much of the U.S. literature.

8.4 Living Wages and Their Political Economy

A more recent development in minimum wages has been the rapid proliferation of living wage laws at the city or local level. Indeed, the political success of the movement to establish local living wages has been phenomenal. The first living wage law was passed in Baltimore in 1994.[25] Since then, living wages have spread quickly throughout the country, and there are now at least 140 living wages on the books in local jurisdictions in the United States. Although some living wage laws have been implemented in smaller cities, they have also become prevalent in large cities. In fact, seven of the ten largest cities in the United States in terms of 2005 population have living wage laws on the books.[26] Moreover, Brenner (2005) estimated that 40 percent of the population of cities with over 100,000 residents lived in cities with a living wage law as of 2003—a number that has since climbed higher. The political economy of minimum wages is likely informative about the recent proliferation of living wage legislation enacted at the local government level. However, before discussing the political economy of living wages, we first need to provide an overview of living wage laws and the research on their effects.

8.4.1 Living Wages

Living wages have three central features that distinguish them from minimum wages. First, they frequently impose a wage floor that is much higher than the typical level of state and federal minimum wages. For example, column (1) of table 8.1 lists living wage levels for the seven largest cities where they have been implemented. Clearly, many of these are considerably higher than the minimum wage in the respective states, as shown in column (2), and there are numerous other cities with similarly high (albeit diverse) living wages. Second, living wage levels are often explicitly pegged to the wage level needed for a family with one full-time, year-round worker to reach the federal poverty line, although cities define the reference family (one or two adults) differently.[27] As a consequence, most living wages are indexed, in contrast to the federal minimum wage and most state minimum wages. The third feature of living wage laws is their much narrower

Table 8.1
Living wage laws among the ten largest cities, as of 2006

	Level	Prevailing minimum wage	Coverage
	(1)	(2)	(3)
New York	$10.00	$5.15	Service contractors
Los Angeles	$9.39	$6.75	Service contractors, financial assistance recipients
Chicago	$10.00	$6.50	For-profit contractors in specific categories
Philadelphia	150 percent of higher of federal or state minimum wage	$5.15	Contractors, businesses with city leases/ franchises/concessions, city employees
San Diego	$10.00	$6.75	Contractors, financial assistance recipients
San Antonio	70 percent of employees in new jobs: $11.14 (services involving durable goods); $10.86 (services involving nondurable goods); minimum for all workers is $9.62	$5.15	Financial assistance recipients (tax abatements)
San Jose	$12.27	$6.75	Service contractors in specific categories, financial assistance recipients

Source: Various sources listed in chapter.
Note: In most cases, the required wage level is higher if health insurance benefits are not provided. The living wage if such benefits are provided is reported. The prevailing minimum wage is the higher of the state or federal minimum. The three cities among the ten largest that do not have living wage laws are Dallas, Houston, and Phoenix.

coverage. As shown in column (3) of table 8.1—and as is true more generally of living wage laws[28]—nearly all living wage laws cover city contractors, while about half also cover companies that receive financial assistance from cities, such as subsidies and tax abatements (and a handful extend to tenants and contractors of the companies receiving assistance, such as retail occupants of a mall that received development subsidies). In contrast, living wages rarely apply to city employees, perhaps in part because of their relatively high unionization rates and wage levels.[29] Most important, though, living wage laws generally are *not* broad wage floors covering large swaths of the private sector. Coverage estimates are very hard to come by, especially for living wage laws that cover financial assistance recipients, for which city-level information is typically decentralized. But estimates of coverage by city contractor provisions are typically below 1 to 2 percent, although in some cities, coverage is higher because of how contractor coverage is specified.[30]

The most recent development with regard to living wages has been the advent of city-level minimum wages—that is, broad minimum wage floors that are similar to state minimum wages, but applied at the city level. Santa Fe and San Francisco enacted a minimum of $8.50 in 2003 and 2004, respectively, with both set to rise through indexation and (in Santa Fe) planned increases in the legislation. Madison and other smaller towns in Wisconsin also recently passed minimum wage laws, but they were subsequently repealed by the state legislature. A city minimum wage in New Orleans was approved by voters in 2002, but subsequently blocked by a state law.[31]

8.4.2 The Effects of Living Wage Laws

Some of the expected effects of living wages are no different from the expected effects of minimum wages. In particular, wages should rise for affected workers, but the competitive model predicts these workers will also suffer some employment losses. With regard to effects on the distribution of family incomes, there is no clear prediction; as for minimum wages, these effects will depend on the distribution across families of workers who gain and lose from living wage laws. At the same time, we would expect to see quantitative differences in effects for a number of reasons, including: the higher wage floors established by living wages; lower coverage; different elasticities of demand for the goods and services that local governments purchase; the possibility that cities allow contractors to pass through the higher costs mandated by living wage laws; and—from the perspective of effects on the income distribution—the likelihood that living wages impact different workers than do minimum wages. A good deal of research using data through about 2002 was reviewed in Adams and Neumark (2004). That research is summarized here, as are some findings from more recent research.[32]

A number of the studies by Adams and Neumark (referenced in the following discussion) attempt to estimate the effects of living wage laws using CPS data from the mid-1990s to 2001 or 2002. Because it is not possible with these data to identify workers directly covered by living wage laws, the authors have to estimate the effects for a broader group that includes uncovered workers. The outcomes examined are wages and employment at the individual level, and poverty at the family level. In all cases, the estimates of the effects of living wages are estimated from specifications that include the log of the minimum wage and the maximum of the log of the minimum wage and the log of the living wage in each metropolitan area. This specification imposes the minimum wage as the floor in the absence of a living wage, but allows a wage floor enacted through a living wage to have a different effect from one enacted through a minimum wage. The specifications also include fixed city and time effects, and allow for a different linear trend in cities that passed a living wage law and those that did not.[33] In addition, the specifications allow for the possibility of lagged effects of living wages and minimum wages, consistent with the evidence on minimum wages in chapter 3; indeed, the lagged effects (at one year), which are reported in the tables that follow, tend to be stronger than the contemporaneous effects. Finally, for the wage and employment effects, the sample is restricted to the bottom decile of the wage distribution for the wage specifications, and to the bottom decile of the predicted wage distribution for the employment specifications, in order to focus on the lowest-skilled workers most likely to be covered or indirectly affected by living wages.[34]

The results from this research are summarized in table 8.2. As reported in the first row of column (1), when living wage laws are treated as homogeneous (with reference only to the wage floor), the estimated elasticity of wages with respect to the living wage is 0.04, which is not statistically significant at the 10 percent level. However, the results differ between contractor-only living wage laws and financial assistance living wage laws (which in almost every case are broader, as contractors are also covered), with considerably sharper

Table 8.2
Estimated effects of living wage laws

Dependent variable	Log wages, lowest decile of wage distribution (elasticity)	Employment, lowest decile of predicted wage distribution	Probability that family income below poverty
	(1)	(2)	(3)
All living wage laws:			
Log living wage, lagged twelve months	0.040	−0.053*	−0.035*
Financial assistance living wage laws:			
Log living wage, lagged twelve months	0.067^{+}	−0.076*	−0.024^{+}
Contractor-only living wage laws:			
Log living wage, lagged twelve months	−0.006	−0.027	−0.038
N	46,374	116,466	142,421

Source: Adams and Neumark (2004).
Note: The data on labor market outcomes and other worker-related characteristics come from the CPS monthly ORG files from January 1996 through December 2002, and the CPS Annual Demographic Files (ADFs) from 1996 through 2002, for individuals or families residing in MSAs, in city-month cells with twenty-five or more observations. The data for the first two columns cover 1996–2002, and for the last column cover 1995–2001. The regressions include controls for city, year, month, minimum wages, and other individual-level controls in the wage and employment specifications, and controls for city, year, and minimum wages in the poverty specification. All specifications also allow differential linear time trends for cities passing or not passing living wage laws, or passing different types of laws. The entries in the first row are from a specification with a single living wage variable, and the entries in the second and third rows are from a specification interacting the living wage variable with dummy variables for the type of living wage. The coefficients for the log wage equation are from log-log specifications, and hence are elasticities. The coefficients from the employment and poverty regressions measure the change in the share employed or poor in response to a one-unit increase in the log living wage (or a 100 percent increase). $^{+}$ and * indicate that the estimate is statistically significant at the 10 percent or 5 percent level; calculated standard errors are robust to heteroskedasticity and non-independence within city cells.

effects for the living wage laws with financial assistance provisions.[35] As reported in the remaining two rows, the evidence points to a significant impact on wages from financial assistance laws, with an estimated elasticity of 0.067 that is significant at the 10 percent level. In contrast, the estimated impact of contractor-only laws is small and statistically insignificant.

Other research on living wages also tends to find evidence of positive effects on the wages of potentially affected workers, including Reich, Hall, and Jacobs's (2005) study of the effect of a living wage program at San Francisco International Airport, Brenner's (2005) study of the Boston living wage law, and Fairris's (2005) study of the Los Angeles living wage law. These studies are distinguished from the CPS analyses by their focus on workers and employers who were actually covered by a living wage law. The disadvantage of CPS-type analyses is that the number of workers directly affected by living wage laws is quite small. As such, the CPS is not well equipped for studying the microeconomics of how firms respond to living wages. On the other hand, the CPS is better suited to asking whether living wages achieve the policy goal of helping low-wage workers and low-income families, because they can capture the net or general equilibrium effects of these laws.[36]

Not surprisingly, perhaps, there is more controversy regarding the employment effects of living wage laws. The CPS results from the research by Adams and Neumark are reported in column (2) of table 8.2. The authors use the same basic framework as described earlier for the estimation of wage effects, but estimate linear probability models for individual employment status for those in the bottom decile of the predicted wage distribution. As shown in the first row, for living wages generally there is an estimated disemployment effect that is significant at the 5 percent level; the estimated coefficient of −0.053 implies an elasticity of −0.12. When separate models are estimated for financial assistance and contractor-only living wage laws, both estimates are negative, but the coefficient is significant only for financial-assistance living wage laws, with an elasticity of −0.17.[37]

The Reich, Hall, and Jacobs (2005) study of San Francisco International Airport (SFO) concludes that employment did not fall as a result of living wage policies implemented at the airport. Their estimates of employment (which come from two different sources) indicate employment gains of about 1,150 employees from 1998 through 2001—from about 7,350 to 8,500.[38] However, there is no control group in this study,[39] and, as the authors point out, a potentially important confounding factor was the opening of a new international terminal during this period, in the fall of 2000. Indeed, the airport had originally projected an employment increase of 11,000 associated with the new terminal, and while this projection may have been overly optimistic, the new terminal would almost surely have generated a substantial number of new jobs. At the same time, air travel declined during 2001 because of the recession (and not the September 11 terrorist attacks, which occurred after the survey date), which would tend to bias the employment change in the opposite direction. However, the study presents evidence suggesting that the recession was not a major factor, noting that passenger volume in 2001 fell sharply at SFO, but not at San Jose's airport, even though the economic downturn was worse in San Jose and San Jose's living wage does not apply to its airport workers. Nonetheless, these other influences and the absence of a control group lead us to view this study as failing to provide convincing evidence on overall employment effects.[40]

Brenner (2005) studies the effects of the introduction of a living wage in Boston in 1998 on firms that had contracts with the city, using survey data collected in 2001 and covering 1998 to 2001. He does not have noncontractor firms to use as a control group, but instead distinguishes between those firms reporting that they raised wages to comply with the living wage law (the treatment group) and those that did not so indicate (the control group, for which presumably wages were already in compliance); this classification is confirmed by the wage results. The data indicate that employment grew in both the treatment and control groups from 1998 to 2001, but grew faster in the control group, so that the difference-in-differences estimate points to a decline in relative employment in the affected firms. (The decline is not statistically significant in levels; the statistical test for percentage changes is not reported.) Of course, these employment figures refer only to the overall workforce of these contractors, and thus do not necessarily capture the effect on workers directly employed on city contracts; employment change among those workers was not studied.[41] The relative employment decline implied by the estimates is 6 percent (i.e., employment grew by 11.2 percent in the treatment group, and 17.2 percent in the control group). Over this same period, the percentage deviation between the living wage and the minimum wage increased from 0 to 35 percent. Thus, the implied employment elasticity is -0.17.

Brenner also estimates the model for FTE employment, and finds no measurable effect. This suggests that the living wage law led to a negative effect on the proportion of the workforce comprised of part-time workers. An apparent shift away from part-time and toward full-time work might reflect fixed costs of employment on which employers economize in response to higher wage costs. Overall, then, this study provides evidence of disemployment effects, although not necessarily an overall reduction in labor demand.

Fairris (2005) studies the effects of the Los Angeles living wage law enacted in 1997. He bases his analysis on two samples: a survey of contractor establishments affected by the Los Angeles living wage, and a second sample of establishments that was collected for a quite different purpose, which he uses as a control group. However, there are some important differences between the two samples, including how they were collected and response rates. In addition, the survey design in the contractor sample does not permit direct measurement of employment changes; instead, employers in the contractor sample were asked whether staffing levels declined as a result of the living wage ordinance.

Based on comparisons with the control sample, there is evidence that wages of low-wage workers were increased about 25 percent as a result of the living wage. For employment, Fairris finds that 18 percent of covered establishments reported declines stemming from the living wage law, and he estimates an overall employment decline among contractor firms of 1.6 percent, and 2.6 percent among affected workers.[42] Given his estimated wage effect, Fairris suggests that the estimates imply "an elasticity of low-wage worker demand of roughly -0.10" (2005, 20). Finally, Fairris also finds evidence of a decline in turnover relative to the control group, and a follow-up study (Fairris and Bujanda 2006) reports rather strong evidence of substitution toward more-skilled workers. Thus, this study tends to confirm the predictions of the competitive model.

These latter three studies focus on contractor-only laws, whereas only the Adams and Neumark studies examine the effects of living wage laws that extend to recipients of financial assistance. A natural question with regard to the Adams and Neumark studies is why the evidence points to much stronger effects of financial assistance living wage laws. One possibility is the broader coverage of financial assistance laws, although, as noted earlier, the extent of this greater coverage is difficult to measure.[43] Another possibility relates to the tendency for contractor-only laws to require that employers pay the mandated wage only for work done as part of the contract. Assuming that contractors' employees do some other work in addition to city contracts, employers can mitigate the costs of complying with living wage laws by shifting higher-skilled or higher-seniority (and therefore higher-wage) labor to the contract work and lower-wage labor to the noncontract work, or they can reduce wages on noncontract work. In contrast, employers covered under financial assistance provisions may have fewer avenues for mitigating the costs (and therefore the effects) of living wages. For example, an establishment created with the help of financial assistance from a city would appear to have no choice but to pay all employees no less than the mandated living wage for all of their work. Finally, although contractors can reduce the share of their business with the city in response to a living wage, financial assistance recipients may have less leeway if they have accepted long-term benefits, such as bond financing or tax relief, in return for locating in the jurisdiction. However, this is conjecture, and the development of a better understanding of the workings of different types of living wage laws and their provisions awaits further research.

The evidence on wage and employment effects sets the stage for examining the effects of living wages on poverty. Results from the CPS analyses are reported in column (3) of table 8.2. Here, the specification is similar to that for the other results reported in this table, but the model is estimated for families, with the dependent variable a dummy variable equal to one if a family's income falls below the federal government's threshold for poverty, and zero otherwise. The evidence yields negative point estimates (implying poverty reductions) for both types of living wage laws, but only the estimated effect of financial assistance living wage laws is statistically significant (at the 10 percent level). For financial assistance living wage laws, the estimated coefficient of −0.024 implies that a one log unit (100 percent) increase in the living wage reduces the poverty rate by 2.4 percent.[44] Relative to an 18.6 percent poverty rate, this represents a 12 percent reduction, or an elasticity of −0.12. This seems like a large effect, given that the wage elasticity for low-wage workers is below 0.1. Of course, the claim is not that living wages can lift families from well below the poverty line to well above it. But living wages may help nudge families over the poverty line, and these average wage effects may reflect much larger gains for a small number of families.[45]

Additional evidence suggests that living wages also deliver some gains for families a bit below and a bit above the poverty threshold. For example, living wages—particularly the financial assistance variety—reduce the probability that families' incomes are below three-quarters of the poverty threshold. And, the estimated effects are in the same direction and larger (and also significant) at 1.5 times the poverty threshold. On the other hand, living wages do not appear to help the poorest families, which is not entirely surprising because such families are less likely to have any workers or, if they do, are more likely to have a worker displaced by an increase in the living wage.

In contrast to the neutral or adverse distributional effects of minimum wages discussed in chapter 5, the distributional effects from living wage laws appear to be beneficial for the poor. This difference should not be viewed as contradictory, however. Although neoclassical economic theory predicts that raising mandated wage floors will lead to employment reductions—and the evidence from both minimum wages and living wages is largely consistent with this prediction—recall that the theory makes no predictions regarding the effects of mandated wage floors on the distribution of family incomes, or on poverty specifically. The distributional effects depend both on the magnitudes of the wage and employment effects (and other effects), and on their incidence throughout the family income distribution. As a consequence, the difference between the distributional effects of minimum wages and living wages suggests that the gains and losses from living wages are of different magnitudes and fall at different points in the distribution of family incomes than do the gains and losses from minimum wages, presumably reflecting differences in the types of workers who are affected by these alternative mandated wage floors.

In fact, there do appear to be some significant differences in the populations of affected workers. Fairris et al. (2005) report descriptive statistics for workers directly affected by the living wage, based on their survey in Los Angeles, and show that 4 percent were teenagers (their table 3.1). We extracted CPS data for the same years (2002–2003) and found that teenagers represented 4.2 percent of the overall workforce in Los Angeles, very similar to their living wage sample. However, when we restrict the sample to minimum wage workers, the share of teenagers is much higher. For example, among those earning between $5.15 and $7.25 (the state minimum wage was $6.75, and the federal minimum was $5.15), 14.9 percent were teenagers; similarly, 14.1 percent of those earning exactly $6.75 were teenagers. Thus, these data suggest that workers affected by the living wage are more likely to be adults than are workers affected by the minimum wage, which may help to explain why living wages have more beneficial distributional effects than do minimum wages. In particular, a greater proportion of the benefits of living wage laws (and perhaps fewer of the costs) appear to accrue to adult earners than is the case for minimum wages. It would clearly be useful, however, to better understand how the different distributional effects arise.

