Foundations and Trends® in
Microeconomics
Vol. 3, No 1–2 (2007) 1–182

DOI: 10.1561/0700000015

Minimum Wages and Employment

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Abstract

We review the burgeoning literature on the employment effects of minimum wages – in the United States and in other countries – that was spurred by the new minimum wage research beginning in the early 1990s. Our review indicates that there is a wide range of existing estimates and, accordingly, a lack of consensus about the overall effects on low-wage employment of an increase in the minimum wage. However, the oft-stated assertion that recent research fails to support the conclusion that the minimum wage reduces employment of low-skilled workers is clearly incorrect. A sizable majority of the studies surveyed in this monograph give a relatively consistent (although not always statistically significant) indication of negative employment effects of minimum wages. In addition, among the papers we view as providing the most credible evidence, almost all point to negative employment effects, both for the United States as well as for many other countries. Two other important conclusions emerge from our review. First, we see very few – if any – studies that provide convincing evidence of positive employment effects of minimum wages, especially from those

studies that focus on the broader groups (rather than a narrow industry) for which the competitive model generally predicts disemployment effects. Second, the studies that focus on the least-skilled groups that are likely most directly affected by minimum wage increases provide relatively overwhelming evidence of stronger disemployment effects for these groups.

1

Introduction

For much of the past century, the minimum wage has been a controversial subject among policymakers and economists in the United States. From even before its inception as a major element of the 1938 Fair Labor Standards Act, the minimum wage was a politically contentious issue, with early attempts by the states to establish a wage floor declared unconstitutional by the Supreme Court, and President Franklin Roosevelt's first attempt to legislate a federal minimum wage in 1933 similarly struck down. Eventually, however, Roosevelt prevailed and Congress passed the FLSA, setting the minimum wage at 25 cents per hour.

For economists, the new minimum wage represented a means of testing alternative models of the labor market. Indeed, during the period immediately following passage of the FLSA, a fierce debate raged between economists who claimed that the low-wage labor market at the time was best characterized as a competitive market (the "marginalists") and those who claimed that it was not (the "institutionalists"), in which the implications of the minimum wage were a central focus (Leonard, 2000). For example, Stigler (1946), while acknowledging that a higher minimum wage could theoretically raise employment in a labor market characterized by monopsony, argued that the competitive nature of low-wage industries suggested that the displacement of low-wage labor was a more likely outcome. In reply, Lester (1947) dismissed Stigler's model of competitive wage determination as inconsistent with existing business practices and argued that "reasoning about labor markets as though they were commodity markets seems to be an important explanation for erroneous conclusions on such matters as the minimum wage" (p. 146). In the aftermath of this exchange, other economists began to accumulate empirical evidence on the effects of the minimum wage, with much of this research suggesting that increases in the wage floor were having adverse effects on the employment opportunities of low-skilled workers (Goldfarb, 1974).

Despite the unsettled debate within the economics profession, the Congress expanded coverage of the minimum wage significantly during the 1960s and 1970s, and by 1975, more than 90 percent of the workforce was effectively covered by the federal minimum wage, up from 63 percent in 1961 (Brown, 1999). In addition, Congress steadily increased the minimum wage over this period from $1.00 per hour in 1960 to $2.30 by 1979, with the 1977 FLSA amendments further raising the minimum to $3.35 by 1981. However, these changes were enacted in an environment of considerable discord among policymakers about the appropriateness of raising the minimum wage, and the ongoing political debate about the costs and benefits of a wage floor led the Congress in 1977 to create the Minimum Wage Study Commission to "help it resolve the many controversial issues that have surrounded the federal minimum wage and overtime requirement since their origin in the Fair Labor Standards Act of 1938" (Minimum Wage Study Commission, Vol. 1, p. xiii).

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), Vol. 1, Letter of Transmittal). Although not its only focus, the report included a lengthy chapter summarizing the existing research on the employment effects of the minimum wage. This chapter was based on a review of the literature by Charles Brown, Curtis Gilroy, and Andrew Kohen (BGK), three of the senior economists on the Commission staff.

These authors subsequently published a revised version of their review in the June 1982 issue of the *Journal of Economic Literature*, in which they summarized the existing research as suggesting that "time-series studies typically find that a 10 percent increase in the minimum wage reduces teenage employment by one to three percent" (p. 524). This range subsequently came to be thought of as the consensus view of economists on the employment effects of the minimum wage.

Given this apparent consensus, economic research on the effects of the minimum wage came virtually to a halt following the report of the Minimum Wage Study Commission and the publication of BGK's survey. However, by the end of the 1980s interest in this topic began to return. Two related circumstances, in particular, seem to have stimulated renewed attention to the effects of the minimum wage. First, the absence of any increase in the federal minimum wage from January 1981 until April 1990 resulted in more than a 30 percent decline in its value in real terms and led to a growing political debate toward the end of the 1980s about the merits of raising the nominal minimum. Second, an increasing number of state governments began to raise state-specific minimum wages above the federal level in response to the lack of action by the Congress. These state-specific increases added to the political debates about the merits of a mandated wage floor. Moreover, these developments also increased the statistical variation in the policy variables traditionally used in minimum wage research, offering a means of reexamining the evidence on which the existing consensus had been based.

As a result of both the renewed prominence of the minimum wage in public policy debates and the additional evidence that could potentially be used to study the economic effects of wage floors, researchers in the early 1990s began to reexamine the effects of the minimum wage on employment. One 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 developed by time-series econometricians to take account of criticisms leveled at the specifications used in the earlier literature. However, a second, and arguably 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. Indeed, despite improvements to the specifications of time-series models, the dearth of variation in the federal minimum wage and the use of aggregate U.S. data to look for its effects continued to be viewed as shortcomings of the existing body of research on the economics of the minimum wage (for example, Kennan (1995)), while other economists raised concerns about the lack of a well-defined counterfactual in the aggregate time-series studies, the potential endogeneity of changes in the federal minimum wage with respect to aggregate labor market conditions, and the difficulty in choosing an appropriate set of control variables in such studies (Card and Krueger, 1995a). Moreover, even aside from the problems with the aggregate time-series studies discussed in the existing literature, 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.

We focus our attention on this more recent literature, which has become known as the "new minimum wage research." Because the earlier literature on the employment effects of the minimum wage was carefully and extensively summarized by BGK, it seems unnecessary to repeat that review in this monograph. In contrast, there is no comprehensive review of the extensive literature that has emerged over the past 15 years.[1] We thus begin our review with the set of four papers that comprised the initial round of the new minimum wage research on the employment effects of the minimum wage. We follow that review with a discussion of the major conceptual and empirical issues that arose out of that initial research, and extend our summary of the U.S. literature with a review of the research that examines more recent increases in minimum wages or otherwise extends the literature. We then complete

[1] There are, however, papers offering critical summaries of some of the first wave of this literature. See, for example, Card and Krueger (1995a,b), Kennan (1995), and Brown (1999).

our review with a discussion of the empirical research on the employment effects of the minimum wage in other countries, an area of inquiry that has also grown markedly over the past decade.

Our intent in reviewing this literature is threefold. First, most of the political debate surrounding proposed changes in the minimum wage concerns the potential effects on employment. Although we do not view that focus as entirely appropriate, the fact that the employment question takes on such importance means that the answers should be based on a comprehensive survey of the literature, recognizing that minimum wage effects may differ across different segments of the population and in different economic circumstances and contexts. We therefore attempt to draw general conclusions about the effects of the minimum wage on employment that are relevant to policymakers, pointing out, in particular, in what context and for which workers the minimum wage will have consequences.

Second, we hope that our review will help readers assess alternative models of the labor market. The recent literature has reopened the debate about the appropriate theoretical description of the low-wage labor market, with some of the empirical research characterized as rejecting the competitive model in favor of other formulations. As we note throughout the monograph, economic theory often fails to make an unambiguous prediction about the employment effects of minimum wages. Even in the neoclassical model, the effect of the minimum wage on any given set of workers will depend on, among other things, the elasticities of substitution across different types of workers and cross-elasticities of demand across different types of goods. However, some empirical tendencies tend to match up better with one model or the other, and we try to provide a sense of what these tendencies are.

Third, many economists or policymakers perusing the literature may find it quite difficult to draw conclusions from the existing evidence. More than 100 studies have been published on the effects of minimum wages on employment since the 1990s, and the findings from this newer research are summarized differently in different places. In some cases, the new minimum wage research is described as failing to find evidence of disemployment effects. For example, Bazen (2000) states that “(t)he latest studies of the experience of the USA and the

UK in general find no evidence of negative effects on youth employment" (p. 64). Somewhat more cautiously, Flinn (2006) writes that "these recent studies have been particularly useful in indicating that the "textbook" competitive model of the labor market ... may have serious deficiencies in accounting for minimum wage effects on labor market outcomes ... " (pp. 1013–4). In contrast, others summarize the findings as more ambiguous, suggesting that *no* conclusions can be drawn, and that positive effects may be as likely as negative effects. Lemos (2004), for example, asserts that "there is no consensus on the direction and size of the effect on employment" (p. 219), while Stewart (2002) notes that some studies find employment effects to be "absent or positive" and that others find "significant negative effects" (p. 585).[2] In contrast, much of our own work tends to find negative employment effects for the lowest-skilled groups. Given the differences in the conclusions one might draw depending on what one reads, and the difficulties of wading through the mass of recent studies, we thought it would be useful to present a comprehensive review of the more recent minimum wage literature that provides an accurate accounting of the range of estimates in existing studies, and attempts to understand some of the sources of differences in results across studies, in addition to determining what conclusions can be drawn.

In putting together this review, we have intentionally eschewed a formal meta-analysis in favor of a traditional narrative review that attempts to provide a sense of the quality of the research and tries to highlight and synthesize the findings that we regard as more credible. Given the many different types of employment effects estimated

[2] Groups advocating for minimum wage increases often make stronger statements. For example, Chapman (2004), in an Economic Policy Institute report, asserts that "[T]here is no valid, research-based rationale for believing that state minimum wages cause measurable job losses" (p. 2). This claim appears to be based on a highly selective reading of the minimum wage literature based mainly on the New Jersey–Pennsylvania fast-food study (Card and Krueger, 1994). However, the literature on minimum wage effects in the United States is far broader than this one study, and clearly much of it does point to disemployment effects. Moreover, as discussed later, evidence on employment effects from a single sector is not necessarily informative about the employment effects of minimum wages more generally. Of course, advocacy groups opposing minimum wage increases also cite the research literature selectively, although we have not come across the same kind of misleading summary statements about the literature as a whole.

in the literature, and the considerable variation in approaches and in the quality of the research, lumping the studies into one meta-analysis does not seem the best way to make sense of the literature. Moreover, meta-analysis is even less useful when the underlying theory does not provide uniform predictions about the effects of the minimum wage in every study. Thus, while we recognize that a narrative review introduces an element of subjectivity into the discussion, we felt that it would be more useful to present our arguments and assessments of the evidence, and invite readers to form their own opinions based on them. To assist in digesting what is a very lengthy review of the evidence, we have collected nearly all of the studies we summarize into a set of four tables arranged by the different types of studies. For each study, we include a brief summary of the minimum wage variation and the group studied, the data used, the results, and what we regard as the most important criticisms. In these tables, we also highlight the studies that we regard as providing the most convincing evidence on the employment effects of minimum wages.

2

Origins of the New Minimum Wage Research

The origins of the new minimum wage research date to November 1991, when the ILR-Cornell Institute for Labor Market Polices and Princeton University's Industrial Relations Section sponsored the "New Minimum Wage Research Conference," at which a new and innovative set of studies on the economic effects of the minimum wage were presented and discussed.[1] Subsequently, these papers, along with an additional paper contributed after the conference, were published together in a symposium in the October 1992 issue of the *Industrial and Labor Relations Review.*

Even fifteen years later, the papers included in this symposium provide 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 this research. In particular, the studies in the symposium included the use of both state and time-series variation over relatively long sample periods (Neumark and Wascher, 1992), the use of regional variation in employment and wages surrounding a particular increase in the federal minimum wage (Card, 1992b), an analysis of

[1] The conference was organized by Ronald Ehrenberg and Alan Krueger.

an increase in a particular state's minimum wage (Card, 1992a), and a survey of fast-food restaurants before and after an increase in the minimum wage (Katz and Krueger, 1992).[2] The findings from this research ranged from disemployment effects similar to those comprising the earlier consensus (Neumark and Wascher) to no effect on employment (Card, 1992b) to a positive effect of the minimum wage on employment (Card, 1992a; Katz and Krueger). Given the relative newness of the methods employed in the papers, all of the contributors to the symposium were appropriately cautious about interpreting their results. But Ron Ehrenberg correctly predicted that these papers would be "cited often in future policy debates over minimum wage legislation" (Ehrenberg, 1992, p. 5).

[2] The symposium also included a paper by Smith and Vavrichek (1992). However, this paper focused on the wage mobility of minimum wage workers rather than on employment effects and so is not included in our review.

3

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

We begin our review by summarizing the studies included in the *ILRR* symposium and a related series of follow-on studies that appeared soon afterwards and that employed methods similar to this initial round of studies. To help set the stage for our subsequent discussion of the literature, we find it useful to separate these papers into two broad categories: (1) panel data studies that used state-specific data over time for the United States as a whole, and (2) case studies that focused on the effects of minimum wage changes in specific states.

3.1 Panel Data Studies

In response to the increasing willingness of state legislatures to raise state-specific minimum wages above the federal wage floor, two papers in the *ILRR* symposium – as well as a number of subsequent studies – attempted to exploit both time and state variation in minimum wages to identify the effects of the minimum wage on employment. In a broad sense, this approach applies the traditional empirical specification used in the earlier time-series literature:

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

to a data set consisting of state-year observations on employment (Y); a minimum wage variable (MW); and a vector of control variables (R) that may include state (i) and time (t) effects. 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 later. 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. 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.

The first paper we consider is Card's (1992b) study of the employment effects of the April 1990 increase in the federal minimum wage. Card recognized 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. Taking advantage of this variation, Card first regressed the change in the mean log wage of teenagers between the final three quarters of 1989 and the final three quarters of 1990 on the fraction of teenagers in each state who earned between \$3.35 per hour and \$3.79 per hour in 1989 and thus were more likely to be directly affected by the federal minimum wage increase in 1990. The results indicated that mean teen wages rose more in states where a greater fraction of teenagers were affected by the minimum wage increase, with the size of the wage increases similar to what would be expected if the affected workers' wages moved up to the new minimum wage of \$3.80 per hour. In contrast, when Card regressed the change in state employment-to-population ratios on the fraction affected variable (and a control for aggregate labor market conditions), the results indicated no effect of the 1990 minimum wage increase on teen employment.

The second such paper in the *ILRR* symposium is Neumark and Wascher (1992), in which we used specification (3.1) to estimate the effects of changes in the minimum wage on the employment-to-population ratio of teenagers (16–19 year-olds) and the broader youth population (16–24), using an annual panel of state-specific observations from 1973 to 1989 for large states and from 1977 to 1989 for smaller states. The minimum wage variable used in this study was similar to the Kaitz index used in the earlier time-series literature, except that it incorporated state-specific minimum wages when they were above the federal level. In particular, we constructed a coverage-adjusted minimum wage for each state-year observation as the higher of the federal or state minimum wage level, multiplied by federal coverage for the state, and divided by the average wage in the state. In addition to state and year effects, the control variables included the unemployment rate for men aged 25–64, the proportion of the population in the relevant age group, and, in some specifications, a school enrollment rate for the age group.

In contrast to Card's paper, the results in our paper generally supported the earlier consensus that increases in the minimum wage reduce employment among youths. In particular, our estimates of the employment elasticities with respect to the minimum wage ranged from about -0.1 to -0.2 for teenagers and from -0.15 to -0.2 for the youth population as a whole. In addition, we found that the presence of a youth subminimum in particular states tended to reduce the impact of the minimum wage in those states, as the standard model would predict.

3.2 The Case Study Approach

The other major line of inquiry that emerged as part of the new minimum wage research consisted of studies that focused on minimum wage increases in particular states. Two papers in the *ILRR* symposium took this approach: Katz and Krueger's study of the effects of the 1991 increase in the federal minimum wage on fast-food restaurants in Texas, and Card's (1992a) study of the 1988 increase in California's minimum wage. The details of the empirical approach in this subset of the literature have varied. But a unifying theme was the authors' views that limiting the analysis to a particular state afforded the opportunity to devise a natural experiment for studying the minimum wage increase because of the availability of valid control groups with which to compare the group directly affected by the minimum wage increase being studied. Although we disagree with that characterization – we would instead argue that these studies are similar in principle to the panel data studies discussed above and differ primarily in the construction of the control group rather than in the overall experimental design – 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.

We start by summarizing Katz and Krueger's (1992) study of the effects of the 1991 increase in the federal minimum wage on employment in the fast-food industry in Texas. These authors conducted telephone surveys of fast-food establishments in both December 1990 and August 1991 and asked the manager or assistant manager of each restaurant a series of questions about wages and employment. Exactly 100 restaurants provided sufficient information for the employment analysis – the number of full-time employees, the number of part-time employees, and the average starting wage for non-management employees. Katz and Krueger defined 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. In this setup, the difference in employment changes between restaurants initially paying relatively higher wages and those paying relatively lower wages identifies the effect of the minimum wage on employment. Estimating a regression that includes controls for city size, chain, and whether or not the restaurant was company owned (as opposed to a franchise), Katz and Krueger found a large positive and statistically significant effect of the minimum wage on employment, with estimated elasticities that ranged from 1.70 to 2.65. They noted that "a model in which the employers of low-wage workers are assumed to have market power and act as monopsonistic buyers of labor is potentially consistent with [their] findings" (p. 17), although they also write that "a potential problem with this monopsony interpretation of our employment findings is that a large degree of monopsony power seems somewhat implausible in the high-turnover labor market of the fast-food industry" (p. 18).

Card (1992a) took a different approach, using 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. In particular, he compared employment changes from 1987 to 1989 in California with employment changes in a group of comparison locations in which the minimum wage did not increase; 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. Using a difference-in-difference estimator, Card found that teen employment in California increased more rapidly than teen employment in the set of control areas and that this difference was statistically significant; the implied elasticity from the estimates is about 0.35. Card also found a relative increase in employment in retail trade in California between 1987 and 1989, and although he did find a small relative decline in the eating and drinking industry in California, he interpreted it as more likely stemming from differences in longer-run trends than the effect of the minimum wage increase. As did Katz and Krueger, Card raised the possibility that these results might indicate the presence of monopsony power in the low-wage labor market.

Given these provocative findings and the interest in using so-called natural experiments to measure the effects of policy changes, similar studies of the minimum wage quickly followed. For example, Spriggs and Klein (1994) took an approach similar to that of Katz and Krueger and conducted telephone surveys 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. More specifically, they asked respondents for information on employment levels, starting and average wages, prices, turnover rates, and their use of the subminimum wage. Defining the wage gap as the average wage increase needed to raise the wages of workers earning below $4.25 per hour in March to the new minimum wage in April, the authors reported estimates of the employment response that were negative but not statistically significant. Defining the minimum wage variable as the implicit change in the overall average wage associated with boosting the wage of those initially paid below $4.25 per hour, they found positive employment effects although, again, the coefficients were not statistically significant. The authors interpreted these results as inconsistent with the conventional view of minimum wage effects.[1]

Of course, by far the best known and most influential 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. These authors followed Katz and Krueger's approach and conducted surveys of 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 minimum wage increase. For stores in New Jersey, they constructed a wage gap variable equivalent to that used by Katz and Krueger in their study of fast-food restaurants in Texas. But they also attempted 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. This allowed them to test for the effect of the increase in New Jersey's minimum wage using three statistical experiments: a comparison of employment changes between New Jersey restaurants initially paying different starting wages; a comparison of employment changes between stores located in New Jersey and stores located in Pennsylvania; and a comparison that makes use of both types of information.

Their results consistently implied that the increase in New Jersey's minimum wage raised employment (as measured by full-time equivalents, or FTEs) in that state.[2] 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. In addition, using a wage gap measure equal to the difference between the initial starting wage and $5.05 per hour for stores in New Jersey, and zero for

[1] The authors also concluded that employers did not change their employment policies or practices. The summary statistics they report indicate that 9 percent changed either the timing or amount of the first wage increase granted to new employees, 2 percent reduced the number of employees per shift or changed the number of shifts per day, and 2 percent reduced fringe benefits. However, none of these effects were statistically significant.

[2] Full-time equivalent employment is calculated as the number of full-time employees plus one-half the number of part-time employees.

stores in Pennsylvania. (or stores in New Jersey with a starting wage exceeding $5.05), Card and Krueger again found a positive and statistically significant effect of the minimum wage increase on employment growth, with an estimated elasticity of 0.73. Various specification tests resulted in a wider range of estimates (both in magnitude and statistical significance), but none that were negative. They interpreted their empirical results as "inconsistent with the predictions of a conventional competitive model of the fast-food industry" (p. 790).[3]

[3] Because they find no evidence that prices fell in response to the minimum wage increase, they also note that their results are inconsistent with monopsony or equilibrium search models.

4

Issues Raised in Subsequent Research

The divergent findings of this initial round of research stimulated a number of lines of inquiry based on these methods. In particular, the differences between the conclusions in our paper and those reported by Card and others involved in the symposium suggested that some important questions had remained unanswered, and much of the ensuing empirical literature attempted to uncover the reasons for the contrasting results reported in the first wave of the new minimum wage research, with an eye toward developing a more consistent view of the effects of minimum wages on employment. As a result, this literature focused heavily on issues related to the appropriate specification of the underlying model, measurement of the appropriate variables, and the relevance of the comparison groups used in the studies. We next summarize some of the key issues discussed in these follow-on papers.

4.1 The Appropriate Measure of the Minimum Wage

In their comment on our original paper, Card et al. (1994) raised important questions about the appropriate measure of the minimum wage. As indicated earlier, we had followed the previous literature in using a variant of the Kaitz index that included state minimum wages as our measure of the effective minimum wage, while Card (1992b) had used the percentage of teenagers earning between the old and new minimum wage just prior to the implementation of the new minimum (the fraction affected variable). Card et al. argued against our (and others') use of the Kaitz index for two reasons. First, they asserted that if the minimum wage is intended to be a measure of the relative price of teen labor, the Kaitz index should be positively correlated with teen wages. They then showed that this correlation is negative because the denominator of the index (the average wage of adults in the state) is positively correlated with teen wages (presumably due to general changes in wage levels associated with inflation, productivity growth, or changes in economic activity). Second, they noted that measuring coverage of teenagers is difficult and imprecise, especially at the state level. Instead, they seem to prefer to use the nominal minimum wage in their regressions, which they showed is positively correlated with both teen wages and employment.

The concern about measuring the coverage of state minimum wages is difficult to address given the lack of available data. Information on coverage at the state level is quite sparse, and it would no doubt be difficult to compile accurate measures of teen coverage.[1] That said, given the absence of any major changes to coverage at the federal level since the early 1970s, and given that the combined coverage of federal and state laws has been very high for some time, changes in coverage are not likely to offer much in the way of identification for samples limited to the 1980s and 1990s, suggesting that it may not be unreasonable for more recent studies to ignore coverage altogether. Indeed, much of the literature in the past decade or so has followed this approach.

In contrast, Card et al.'s argument against the relative nature of the Kaitz index seems misguided to us. First, it is not the case that the Kaitz index should always be positively correlated with teen wages. As

[1] Schiller (1994) attempted to measure state coverage in 1980 by classifying state minimum wage laws into those that exempted youths, those that exempted students, those that exempted both students and youths, and those with no broad exemptions. In a cross-state regression of youth employment rates on state-specific minimum wage levels and coverage dummies (and other controls) in 1980, he found that youth employment rates were significantly lower in states without any exemption than in other states.

we showed in our reply (Neumark and Wascher, 1994), the Kaitz index should be (and is) positively correlated with changes in the relative teen wage, indicating that the Kaitz index is correctly capturing an increase in the nominal minimum wage as an increase 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 et al. is, in fact, correctly picking up an erosion in the effective minimum wage associated with an increase in the general wage level; in particular, nominal wage increases will tend to reduce the proportion of the labor force whose wages are directly determined by the minimum wage and thus to reduce the bite of the minimum wage. In addition, the use of a relative minimum wage measure provides a means of comparing the nominal value of the minimum wage with the market-determined wage for above-minimum wage workers who may be substitutes for minimum wage workers in the production function. Because the principal response to a minimum wage is likely substitution away from lower-skilled minimum wage workers toward higher-skilled, higher-wage workers, it seems particularly important to consider including a relative minimum wage variable to capture this effect.[2] Finally, 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 in different parts of the country.

The fraction affected variable used by Card (1992b), which is defined as the proportion of the teen population earning between the previous year's and current year's minimum wage, does adequately capture regional variation that is of use in identifying the effect of a minimum wage increase. However, that variable is not well equipped to capture the effects on employment associated with a gradual erosion of the real minimum wage. In this sense, such a variable is a reasonable proxy for effective changes in the minimum wage in studies of specific minimum wage increases, such as the 1990 federal increase studied by Card. But it is less informative in studies that use longer time periods because it excludes the variation in real minimum wages that results from inflation and other aggregate factors.[3]

[2] See Grant and Hamermesh (1981).

[3] Wessels (2007) argues that the fraction-affected specification used by Card (1992b) is an inappropriate measure of the minimum wage because it ignores some of the cross-state

Although it seems important to specify the minimum wage variable in real or relative terms in studies that use longer samples, it is less clear that the constraint embedded in the relative minimum wage variable – that changes in the adult wage should have the same size, but opposite-signed, effects on employment as changes in the nominal minimum wage[4] – is appropriate. From the standpoint of the effect of the minimum wage itself, this seems like a reasonable constraint – there is probably no reason to expect a decline in the relative value of the minimum wage to have a different effect on employment than would an increase in the relative minimum wage (except that the effect should be of the opposite sign). In Neumark and Wascher (1994), we showed that, with the data we used in our original paper, we could not reject the constraint implicit in specifications that included only the relative minimum wage and thus that the exclusion of the average adult wage as a separate variable in the equations did not bias the results. However, as Card and Krueger (1995a) pointed out, a theoretical specification of the labor demand function for youths would include prices of other 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).