8.4.3 The Political Economy of Living Wages

The authors of a prominent book advocate municipal living wages as building "a lasting foundation for social and economic justice in this country" (Pollin and Luce 1998, 193). And living wage campaigns have often involved religious communities pursuing these same goals, as documented by Figart (2001) and exemplified by the "Let Justice Roll Living Wage Campaign."[46] We see no reason to doubt that, similar to the ideological support for the minimum wage, part of the motivation of those advocating for living wages stems from a concern with "economic justice" and a belief that living wages will help low-wage workers and low-income families.

More unique to living wage campaigns is the perspective that they are a potential catalyst for increasing cooperation between community groups and organized labor on behalf of other economic justice issues, and serve to increase the political strength of organized labor (Levi, Olson, and Steinman 2002; Reynolds 2004).[47] Nissen (2004b) provides some evidence on the extent to which living wage campaigns can lead to the creation and growth of coalitions between labor unions and community groups, whether these coalitions have taken on a broader progressive agenda, and if these coalitions have strengthened organized labor. Based on a case study from southern Florida, he argues that living wage campaigns have spurred labor-community coalition building, but that the success of these coalitions in bolstering unions or harnessing union efforts on behalf of issues of broader interest has been mixed at best. Reynolds (2004) provides a more upbeat view of the contribution of these coalitions to activism along other fronts related to low-wage work. On a more concrete level, Reynolds and Kern (2001–2002) and others note that some living wage laws include provisions meant to encourage unionization or to deter anti-union efforts, although this is typically not their main thrust.

A political economy perspective on living wages asks how economic motivations of the various actors in living wage campaigns shape their behavior, as reflected in the earlier discussion of the political economy of minimum wages. This perspective does not dismiss the role played by groups acting out of concerns about economic justice with respect to inequality, poverty, and so on (irrespective of whether living wages achieve these goals), or pursuing other political goals. In the case of the living wage movement, it would take an extreme cynic to tell a story whereby all of the groups involved are acting solely on behalf of their own economic interests. But while extreme cynicism is probably inappropriate, healthy skepticism is not, and so a political economy perspective that considers economic motivations *as well* may help to explain the advent and growth of living wages, and some of the features of living wage laws.

Freeman considers these issues from the perspective of organizations agitating for living wages. He terms these "nonworker organizations" (NWOs), and views them as working on behalf of *other* people's wages, in contrast to the traditional union model of organizing and bargaining on behalf of the wages of the members of an organization. Freeman attributes the growth of NWOs pursuing these goals to the declining rate of unionization in the U.S. economy, arguing that NWOs are "filling the gap left by the absence of unions by bringing community pressures on employers to grant workers some of the economic benefits and rights that unions would have won for them" (2005, 18). It would, however, be difficult to establish this causal link, and Freeman presents no such evidence.

Freeman considers more fully two specific questions regarding living wage campaigns. First, as suggested by the title of his paper, why do people choose to fight "for other folks' wages"? He considers whether, particularly for students, working on living wage campaigns represents human capital investment in marketing, organizational change, and so on. However, he rejects this view based on evidence reported in Elliott and Freeman 2003 regarding the self-reported motivation of activists in a different but related realm—antisweatshop campaigns—and concludes that the motivations are non-pecuniary, "doing what they view as morally right" (Freeman 2005, 19).

Second, why have the NWOs chosen to focus on wages at the local level for narrow sets of workers? This question goes less to the matter of why living wage campaigns have arisen than to the form they have taken. One argument is that, conditional on wanting to help improve living standards of some workers somewhere, living wages are appealing because by tailoring them to local labor market and political conditions, there is a greater chance for success. On the surface, this is consistent with the substantial variation in the levels of living wages that have been adopted across the country, and with a tendency to target employers that may be earning relatively large economic rents (such as Harvard University, or hotels and restaurants within the coastal zone of Santa Monica, California),[48] although this conjecture could bear more systematic empirical scrutiny. Similarly, Freeman suggests that city contractors may be targeted because their labor demand is relatively inelastic, implying larger earnings gains and smaller employment losses, although the available empirical evidence does not appear to support this supposition. At the same time, Freeman notes that this local and narrow focus of living wage campaigns necessarily limits their effects to small numbers of workers. It is the contrast between the motivations for living wages and the limitations of local, narrow efforts that constitute, in his view, the "logic and illogic" of living wage campaigns.

One way out of this apparent conundrum is to understand living wage campaigns and their local emphasis as initial efforts that are intended to be "scaled up." This explanation is consistent with the perspective that living wage campaigns seek to build capacity for community organizing along similar lines. In addition, local efforts may be the place to start because it may be easier for small groups to influence local policy, and the local successes may provide a foundation for broader efforts. Indeed, Pollin and Luce suggest that as living wage laws take hold at the municipal level, the "process of political and economic education will then provide a stronghold on which to launch more ambitious programs of egalitarian wage and employment policies" (1998, 191).

Is this scaling up likely to occur? Neumark (2004) considered the view that local living wages provided a "foot-in-the-door" from which to broaden such policies, but discounted it based on the fact that narrow contractor-only living wage laws had generally not expanded into broader laws (incorporating financial assistance provisions, or turning into broader local minimum wages). On the other hand, recent events are more consistent with this hypothesis, as municipal *minimum* wages have been passed in a handful of cities and there has been a strong diffusion of state minimum wage laws in the last few years (although continuing inaction on the federal minimum prior to the Democratic victories in the 2006 congressional elections can also explain the growth in city and state minimum wages).

A more conventional political economy analysis looks at the motivations of particularly powerful groups likely to gain from living wage laws. One hypothesis explored in Neumark (2004) is that municipal unions work to pass living wage laws as a form of rent-seeking. Specifically, by forcing up the wage for contractor labor, municipal unions reduce or eliminate the incentives of cities to contract out work done by their members, and in so doing increase the bargaining power of these unions and raise wages of their members. There is ample indirect evidence consistent with this, as municipal unions are strong supporters of living wage campaigns; for example, the American Federation of State, County, and Municipal Employees (AFSCME) was one of the major organizers of the Baltimore living wage campaign (Osterman et al. 2001; Fine 2000–2001). Rent-seeking behavior could also help to explain the puzzling narrow coverage of living wage laws that Freeman emphasized—in particular, why, despite the antipoverty rhetoric of living wage campaigns, they often result in passage of contractor-only laws that cover a very small share of the workforce.

Other observers of living wage campaigns have also noted the plausibility of this type of rent-seeking by unionized municipal workers—although they have not necessarily labeled it as such. For example, describing the Baltimore campaign, Fine writes that "AFSCME viewed the living-wage issue as a way to stem privatization," and quotes Bob Linehard, an AFSCME attorney closely involved in the Baltimore campaign, as saying, "Privatization is the maquiladora for the public sector" (2000–2001, 67). Levi, Olson, and Steinman note, "In an era of privatization, where unionized public workforces see their numbers dwindling due to outsourcing of their work, forcing contractors to pay living wages to their private employees levels the cost competition between public and private provision of services. Living-wage laws make it less tempting for cities to privatize service provision, thus increasing union bargaining power" (2002, 114). And Martin claims, "In Baltimore, AFSCME members employed by the city have benefited from the living wage ordinance, which raises the cost of contracting with the private sector for work that could be done by municipal employees" (2001, 473).

To study this hypothesis more directly, Neumark (2004) explored the impact of living wage laws on the wages of lower-wage unionized municipal workers (excluding teachers, police, and firefighters, who do not face competition from contractor labor). The key result is reported in column (1) of table 8.3. The estimate indicates that these workers' wages are indeed boosted by living wage laws that cover contractors, with an elasticity of 0.19. Columns (2) through (5), in contrast, show estimated effects of living wages on groups for which, under the rent-seeking hypothesis, no effects should appear (such as other non-unionized city workers, or unionized teachers, police, and firefighters); under other scenarios, such as living wage increases being associated with rising city wages generally, such effects might appear. Thus, these columns serve as falsification exercises—verifying that we *do not* see an effect where, according to the rent-seeking hypothesis, we *should not* see an effect. The fact that there are gains for unionized municipal

Table 8.3
Estimates of effects of contractor living wages on wages of below-median wage workers

	Contractor living wage laws					Noncontractor living wage laws
Workers	Unionized, municipal, excluding teachers, police, and fire	Unionized, municipal, teachers, police, and fire only	Unionized, nonmunicipal	Nonunionized, municipal	Nonunionized, nonmunicipal	Unionized, municipal, excluding teachers, police, and fire
	(1)	(2)	(3)	(4)	(5)	(6)
Living wage effect over twelve-month period	0.188*	−0.074	0.018	0.011	0.026	-0.133^{+}
N	2,025	848	9,807	5,407	124,478	1,395

Source: Neumark 2004.
Note: The data come from the CPS monthly ORG files from January 1996 through December 2000, for individuals residing in MSAs, in city-month cells with twenty-five or more observations. The regressions include controls for city, year, month, minimum wages, and other individual-level controls. The coefficients are from log-log specifications, and hence are elasticities. The particular workers included in each column are noted in the column heading. $^{+}$ and * indicate that the estimate is statistically significant at the 10 percent or 5 percent level; calculated standard errors are robust to heteroskedasticity and non-independence within city cells.

workers, but no significant positive estimates for other groups of workers that do not face competition from contractors, bolsters the likelihood that living wages boost wages of unionized municipal workers by increasing rents.

Finally, to test the interpretation that it is the contractor coverage, in particular, that generates gains for unionized municipal workers, column (6) of table 8.3 reports estimated effects on these workers only for living wage laws that do *not* cover contractors (of which there are very few). In this case, there is no evidence of a positive effect on wages, and, if anything, some evidence of a negative effect. Although the explanation of a negative effect is not obvious, the absence of a positive effect implies that it is contractor coverage of living wage laws that generates higher wages for unionized municipal employees, as the rent-seeking hypothesis would suggest.

Consistent with this view, Martin (2006) finds that declines in the local government's share of employment from 1980 to 1990 predict adoption of a living wage law between 1994 and 2004, and concludes that this implies that privatization spurred living wage laws. However, this is not fully convincing, because the local government's share of employment is not necessarily a measure of privatization and could change for other reasons; as just one example, changes in this share could be driven by changes in total private-sector employment. Stronger conclusions require direct data on privatization, and even then these data may not capture the current *threat* of privatization, which could, in principle, be stronger in cities that have—as of the time of the living wage—undergone *less* privatization. The ambiguity of the conclusion is furthered by evidence that prior municipal purchasing per capita—a proxy for contracting out—is not associated with passage of living wage laws. As a result, more work is needed to test directly whether the economic motivations of various actors actually shape living wage laws, paralleling the much larger body of work on minimum wages.

But do the same living wage laws that benefit unionized municipal workers also reduce urban poverty, perhaps as a happy coincidence?[49] The results in this section are for contractor living wage laws—that is, laws that cover contractors, whether or not they cover other employers. In contrast, the earlier results showed significant reductions in urban poverty only in response to living wage laws extending to financial assistance recipients. Thus, there is not detectable evidence of benefits to other workers stemming from living wage laws that cover only contractors—by far the most prevalent type of living wage.

8.5 Conclusions

Proponents of the minimum wage typically phrase their support in terms of the benefits that raising the minimum wage has for poor families. As we have mentioned on several occasions in this chapter, although we do not view the minimum wage as an effective antipoverty tool, we do not doubt the good intentions of those in today's society who promote it as a means to help those less well off than themselves. That said, it is also the case that a wide range of individuals and organized groups have an economic incentive to support minimum wages based on their own self-interest. For example, some labor unions stand to gain from minimum wage increases because a higher wage floor shifts labor demand toward higher-paid and higher-skilled workers who are more likely to be union members. Similarly, larger corporations that are either better positioned to absorb a minimum wage increase or that already pay their lowest-skilled workers more than the minimum wage may see their competitive position enhanced from a higher minimum wage as the labor costs of their lower-wage competitors rise.

The key political economy question is whether these economic motives have an effect on the political process of decision making regarding minimum wages. Theoretical models point to a variety of potential influences on the decisions that legislators make, and they suggest that one important determinant of minimum wage legislation is the power of particular constituent groups whose members' economic outcomes are directly or indirectly affected by increases in the wage floor. But politicians may respond to other influences as well, including pressures from other members of their political party and their own personal views about the desirability of raising the minimum wage.

Thus, in the end, the relevance of the political economy model to the determination of minimum wages is an empirical question. In this regard, we would characterize the existing evidence on the importance of special interest groups in influencing federal and state minimum wage policy as mixed. Many of the studies surveyed here do find a relationship between the size of labor or business groups and either the voting tendencies of members of Congress or the relative levels of state- or province-specific minimum wages. But other studies stress the importance of ideology or broader economic considerations.

However, we would also emphasize that assessing the evidence from this literature is complicated by an inability to clearly distinguish among the alternative hypotheses. The variables included in the studies are often highly correlated, and many have more than one possible interpretation. For example, one finding that comes through fairly clearly in the U.S. studies is a positive relationship between unionization and minimum wage increases. Such a finding is clearly consistent with a political economy interpretation of the legislative process for minimum wages, especially given the evidence that union workers benefit from the minimum wage. However, we do not know for sure the motivations of union leaders, and thus we cannot reject the possibility that their intent is to raise the earnings of low-wage workers more generally.[50]

We also reviewed the growing literature on living wage laws in this chapter. Similar to the evidence for the minimum wage, living wage laws appear to raise the wages of those who are directly affected by the law, but they also entail some disemployment effects. In contrast to minimum wages, however, the distributional effects of living wage laws appear to be beneficial in that such laws tend to raise the incomes of families near the poverty line. The primary reason for this difference is likely that living wage laws are better targeted toward low-wage adults and away from teenagers in higher-income families.

With regard to the political economy of living wage laws, our sense is that, as for the minimum wage, support for living wage laws is guided by both economic self-interest and by non-pecuniary motivations. The evidence suggests that the members of special interest groups likely to exert considerable influence in local elections, such as municipal unions, tend to benefit from living wage laws. However, it seems difficult to attribute the broader campaigns for living wage laws solely to the economic self-interests of particular constituent groups.

This summary of the evidence clearly leaves "on the table" the hypothesis that minimum wages (and living wages) receive political support from an ideological desire to help low-income families. Of course, the evidence we presented in the earlier chapters indicates that minimum wages, in particular, fail to achieve this goal, in that they primarily redistribute income among low-income families (or even away from them) rather than towards them. It remains to be seen whether a better understanding of the consequences of higher minimum wages will reduce the support that minimum wage increases receive from those who desire to help poor and low-income families. Indeed, one might argue that continued support for minimum wages by labor unions and others in the face of this evidence would help to establish that such support is more consistent with economic self-interest than with altruism.

9 Summary and Conclusions

9.1 Introduction

We have reviewed and discussed many facets of the effects of minimum wages, including their impact on employment, the wage and income distributions, skills and education, and prices and profits. Aside from the breadth of the topics covered in this book, the extent of the evidence that we have presented is immense, and of course some of it is in conflict. Nonetheless, one of the important tasks of synthesizing a large body of research is to try to draw some general conclusions—a task that is particularly important in public policy research, where the goal is ultimately to provide some guidance to policymakers. To that end, in this final chapter we try to summarize in a very succinct fashion the main conclusions from the research discussed in the preceding chapters. Of course, any attempt to boil down a large body of research into a few key conclusions will miss many of the complexities and subtleties present in the research. But we also believe that pulling together the conclusions from the various chapters can help to provide a comprehensive assessment of the merits of the minimum wage.

In reading this chapter, it is important to keep in mind the qualification that much of the evidence we have discussed is based on research for the United States. This is truer of some areas than of others. At one extreme, the literature on the employment effects of minimum wages is now quite international in scope, and we are therefore more confident in drawing generalizations that apply to other countries. At the other extreme, the literature on distributional effects is nearly entirely from the United States. As we emphasized in chapter 5, even in the face of similar disemployment effects, the effects of minimum wages on the distribution of family incomes can vary widely depending on who gains and who loses from minimum wage increases, and thus we might well expect the distributional effects of minimum wages to differ across countries.

9.2 Minimum Wages: A Scorecard

Our summary of the evidence discussed in this book is presented in table 9.1, which might be considered a "scorecard" for minimum wages. We list the main effects of minimum wages that we have considered, summarize the evidence very briefly, and provide our assessment of the strength of the conclusions that can be drawn from this evidence.

Given that we consider evidence for many different outcomes, do the conclusions lead to a relatively clear sense of whether minimum wages constitute good social policy? We believe they do. Indeed, we would argue that upon examining the scorecard in table 9.1, the question that virtually leaps off the page is, "Why would we want a higher minimum wage?"

Three conclusions, in particular, stand out. First, as indicated in chapter 3, the literature that has emerged since the early 1990s on the employment effects of minimum wages points quite clearly—despite a few prominent outliers—to a reduction in employment opportunities for low-skilled and directly affected workers. Second, the research on the distributional effects of minimum wages, though far less extensive, finds virtually no evidence that minimum wages reduce the proportion of families with incomes near or below the poverty line, and some of it indicates that minimum wages adversely affect low-income families. Finally, minimum wages appear to inhibit skill acquisition by reducing educational attainment and perhaps training, resulting in lower adult wages and earnings.