Subsequent research has frequently relaxed the constraint implied by including a relative minimum wage measure. For example, Burkhauser et al. (2000a,b) rewrite the relative minimum wage effect

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variation in the effective size of federal minimum wage increases. In particular, if the distribution of wages differs across states, two states could have a different sized increase in the minimum wage but a similar fraction of workers affected by the change in the minimum. To address this concern, he includes a control for the relative size of the low-wage population (the proportion paid between the minimum wage and 20 percent more than the minimum wage) along with the magnitude of each minimum wage increase and finds that employment fell relatively more in states with larger effective minimum wage increases in both 1990–1991 and 1996–1997 (although the results for the latter period are not significant). However, by focusing on the minimum wage increase and not the fraction near the minimum, this approach may also fail to fully capture the increased cost of low-wage labor generated by a minimum wage increase. That is, a given minimum wage increase should have greater impact when there are more affected workers.

[4] This constraint is imposed in a log specification. In levels, the constraint is simply that the two variables enter only as a ratio.

in an equation for log employment rates as:

$$\beta \ln(MW/W) = \beta_1 \ln MW + \beta_2 \ln W, \tag{4.1}$$

where W is the average wage for adults and β_1 is interpreted as the minimum wage elasticity. For the reasons noted above, we are uncomfortable with including the wage variables in nominal terms in studies that use longer sample periods, both because such a specification would seem to violate standard homogeneity assumptions and because it does not allow for the possibility that an erosion in the real value of the minimum wage will reduce its effect on employment.[5] In contrast, Keil et al. (2001) include both the relative minimum wage variable and a real wage:

$$\beta_1 \ln(MW/W) + \beta_2 \ln(W/P). \tag{4.2}$$

Again, the authors interpret β_1 as the minimum wage elasticity and generally report negative estimates of that coefficient.[6]

Finally, a couple of studies have attempted to reduce the parameterization of the minimum wage effects even further by allowing the effects to be freely estimated for each change in the minimum wage. For example, Deere et al. (1995) introduce indicator variables for each level of the federal minimum wage in their sample period (1985–1992), while Burkhauser et al. (2000a) extend this approach to also include a separate dummy variable for every value of a state minimum wage that exceeded the federal level. Strictly interpreted, the results in both studies tend to show that increases in the minimum wage significantly reduced teenage employment rates. In general, however, these spec-

[5] The inclusion of state and year effects in the model would mitigate this concern to some extent (although not completely in the case of quarterly or monthly data). However, as we will discuss later, a central argument in the Burkhauser et al. (2000a) study is that year effects should not be included in the employment equation, for the sample period they study.

[6] We should note that in an appendix table Burkhauser et al. (2000b) report estimates of minimum wage effects using a specification that includes the minimum wage and adult wage in real terms. The estimated elasticities in these specifications are smaller than in specification (4.1) (−0.25 vs. −0.30 in the equation for teenagers), albeit still statistically significant.

ifications seem problematic because the coefficients on the indicator variables may be capturing other influences as well.[7]

4.2 Lagged Effects of the Minimum Wage

The new minimum wage literature also resurrected questions about how long it should take for minimum wages to have their full effect on employment. Many economists believed that any effects from changes in the minimum wage should be felt relatively quickly. For example, Brown et al. (1982) argued that, from a theoretical standpoint, "lagged adjustments to minimum wage increases are probably less plausible than in most other contexts where such lags are routinely assumed" (p. 496), and offered two reasons for this view. First, minimum wage workers tend to have high turnover rates, suggesting that the desired adjustments in employment levels could be accomplished quite quickly through normal turnover. Second, increases in minimum wages are typically announced several months in advance of when they become effective, so that employers should be well prepared to adjust quickly when (or even before) the new law takes effect. Card and Krueger (1995a) also argue that the industries that typically employ minimum wage workers (for example, the fast-food industry) can "easily vary their staffing levels by cutting back on off-peak or store hours, and by allowing longer queues" (p. 67), so that any disemployment effects should be evident shortly after the minimum wage is raised.

However, these considerations do not negate the possibility that the full adjustment to a higher minimum wage may take some time. Although factors such as hiring, firing, or training costs may be less important for workers with normally high quit rates, Hamermesh (1995) points out that firms may adjust non-labor inputs (for example, capital) slowly, which will tend to slow the adjustment of other inputs, including labor. Thus, the omission of lagged effects may inappropriately exclude

[7] Williams (1993) takes a somewhat different approach by allowing the coefficient on the minimum wage variable to differ across Census regions. For the United States as a whole, he estimates an employment elasticity of -0.18 using data from 1977–1989. However, he also reports considerable variation across the country in the effects of the minimum wage on teenage employment, with region-specific elasticities ranging from 0.09 in New England to -0.62 in the Pacific region.

the possibility of longer-run substitution between labor and capital, as well as the potential for scale effects associated with changes in expansion plans and establishment births and deaths.

As a result, the importance of lagged effects seems to us to be primarily an empirical question. In their survey of the earlier literature, Brown et al. (1982) found little difference in the overall estimated employment elasticities between time-series studies that included lagged minimum wage variables and those that did not. However, the potential for lagged effects did seem to matter in interpreting the results from the papers in the 1992 *ILRR* symposium. For example, in Neumark and Wascher (1992), we found statistically significant employment effects from lagged values of the minimum wage with our time-series panel of state-level data. Moreover, we hypothesized that the discrepancy between our results and those reported in Card's (1992b) study of the 1991 federal increase was due to the importance of allowing for the possibility of a lag in the effects of the minimum wage. We noted, in particular, that using our sample, a one-year first-difference estimator equivalent to that used by Card (1992b) produced minimum wage effects close to zero, similar to what was reported in his paper. But adding a lagged minimum wage effect to the model resulted in a negative and statistically significant employment effect in both the levels and first-differenced versions of the basic model.[8]

Baker et al. (1999) took this analysis a step further in a study of the effects of the minimum wage on employment in Canada.[9] The authors began by replicating some of the U.S. panel data estimates for teenagers with Canadian data.[10] They reported that one-year first-

[8] In response to our finding, Card et al. (1994) re-estimated 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.

[9] Although we assess most of the international evidence later in the paper, we include several papers from Canada in our discussion of the U.S. literature, both because of their relevance for the discussion of the U.S. results and because of the similarity of U.S. and Canadian labor markets.

[10] As the authors explained, data from Canada offer the possibility of using approaches similar to the panel data studies across states and years in the United States because the minimum wage varies across provinces. However, an important advantage of the Canadian

difference estimates of minimum wage effects in Canada are positive, whereas longer differences (from using the within-group estimate over a longer period) 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).

Baker et al. investigated more formally how the elasticity of employment with respect to the minimum wage differs depending on whether one looks at high-frequency or low-frequency variation in the data. In particular, they showed 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 amounting to filters that emphasize more of the low-frequency variation in the data. After filtering the variables to separate their high-frequency and low-frequency movements, the authors estimated the minimum wage elasticities separately at high and low frequencies.[11] The results show 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 reported an employment elasticity of −0.25 in Canada, noting that "this result is driven by low-frequency variation in the data" (p. 345). In addition, although they did not analyze U.S. data directly, they used the U.S. literature to demonstrate – through equations that relate estimated coefficients for alternative estimators to the implied elasticities at different frequencies – that their conclusions explain the different findings for the United States equally well.

The Baker et al. results indicate that the disemployment effects of minimum wages show up as longer-run responses to more evolutionary changes in the level of the minimum wage, rather than as a short-term

setting is the far greater frequency (until more recently in the United States) of minimum wage changes.

[11] The simplest filter they used is $MW_{it} = (1/2) \cdot (MW_{it} - MW_{it-1}) + (1/2) \cdot (MW_{it} + MW_{it-1})$, where the first term captures high-frequency changes in the minimum wage variable and the second term captures low-frequency changes. They also employed a more precise finite Fourier transform with similar results.

response to a particular change in the minimum wage. Consistent with our earlier discussion, the authors suggest that this longer-run response can be understood in the context of capital adjustment and interfirm (as opposed to intrafirm) adjustment rather than as labor adjustment in and of itself, which should be relatively quick in the low-skill labor market. In any event, their analysis clearly suggests that studies that claim to find no minimum wage effect on employment should perhaps be discounted unless the evidence points to no effect at both high and low frequencies.

Although the issue of lagged minimum wage effects was not the primary focus of subsequent studies, later research using state panels of time-series observations has also tended to find evidence of lags in the effects of the minimum wage on employment. For example, Burkhauser et al. (2000a) re-estimated their specifications including lags and found that the coefficient on the lagged minimum wage variable was typically statistically significant, in some cases when the contemporaneous coefficient was not. Moreover, estimates of the elasticities including lagged effects were considerably larger than those calculated from specifications that included only contemporaneous terms.[12] Keil et al. (2001) also allowed for lagged minimum wage effects, although they accomplished this 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, which they estimated using a Factor-GLS procedure with instrumental variables, they reported a short-run employment elasticity of -0.37 for youths and a long-run elasticity of -0.69.

Thus, the overall conclusion from the panel data literature on minimum wage effects seems to be that lags do matter. Firms evidently continue to adjust their employment levels well after an increase in the minimum wage, and studies that focus only on contemporaneous effects will miss some of this adjustment; as noted above, the existence of lags likely accounts for at least part of the variation in the results reported in the first wave of the new minimum wage research. More broadly,

[12] Similarly, Partridge and Partridge (1999) found that the effects of minimum wages on employment occur with a lag, both in the retail sector and for employment more generally.

it seems to us that estimates of the elasticities relevant both to the testing of alternative theories of the labor market and to the public policy debate should always at least consider both contemporaneous and lagged responses to a change in the minimum wage.

4.3 Interactions between Employment and School Enrollment

In their subsequent comment on our original *ILRR* paper, Card et al. (1994) also criticized both the inclusion of the school enrollment rate in the model and the specific measure of school enrollment that we used in the regressions. From a measurement standpoint, they pointed out that the school enrollment variable we used includes only individuals who are enrolled in school and not employed, which they argued would lead to a negative bias in the estimated employment effects from the specification we used. More broadly, they argued 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 is only evident in our specifications that included the school enrollment rate.

Turning first to the question of whether the enrollment rate should be in the regression at all, it is important to remember that the aggregate employment equation consists of observations for which the minimum wage is binding and observations for which it is not binding (or averages over such observations). Although employment for the first group is determined solely by the labor demand curve in the standard competitive model, employment for the second group of observations 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.

Regarding the measurement of school enrollment, 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 re-estimated 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 fell to −0.11, toward the low end of the range we reported originally. 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, 1996b).[13]

Despite our reanalysis, there is still a potential problem with including the school enrollment rate in the standard employment regression. Because the decision to enroll in school is likely not independent of the decision to work, the estimates from a version of Eq. (3.1) that includes the enrollment rate may be subject to endogeneity bias. In particular, changes in minimum wages may influence the choice between school enrollment and employment for some youths, which may, in turn, have implications for the opportunities available to other teenagers.

In other research, we approached this issue in two ways. First, in Neumark and Wascher (1994) we computed instrumental variable estimates of Eq. (3.1), using school expenditures, student–teacher ratios, and compulsory schooling laws as instruments. The resulting estimates, which should be largely free from endogeneity bias, showed employment elasticities for teenagers ranging from −0.17 to −0.39. Thus, although the point estimates of the effects are sensitive to which enrollment measure we used, in general the IV estimates support the view that minimum wages reduce employment among teenagers.

Second, in Neumark and Wascher (1995a) we extended Eq. (3.1) to include enrollment as an activity that is potentially influenced by changes in the minimum wage. In particular, we respecified the model to relate the utility of four possible states of employment/enrollment

[13] 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 Neumark and Wascher (1996b), which uses October observations for all of the variables in the model.

status (j), for each individual (k) to the minimum wage and other controls (X):

$$U_{kj} = \alpha_j MW + X\beta_j + \varepsilon_k, \quad j = 1, \ldots, 4. \tag{4.3}$$

The assumption that ε_k follows an extreme value distribution gives rise to a conditional logit model, with four mutually exclusive categories of activities: enrolled and employed, enrolled and not employed, not enrolled and employed, and not enrolled and not employed (or idle).[14]

Estimating the model using state-year data from 1977 to 1989, we found that a higher minimum wage is associated with a net decline in employment, with the employment elasticity similar in size to our previous studies of employment alone. However, we also found that a higher minimum wage leads to a significant decline in the proportion of teenagers who are both in school and employed and a significant increase in the proportion of teenagers who are neither in school nor employed. Moreover, the magnitudes of the estimates indicated that the effects of minimum wages on teenagers are more important and more complicated than is suggested by the employment elasticities alone.

These 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) 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, this hypothesis cannot be explicitly tested without information on the flows of teenagers across enrollment/employment states. Thus, in Neumark and Wascher (1995b and 1996a), we turned to individual panel-level data from matched CPS surveys. We applied the same basic modeling approach to the individual data as in the state-level analysis, but included in the model indicators for each individual's school/work activity in the previous year in order to calculate the effect of the minimum wage on transitions between the four distinct enrollment/employment states.

Consistent with the hypothesis above, the results suggested that the disemployment effects of the minimum wage fall largely on the

[14] This approach was based on an earlier time-series study by Wachter and Kim (1982).

least-skilled teenagers. In particular, we found that a higher minimum wage increases the probability that a teenager leaves school, presumably to look for a job or to work. Moreover, the estimates indicated that increases in the minimum wage tend to raise the probability that non-enrolled teenage workers become both non-enrolled and non-employed and reduce the probability that already non-enrolled/non-employed teenagers find a job; these results were especially pronounced for blacks and Hispanics and for individuals who had a lower wage prior to the increase in the minimum wage. In sum, the evidence from this analysis suggests that the teenage employment elasticities typically reported in the literature likely understate the size of the disemployment effects on the lowest-skilled individuals, instead capturing net employment changes among a broader group of teenagers that mask labor–labor substitution.

Only a handful of papers in recent years have focused on the joint effects of the minimum wage on teenage employment/enrollment outcomes. In Neumark and Wascher (2003), we updated our state-level analysis through 1998 and found positive and significant effects of the minimum wage on the proportion of teenagers employed but not enrolled and on the proportion of teenagers neither employed nor in school; these results are similar in nature to our earlier analysis, albeit a bit more muted. Turner and Demiralp (2001) used an approach similar to Neumark and Wascher (1995b) but focused, as in Card (1992b), on the early 1990s increases in the federal minimum wage. In particular, they examined employment-enrollment transitions between January–April 1991 and January–April 1992 in order to isolate the effects of the April 1991 increase in the federal minimum wage. Their results suggested that, overall, the higher minimum wage induced some teenagers to leave school for employment, but that teenagers initially neither employed nor in school also were more likely to find jobs. However, this result was driven entirely by transitions among non-minority teenagers living outside of central cities. In contrast, Turner and Demiralp found that black and Hispanic teenagers were more likely to become idle following the minimum wage increase, as were teens who live in a central city. This evidence also points to labor–labor substitution as an important consequence of a higher minimum wage and suggests that the elasticities typically reported in the literature understate the effects of the minimum wage on the lowest-skilled subgroups. Finally, Campolieti et al. (2005a) examined longitudinal data for Canada from 1993 to 1999 using a similar approach. In contrast to the results for the United States, these authors found little evidence of an effect of the minimum wage on school enrollment. Instead, the net disemployment effect they report appears to reflect decreased employment opportunities for both the student and non-student populations in Canada.

4.4 Other Specification Issues Relevant to the State-Level Panel Data Approach

Researchers 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, in reviewing Card's (1992b) analysis of the 1990 federal minimum wage increase, Deere et al. (1995) highlighted the possibility that differences in trend employment growth across states might bias estimates of the effects in short state-year panels like that used by Card.[15] In the case of the 1990 (and 1991) increase in the federal minimum wage, they noted that the low-wage states where Card's fraction affected variable was large also tended to be states where trend employment growth was faster, creating a positive bias in the estimated employment effect. In particular, they showed that rates of employment growth for well-educated adult men were 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 resulted in statistically significant negative effects of the 1990 and 1991 minimum wage increases on the employment rates of teenagers and high-school dropouts. The implied elasticities from these regressions were relatively large – ranging from -0.27 to -0.36 for teenage men and from -0.42 to -0.49 for teenage women. As noted above, however, the minimum wage effect is taken

[15] More broadly, in a review of the natural experiment methodology, Meyer (1995) notes that "a[n] . . . underemphasized advantage of a long time-series for outcome measures is that they may allow the researcher to examine if the treatment and control groups tend to move in parallel" (p. 158).

from year dummies for the 1990 and 1991 minimum wage increases and thus should be interpreted somewhat cautiously.

In a different vein, Burkhauser et al. (2000a) pointed out that the tendency for researchers to include year effects in empirical specifications based on time-series of state-level observations effectively eliminates the identification associated with variation in the federal minimum wage. As a result, they asserted 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" (p. 655). They then showed that equations estimated without year effects consistently 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 when the models are estimated with data from 1979 to 1997, while specifications that include year effects consistently produce small and insignificant coefficients.[16] Burkhauser et al. (2000b) applied the same methods to other demographic/education groups and find especially large negative effects for black youths and high-school dropouts aged 20–24. In a broader sense, they interpreted the results from these two studies as suggesting that the exclusion of federal variation in the minimum wage in empirical analyses tends to lead to an understatement of the disemployment effects of the minimum wage and that this factor, rather than alternative formulations of the minimum wage variable, largely explains the difference in results reported by Card and Krueger (1995a) as compared to those in Neumark and Wascher (1992) and Deere et al. (1995).

Burkhauser et al. raise an important question – how to balance the loss of identification associated with including year effects with

[16] The paper also presents results through 1992. For this shorter sample period, the authors report a negative and significant minimum wage effect (with an elasticity of -0.22) when year effects as well as a lagged minimum wage effect are included and larger negative effects (with elasticities up to -0.6) when year effects are excluded. The use of panels of differing lengths is meant to address the question of how minimum wage effects vary with the business cycle, and the authors interpret the smaller elasticities they find with the longer sample period as consistent with the hypothesis that minimum wage effects are likely to be smaller during periods of robust economic growth (as in the mid- to late-1990s).

the potential bias associated with omitted macroeconomic effects that would be captured by the year dummies. They recognize that the possibility of omitted macroeconomic or other aggregate effects is a potential criticism of their specification. But, in response, they noted that the model already includes the unemployment rate and the average adult wage, which they believe should effectively capture the influences of the business cycle, inflation, and aggregate productivity growth. In addition, when they added dummy variables corresponding to recessions to the specification, they found little difference in the estimated minimum wage elasticity.

Our own approach has been to include year effects in specifications using time-series panels of state observations, for several reasons. First, including a relative minimum wage measure (in levels, but not logs) in the specification will permit some identification from variation in the federal minimum wage because of differential movements in state wages. Second, we believe that it is difficult to include all relevant macroeconomic or other aggregate-level variables and thus perhaps are more concerned than Burkhauser et al. that elasticities estimated without year effects will suffer from omitted variable bias. Third, it is not as obvious to us that the diminution of federal variation in the minimum wage is as much of a problem today than it might have been a decade ago. Samples that include more recent data have significantly more variation in state minimum wages than did the samples that ended in the early 1990s, thus making the presence of federal variation relatively less important for identification of the minimum wage effect; moreover, with the additional variation in state minimum wages, even increases in the federal minimum will generate state variation in the effective minimum wage (the higher of the state and federal minimum wage levels for each state) typically used in minimum wage research.

Indeed, when Sabia (2006) reestimated the Burkhauser et al. specifications using data through 2004, he found a negative and statistically significant effect of the minimum wage on employment regardless of whether year effects were included in the regressions, with estimated elasticities of −0.18 without year dummies and −0.30 when year dummies were included (his Table 3). These results clearly indicate that the greater variation in minimum wages associated with recent state increases has helped to better identify the effects of the minimum wage on teenage employment, making the potentially problematic choice to exclude year effects unnecessary. In addition, with the policy question often focused on whether a particular state should raise the minimum wage, the greater reliance on state variation may, in many circumstances, be entirely appropriate.

A recent paper by Bazen and Le Gallo (2006) takes this approach a step further by attempting to separate the employment effects of federal increases in the minimum wage from those enacted at the state level. Using the specification proposed by Burkhauser et al., they first used quarterly state-level data from 1984 to 1989, when the federal minimum wage was unchanged, and estimated the effect on teenage employment of changes in state-specific minimum wage increases over that period. The coefficient on the minimum wage variable in these specifications is close to zero and insignificant, whether or not year effects are included. They then re-estimated the model through 1992 and found a negative and statistically significant effect, with an elasticity of -0.42; as in Burkhauser et al., however, the negative effect is only evident in specifications that exclude year dummies. The authors performed a similar analysis for the 1992–1996 period, when the federal minimum was again unchanged, and also found no negative effects of state minimum wage increases (although there were only four state increases during this period). Again, however, adding in observations through 1997 leads to an estimated negative effect in models that exclude year dummies (although the effect disappears when 1998 is added as well).

The authors then specified a more general model that included three separate minimum wage variables: the change in the federal minimum wage, the change in a state's effective minimum wage due to an increase in the federal minimum wage (if the state's minimum wage was originally higher than the federal level), and the change in a state's own minimum wage (provided that the change resulted in a state minimum wage level that exceeded the federal level). Consistent with their earlier results, for both time periods the only negative and significant increases are for the federal minimum wage increases. Although the coefficients on the state-specific increases are also negative in both periods, they are imprecisely estimated and thus usually indistinguishable from both zero and the coefficient on the federal minimum wage. The coefficient on the third component (the change in a state's effective minimum due to an increase in the federal wage floor) varies considerably across minimum wage increases (both in magnitude and sign), although it is never statistically significant. The authors interpreted their results as suggesting that small state-level increases may not have significant disemployment effects, although they admitted that their tests are relatively weak.

4.5 Reactions to the Initial Round of State Case Studies

The case studies that comprised the other strand of the new minimum wage literature were quite controversial within the economics profession. 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. For example, in a review piece on Card and Krueger's book *Myth and Measurement*, Richard Freeman (1995) wrote that "their analysis is a model of how to do empirical economics" (p. 831). Similarly, Paul Osterman (1995) asserted that Card and Krueger "make a powerful case that what they term "natural experiments" are a more appropriate way to conduct policy analysis than cruder research based on time-series or broad cross-sections" (p. 839).

However, other labor economists were more critical of these studies. In particular, Finis Welch (1995) – again referring to *Myth and Measurement* – stated that: "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" (p. 842). Likewise, Hamermesh (1995) concluded that "even on its own grounds, CK's strongest evidence is fatally flawed" (p. 838).

In general, the criticisms of the case study approach focus on three main issues. The first is the question of adequacy of the control groups used in the studies, a concern emphasized by both Hamermesh and Welch. On its face, for example, it seems reasonable to question the use of Georgia, Florida, and Dallas/Ft. Worth as adequate control groups in Card's (1992a) 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 a different state as a control may be problematic. For example, Deere et al. (1995) pointed out that teenage employment rates (as measured by the CPS) in New Jersey diverged significantly from those in Pennsylvania beginning in 1988, casting doubt on Card and Krueger's claim that Pennsylvania represents a sensible control group with which to compare New Jersey. More broadly, Hamermesh questioned the practicality of this entire approach for studying the effects of minimum wages, noting 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" (p. 837).

Second, in each of the fast-food case studies from the first round of the new minimum wage research, the post-treatment observation comes less than a year after the relevant minimum wage increase (and sometimes much less so). As we noted earlier, there is substantial empirical evidence that the disemployment effect of an increase in the minimum wage may occur with a lag of one year or more, suggesting that these case studies may understate the effect of a minimum wage hike and even fail to detect a negative effect. For the same reason, both Brown (1995) and Freeman, in their reviews of *Myth and Measurement*, speculated that these studies are more appropriate for examining the short-run effects of minimum wage changes than for estimating their long-run effects, a point made more significant by the findings of Baker et al. (1999).

Third, some observers have questioned the reliability of the data used in these case studies. This obviously is less of a concern for Card (1992a), who used publicly available data from the CPS. But Katz and Krueger (1992), Spriggs and Klein (1994), and Card and Krueger (1994) all conducted their own telephone surveys of fast-food restaurants, which were not subject to the same rigorous standards as those used to develop the surveys used in government statistical programs. Indeed, Welch (1995) noted that these authors provide little documentation about the survey methodology or data collection process, and he expressed significant doubts about the quality of the data, noting in particular some puzzling features of the sample collected by Card and Krueger (1994). Likewise, in Neumark and Wascher (2000), we documented what seemed to us to be an unusually high degree of volatility in the employment changes measured with Card and Krueger's data.[17]

In light of these concerns, a number of researchers subsequently reexamined the results reported in the initial round of state-specific case studies. Among the first of these reassessments was a paper by Kim and Taylor (1995), which revisited 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 replicated 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 pointed out that the volume of retail sales in California rose much more rapidly during that period than in the United States, raising questions about the validity of this experiment. In response, Kim and Taylor turned to more detailed industry data within the retail sector and examined 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 showed a statistically significant and negative 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. The authors argued that because the relative wage changes over the 1988–1989 period were more likely to be driven by the exogenous minimum wage increase in California in July 1988, the coefficient on the

[17] Hamermesh also questioned whether the initial observation in these studies was sufficiently prior to the effective date of the minimum wage change so as to avoid the possibility that some employers had already begun to adjust to the higher minimum wage, which would lead difference-in-difference estimates to understate any employment effects. However, this cannot explain the large positive effects found in two of the fast-food studies (Katz and Krueger, 1992, Card and Krueger, 1994).

wage variable can be interpreted as a labor demand elasticity, which they estimated to be −0.9. The authors conducted a similar analysis that related county-level employment growth to county-level wage growth and again found a negative and statistically significant coefficient on wage changes only for the 1988–1989 period, with an estimated demand elasticity of −0.7.[18]

As Card and Krueger (1995a) – and later Kennan (1995) – pointed out, an important shortcoming of the Kim and Taylor analysis is the absence of a direct wage measure in the County Business Pattern data they used. In particular, wage rates have to be computed 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, but they argued that any associated biases were likely to be relatively unimportant; as evidence, they noted that there is no indication of a negative correlation in years in which the minimum wage was constant, and that IV estimates that use lagged wages and average firm size in the industry produce similar results. Card and Krueger addressed the first point by showing that there is a negative correlation as well in the 1989–1990 change (although this could reflect a lagged effect from the 1988 increase in the minimum wage). In addition, they pointed out that the significant negative coefficient in the IV estimates relies on the inclusion of average firm size as an instrument, which they argued is inappropriate.