One potentially positive impact established by the extensive research that we have conducted and surveyed is that a higher minimum wage reduces wage inequality. But this evidence on the effects of the minimum wage on the wage distribution ignores any disemployment effects and thus does not provide an adequate characterization of the effect of the minimum wage on the earnings of low-wage workers. Because of these disemployment effects, together with the weak link between low-wage work and low family income, the boost to the wages of low-wage workers does not carry over to beneficial effects on earnings of low-wage workers or incomes of low-income families. The other evidence of a positive impact concerns the distributional effects

Table 9.1
Minimum wages—A scorecard

Effects considered	Summary of evidence	Strength of conclusions
Employment	Minimum wages reduce employment of low-skilled workers; adverse effects even more apparent when research focuses on those directly affected by minimum wages	Fairly unambiguous: a handful of studies find positive effects, and some find no effect, but the preponderance of evidence points clearly to negative effects, and even more so when one focuses on the most convincing evidence
Wage distribution	Minimum wages increase wages of workers bound by the minimum; modest spillovers on higher-skilled workers earning slightly higher wages; decline in real value of federal minimum in United States exacerbated growth of wage inequality	Effects on wages of bound workers unambiguous; some conflicting evidence on strength of spillovers, especially for developing countries; unanimity that declining federal minimum wage contributed to rise in wage inequality, but remaining debate over whether minimum wages were more important than other contributing factors that have been emphasized
Income distribution	No compelling evidence that minimum wages on net help poor or low-income families, and some evidence that minimum wages adversely affect these families, and increase poverty; some evidence that living wages have beneficial effects	For U.S. economy, clear conclusion that minimum wage effects range from no beneficial distributional effects to adverse effects, with no evidence of beneficial distributional effects; evidence for developing countries relatively sparse, and more varied
Skills		
Training	Some evidence of negative effects and some evidence suggesting no effect; no convincing evidence of positive effects	Unclear whether evidence is more consistent with negative effects or no effects
Schooling	Most evidence points to negative effects	Evidence for the United States is unambiguous; international evidence (Canada) is less clear
Longer-run earnings	Negative effects of exposure to higher minimum wage when young on wages and earnings in late twenties	Strong and robust evidence, but only one study
Prices and profits	Minimum wages increase prices of goods and services produced with low-wage labor, but no appreciable impact on inflation	Clear conclusions

of living wages. However, it is important to realize that efforts to broaden living wage laws to try to cover far more workers would make them more like minimum wages, which have no beneficial distributional effects and possibly adverse effects.

9.3 Directions for Future Research

Despite the large number of studies referenced in this book, we see considerable opportunity for future research efforts that would add to our knowledge about the economic effects of minimum wages. We have highlighted specific issues of concern throughout the previous chapters, but it seems useful to provide some general thoughts here as to what we see as the most pressing or promising questions to pursue.

First, and perhaps most obvious, the existing body of minimum wage research has tended to overemphasize the effects of minimum wages on employment. As a result, the literature on the implications of minimum wages for other outcomes, although growing, remains well behind the state of our knowledge regarding employment effects and, as should be apparent from the discussions in earlier chapters, is sometimes quite sparse. The focus on employment effects undoubtedly reflects both the greater availability of data on employment outcomes and the usual emphasis of policy debate, as well as perhaps the interest of economists in testing the law of demand by exploiting the experiment provided by a government-mandated wage floor. But as we have stressed repeatedly, such an analysis represents only one piece of what is needed to assess whether minimum wages are a useful policy tool.

In particular, further research on the implications of minimum wages for the levels and distributions of wages, incomes, and human capital accumulation strikes us as particularly important for evaluating minimum wage policies. For example, most of the literature on the effects of minimum wages on the distribution of family incomes uses data from the United States through the mid-1990s. In this regard, efforts to expand this literature to include the more recent period of welfare reform and EITC expansion, and to compare the effects of the minimum wage with those of other policy options (as well as potential interactions) seems likely to be especially fruitful. In addition, more evidence on the effects of minimum wages on incomes in other countries—and comparisons across countries—would help in understanding how minimum wages interact with the variety of institutions and policies in place in today's world.

Similarly, further research on the effects of the minimum wage on human capital accumulation could potentially contribute to our understanding of the longer-run implications of minimum wages. The available evidence on this issue is relatively sparse and far from conclusive, in part because of difficulties in accurately measuring skill acquisition. Thus, additional studies using alternative data sources, both for the United States and for other countries, are clearly needed. Moreover, extending the longer-run perspective to the evaluation of the effects of minimum wages on poverty and the income distribution would be worthwhile.

Second, we see some areas for future research even with regard to the vast literature on employment effects. At a minimum, the wide range of estimates in the recent literature suggests that efforts to reconcile different studies may help in understanding the economics of minimum wages. We have suggested a few possible reasons for such differences—including lags in adjustment, labor-labor substitution within low-skill groups, and substitution in product demand across similar goods. But a systematic assessment of the sources of differences in the estimates across studies using meta-analysis techniques and other approaches could improve our understanding of how to interpret the literature. In addition, the predictions of theory tend to be about overall labor input rather than about employment specifically, and although a few studies have attempted to distinguish the effects on hours from those on employment, there is clearly room for more research on this topic.

Finally, minimum wage changes may be endogenous with respect to economic conditions and other policy choices. To date, most studies have largely ignored this issue, with the result that many of the estimates reported in the literature may be biased to some degree. But given the political economy literature, it is clear that this problem cannot be discounted entirely. As a result, efforts to account for such endogeneity should increase our confidence in estimates of minimum wage effects and may produce further refinements to our knowledge.

9.4 Concluding Thoughts on Minimum Wages

Based on the evidence from our nearly two decades of research on minimum wages, coupled with the evidence accumulated from an impressive body of research conducted by others, we find it very difficult to see a good economic rationale for continuing to seek a higher minimum wage. We recognize that the effects of minimum wages on the wage or family income distribution (as well as the other effects covered in table 9.1) cannot be translated directly and unambiguously into economic welfare formally defined. But we are hard-pressed to imagine a compelling argument for a higher minimum wage when it neither helps low-income families nor reduces poverty.

Of course, there may be other reasons to support a minimum wage, and, in fact, some proponents appeal to arguments about the morality of low wages. For example, the president of the AFL-CIO has argued that "paying someone $5.15 an hour is just immoral."[1] Similarly, the cover of the *New York Times Magazine*, for a recent story on living wages, asked "Is How Much You Pay a Worker a Moral Issue?" (Gertner 2006). We are obviously not in a position to judge such arguments. However, we would point out that societal values and economic analysis do intersect in the construction of social welfare functions for evaluating alternative distributions of income (or more broadly resources) in an economy. In this context, the social welfare implications of the effects of minimum wages on the distribution of family incomes obviously depend on how society values the winners and the losers of a minimum wage increase. For example, the findings in Wu, Perloff, and Golan (2006), showing that higher minimum wages reduce social welfare when those at the bottom of the distribution are weighted more heavily, would be an argument against a higher minimum wage floor based on social welfare functions with such a property. In any event, we suspect (although we cannot be certain) that arguments about the morality of a higher minimum wage would be tempered by evidence that minimum wages do not help and may in fact hurt low-income families.

Three questions arise quite naturally from our conclusions, and we close by addressing them in turn.

9.4.1 Should We Eliminate the Minimum Wage?

Our conclusions imply that a higher minimum wage will impose costs on low-skill workers and low-income families without delivering benefits that offset these costs. Conversely, of course, our results indicate that reductions in minimum wages would yield net benefits. Does this imply that we should take this further, and eliminate the minimum wage?

In answering this question, it is important to keep in mind that most of the estimates of minimum wage effects discussed in this book are based on the experiences following relatively modest changes in minimum wages. Using such estimates to predict the effects of much larger changes than are in the data will quickly lead to a sharp deterioration in the precision of these predictions. As a result, there is little justification for extrapolating our conclusions and assuming that if modest reductions in minimum wages would yield benefits of a certain magnitude, then large reductions would yield proportionately larger benefits. For example, reducing the minimum wage from a level that is already below the market wage for low-skilled labor might be expected to have little or no effect on employment or incomes because few individuals would be affected by the change. Similarly, we could not with much confidence argue that we should simply scale up the effects of the modest changes in minimum wages that we have documented to provide a sense of the consequences of much larger increases than those we have considered. Indeed, a much higher minimum wage could have macroeconomic spillover effects that are not captured in the available historical experience and thus could be disproportionately costly in terms of its effects on employment and incomes.

Nonetheless, moving beyond our evidence and adopting a somewhat more speculative stance, we are skeptical that eliminating the minimum wage would, as non-economists sometimes argue, lead to a widespread decline in wages to subsistence levels.[2] In most labor markets, wages are set by labor supply and labor demand, and not by the unconstrained wage offers of employers. For example, Gonzales (2007) finds that the average wage for day laborers in California—who are often unskilled illegal immigrants working in a completely unregulated labor market—is more than \$11.00 per hour. And, in one recent case where the minimum wage was eliminated—the abolition of the Wage Councils in the United Kingdom—wages did not fall precipitously.[3] Thus, although not recommending such a policy, we wonder whether eliminating the minimum wage might improve conditions of our nation's most blighted and depressed urban areas, where one can hardly argue that policies adopted so far have been successful.

9.4.2 Can 665 Economists Be Wrong?

A second question that arises from our conclusions is what to make of the endorsements of a higher minimum wage that are typically circulated during high-profile national debates about minimum wages and signed by hundreds of economists—many of whom we respect greatly. The most recent of these endorsements, signed by 665 economists in 2006, asserted that an "increase in the federal minimum wage to $7.25 falls well within the range of options where the benefits to the labor market, workers, and the overall economy would be positive."[4] On more than one occasion, upon reading this and previous endorsements, we have stopped to ask ourselves whether we have missed something. Obviously, we think the answer to this question is "no," and, indeed, writing this book has reinforced the conclusions we had arrived at in the course of our own research.

In pondering the policy debate, we have two related conjectures as to the source of support for these endorsements. First, it seems to us that, with regard to questions about the disemployment effects of minimum wages, the handful of studies reporting positive or no effects are given far too much weight. Researchers often summarize the existing literature by citing one or two studies claiming positive effects, along with a couple of studies reporting negative effects,[5] which can give the impression that labor economics research is roughly equally balanced on the two sides of this question, and that, therefore, one should not confidently hold the view that minimum wages reduce employment. However, as we have discussed, it is simply not the case that the research literature stacks up so evenly. Rather, the research leans heavily toward the finding of disemployment effects.

Second, we suspect that many well-meaning economists endorse minimum wage increases because of their unfamiliarity with the body of research on the distributional effects of minimum wages. The most recent endorsement claims that "research has shown that most of the beneficiaries are adults, most are female, and the vast majority are members of low-income families." And, as we noted in chapter 5, simulations of the effects of minimum wages often do point to beneficial distributional effects—even if the targeting is bad. As we pointed out, however, we view these simulation results as problematic because they typically rely on parameter values that essentially allow for no disemployment effects and make unwarranted assumptions about how minimum wages affect workers in different parts of the family income distribution. In contrast, the arguably more reliable research that examines the actual effects of minimum wage increases on the distribution of incomes finds virtually no evidence that a higher wage floor leads to a reduction in the proportion of families near or below the poverty line and some evidence that it leads to an increase in the number of poor families. We suspect that economists are quite familiar with the longer record of research based on simulation methods, but less so with the newer research that draws conclusions about the distributional effects of minimum wages from the same types of before-and-after or panel data analyses that are standard in much public policy research.

In suggesting that the endorsements of higher minimum wages by well-respected economists are based on a lack of a thorough understanding of the research record, we are not implying that these economists have been remiss in keeping up with the research record. When we embarked on writing this book, we were not fully aware of the extensive body of research on minimum wages that has emerged over the past decade and a half. However, in surveying this literature, we have been struck by the strength of the evidence that has emerged consistent with disemployment effects of minimum wages, and by the consensus in the (admittedly much smaller) research on distributional effects that they are either adverse or nonexistent. If doing the research for this book shifted *our* prior views about what the broader literature said about the effects of minimum wages, and we have been intimately involved in research on minimum wages for a long time, it would not be surprising if economists for whom minimum wages are at most a peripheral component of their research would also be unaware of the balance of the evidence.

9.4.3 What Alternative Policies Should Be Considered?

Minimum wages have a number of alluring features. They are simple to implement. They do not have to be funded by federal or state governments and hence do not get caught up in budget battles (although of course minimum wages do impose costs). And, they have an appealing moral sensibility. However, these features of minimum wages are misleadingly seductive. Minimum wages do not deliver on their goal of improving the lives of low-wage workers, low-skill individuals, and low-income families.

If minimum wages are not an effective tool to help low-wage workers and low-income families, what are the alternatives? In our view, the overriding question motivating most policy-oriented research in labor economics is how to best provide families with an acceptable standard of living. And either for philosophical reasons, or because of concerns about incentives, there is a strong preference for policies that enable families to become economically self-sufficient—*earning* their way to an acceptable living standard.

The most prominent set of policy interventions in recent decades that have sought to increase economic self-sufficiency have targeted the supply side of the labor market. Welfare reform, the expansion of the EITC, modifications to Medicaid and public health insurance for children, and income tax changes for low-income households have clearly had this aim.

Of course, minimum wages (and living wages) are often viewed as part of the policy toolkit to increase economic self-sufficiency.[6] Indeed, it is sometimes argued that policies that boost labor supply have the potential to drive down wages, and hence need to be countered by a higher minimum wage. However, because the minimum wage attempts to increase earnings of less-skilled workers by mandating a higher price for less-skilled labor rather than by raising demand, attempting to balance the increased supply with a higher wage floor may only lead to adverse effects on the individuals targeted by these policies.

An alternative policy that is aimed at increasing demand is a wage subsidy program targeted on low-skilled or disadvantaged individuals. Such a program can take many forms, depending in part on who is targeted, but all share the basic structure of subsidizing wages to increase demand for workers (shifting out the demand curve), thereby raising their employment and earnings (see Katz 1998).[7]

A third approach is to try to raise the productivity of low-skilled workers by increasing their human capital. Greater skill acquisition can occur through a number of channels, including additional schooling, a higher quality of schooling, and training.

We are by no means experts with regard to all of these policies. However, there seems to be little doubt that supply-side policy interventions have contributed to higher employment among the target population (e.g., see Meyer and Rosenbaum 2001; Blank and Schmidt 2001; Blank 2002). And the research on the EITC discussed in chapter 5 points more directly to increased earnings. We believe more research is in order on the potential benefits of wage subsidy programs, although there are clearly potential limitations and problems posed by such programs. Finally, although we suspect that nearly every labor economist would agree with the goal of increasing human capital, finding practical solutions to increase skills has been difficult. For example, training programs have a checkered track record, with benefits for some populations but not others (see, e.g., Heckman, Lalonde, and Smith 1999). And policies to increase schooling and improve school quality are often expensive and controversial (Dynarski 2002; Cornwell, Mustard, and Sridhar 2006; Neumark and Rothstein 2007).

Nevertheless, as this discussion suggests, there are a number of preferable alternatives to the minimum wage that can potentially be used to increase the economic resources in the hands of low-income families. Aside from the EITC, perhaps, we do not yet have a good handle on how to most effectively use these other policy levers. However, these other policies do hold promise, especially as continuing research and practical experience enhance their effectiveness. Thus, to the extent that our society endorses efforts to increase economic resources for those at the bottom of our income distribution, we have to grapple with, and pay for, policies that increase incentives to work and enable members of our society to become more productive.

Notes

1 Introduction

1. States are free to set a minimum wage higher than the federal level for workers covered by the federal minimum wage, and any minimum wage they choose for workers not covered by the federal law.

2. A lengthier discussion of the research literature on the employment effects of minimum wages, which is comprehensive and also explains *why* we find the studies emphasized in this chapter as particularly important or compelling, is provided in Neumark and Wascher 2007a.

2 The History of the Minimum Wage in the United States

1. Detailed descriptions of the early minimum wages in New Zealand and Australia are contained in Hammond 1913 and Verill 1915.

2. See Holcombe 1910 and Verill 1915 for additional details. According to Nordlund (1997), the word "sweating" is often attributed to Mrs. Sidney (Beatrice) Webb.

3. Support for the introduction of a minimum wage also came from Christian social reformers, who expressed concerns about the ability of individuals to bargain effectively with their employers for fair working conditions. For example, Thies (1991, 721) notes that "Father (later Monsignor) John A. Ryan, considered the leading American Catholic commentator on economic issues during the first part of the twentieth century, argued in *A Living Wage* that employers were morally obligated to pay workers at least a living wage, and advocated state coercion of the same through laws providing for minimum wages and maximum hours, old-age pensions, low-cost housing, and prohibition of child labor."

4. Leonard takes a somewhat different perspective on the motivation of at least some of the Progressive proponents of the minimum wage, arguing that they viewed "minimum-wage induced disemployment as a social benefit" that would have "the useful property of sorting the unfit, who would lose their jobs, from the deserving workers, who would retain their jobs" (2005, 213).

5. Indeed, according to Phelps: "Congress' first try at wage-fixing was a curious measure in 1892 setting *maximum* rates of pay of $6.00 a day for persons 'employed outside of the District of Columbia, in any capacity whatever, whose compensation is paid from appropriations for public buildings in course of construction'" (1939, 47).

6. Although some states did set up wage boards to recommend minimum wage rates to a minimum wage commission, the commission itself set the appropriate wage rates.

7. Lucas concludes, however, that despite its low levels and ongoing compliance problems, the Massachusetts minimum wage law did raise the wages of working women, although not to subsistence levels.

8. Prasch characterizes Clark's position on the minimum wage as conditionally supportive (2000, 257). However, our own interpretation of Clark's original essay is closer to that of Leonard.

9. More extensive summaries of the history of economic theory as it relates to the minimum wage can be found in Leonard (2000) and Prasch (1998).

10. Although Webb acknowledged that the least able individuals would be displaced, he argued "that to allow them to remain at large, in parasitic competition with those who are whole, is to contaminate the labor market" (993).

11. Taylor and Selgin (1999) characterize Filene's position as a precursor to the "high-wage doctrine" that became popular during the Great Depression.

12. See Verrill 1915, 179–180, for the specific questions and answers. In general, however, the Board of Trades responded that while it was too soon to come to a definite judgment, the Board's preliminary view was that minimum wages had had only a small effect on employment and that they had led to increases in efficiency among the affected employers.

13. Some of these studies acknowledged that women's employment (either in absolute terms or relative to men) fell following the introduction of the minimum wage, but attributed the declines to weak economic conditions rather than to the new minimum wage laws.

14. Both Thies (1991) and Kennan (1995) highlight this study.

15. The level of the minimum wage in Oregon varied by experience and became effective on October 4, 1913, for girls between ages sixteen and eighteen; on November 23, 1913, for experienced adult women in Portland; and on February 7, 1914, for experienced women outside of Portland and for all inexperienced women.

16. Peterson (1959) takes issue with this interpretation, arguing that the negative correlation between women's average wages and employment, together with the relatively larger decline in employment for women compared to that for men, is consistent with the hypothesis that the minimum wage had adverse consequences for employment of adult women. However, Lester (1960) argues that the larger drop in women's employment can instead be explained by a change in the regulation of hours for women and by the likelihood that women's employment was more sensitive to aggregate economic conditions than was men's employment. Peterson (1960) finds Lester's arguments unconvincing. See also the more detailed summary of the Obenauer and Nienburg study by Kennan (1995).

17. See Grossman (1978) and Nordlund (1997) for more detailed accounts of the events leading up to the passage of the FLSA.

18. Robert's vote to uphold the Washington minimum wage law is often referred to as "The Big Switch." See Samuel (2000) and Brinkley (2005).

19. The initial bill also included a forty-hour maximum work week and a minimum working age of sixteen, except in certain industries.

20. In contrast to today, unions were not uniformly supportive of federal minimum wage legislation in the 1930s. The AFL had a long history of opposing minimum wages prior to enactment of the FLSA, in part because they feared that minimum wages would usurp unions' role in collective bargaining. However, the Congress of Industrial Organizations (CIO), a group of unions that split from the AFL in 1935, was much more supportive. See, for example, Samuel 2000 and Nordlund 1997.

21. In addition, the FLSA initially set the maximum workweek at forty-four hours per week, with a reduction to a forty-hour week by the third year, and banned child labor (under age sixteen).

22. Nordlund puts the percentage of nonsupervisory wage and salary workers initially covered by the FLSA at about 40 percent (1997, 201).

23. See also the discussion in Leonard 2000, 128–130.

24. Lester acknowledged the possibility that other influences may account for the results but argued that "there is no need to analyze individual cases where the results are so opposite to the expectations of marginal analysis" (76). Peterson (1957) specifically mentioned some potential influences, including a defense build-up just prior to World War II, a long-run upward trend in furniture employment in the South, and a tendency for government contracts to favor southern garment production in the second half of 1940.

25. In 1974, the Congress brought all nonsupervisory employees of federal, state, and local governments under the realm of the FLSA. However, the Supreme Court ruled in 1976 that the minimum wage and overtime provisions of the FLSA were not applicable to state and local employees. This ruling was subsequently reversed in 1985.

26. The minimum wage increase was phased in more slowly for workers who would not have been covered prior to 1966 and for farmworkers.

27. In addition, Brown, Gilroy, and Kohen (1982) point out that these studies generally did not control for prior trends and, by focusing on only one industry at a time, did not make full use of the available data. In reanalyzing the results to take account of these shortcomings, Brown, Gilroy, and Kohen reported a negative and statistically significant elasticity of −0.24 for low-wage employment.

28. In large part, the increased use of time-series methods likely reflected the absence of cross-section variation in minimum wages in the 1960s and 1970s. As Brown, Gilroy, and Kohen note, in reference to the existing cross-sectional studies, "most of the variation in the 'minimum wage' variable . . . comes from variation in wage levels across states or areas, [so that] one is usually not certain whether the estimated effects are 'minimum wage' effects or 'state average wage' effects" (1982, 510).

29. Although other time-series studies of minimum wages were conducted in the 1960s, they tended to use very simple formulations of the minimum wage variable and included few, if any, control variables.

30. Although the two discussants of Gramlich's paper, Robert Flanagan and Michael Wachter, were complimentary of Gramlich's focus on distributional issues, they interpreted his empirical results as less favorable to the minimum wage than he did. For example, Flanagan (1976, 454) wrote, "After absorbing the paper's empirical results, I began the final paragraph and read with astonishment that minimum wages 'are not terribly harmful and in fact even have slightly beneficial effects both on low-wage workers and on the overall distribution of income." Similarly, Wachter concludes that "Gramlich's arguments in favor of minimum wages are not supported by the evidence" (1976, 459).

31. Indeed, the editorial, which was titled "The Minimally Useful Minimum Wage" (March 21, 1977), anticipates remarkably well some of the arguments we make in chapter 5 of this book. For example, the editorial asserts that "some poor people would benefit at the expense of other poor people" and that "many teen-age workers are members of middle-class families," with a citation to the research by Gramlich (1976).

32. The *New York Times* editorial also addressed this issue, arguing, "Organized labor favors a high minimum wage because that reduces management's resistance to union recruiting. Where cheap alternative sources of labor are eliminated, high-priced union labor no longer looks so bad to company managers." We return to this issue in chapter 8.

33. Although the 1977 amendments directed the commission to study twelve topics, the commission viewed some of them as interrelated and grouped them into six broader areas (Minimum Wage Study Commission 1981, xiv).

34. Similar actions by states occasionally occurred in earlier periods in which the federal minimum wage was unchanged for several years. For example, Levy (1974) notes that twelve states and the District of Columbia had minimum wage rates that exceeded the federal level in January 1974. However, most of these state minimum wages were quickly overtaken by the increase in the federal minimum wage to $2.00 in May of that year.

35. In addition, legislation passed in Connecticut in 1974 set that state's minimum wage 0.5 percent (or about 2 cents) above the federal level.

36. The average minimum wage in Washington, D.C., was even higher, at $4.38 per hour.

37. In states where the state minimum wage is higher than the federal level, the state minimum wage applies to all workers covered by the state law even if they are also covered by federal law. In states where the minimum wage is set at or below the federal level, the federal wage floor is applicable to all workers covered by the FLSA, and the state minimum wage applies to other workers covered by state laws. This latter set of workers would tend to include, for example, those who are employed by very small businesses that are not engaged in interstate commerce and seasonal workers in recreational establishments.

38. The minimum wages in Colorado, Florida, Montana, Oregon, Vermont, and Washington are indexed to inflation. In addition, several states have already passed legislation that will increase the minimum wage further in coming years, and Arizona and Missouri are set to index their minimum wages beginning in 2008.

3 The Effects of Minimum Wages on Employment

1. See table 2.3 for details.

2. Card and Krueger (1995a), Kennan (1995), and Brown (1999) provide earlier critical summaries of the first wave of this literature.

3. The broader literature also includes a few studies of city minimum wages, which we do not discuss in this chapter (see Dube, Naidu, and Reich 2007; Potter 2006; and Yelowitz 2005). In addition, we do not focus much on some newer research that attempts to draw inferences about the effects of minimum wages from more structural models (Aaronson and French 2007; Aaronson, French, and MacDonald 2006; Ahn, Arcidiacono, and Wessels 2005; Arcidiacono and Ahn 2004; Flinn 2006; and van den Berg and Ridder

1998), although some of these papers are discussed in chapter 7 in relation to the effects of minimum wages on prices. Finally, this chapter discusses a few studies that were made available to us after we published our review.

4. These numbers are little changed if we exclude our own work. In particular, 63 percent of the remaining studies indicate negative employment effects, and 81 percent (instead of 85 percent) of those we deem most reliable point to negative employment effects.

5. In addition, the more elastic is the supply of capital, the less the price of capital rises as firms substitute toward it, and hence the more elastic is labor demand.

6. This latter possibility may be largely a theoretical curiosity. Most evidence suggests that low-skilled labor and capital are more substitutable in production than are high-skilled labor and capital (e.g., Griliches 1969; Bergström and Panas 1992), although the support for this hypothesis is often characterized as relatively weak or mild (e.g., Hamermesh 1993; Duffy, Papageorgiou, and Perez-Sebastian 2004). However, the existing research does not speak directly to the substitutability between minimum wage labor and capital compared with the substitutability between other types of labor and capital, but instead tends to use broad skilled and unskilled labor aggregates. Interestingly, however, Henderson (2005) and Duffy, Papageorgiou, and Perez-Sebastian use cross-country panel data and find that capital-skill complementarity receives more support when a relatively low threshold is used to define skilled labor (and hence unskilled labor is defined more narrowly); such evidence may correspond more closely to the production function parameters of most interest in considering the employment effects of the minimum wage.

7. Kennan proposes a model in which the shift in product demand results from a change in the distribution of incomes rather than from a change in relative prices. In this model, which he labels "the hungry teenager theory" (1995, 1961), the minimum wage raises the income of workers more likely to consume fast food (e.g., teenagers) and reduces the income of other individuals, thus leading to an increase in the demand for fast food and possibly raising employment in that industry. More generally, in the past it has sometimes been suggested that an increase in the minimum wage will stimulate aggregate demand and boost employment by raising the incomes of individuals with a high marginal propensity to consume out of income (e.g., Filene 1923). Taylor and Selgin (1999) provide an interesting survey on the origins of this "high-wage doctrine" and its popularity during the Great Depression.

8. As Brown (1999) notes, a minimum wage effectively makes the supply of labor perfectly elastic up until the point at which that wage is not sufficient to attract any additional workers into the labor market.

9. As we discuss in more detail in chapter 7, the monopsony model also implies that an increase in the minimum wage leads to a higher level of output and a lower price.

10. As noted in chapter 2, efficiency wage models in the context of minimum wages were first proposed by Sidney Webb (1912), although, in his view, the additional productivity stemming from the minimum wage would offset the costs of the higher wage floor rather than raise employment. Stigler (1946) also refers to the possibility that higher minimum wages would lead employees to work harder or firms to be "shocked" into reducing other inefficiencies. However, he viewed these possibilities as unlikely in the competitive environment of most low-wage industries.

11. See also Lang (1987), who presents a signaling model in which worker productivity is not observable by firms. In this model, a minimum wage set above the equilibrium

wage for low-productivity workers reduces their incentive to attempt to pass themselves off as high-productivity workers and thus lowers the hiring costs incurred by firms.

12. Another model along these lines is presented in Dickens, Machin, and Manning 1999. These authors introduce firm heterogeneity by including in each firm's labor supply schedule a variable measuring the general attractiveness of working at that firm. Their additional assumption that labor market frictions cause individual firms to face an upward-sloping labor supply curve leads to an environment of monopsonistic competition.

13. See Manning 2003 for a more complete review of monopsony models based on labor market frictions.

14. See also studies by Eckstein and Wolpin (1990), Swinnerton (1996), van den Berg (2003), and Flinn and Mabli (2005).

15. His model is estimated in first differences over two years, so the state effects drop out.

16. The comparison sample included Arizona, Florida, Georgia, New Mexico, and Dallas–Fort Worth, which were chosen because they had similar labor force participation rates, employment-to-population ratios, and unemployment rates to California in 1987.

17. The inconsistency of the results from their minimum wage studies with the negative employment predictions of the competitive model is presumably the source of Card and Krueger's characterization of many economists' competitive views of labor markets as a "myth" in the title of their influential book (1995a).

18. See Grant and Hamermesh 1981. In addition, in the absence of state-level data on prices, using the average wage in the denominator provides a way to measure differences in the real minimum wage.

19. In a log specification, changes in the adult wage are constrained to have the same size, but opposite-signed, effects on employment as changes in the nominal minimum wage. In a levels specification, the constraint is simply that the two variables enter only as a ratio.

20. In response to our finding, Card, Katz, and Krueger (1994) reestimated Card's specification using a two-year difference (from 1989 to 1991) rather than a one-year difference, and still found positive effects of the 1991 minimum wage increase on employment. As we showed in our reply, however, estimating a two-year difference is not the same as including a lagged minimum wage variable in the model, because the two-year difference still omits a lagged effect of minimum wages.

21. Baker (2003) updates the key employment results using data through 2000 instead of 1993 (both studies begin with 1983). He finds stronger disemployment effects, which he attributes to a higher share of teenagers bound by provincial minimum wages. This paper focuses more on the effects of minimum wages on training and schooling, and hence is discussed in greater detail in chapter 6.

22. For sixteen- to twenty-four-year-olds as a whole, however, the disemployment effects were significant and negative regardless of the inclusion or exclusion of this variable.

23. This finding was in response to a comment on our paper by Evans and Turner (1995), who argued that using this broader definition of school enrollment caused the estimated employment elasticity for teenagers to become small and insignificant. However, this

measure of enrollment is available only in the October CPS, and Evans and Turner's results were based on a specification that combined school enrollment data for October of each year with minimum wage information for May. The results reported in the text are from estimates using October observations for all of the variables in the model. Additional evidence on this exchange is reported in Neumark and Wascher 2003.

24. The net employment change is most relevant to the usual discussion about whether minimum wages reduce employment. However, the gross employment change from a regression of this type may be more informative about how minimum wages affect the lowest-skilled workers they are most intended to help.

25. We discuss the specific estimates from their preferred specifications later.

26. See http://www.globalinsight.com/Perspective/PerspectiveDetail6081.htm (viewed November 6, 2007).

27. Labor economists refer to this approach as adding a "third level of differencing," or using a "difference-in-difference-in-differences" estimator. This procedure is used in analyses of the effects of a wide variety of policies, but in the case of minimum wages, it compares changes in outcomes for affected individuals (or families) where the minimum wage did increase and did not increase (the first two differences) to the same types of changes for individuals who should not have been affected—typically high-skill or high-wage individuals (or high-income families); the comparison between the affected and unaffected groups is the third difference.

28. Bertrand, Duflo, and Mullainathan (2004) originally highlighted the importance of this issue for panel data analyses of the type common to minimum wage studies.

29. The correction for non-independence is typically done by "clustering" the standard errors by geographic area. In this method, the error terms in the regression model are assumed to be independent *across* geographic areas but not within them, and the procedure therefore computes standard errors that are robust to arbitrary pattern of correlations among observations in the same geographic area (including across different periods) as well as heteroscedasticity. Details are given in Bertrand, Duflo, and Mullainathan 2004; for additional discussion see Wooldridge 2003.

30. Many earlier studies did, however, allow for serially correlated errors (e.g., Neumark and Wascher 1995a; Burkhauser, Couch, and Wittenburg 2000a), which can help in addressing the same problem.

31. A potential shortcoming of Kim and Taylor's analysis is the absence of a direct wage measure in the County Business Pattern data they used (Card and Krueger 1995a; Kennan 1995). In particular, they computed wage rates by dividing total payrolls for the first quarter of each year by total employment for a single pay period in March, which may induce measurement errors associated with differences in the timing of the numerator and denominator and with variation in the average number of hours included in the pay period. Kim and Taylor were well aware of this data problem and noted that there is no indication of a negative correlation in years in which the minimum wage was constant; they also showed that IV estimates that use lagged wages and average firm size in the industry as instruments produce similar results. Card and Krueger address the first point by showing that there is a negative correlation in the 1989–1990 change (although this could reflect a lagged effect from the 1988 increase in the minimum wage). In addition, they point out that the significant negative coefficient in the IV estimates relies on the inclusion of average firm size as an instrument, which they argue is inappropriate.

32. The same problem likely exists in the Katz and Krueger (1992) study. Although the paper is not very specific about the nature of the questions used to elicit the employment data for the Texas study, the survey instrument appears to be included in an appendix to a related paper (Katz and Krueger 1991), with wording similar to that used in Card and Krueger's New Jersey study. Figure 2 in the 1992 study similarly indicates some very large changes in employment.

33. The estimates for total hours are somewhat stronger. All four estimates (for the two specifications, and with hours changes measured in absolute and percentage terms) are negative, with the state difference-in-differences estimates statistically significant at the 5 percent level. However, the estimated effects are very large, with the implied elasticities ranging from -0.85 to -0.92. The authors indicate that they are less confident about the hours results, however, because of measurement problems.

34. We concluded, "The payroll data raise serious doubts about the conclusions CK drew from their data, and provide a reasonable basis for concluding that New Jersey's minimum-wage increase reduced fast-food employment . . . in New Jersey relative to the Pennsylvania control group. Combined with the new evidence from the ES-202 data that CK present . . . we think we can be more decisive in concluding that New Jersey's minimum-wage increase did not *raise* fast-food employment in that state" (2000, 1391).

35. Tip credits specify a dollar or percentage amount of the minimum wage that can be made up by tips. For example, a 50 percent credit coupled with a \$5.00 minimum wage would imply that as long as hourly tips exceed \$2.50, the employer has to pay only a base hourly wage of \$2.50. The paper makes no reference to taking account of tip credits in defining state minimum wages, and tip credits vary across states; see http://www.dol.gov/esa/programs/whd/state/tipped.htm (viewed November 6, 2007).

36. Indeed, earlier work by Partridge and Partridge (1999) noted the potential for the tip credit to render results for the restaurant industry inapplicable to other industries. In their study, they present some evidence of disemployment effects for the retail sector as a whole and for the retail sector excluding eating and drinking establishments. However, the estimated effects for eating and drinking establishments, although negative, are mostly insignificant. That said, we also have doubts about their analysis, as they find significant negative effects of the minimum wage on overall nonfarm employment growth as well, which seems implausible.

37. More specifically, they estimate the specification proposed by Solon (1985), which also includes interactions between the quarterly seasonal dummies and a linear and quadratic trend, along with a standard autoregressive (AR(1)) correction.

38. Another possibility is that mismeasurement of the minimum wage variable in such studies has increased over time because of the proliferation of state minimum wages.

39. As evidence, they revisit the Card and Krueger time-series analysis of minimum wage effects and note that, using the standard specifications, the estimated minimum wage effects are quite sensitive to the method used to estimate the AR(1) error process and that the AR(1) coefficient rises to close to unity as the sample length is increased, suggesting the possibility of a unit root in the error term. They then use Augmented Dickey-Fuller tests for unit roots in the data and find that the teenage employment-to-population ratio is I(0) but that the Kaitz index is I(1), implying that the standard specifications yield inconsistent estimates.

40. The authors show that the forecast performance of the other models, estimated through 1979, deteriorates badly.

41. Wolfson and Belman (2004) estimate the employment effects of the minimum wage using time-series data for individual three-digit Standard Industrial Classification industries with either a relatively high fraction of young workers or a relatively low average wage. Most of the employment estimates by industry are statistically insignificant, although they are more often negative than positive, especially for legislated minimum wage increases (rather than real declines). Given that the models are so highly disaggregated (and pooling restrictions are not tested), the lack of significance of many of the results may not tell us much. The authors also argue that the competitive model predicts a sharper employment reduction when the minimum wage has a larger positive effect on the average wage, which is contradicted by their evidence. However, this evidence is not necessarily inconsistent with the standard model of the minimum wage. Absent any employment changes, the average wage should go up the most in industries for which the gap between prevailing wages and the new minimum is the largest, and in this case, their filter would pick out the industries with the most workers bound by the minimum wage. However, the authors only observe wage changes that accompany employment changes, and their result could arise because average wages rise the most in industries with the least possibility to substitute away from low-wage labor and toward nonlabor inputs, or where it is easiest to substitute higher-skilled for lower-skilled labor. In either case, their filter would tend to pick out industries that should have smaller disemployment effects rather than larger ones.