In Neumark and Wascher (2000), we revisited Card and Krueger's analysis of the effects of New Jersey's increase in the minimum wage. In particular, we collected administrative payroll records on hours worked from fast-food establishments in the universe from which they drew their sample, and compared the properties of these administra-

[18] As the authors pointed out, because they used the change in average wages as the explanatory variable in their specification, the comparable elasticity of employment with respect to the minimum wage is considerably smaller. Given the information provided in their paper, we calculate that the −0.7 to −0.9 demand elasticities they reported are roughly equivalent to minimum wage elasticities of −0.15 to −0.2.

tive records with Card and Krueger's survey data. 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. In contrast, the payroll data referred unambiguously to the payroll period. Reflecting this problem, the data collected by Card and Krueger indicated far greater variability across the two observations than did the payroll data, with changes that were sometimes implausible.

We then replicated Card and Krueger's difference-in-differences test after replacing their survey-based data with observations based on the payroll records. In contrast to Card and Krueger's results, the results from our replication indicated 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) presented 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 reported 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 found small and statistically insignificant effects of the increase in New Jersey's minimum wage on employment, and they concluded 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" (p. 1419). Our own conclusion from this array of statistical results is that:

> the data presented in CK's Reply ... raise some questions about the validity of the assumptions needed to interpret the difference-in-differences estimates as a nat-

> ural experiment. Nonetheless, even under the premise that the geographic proximity of the samples renders all other things equal, we believe that, in the final analysis, 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 these chains in New Jersey relative to the Pennsylvania control group. Combined with the new evidence from the ES-202 data that CK present in their Reply, 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 (p. 1391).

An important point that has often been ignored in interpreting the evidence from case studies of a specific industry is that the neoclassical model of the labor market does not predict that employment in a particular sub-sector of the economy will decline in response to a general minimum wage increase. As a consequence, the absence of an employment decline for a narrow sample of establishments should not be viewed as a contradiction of that model. As Welch (1995) emphasized, the effect on employment for any particular sub-sector depends on relative factor intensities. For example, if fast-food restaurant chains are less intensive in low-wage labor than are their competitors (for example, sandwich shops or other small privately-owned restaurants), 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 thus an increase in fast-food employment.[19]

[19] To be clear, we are not asserting that this *does* occur, but rather that it is a potentially important criticism of industry-specific studies. Additional evidence on the importance of this type of effect, as well as additional reduced-form employment estimates for a range of low-wage industries (see the discussion of work by Wolfson and Belman below) can enhance our understanding of industry-specific studies.

4.6 Hours versus Employment Effects

Some authors have pointed out that the basic predictions of the various theoretical models of the minimum wage refer to labor input rather than to employment specifically, and have suggested that one potential reason for a small employment effect is that employers can also adjust the number of hours worked by their employees.[20] Michl (1996, 2000) speculated that the difference between our results and Card and Krueger's with respect to New Jersey's minimum increase reflects the fact that Card and Krueger essentially measure employment, while we measure total hours.[21] To test this hypothesis, he compared the changes in employment and total hours in a subset of 52 observations from our payroll data set in which the respondent reported both total hours and employment. For this subset of observations, the difference-in-differences estimates indicate a negative effect of the minimum wage increase on both total and average hours and a small positive effect on employment, although only the coefficient on average hours per worker is statistically significant. Michl also examined the ratio of full-time workers to total employment in Card and Krueger's data, which should be positively correlated with average hours. In this case, the results are suggestive of a positive effect of the minimum wage on the fraction of workers who work a full-time schedule. However, Michl effectively dismissed this result as irrelevant on the grounds that most fast-food workers are part-time employees and that reductions in their average working hours could more than offset an increase in the number of full-time employees.

Both we and Card and Krueger (2000) also considered the possibility that the difference in our results might be associated with changes in average hours. For example, we reported both that Card and Krueger's data showed a shift toward full-time workers in response to New Jersey's

[20] As Couch and Wittenburg (2001) noted, the use of employment rather than total hours could either understate or overstate the effect of the minimum wage on labor input, depending on whether employers increase average hours per worker to partially offset the reduction in employment or reduce both employment and average hours.

[21] As noted above, Card and Krueger attempted to transform their data into full-time equivalent employment by weighting the number of part-time workers by one half. However, this is obviously an imperfect measure of total hours.

minimum wage increase and that the 52-restaurant subset of the payroll data showed a positive effect on employment and a negative effect on total hours, effectively the results cited by Michl.[22] However, because of the small number of restaurants reporting both employment and hours, both we and Card and Krueger cautioned against making much out of the estimates from the subsample. Moreover, we are less inclined to ignore the above-mentioned increase in the ratio of full-time workers to part-time workers in New Jersey as evidence against the hypothesis that this difference in data definitions (employment vs. total hours) was the primary source of difference in the results. In any case, it seems to us that the effect of minimum wages on total hours is most relevant for testing the validity of the competitive model of labor demand.

The effect of the minimum wage on hours has also been examined using longer sample periods for the United States as a whole. In particular, both Zavodny (2000) and Couch and Wittenburg (2001) investigated the effects of minimum wage changes on average hours in the state-level panel data framework described earlier. Zavodny used the standard specification described in Eq. (3.1) above and included both state and year effects in her analysis, which is based on data from 1979 to 1993. When the minimum wage variable is specified in relative terms, her results show a negative effect of the minimum wage on teen employment, with an elasticity of -0.12, similar to that reported in Neumark and Wascher (1992). However, using the real minimum wage, she found a small and insignificant effect of the minimum wage on employment. Moreover, the estimated effects of the minimum wage on average hours per worker (conditional on employment) are either positive (with the real minimum wage) or close to zero (with the relative minimum wage), suggesting that firms did not adjust average hours of teenagers downward in response to the higher minimum wage. The elasticity for total hours worked (unconditional on employment) for all teenagers is 0.24 (and statistically significant) using the real minimum wage and -0.11 using the relative minimum wage.

Zavodny augmented these state-level results with an analysis of matched CPS individual-level data from 1979–1980 to 1992–1993. In

[22] The latter finding was also reported by Card and Krueger (2000).

this analysis, she identified affected teenagers as those with an initial wage between the old and new minimum wage (in real terms), and calculated the implicit wage gap as the amount needed to raise their wage to the new minimum (with the gap for those with higher wages set to zero). Zavodny then regressed year $t + 1$ employment status and weekly hours on the wage gap variable and other controls to estimate the effect of the minimum wage. In this case, the results indicate that an increase in the minimum wage reduces the probability that an affected worker will remain employed. However, for those that do keep their jobs, the effect of the minimum wage is to increase their average hours. On balance, the results suggest that total hours of initially employed teenagers do not fall in response to an increase in the minimum wage, a result that is confirmed by a positive and statistically insignificant effect of the minimum wage on hours using the entire sample (so that the hours effect is not conditional on employment). Zavodny cautioned that these estimates do not incorporate the effects of the minimum wage on transitions from non-employment to employment. Similarly, they do not capture the influence of the minimum wage on the likelihood that those who make this transition work part-time or full-time.

In sharp contrast to Zavodny, Couch and Wittenburg (2001) found that minimum wages reduce both employment and total hours worked by teenagers. These authors followed the model put forth by Burkhauser et al. (2000a), using monthly data from January 1979 through December 1992 and excluding year effects from the analysis. The estimated elasticity for hours ranges from -0.48 to -0.77 depending on the exact specification used. Moreover, the estimated elasticities are 25 percent to 30 percent larger than those estimated for employment based on identical specifications. The authors interpreted 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.

5

More Recent Evidence on Employment Effects

Most of the papers discussed thus far include minimum wage increases up to and including the 1990 and 1991 increases in the federal minimum wage and a selected number of state minimum wage increases that were enacted in the late 1980s or early 1990s. With the federal minimum wage boosted again in 1996 and 1997 and with a number of other states having raised their minimum wages above the federal level, research on the effects of minimum wage increases on employment in the United States has continued to be of considerable interest to economists and policymakers. One segment of this more recent literature on minimum wage effects has looked for ways to more effectively identify the economic consequences of minimum wage changes by sharpening the focus on those individuals most likely to be affected by a change in the minimum wage, while another segment has examined new approaches for estimating minimum wage effects.

In this section, we review the literature that has moved beyond the issues that arose out of the first round of the new minimum wage research; because this research has not been discussed much elsewhere, we also provide somewhat more detail on the procedures and results for these studies than we did for those discussed in the previous section.

Tables 5.1 and 5.2 summarize the studies reviewed in this section, as well as those reviewed in Section 4, grouping the studies by the type of evidence they consider.

5.1 Studies Limited to the Most Recent (1996 and 1997) Federal Minimum Wage Increases

We first review research that has focused on the most recent federal minimum wage increases in 1996 and 1997. Some of this literature has been produced by researchers at advocacy organizations and often concludes that these minimum wage increases did not reduce employment. For example, a recent study by Bernstein and Schmitt (2000) computes changes in average employment rates for teenagers and for adults with less than a high school education over three overlapping periods: 1995–1996, 1995–1997, and 1995–1998.[1] Following Card (1992b), they then regress these changes on the fraction of each group affected by the federal minimum wage increases.[2] The increasingly longer differences are intended to capture more of the effects of the two federal increases. Bernstein and Schmitt also report results beginning in 1994, arguing that these estimates should be free of any effects of future anticipated minimum wage increases.

For teenagers, a negative and significant disemployment effect is only evident for the 1995–1996 period, which provides an estimate of the contemporaneous effect of the first minimum wage increase. When the sample period is extended to 1997 or 1998, or when it is extended back to 1994 (or both), the estimates are mostly negative, but not significant. Nevertheless, some of the estimated elasticities are quite large. For example, the estimated elasticity for teenagers is -1 (and statis-

[1] These researchers are from the Economic Policy Institute, which supports minimum wage increases. Despite the potential lack of objectivity from organizations that are funded by interest groups that publicly support or oppose minimum wage increases, we discuss this research because it has sometimes been influential in the policy debate. For papers that are either circulating or published based on work that appeared earlier under the cover of advocacy organizations, we refer to the later papers, as these are the versions that either underwent or are likely to undergo peer review. Ultimately, research that undergoes peer review should be regarded as the most credible.

[2] Although not specified, we assume that this fraction is computed from the 1995 wage distribution and is measured as the fraction of workers below the new minimum wage, as of either 1996 or 1997.

Table 5.1 Studies of general minimum wage employment effects in the United States

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Aggregate Panel Data Studies					
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	For teens, significant negative effects only when enrollment is included; enrollment rate too narrow
Williams (1993)	Federal and state	Teenagers	CPS, 1977–1989	Different coefficients across regions: ranging from 0.09 (New England) to −0.62 (Pacific)	
Neumark and Wascher (1994)	Federal and state	Teenagers and 16–24 year-olds	CPS, 1973–1989	**IV for enrollment in some specifications:** **Teenagers: − 0.17 to −0.39** **Youth: −0.12 to −0.16**	
Evans and Turner (1995)	Federal and state	Teenagers and 16–24 year-olds	CPS, 1978–1989	Teens: −0.09 Youth: −0.04 Not significant	Mixes October employment data with May minimum wage data
Neumark and Wascher (1995a)	Federal and state	Teenagers	CPS, 1977–1989	Conditional logit analysis: Non-employed and enrolled: −0.13 Employed and enrolled: −0.40 Employed and not enrolled: 0.28 Idle: 0.64	
Neumark and Wascher (1995b)	Federal and state	Teenagers	Matched CPS, 1979–1992	Multinomial logit analysis: increase in probability that teens leave school to work; increase in probability that teens leave school to become idle; increase in probability that employed low-wage teens become idle	

Table 5.1 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Neumark and Wascher (1996b)	Federal and state	Teenagers and young adults	October CPS, 1980–1989	Teenagers: −0.22 Young adults: −0.14; significant	
Burkhauser et al. (2000a)	Federal and state	Teenagers	CPS, 1979–1997; monthly data	**1979–1997: 0 to −0.35** **1979–1991: −0.22 to −0.6** **1996–1997: −0.17 to −0.27** **Estimates generally significant in specifications excluding year effects**	Estimates excluding year effects less convincing
Burkhauser et al. (2000b)	Federal and state	Teenagers and young adults, by race and educational attainment	CPS, 1979–1997; SIPP, 1990–1992; monthly data	**Teens: −0.3 to −0.6** **Youths: −0.20 to −0.25** **Black youths: −0.85** **Nonblack youths: −0.18** **High school dropouts (20–24): −0.85** **High school grads (20–24): −0.16**	Estimates excluding year effects less convincing
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**	

Table 5.1 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
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	Excludes year effects
Turner and Demiralp (2001)	Federal minimum wage increase in April 1991	Teenagers by race and location (city versus non-city)	CPS, Jan–April 1991 to Jan–April 1992	Multinomial logit analysis: increase in overall teen employment; sizable negative effects for black and Hispanic teens and for teens in a central city	
Keil et al. (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)**	
Pabilonia (2002)	State	14–16 year-olds	NLSY97, data for 1996	Cross-section probit analysis: Males: −0.6 Females: −1.3 Some estimates significant	
Neumark and Wascher (2002)	Federal and state	Youths (16–24) in the binding regime	CPS; 1973–1989	Switching regressions with state-year panel: −0.13 to −0.21; significant	
Neumark and Wascher (2003)	Federal and state	Teenagers	October CPS, 1980–1998	**Non-employed and enrolled: −0.11** **Employed and enrolled: −0.09** **Employed and not enrolled: 0.41** **Idle: 0.18**	
Bazen and Le Gallo (2006)	Federal and state	Teenagers	CPS, 1984–1992 and 1992–1998	0 to −0.45; significant effects only evident for federal minimum wage increases	Excludes year effects in specifications showing significant negative effects; no allowance for lagged effects

Table 5.1 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Sabia (2006)	1. State	Teen employment and hours in retail and at small businesses (100 or fewer in firm)	CPS ORG's, 1979–2004 (retail); March CPS files, 1989–2004 (small businesses)	Share of 16–64 year-olds employed in retail: −0.09 to −0.29 Share of 16–64 year-olds employed in small businesses: −0.08 to −0.12 Share of teens employed in retail sector: −0.27 to −0.43 Average retail hours worked by teens: −0.53 Average retail hours worked by employed teens: −0.05 to −0.28 Share of teens employed in small businesses: −0.46 to −0.89 Average small business hours worked by teens: −0.48 to −0.88 Average small business hours worked by employed teens: −0.54 to −0.70	Focus on teen employment in low-wage sectors generates ambiguous results; declines do not imply overall declines in these sectors or in teen employment
	2. State	Teenagers	CPS ORG's, 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**	
Federal Variation					
Card (1992b)	1990 federal minimum wage increase	Teenagers	CPS, 1989–1990	−0.06 to 0.19; not significant	No allowance for lagged effects

Table 5.1 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Deere et al. (1995)	1990 and 1991 federal minimum wage increases	Teens and adult high school dropouts	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	Indicator variables for higher minimum wages may pick up other influences
Bernstein and Schmitt (2000)	Federal	Teens and young adult high school dropouts	CPS ORG's 1995–1998	Teens: −1.0 (significant) for 1995–1996, −0.1 to −0.4 for other intervals, not significant; smaller when sample begins in 1994 20–54 year-old high school dropouts: estimates variable, non-robust, of varying sign	Excessive disaggregation by year likely contributes to non-robustness and statistical insignificance
Bernstein and Schmitt (1998)	1. Federal	Teens and young adult high school dropouts	CPS ORG's, 1995–1998	Many estimates, roughly centered on zero; large positive elasticities for minority females, sometimes significant; large negative estimates for minority males, insignificant	Ignores state variation in minimum wages
	2. 1996 and 1997 federal minimum wage increases	Teens	CPS ORG's, 1991–1998	Deere et al., specification: Male teens: −0.45 to −0.61; estimates sometimes significant Female teens: 0.32 to 0.86	Ignores state variation in minimum wages

Table 5.1 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Neumark (2001)	Cross-state variation generated by 1996 and 1997 federal minimum wage increases	Teens, youths (16–24), non-enrolled youths and 20–24 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 zero** **16–24 year-olds: −0.02 to −0.22; insignificant** **Non-enrolled, 16–24, high school or less: −0.11 to −0.53; significant** **Non-enrolled, 20–24, high school or less: −0.09 to −0.15; sometimes significant** **Non-enrolled, 16–24, less than high school: −0.21; significant** **Non-enrolled, 20–24, less than high school: −0.11 to −0.12; insignificant**	Limited period and small number of minimum wage increases
State Increases					
Chapman (2004)	Cross-state variation in share between 100 and 120% of state minimum in 2003	Total workforce	Payroll Survey	Elasticity N/A; estimated effect of −0.01, insignificant	Regression does not test effect of minimum wage on employment; focuses on total employment rather than low-skilled group
Fiscal Policy Institute (2004)	State minimum wage increases after 1997	Overall employment, and employment in retail, in small businesses, and in small retail businesses	Payroll Survey, County Business Patterns	Simple comparisons of employment growth: elasticity N/A; higher growth for all four measures in states that raised their minimums, but not for retail, small business, or small retail employment growth relative to total employment growth, for which simple difference-in-differences estimates are centered on zero	Ignores variation in timing of state minimum wage increases; no controls for other factors affecting employment growth

Table 5.1 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Reich and Hall (2001)	California minimum wage increases, 1996–1998	Various groups	CPS, 1994–1999	Teen employment rate rose, but by much less than for other age groups; rate of change in growth rate fell in retail and restaurant sectors relative to manufacturing and construction, but employment growth fell in retail and restaurant sectors relative to manufacturing and construction	Absence of comparison groups or problematic comparison groups
Orazem and Mattila (2002)	1990–1992 increases in Iowa minimum wage	Retail and non-professional services	QCEW; Establishment UI records and author survey, 1989–1992	*County level* Employment: −0.06 to −0.12; mostly significant *Firm level* Employment: −0.22 to −0.85 Hours: −1.01 to −1.50 All estimates significant	
City Increases					
Yelowitz (2005)	Introduction of Santa Fe minimum wage	Total, and more- versus less-educated	CPS, 2003–2005	**Employment effects near zero and insignificant; significant negative hours effects for workers with 12 or fewer years of education, elasticity of −0.12**	
Potter (2006)	Introduction of Santa Fe minimum wage	Total, construction, health care, retail, and accommodations and food	ES-202 data for Santa Fe and Albuquerque	Difference-in-difference-in-differences estimates: −0.015 (all industries); −0.16 (construction); −0.009 (retail); −0.03 (health care); −0.009 (accommodations and food); significant only for construction	Control city of Albuquerque may have been chosen to minimize minimum wage effects

Table 5.1 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
See also Dube et al. (forthcoming)					
Aggregate Time-Series Studies					
Wellington (1991)	Federal	Teenagers and 20–24 year-olds	1954–1986	Teenagers: −0.05 to −0.09 20–24 year-olds: 0.002 to −0.02	
Card and Krueger (1995a)	Federal	Teenagers	1954–1993	−0.050 to −0.087; only a few significant	
Bernstein and Schmitt (2000)	Federal	Teenagers	Data from Card and Krueger (1995a), 1954–1993, updated through 2001:Q1	Teenagers: −0.001 to −0.06	
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**	

Table 5.1 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Wolfson and Belman (2001 and 2004)	Federal	Low-wage industries and industries employing large share of young adults	BLS payroll survey, various years through 1997	Pooled time-series estimates by industry: Employment elasticities vary across industries, with many insignificant; of significant estimates of effects of legislated increases, most are negative; effects of real declines in minimum are of unexpected sign in one-half of cases; no greater evidence of disemployment effects in industries where minimum wages increased average wages more	Theory does not predict employment declines in all industries; industries with larger wage increases may be those with less ability to substitute away from low-wage labor toward non-labor inputs, or greater ability to substitute towards more-skilled labor
Studies Focused on the Workers Directly Affected					
Currie and Fallick (1996)	1980–1981 increases in federal minimum wage	Workers with initial wage between old and new minimum	NLS, 1979–1987	−0.19 to −0.24; significant	Control group includes all workers above minimum wage
Abowd et al. (1999)	Change in real federal minimum wage	Low-wage workers (ages 16–60) freed by decline in real minimum wage relative to those marginally above	Matched CPS, 1981–1982 to 1986–1987	Varies by age Male average: −0.42 Female average: −1.57 (conditional on employment in $t + 1$)	No change in nominal minimum wage

Table 5.1 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Abowd et al. (2000a)	Change in real federal minimum wage	Low-wage young workers (ages 16–30) freed by decline in real minimum wage relative to those marginally above	Matched CPS, 1981–1982 to 1986–1987	Varies by age Male average: -2.23 Female average: -1.87 (conditional on employment in $t+1$)	No change in nominal minimum wage
Abowd et al. (2000b)	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**	
Neumark et al. (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 1.2 times the old minimum wage**	

Note: Results from studies we regard as more reliable tests of employment effects of minimum wages are highlighted.

Table 5.2 Studies of minimum wage employment effects in specific sectors in the United States

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Katz and Krueger (1992)	1991 federal minimum wage increase	Fast-food employment in Texas	Survey of restaurants in December 1990 and July 1991	1.7 to 2.65; significant	
Card (1992a)	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	Questionable control groups
Spriggs and Klein (1994)	1991 federal minimum wage increase	Fast-food employment in Mississippi and North Carolina	Survey of restaurants in March 1991 and April 1991	Estimates centered on zero, not significant	Short period over which to observe effects
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	Large amount of measurement error
Kim and Taylor (1995)	1988 California minimum wage increase	Retail trade employment	QCEW	−0.15 to −0.2; some estimates significant	No direct measure of hourly wages
Partridge and Partridge (1999)	Federal and state	Retail trade employment	CPS and BLS establishment survey; 1984–1989	Retail: −0.08 to −0.25 Eating and drinking: −0.05 to −0.2 Other retail: −0.09 to −0.26 Total nonfarm: −0.10 to −0.21 Teens: −0.23 to −0.72	

Table 5.2 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
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; some 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**	
Michl (2000)	1992 New Jersey minimum wage increase	Fast-food employment in New Jersey and Pennsylvania	Neumark-Wascher payroll data (subsample of observations reporting employment)	Employment: 0.044 Total hours: −0.018 Hours per worker: −0.062	Small sample
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**	Want-ad specifications different from industry employment specifications

Table 5.2 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Dube et al. (Forthcoming)	Introduction of San Francisco minimum wage	Restaurant employment in mid-size establishments	Survey data	**0.01 to 0.12; not significant**	Low survey response rate; short-term effects only; exclusion of larger restaurants

See also Reich and Hall (2001), Sabia (2006)

Note: Results from studies we regard as more reliable tests of employment effects of minimum wages are highlighted.

tically significant) for the 1995–1996 period, and for the other periods that begin in 1995 it ranges from -0.1 to -0.4; these estimates fall by about half when the sample begins in 1994.[3] Of course one might ask why these authors did not pool the results over more observations, such as the four one-year changes that can be constructed using data from 1994 to 1998. Indeed, it seems likely that their variable and insignificant estimates may result from this shortcoming, and hence we are unconvinced by their conclusion that the research does not uncover significant disemployment effects of the minimum wage.

As noted above, this study also examines evidence on adults aged 20–54 with less than a high school education. For this group, the estimates for the 1995–1996 change are positive, while those computed through 1997 or 1998 are negative, small (in terms of the elasticity), and insignificant. The authors also present results for different periods – for example, beginning in 1996 – and find further evidence of non-robustness, with strong negative employment effects for low-skilled adults but the opposite for teens. One contributing factor to the lack of robustness of the results may be that limiting the estimation to this period results in a sample that is too small to reliably detect employment effects of minimum wages.

In an earlier paper, Bernstein and Schmitt (1998) also report results from two other analyses. The first is a difference-in-differences analysis of changes in employment rates for various groups of teens and young adult high school dropouts relative to changes from the period prior to the 1996 minimum wage increase; this study also controls for aggregate employment changes by including employment rates for men and women aged 25–54 (separately) in the regression. However, neither this analysis nor the previous one accounts for state-level variation in min-

[3] These results are consistent with those reported by Wessels (2007). In particular, Wessels regresses the log change in teenage employment in each state from the 12-month period just prior to the October 1996 increase to the twelve-month period immediately following the September 1997 increase on the fraction of workers in each state that were affected by these increases. The results indicate that states with a greater fraction of affected workers had relatively larger employment declines. Although Wessels does not report minimum wage elasticities or estimates for wages that could be used to compute such elasticities, the estimated coefficient on the fraction affected variable is -0.30 (significant at the 10-percent level), somewhat larger than that reported by Bernstein and Schmitt.

imum wages. This is a problem because many states had higher minimums prior to the 1996 and 1997 federal increases, so that the federal increases induced different minimum wage changes in different states. Moreover, while the estimates across many groups are centered on zero, the point estimates are often extremely large. For example, the minimum wage increases are estimated to have induced a 4.8 percentage point drop in the employment rate for black men and a 7.8 percentage point increase for Hispanic women. Although these estimates are not statistically significant, they imply huge elasticities and make us reluctant to put a great deal of store in these estimates, especially given the problem of omitted variation in state minimum wages.