42. Card and Krueger also advocate this approach (1995a, 398).

43. Over the sample period, neither state's minimum wage law had a tip credit.

44. The interpretation of evidence based on help-wanted ads is potentially problematic, because a drop in help-wanted advertising could be associated with a decline in quits rather than a decline in hiring (in which case employment would rise). However, given the other evidence of employment declines, at least in the restaurant sector, this does not seem like the most plausible explanation of the findings.

45. This same idea has led researchers to study other low-wage groups. For example, Orrenius and Zavodny (2007) estimate the effects of minimum wages on employment of low-skill immigrants. They do not detect adverse employment effects for this group (although they do for teenagers). They present evidence consistent with low-skill immigrants moving away from states that increase their minimum wages, which could obscure negative labor demand effects. In addition, if immigrants work off the books, then a higher minimum wage can induce substitution towards them.

46. Dividing -0.1 by 0.213 adjusts the numerator of the conventional elasticity to obtain the percentage employment decline among affected workers. Dividing by (10.8/21.2) corrects the percentage wage increase in the denominator of the conventional elasticity to reflect the fact that the average wage increase for affected workers is smaller than the minimum wage increase itself.

47. On the other hand, there may be some positive distributional effects from wage increases for workers already earning slightly more than the minimum wage, whether stemming from relative demand shifts toward slightly higher-wage workers or from relative wage constraints faced by employers (Gramlich 1976; Grossman 1983). The focus on employment effects also ignores potential changes in hours, which could increase or decrease.

48. Note that this estimate is low compared with the illustrative calculation above. The same is true regarding some other evidence reported later in this section. However, that evidence also points to hours reductions as having a more significant negative impact on earnings. In addition, this elasticity does not take into account declines in the probability of finding employment for individuals who were not employed prior to the minimum wage hikes, or the adjustment for the actual wage increase versus the legislated minimum wage increase highlighted in equation (3.2).

49. They also study data for France, discussed in the next section, in which case they find strong evidence of disemployment effects among affected workers.

50. Although not shown in equation 3.3, we also include interactions between the *R* dummy variables and the ratio of the year 1 wage to the year 1 minimum wage, to allow even more flexibly for differences at various points of the distribution.

51. We adapt the econometric procedure to capture lagged effects of changes in the minimum wage. This complicates the estimation procedure because each individual is observed for only two years in the CPS.

52. We also estimated a version of the model allowing for monopsony, introducing a third regime in which employment was determined by movements along the supply curve. In this case, the additional switch point occurs where the labor demand curve intersects the marginal cost of labor curve.

53. The language barrier is more problematic for new and unpublished studies, and for research work done outside of academia because such work is less likely to circulate in English. The bias towards research written in English is not necessarily innocuous. For example, in our study of minimum wage effects in the OECD countries (Neumark and Wascher 2004, discussed later), we find that three of the four countries with institutional settings most likely to lead to negative effects of minimum wages on employment are English-speaking countries (the United States, the United Kingdom, and Canada), and that the two other English-speaking countries (Australia and New Zealand) are in the set of countries with institutions that are also relatively conducive to disemployment effects.

54. We again refer the reader to Neumark and Wascher 2007a for a comprehensive review. Dolado et al. (1996) provide a review of earlier evidence for many European countries. There is also some emerging work on the effects of the minimum wage in transition economies (e.g., Eriksson and Pytlikova 2004), which we do not cover.

55. Our empirical analysis follows a theoretical model developed by Coe and Snower (1997) in which various labor market policies—including the minimum wage—can have complementary effects on labor market outcomes.

56. There has been quite a bit of variation in youth subminimum wages in industrialized countries. In recent years, the United Kingdom, Canada, Portugal, Spain, and New Zealand have either eliminated youth subminimums or reduced the differential relative to adults. Some research exploiting this variation is discussed later in this chapter.

57. Some other solid evidence for Canada (Baker, Benjamin, and Stanger 1999) was discussed earlier.

58. This evidence is stronger than the findings from a similar approach presented in Yuen 2003, which the authors speculate reflects the greater bite of the minimum wage in the 1990s than in Yuen's sample, which ended in 1990.

59. We concentrate our discussion on the more recent paper, which is based on a slightly longer time period and includes lags in some of the specifications estimated.

60. They instrument for sales with lagged sales. However, we are skeptical of this approach in dynamic panel data models, because identification requires the exclusion of lagged values from the equation of interest, yet theory provides little guidance in specifying the appropriate lag length of the underlying model.

61. This same factor may explain why, in our 2004 paper using data across countries and over time, there is less evidence of disemployment effects of minimum wages when minimum wage levels are set by bargaining.

62. Employment in the Wages Councils sector grew by 1.29 percent, and was about unchanged (a decline of 0.04 percent) in the non–Wages Councils sector (see their table 10). A similar pattern is evident from the data they report on hiring and exit rates. The average hiring rate in the Wages Council sector during the three quarters preceding abolition exceeded the hiring rate in other industries by 2.69 percentage points, while the average exit rate in the Wages Councils sector exceeded the exit rate in the other sectors by 2.58 percentage points; that is, hiring and turnover were both higher by roughly the same amount—not surprising for low-wage industries. But in the post-Councils period, the difference between hiring rates in the two sectors widened sharply to 3.67 percentage points, while the difference in exit rates increased only negligibly to 2.88 percentage points (see their table 11). This relative increase in hiring rates in the Wages Councils sector, coupled with no relative change in exit rates, also points to disemployment effects from minimum wages.

63. The authors use two data sets, the Labour Force Survey (LFS) and the New Earnings Survey (NES). The results summarized in the text refer to the findings from the NES (which the authors emphasize). The results from the LFS point in the same direction but are weaker. The LFS data are problematic, however, because for a majority of workers the wage has to be constructed from information on usual hours and earnings over a pay period. Mismeasurement in the wage is particularly important in this application because the effects of the minimum wage are identified from differences in changes in hours below and just above the minimum.

Stewart has some earlier papers (2002, 2004a, and 2004b) that use this framework to estimate the effects of the minimum wage on employment, but that do not find a disemployment effect; however, those studies do not allow for the lagged minimum wage effect that, at least for hours, turn out to be important. Given the results for hours, it would have been useful to reexamine these earlier employment estimates using a specification that allowed for lags.

64. Strobl and Walsh (2007) explore the effects of the minimum wage in the United Kingdom on hours and find no evidence of adverse effects. However, they focus on a restricted subsample and are more interested in estimating the effects of minimum wages on hours for workers who express different preferences about how many hours they would like to work at the current wage (to test the predictions of a model for the effects of minimum wages on hours depending on whether they are on a positively or negatively sloped wage-hours locus).

65. They also presented some results using matched firm and individual data from other sources, which do not point to disemployment effects. But the matching yields a highly non-representative sample biased toward large firms. Here, we emphasize their two approaches using more convincing data.

66. Abowd et al. ("The Tail of Two Countries," 2000) extend the analysis for France to the 1990s, studying the impact of the effective minimum wage based on both minimum wage levels and payroll taxes and subsidies, with qualitatively similar conclusions regarding the employment effects of policy variation in low-wage labor costs.

67. The subminimum wage was also increased for seventeen-year-olds, from 50 percent to 75 percent of the adult minimum wage. However, Pereira focuses only on eighteen- to nineteen-year-olds, because there are few workers in the younger age group and few firms employing them in her data.

68. Portugal and Cardoso (2006) take issue with Pereira's work. They study the same minimum wage change using the same data source (although they use nearly the full universe of firm-level data, whereas Pereira used a 30 percent random sample), and report aggregate employment figures for teens and other age groups indicating that teen employment grew faster in 1988 and 1989 than employment of other age groups. They then compare these figures to those reported by Pereira, which show declining employment for teens and all other age groups, and argue that these discrepancies in growth rates indicate that Pereira's sample is "severely biased with respect to the actual trend in employment for the affected group" (2006, 995). However, Pereira reports employment levels only for firms that survive, so that her employment totals naturally shrink. (Pereira also shows that her regression results are qualitatively similar whether or not she takes account of entering and exiting firms.)

69. The authors summarize the results for hours as indicating a 10 to 20 percent increase in hours for sixteen- to seventeen-year-olds, and "up to a" 10 percent increase for eighteen- to nineteen-year-olds. Because these percentage changes outweigh the percentage declines in employment, they conclude that labor demand for affected workers, on balance, increased in response to the subminimum wage increases (227). However, these results are only found in specifications that exclude business cycle controls (the unemployment rate for twenty-six- to forty-nine-year-olds interacted with age dummy variables). The authors argue that although these controls "are important in the employment outcome specification, it is not obvious that they should have a large impact on other outcomes" (224). It seems to us that the same reason for preferring these controls to be included in the employment models applies to models for hours.

70. We also tend to discount this evidence to some extent because the authors question results for other outcomes they study using the same research design (in particular, for welfare benefits), suggesting that they are "simply *too large* to credibly be attributed solely to the minimum wage reforms" (227). This naturally raises questions about the interpretation of the minimum wage effects on employment and hours.

71. She also presents estimates instrumenting for the relative minimum wage variable with a real minimum wage variable, to correct for the possible endogeneity from unmeasured factors that positively affect both the average wage and employment. However, her IV estimates are almost always more strongly negative, contrary to expectations. The discussion therefore focuses on the OLS estimates.

72. Claims about the research evidence on minimum wages from organizations advocating for minimum wage increases are, perhaps not surprisingly, more extreme. For example: "The best recent research on the economic impact of the minimum wage shows positive effects without job loss" (Fox 2006); and "There is no valid, research-based rationale for believing that state minimum wages cause measurable job losses" (Chapman 2004, 2).

4 Minimum Wage Effects on the Distribution of Wages and Earnings

1. In the United Kingdom, these are also sometimes referred to as "knock-on" effects.

2. Although we have emphasized the positive effects of the minimum wage on other wages, in principle, wages higher in the distribution could fall following a minimum wage increase if a decline in the income of less-skilled family members owing to disemployment effects of the minimum wage led to an increase in the supply of more highly skilled workers.

3. For example: "Some economists are saying that minimum-wage increases have a ripple effect, bumping up the pay of a large portion of the working poor. If they are right, that would strengthen the political appeal of a minimum wage hike by increasing the number of potential voters who are helped" (Coy 2006); and "The additional 8.3 million workers (6 percent of the workforce) earning slightly above the minimum would also be likely to benefit from an increase due to 'spillover effects'" (Economic Policy Institute 2007).

4. Another issue that arises in developing countries is that with a large informal or uncovered sector, any positive wage spillover effects in the covered sector could, in principle, be offset by wage decreases in the uncovered sector associated with the flow of labor from the covered to the uncovered sector (Mincer 1976; Gramlich 1976).

5. This model is attributable to Welch (1969) and is outlined in Card and Krueger (1995a).

6. Of course, the worker is still worse off in this case. Prior to the minimum wage increase, the worker chose a lower effort level and wage. After the increase, however, the worker's choice is restricted to the higher effort-wage combination, or nonemployment. Teulings (2000) also develops a competitive model of the labor market in which workers have a continuum of skills and workers are assigned to jobs that have a continuum of complexity, so that the elasticity of substitution between workers declines with the skill difference between them. This model is similar to Pettingill's in that more-skilled workers are more productive in all jobs, but have a comparative advantage in more complex jobs. Although the model generates both a probability mass at the minimum wage and spillover effects, the main point of the paper is to suggest that elasticities of complementarity (which measure the effect of changes in supplies of inputs on the prices of other inputs) between workers of different skill levels can be much higher than other estimates suggest, helping explain what Teulings regards as surprisingly small disemployment effects of minimum wages in light of their sizable effects on the relative wages of low-skilled workers. As we indicated in chapter 3, however, disemployment effects for affected workers may not, in fact, be that small. Moreover, as we will show later in this chapter, the evidence points to a smaller effect of the minimum wage on relative wages than is suggested by the selective studies cited by Teulings (which are limited to DiNardo, Fortin, and Lemieux 1996 and Lee 1999).

7. Along the same lines, a spike could arise if some workers increased their skills to reach the minimum quality level at which workers are hired, a point discussed further in chapter 6.

8. Card and Krueger (1994) also looked at the question of offsets and reported that the New Jersey minimum wage increase did not lead to any significant changes in the

proportion of establishments offering free or reduced-price meals to their employees. Interestingly, however, in the course of our surveying these employers to collect payroll data in the research described in chapter 3, one franchise owner indicated that free meals were not a benefit, but rather were a means of keeping employees on the premises during breaks so that they would not leave and subsequently return to work high or drunk.

9. The wage-effort contract is assumed to be costlessly and perfectly enforced.

10. This type of effect could arise in a variety of settings where wage differences between less- and more-skilled workers are intended to achieve some goal. For example, firms might establish wage differences by tenure to elicit effort (Lazear 1979) or to encourage lower turnover workers to apply for jobs (Salop and Salop 1976).

11. One exception is Dolado et al. (1996), who find that the elimination of minimum wages in the United Kingdom did not result in a sharp drop in low-skilled wages.

12. There is no way to discern from these estimates whether there are spillovers above the minimum wage, an issue considered in more detail shortly. However, the spike itself is clearly created by hollowing out the tail below the minimum wage.

13. These authors also address the measurement error question by comparing a wage distribution measure constructed from BHPS data on earnings and hours with the distribution of directly measured hourly wages. Consistent with the measurement error hypothesis, they find no pronounced spike at the minimum using the constructed hourly wage. The authors then consider whether this difference reflects overtime premia, non-corresponding periods for the "usual" earnings and hours figures used to compute the wage, or additional components of pay captured in the BHPS (like commissions); they conclude that the second explanation is the most likely culprit.

14. In Costa Rica, they find that these spikes are larger for small rural firms where the minimum wage is expected to be more binding, which they regard as surprising because of the tendency for enforcement efforts in developing countries to be weak and focused on larger firms. The latter hypothesis appears to hold for Honduras, where, as we reported in chapter 3, they find a large spike in the large-firm covered sector but little evidence of a spike in the small-firm covered sector.

15. This does not contradict the evidence in Bell 1997 that the minimum wage in Mexico affects wages at the low end of the distribution. First, the effect on wages was much weaker for Mexico than Colombia; and second, the minimum wage in Mexico fell sharply over the 1980s.

16. An earlier study by Gramlich (1976) presents some time-series estimates and back-of-the-envelope calculations regarding the effects of minimum wages on overall wages. We return to this issue in chapter 7, which considers the impact of minimum wages on aggregate prices.

17. The restriction to non-manufacturing is intended to avoid the confounding effects of unions.

18. For example, their table 9.4 indicates that in the quarter preceding the 1990 increase, 7.4 percent of workers were paid between \$3.35 (the old minimum) and \$4.24 (the second increase in 1991 took the minimum to \$4.25). This figure is national, and so the percentage would be higher in low-wage states.

19. They do find some evidence of a positive effect on the median, but they note that this effect is limited to California, which is heavily weighted in their estimation.

20. Lee's study is more focused on the effects of the minimum wage on wage inequality and thus is discussed later in this chapter.

21. Formally, the limit of the ratio in equation (4.2), as $w_m - w^*(F)$ approaches zero, is $1/\beta$.

22. Panel c summarizes the employment effects discussed in section 3.5 of the previous chapter. Later, we return to discuss the evidence presented in the other panels of this figure.

23. In particular, we report estimates for workers earning 10 cents or more below the minimum, within 10 cents (on either side of the minimum), from more than 10 cents above the minimum to 110 percent of the minimum, from 110 to 120 percent of the minimum, 130 to 150 percent, 150 to twice the minimum, two times the minimum to three times, three to four, four to five, five to six, and six to eight times the minimum.

24. Note that the estimated contemporaneous elasticity for wages below the minimum is relatively high. However, we suspect that estimates for this part of the wage distribution are less reliable for a couple of reasons, including regression to the mean in wage data erroneously reported as below the minimum, and transitions between below-minimum-wage jobs (either because they are in the uncovered sector or include tips) and jobs that are subject to the full minimum wage. A transition to a covered job is likely to have an especially strong influence on the estimate for this cell because the jump in the wage upon moving to a covered job will be higher the more the minimum has increased. We also suspect that minimum wage increases may be followed by an upward (perhaps temporary) ratcheting of minimum wage compliance, as employers and workers become better informed about prevailing minimum wages.

25. The estimates displayed in figure 4.1a suggest that the elasticity of wages with respect to minimum wages at the low-end of the distribution falls to about one-half of the original effect after one year. These magnitudes appear quite reasonable, given that minimum wage increases over this period averaged around 10 percent, and wage inflation averaged a little over 4 percent. (The Employment Cost Index for wages and salaries grew at about 3.8 percent per year over this period in retail, and 4.9 percent in services; see U.S. Department of Labor 2000.)

26. They reach the same conclusion in their 2004a paper, which uses data from the Labour Force Survey but attempts to account for the measurement problems that stem from the small share of workers who report hourly wages. In contrast, in earlier work based on data for fourteen Wages Councils industries, the authors concluded that spillovers were substantial, with the result that "Wages Council minimum wage rates appear to significantly compress the distribution of earnings" (Dickens, Machin, and Manning 1998, 117). Spillovers appeared to be especially pronounced for women, for whom they detected positive and at least marginally significant effects of minimum wages on wages as high as the 60th percentile of the wage distribution. The more recent work does not reconcile the disparate findings.

27. Stewart and Swaffield present evidence from the BHPS in which, among workers who do not indicate that their pay was increased to comply with the new minimum wage, 2.1 percent indicate that they nonetheless had a pay increase "to maintain the difference (gap) between your pay and that of lower paid workers in your organization or outside who were affected by the National Minimum Wage" (2002, 652). Of course a reasonable question is whether workers would have any idea why their pay was increased.

28. Note that this evidence is not for wages directly, but rather for monthly earnings. But the minimum wage in Brazil is set in terms of monthly earnings.

29. See Neumark, Cunningham, and Siga 2006 for background on inflation and minimum wage increases in Brazil.

30. One problem with the Fajnzylber study, aside from the inflation issue, is that for most of the 1982–1997 sample period the minimum wage was national (beginning in 1984). As a result, the model specification includes dummy variables only for broad two- to three-year periods.

31. The higher figure in the latter case makes sense, because the minimum wage is higher relative to the distribution of wages in the informal sector. Lemos (2006a) also presents evidence from the posthyperinflation period, but still finds positive effects on wages quite high up in the distribution, especially in the informal sector. We do not have an explanation for these apparently anomalous results.

32. With regard to the Krueger study, see also Lang (2002), who criticizes the study's methods but suggests that the basic conclusion that computers increased the demand for skill is likely correct.

33. This method proceeds sequentially—for example, first asking what would have happened had the minimum wage not declined, then if, in addition, unionization had not fallen, and so on. However, the authors show that their qualitative conclusions are robust to varying the order of the decomposition, although the empirical magnitudes they estimate do change.

34. The value of their nonworking time is presumably below the minimum wage. If it were higher, they would not have chosen to work prior to the minimum wage increase.

35. Nonetheless, many of the studies in this area do not point out this difference, but simply focus on the effects of minimum wages on wage inequality. Fortin and Lemieux 1997 is one of the few studies to discuss this issue explicitly. Changes in employment effects can also influence the other calculations that DiNardo, Fortin, and Lemieux (1996) present; but it comes to the fore in the analysis of minimum wages, given the likelihood that there are employment effects.

36. These numbers are from their table III, which treats the minimum wage as the first step in the sequential decomposition. Their table V reverses the order. In that case, they attribute 33.9 percent of the increase in the 50/10 differential for men to the minimum wage but only 13.0 percent of the increase for women. As they point out, this diminution comes about because when demand and supply influences are accounted for first, "reversing" the decline in demand for less-skilled workers implies that there are fewer workers at the lower end of the wage distribution who are influenced by the minimum wage.

37. Teulings (2003) takes this conclusion even further, arguing based on estimates from a structural model that minimum wages can explain the entire increase in wage inequality in the 1980s. This result stems from his finding of much larger wage spillover effects—including effects on the returns to human capital—than is suggested by other research reviewed earlier.

38. In this case, the data extend through 2003, which raises concerns about the measurement of the minimum wage if state minimum wages are ignored, as emphasized with respect to aggregate time-series studies of employment effects of minimum wages dis-

cussed in chapter 3. Note also that these results for upper-tail inequality are the opposite of what Lee reports (1999, table II).

39. Although neither the correlations nor the series are reported, we estimate from the graph that the correlation is about 0.9 through 1987, and 0.4 for 1988–2003.

40. The lagged effects in isolation are not displayed in the figures, but are reported in table 2 of the paper.

41. In light of the earlier discussion in chapter 3 about employment elasticities for affected workers, these employment elasticities appear relatively small. They do not correct for the actual wage increase versus the legislated increase in the minimum wage, but given that the wage distribution near the minimum is finely disaggregated, this adjustment likely is not very important in this context. Of course the hours elasticities are more substantial.

42. Because most of the evidence is quite similar using the polynomial specification, and because the restrictions imposed by that specification are even more useful when we take smaller cuts of the sample, these disaggregated results were estimated using the polynomial specification. Results were qualitatively similar using the unrestricted specification—paralleling the full-sample results.

43. We also looked at separate results for men and women. The findings were similar for the two groups, with a slight hint that the consequences of minimum wages are worse for women.

5 The Effects of Minimum Wages on the Distribution of Incomes

1. This quote comes from a transcript of a press conference (http://www.cnn.com/ALLPOLITICS/1998/02/12/transcripts/clinton/, viewed April 16, 2007).

2. Similarly, in the international context, the World Bank report discussed in chapter 4 concludes, after discussing the wage and employment effects of the minimum wage, "Ultimately . . . we are concerned with the impact of the minimum wage on poverty" (World Bank 2006, 45).

3. One exception is the bilateral search model of Lang and Kahn (1998), in which a higher minimum wage can both increase total employment *and* shift employment from lower- to higher-skill individuals. The employment increase comes about because the higher minimum wage induces higher-skilled individuals to enter the labor market, competing with low-skilled workers for minimum wage jobs, which in turn spurs firm entry. Yet the employment declines among lower-skill individuals lead to lower welfare for them (and overall, as it turns out).

4. Disemployment effects are not necessarily the only source of inefficiencies introduced by minimum wages. For example, Luttmer (2007) notes that minimum wages may induce inefficient job rationing, which can arise when employers select from an excess supply of employees with homogeneous skills but heterogeneous reservation wages. This situation can be induced by minimum wages, because the wage floor, as Palda puts it, "short-circuits the ability of the low reservation wage workers to compete on price" (2000, 752). If, in response to a minimum wage, employers hire workers with higher reservation wages, then the rationing is inefficient, because the individuals who are working value leisure more than those who are not working. The low reservation workers might be able to spend resources to get the rationed jobs, but this just means that the costs from rationing are paid in a different manner. (Palda also discusses the social costs associated

with noncompliance with minimum wage laws, when low-productivity firms that are good at evading minimum wage laws out-compete high-productivity firms less adept at evasion; on the other hand, there may also be social costs associated with enforcement.)

5. In addition, some economic theorists point out that minimum wages can be part of a "second best" tax and transfer scheme to redistribute income in a way that maximizes social welfare when lump-sum taxes are not available. However, the ability of minimum wages to increase social welfare depends critically on what tax systems are available to the government and how the decline in labor demand associated with the minimum wage is distributed across low-income households. For example, both Guesnerie and Roberts (1987) and Allen (1987) show that with an optimal linear income tax, the minimum wage can be an effective means of redistributing income as long as the decline in labor demand is distributed equally among low-ability workers in the form of reduced hours. However, when the employment effects of minimum wages are concentrated among a small set of workers or when nonlinear tax schemes are available, the minimum wage is usually not a desirable policy in these models.

Marceau and Boadway (1994) extend the analysis to allow for unemployment insurance and show that if the minimum wage leads to involuntary job loss, a combination of minimum wages and unemployment insurance can raise social welfare, even though unemployment is higher because of the minimum wage. More generally, Boadway and Cuff (2001) show that a minimum wage combined with a traditional welfare system that requires individuals to accept any job offer will enable minimum wages to raise social welfare under both optimal linear and optimal nonlinear income tax schemes. Effectively, this combination of policies eliminates the constraint that workers must be as least as well off as nonworkers and allows the government to increase the redistribution of income from the employed to the unemployed. Boadway and Cuff note, however, that their results depend on several informational and enforcement assumptions that, if not borne out, "will limit the extent to which the minimum wage can accomplish the objectives set out in this paper, just like tax evasion limits the ability of the government to redistribute using the tax-transfer system" (572).

6. Keep in mind the possibilities discussed in the previous chapter, however, in particular that benefit reductions or increased work effort after a minimum wage increase could offset the increased utility from the higher wage.

7. The potentially weak link between low-wage work and family income was noted earlier by Stigler (1946).

8. The numbers in table 12 of the Burkhauser and Sabia paper are slightly different, because they consider only those earning between \$5.00 and \$7.25, while we consider all workers earning less than \$7.25. (The \$7.25 figure seems like a useful benchmark, because this is the final level included in the recent federal minimum wage bill passed by Congress.) It is unclear how to treat those whose reported wage is well below the minimum. Burkhauser and Sabia assume that they are not covered and would not be helped by the minimum wage. But the evidence discussed in the previous chapter on the effects of minimum wages on wages suggests that minimum wage increases do help those below the minimum, perhaps because compliance increases following a minimum wage increase. Regardless, the qualitative conclusions are the same, and the quantitative magnitudes are quite similar.

9. However, the 1939 data do not include income other than wages and salaries.

10. The link between low-wage work and low-income families strengthened slightly in the 1990s, with the share of low-wage workers that headed poor households rising from

7 percent in 1989 to 9 percent in 2003. Most of this increase reflects an increase in the share of workers who were single mothers.

11. Note, though, that this analysis is based on a definition of low-wage workers earning half the average private sector wage, and things would differ if we focused on workers at the prevailing minimum wage. Burkhauser and Sabia suggest that this is a reasonable benchmark to consider, however, as the AFL-CIO advocates a minimum wage set at this level (264, footnote 4).

12. Despite this view, Card and Krueger (1995a) overstate the strength of the link between low-wage workers and low-income households. Using data from early 1990 to study which workers were likely affected by the 1990–1991 increases in the federal minimum wage, they reported that 42.8 percent of affected workers (i.e., those earning between $3.35, the minimum prevailing in 1989, and $4.25, the wage to which the minimum had increased in two steps by 1991) were in families in the bottom three deciles of the income distribution. However, Burkhauser, Couch, and Wittenburg (1996) point out that this classification does not accurately reflect economic well-being because it takes no account of family structure (mainly family size). The federal government defines the poverty line as considerably lower for a single adult than for a family of four, so whether a family in, say, the second decile of the income distribution is classified as poor depends on its makeup. As a consequence, we find it more meaningful to consider family income relative to needs—that is, family income relative to the poverty line for that type of family. To illustrate the difference, Burkhauser, Couch, and Wittenburg reexamine the data based on income-to-needs, and show that about the same share of workers affected by the minimum wage increase were in poor or near-poor families (35 percent) as were in families with incomes at least three times the poverty line (32.8 percent).

13. "Near-poor" refers to family incomes between 1 and 1.5 times the poverty line. Analysis of the near-poor as well as the poor can be motivated in two ways. First, studying whether families are near-poor tells us something about families that have quite low incomes but may not be poor. Second, the definition of poverty is somewhat arbitrary (see, e.g., Formby, Bishop, and Kim 2005, chapter 2), and looking at results for the poor and near-poor combined is informative about what conclusions we would draw using a higher threshold for classifying families as poor.

14. For example, Card and Krueger report that 28.8 percent of workers in the bottom decile are affected by a minimum wage increase, in contrast to 6.2 percent in the fifth decile. But as a share of people in the decile who are affected, the corresponding numbers are 6.7 percent in the bottom decile versus 3.7 percent in the fifth decile.

15. This change in shares occurred in spite of an increase in the share of low-wage workers who were heads of poor households, in part because a smaller share of all workers lived in poor or near-poor households in the more recent period.

16. This problem reflects some of the same issues discussed in footnote 5 of this chapter regarding conditions under which a minimum wage (combined with an optimal linear tax) can be part of a second-best scheme to redistribute income.

17. For the regression that includes changes in state employment rates and regional dummy variables, the estimated effect is −0.03 with a standard error of 0.11. Using the unemployment rate control, the estimate is larger (−0.14 with a standard error of 0.12). Card and Krueger do not show results with the unemployment rate control and the regional controls included in the same specification.

18. Some statements by policymakers—parsing their words carefully—could be construed as appealing more to the idea that a higher minimum wage reduces poverty among workers than overall, to which Card and Krueger's analysis of workers may be more relevant. For example, in a statement in the U.S. Senate on June 19, 2006, in arguing for an increase in the federal minimum wage, Senator Edward Kennedy argued that "no one who works hard for a living should have to live in poverty."

19. The disemployment effects are not completely ignored because a worker is defined as someone who worked any time during the year covered by the data.

20. Burkhauser and Sabia do not report results with the employment rate controls, which were included in the preferred specifications reported by Card and Krueger.

21. Policies to help poor children may command more universal political support than policies to help poor adults, because even if one views adults as partly responsible for the decisions that land them in poverty, this is clearly not the case for children. That is, it is conceivable that poor adults are making utility-maximizing choices based on their own utility, but not internalizing the costs and benefits of their decisions for their children. Moreover, even if adults do internalize these costs and benefits, to the extent that childhood poverty leads to worse adult outcomes there may also be social returns to avoiding poverty that outweigh the private returns. The particular group for which Addison and Blackburn find the strongest evidence of minimum wage effects—junior high school dropouts aged twenty-five and over—is relatively old (based on CPS ORG data for 1996, an average age of 59.9, compared with 47.9 for the overall population aged twenty-five and over), and hence less likely to live with children. Moreover, this group is a small share of the population aged sixteen and over (6.6 percent in 1996).

22. He also estimates the effects of minimum wages on the probability that single mothers receive welfare benefits.

23. This parallels what occurs in the models of Luttmer (2007) and Palda (2000).

24. They prefer this measure because it satisfies two properties. First, a reduction in the income of a poor family must increase the poverty measure; and second, a transfer of income from a poor family to any less-poor family must also increase the poverty measure. The simple poverty measure (which is a headcount) satisfies neither of these properties.

25. The sample design of the CPS limits us to only two consecutive annual observations for each family.

26. If we omit the lagged effect and reestimate the model, the calculation in panel b implies an increase in the proportion of families in poverty of 0.003, and this estimate is again insignificant.

27. We also find a significant effect on the probability of falling from two or more times the poverty line into poverty, although the implied flow is small.

28. In contrast, there is no evidence that minimum wages lead to a decrease in the number of workers per family in poor families.

29. On the other hand, as we explain shortly, the nonparametric estimation comes at some cost, most notably the added complexities of recovering estimates of the combined effects of contemporaneous and lagged increases in minimum wages, and the inability to include other regression controls and to fully exploit continuous variation in the minimum wage. In our view, the advantages of the nonparametric approach outweigh the

disadvantages. Regardless of one's view on this question, however, the nonparametric approach clearly provides complementary evidence to parametric approaches.

30. Details of the estimation procedure are given in Neumark, Schweitzer, and Wascher 2005.

31. As described previously, our 2002b study incorporated a wide variety of control variables into a parametric regression framework, including unemployment rates, the wage distribution, AFDC benefits, and welfare reform. These other controls had relatively little impact on the qualitative conclusions. However, adding controls for AFDC benefits and waivers (which reduce poverty) led to slightly stronger adverse effects of minimum wages, suggesting that if we were able to account for these factors in the nonparametric framework, we would find more adverse effects of minimum wages on poverty than are reported in section 5.3.3.2.

32. In particular, this alternative procedure necessitates throwing out three years of data because we also allow for lagged effects of minimum wages.

33. We use the proportional change, because the first difference of the level of income-to-needs is unlikely to apply very well to either tail of the distribution. In addition, using the proportional change controls for the possibility that income-to-needs distributions shifted because of changes in the price of skill (see chapter 4) that affect incomes multiplicatively. In particular, increases in the price of skill that are common across all states would "hollow out" the left-hand tail of the distribution relatively more in higher-wage, higher-income states, which could in turn bias our estimated minimum wage effects to the extent that minimum wage increases in higher-wage, higher-income states provide relatively more (or less) identifying information. Because we use proportional changes, we estimate state-specific and year-specific medians rather than means to avoid outliers caused by very high or low income-to-needs values for either of the two observations on each family.

34. Because we rely partly on state minimum wage increases to identify the effects of minimum wages and because changes in state minimum wages may influence decisions by workers and firms to move into or out of a state, the effects identified from state minimum wage increases may differ from the effects of federal increases. As an example, Cushing (2003) finds that poor families tend to migrate into a state in response to minimum wage increases. The magnitude of the migration response is modest, especially relative to the non-migratory population, suggesting that migration has probably only a small impact on our estimates. Moreover, any effect will be muted considerably in our data, because migrants are not included in the matched CPS data; thus, we would only detect the indirect effects of migration on those who already resided in the state. Nonetheless, the presence of migration could lead our estimates to overstate the adverse impact of a federal minimum wage increase. In particular, if poor families migrate into a state that has recently raised its minimum wage, then poor families already in the state may face slightly more labor market competition, while poor families remaining behind in other states may face slightly less, thereby worsening outcomes in the treatment group and improving them in the control group. On the other hand, minimum wage changes at the state level have proliferated in recent years, and for these increases, our method likely understates (again, only slightly) the effects of the minimum wage because our sample includes some federal variation.

35. That is, we exclude observations in both the treatment and control groups corresponding to these increases.

36. For this estimation, the bootstrapping encompasses the estimation of the fixed state and year effects.

37. In the estimations that do not throw out years of data (figure 5.1, and panels c and d of figure 5.2), the magnitudes of the estimated effects displayed in the figures are largest below the near-poverty line, while those above three times the poverty line are quite small and nearly always insignificant. However, this is no longer the case in the estimations in which many years of data are dropped (figure 5.2, panels a and b). We suspect that the anomalies in the latter estimations reflect the loss of identifying information caused by excluding many minimum wage increases, and hence we interpret these results as illustrating the importance of using all of the available information, rather than as suggesting that we are identifying something other than minimum wage effects.

38. Of course, as noted earlier in this chapter, one criticism of minimum wages is that many minimum wage workers are members of high-income families, which suggests that some of the gains from a higher minimum wage accrue to these families. However, low-wage workers in high-income families account for only a small share of total family income. Calculations based on our data indicate that even minimum wage workers in families with income-to-needs ratios between 2 and 3 contribute only about 5 percent of overall income in those families, so that the effect of minimum wages on the income-to-needs distribution is likely to be quite small for families with income-to-needs above 3.

39. Of course, observed effects associated with a future policy change are conceivable, but these have never been established in the minimum wage employment literature.

40. Because we found that the extraction of pure contemporaneous and lagged effects made little qualitative difference in our basic results, we did not extract the pure from the contaminated effects for this robustness analysis. Also, because the estimates were so similar with the fixed state and year effects or reweighting to take account of differences in changes in unemployment rates, this analysis and the subsequent ones in this subsection are based on the approach used in figure 5.1.

41. To accommodate the widely differing sample sizes that result from this disaggregation, we pool the data for initial bandwidth selection following Marron and Schmitz's approach (1992). This keeps the level of smoothing equal for the analyses of families in different initial income-to-needs categories, whereas standard rules would result in more smoothed estimates for smaller sample sizes.

42. We report estimates only for the analysis using the entire matched data set without controls, reweighting, and so on.

43. They use estimated after-tax income divided by the number of adults. They report that their results are qualitatively unchanged if they divide by the total number of family members or just use family income.

44. Although the inclusion of year dummy variables would make the identification of some of the federal policy changes impossible, most of the policy effects would still be identified; in addition, a time trend might have been sufficient to capture the effect of changing wage inequality.

45. The results discussed thus far (and reported in the tables of the paper) refer to after-tax income. The authors note that for pretax income the results always point to a positive and statistically significant effect of minimum wages on inequality.

46. Based on different versions of the Atkinson index, the authors estimate that a 10 percent increase in the minimum wage leads to welfare losses of between $22 billion and $97 billion in 1981 dollars.

47. Although most of the work discussed in the World Bank report concerns microlevel evidence from different countries, the report also discusses two earlier studies that use data across countries and report some evidence suggesting that higher minimum wages reduce poverty (Lustig and McLeod 1997; Morley 1995). This research design appears not to have been adopted in subsequent work.