Bernstein and Schmitt also present results from an analysis covering a longer period (1991–1998). This specification, which follows Deere et al. (1995), regresses age-sex-specific employment changes on an aggregate employment measure and dummy variables for federal minimum wage increases. These models reveal disemployment effects for teenage males that are statistically significant in some specifications, but the estimated employment effects are positive and not significant for teenage females and teenage blacks. Again, paralleling the earlier estimates, some of the implied elasticities are fairly large. For example, for teenage males, the regression estimates for the 1996–1997 federal minimum wage increases imply an elasticity of approximately −0.45.[4] For females, the estimated positive employment effect is nearly twice as large (in absolute value). Again, this approach does not use information on state minimum wages, and the large estimates (both positive and negative) are troublesome. Moreover, these results are largely inconsistent with those reported in Deere et al. (1995) for the earlier federal increases, which indicated negative and significant effects for teenage males, teenage females, and teenage blacks (although those estimates were also quite large). These problems are compounded by the disaggregation of results across demographic groups and years, which may produce estimated minimum wage effects that appear non-robust and statistically insignificant even when estimates from pooled models are

[4] This is computed from the summed effects in Table 7, column 1 (which are individually insignificant although their joint significance is not reported), and the approximate 44 percent employment rate for teenage males reported in Table 4.

stable and significant. Given these issues, it is not entirely clear what to conclude, other than that tests of the effects of isolated minimum wage increases may not be very reliable.

A related paper is Neumark's (2001) "pre-specified research design" study of the effects of the federal increases in 1996 and 1997. This study resulted from an effort by David Levine, as editor of *Industrial Relations*, to get various researchers who had studied minimum wages to pre-specify a research design for studying this set of federal minimum wage increases. 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. The motivation for this project was to try to cut through an apparent relationship between authors who had written previous minimum wage studies and the answers they found (or "author effects"). As documented in Table 1 of that paper, perhaps the most pronounced tendency was for research by us to find negative employment effects and for research by Card and Krueger to find positive (or zero) effects. Such a pattern could be an indication that prior biases had affected the reported results, an issue discussed in greater detail below. The journal's project would have been more valuable had more researchers involved in the minimum wage debate decided to participate, but only this one pre-specified research design was submitted and published.[5,6]

In this analysis, standard panel data models are 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. These variables are entered both contemporaneously and lagged,

[5] Despite his submission of a paper, Neumark expressed initial reservations about the usefulness of this approach because it requires throwing away a considerable amount of information on previous minimum wage increases (2001, p. 124), paralleling some of the concerns we have about the Bernstein and Schmitt studies.

[6] A recent paper for Canada (Campolieti et al., 2006) has a similar flavor. These authors did not pre-commit to a research design before obtaining and studying data on minimum wage increases in Canada, and hence avoided discarding much useful data. Instead, they simply apply the specifications proposed in the Neumark (2001) paper, as well as other modifications proposed by others, to the Canadian data, in this manner avoiding specification search that could introduce biases. They find quite uniform evidence of disemployment effects of minimum wages from standard reduced-form specifications, with elasticities ranging from about -0.14 to -0.44, and centered on about -0.3.

and the models include state and year fixed effects and a control for the employment rate of adults with more than a high-school education. The data set spans the period from October 1995 to December 1998, roughly one year before the first federal minimum wage increase in October 1996 to one year after the second increase in September 1997. The minimum wage variables account for the variation in state minimum wages, so that the identifying information is the state-specific change in the effective minimum wage associated with the federal increases. Even so, a general lack of 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 imprecise, but near zero. For example, in a specification that includes current and lagged relative minimum wages, the estimated elasticity is 0.06. For youths (aged 16–24) the estimates are frequently negative, although they are again insignificant. In this case, the estimates are sometimes larger in absolute value, with elasticities of approximately −0.15. However, similar to what Bernstein and Schmitt found, the results are not particularly robust for either teenagers or 16–24 year-olds as a whole. For example, for 16–24 year-olds, the estimates change sign when the data are restricted to observations from the CPS Outgoing Rotation Group (ORG) files. In contrast, evidence of disemployment effects is stronger when the sample is restricted to the less-skilled individuals in these age groups. For non-enrolled 16–24 year-olds with no more than a high school education, the estimated elasticities are around −0.3, and for non-enrolled 20–24 year-olds with no more than a high school education the elasticities are around −0.15; these estimates are often significant, but not always. Negative employment effects for these groups are also evident in specifications that use the fraction below minimum wage variable, and they are often statistically significant as well. The point estimates in these specifications are somewhat variable, but for the main ones, the elasticities range from −0.11 to −0.21. Thus, we read the evidence in this paper as pointing to disemployment effects of minimum wages for young, unskilled workers, and as suggesting that the absence of an overall effect (or a relatively weak effect) may nonetheless mask compositional shifts.

Finally, although Burkhauser et al. (2000a) focus most of their attention on the 1979–1992 period used in the first round of the new minimum wage research, they also present results that isolate the effects of the 1996 and 1997 increases by using indicator variables to identify the effects of each of these minimum wage increases. In these specifications, the estimated elasticities are −0.27 for the 1996 minimum wage increase and −0.17 for the 1997 increase (in specifications excluding year dummies), roughly half the size of the estimated elasticities for the 1990 and 1991 minimum wage hikes. As noted in the previous section, however, we have reservations about estimates based on this type of specification.

5.2 Studies Focused on Recent State Minimum Wage Increases

The last decade has witnessed an unprecedented number of states raising their minimum wages. Although the federal minimum wage has remained at $5.15 per hour since 1997, as of January 2007, 29 states and the District of Columbia had minimum wages that exceeded the federal wage floor. Moreover, state minimum wages have recently been raised above the federal level in some large states (such as Wisconsin, 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.[7] As a result, the share of the population aged 16–64 residing in states with a minimum wage higher than the federal level rose from 15.6 percent in 1998 to above 50 percent as of January 2007 (based on CPS data).[8] Finally, many state minimum wages are currently quite high – 11 states and the District of Columbia have minimum wages of at least $7. From a research perspective, this

[7] For details on state minimum wages, see http://www.epinet.org/issueguides/minwage/table5.pdf.

[8] In addition, although we ignore them here, living wages, which typically set a higher minimum wage for a subset of workers in an area, have spread to scores of cities, while city-wide minimum wages have recently been enacted in San Francisco and Santa Fe. For an up-to-date review of living wages and research on their effects, see Adams and Neumark (2005).

proliferation of state minimum wages provides an unparalleled opportunity to study their effects.

One paper that examines the employment effects of the recent state increases in minimum wages is an Economic Policy Institute study by Chapman (2004). He estimates a cross-section regression of state-level employment growth between 2000 and 2003 on the share of each state's workforce earning between 100 and 120 percent of the state's minimum wage in 2003.[9] The results suggest no relationship, but it is unclear to us why the correlation between the proportion of the workforce earning near the minimum wage in 2003 and aggregate employment growth from an earlier year to 2003 is informative about minimum wage effects. For example, if the overall gains in employment disproportionately reflected growth in low-wage employment, or if the rise in employment was due in part to an increase in labor supply among less-skilled workers, we might find a positive relationship between the low-wage share in 2003 and employment growth from 2000 to 2003, even if minimum wages reduce employment. Moreover, most economists have focused on the effects of the minimum wage on the employment opportunities of the low-skilled individuals who are most affected by minimum wages, rather than on its effects on aggregate employment. On the other hand, as Chapman points out, some organizations opposing minimum wages, such as the Employment Policies Institute, have suggested a link between minimum wages and state unemployment rates.[10]

A study of state minimum wages by the Fiscal Policy Institute (2004) – another group advocating minimum wage increases – shows that employment rose faster between 1998 and 2001 in states with a minimum wage higher than the federal level than in states where the federal minimum was binding. For overall employment, the authors report that the states with a relatively high minimum wage (11 states plus the District of Columbia) had faster employment growth from 1998 to 2001 than did other states and about the same employment growth from 2001 to 2004. In retail trade, employment also rose more in the high minimum wage states, but this difference was most evident

[9] This paper also discusses trends in specific states; we focus only on the regression analysis.
[10] See, for example, www.epionline.org/oped_detail.cfm?oid=18.

in the latter period. This study also examines growth in employment at small businesses (fewer than 50 employees) and at small retail businesses. This analysis, which can only be done with County Business Patterns data through 2001, showed greater employment growth in the high minimum wage states both at small businesses and among small retail businesses.

However, this study suffers from two serious shortcomings. First, it makes no effort to exploit differences in the timing of state minimum wage increases. Although many of the states raised their minimum wage in 1999 or later, so that the minimum wage variable captures increases in the effective wage floor in a general sense, differentiating the timing of those increases would improve the identification of the estimates. Second, the analysis does not control for other factors that might have influenced employment growth. A better strategy might have been to compare the difference between retail employment growth and overall employment growth in the two sets of states, on the assumption that minimum wage effects would be more evident in the retail sector, while other state-specific factors had a similar effect on both aggregate and retail employment. Thus, we are skeptical that much can be learned from this study despite the authors' claims of relatively large positive effects.[11]

Partly in response to the Fiscal Policy Institute study, Sabia (2006) re-estimated the model controlling for other potential influences on employment, including the demographic characteristics of state populations, aggregate state-level economic activity, and other factors; in addition, he used a longer sample period (1979–2004). More specifically, Sabia uses a relatively standard panel data analysis along the lines of Burkhauser et al. (2000a and 2000b). Following the specifications in these papers, he includes contemporaneous and lagged values of the minimum wage variable, which is the log of the higher of the federal or state minimum wage. Sabia focuses on employment in retail trade and

[11] An updated version of this study was released in 2006 and included data through January 2006 for the analysis of total and retail employment. However, even though five additional large states had increased their minimum wages by 2004 (Florida, Minnesota, New Jersey, New York, and Wisconsin), the treatment group of states is still the 11 states that had raised their minimum by 2003. We therefore focus on the earlier study.

at small businesses, and aggregates the data to the state-by-month cell for each of these sectors.

For overall employment (as a share of the population) in retail trade, the results indicate statistically significant employment declines, with estimated elasticities of about −0.10. Sabia reports a larger range, up to −0.29, but this larger elasticity only results when the year effects are omitted. For total employment in small businesses (defined as firms with 100 or fewer employees), the evidence also consistently points to significant negative effects of minimum wages, and in this case the results are less sensitive to whether year effects are included; the elasticities range from −0.08 to −0.12.

In addition to reporting results for total employment in these sectors, Sabia also presents results for teen employment in retail trade and in small businesses as a share of the teen population, arguing that the use of overall employment in these sectors may understate the effects of the minimum wage on low-skilled labor. This is a reasonable criticism, although one reason for focusing on low-wage sectors to begin with is that conventional theory predicts that employment in such sectors will decline in response to a minimum wage increase (keeping in mind the reservations we noted earlier about focusing on too narrow a sector).

The estimated disemployment effects for teens in retail trade are large and statistically significant, with elasticities in the range of −0.27 to −0.43. For teen employment in small business, the elasticities are uniformly negative and about twice as large. However, it is not entirely clear what to make of the findings that the minimum wage reduces the share of teens employed in retail businesses or small businesses. These results neither imply employment declines in the retail sector (as firms could substitute toward other workers) nor employment declines for teenagers overall (as teens could shift to other sectors), although it seems unlikely that either of these other channels could fully absorb the teens displaced from the retail and small business sectors. As a result, Sabia's estimated effects of minimum wages on overall employment in the retail and small business sector seem more relevant to the traditional policy question.[12]

[12] Of course, we are sometimes more interested in whether minimum wages reduce employment of the least skilled. However, in this case, we might learn more about the implications

A few additional studies have adopted the case study approach to estimate the effects of specific state minimum wage increases on employment. For example, Orazem and Mattila (2002) examined the effects of a series of minimum wage increases that took place in Iowa beginning in 1990, when the Iowa minimum wage rose faster than the federal minimum. The authors begin with a county-level analysis of low-wage industries in the state using data from the Quarterly Census of Employment and Wages (QCEW) program, the same data source used by Kim and Taylor in their study of California's minimum wage increase. However, in order to circumvent the lack of hourly wage information in this data set, the authors supplement these data with information on average hourly earnings by industry from the BLS's Current Employment Survey. The minimum wage variable is the level of the minimum wage relative to the lagged average wage for each county-industry cell; the authors also include controls for changes in national employment and wages, county per capita income, the proportion of firms in each cell that are covered by the Fair Labor Standards Act, and whether a county is urban or rural. The results indicate a negative effect of the minimum wage on employment, with an estimated elasticity of between −0.06 and −0.12. The estimates are mostly statistically significant, with the larger estimates evident for four-quarter changes in employment (as opposed to one-quarter changes).

Recognizing that these estimates are based on aggregates that include both workers directly affected by the minimum wage increases and other higher-paid employees, the authors supplement these results by collecting unemployment insurance tax filings for a subset of the firms. These records include quarterly information on employment and earnings for individual employees, which were then merged with driver license records to obtain the gender and age of each worker. Using this information, Orazem and Mattila estimate predicted wages by age and sex and, using the demographic profile of the workforce for each

of an increase in the minimum wage for their employment prospects by focusing on their overall employment rates. Indeed, there is no reason to necessarily expect that teenagers in these sectors will be more adversely affected by minimum wage increases than teenagers in other sectors, because the production technology that leads to the greater use of teen labor in these sectors may also entail fewer substitution possibilities.

firm, calculate the predicted proportion of workers at each firm who were initially paid less than the new minimum wage. The authors then regress this percentage on the level of the minimum wage relative to the predicted average wage of the subminimum wage group to obtain an estimate of the employment elasticity for affected workers. They emphasize that because they do not use actual wage data in this regression, the minimum wage effect is effectively identified by changes in the demographic makeup of each firm's employees. In particular, a shift away from employees in traditionally low-wage demographic groups (for example, teenagers) would be evidence of a negative employment effect from the minimum wage.

Indeed, the estimates show exactly this result. For changes measured over four-quarter periods, the estimated employment elasticities range from −0.22 to −0.54 when no industry controls are included and from −0.31 to −0.85 when such controls are included; moreover, the estimates are statistically significant in all cases. As the authors note, the larger elasticities relative to their county-level analysis likely reflect their efforts to identify those individuals most likely affected by the minimum wage increases, an issue to which we return below. However, the size of the elasticities is somewhat surprising given that the demographic characteristics used to identify the minimum wage effects (age and sex) are the same groups on which previous studies have focused.

Reich and Hall (2001) analyze the effects of a set of increases in California's minimum wage from $4.25 in September 1996 to $5.75 in March 1998 and conclude that these increases did not reduce employment. As one piece of evidence in support of their conclusion, they note that employment rates increased for all age groups between 1995 and 1999. However, California's economy was booming during that period, and the fact that there was a general increase in employment at that time says little about the effects of the minimum wage increase. Indeed, when we construct a difference-in-differences comparison with their data, using older ages as a crude control group, we find the opposite result: the employment rate for teenagers rose much less (0.8 percentage point) than did employment rates for other age groups (between 3 and 7.8 percentage points). Because teenagers are disproportionately more likely to be affected by the minimum wage, this comparison suggests a negative employment effect of the minimum wage.[13] On the other hand, the employment rate for 19–22 year-olds, who were also more likely than adults to be affected by this set of minimum wage increases, rose relatively faster than that for other age groups. Of course, all of these comparisons are overly simplistic relative to the existing literature because the absence of control groups from other states ignores the possibility of differential trends in employment rates by age group.

As another piece of evidence, Reich and Hall show that the change in the rate of employment growth in the retail and restaurant sectors increased from 1996 to 1998, whereas it decreased in the manufacturing and construction sector. Because manufacturing and construction jobs should have been largely unaffected by the minimum wage, they interpret this difference-in-differences comparison as suggesting that the minimum wage increases did not reduce employment. However, elsewhere in the text they note that the manufacturing and construction sector experienced a "sharp fall . . . in 1998, when the Asian financial crisis particularly affected manufacturing." This industry-specific demand shock raises serious doubts about the validity of their difference-in-differences estimate.

In addition, they offer no explanation as to why they use the difference in the *change* in employment growth before and after the minimum wage increase as their estimate of the minimum wage effect, rather than simply comparing growth rates before and after the policy change. When we compare average growth rates for the period 13 months before the first minimum wage increase with the growth rates for 12 months after the last increase (to minimize anticipatory effects, and to allow for longer-term effects), we find that employment growth fell sharply in retail trade (2.13 percentage points) and relatively strongly in restaurants (0.82 percentage point), but fell only slightly in manufacturing and construction (0.26 percentage point), suggesting that the minimum wage increases had an adverse effect on employment in low-wage industries. Regardless, by the authors' own admission, this difference-

[13] According to their data, teenagers comprised just 2.9 percent of workers in 1999 but accounted for 13.1 percent of workers earning $6 or less.

in-differences experiment is of modest value, and the combined evidence presented in this paper is unconvincing.

Singell and Terborg (2007) examine the effects of minimum wages on the eating and drinking sector, and the hotel and lodging industry, in Oregon and Washington. They use data from 1994 to 2001, a period that includes three increases in the minimum wage in Oregon (in 1997, 1998, and 1999) and three increases in the minimum wage in Washington (in 1999, 2000, and 2001). Thus, this sample provides two different experiments for evaluating the effects of minimum wage increases in distinct labor markets that, according to the authors, faced similar economic conditions.

Singell and Terborg first use BLS wage survey data to explore the extent to which minimum wages are binding in these two sectors. For the eating and drinking sector, the 10th percentile of the wage distribution tracks the minimum wage in each state quite closely, with the exception of 1997 and 1998, when the minimum wage was only $4.90 in Washington; even so, the 10th percentile clearly increases to the later minimum wages of $6.50 (in 2000) and $6.72 (in 2001). This pattern holds for three of the four jobs in this sector – hosts and hostesses, waiters and waitresses, and fast food cooks – but is less evident for restaurant cooks, whose wages tend to be somewhat higher. Indeed, for the first three jobs, even the median wage rates seem to move closely with changes in the minimum wage. Wages are higher in the hotel and lodging sector, and a systematic pattern relating minimum wages to the 10th percentile of the wage distribution is harder to discern; the one exception is for maids and housekeeping, which is the lowest-wage category.[14] These wage distributions suggest that minimum wages are binding in the eating and drinking sector, but less so in the hotel and lodging sector.

The authors next turn to the monthly BLS employment data for these two sectors. They regress the change in employment on a minimum wage variable, controls for which state the observation comes from, population and per capita income growth, calendar month dummies and interactions between these and a dummy variable for Ore-

[14] The other two jobs surveyed are desk clerks and managers.

gon (to allow for different seasonal patterns in each state), and a cubic time trend. They identify the minimum wage effect from the annual first difference in the log of the real minimum wage, and in some specifications include the lag of this variable as well. The authors also make a concerted effort to establish the robustness of their results. The results for the eating and drinking sector consistently indicate that increases in the minimum wage reduced employment. The estimates are statistically significant whether or not lagged effects are included, although they are stronger in the latter case. For the specification that includes both contemporaneous and lagged effects, the employment elasticity is −0.2.

In contrast, the estimates for hotel and lodging are positive and significant, with elasticities of about 0.15. The authors speculate that the absence of negative effects for this sector may be because minimum wages are considerably less binding, although of course that fact does not explain a positive employment effect. They also suggest that voters may pass minimum wage legislation when "times are good" – the endogeneity problem we discussed earlier – but offer no explanation as to why this endogeneity would be particularly relevant with regard to economic conditions in the hotel and lodging sector, nor do they provide any evidence that the timing of minimum wage increases fits this explanation. In any event, their analysis suggests that minimum wage effects may vary substantially across industries.

Finally, Singell and Terborg report on an analysis of the number of help-wanted ads for the different types of jobs in these two industries. The help-wanted data are a 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. By classifying the help-wanted data by the jobs for which wage distributions are reported, it is possible to focus on those jobs for which minimum wages were binding (most importantly, maids and housekeepers in the hotel and lodging industry).

The dependent variable in the want-ad regressions is the ratio of want-ads to industry employment, because a given *level* of want-ads generates a particular *flow* of applicants. That is, the level of want-ads and changes in employment should be affected in the same way by minimum wages. However, by the same logic, it seems that the form of the minimum wage variable should be the same as in the employment analysis, although the authors also switch to a levels specification for the minimum wage variable. Their rationale for this specification is that, in equilibrium, different levels of want-ads will be needed to maintain the desired level and quality of employment; for example, if the desired level of employment is lower with a higher minimum wage, then fewer want-ads will be needed to maintain that level.[15] Nonetheless, given that the want-ad regressions are interpreted as complementary evidence, it would have been useful to see the results for the same specifications used in the analysis of employment, perhaps coupled with some more-detailed, independent analysis of how minimum wages affect want-ads.

The want-ad regressions are presented for five jobs in eating and drinking – wait staff, buss staff, dishwasher staff, hosts, and cooks – and one job in hotels and lodging – housekeepers.[16] 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 reducing the number of ads by 10 to 47 percent (the number of monthly ads ranges from about 2 to 48, depending on the period and job). Among the restaurant jobs, the only insignificant result is for cooks, which is the highest-paying job in the eating and drinking industry and thus less likely to be affected by minimum wage changes. Thus, the general conclusion 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 somewhat higher-wage hotel and lodging sector.[17]

[15] Personal communication with Larry Singell, July 6, 2007.

[16] Of course, these jobs need not necessarily be specific to an industry. For example, we presume that a want-ad may be advertising for waitpersons in a hotel restaurant.

[17] In a similar vein, Skedinger (2006) uses firm-level survey data to study the effects of minimum wage changes on hotel and restaurant workers in Sweden. The evidence indicates that minimum wage increases led to higher separations of affected workers aged 20 and over, but there is only weak evidence of an effect on teenagers. This study also finds that minimum wage decreases raise the job accession rate of affected workers, although this evidence is not as robust as the evidence on separations. The context of this study is also quite different from the U.S. case studies, as the minimum wages used in the analysis consist of an extensive set of wage floors that vary by job, age, tenure, and location, and

5.3 Studies of Recent City Minimum Wages

With the introduction of minimum wages in Santa Fe and San Francisco in 2004, we now also have a few studies of the effects of city-specific minimum wages.[18] In particular, Dube et al. (forthcoming) present an analysis of the impact of the San Francisco minimum wage on restaurant employment in that city. Using survey data on restaurants employing 30 or fewer workers, they find that restaurant employment in establishments more likely to be affected by the new minimum wage law increased more rapidly than in three control groups: small restaurants not covered by the minimum wage in the first year; restaurants with 14–30 employees that were already paying all of their workers a wage above the new minimum prior to its introduction; and restaurants with 14–30 employees in neighboring jurisdictions not subject to the new minimum wage. The estimated elasticities for both employment and FTE employment range between 0.01 and 0.12 (with most at the lower end), and are never statistically significant.

The study is generally careful, and uses an appropriate research design. One nice feature is that the authors document that their data do not suffer from the measurement error problems exhibited in the Card–Krueger (1994) fast-food study, although they do not provide information on their survey that might explain why they apparently do better. There are a few potential problems, however. First, the response rate is low – only 38 percent for the first wave.[19] Second, like many of the earlier case studies, this analysis uses a relatively short window over which to estimate the employment effects, with the post-minimum wage observation coming about nine months after the minimum wage was implemented; as a result, the estimates will not capture any potential longer-term adjustments to minimum wage

that are negotiated via centralized bargaining. Thus, the implications of the results for more traditional uniform minimum wage floors are not clear.

[18] A minimum wage of $6.75 took effect in Albuquerque on January 1, 2007.

[19] Although the authors use employment data from the Dun & Bradstreet file – their sample frame – to show that initial employment levels for the respondents to their survey were similar to those for all establishments, it also would have been useful to use the Dun & Bradstreet data from one year later to see whether the non-respondents were different in terms of employment changes, and, moreover, to see whether the empirical analysis of employment using the Dun & Bradstreet data led to the same conclusion.

increases. Third, we do not see a compelling reason to exclude restaurants with more than 30 employees. Although the authors argue that larger restaurants "may be different types of enterprises operating in markets with distinct dynamics" (p. 8), they offer no particular justification for that view, and it seems problematic to exclude restaurants of a particular size from the study if the goal is to estimate the employment effects for the industry most affected by the city's minimum wage increase. That said, absent evidence that these problems biased the results, this study has to be regarded as providing reasonably good evidence that the minimum wage implemented in San Francisco did not reduce employment at established mid-size restaurants. How well this generalizes to the rest of the industry and other industries, and how well the results from this single episode generalize to other minimum wage increases, remain open questions.[20]

In addition, two recent studies have examined the effects of the \$8.50 hourly minimum wage that was introduced in Santa Fe in June 2004.[21] Yelowitz (2005), in a paper circulated by the Employment Policies Institute, estimates standard difference-in-differences models using CPS data for Santa Fe and the rest of New Mexico. His estimates indicate that the minimum wage had small and insignificant effects on the employment-to-population rates of both more- and less-educated (12 or fewer years of education) individuals, but that weekly hours

[20] Sims (2005) also attempts to provide some evidence of the effects of the city's minimum wage on the San Francisco restaurant industry. His study, which was financed by the industry, reports that the minimum wage had an adverse effect on employment. This conclusion is based on a survey in which respondents predominantly indicated that they reduced employment in response to the minimum wage, although about two-thirds of respondents indicated that their response was small. However, this study suffers from two major problems. First, response rates for many parts of the survey were extremely low – below 4 percent. Second, the survey did not collect objective measures of employment from before and after the minimum wage increase, but instead simply asked respondents how they were affected by the new minimum wage. Some restaurant owners may have had an incentive to claim that the minimum wage affected them adversely, given that the minimum wage probably raised their labor costs (most respondents indicated that the minimum wage decreased profits); in addition, those owners who were more adversely affected by the minimum wage may have been more likely to respond to the survey.