48. Poverty is defined in terms of earnings per capita in the family. The poverty line is based on a basic basket of goods including housing, education, and food. The extreme poverty line is based on a basic basket of food yielding a minimal calorie level.

49. Clearly longitudinal data could be helpful in sorting out whether such distributional effects occurred.

50. Colombia did not witness the hyperinflation that occurred in Brazil, so the explanation of these findings must lie elsewhere. The authors, however, do not discuss this issue or provide much information with which to try to determine what is driving these estimates.

51. These parameters determine the level of income at which the credit falls to zero, which was $36,348 in 2006 for a family with two children.

52. This is true for those without children as well, although this group is typically ignored, as single individuals are eligible only for a small EITC payment (and have been only since 1993).

53. Indeed, there is some evidence suggesting that the EITC has adverse effects on low-wage workers ineligible for its benefits (or eligible for only modest benefits), because the outward labor supply shift from labor market entry pushes down wages of low-wage workers already in the labor market. For example, Leigh uses variation in both federal and state EITC rates and finds that increasing the generosity of the EITC reduces wages of low-skilled workers by enough so that the decrease in wages "probably exceeds the full value of the credit" (2007, 18) for those already working. (Note that this does not contradict the evidence discussed next, from Neumark and Wascher 2001a, that a more generous EITC reduces poverty even when we focus on earnings only, because of the increased earnings among those who enter the labor market.) Rothstein (2007) studies the expansion in the federal EITC in 1996, which requires controlling for trends in wages for different skill groups attributable to technological change and other aggregate influences, and similarly concludes that the EITC puts considerable downward pressure on wages of low-skill workers.

54. This evidence is based on specifications that rely solely on the state-level variation in EITC parameters. It is difficult to separate out the influence of the federal EITC from the effects of other policy changes and other aggregate changes.

55. As reported in the paper, we find that the effect tapers off considerably with increased generosity.

56. Paralleling their treatment of the minimum wage, they use the difference between the logs of the state and federal maximum benefit, although it is not clear for what type of family, or whether this is averaged over families in each state-year cell.

57. Wu, Perloff, and Golan conclude that higher marginal tax rates have larger beneficial redistributional effects than the EITC. In addition, they find either no effects or adverse effects of AFDC and food stamps, which they attribute to work disincentives.

58. For those families with no workers and for whom the EITC does *not* induce employment, the EITC of course delivers no benefits.

59. The purpose of comparing equal cost policy changes is to abstract from efficiency considerations. In reality, however, the general equilibrium effects of these policies are difficult to fully measure, and thus the deadweight loss could well differ across the specific policies they consider.

60. Their results show that second-order dominance results cannot be established for the payroll tax rebate relative to the EITC or the minimum wage, but that the EITC third-order dominates the tax rebate. Third-order dominance is based on a stronger transfer principle—that the same transfer from a higher- to a lower-income family does more to raise welfare if it occurs lower down in the income distribution. This influences the ranking of the EITC relative to the payroll tax rebate, because the EITC increases income considerably more for lower-income families, but a little less for families between 150 and 200 percent of the poverty line. The minimum wage and payroll tax rebates cannot be ranked in terms of third-order dominance.

61. At the same time, there are many commendable aspects of the Formby, Bishop, and Kim study, including the welfare evaluation of changes in the distribution of incomes (and statistical inference about these changes), and the attention to alternative measures of poverty.

6 The Effects of Minimum Wages on Skills

1. The same could be said about the longer-run effects of minimum wages on family income and poverty, a question that has not yet been studied.

2. On the other hand, higher wages or earnings in the longer run do not necessarily imply that affected workers experience increased utility, because their higher wages come at the cost of higher investments, and the time path of income (and probably consumption) is affected.

3. For example, Cahuc and Michel (1996) develop a model in which human capital generates positive externalities; in such a case, a higher minimum wage that induces greater net skill acquisition will increase welfare. In a similar vein, Agell and Lommerud (1997) write down a dual-sector model in which deviations from the first-best solution can lead to a situation in which minimum wages may increase education for some workers and decrease it for others, and can increase welfare even if a higher minimum reduces schooling on average.

4. The U.S. Code of Federal Regulations (sections 785.27–785.32) states that compensation is required unless training is outside regular working hours, is voluntary, results in no productive work, and is not directly related to the employee's job. Thus, for example, training offered by an employer that gives the worker skills for a different job, rather than improving skills in the current job, would not necessarily be compensable.

5. Perhaps reflecting this concern, minimum wage laws sometimes include a "training wage" provision that allows for a lower wage during some specified period very early in a worker's tenure. For example, Minnesota's current minimum wage is $6.15 for large

employers, $5.25 for small employers, and $4.90 for workers under age twenty in their first ninety days of employment (http://www.doli.state.mn.us/minwage.html, viewed January 1, 2007). Federal legislation underlying the 1990 and 1991 minimum wage increases established an 85 percent youth subminimum for workers under age twenty, which expired in 1993, and with the 1996 and 1997 increases created a youth subminimum of $4.25 applicable during a worker's first ninety days with an employer (http://www.dol.gov/esa/minwage/coverage.htm, viewed January 1, 2007). The United Kingdom has a lower "development rate" (£4.45, versus the £5.35 minimum) for eighteen- to twenty-one-year-olds in a new job who are receiving accredited training during the first six months of employment, and a youth subminimum wage (£3.30) for sixteen- to seventeen-year-olds (http://www.hmrc.gov.uk/employers/rates_and_limits.htm, http://www.hmrc.gov.uk/nmw/#b, viewed January 1, 2007).

6. As long as workers bear part of the cost, though, the minimum wage will still reduce training in this model.

7. They raise this possibility in the context of schooling decisions (discussed in more detail later in this chapter), but it applies equally well to training.

8. The second paper also presents evidence suggesting that there are returns to specific training at other employers, which also contradicts the standard model but is less relevant to the issue of minimum wage effects.

9. That is, there is "wage compression" relative to marginal productivity.

10. The authors offer several examples of market imperfections that can generate the wage compression that is central to this result, including the presence of transaction costs in the labor market, asymmetric information between a worker's current employer and other potential employers about a worker's ability or level of training, asymmetric information between the worker and employer about the worker's effort, and interactions between general and specific training. Although we recognize that these factors may in principle lead to wage compression, we would also note that much of the extensive literature on the slopes of wage profiles relative to productivity profiles suggests that wages rise as fast or faster than productivity. See, for example, Hellerstein and Neumark 2007 and Kotlikoff and Gokhale 1992.

11. In contrast, in the competitive model, the firm will not retain a worker whose marginal product is below the minimum wage. The firm could choose to train such a worker, and up to a point (the minimum wage) his productivity would increase without necessitating a wage increase. But it is cheaper to hire a worker whose productivity equals the minimum wage.

12. In addition, employers could in principle initially raise wages to comply with a higher minimum, and then over time train workers to bring their productivity up to the now higher wage. In that case, the minimum wage leads to more training but a flatter wage profile.

13. Of course, even for these studies, the measurement of training is no simple matter. In particular, as Schiller (1994) points out, reported training measures are likely to understate informal training, which may be especially prevalent for young, unskilled workers.

14. Other material in the paper describes the rate of advancement out of minimum wage jobs (via wage growth), a question also explored by Carrington and Fallick (2001) and earlier by Smith and Vavrichek (1992).

15. For the training to obtain one's current job, respondents are also asked about in-school training.

16. We do not want to control explicitly for workers' industries, because the industrial composition of employment could in principle be influenced by the minimum wage and its effects on training.

17. Other individual-level controls are also included.

18. It is possible that minimum wage effects on employment give rise to changes in tenure of currently employed workers, and this change in tenure could have implications for the observed training differentials associated with minimum wages. We suspect this is a second-order problem; even so, it would not invalidate conclusions regarding how minimum wages affect the average incidence of training among workers. Addressing this question explicitly would require highly detailed tenure information, given the relatively high turnover among young workers.

19. Acemoglu and Pischke (2003) take this further, and argue that our estimates are implausibly large. As we discuss in Neumark and Wascher 2001b, however, we believe that the implied effects they show are exaggerated, and that the implied effects we report here are not unreasonable.

20. There are many other differences in the training questions, although it is not obvious how they would influence the estimates.

21. This evidence would be more decisive if we could also show that for twenty- to twenty-four-year-olds in the NLSY79, the evidence pointed to negative and significant minimum wage effects. However, there are very few observations in this age group covered by their data, especially at the younger end. Specifically, there are no twenty- to twenty-one-year-olds, twenty-two-year-olds only in 1987, twenty-three-year-olds only in 1987–1988, and twenty-four-year-olds only in 1987–1989.

22. This is presumably for workers who receive training, although the survey question does not make this clear.

23. In fact, the survey question asks whether the worker's wage was increased to bring them up to the new minimum, "or has it remained the same?" We are not quite sure how workers whose wage increased to above the new minimum (whatever their initial wage) would have responded to this question.

24. In a longer unpublished version of this paper (Arulampalam, Booth, and Bryan 2004b), these authors also implement the richer specification proposed by Acemoglu and Pischke (2003) to allow the effects of the minimum wage on training to vary with rents (for which they construct a proxy using industry wage differentials). Although not significant, the estimates are in the opposite direction to what Acemoglu and Pischke find, with a positive effect of minimum wages on training for workers in lower-wage industries, and vice versa. They also implement a related test that exploits variation in the size of different geographic labor markets, and find a larger positive effect in smaller markets, which they suggest is consistent with larger positive effects when firms have more monopsony power. We have no idea whether this hypothesized relationship between the size of the labor market and monopsony power is in fact true, especially with a cutoff of 500,000 for the size of the labor force between larger and smaller markets.

25. Note that this contrasts quite sharply with the findings in Neumark and Wascher 2001b.

26. A higher minimum wage could also reduce schooling if teenagers are myopic (or have a very high rate of time discount) so that they overemphasize the potential for higher earnings in the minimum wage sector and give insufficient attention to the cost in terms of foregone higher earnings from more education.

27. And technically, of course, there are people who work full-time but are still investing in education.

28. This type of question about the variation in effects of minimum wages on enrollment for youths in families with different income levels is quite important, but has not been taken up in subsequent research.

29. As discussed in chapter 3, this inquiry was motivated in part by criticism of the enrollment rate we used in our 1992 analysis, by Card, Katz, and Krueger (1994).

30. The estimates reported here are from table 1 of Neumark and Wascher 2003, which corrects a minor programming error in our 1995a paper.

31. Average weekly hours are higher for those who are nonenrolled and employed than for those who are in school and employed. In addition, the average increase in weekly hours for those making the transition from in school and employed to nonenrolled and employed is 11.8 hours.

32. We find some parallel results based on the prior year's wage, although the sample is considerably smaller and the estimates less precise.

33. A potential advantage of the SIPP data for this purpose is its better longitudinal coverage of teens than is available from matched CPS files, which do not include teens who change addresses.

34. There were eight state minimum wage increases in this period. However, four of the increases occurred in small states that are not identified in the SIPP and hence are excluded from the analysis.

35. The results they report for the specifications that estimate separate effects by race or age are clearly problematic, with some standard errors that are very large.

36. They do find a disemployment effect, but it reflects decreased employment opportunities for both the student and nonstudent populations in Canada.

37. This study seems to use data from a number of sources, unlike the Baker study, which uses data from the Canadian Labour Force Surveys. The differences are more surprising because (according to Baker 2003, 27), Landon's enrollment data are as of September of each year, whereas Baker uses enrollment as of April and as of October, and the more negative estimates come from the April data.

38. Portugal and Cardoso (2006), studying data for Portugal, present some indirect evidence on the influence of minimum wages on schooling. In particular, they report evidence suggesting that a higher minimum wage reduces job separations and accessions among teenagers, whereas they point out that the types of enrollment and employment changes we found for the United States would suggest increased separations and increased accessions. Of course, the relationship between secondary schooling and the employment behavior of teens in Portugal could be quite different from that in the United States, so we might not expect results to carry over, although we might reasonably expect more similarities between the United States and Canada.

39. Longer-run effects that counter some of the potential adverse short-run effects are also possible, if minimum wages raise training or increase schooling. But the evidence in the previous two sections provides no reason to believe these positive effects occur. Pettengill (1981) also raises the possibility that a higher minimum wage, by leading workers to exert more effort on the job, may lead to the development of better work habits among youths.

40. The research uses data from CPS ORG files for 1979 to 2001, but minimum wages going back to 1973 to construct the minimum wage history. It does not consider the longer-run effects of minimum wages for individuals past age twenty-nine, because for them, exposure at young ages would have come from the early part of the sample period when there was very little state variation in minimum wages. For example, the latest birth cohort of thirty-four-year-olds in the last year of the sample left its teens by 1987, before most of the state variation in minimum wages began. Even in the absence of this problem, there would be relatively few complete sets of observations on these older cohorts all the way back to age sixteen.

41. The minimum wage history is necessarily based on the state in which the individual currently resides, because the CPS has no migration information. This introduces measurement error with respect to the true minimum wage history, because of state-to-state migration, but the authors present evidence that this type of measurement error does not generate biases that explain their results. Longitudinal data that followed individuals as they moved from state to state would better capture their minimum wage history, but would perhaps be more plagued by the endogeneity of migration. And the existing longitudinal data sets provide relatively few observations, or do not cover many teens and young adults during periods when state minimum wages were frequently above the federal level.

42. However, for young adults with a high school degree or less, there is also a significant negative employment effect of the contemporaneous minimum wage.

43. In contrast, the existing literature on contemporaneous employment effects (mainly older time-series studies) does little to establish stronger disemployment effects for minorities (Brown 1999). In estimates using these data and a specification similar to equation (6.4), but focused only on contemporaneous effects, the estimates for black young adults indicate larger disemployment and hours effects, although these estimates are not significant.

44. They omit contemporaneous unemployment rates to avoid endogeneity.

45. Another relevant set of influences on young adults' labor market experiences is changes in welfare and taxes. The latter part of the 1990s witnessed sharp changes in welfare and tax policy that strongly affected work incentives among single mothers. However, this is unlikely to drive the results. For twenty-five- to twenty-nine-year-olds, very little identifying information comes from the late 1990s, as the sample ends in 2001 and the regressions estimate the effects of minimum wages many years earlier. Also, if the minimum wage effects reflect changes in these other policies, we might expect the effects to be more apparent for women. In contrast, the evidence of longer-run effects of minimum wages was relatively similar for males and females, and if anything somewhat stronger for males.

46. Details are provided in the paper (Neumark and Nizalova 2007).

47. The longer-run effects of minimum wages are also qualitatively consistent with work by Mroz and Savage (2006) indicating that—after accounting for heterogeneity that may generate a correlation between individuals' employment experiences at different ages—

early spells of unemployment experienced by youths result in wage declines that taper off only slowly over time, lowering wages as much as ten years later (despite some increased training and work activity to mitigate the effects of earlier unemployment). Although Mroz and Savage do not focus on minimum wage effects, and the effects of minimum wages that Neumark and Nizalova find are not limited to those acting through lowered employment among teens and young adults, there is substantial overlap in the finding that factors generating worse youth labor market outcomes can have longer-lasting negative effects.

7 The Effects of Minimum Wages on Prices and Profits

1. Although the federal minimum wage in the United States is not indexed, whether it should be is a perennial issue of discussion, and indexing has, at times, been included in proposed legislation.

2. See, for example, Chasanov 2004.

3. For a critical review of the high-wage doctrine, see Taylor and Selgin 1999.

4. See, for example, Hamermesh 1993, Card and Krueger 1995a, and Aaronson and French 2007, among others.

5. The decline in low-skilled employment depends both on the elasticity of substitution between low-skilled labor and other factors of production and on the elasticity of demand for output.

6. However, Bhaskar and To (1999) also show that allowing for firm exit in the monopsonistic competition model can lead to an increase in both firm-level employment and prices in response to a higher minimum wage. In particular, though raising the minimum wage will induce each individual employer to raise employment in this model, the increase in the wage floor may cause some firms to exit the industry, so that the degree of monopoly power in the product market increases. In this case, both prices and employment may eventually rise at the surviving firms as a result of their greater pricing power; industry-level employment may be higher or lower depending on the substitutability of labor and capital. See also Shepherd 2000.

Wessels (1997) offers another way in which employment and prices could both rise in response to a minimum wage increase in the restaurant industry. In particular, he notes that in a model in which there are tipped servers, tip income is inversely related to the number of servers, so that base wages have to be raised for all workers when employment increases. This model implies that a minimum wage increase for tipped workers will raise their employment over some range, but because average wage costs also increase, will cause prices to rise as well.

7. Stigler (1946) briefly discusses both the efficiency wage and shock theories of minimum wages.

8. For example, Nordlund (1997) notes that the Department of Labor assessed the linkage between minimum wages and prices in several industries following the 1961 and 1966 FLSA amendments.

9. Friedman (1968) originally hypothesized that a minimum wage would raise the NAIRU. For a more recent investigation, see Tulip 2004, although his estimated effects seem quite large as compared to the range of estimates of the disemployment effects of minimum wages reported in chapter 3.

10. Sellekaerts incorporated her equations for wages and prices into a variant of the MPS large-scale econometric model of the U.S. economy. Using the actual MPS model, Falconer (1978) reported similar results.

11. In an update, Gordon (1988) reports a similarly sized, albeit statistically insignificant effect.

12. In addition, the Institute for Social Research at the University of Michigan conducted a survey of firms to elicit information on how employers responded to the 1979 and 1980 increases in the minimum wage. According to Converse et al., about one-third of respondents with employees who were paid the minimum wage indicated that they had "raised prices in response to the minimum wage increase, and almost two-thirds raised prices sufficiently to cover increased costs" (1981, 243). Of course, one should be skeptical of ex post surveys of employer behavior in response to a policy change, and, in this particular study, the lack of a relationship between the proportion of minimum wage workers at an establishment and the tendency to increase prices raises a warning flag.

13. We found only one study that explicitly examined the effect of the minimum wage on inflation in another industrialized county. That study, by L'Horty and Rault (2004), estimated a simple vector autoregressive model for prices, wages, and the minimum wage in France and found no evidence that changes in the minimum wage in France had any influence on price inflation in either the high-inflation period from 1970 to 1981 or in the period of disinflation from 1981 to 1999.