[21] Although the city refers to this wage floor as a living wage, it is more comparable to a minimum wage because it has much broader coverage than the living wage laws in many other cities (see, for example, Adams and Neumark, 2005). In particular, the new minimum wage applies to all firms with 25 or more employees.

worked by less-educated workers declined by a statistically significant 3.2 hours. This decline, which is conceptually equivalent to the effect of the minimum wage on FTE employment, corresponds to an elasticity of −0.12. The methodology used in this study is relatively standard and is reasonably convincing, albeit subject to concerns raised earlier about studies of single episodes of minimum wage increases.[22] Moreover, the data extend from 17 months prior to the minimum wage's imposition to 17 months afterwards, which should allow longer-term effects to be reflected in the estimates.

Potter (2006) takes a somewhat different approach to estimating the effects of the Santa Fe minimum wage. Using establishment-level ES-202 data compiled by the New Mexico Department of Labor, he estimates the effects of the new minimum wage on both total private employment and on employment in four low-wage industries – construction, retail, health care, and accommodations and food. In particular, Potter uses firms in Albuquerque with more than 25 employees as the control group and estimates difference-in-differences models for the average level of employment in each city in the 12 months prior to the minimum wage's introduction and in the 12-month period that followed. The choice of Albuquerque as the control group is problematic. The author indicates that Albuquerque was chosen as the control because the annual percent changes in employment over the period from 1996 to 2005 most closely matched those for Santa Fe. However, because that period includes the 18 months that followed the introduction of the minimum wage in Santa Fe, such a selection rule will bias the estimates of the minimum wage effects toward zero. Instead, it would have been preferable either to choose the control region on the basis of the similarity of changes prior to the introduction of the minimum wage or to include the rest of the state, as Yelowitz did.

[22] One curious result in this study is the absence of an estimated positive effect from the new minimum wage on wages at the 10th or 25th percentile of the wage distribution in quantile regressions with controls for race, education, and marital status. Although this may cast doubt on the validity of the estimated negative effect on hours, Adams and Neumark (2005) point out that tests for the effects of a minimum wage on the wage distribution should not condition on skills because minimum wage laws are expected to affect the *unconditional* distribution of wages rather than the *conditional* distribution.

The regression estimates for the proportionate change in employment at firms subject to the minimum wage (that is, firms with 25 or more workers) are 0.012 for total employment, −0.08 for construction, −0.007 for retail, −0.002 for health care, and 0.031 for accommodations and food; only the negative estimate for construction is statistically significant. At first glance, the absence of a negative effect for the accommodations and food industry seems at odds with the prediction of the competitive model, given the relatively high number of low-wage workers employed in that industry. However, the Santa Fe law includes a 100 percent tip credit for workers earning at least $100 a month in tips or commissions,[23] so the minimum wage may have been of little relevance for this industry.[24] Moreover, when Potter estimates difference-in-difference-in-differences models (by including smaller firms in both Santa Fe and Albuquerque in the analysis), all five of the estimates are negative (although again significant only for construction). Indeed, this last comparison arguably provides the strongest evidence because it controls for other differences in employment change in the two cities by differencing relative to the change for smaller firms.[25] In this regard, the evidence for Santa Fe's minimum wage increase generally points to negative effects, although the evidence is not strong and is potentially problematic.

Finally, we reiterate our concerns about drawing broad inferences from case studies of the effects of a particular minimum wage increase. Although case studies of a minimum wage increase in one state that provide estimates of employment effects for a broad group of the least-skilled or for a variety of industries do not suffer from the same problems faced by studies that focus on one particular industry, they still are subject to biases associated with demand shocks or sampling vari-

[23] See http://santafenm.gov/cityclerks/livingwageeng-span.pdf.

[24] Potter shows no results on wage effects.

[25] Potter incorrectly interprets the evidence as indicating that, after the minimum wage was introduced, "either large Santa Fe businesses are increasing employment more than large Albuquerque businesses or small Albuquerque businesses are increasing employment more than small Santa Fe businesses" (p. 19). What the estimates actually show is that the difference in employment growth in large Santa Fe businesses relative to large Albuquerque businesses was less than the corresponding difference for small businesses, consistent with the minimum wage slowing employment growth in large business in Santa Fe.

ation that might be correlated with the minimum wage increase. In contrast, a larger panel data study that averages over many episodes of minimum wage increases is more likely to produce reliable results because other unobserved shocks will tend to average out and because sampling variation will be smaller.

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

Although most of the new minimum wage research has moved away from aggregate time-series studies of the effects of the federal minimum wage, there is a small body of time-series research over the past 15 years that warrants a brief summary. This segment of the literature has its genesis with Wellington (1991), who updated the basic time-series specification in Brown et al. (1983):

$$Y_t = \alpha MW_t + R_t\beta + \varepsilon_t. \tag{5.1}$$

In this specification, Y represents the employment-to-population ratio for a particular demographic group (16–19 or 20–24 year-olds), MW is the minimum wage variable, and R is a set of control variables that includes a cyclical control, supply side variables, linear and quadratic time trends, and seasonal dummies.[26] The minimum wage variable is typically the Kaitz index, which was the most widely used measure in the earlier time-series literature. Brown et al. estimated the model with quarterly data from 1954 to 1979, while Wellington extended the data through 1986. Using the longer sample period, Wellington's results showed employment elasticities ranging from -0.05 to -0.09 for teenagers and from zero to -0.02 for 20–24 year-olds, estimates that fall below the lower end of the range found in the BGK survey. In addition, only a minority of the estimates for teenagers – and none of the estimates for young adults – were statistically significant at conventional levels.

Subsequent studies also found that estimating a standard time-series model with a data set that included more recent observations

[26] More precisely, Wellington updated the specification suggested by Solon (1985), which also includes interactions between the quarterly seasonal dummies and a linear and quadratic trend, along with a standard AR(1) correction.

produced a smaller elasticity of teen employment with respect to the minimum wage. For example, Card and Krueger (1995a) estimated variants of the Solon model using data through 1993 and reported elasticities for the Kaitz index that center on -0.07 and that are generally not statistically significant.[27] Likewise, Bernstein and Schmitt (2000) reported results from time-series analyses that extend the data into 2000. These results provide additional evidence of a downward drift in the disemployment effect of the minimum wage, with estimates for the full sample period indicating an elasticity of -0.06 and a t-statistic (1.63) just below the 10-percent level of significance. These authors also reported estimates that augment the earlier model by differencing the data to account for non-stationarity and by treating seasonality differently than simply seasonal dummy variables; in addition, they report results using annual data. The elasticities from these alternative analyses range from -0.001 to -0.05, with four of the five estimates well below standard levels of significance.

The likely 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) argued 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, Card and Krueger found 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 become significant as the sample size grew longer, Card and Krueger argued that the declining estimates constituted evidence that researchers were more likely to choose and report specifications that produced statistically significant estimates corresponding to their theoretical priors, that reviewers and editors were more likely to publish such studies, and thus that the earlier literature was "biased in the direction of finding statistically significant results" (p. 194).

[27] However, Bazen and Marimoutou (2002) note that Wellington and Card and Krueger enter some variables in levels that BGK and Solon entered in logs. When they estimate the same specification as Solon with data through 1993, 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.

However, in Neumark and Wascher (1998), we showed that successive estimates with increasingly longer time series from a benchmark specification that is arguably uncontaminated by publication bias produce a pattern of results not materially different from those generated by the studies included in Card and Krueger's meta-analysis. This finding points to parameter instability rather than publication bias as the likely reason for the decline in the estimated effects of minimum wages. We offered two possible reasons for the decline in the coefficient on 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 – depending exactly on how the distribution changed – overstate the decline in the bite of the minimum wage that took place during the 1980s (if the close substitutes for minimum wage workers earn below-average wages), leading to a growing downward bias (towards zero) in estimates of the minimum wage effect on employment. One other possibility, which we highlighted earlier, is that mismeasurement of the minimum wage variable in such studies has increased over time because of the proliferation of state minimum wages.

Some of the most recent studies in this genre find 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. 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 procedures to test for the presence of 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), suggesting that the estimates from the standard specifications are not consistent.

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 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) present what we believe is the most recent time-series study of minimum wage effects in the United States. They also argue that the specifications used in the earlier time-series literature were dynamically mis-specified, but they address this issue in a different manner than did Williams and Mills. In particular, they extend the standard Solon 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.[28] 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 Solon model.[29] In addition, although the coefficient on the Kaitz index is not statistically significant, the coefficients on both the minimum wage and average

[28] Bernstein and Schmitt (2000) also report estimates from this structural time-series approach, although they give no details.

[29] The authors show that the forecast performance of the other models, estimated through 1979, deteriorates badly.

manufacturing wage are significant, and the restriction that the minimum wage and average wage enter with equal but opposite-signed effects is rejected. They also 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, report statistically significant negative effects of the minimum wage on teenage employment, with an elasticity of -0.11 in the short-run and -0.27 in the long-run.

We are not time-series econometricians, and thus we leave it to those with more expertise to fully assess the contributions of Williams and Mills and of Bazen and Marimoutou to the time-series literature on minimum wage effects. And, we reiterate our earlier concern that time-series studies are less relevant to the present context given the proliferation of state minimum wages. Nonetheless, these papers 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.

Finally, Wolfson and Belman (2001 and 2004) estimate the wage and employment effects of the minimum wage using time-series data for 3-digit SIC industries that have either a relatively high fraction of young workers or a relatively low average wage. In particular, they specify time-series models for each industry and estimate the models as a system of equations; this set-up allows them to account for common unobservable influences and to more easily test some cross-equation restrictions. The authors estimate these models for a variety of panels – some shorter ones that include more industries, and some longer ones that include fewer industries. Their study uses aggregate national data, which means that they are identifying the effects of the federal minimum wage.

Not surprisingly, this approach generates a large number of estimated minimum wage effects. To summarize their results, the authors report the estimates in a graphical framework that displays the sign, size, and significance of the estimates. The figure indicates that many of the employment estimates by industry are statistically insignificant, although they are more often negative than positive. In addition, when the authors focus on the effects of legislated minimum wage increases (rather than declines in the real value of the minimum wage), 14 of the

18 significant estimates, out of a total of 128 estimates, are negative. Finally, the authors also present results that display the relationship between the estimated employment elasticity and the wage elasticity, arguing that we should see a sharper employment reduction when the minimum wage has a larger positive effect on the wage. However, the evidence does not support this conjecture.

We have a couple of comments on these studies. First, we are not certain how much should be made of the lack of significance of many of the estimates, since the models are so highly disaggregated. It seems likely that pooling restrictions across many industries would not be rejected, in which case more precise estimates would likely be obtained by aggregating them. On the other hand, there is some value to providing the estimates of employment effects for many low-wage industries. In our discussion of the industry-specific studies, we noted that theory does not predict that employment in a narrow industry should fall in response to a minimum wage increase, which makes it difficult to interpret the results of such studies. In this sense, the absence of negative employment effects for a large number of low-wage industries would be more compelling than evidence from a single industry.

Second, we are not convinced that the absence of stronger disemployment effects for industries in which the minimum wage has a larger effect on average industry wages should be viewed as 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. Thus, one alternative explanation for their result is that average wages rise the most in industries in which there is the least possibility to substitute away from low-wage labor and toward non-labor inputs. A second possible explanation is that average wages increase the most in industries in which it is easiest to substitute higher-skilled for lower-skilled labor. In either of these cases, the filter that Wolfson and Belman apply would tend to pick out industries that should have smaller disemployment effects rather than larger ones.

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

Much of the literature discussed thus far has focused on the effects of the minimum wage on the aggregate employment rates of teenagers, although some has also discussed results for other low-skill groups. As noted earlier, the choice of teenagers in these studies reflects the fact that they make up a disproportionate share of the minimum wage workforce, so that the effects of minimum wages are more likely to be evident for this group than for other broad demographic groups. However, from a policy perspective the effect of a minimum wage increase on teenagers is arguably of less interest than the effect on low-wage adult workers, both because teenagers are less likely than adults to be permanently low-wage workers and because many teenagers are secondary earners from non-poor families. Moreover, even among teenagers, many workers earn significantly more than the minimum wage, and because the proportion not directly influenced by a change in the minimum wage can be substantial, it is often quite difficult to distinguish minimum wage effects from the myriad of other factors influencing the supply of and demand for teenage labor, as well as from noise in the data.

For the same reason, the reported elasticities from studies of aggregate groups will tend to understate both the effects of the minimum wage on the minimum wage workforce and the elasticity of demand for such workers with respect to the minimum wage. For example, one can think of the minimum wage elasticity for the teenage group as a whole 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. If we assume that the latter elasticity is zero, then the minimum wage elasticity for affected workers (e^A) can be written as $e^A = e/p^A$, where e is the estimated elasticity for the group as a whole and p^A is the proportion of the group directly affected by the change in the minimum wage. Moreover, it is incorrect to interpret the minimum wage effect as a demand elasticity in these studies. The size of the average wage increase associated with a minimum wage increase will typically be less than the minimum wage increase itself because some affected workers are already earning more than the old minimum wage (but less than the new minimum wage). In this case, the demand elasticity can be written as

$$e^A = (e/p^A) \cdot (\Delta MW/\Delta W^A), \tag{5.2}$$

where ΔW^A is the average wage change of those workers whose wages were directly affected by the change in the minimum wage.

As indicated by our summary thus far, one approach that researchers have used to estimate minimum wage elasticities for individuals most likely to be affected by the minimum wage has been to narrow the sample to groups more likely to work at minimum wage jobs.[30] However, even samples of narrow demographic groups or specific low-wage industries will include both minimum wage workers and higher-wage workers. Thus, some researchers have attempted to identify observations for which the minimum wage increase is binding and to estimate minimum wage employment effects for these individuals. At the aggregate level, in Neumark and Wascher (2002) we attempted to classify state-year observations (in a probability sense) into one of three categories: observations for which the minimum wage was binding, so that teenage employment was determined by the labor demand curve; observations for which the minimum wage was not binding, so that teenage employment was determined by both supply and demand; and observations for which the monopsony model was relevant, so that employment was determined by the supply curve.

To do this, we estimated 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 non-binding regimes) and the point at which the labor demand curve intersects the marginal cost of labor curve (to differentiate a monopsony regime). As might be expected, this approach yields few observations in the monopsony regime. However, there are a substantial number of observations in the non-binding regime, allowing us to estimate minimum wage effects for those observations for which the minimum wage is more likely to be binding. The model is estimated using employment data on 16–24

[30] In addition to the research reviewed earlier, see also Pabilonia (2002), who focuses on the effects of minimum wages on employment of 14–16 year-olds.

year-olds and points to negative and significant effects of the minimum wage for observations in the binding regime. In particular, the estimated elasticities in the binding regime range from about -0.13 to -0.21, somewhat larger than the estimates from reduced-form models of youth employment. Perhaps more importantly, the results suggest that minimum wage effects from single-equation reduced-form regressions will be sensitive to the sample used for the estimation.

Other researchers have turned to cross-sections or panels of individual-level data to identify individuals likely to be directly affected by changes in the minimum wage. One of the first papers in the new minimum wage literature to take this approach was Currie and Fallick (1996), who used longitudinal data from the NLSY to study the employment effects of the increases in the federal minimum wage in 1980 and 1981.[31] In particular, these authors 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 use a linear probability model to compare subsequent employment rates for individuals who were directly affected by the increases in the nominal minimum wage with individuals who were not directly affected. They also estimate the model 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. Their preferred estimate suggests that individuals directly affected by the minimum wage increases in 1980 and 1981 were 3 percent less likely than other workers to be employed one year later. Given that the minimum wage rose about 15 percent over those two years, this estimate is consistent with an employment elasticity of about -0.2, although it does not take into account the possible decline in employment probabilities in year $t+1$ for individuals who were not employed prior to the minimum wage hikes.[32]

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[31] An earlier paper by Linneman (1982) also used this general approach.

[32] The authors report that they attempted to identify non-employed workers likely to be affected by the minimum wage by imputing their wage rates from a hedonic regression of

Card and Krueger (1995a) criticize this study on a number of grounds. One of their primary concerns seems to be Currie and Fallick's estimate of a large negative minimum wage effect for workers classified as not covered by the minimum wage. However, Currie and Fallick clearly believe that their attempt to identify uncovered workers was unsuccessful because the wage distribution for uncovered workers also exhibits a large spike at the nominal minimum wage in both 1980 and 1981.[33]

In a series of papers, Abowd et al. (1999, 2000a, 2000b) attempt to improve on this approach by respecifying the wage variable in real terms and identifying individuals who were "freed" by a decline in the real minimum wage to below their real wage as well as those who were newly bound by a nominal increase in the wage floor, thus introducing variation between workers at the same real wage level at different times. 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 $t + 1$. This variable is conceptually similar to the minimum wage variable used by Currie and Fallick (as well as to Card's fraction affected variable). In contrast, when the nominal minimum wage is unchanged between year t and year $t + 1$, they count an observation as affected 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 concept captures individuals who are no longer bound by the minimum wage because of its erosion in real terms.

Using longitudinal data for both France and the United States, Abowd et al. use this minimum wage measure to examine transitions into and out of employment. For the United States, which exhibits both increases and decreases in the real minimum wage, their results are

wages on a set of observed demographic variables. Although they found sizable negative employment effects for this group, they expressed skepticism about their identification scheme and did not report any point estimates.

[33] Currie and Fallick identify uncovered workers as those with a wage between the old and new minimum wage but who work in a sector in which coverage by the federal minimum wage is low.

mixed.[34] Abowd et al. (1999, 2000a) use data from the 1980s and find that the gradual decline in the real value of the minimum wage raised transition rates from non-employment to employment, which is consistent with a negative elasticity of employment with respect to increases in the minimum wage. However, Abowd et al. (2000b) use data from 1981 to 1991, thus including both decreases and increases in the real value of the federal minimum wage, and incorporating information on state minimum wages. In this case, they find little evidence of statistically significant effects of the minimum wage on either exit rates from or entry rates into employment.

In Neumark et al. (2004), we use individual-level matched observations from the CPS 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, as well as by Abowd et al., but is more general in that it estimates the effects of minimum wages at various points throughout the wage distribution. The state minimum wage changes allow us to avoid using higher-wage workers to construct a counterfactual for what would have happened to low-wage workers absent the minimum wage change. For example, when we study transitions from employment to non-employment, comparisons to higher-wage workers may not be picking up minimum wage effects if lower-wage workers have less stable employment histories and are more likely to leave employment even in the absence of a minimum wage increase.

In particular, we specify a model that interacts the change in the effective minimum wage for each state-month observation with a set of indicator variables that describe where each individual's wage stands in relation to the minimum wage. The model also includes interactions that capture differential changes in the dependent variable at different points in the wage distribution that are unrelated to minimum wage changes, as well as a set of demographic and skill-type variables and state-year interactions; in this sense, the minimum wage effects are identified from differential changes in the effective minimum wage for workers at similar points in the wage distribution. In addition, we adapt the econometric procedure to capture any lagged effects of changes in

[34] The results for France are discussed in Section 6.

the minimum wage and estimate the equation for wages, employment, hours (conditional on employment), and weekly labor income.[35] As in the studies by Currie and Fallick and by Abowd et al., we restrict the analysis to individuals employed in year t because we do not have an initial wage for those initially non-employed.

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. Wages rise for those who remain employed, but employment and hours decline, resulting in a net negative overall effect of the minimum wage on labor income among these individuals. 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. The effect on hours is particularly noticeable at the low end of the wage distribution, suggesting that employers also respond to minimum wages by shortening the workweeks of their lowest-paid employees. In addition, the results are not driven by the inclusion of teenagers or young adults in the sample. Estimates for a sample restricted to individuals aged 25 and over show a strikingly similar pattern.

There has also been research along these lines using Canadian data. Yuen (2003) uses the Canadian Labor Market Activity Survey, which contains weekly longitudinal labor market data. However, since many important variables, such as the consumer price index and provincial unemployment rates, are recorded less frequently, Yuen creates a quarterly panel and records individuals' employment status as of mid-quarter. Yuen estimates employment effects for 16–24 year-olds over the period 1988–1990. As Baker et al. noted, provincial variation in the minimum wage in Canada is extensive. Reflecting this, in the three-year period Yuen studies, there were 19 minimum wage changes.

The paper focuses on the definition of a control group for estimating minimum wage effects to try to address some of the ambiguity regarding the estimated employment effects in the recent literature. In particular, Yuen estimates models for the employment effects of minimum wages

[35] Including lagged effects complicates the estimation procedure because each individual is observed for only two years in the CPS.

in two ways – first, by using both high-wage workers from the same province and all workers in provinces without minimum wage changes as a control group, and then again using only low-wage workers from other provinces with no minimum wage change as a control group. In the second method, workers in a province with no change in the minimum wage are considered a low-wage control if their wage rate falls between the minimum wage of their province and $.25 above.

Like the papers just discussed, Yuen limits his sample to those who are initially employed and therefore only addresses possible transitions from employment to non-employment. The author identifies and uses at-risk individuals whose wage is between the old and new minimum wage. Dummy variables are also included for province, quarter, and year. He finds that estimates using the control group that includes high-wage workers are consistent with previous work, with large significant negative employment effects for the at-risk group. Employment elasticities can be calculated as roughly -0.75 to -0.84 for teens and -1.23 to -1.77 for young adults.[36] These are large elasticities, but they are based on a very narrow group of workers, and hence are not necessarily inconsistent with overall estimated elasticities for teens or young adults that are smaller in absolute value, echoing the earlier discussion. In contrast, estimates using only the low-wage control group lead to employment effects that are insignificant and near zero for both teens and young adults. These finding contrast with those in Neumark and Wascher (2004), which also uses workers in other geographic areas (in this case states) but in the same position in the wage distribution as controls.

However, Yuen illustrates that low-wage workers are not a homogenous group by breaking the low-wage group into two subgroups: "transitory" low-wage workers, who worked fewer than three quarters at low wages over the study period; and "permanent" low-wage workers, who had three or more quarters of low-wage employment. The author motivates this distinction by arguing that transitory low-wage workers more

[36] The author provides an average minimum wage increase (8.4 percent), which is used to calculate the elasticities. This average is calculated as the average percentage increase across all 19 increases in the minimum wage weighted by the number of at-risk individuals at the time of each provincial increase.

likely consist of those whose marginal productivity is higher than their current wage – for example, a student with a summer job. Yuen reports estimates with implied elasticities of employment of 1.18 (significant) for transitory low-wage working teens and 0.31 (insignificant) for transitory low-wage working young adults. For permanent low-wage teen workers (which is a bit of a misnomer, since no teenager can accumulate a long history of minimum wage work) the elasticity of employment is approximately -0.86 (significant), and for permanent low-wage young adults the estimated elasticity is -1.15 (significant). Furthermore, for the permanent low-wage workers, the estimates are not sensitive to the choice of control group. Yuen concludes that "when the treatment group is defined appropriately, the standard "textbook prediction" of a negative employment effect can still be retrieved" (p. 671). It is not immediately obvious that the textbook prediction should apply to what Yuen classifies as permanent low-wage workers, but not transitory ones. His argument, however, is that it is more likely that permanent low-wage workers are the intended beneficiaries of minimum wage laws, and in that sense the negative employment results for the permanent low-wage workers are potentially significant from a policy perspective.[37]

Campolieti et al. (2005b) apply a similar methodology to a different data set for the period 1993–1999 and find results more consistent with negative employment effects. In particular, these authors use longitudinal data from the Canadian Survey of Labor and Income Dynamics to examine the effect of provincial changes in minimum wages on the transitions from employment to non-employment among low-wage youths. As in Yuen (2003) and the earlier U.S. studies, the paper defines an at-risk group as consisting of those youths who resided in a province in which the minimum wage changes between year t and year $t+1$ and whose initial wage was between the old and new minimum wage. They then compare transition probabilities for these individuals with a control group consisting of young workers who resided in provinces in which the minimum wage did not change during that year. The study presents

[37] Moreover, the different results for permanent low-wage workers, if they hold for the United States, could help explain the adverse effects of minimum wages on low-income families reported in Neumark et al. (2005), since it seems likely that such workers are likely to be in low-income families.

estimates from comparisons with a variety of control groups, ranging from workers with a wage between the minimum wage and 25 cents above the minimum to all workers in the control set of province-year observations. In addition, they present evidence from both the standard "affected" indicator minimum wage variable and the wage gap variable used in previous analyses, as well as a variant 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.

Similar to Yuen, Campolieti et al. find large negative effects from the minimum wage (elasticities ranging from −1.61 to −1.24) when they use all youth workers from other provinces as the control group. However, they also report significant negative effects using low-wage control groups (MW + 0.25 to MW + 0.75), with elasticities ranging from −0.83 to −1.68. Converting these to an overall employment elasticity for youths by adjusting for the relative sizes of the at-risk group and low-wage control groups yields elasticity estimates between −0.33 and −0.54. The authors speculate that their finding of significant disemployment effects reflects the greater bite of the minimum wage in the 1990s than in Yuen's sample, although they present no direct evidence in support of this potential nonlinearity.

The question of the impact of the minimum wage on the least-skilled is sometimes framed in terms of "labor–labor" substitution – the hypothesis that a rise in the minimum wage prompts employers to hire a more-skilled workforce and hence impacts the least-skilled more adversely than might be indicated by a standard employment study. Connolly (2005) focuses on this issue directly.[38] Using SIPP data, she finds that the probability that a minimum wage worker (defined as earning a wage below 130 percent of the minimum prior to an increase) has less than a high school degree increases in states where the minimum wage increases, but is unchanged in states without minimum wage

[38] Lang and Kahn (1998) study the effect of the minimum wage on the distribution of employment between teenagers/students and adults working at the minimum wage. Their model predicts shifts in employment toward the more-skilled. They find some evidence that employment shifts toward teenagers and students, who may well be more skilled than adults at minimum wage jobs.

increases. She concludes that "the low-educated minimum wage workers benefit proportionally more than the high-educated" (p. 17). However, when the minimum wage increases, there is likely to be reduced demand for less-skilled workers and increased demand for more-skilled workers. In this case, the relative reduction in the share of minimum wage workers with higher education does not necessarily mean they are being displaced, but instead that they are now earning higher wages, as demand for more-skilled workers increases and their wages are bid up. Indeed her difference-in-differences estimates (Tables 6 and 7) are consistent with this. For males, minimum wage increases are associated with a higher rate of transition out of minimum wage jobs and into higher-wage jobs for those with more education, while transitions from above-minimum wage jobs into minimum wage jobs occur at a higher rate for those with less education. For females, the differences are much smaller or non-existent, implying that the qualitative results for males hold for the whole sample. Thus, her findings could arise simply from increased demand for more-educated workers in above minimum wage jobs.[39]

5.6 Other Issues and Approaches

We close this section by highlighting a couple of new avenues of research on the employment effects of minimum wages. First, there is a budding literature on the effects of minimum wages in structural equilibrium search models, which frequently takes as its starting point the positive employment effects of minimum wages found in some of the new min-

[39] The analysis is also potentially flawed because it focuses only on out-of-school individuals, and as noted earlier, there are important flows of teenagers between schooling and employment induced by minimum wage increases. Also, although not the focus of her paper, Connolly reports results that she claims are consistent with minimum wages increasing employment. In particular, she reports that there was a larger increase in minimum wage employment following a minimum wage increase in affected states than in control states. But if the minimum wage increases, more workers are caught below the new minimum wage (or in her case, the new minimum wage plus 30 percent). This says nothing about what happens to overall employment. Nor does the result that among high school dropouts minimum wage employment increased more in states raising the minimum than did minimum wage employment among more-educated groups imply that the less-educated gained. More of the less-educated are caught by any increase in the minimum.

imum wage research. Prominent examples include van den Berg and Ridder (1998), Flinn (2006), and Ahn et al. (2005).[40] These types of studies can provide answers about both employment effects and welfare effects, although the answers are highly dependent on the underlying assumptions. For example, the Flinn paper yields very different results depending on whether the contact rate (the arrival rate of contacts to job searchers) is endogenous or not. When it is exogenous, employment (of 16–24 year-olds) is increasing in the minimum wage up to about $8 (in data from the period when the federal minimum went from $4.25 to $5.15), and minimum wages increase welfare. But when the contact rate is treated as endogenous, the minimum wage reduces employment sharply, even at a minimum wage of $5, and welfare is decreasing in the minimum wage for a minimum above $3.33. Furthermore, the data have difficultly distinguishing between these two cases.

As should be obvious by now, we do not focus on this literature in our review. Rather, we focus on research that, as Flinn accurately states, pursues "the more limited objective of carefully describing the observed effects of recent minimum wage changes using quasi-experimental methods" (2006, p. 1013). We do this in part because most of the debate about minimum wages focuses on the results from quasi-experimental empirical studies (and it is a sufficiently daunting challenge to summarize and synthesize this literature), and in part because we find these results more defensible, while recognizing the limitations of what this approach can tell us. We also feel that the search model approach to minimum wages is as yet highly dependent on unsettled theoretical questions regarding model specification, and we do not know how to discern the validity of alternative estimates. In addition, based on the evidence reviewed thus far (and the international evidence that follows), we are much less convinced that the textbook model cannot account for the existing findings, although the equilibrium search approach may still turn out to provide valuable insights and a better accounting of the evidence.

Second, some very recent research has focused on testing for monopsony via evidence on prices, which is an indirect approach to exploring

[40] Arcidiacono and Ahn (2004) present a simplified discussion of such a model.

whether minimum wages could lead to employment increases. In particular, Aaronson et al. (2005) and Aaronson and French (2007) look at the restaurant industry, where evidence of positive employment effects has prompted researchers to propose monopsony explanations. Aaronson et al. (2005) present evidence that minimum wage increases lead to price increases and consider how that result conforms to the predictions of alternative theoretical models. In the standard competitive model, for example, minimum wage increases cause prices to rise and employment to fall. In contrast, the authors show that monopsony models and efficiency wage models that have a similar flavor (for example, Rebitzer and Taylor, 1995) generally imply that prices either fall or do not change in response to a minimum wage increase, if employment rises. One exception is a version of the monopsony model in which a significant number of firms exit in response to a minimum wage increase, in which case prices and employment can both rise. However, they discount this possibility based on what they claim are small observed exit rates in the restaurant industry, although they do not present direct evidence on exit or on how much would be needed to overturn their result.[41]

Based on these findings, Aaronson and French (2007) calculate employment effects in the restaurant industry via a calibrated competitive model of the labor market. They find that with their calibrated substitution elasticities, the benchmark competitive model that is consistent with the price increases found in the earlier paper 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, but – based on the earlier paper on price responses – assume that few

[41] Wessels (1997) suggests another possibility not considered by Aaronson et al. He presents a model of monopsony in the restaurant industry based on the notion that with tipped workers, 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, cause prices to rise as well. Although Wessels does not analyze price effects directly, he does find that the ratio of employment to sales in the restaurant industry (which he views as a proxy for total hours of servers) is positively related to state-specific tipped minimum wages for relatively low levels of the minimum wage.

employers increase employment in response to the minimum wage. This results in only slightly smaller employment elasticities.

Of course, the standard monopsony model implies that if the minimum wage is set high enough, prices will rise and employment will fall, just as predicted by the competitive model. In this case, however, the monopsony model cannot be used to explain zero or positive employment effects of minimum wages, so those arguing for a monopsony-based explanation must implicitly believe that the minimum wage is in the range where employment will increase, in which case prices should not rise. Aaronson et al. therefore regard their results for prices as providing evidence against the view that monopsony power can explain findings of employment effects near zero in studies of the restaurant industry. Of course, as the earlier discussion suggests, the evidence for the restaurant industry is, on balance, more consistent with negative effects of minimum wages on employment.

6

International Evidence

The international evidence on minimum wages is large and growing, and covers both industrialized and developing countries. We cannot cover the international evidence as extensively as we do the evidence for the United States, if for no other reason than that some of the studies are written in languages other than English.[1] Our review of the international evidence may therefore provide a less reliable description of the distribution across studies of estimated employment effects of minimum wages. In this section, we begin with a review of the evidence for the industrialized countries, and then turn to studies of developing countries.[2] As we have already discussed the available evidence for Canada and Sweden, we do not repeat that material here, although we

[1] The language barrier is not necessarily innocuous. For example, in our study of minimum wage effects in the OECD countries (Neumark and Wascher, 2004, discussed below), 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.

[2] There is also some emerging work on the effects of the minimum wage in transition economies (for example Ericksson and Pytlikova, 2004), which we do not cover in this survey.

do include the findings in the summary table for employment effects in industrialized countries (Table 6.1).

6.1 Industrialized Countries

6.1.1 Panel Studies

Two studies estimate minimum wage effects using data from a panel of industrialized countries, essentially paralleling the state-level panel data studies for the United States. The first such study is a report written by economists at the Organization for Economic Co-operation and Development (OECD, 1998), who motivate their use of international comparisons to study the employment effects of minimum wages by noting that national wage floors vary considerably more across countries than over time within a country.[3] The study begins by summarizing minimum wage setting and levels in OECD countries that have a national minimum wage.[4] The authors construct a measure of the relative minimum wage by dividing the nominal minimum wage by median earnings of full-time workers.[5] This ratio, which varied in 1997 from 0.36 in Spain to 0.69 in France, is used in a set of pooled regressions with data for seven to nine countries from 1975 to 1996.[6] In particular, the authors regress the employment-population ratio on the relative minimum wage; a business cycle control (either the prime-age male unemployment rate or the output gap); institutional factors such as union density, the unemployment benefit replacement rate, and the payroll tax rate; and fixed country and year effects. The authors' preferred specifications, which also control for country-specific serial correlation and heteroscedasticity, generally show negative and statistically significant disemployment effects for teenagers, and negative but only marginally significant or insignificant effects for 20–24 year-olds. The estimated employment elasticities for teenagers range from −0.07 to

[3] See also Hamermesh (2002).
[4] Bazen (2000) also provides details on minimum wage setting in various OECD countries.
[5] For a few countries, median earnings are not available and so mean earnings are used instead.
[6] The countries included in the regression are Belgium, Canada, France, Greece, Japan, the Netherlands, Portugal, Spain, and the United States. A lack of data for Portugal and Spain limited some of the analyses to the other seven countries.

Table 6.1 International studies of minimum wage employment effects, industrialized countries

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Panel Studies					
OECD (1998)	Across countries and over time	Teens (15–19), young adults (20–24), and adults (25–54)	National sources	Teens: −0.07 to −0.41 20–24: −0.03 to −0.10 25–54: 0 to 0.02 Estimates for teens mostly significant	
Neumark and Wascher (2004)	Across countries and over time	Teens (15–19) and youths (15–24)	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					
Baker et al. (1999)	Across provinces and over time	Teenagers (15–19)	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**	

Table 6.1 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Yuen (2003)	Across provinces and over time	Teenagers (16–19) and young adults (20–24)	Labor Market Activity Survey, 1988–1990	Permanent low-wage teen workers: −0.86 (significant) Permanent low-wage young adult workers: −1.15 (significant) Transitory low-wage teen workers: 1.18 (insignificant) Transitory low-wage young adult workers: 0.31 (insignificant)	
Campolieti et al. (2005a)	Across provinces and over time	Teenagers	Survey of Labor and Income Dynamics (1993–1999)	Multinomial logit analysis: Non-employed and enrolled: 0.72 Employed and enrolled: −0.57 Employed and not enrolled: −1.92 Idle: 0.02 Net employment: −2.49; significant Net school enrollment: 0.15; not significant	
Campolieti et al. (2005b)	Across provinces and over time	Youths (16–24)	Survey of Labor and Income Dynamics (1993–1999)	**−0.33 to −0.54**	
Campolieti et al. (2006)	Across provinces and over time	16–19, 20–24, and 16–24 year-olds, including full-time versus part-time and non-enrolled	April Labor Force Surveys, 1981–1997	**Teens: −0.17 to −0.44** **20–24 year-olds: −0.14 to −0.43** **16–24 year-olds: −0.17 to −0.44**	

Table 6.1 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Sweden					
Skedinger (2006)	Union negotiated minimum wages in hotels and restaurants	All workers	Surveys from Confederation of Swedish Enterprise, 1979–1999	Job separations in response to minimum wage increases: elasticity 0.58 overall; 0.36 to 1.00 for 20–65 year-olds; 0.77 to 0.80 for teenagers (although −0.12 to −0.14 and insignificant for the 1993–1998 subperiod) Job accessions in response to minimum wage decreases: 0.84 overall; 0.45 to 0.55 for teenagers	Wage floors vary by worker characteristics, so may not be applicable to uniform minimum wages
United Kingdom					
Machin and Manning (1994), Dickens et al. (1999)	Wages Councils	All workers in covered (low-wage) industries	New Earnings Survey, Employment Gazette, 1978–1992	0.05 to 0.43	Questions about exogeneity of minimum wages
Dolado et al. (1996)	Abolition of Wages Councils	Workers in Council and non-Council sectors	Quarterly Labor Force Survey Micro Data	Relative increases in hiring rate and employment in Council sector after Councils abolished	Questions about exogeneity of minimum wage increases chosen by Wages Councils, and hence of variation created by abolition of Wages Councils

Table 6.1 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Machin et al. (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 (2002)	Variation across local areas in effect of imposition of national minimum wage in 1999	All workers and various lower skill groups	New Earnings Survey, 1998, 2000; Labor Force Survey Local Areas Data; Annual Business Inquiry, 1998-1999	Wide variety of estimates, and not easily translated into elasticities given that estimates are for introduction of new minimum wage; many positive and many negative estimates, none significant	Ignores possible workings of youth subminimums; many estimates for all workers rather than young workers; analysis of less-skilled individuals does not provide information on wage effects; focus is on short-run effects, and no evidence of lagged effects
Stewart (2004b)	Variation across workers at different points of the wage distribution	Adult men and women (aged 22 and over), and young men and women (aged 18-21)	Matched Labor Force Survey, March 1997–March 2000; British Household Panel Survey, Fall 1994–Fall 1999; New Earnings Survey, April 1994–April 1999	Elasticities for transitions to non-employment almost always insignificant, more likely positive than negative	Focus is on short-run effects, and no evidence on lagged effects
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 decline by 1 to 2 hours, with the reduction occurring at a lag of approximately one year**	No parallel evidence on employment, despite suggestion of lagged hours reductions

Table 6.1 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Galindo-Rueda and Pereira (2004)	1. Variation across firms in exposure to higher minimum based on matched worker data	All	Annual Business Inquiry and New Earnings Survey, 1994–2001	Manufacturing and services: small disemployment effects, insignificant	Highly non-random sample because of worker-firm match, and potential measurement error in exposure to minimum wage
	2. 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**	
	3. Variation across regions in fraction below	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 reduced firm entry in low-wage sectors**	
Australia					
Leigh (2004a)	Minimum wage increases in Western Australia relative to rest of country	Aggregate, and disaggregated by age and sex	Labor Force Survey, 1994–2001	Aggregate: −0.25 to −0.40 15–24: −1.01 15–24, male: −0.68 15–24, female: −1.44 Older groups: −0.03 to −0.14 (mostly insignificant)	Very large elasticities for aggregate employment and for 15–24 year-olds

Table 6.1 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Harding and Harding (2004)	State minimum wage increases	Minimum wage workers	Survey of employers, 2003	−0.2	Employer attributions of employment changes to minimum wage increases unlikely to be reliable
Mangan and Johnston (1999)	State minimum wage differences (over time and cross-sectionally)	15–19 year-olds	Panel analysis: Australian Bureau of Statistics annual data, 1980–1995 Cross-section analysis: unit record census data, year unspecified	Panel analysis: −0.21 to 0.08, almost all estimates negative, none significant Cross-section analysis: full-time, −0.05 to −0.31, generally significant	Source of minimum wage variation unclear; model should include non-teen minimum wage
Junankar et al. (2000)	Time-series variation in youth minimum wages	16–20 year-olds	Quarterly data, 1987–1997 (source unspecified)	Time-series regressions by industry, age, and sex: effects insignificant and often positive, except for retail where there is some evidence of disemployment effects; elasticities for retail range from −1.6 to −23.1	Absurdly large elasticity estimates; likely weak identification given short time-series; model should include non-teen minimum wage
New Zealand					
Maloney (1995, 1997)	National minimum wage for workers aged 20 and over, and introduction of teen minimum wage	Young adults (20–24) and teenagers (15–19)	Household Labor Force Survey (HLFS), 1985–1996	Effect of adult minimum: 20–24: −0.1 to −0.4 15–19: −0.1 to 0.4 No effect of introduction of teen minimum on teen employment	Teen minimum omitted from young adult equation, could bias estimates of teen and young adult effects

Table 6.1 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Chapple (1997)	National minimum wage for workers aged 20 and over	Young adults (20–24)	Time-series: HLFS, 1985–1997 Cross-industry: Labor and Employment Gazette and Statistics New Zealand Quarterly Employment Survey, 1980–1997	Time-series: 20–24: −0.17 to −0.34 Cross-industry: −0.06 to −0.10 Separate time-series by industry: estimates centered on zero	Ignores introduction of teen minimum wage in time-series analysis; in cross-industry analysis year effects omitted, and effects very sensitive; separate time-series by industry have few degrees of freedom
Hyslop and Stillman (2007)	Introduction of higher minimums for 16–17 and 18–19 year-olds	Teens, 20–21, and 20–25 year-olds	HLFS, 1997–2003	Employment of 16–17 and 18–19 year-olds rose relative to 20–25 year-olds, but fell relative to 20–21 year-olds in years corresponding to largest minimum wage increases	Inspection of graphs and regressions give some suggestion of negative employment effects
France					
Dolado et al. (1996)	1. Higher national minimum wage increases in early 1980s than late 1980s	All	Enquête Emploi, 1981–1989	Weak evidence that low-wage groups suffered larger employment losses in period when national minimum increased more sharply	No wage impact, so no employment impact expected
	2. Differential impact of national minimum wage increases across regions with varying initial wages	All	Declaration Annuelle de Salaires, 1967–1992	Regions with low initial wages experienced greater employment growth	Fluctuation in labor demand could explain results; not restricted to low-skill workers

Table 6.1 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Bazen and Skourias (1997)	National minimum wage increases across sectors with different percentages of minimum wage workers	Youths (under age 25)	French Labor Force Survey, 1980–1984	Youth employment fell more in sectors where minimum wage was more binding	Questions about specification
Abowd et al. (2000a)	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 24 not protected by employment promotion contracts:** **Men, 25–30: −4.6** **Women, 25–30: −1.38** **Men, 20–24: −0.77** **Women, 20–24: −1.21** **Men, 16–19: −0.08** **Women, 16–19: 0.46**	
Netherlands					
Dolado et al. (1996)	Declines in youth subminimums relative to adult minimum in 1981 and 1983	Youths (17–22)	Labor Market Survey, 1979–1985	Youth employment fell by less or rose over this period in occupations most intensive in use of young, unskilled workers, relative to overall changes in youth employment	

Table 6.1 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Spain					
Dolado et al. (1996)	1. National minimum wage, and variation in effects across industries where minimum wage more or less binding	Teens (16–19)	Contalilidad Nacional Sectorial	Teens: −0.15, stronger in industries where minimum wage more binding	Specifications exclude year effects
	2. Sharp increases in minimum for 16 year-olds and more modest increase for 17 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 20–24 year-olds**	
Portugal					
Pereira (2003)	Abolition of teenage subminimum wage in 1987	18–19 and 20–25 year-olds	Quadros de Pessoal, 1986–1989	**Teen employment (and hours) declined relative to employment of 30–35 year-olds, with elasticity of −0.2 to −0.4; substitution towards 20–25 year-olds**	
Portugal and Cardoso (2006)	Abolition of teenage subminimum wage in 1987	16–65 year-olds	Quadros de Pessoal, 1986–1989; Labor Force Survey, 1986–1989	Overall teen employment grew faster than employment of 20–25 year-olds or older workers following minimum wage increase; main results concern effects of minimum wage on accessions and separations	No regression analysis of net employment effects or evidence of failure to replicate Pereira's results

Table 6.1 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Greece					
Karageorgiou (2004)	Variation in teen and young adult minimum wages relative to average adult wages	15–19 and 20–24 year-olds	Labor Force Survey, Statistical Yearbooks of National Statistical Service of Greece, United Nations Educational Scientific and Cultural Organization, OECD: 1974–2001 for young adults; 1981–2000 for teens	Teens: 0.22 to 0.63 (larger estimates significant) Young adults: −0.05 to −0.12 (insignificant)	Little real time-series variation, unexpected results for other controls, failure to account for minimum wages for other groups

Note: Results from studies we regard as more reliable tests of employment effects of minimum wages are highlighted. Effects are sometimes long-run effects from dynamic models, depending on which were emphasized by authors.

−0.41, with the larger estimates evident in the sample that excludes Portugal and Spain. For 20–24 year-olds, the elasticities range from −0.03 to −0.10, with only the latter estimate statistically significant at conventional levels. The study also reports results for adults, but these show no effect of the minimum wage on their employment rates.

Although the OECD study includes a few variables to account for institutional differences across countries, critics of the cross-country approach stress the difficulty of distinguishing the impact of minimum wages from other labor market policies and institutions and stress the importance of considering how the latter may influence the impact of the minimum wage. In a general sense, a large literature has explored variation across the industrialized countries in other labor market policies and institutions, with Scarpetta (1996) and Nickell and Layard (1999), among others, presenting cross-country evidence on the effects of a variety of labor market institutions on employment and unemployment, and Blanchard and Wolfers (2000) and Belot and Van Ours (2001) emphasizing potential interactions between institutions and economic shocks, and between different types of labor market institutions. From a theoretical standpoint, Coe and Snower (1997) develop a model in which various labor market policies – including the minimum wage – can have complementary effects on labor market outcomes.

To address these criticisms, we studied the effects of minimum wages across a larger number (17) of the OECD countries, taking account of variation in a variety of labor market policies and institutions (Neumark and Wascher, 2004). The inclusion of additional countries in the analysis increases the variation in the minimum wage variable; for example, in the last year of data for each country the relative minimum wage ranges from 0.32 to 0.71, and changes within countries over the sample period range from −0.18 to 0.08, with five countries exhibiting declines of 0.1 or greater. The use of additional countries also increases variation in the institutional variables included in the model, thus increasing identification along that dimension as well.

The study begins with the standard panel data specification for employment, including a one-year lag of the minimum wage relative to the average wage, aggregate labor market and demographic controls, fixed country and year effects, and country-specific time trends. We also estimate a dynamic specification that includes a lagged employment rate. The models are estimated for teenagers (aged 15–19) and youths (aged 15–24), with data extending from the mid-1970s through 2000. The results consistently point to negative effects of the minimum wage on employment. For the standard model, 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 the dynamic specification are somewhat larger: roughly −0.40 for teenagers and −0.23 for youths.

We then augment the models to control for institutional differences in other characteristics of the minimum wage policies in each country, as well as for cross-country differences in other labor market policies. Following the theoretical argument of Coe and Snower, we also include interactions of the minimum wage with indicators for these institutional and policy differences. With regard to minimum wage systems, the strongest evidence is that the negative effect of the minimum wage on teenage or youth employment appears only in countries without a youth subminimum, consistent with the hypothesis that a higher minimum wage might induce substitution toward young workers when there is a subminimum. There is also evidence, although 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. This evidence is consistent with the argument that collective bargaining takes more explicit account of (and hence avoids) potential disemployment effects in setting minimum wages.

We also interact the minimum wage variable with country-specific measures of the rigidity of labor standards (for example, 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. In accordance with expectations, minimum wages have more adverse effects when labor standards 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, there is quite strong evidence that when employment protection is high, the disemployment effects of minimum wages are muted.[7] The same is true when active labor market policies are more prevalent, presumably because some of those who would otherwise be considered non-employed 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 (Coe and Snower, 1997); in contrast, Dolado et al. (1996) suggest that sources of wage compression – such as unions – can make the minimum wage less relevant because they reduce the share of workers at or near the minimum.[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, while 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 the 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/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.

[7] Dolado et al. (1996) also discuss some of these issues, pointing out, for example, that with higher firing costs, the adjustment of employment to an increase in the minimum wage may be smaller or slower.

[8] Of course, a fraction below the minimum measure would capture this phenomenon, so in this case the findings for the interactive effects may depend on the specification of the minimum wage variable, an issue we have not explored.

6.1.2 Studies of Individual Industrialized Countries

Although the country panel studies yield some interesting findings, it is difficult to effectively capture the institutional and policy environment of any particular country with a few regressors and their interactions, making these studies less informative about the effects of the minimum wage on employment in a given country. To provide this evidence, which is of greater relevance to policymakers, researchers have conducted studies of minimum wage effects for specific industrialized countries. Because most industrialized countries have a uniform minimum wage that varies only over time, the challenge is to identify an appropriate control group, similar to the issues that confronted U.S. researchers prior to the proliferation of state-specific minimum wages. However, there a few countries in which the minimum wage varies by age or industry. Many of the first-generation country-specific studies are reviewed and discussed in Dolado et al. (1996), so we do not repeat that material here. Instead, we focus on the more recent evidence of minimum wage effects in Europe and elsewhere, including new evidence that Dolado et al. report.[9]

6.1.2.1 United Kingdom

We discuss most extensively the evidence for the United Kingdom, for which there has been a large number of interesting studies. Some of the U.K. studies found no negative effects of minimum wages or even positive effects, and these results seem to have played an important role in undermining the earlier consensus among economists that minimum wages reduced employment. Moreover, the authors of these papers have been influential in promoting monopsony explanations of their findings (see especially Manning, 2003) – not in the context of the company town of labor economics textbooks, but in dynamic monopsony models along the lines of those developed by Burdett and Mortensen (1998).

The United Kingdom experienced two policy changes that have been used to identify the effects of minimum wages. Prior to the early 1990s

[9] That paper presents analyses for a number of countries, with each analysis apparently conducted by a subset of the paper's eight authors.

the United Kingdom had a system of Wages Councils, which consisted of equal numbers of employer and worker representatives together with independent members appointed by the government, and which set minimum wages in low-wage sectors. However, the Wages Councils were abolished in 1993, and from 1993 to 1998 there was no minimum wage in the United Kingdom. Subsequently, in 1999, a national minimum wage was introduced.

In their 1994 paper and in a later paper co-authored with Richard Dickens (Dickens et al. (1999)), Machin and Manning study the period from 1979 to 1992, during which minimum wages declined relative to average wages in 18 industries covered by the Wages Councils.[10] The authors first establish that the minimum wages were binding by verifying that an increase in the minimum wage in each Council-year observation raised wages at the bottom of that sector's wage distribution. This result is particularly important because the authors include all workers in an industry in their study rather than just teenagers or young adults. In addition, earlier research had raised doubts about the Wages Councils' ability to enforce minimum wage rates, which these results appear to put to rest. The papers then 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 and often quite large with elasticities as high as 0.43. Moreover, the estimates remain positive when lags are included.

There are legitimate questions about the source of minimum wage variation in these data. Committees of workers and owners may set minimum wages in such a way that boosts the pay of low-wage workers relatively more when conditions in the industry are (or are projected to be) good, which would impart a positive bias to estimates of the employment effects. On the other hand, the authors' minimum wage variable is a relative wage measure, so the story would have to be more complicated than just raising the minimum wage in response to

[10] The empirical analyses in these two papers are very similar, and the qualitative conclusions are the same. Our discussion focuses on the more recent paper, which is based on a slightly longer time period and, in some specifications, includes lags.

a general increase in demand. Of course the potential problem of the endogeneity of minimum wage increases is not unique to this study, and 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 more regard for political than economic timing).