14. See, for example, Hooker 2002.

15. See, for example, Brayton et al. 1997.

16. Spriggs and Klein (1994) conduct a similar survey of fast-food restaurants in Jackson, Mississippi, and Greensboro, North Carolina, roughly one month before and one month after the April 1, 1991 increase in the federal minimum wage. Their results and conclusions are similar to those in the Katz and Krueger paper.

17. The regressions also include city-level labor market variables.

18. Aaronson speculates that lower rates of compliance at pizza establishments may account for the absence of a price response for that component of prices.

19. Previous research on the retail sector has produced varying results on whether increases in sales taxes are fully shifted onto consumers, with Poterba (1996) reporting a one-for-one passthrough and Besley and Rosen (1999) finding evidence of full passthrough for some goods and an overshifting of sales tax increases to consumers for other goods.

20. A low passthrough coefficient could also indicate that employers are offsetting the minimum wage increase in the nonwage part of total compensation.

21. Full passthrough is sometimes incorrectly interpreted as suggesting that an increase in the minimum wage has no effect on profits. However, the competitive model makes clear that industry profits will fall in response to a minimum wage increase, because the increase in output price leads to a decline in demand for the product.

22. Building on the research that pointed to price increases in the fast-food industry, O'Brien-Strain and MaCurdy (2000) simulate the welfare implications of a minimum wage increase under the assumption of complete passthrough and no employment effects. Using household-level data on income and expenditures in California, they find

that the price effects of the minimum wage are disproportionately paid by low-income households because such households spend a larger share of their income on goods and services produced by low-wage workers.

23. Note that this is not consistent with the suggestion we made in chapter 3 that prices may rise more at restaurants that compete with the fast-food establishments that Card and Krueger (1994) study. However, the entire full-service sector is clearly much larger than the set of potential competitors of fast-food restaurants.

24. Another study, by Machin, Manning, and Rahman (2003), looks at the U.K. residential care home industry, a nonunionized sector made up of many largely homogenous small businesses employing a large share of low-wage workers. However, the government pays the cost of care for a large fraction of residents in these facilities, and those payments were not increased when the minimum wage was raised. As a result, this study is more informative about the effects on employment when firms cannot pass through much of the minimum wage increase into prices than it is about the size of the passthrough.

25. The authors also report that only nineteen of the eighty-two minimum wage change observations showed a decline in prices in response to a higher minimum wage, and that only two observations (both from Denver) showed a decline larger than 0.2 percent.

26. Indeed, in some versions of this model, employment can even rise in response to a minimum wage increase (Rebitzer and Taylor 1995). However, such models would also predict a decline in prices, inconsistent with the empirical results reported by Aaronson, French, and MacDonald.

27. Of course, this result would not constitute a rejection of the competitive model of the low-wage labor market, but rather a potential secondary effect associated with heterogeneous consumers.

28. They also mention the possibility that firms might offset an increase in the minimum wage by reducing benefits. However, they argue that such effects would be inconsistent with their evidence of sizable price responses.

29. Lemos (2008) reports on some preliminary results from her research on minimum wages and inflation in Costa Rica. The methods in this study seem to be broadly similar to those in her study of Brazil, except that she uses an annual panel of industry price and wage data from 1987 to 1994. In this study, she finds little evidence of an effect of minimum wages on prices, although this may reflect the use of annual data and the short sample period used in the study.

30. As is standard in this literature, excess returns are defined as the prediction error from a regression of the daily return for a particular stock on a constant term and the overall market return for that day (measured here as the average of the returns on the NYSE and AMEX indexes). In Card and Krueger's analysis, average market returns are estimated using data for 1987 for the first event study and using data for 1992 for the event study that focuses on 1993.

31. However, they also find a positive and significant coefficient in the regression for the 1992–1993 period, which suggests that wage growth was faster over that period for firms with initially low wages. This result raises questions about their evidence, but the authors make no attempt to explain it.

32. The dataset includes information that allows the authors to construct estimates of total revenue and total costs for each firm.

33. In both cases, the control group is limited to firms with average wages above £12,000 but below £20,000 in order to make other characteristics of the firms more comparable.

34. The three-year period after the introduction of the minimum wage included two additional increases that raised the wage floor to £4.10 per hour by October 2001.

35. The authors also estimate the effect of the minimum wage on the probability of exit and entry among the firms most affected by the minimum wage. Despite the negative influence of the minimum wage on profit margins, they found no evidence that the minimum wage had a significant effect on exit or entry.

8 The Political Economy of Minimum Wages

1. See http://www.livingwagecampaign.org/index.php?id=1958 (viewed November 11, 2006).

2. In 1987, for example, the *New York Times* published an editorial titled "The Right Minimum Wage: $0.00."

3. See, for example, http://www.epi.org/content.cfm/minwagestmt2006 (viewed July 9, 2007).

4. See, for example, Keech 1977. Stigler, however, cautions against the interpretation "that noneconomists are slow and perverse in accepting the reasonably reliable findings of science," noting "that physical scientists do not encounter the difficulties in public adoption of their findings that we economists meet" (1976, 348). He instead attributes public confusion to the tendency for "each sector of the public [to] demand services from intellectuals favorable to the interests of that sector" (349).

5. Interestingly, despite its popularity among economists, the EITC does not enjoy the same degree of popularity among the public as does the minimum wage. For example, according to Bartels, "In the 2003 NPR/Kaiser Foundation/Kennedy School survey, almost 40 percent of respondents admitted that they had never heard of the Earned Income Tax Credit or did not know what it was. In 1995, when the Republicans floated the possibility of reducing the EITC, only a bare majority of survey respondents opposed doing so, while 25 percent said they favored eliminating it" (2006, 419–420). We suspect that Americans' tepid response to the EITC reflects both the fact that it is more difficult to understand than the minimum wage and the fact that a more generous EITC has the effect of increasing the federal budget.

6. For example, a *Wall Street Journal* article in August 1995 indicated that twice as many small businesses indicated that they would reduce hiring in response to a 90 cent increase in the minimum wage than said that they would be forced to lay off workers.

7. Obviously, this incentive is more applicable to unions that represent workers in low-wage industries (e.g., textiles or janitorial services) than it is to unions representing workers in higher-wage industries (e.g., autos or steel).

8. See, for example, http://money.cnn.com/2005/10/25/news/fortune500/walmart_wage/ (viewed November 15, 2007). Although Wal-Mart is typically described as a low-wage employer, survey evidence suggests that it pays well above the minimum wage (Global Insight 2005), and it likely competes with other retailers paying the minimum wage.

9. Saint-Paul (1996) presents a similar model, although he focuses on the substitutability across different types of unskilled labor. In particular, in his model, unskilled labor consists of "insiders" and "outsiders," and the minimum wage raises the wages of insiders by reducing opportunities for low-skilled outsiders who would otherwise be employed.

10. These results are reported in Neumark, Schweitzer, and Wascher 2000.

11. These results point to substitution of union labor for nonunion labor in response to a higher minimum wage rather than the use of the minimum wage as a "reference wage" for collective bargaining. See also Farber 1981.

12. Although the Senate subsequently passed a very similar bill, President Nixon vetoed it. However, with presidential influence weakened by the Watergate scandal, legislation to raise the minimum wage and extend its coverage was eventually passed and signed into law in April 1974 (Nordlund 1997).

13. Although five representatives voted against both bills, the authors excluded them from the analysis because of their small numbers and the authors' view that the debate in Congress was largely over the extent of coverage and the timing of a minimum wage increase.

14. The authors emphasize the difference between their positive coefficient on average hourly earnings (which implies a positive correlation between wages and support for the minimum wage) and Silberman and Durden's finding of a positive coefficient on the proportion of low-wage workers in the state (which Kau and Rubin interpret as implying a negative correlation between wages and support for the minimum wage). Aside from Silberman and Durden's observation that the magnitude of the effect was small in their specification, the presence of other variables (e.g., small business share and region) in their analysis that are likely correlated with the average wage makes such a comparison difficult.

15. In contrast, they did not find any independent link between party affiliation and voting patterns, which they suggest reflects the opposing views of minimum wages held by northern and southern Democrats.

16. Bloch 1980 contains his original analyses of the 1966 and 1974 amendments.

17. He also argues that certain legislators—for example, more senior members of Congress and senators not currently up for re-election—may feel less obligated to vote according to the preferences of their constituent interest groups, especially for legislation that is less important as an election issue. Kalt and Zupan (1984) make a similar argument, with an application to voting on coal strip-mining policy.

18. Voting to send the bill back to committee is equivalent to voting against the minimum wage in this analysis.

19. This specification is derived from an equation that relates the earnings of minimum wage workers (Y) to the product of the minimum wage (MW) and the number of workers employed at the minimum wage (E), where employment is assumed to be a linear function of the minimum wage ($Y = MW \cdot \mathrm{E}(MW) = MW \cdot (\alpha + \beta \cdot MW)$). Sobel also emphasizes that lags in the adjustment of labor demand to the minimum wage will lead to different optimal values of the minimum wage in the short run and in the long run, perhaps leading shortsighted politicians to pursue an inappropriately high minimum wage target.

20. In 1970, for example, only three states (Alaska, California, and New York) had a level of the minimum wage that was higher than the federal minimum.

21. In this analysis, the authors included the share of personal income accruing to proprietors as their measure of business interests in order to capture those firms more likely to be adversely affected by the state minimum wage.

22. The intent of Zavodny's analysis was to obtain reliable instruments for the minimum wage rather than to analyze the political economy of minimum wages.

23. The initiative was placed on the ballot in response to the state legislature's repeated refusal to raise the minimum wage, and was approved with 76 percent of the vote.

24. Prince Edward Island was excluded because of missing data.

25. Nissen (2004a) notes that versions of living wage laws were passed earlier in Des Moines, Iowa, and Gary, Indiana, but that the Baltimore living wage in 1994 is typically considered the beginning of the modern living wage movement.

26. The city rankings are based on census population estimates.

27. We believe that the phrase "living wage" is best understood in the context of efforts to mandate a wage floor sufficient to lift a family to or above the poverty line, in contrast to a "minimum wage." However, the contrast is blurred, because some city living wages are quite low, and some city-specific minimum wages (see following) have been labeled living wages.

28. See Adams and Neumark 2005a, Neumark 2004, and Reynolds 2004 for more details on living wage laws' provisions.

29. See, for example, table 2 in Neumark 2004.

30. See the summary of coverage estimates in Neumark and Adams 2003a and Freeman 2005. For more systematic estimates of coverage by the living wage laws in Los Angeles and San Francisco, see Fairris et al. 2005 and Alunan et al. (1999).

31. Washington, D.C., has its own minimum wage, but is often treated as a state in state-level analyses of minimum wage effects like those described in earlier chapters.

32. Related results are reported in a sequence of papers, including Neumark and Adams 2003a and 2003b, and Adams and Neumark 2005a, 2005b, and 2005c.

33. The inclusion of fixed city and year effects helps to control for fixed differences in outcomes across cities, as well as changes common to all cities, which happen to be correlated with living wage changes, just as was done for states and years in the minimum wage studies discussed in chapter 3. Adams and Neumark (2005c) further improve upon the identification of living wage effects by restricting the control group to cities where living wage campaigns either came very close to succeeding or did succeed but subsequently had the law overturned by a court ruling or state legislation. This approach better holds constant the factors that might be associated with living wage campaigns and hence with living wage laws. However, the estimates were insensitive to using this restricted control group.

34. Criticism of this method of estimating wage effects, and a response, is provided in Brenner, Wicks-Lim, and Pollin 2002 and Adams and Neumark 2005b.

35. Adams and Neumark (2005a) study how the effects of living wage laws vary with other characteristics of these laws.

36. This brief review does not discuss a set of ex ante simulations of the effects of living wage laws, which were popular (and the only feasible option) when living wage laws

were in their infancy and there was no track record with which to do before-and-after analyses (many of these are reviewed in Pollin 2005).

37. Evidence of wage and employment effects above the 10th percentile of the wage or predicted wage distribution is weaker, although there are some hints of positive wage and employment effects between the 10th and 50th percentiles, consistent with substitution toward higher-skilled labor (Adams and Neumark 2005b, table 7).

38. Some of this discussion is based on a longer presentation of the results of this study (Reich, Hall, and Jacobs 2003).

39. For some of their other analyses, such as effects on turnover, the authors contrast results for jobs for which the living wage entailed larger or smaller wage increases. The employment analysis, however, presents no such comparisons.

40. The study also provides some evidence that the living wage reduced turnover and increased effort—which would partially offset some of the higher labor costs associated with the wage floor. On the other hand, Brenner (2005), discussed next, does not find that a higher living wage in Boston reduced turnover or absenteeism; if anything, the evidence points in the opposite direction. Nissen (2004a) provides some anecdotal evidence from Miami, which indicated that at least some employers reported greater loyalty and morale, but also stricter hiring standards, after the implementation of a living wage in that city. However, we would not expect such effects to fully compensate firms for the increase in labor costs, absent an explanation of why firms would not have chosen the profit-maximizing higher wage in the first place.

41. The survey also asked some direct questions about whether staffing levels for city contracts changed, and the results apparently gave no indication of declines in staffing levels due to the city's living wage policy. However, because the data only establish slower employment growth at affected firms, rather than employment declines, we would not necessarily expect absolute reductions in employment on city contracts.

42. This analysis is less than ideal, because of both the subjective nature of the employment change question for the treatment sample and the lack of any information on employment change for the control sample, which is therefore not used in this part of the analysis.

43. We are unaware of any comprehensive estimates of workers covered by financial assistance provisions, and there appear to be contradictory claims about the extent of coverage in the literature. For Los Angeles, Fairris et al. (2005) report that coverage via the financial assistance provision was minimal—affecting only a couple of firms and thus a small percentage of total workers subject to the living wage. In particular, they claim that most economic subsidies were channeled through the Community Redevelopment Authority, while the Los Angeles living wage law, originally passed in 1997, did not extend to this agency until 2003, and even then did not cover developers' tenants. On the other hand, Luce discusses the application of the Los Angeles living wage law to major economic developments in the city, including the retail workers employed after the developments are completed (2004, 122). See Brenner, Wicks-Lim, and Pollin 2002 and Adams and Neumark 2005b for additional discussion of conflicting evidence on financial assistance coverage. In addition, Nissen reports that more than half of workers covered by the living wage law in Miami-Dade County were workers at the airport "working under a permit with the county" (2004a, 168). Such workers are not necessarily the same as those employed by a recipient of financial assistance from the city, but nonetheless are not covered by contractor provisions; moreover, Nissen estimates that coverage in the

contractor sector is much lower than it should be, because of lax enforcement or narrow interpretation of the law's coverage (160).

44. At first glance, the larger (albeit insignificant) estimate for contractor-only living wage laws might seem puzzling in light of the smaller wage and employment effects for this type of law. One possibility is that it reflects the effect of contractor-only living wage laws on wages of higher-paid workers, discussed more later. In addition, the offsetting positive wage effects and negative employment effects of financial assistance living wage laws imply that these laws need not have a stronger effect on poverty.

45. More generally, the size of some of the effects that Adams and Neumark estimate may seem surprisingly large, given relatively low coverage by living wage laws. With respect to poverty reductions, Adams and Neumark (2004) explain that their estimates are of the same order of magnitude as those reported by Brenner (2005) in his study of the Boston living wage. With respect to wage and employment effects, the estimates are large given coverage estimates. However, one possible explanation is that living wage laws may affect community norms for wages, leading to wage increases for workers other than those covered by the laws; one channel for this type of spillover may be that firms desirous of future contracts or development subsidies believe it is advantageous to pay higher wages (Bartik 2004). Even so, some critics have grossly overstated the implications of these estimates for employment declines. In particular, Fairris and Reich (2005, 10) incorrectly calculate that the Adams and Neumark estimates imply huge employment losses of more than 90 percent. In fact, the 35 percent living wage increase that they consider for a financial assistance living wage law is estimated to lead to a 6 percent employment decline among those in the bottom decile of the skill distribution (0.35 multiplied by the employment effect of −0.076 reported in column (2) of table 8.2, divided by the 0.44 employment rate in the bottom decile of the skill distribution).

46. See http://www.letjusticeroll.org/member.html (viewed November 8, 2006).

47. See http://www.livingwagecampaign.org/index.php?id=2071 (viewed November 8, 2006). As Levi, Olson, and Steinman note, "Labor's support for the living wage allows it to target previously ignored employment sectors, such as low-wage service sectors where women and people of color predominate as well as employee groups whose employers have been particularly resistant to union organizing" (2002, 113).

48. Presumably, the argument is that because high rents are being earned, adverse economic consequences will be trivial. But high rents do not rule out a strong substitution response away from low-skilled workers.

49. A parallel might be the support of unionized auto workers for trade import restrictions, which could also turn out to benefit some groups of low-skilled workers.

50. For example, unions tend to support expansion of the federal EITC as well as state-level EITCs, despite the possibility that higher EITCs will raise the supply of nonunion workers to employers who compete with union shops for business.

9 Summary and Conclusions

1. See http://www.sevendaysatminimumwage.org/site/?page_id=25 (viewed June 4, 2007).

2. One can easily find such claims in the running commentary on various economics-related blogs on the Internet.

3. On the other hand, going back to one of the original objectives of the minimum wage, a legal wage floor may sometimes provide a means to prevent a few employers from paying certain disadvantaged or vulnerable segments of the workforce a wage below that warranted by the productivity of those workers.

4. See http://www.epi.org/minwage/epi_minimum_wage_2006.pdf (viewed June 4, 2007).

5. Recent examples include Aaronson and French 2007; Ahn, Arcidiacono, and Wessels 2005; Hyslop and Stillman 2007; Lemos 2004a; Singell and Terborg 2007; Skedinger 2006; and Stewart and Swaffield 2006. As an even more extreme example, Flinn (2006), in motivating his search model, cites *only* the evidence of Card and Krueger in characterizing what the literature says about the effects of minimum wages.

6. There is a semantic issue here. Both the EITC and the minimum wage attempt to raise earnings by effectively taxing someone else. Nonetheless, we think it is reasonable to view these policies as attempting to promote economic self-sufficiency by making work more remunerative, as opposed to providing income transfers to individuals not in the workplace.

7. The latest incarnation of wage subsidies in the United States is the Work Opportunity Tax Credit, which targets young workers and members of families receiving Temporary Assistance for Needy Families (TANF), as well as a few other groups (see http://www.uses.doleta.gov/wotcdata.cfm, viewed May 2, 2007).

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