The Dickens et al. paper acknowledges the endogeneity problem. However, the authors cite discussions with independent members of Wages Councils (although in a footnote they only mention one) as 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" (p. 8). That anecdote provides some reassurance, although more systematic evidence on what influenced the minimum pay rates set by Wages Councils – admittedly, no simple task – would be preferable. In any event, while the potential for endogeneity bias in these studies is of some concern, it does not follow that such a bias would be large enough to overturn the finding that the minimum wages set by Wages Councils in the United Kingdom had positive employment effects. At the same time, one should be cautious in presuming that these results carry over to the effects of legislated minimum wage changes, especially when they are nationwide.

Although Dickens et al. are inclined toward a monopsony interpretation of their findings, the specifications that show the largest positive effects are arguably the least defensible. In particular, many of the specifications estimated by the authors include a control for sales in the industry covered by the Wages Council. However, conditioning the results on sales is problematic because an important channel through which the minimum wage is thought to influence employment is by raising labor costs and prices, which reduces product and labor demand. The authors do instrument for sales with lags in order to remove the contemporaneous endogeneity between sales and employment.[11]

[11] We are skeptical of using lags as instruments in dynamic panel data models because identification requires the exclusion of lagged values from the equation of interest, yet

However, their reduced-form estimates, which are generally smaller and not statistically significant, strike us as more meaningful.

Moreover, more convincing evidence in favor of the monopsony model requires the researcher to tie the minimum wage effects more explicitly to monopsony power – that is, to show that the positive effects predicted over some range of the minimum wage actually arise over that range – or to provide additional evidence that confirms or contradicts other explicit predictions of the monopsony model (as, for example, in Aaronson and French, 2007). Dickens et al. take a small step in this direction. They develop a stylized theoretical model that predicts, as does the textbook monopsony model, that below some level a higher minimum wage will increase employment, while above this level a higher minimum wage will reduce employment. They then report evidence that the positive effects of the minimum wage on employment are stronger at low values of the minimum wage, while the effect of the minimum wage on employment is essentially zero at higher levels of the minimum. However, this is not really what the model predicts, and so, in the end, the correspondence between the empirical findings in the paper and the theoretical implications of the monopsony model is not particularly compelling. Regardless, we think this is at least a step in what we consider a useful direction of inquiry.[12]

Moving chronologically through the changes in minimum wage policy in the United Kingdom, Dolado et al. (1996) present evidence stemming from the abolition of the Wages Councils in 1993. The authors find that eliminating the minimum wage did not result in sharp declines in wages in these sectors, or (in the retail sector, which they examine more closely) to the disappearance of a spike in the wage distribution at the minimum wage, although the spike became less pro-

theory provides little guidance in specifying the appropriate lag length of the underlying model. The same issue arises when the authors instrument for minimum wages with lags. They note that there is reason – as discussed above – to believe that minimum wage increases are driven by past minimum wage increases. However, that does not imply that lagged minimum wages can be excluded from the employment equation.

[12] As noted above, we presented related evidence in Neumark and Wascher (2002) suggesting that some state-year observations could be characterized as being on the upward-sloping portion of the labor supply curve; but we find that the monopsony model, in general, does not fit the data significantly better than a competitive model.

nounced. As the authors point out, however, employers may have been reluctant to cut nominal wages for their current employees, and relative wage concerns may have limited the willingness of employers to bring in new workers at wage rates below the minimums previously set by the Wages Councils.

Turning to employment effects, the authors present data on the share of total employment accounted for by the Wages Councils industries before and after the abolition of the Councils and compare hiring rates 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 behavior of the Wages Council sector relative to the rest of the economy" (1996, p. 355). However, we do not read the evidence this way. Using average employment totals for the three quarters before and two quarters after the abolition of the Wages Councils, it is clear that employment grew more rapidly in the Wages Councils industries following the elimination of the minimum wage; employment in this sector grew by 1.29 percent, while falling trivially (by 0.04 percent) in the non-Wages Councils sector (see their Table 10). Moreover, a similar pattern is evident from the data 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). Because the difference in hiring rates increased by more than the difference in exit rates, 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.

Finally, a number of recent studies have examined the effects of the introduction of a national minimum wage in April 1999. Machin et al.

(2003) focus on the low-wage residential care home (nursing home) sector in the period from nine months before to nine months after the minimum wage was implemented. Although not central to their paper, the authors first describe the behavior of aggregate employment in this sector, which trended up rather strongly from 1994 through 1999 and then flattened out; this pattern is evident in the data even after controlling for the aggregate unemployment rate and despite the fact that demand, in the form of the number of people aged 75 and over, likely kept rising.[13] Although suggestive of a negative effect of the minimum wage on employment, this simple analysis ignores other changes that may have influenced the nursing home sector during this time period and does not establish that employment fell at the establishments that were more strongly impacted by the minimum wage.

To study the latter question, the authors conducted a survey of nursing homes and collected data on employment, hours, and wages at each establishment. They measure the extent to which the minimum wage was binding at each establishment by calculating both the share of workers initially paid less than the minimum and the average wage gap (hours weighted). Although the response rate to the survey was low (20 percent), the data suggest that the introduction of the minimum wage boosted wages in this sector. In addition, the evidence on employment and hours points to adverse effects of the minimum wage on both. The estimated employment effects are mostly statistically significant and range from -0.08 to -0.39.[14] Reiterating the point we made about other studies of specific industries, these estimates are not necessarily informative about the overall employment effects of minimum wages on low-skill individuals. However, they do seem to establish that conventional effects of minimum wages can be found in the low-wage sector in the United Kingdom. Exactly why these findings differ from those reported in the studies of the Wages Councils is unclear,

[13] They do not report the aggregate regression with this control as well, which we suspect would suggest even more of a downturn.

[14] The authors also speculate that the effects might be larger over the longer term. In this context, they report evidence suggesting that the minimum wage boosted closures, although these estimates were not statistically significant. Machin and Wilson (2004) extend this analysis to include the 2001 increase in the minimum wage and report similar results for both employment and closures.

although the focus of this study on a single sector 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" (p. 155).

In a series of papers, Stewart (2002, 2004a, and 2004b) and Stewart and Swaffield (2006) provide a broader investigation of the effects of introducing the national minimum wage in the United Kingdom. In particular, Stewart (2002) follows the approach taken by Card (1992b) to test whether employment changes differed across 140 geographic areas in which the introduction of the national minimum wage had a varying impact on wages. He first uses information from the New Earnings Survey to document that wages at the bottom of the wage distribution showed a larger increase following the introduction of the minimum wage in areas that had a higher share of workers paid below the new minimum.[15] Stewart then uses a fraction affected variable to test for the presence of employment effects. He also presents results from a difference-in-differences estimator that compares employment changes in high wage areas with employment changes in low wage areas.

For workers covered by the minimum wage (ages 18 and over), Stewart's estimates of the effect of the minimum wage on employment are generally negative, but not statistically significant. However, his point estimates are suggestive of potentially sizable effects. For example, the difference-in-differences estimate in the first row of his Table 3 indicates that the introduction of the minimum wage reduced the employment rate by 2 percentage points, while raising wages at the 5th and 10th percentile about 5 percent. If we treat this wage increase as the effective increase in the minimum, then the implied elasticity is

[15] Because the samples are relatively small in some areas, this finding might also reflect regression to the mean. This possibility could have been explored by examining evidence on wage declines among the highest wage areas, or by showing that the same result at the lower end of the wage distribution was not present in years prior to the imposition of the national minimum wage. For example, Machin et al. (2003) do the latter exercise for nursing home workers, and find some relationship, albeit a weaker one, in the period prior to the introduction of the national minimum wage.

-0.4.[16] The fact that an estimate this large is not detectable as statistically significant is suggestive of deficiencies in either the data Stewart uses or in his research design.

Stewart also estimates employment models for a variety of low-wage groups of workers (women, those with less tenure, less-skilled occupations, and so on), to see if the impact of the minimum wage is more apparent for workers more likely to be affected by it. In general, the estimated effects are smaller, more often positive than negative, and never significant. However, the samples are considerably smaller for these subgroups, and the paper does not present any evidence on the effects of the new minimum on wages for these groups. Finally, most of the employment models are estimated only over the one-year window surrounding the introduction of the new minimum wage, which may not be long enough to observe an effect on employment.[17] This short-term focus is not unusual, but given the evidence of lagged effects in other studies, it would have been preferable for the employment analysis to cover a period at least as long as the wage analysis, if not longer.

Stewart (2004b) looks at the effects for workers in different parts of the wage distribution prior to the minimum wage's introduction, building on the framework used in Abowd et al. (1999), Neumark et al. (2004), and Yuen (2003). In particular, he uses a standard difference-in-differences approach, comparing the change in the probability of a transition from employment to non-employment for those initially paid less than the new minimum wage to the change in transition probabilities for workers who were initially paid just above the new minimum wage. One potential drawback to this identification strategy is the possibility that aggregate influences on transition probabilities 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.

[16] Stewart also reports estimates for youths and finds either weak positive or weak negative effects. However, the 1999 legislation included a youth subminimum, which may have induced substitution toward young workers.

[17] The exception is the analysis of data from the New Earnings Survey (NES), for which the analysis focuses only on employment changes among those individuals who were initially employed.

The empirical analysis uses three different data sources. Stewart first establishes that the introduction of the minimum wage boosted wages of workers whose wages were initially below the minimum. Turning to the results for employment, he consistently finds small and insignificant effects that are generally positive for men and more mixed for women, and he presents a variety of robustness checks that yield the same answer. However, Stewart does not consider the possibility that minimum wage effects on employment may occur with a lag. Of the three data sets, one (the NES) ends in the same month as the imposition of the minimum, one extends eight months beyond this month, and the third extends 11 months beyond it, raising questions about whether the sample period is too short to adequately pick up the full impact of the minimum wage.[18]

Stewart and Swaffield (2006) extend the analysis to an investigation of the effects of the minimum wage on hours worked by workers who remained employed, appealing both to U.S. studies suggesting that the minimum wage reduced hours worked and to reports by the Low Pay Commission that many U.K. employers responded to the introduction of the minimum wage by reducing hours. This paper uses the same empirical framework as in Stewart (2004b), but includes a lagged effect of the minimum wage on hours. The estimates show a small and insignificant contemporaneous effect of the minimum wage on hours, paralleling the employment results in the earlier papers. However, the evidence also suggests that the longer-run effects of the minimum wage are more adverse. In particular, the lagged effect on hours is always negative, larger in absolute value, and generally, although not always, statistically significant. Summarizing the results, Stewart and Swaffield conclude that the minimum wage led to reductions of one to two hours per week for affected workers.[19] However, the evidence of lagged mini-

[18] In a brief note, Stewart (2004a) extends this analysis to include the 2000 and 2001 increases in the minimum wage. However, this analysis also focuses only on the short-run effects of minimum wages. This paper also attempts to account for the possibility that macroeconomic influences had different effects on the transition rates of affected and non-affected workers. However, the identification strategy used for this purpose requires rather strong assumptions, and, of course, this is not the only source of potential differences in changes for higher-wage and lower-wage workers.

[19] The authors do not report the mean level of hours in their sample, but if we assume that the average workweek is 30 hours, then the decline in hours is on the order of 3 to 7

mum wage effects on hours raises the question of whether there might also be lagged effects on employment, and, in this regard, it is surprising that this paper did not consider this possibility, especially given that the contemporaneous effects on hours were similar to what Stewart's earlier papers showed for employment.[20]

Finally, Galindo-Rueda and Pereira (2004) use firm-level data to study the introduction of the national minimum wage in the United Kingdom. Their identification strategy, like many of the other studies, compares changes at firms more affected by the minimum wage relative to firms less affected, although they implement this strategy in a number of different ways. First, they match firm-level data from the Annual Business Inquiry (ABI) with individual-level data from the NES to create a data set that has detailed information about the characteristics of each firm and information on the internal distribution of wages in each firm. However, because the NES randomly samples only about 1 percent of workers, larger firms are much more likely to have enough matched workers to make a reasonable inference about the internal wage distribution. As the authors acknowledge, this leads to a sample that is heavily biased toward larger firms, whereas smaller firms may be more adversely affected by minimum wages. In addition, with few matched workers per firm, the sample-based estimates of the fraction of workers in each firm that earn below the minimum wage (or some other wage floor) may be imprecise. Despite these limitations, the authors report regression results for employment and other outcomes from 1994 through 2001, using specifications that include a

percent. Using our earlier calculation that places the effective minimum wage increase at about 5 percent, we get implied elasticities ranging from about −0.6 to roughly −1.4.

[20] Connolly and Gregory (2002) also study the effects of the introduction of the national minimum wage on hours, although they limit their analysis to women. They employ a very similar empirical approach to Stewart (and Swaffield) and use many of the same data sets (although they include a slightly longer time frame). The difference-in-differences estimates using the British Household Panel Survey (BHPS) are positive but not statistically significant. In contrast, the estimates based on the NES show essentially no effect in the year in which the minimum wage was introduced, but negative effects two and three years later – results which more closely resemble those reported by Stewart and Swaffield. The source of the differences between the estimates from the two data sets is not readily apparent, as the second-year NES estimates should roughly correspond to the BHPS estimates. However, the authors note that the NES has the advantage of much larger sample size (60,000 women per year versus 3,000 in the BHPS).

dummy variable for firms more exposed to a higher minimum, year effects, firm fixed effects,[21] and year dummy variables interacted with the exposure dummy variable; these interactions trace out the evolution of changes in the dependent variables since the introduction of the minimum wage. For manufacturing, the results indicate that the minimum wage raised per person pay and reduced employment in 1999 and 2000, although none of the estimates are significant. For services (for which data are available only from 1998 to 2001), the evidence points to positive and significant effects on pay (in 2000 only) and small and insignificant negative effects on employment in 1999 and 2000. Thus, this analysis fails to find evidence of a significant disemployment effect.

Their second approach seeks to avoid the problems created by matching to a random sample of workers by using the full ABI file and imputing exposure to the minimum wage (specifically, the percentage of workers with a wage at or below the new minimum) based on the distribution of wages at the regional and sectoral level. They estimate models for changes in employment and other outcomes using a specification that includes this fraction affected variable interacted with year dummy variables, along with 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. For services, the estimates imply that a one percentage point increase in the share of workers affected reduces employment (total or full-time equivalent) growth from 1998 to 1999 by between 0.06 and 0.12 percentage point, which seems like a rather large effect. Because the fraction affected variable is based only on the 1998 data, and hence captures only the introduction of the new minimum wage in 1999, this implies a one-time relative reduction in the level of employment in 1999.

Finally, Galindo-Rueda and Pereira study the entry and exit of business establishments in low-wage sectors, using data from the Office of National Statistics (which covers all companies in the United Kingdom). For this analysis, they estimate models for the total number of establishments by sector and for total employment, and identify minimum wage effects from the variation in wage levels across regions.

[21] They also report results with industry fixed effects, but we focus on the former.

So, for example, if the minimum wage deters entry in a particular low-wage sector, the rate at which new establishments enter that sector should be relatively lower in the lower-wage regions, where the introduction of the minimum wage had a larger impact. Our interest is in the employment results, which indicate significant disemployment effects in four of the eight low-wage sectors they study, negative estimates in seven of the eight, and no significant evidence of positive effects in the eighth. Because these results come from information on all establishments rather than from employment changes at existing firms, and because auxiliary evidence suggests that minimum wages may deter entry in lower-wage areas, the authors interpret the combined evidence as suggesting that the introduction of the minimum wage had relatively little effect on already employed workers, but likely exerted more of its impact through its effect on job creation in low-wage sectors.

Aside from the United States, the effect of minimum wages on employment has probably 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 it seems to be widely cited as providing evidence that an increase in the minimum wage does not reduce employment. What conclusions do we take away from our review of the evidence for the United Kingdom? There is clearly a lot of variation in the estimated effects across studies, and, in general, the evidence of significant disemployment effects appears to be weaker for the United Kingdom than for the United States. However, we see two reasons to be cautious about concluding that minimum wages have not had adverse consequences for employment in the United Kingdom. First, the evidence based solely on the Wages Councils era, which tends to indicate zero or positive effects, would seem to be of only limited relevance to the current policy environment, both because of the potential endogeneity of minimum wages in that era and because of the focus of this research on narrow industries. Even then, the evidence from the abolition of the Wages Councils is more consistent with disemployment effects. Second, most of the existing research on the United Kingdom has been limited to estimating short-run effects, and, in our view, the question of the longer-run influences of the national minimum wage on U.K. employment has yet to be adequately addressed; indeed, the research tends to find negative effects on hours (of the employed) when lagged effects are allowed. Thus, we do not think one can yet state definitively that the evidence for the United Kingdom points unambiguously in one direction or the other, and we would regard it as incorrect to point to the evidence from the United Kingdom as making a strong case that the minimum wage does *not* reduce labor demand.

6.1.2.2 Australia

Although Australia's labor market is similar in many respects to labor markets in the United States, the United Kingdom, and Canada, it has a relatively complicated set of rules and institutions governing wage setting. Prior to 2005, federal minimum wages were set by the Australian Industrial Relations Commission (AIRC), consisting of employer, union, and government representatives who determined minimum wage rates based on economic conditions.[22] In particular, the minimum wage set in any given industry consisted of a national statutory minimum wage applicable to all industries and an additional amount that was dependent on worker productivity. In addition, state governments were permitted to, and often did, set their own minimum wages, although these typically did not apply to all workers. Leigh (2003) studies a series of statutory minimum wage increases enacted by the state government in Western Australia between 1994 and 2001. This minimum wage covered workers not covered by the federal minimum wage or other wage agreements or industrial commission awards. The level of the minimum wage in Western Australia was initially below the national minimum, but gradually caught up to it over this period. While the setting is complicated, Leigh argues that the minimum wage increases in Western Australia provide exogenous variation that can be used to assess the effects of the minimum wage on employment. Indeed, his discussion of the economic and political conditions surrounding these minimum wage increases is a nice example of the type of consideration that should be given to the potential endogeneity of such increases, absent a more complete solution to the endogeneity problem.

[22] In 2005, the wage-setting functions of the AIRC were replaced by the Australian Fair Pay Commission, which sets a single federal minimum wage for all adult workers.

Leigh reports a variety of estimates. He first constructs short (seven-month) first differences for total employment for the periods surrounding each of six minimum wage increases in Western Australia and compares these changes to similarly constructed employment changes outside of Western Australia. The difference-in-differences estimates are mostly negative, with elasticities ranging from 0.01 to −0.81; in addition, the four largest estimates (ranging from −0.38 to −0.81) are statistically significant.[23] Leigh also estimates a pooled model that combines these observations and finds an overall employment elasticity of −0.29, which is again statistically significant. In addition, he estimates models that disaggregate by age and sex, to try to isolate those more likely to be bound by the minimum wage. When he disaggregates by age, the estimated elasticity is −1.01 for 15–24 year-olds and ranges from −0.03 to −0.14 for the other age groups; however, the estimated effect is significant only for 15–24 year-olds. In addition, the estimated elasticity is larger for young women (−1.43) than for young men (−0.68), although both are statistically significant. The elasticities that Leigh reports for aggregate employment of young individuals are quite large relative to those found for other industrialized countries, especially given his estimate that only about 4 percent of workers were affected by these changes in the minimum wage. Unfortunately, he does not offer a potential explanation for the size of his estimates, and in the absence of such an explanation, the magnitudes of these estimates might be regarded skeptically.

A report by Harding and Harding (2004) estimates the effects of state minimum wage increases in 2003, based on surveys of employers with minimum wage workers who report their change in employment in a relatively short period following the minimum wage increases, as well as the counterfactual regarding how much employment would have changed absent the increases. They arrive at a short-run elasticity for minimum wage workers of −0.2. However, it is unclear whether we

[23] There have been a series of papers fixing problems in the original paper (Leigh, 2004a), criticizing his work (Watson, 2004), and responding to that criticism (Leigh, 2004b). The paper by Watson speculates about potential problems with the study, but does not present any evidence, and Leigh's reply suggests that Watson's criticisms have little merit. Here, we present the corrected estimates in Leigh (2004a).

should view employers' responses regarding the employment change due to minimum wage increases as reliable.[24]

6.1.2.3 New Zealand

New Zealand, like Australia, has a relatively complicated history of minimum wages interacting with industry specific mandated wage floors ("awards"). Prior to 1991, workers in many industries were covered by Arbitration Court awards, which fixed a minimum wage for the industry that applied to all workers, whether unionized or not. In 1983, a national minimum wage was enacted for workers aged 20 and over. Initially, given the industry awards, the minimum may not have been relevant for many workers. However, Chapple (1997) cites evidence suggesting that by 1985 the minimum wage was high enough to be binding in a number of industries. In 1991, the industry awards system was eliminated, leaving New Zealand with only the national minimum wage. In March 1994, a subminimum wage for teenagers was introduced, initially at 60 percent of the adult minimum. Finally, in March 2001, the minimum wage for 18–19 year-olds was raised to the adult minimum in one step, and the minimum wage for 16–17 year-olds was raised to 80 percent of the adult minimum wage in two steps.

[24] Two other papers for Australia report evidence on minimum wage effects, although both have rather serious limitations and thus warrant only a brief mention. Mangan and Johnston (1999) present panel data evidence across states as well as evidence from individual-level census data, on the effects of minimum wages on 15–19 year-olds. The first analysis leads to negative but insignificant estimates of employment effects of minimum wages, while the second analysis indicates that the minimum wage reduces the probability of employment, with estimates that are generally significant. However, this study is quite sketchy on the details, with little or no information provided on the source of the minimum wage variation, such as the relationship between minimum wage and industrial commission awards, the source of cross-state variation, and the teen minimum ("junior award wage") relative to the minimum wage for other workers. In addition, a study by Junankar et al. (2000) estimates separate time-series regressions for the retail and the manufacturing sector on quarterly data for a ten-year period covering 1987–1997, using teen minimum wages relative to average weekly earnings for the industry. The specifications should include minimum wages for non-teenagers as well, since it is the spread between the various minimum wages that may drive teen employment (the same comment applies to the first paper). The authors report negative effects of minimum wages on total hours worked by some groups of teenagers in retail, but little other evidence of either positive or negative employment effects. However, the elasticities they report are sometimes extraordinarily large, exceeding 5 or even 10 in absolute value for the retail sector.

Maloney (1995) reports estimates from relatively standard time-series specifications for teens and young adults. He uses time-series data from the quarterly Household Labor Force Survey, a CPS-style data set, for the period 1985 through 1993. Because the minimum wage in effect in this period only covered workers aged 20 and over, it provides an opportunity to test whether the minimum wage induced employers to substitute away from young adults and toward teenagers. Indeed, Maloney finds evidence of a significant negative effect of the minimum wage on employment of young adults, with an elasticity of −0.35, and a significant positive effect on teens, with an elasticity of 0.69. Weighted by the employment rates of the two groups, these elasticities imply that the employment declines among young adults were closely balanced by the employment gains among teens, suggesting that workers in these age groups are close substitutes. Furthermore, when Maloney restricts the sample to less-educated young adults, he finds a much larger disemployment elasticity (−0.57) than for young adults as a whole, suggesting that the adverse employment effect of the minimum wage falls mainly on them.

Although these results are quite consistent with the predictions from the standard competitive model, results in a subsequent paper (Maloney, 1997) are somewhat less so. In this paper, Maloney extends the data set to 1996, enabling him to examine the effects of the introduction of the subminimum wage for teenagers in March 1994, which he does by adding to the teen equation a dummy variable for the introduction of the teenage minimum wage. His specifications yield similar evidence of negative minimum wage effects for young adults, but somewhat weaker evidence of a positive effect of the adult minimum wage on teen employment. From a variety of specifications, he reports estimated elasticities for young adults ranging from −0.1 to −0.4, with the smaller (absolute value) estimates not significant. For teens, the estimated effects of the adult minimum range from −0.1 to 0.4, and are never significant.

Finally, the estimated coefficient on the introduction of the teen minimum in the equation for teen employment is near zero and insignificant, in contrast to the negative effect we might expect. Maloney speculates that this may be because the teen minimum was non-binding, although he does not present any evidence to support this claim. He also suggests that the teen minimum may not have been in effect long enough to see any effect. Given that his data extend nine quarters beyond its introduction, this argument does not seem very compelling, although evidence differentiating shorter- and longer-run effects of the teen minimum might have made this case more strongly. Finally, given the evidence from Maloney's first paper, one might have expected the introduction of the teen minimum to have had a positive effect on young adult employment, given the compression in the wage differential between young adults and teenagers. But Maloney omits this variable from his young adult employment regression, and given that he uses a systems estimator, the bias from this omission would also affect estimates of the teen employment regression. With a positive correlation between the errors of the two equations, the bias in the estimated coefficient of the teen minimum wage variable in the teen employment equation is positive, which could potentially explain the absence of a negative estimate for this coefficient.[25] It is also not clear why Maloney does not use the value of the teen minimum wage, paralleling the treatment of the adult minimum wage, rather than just a dummy variable for its introduction.

Chapple (1997) revisits Maloney's time-series analysis, using somewhat different specifications. For some reason he focuses only on young adults, ignoring both the question of how the adult minimum affected teen employment before the teen minimum was introduced, and how the introduction of the teen minimum itself affected teen and young adult employment. He generally confirms Maloney's results for young adults, obtaining time-series estimates of elasticities ranging from −0.17 to −0.34, again with the smaller estimates insignificant. He offers some reasons why the specifications with the smaller estimates should be preferred, although we do not find these particularly compelling. Regardless, the time-series estimates are all negative, and the

[25] When the teen minimum is implemented, the residual in the young adult equation is too high. Given the positive error correlation, this is transmitted to the residual in the teen equation, which is therefore positively correlated with the dummy variable for the teen minimum wage.

lower estimates – even if insignificant for the relatively short time-series available – are in the consensus range from the earlier U.S. literature.

Chapple then estimates panel data models across industries for the period 1980–1997, using the national minimum wage divided by industry price deflators. Because the minimum wage varies nominally only at the national level, he chooses to omit the year fixed effects and instead includes aggregate controls (for the inflation rate, GDP, and the exchange rate). We have already raised concerns about the reliability of specifications that omit year effects and identify the effects of minimum wages from aggregate minimum wage changes. In addition, in the context of research on Puerto Rico, discussed below, Krueger (1995) points out that cross-industry estimates are reliant on the assumption that the elasticity of labor demand is the same across industries. Otherwise, it is possible that minimum-wage-induced employment losses in a low-wage industry with less elastic labor demand could be of similar magnitude to employment declines in a higher-wage industry with more elastic labor demand, leading to the erroneous conclusion that there is no effect of the minimum wage on employment.

Regardless of these problems, the estimated employment effects are negative and significant, with elasticities of -0.06 to -0.10, which seem rather large given that these are for total industry employment rather than for just unskilled employment. The estimates become a bit smaller and insignificant, although still negative, in specifications that include the (log) minimum wage and price index separately, rather than in ratio form. Moreover, the estimated coefficients of the minimum wage and the price deflator are both negative, rather than of equal and opposite sign as we might expect if the minimum wage and an average wage were entered into the equation. In addition, the estimates are not robust. For example, for the latter half of the sample, the elasticity of industry employment with respect to the minimum wage is -0.48 in one specification and 0.66 in another. Finally, Chapple also estimates separate time-series models for each industry. In these specifications, the elasticities are roughly centered on zero; but with only 10 degrees of freedom per industry, we do not regard these as meaningful.

All told, Chapple's time-series evidence parallels Maloney's, while his industry-level estimates are often negative, but not robust. Given the potential problems with the industry analysis, we view this set of papers as pointing more in the direction of negative employment effects of minimum wages in New Zealand.

A more recent paper by Hyslop and Stillman (2007) challenges this conclusion. These authors study the effects of large increases in teen minimum wages beginning in 2001 – a 69 percent increase in the minimum wage for 18–19 year-olds in 2001 and 19 percent increases for 16–17 year-olds in both 2001 and 2002 (the adult minimum wage increased by 2 and 4 percent, respectively, in these two years). They also use the Household Labor Force survey, with data covering the 1997–2003 period, and conclude that the minimum wage had essentially no effect on teen employment immediately following the increase, but perhaps a small negative effect by 2003.

They begin their analysis by computing, for 16–17, 18–19, and 20–25 year-olds, the change in the average level of employment from the period 1998:Q2-1999:Q3 to the period 2002:Q2-2003:Q3. The simple difference-in-differences estimate from these averages indicates that employment of the two younger groups rose slightly relative to employment of 20–25 year-olds, in contrast to what one would expect if the minimum wage reduced employment.[26] However, when they include additional controls, the estimated effects are negative, although still insignificant. In addition, if disemployment effects occur with a lag, a comparison based on data further beyond the implementation date might be more informative.

The authors do provide a figure (Figure 4) that provides quarter-by-quarter employment rates for each age group. They describe this figure as providing little "to suggest that employment . . . by teenage workers was affected by the minimum wage increases" (p. 215). However, this figure shows that in the first quarter of 2001, when the minimum wage jumped sharply for 18–19 year-olds, employment for this age group fell while employment of the other two groups rose. Similarly, in the

[26] They also report results for hours conditional on working, as well as for other outcomes less related to employment.

first quarter of 2002, when the minimum wage jumped for 16–17 year-olds, their employment fell, employment of 18–19 year-olds rose, and employment of 20–25 year-olds was little changed. The dynamics may tell a different story, but the short-term differences are suggestive of some disemployment effect.

Finally, Hyslop and Stillman present regression estimates in which they allow separate employment changes for each of the three age groups in 2001, 2002, and 2003. Although the estimated coefficients for 16–17 and 18–19 year-olds tend to be positive in 2001 and 2002, the estimated changes for 20–21 year-olds are almost always larger, and sometimes considerably so. For example, in 2001, the regression estimates indicate that employment rose (net of the controls) by 0.042 for 20–21 year-olds, 0.007 for 18–19 year-olds, and 0.022 for 16–17 year-olds. That is, employment of 18–19 year-olds – for whom the minimum wage increase was by far the largest – fell relative to both groups, and employment of 16–17 year-olds, who experienced the second-largest minimum wage increase, fell relative to 20–21 year-olds. Thus, if overall employment of young workers (aged 16–21) rose for reasons unrelated to the minimum wage (the type of influence that researchers can control for in a triple-differenced estimate), these results would be quite consistent with the predictions of the standard competitive model. Of course, part of the relative increase in employment for 20–21 year-olds may have reflected substitution by employers toward them, but that effect is also predicted by the competitive model. In addition, employment fell (net of controls) for all three age groups in 2003, and the decline was largest for 16-17 year-olds for whom the minimum wage increased the most in 2002. Thus, in the end, we view the evidence for New Zealand as more consistent with negative employment effects of minimum wages.

6.1.2.4 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. The level of the minimum wage tends to be high relative to the average wage in France and is often blamed for the high level of youth unemployment in that country. Dolado et al. (1996) present evidence from two analyses. First, they compare wage and employment changes over two periods – 1981–1985 (when the minimum wage rose sharply) and 1985–1989 (when it did not). In particular, they estimate the changes over these periods in hourly wages and in employment and unemployment rates for 48 age-sex-education groups, and then regress these changes (by sex) on the proportion of each group at or below the minimum wage. Contrary to expectations, they find little evidence that the minimum wage increases in 1981–1985 raised the wages of low-wage workers or that wage increases for low-wage workers were held down by the absence of an increase in the relative minimum wage in the second half of the 1980s. Perhaps surprisingly given the wage results, they find that the unemployment rate rose more for groups with a greater proportion of workers paid at or below the minimum wage in the early period (when the relative minimum wage was rising) and fell more for these groups in the later period (when the relative minimum wage did not change); the patterns of change in employment rates are weaker, but in the consistent direction. As the authors note, however, the two periods are less comparable than one would like, in that the first – when the minimum wage rose more quickly – included a recession, while the second – when the relative minimum wage was about unchanged – was characterized by an economic recovery. This lack of comparability makes it difficult to distinguish minimum wage effects from the differential impact of the business cycle on the employment and unemployment rates of different skill groups.

The authors then take a different approach and focus on regional differences in employment change from 1967 to 1992.[27] Their rationale is that in the earlier part of this period the national minimum wage rose sharply, but because initial wage levels were very different across regions, the impact of the national minimum wage increase should have been greater in the lower-wage areas. The authors find a negative relationship between regional wage growth and the level of the

[27] This analysis is also reported in Machin and Manning (1997), who are co-authors of the Dolado et al. paper.

initial wage during the period in which the relative minimum wage was rising, consistent with the rise in the minimum boosting wages more in low-wage regions. However, a regression of the change in employment on the initial wage also yields a negative estimate, in contrast to what would be expected if the minimum wage reduced employment more in low-wage areas. Although the authors seem to find this evidence more compelling, there are problems with this analysis. First, they do not control for differences in economic conditions across regions. If wages in the initially low-wage regions were held down at the beginning of the sample by cyclically weak labor market conditions, these areas would also have tended to exhibit initially low employment rates. In this case, a regression of the change in employment on the initial wage could yield a negative coefficient simply because labor demand returned to normal in subsequent years. Second, this part of the analysis does not focus on low-skill individuals along any particular dimension, and as for the United States, the effects of the minimum wage may be difficult to detect from data on aggregate employment.

Bazen and Skourias (1997) study the effects of the 1981–1984 increases in the French minimum wage on youth employment (for 15–24 year-olds). They split their sample into 32 sectors and calculate the ratio of youth employment to total employment in each sector and the proportion of workers in the sector whose wage was below the level of the minimum wage set in June 1981. Using a difference-in-differences approach, they find that, conditional on overall employment growth in the sector, the share of youth employment fell in relative terms in the sectors for which the minimum wage increase was more binding. The estimates are negative and statistically significant for the first-differences estimates (for October 1980 to October 1981 and March 1981 to March 1982) and become somewhat larger when a longer difference is used.

We have two concerns about their specification. First, the authors estimate an equation for the change in the youth employment share, using as a control the percentage change in total sector employment. Since the percentage change is sensitive to the base, and employment levels can differ sharply across sectors, it would have been preferable to define the dependent and independent variables consistently. Second, their longer-differenced specifications regress changes in youth employment from March 1980 to March 1984 on the proportion of workers directly affected by the June 1981 increase in the minimum wage, whereas it would seem more natural to use a measure of workers affected by the entire set of minimum wage increases from 1980 to 1984 (17 percent of which occurred, in relative terms, prior to June 1981). Otherwise, sectors that adjusted employment more slowly to the increases in 1980 and early 1981 would have had more minimum wage workers in June 1981, and possibly sharper employment declines subsequently, even if their employment declines over this period were no larger than in those sectors with fewer minimum wage workers prior to entire set of increases.

The final set of studies we consider are those by Abowd et al. (1999, 2000a), whose results for the United States were summarized in the previous section. As we described earlier, these authors use individual-level panel data and test for disemployment effects among initially employed workers who are "caught" by an increase in the minimum wage. For France, which generally had a rising nominal minimum wage over the period they study (1982–1989), the authors consistently find considerably higher transitions to non-employment for workers newly bound by the minimum wage than for workers with marginally higher wages, especially among those just above age 24 who were not protected by employment promotion contracts.[28] For example, for men aged 25–29 caught by the minimum, 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 not significant, but the elasticity is still large (-1.38). For those aged 20–24, the elasticities are smaller and not significant, and the elasticities are smaller still, and insignificant, for males and females aged 16–19. The elasticities for those above 25 are large, but as the authors point out, these are elasticities that apply to a very small share of the population in the age group. Thus, these results reinforce our previ-

[28] Abowd et al. (2000b) extend the analysis for France to the 1990s, studying the effects of the effective minimum wage based on both minimum wage levels and payroll taxes and subsidies. They reach qualitatively similar conclusions, although they provide no detail on how the effects vary by age.

ous argument that the effects of minimum wages are quite different when one focuses on directly affected workers rather than on a broader group.

All told, the evidence for France regarding overall employment effects of minimum wages on young and less-skilled workers is mixed. Our paper on European and other OECD countries, discussed above, suggests that France may have a combination of labor market institutions that make it less likely that minimum wages will have detectable disemployment effects on young workers, and the results in the Abowd et al. papers, on those under age 25, appear to confirm this.[29] At the same time, 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, and the evidence from France, on balance, is consistent with disemployment effects.

6.1.2.5 The Netherlands

The minimum wage is also set by statute in the Netherlands. However, this country represents an interesting case because the government instituted reductions in the youth subminimum wage in 1981 and 1983 (as well as a reduction in the nominal adult minimum wage in 1984), which, for example, lowered the subminimum wage for 20 year-olds from 77.5 percent to 61.5 percent of the adult minimum wage and the subminimum wage for 16 year-olds from 47.5 percent to 34.5 percent of the adult minimum wage. Dolado et al. (1996) study the effects of these declines in youth subminimums.[30] The authors first verify that relative wages of young workers declined following these changes, which indicates that the youth subminimum wages were a

[29] In a related vein, Kramarz and Philippon (2001) study the effects of the combination of minimum wages and other labor costs, with a focus on payroll taxes and exemptions. Similarly, Laroque and Salanié (2002) present estimates of a relatively simple structural model intended to capture the effects of the minimum wage and the welfare system on the employment of married women in France, based in part on the Meyer and Wise (1983) approach to estimating the employment effects of minimum wages (discussed below).

[30] One problem they note, which also applies to Spain and Brazil (discussed below), is that in the Netherlands the minimum wage also influences benefit levels – in this case unemployment benefits – making it more difficult to separately identify the effects of legislated minimum wage changes.

binding constraint on the wages of these workers. They then calculate that the ratio of youth employment to total employment fell by 3 percentage points from 1979 to 1985, contrary to what would be expected from an exogenous reduction in wages for younger workers. However, they also recognize that a severe recession in the Netherlands at that time may have disproportionately affected youth employment, making it difficult to distinguish the effects of the minimum wage reduction from cyclical influences. As a result, the authors examine changes in the shares of youth employment in four low-skilled occupations relative to changes in the shares of youth employment in somewhat higher-skilled occupations. Among the low-skilled occupations, the youth share of total employment fell by 2 percentage points in one, remained constant in one, and rose by 2 and 4 percentage points in the other two. They summarize by suggesting that the evidence from the Netherlands about adverse employment effects of the minimum wage is "scarcely compelling" (p. 347). However, in all four industries the youth share in employment rose both relative to the overall youth share of employment (which fell by 3 percentage points over the period) and relative to the youth share of employment in the higher-skilled occupations, consistent with the response we would expect if minimum wages reduce employment.[31]

6.1.2.6 Spain

The final country for which Dolado et al. (1996) present evidence is Spain. The national minimum wage in Spain is also set by statute, but is determined 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 has gradu-

[31] There are also some papers that estimate the effects of the minimum wage on employment in the Netherlands using either structural search models (Koning et al. (1995)) or the Meyer and Wise (1983) approach of inferring minimum wage effects from "missing" workers in the wage distribution (van Soest, 1994). The Meyer-Wise approach has seldom been used in recent research on minimum wages and hence is not covered in this review. One exception is a paper by Dickens et al. (1998), who present evidence using this method for the United Kingdom and find that the employment estimates are quite sensitive to assumptions about the distribution of wages and the wage above which the minimum has no effect.

ally declined over time, limiting the extent to which the time variation can be used to identify the effects of minimum wages on employment. In an attempt to increase the exploitable variation in the data, the authors use a panel-data approach to investigate the effects of minimum wage changes on six low-wage sectors. In particular, they regress sector-specific changes in employment and teen employment (16–19 year-olds) on changes in the relative minimum wage, cyclical controls, and fixed sector effects.[32] The results show a positive and statistically significant elasticity of 0.08 for total employment, but a negative elasticity of −0.15 for teens (significant at the 10-percent level). They also find stronger disemployment effects for 16–19 year-olds in the set of industries for which the minimum wage is most binding.

Dolado et al. also report on what is likely a cleaner experiment – an 83 percent increase in 1990 in the minimum wage for 16 year-olds; there was also a more modest increase of 15 percent for 17 year-olds. In this case, they use regional data and 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 16 to 19 year-old workers after the increase in the minimum wage for 16–17 year-olds. This effect is not evident in the data for 20–24 year-olds (indeed the coefficient is positive), which suggests that the results 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 reduced teen employment.[33]

[32] The authors exclude fixed year effects from their panel data specifications, which hampers comparability with other studies that typically include such effects to sweep out the effects of aggregate changes. They report that they could not reject the exclusion of these year effects, but with small samples (168 in the pooled analysis, and 28 in the analysis of each industry) the statistical power of these tests may be weak. Unfortunately, it is not possible to determine the effect on the results of omitting fixed year effects from the information provided in the paper.

[33] At the same time, they interpret the evidence of positive effects on aggregate employment as consistent with a "monopsony effect" (p. 352). However, we question whether the apparent positive effect of minimum wages on overall employment should be taken seriously. Few economists would expect any detectable effect of minimum wages on aggregate employment in modern industrialized economies. And even if we were inclined to the monopsony view, we would expect such effects to show up most strongly for workers

6.1.2.7 Portugal

A similar quasi-experiment took place in Portugal in January 1987. In this case, the government eliminated the 75 percent subminimum wage for 18–19 year-old workers, 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 20 and over. Pereira (2003) uses 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, with observations pertaining to March of each year.[34] From this data set, Pereira extracts firm-specific information on employment, hours, and wages for three age groups: 18–19 year-olds, 20–25 year-olds, and 30–35 year-olds. She then uses two related research designs to test the effects of minimum wages on employment. First, she compares changes in employment and hours (and wages) across the three age groups for intervals of one, two, and three years after the minimum wage increase. Second, she 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, and she also presents analyses that account for firm entry and exit, which is substantial.[35]

Pereira seems able to rule out any anticipatory effects, arguing that the news of the impending change in the minimum wage first surfaced in August 1986, well after the March 1986 measurement of initial employ-

directly affected by the minimum wage. In contrast, the evidence for Spain points to negative employment effects for the most affected workers.

[34] The subminimum wage was also increased for 17 year-olds, from 50 percent to 75 percent of the adult minimum wage. However, Pereira focuses only on 18–19 year-olds because there are few workers in the younger age group and few firms employing them in her data. One year later, the subminimum for workers aged 16 and under was increased as well (Portugal and Cardoso, 2006).

[35] Note that Krueger's question about the role of variation in labor demand elasticities applies here as well.

ment levels (and wages). In addition, she finds that the minimum wage increase pushed up wages of teenagers in both absolute and relative terms, and that the spike at the subminimum prior to the increase disappeared soon after the minimum wage increase for teenagers took effect.

The evidence indicates that teen employment fell relative to employment for 30–35 year-olds, with the difference statistically significant and implied elasticities from her preferred estimates ranging from −0.2 to −0.4. In contrast, employment of 20–25 year-olds increased relative to the older group, consistent with substitution away from teenagers and toward this group. This substitution is exactly what would be expected if the minimum wage increased the price of teen workers relative to their close substitutes. In addition, the estimates imply that overall youth employment (ages 18–25) declined slightly. The evidence is particularly strong for the specifications that identify the minimum wage effects from the most-affected firms. The evidence also indicates that the effects are stronger one or two years after the minimum wage increase than in the first year of the increase, consistent with other evidence on lagged effects. Pereira also estimates similar models for total hours and finds even larger effects, suggesting that employers reduced the average workweeks of their teenage employees as well.

Portugal and Cardoso (2006) 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). They focus mainly on the effects of minimum wages on worker flows – that is, accessions and separations. We do not delve into these results because this review is focused primarily on net employment effects. However, we note that their results suggest that the increase in the minimum wage reduced both job separations and new hires among teenagers (in all cases relative to workers aged 20 to 35 years old). The reduction in teen hiring is evident for new firms as well as for existing firms, while the separation results vary – with the higher minimum wage increasing the share of teenage job loss due to firm closures.[36]

[36] In addition to providing information on the effects of minimum wages on the dynamics of employment change, the authors assert that their results are at odds with the results we

The authors take issue with the net negative employment effects for teenagers estimated by Pereira (2003). They 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" (p. 995) – and, they could have added based on the comparison they do, for all groups. However, Pereira reports employment levels only for firms that survive, and given the large turnover of firms in these data, her employment totals naturally shrink; thus, this comparison is invalid. Pereira also shows that her regression results are qualitatively similar whether or not she excludes entering and exiting firms. Given that Portugal and Cardoso do not present any kind of overall analysis of net employment effects (they only report overall levels, with no regression controls), we do not find their criticism of Pereira's estimates convincing.[37]

have reported for the United States, which suggests that higher minimum wages cause teenagers who have already dropped out and are working to lose their jobs, and teens in school (perhaps already working part-time) to leave school for full-time employment (for example Neumark and Wascher, 1996a). Portugal and Cardoso argue that these transitions are unlikely to be occurring in Portugal because this type of response would suggest increased separations and accessions of teenagers rather than reduced separations and accessions. Of course, the importance of secondary schooling to the employment behavior of teens in Portugal could be quite different from that in the United States and so it is difficult to evaluate the relevance of how these transitions differ between the two countries.

[37] Portugal and Cardoso present no other evidence – such as erroneous computer code – that undermines Pereira's claim that she drew a 30-percent random sample that should be unbiased. They do note that even when they tried to replicate Pereira's sample, they found opposite results from hers (footnote 9). However, it appears that this exercise attempted to replicate the employment trends reported by Pereira rather than her regression estimates. Portugal and Cardoso also present employment figures from another household-level data set, but they report only that employment of teenagers grew (and unemployment fell) from 1986 to 1989 rather than anything about the difference in employment growth between teenagers and older workers. Given that this period was characterized by an economic expansion, these calculations are not especially informative.

6.1.2.8 Greece

The only evidence we have uncovered for Greece comes from a time-series study that uses annual data from 1974 to 2001 (Karageorgiou, 2004). This paper focuses on teens (ages 15–19) and young adults (ages 20–24). Greece had different minimum wages for these groups, but although their levels were different, their relative movements were quite similar over much of the period, with the exception of an increase in the teen minimum relative to the young adult minimum in the early 1980s.

Estimating separate models for these two age groups, and paying attention to the specification issues raised in Williams and Mills (2001), Karageorgiou finds evidence of negative employment effects of the minimum wage for young adults, with elasticities (from the log specification) ranging from −0.05 to −0.12, although never significant. For teens, in contrast, the estimates are large and positive, with elasticities ranging from 0.22 to 0.63, with the larger estimates significant.

There are a few reasons to be skeptical of the results. First, minimum wages in Greece vary by age, education, tenure, occupation, and marital status. As a result, it is not clear that we necessarily want to focus solely on young or unskilled workers – in contrast to the more common case where there is a single wage floor that is presumably most binding on the least skilled. Second, there is relatively little real variation in minimum wages, especially after the late 1990s, because the minimum wage was indexed to inflation. Third, the estimated effect of the aggregate unemployment rate on young adult employment is positive and significant, which is suggestive of model misspecification. Fourth, the author does not adequately explain the extraordinarily large positive minimum wage effects for teenagers. Although he suggests that part of the explanation may be that teens and young adults are very close substitutes, so that a higher minimum wage causes strong substitution toward teenagers, this would seem to require large negative effects for young adults as well. Moreover, testing this hypothesis requires estimates of how teen (and young adult) employment responds to the minimum wages for both groups. However, the employment equation for each group contains only that group's minimum wage relative to an adult average; in addition, as noted above, there seems to be little variation in the difference between the minimum wage for these two groups, and so it may not be possible to test this conjecture. These problems, coupled with the difficulty of interpreting the evidence in the case where many other workers are also bound by minimum wages, leads us to regard the evidence from Greece as inconclusive one way or the other.

6.2 Developing Countries

The analysis of minimum wage effects in developing countries is complicated by a number of factors. First, there is often 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, there are serious concerns about the enforcement of and compliance with minimum wage laws. Third, for some countries confounding factors such as anti-sweatshop campaigns have also created upward pressure on wages of low-skilled workers. And fourth, in some cases minimum wage increases have occurred in the context of high inflation, in which case legislated minimum wage increases may convey little more than extraordinarily short-term changes in relative prices of different kinds of labor. Because of these complications, the results from developing countries are more difficult to interpret and are less likely to be applicable to other countries. In addition, after reviewing the literature, it seemed to us that many of the complexities involved in thinking about the evidence for each country were better left to scholars of those particular countries' economies.[38] We therefore provide a much briefer overview of this evidence, more as a guide to the literature than as a comprehensive review (Table 6.2).

[38] A significant portion of this literature is also written in languages other than English.

Table 6.2 International studies of minimum wage employment effects, developing countries

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Brazil					
Fajnzylber (2001)	Largely time-series variation	All, but effects differ based on initial wage	Brazilian Monthly Employment Survey (BMES), 1982–1997	Backed out from estimates of effects of minimum wages on income throughout the wage distribution: Formal sector, below and near minimum wage: −0.05 to −0.08 Informal sector, below and near minimum wage: −0.05 to −0.15	Stronger results for informal sector unexpected
Lemos (2004, 2006, forthcoming)	Largely time-series variation, with different impact across regions based on different wage levels	All ages, many other comparisons (public versus private, less- versus more-educated, formal versus informal sector)	BMES, 1982–2000	Aggregate employment: centered on zero for hours and employment Employment, formal sector: 0 Hours, formal sector: −0.02 Employment, informal sector: −0.02 Hours, informal sector: 0.02 Employment, private sector: 0 Hours, private sector: 0.01 Employment, public sector: 0.03 Hours, public sector: −0.07 More evidence of negative effects (still insignificant) in low-inflation periods, and for less-educated	Some differences in estimates difficult to explain or reconcile
Lemos (2005)	Largely time-series variation, with different impact across regions based on different wage levels	All ages	BMES, 1982–2000	Instruments for minimum wage variables with political variables: OLS: −0.12 to 0.02 (most negative) IV: large number of estimates, ranging from −0.29 to 0.12 (most negative)	Political variables may influence other labor market policies as well

Table 6.2 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Neumark et al. (2006)	Largely time-series variation, with different impact across regions based on different wage levels	All ages	BMES, 1996–2001	Household heads: −0.07 (significant); positive for other family members	
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 6.2 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
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)**	
Colombia					
Maloney and Nuñez Mendez (2004)	Two federal minimum wage increases during 1997–1999	Men working 30–50 hours per week	National Household Survey, 1997–1999	Uses self-employed as control group: Employment elasticity: −0.15; stronger effects near minimum wage but effects also present higher in wage distribution	Select group of workers
Chile					
Montenegro and Pagés (2004)	Time-series variation in real minimum wage, and variation in teen relative to adult minimum wage	All, with effects differentiated by age, sex, and skill	Household survey for Santiago, Chile, 1960–1998	Minimum wages reduce relative employment of young, unskilled workers, but increase relative employment of women	Tenuous evidence on overall employment effects
Costa Rica					
Gindling and Terrell (2004)	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**	

Table 6.2 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Trinidad and Tobago					
Strobl and Walsh (2003)	Implementation of national minimum wage in 1998	Females and males working in small (fewer than 10 employees) and large firms	Continuous Sample Survey of Population (CSSP), 1996–1998	Difference-in-differences in job loss between those bound and those not bound by new minimum wage: Males bound by new minimum more likely to lose job by 9 percentage points; females by 2.3 percentage points (insignificant), although more in large firms	Short time horizon after minimum wage increase and potential difficulties controlling for aggregate trends for comparable workers
Puerto Rico					
Castillo-Freeman and Freeman (1992)	U.S. federal minimum wage as applied to Puerto Rico, as well as cross-industry variation	Puerto Rican manufacturing workers working over 20 hours per week	Puerto Rican Census, and the Puerto Rican Survey of Manufacturing, supplemented by data from the Departmento del Trabajo and Recursos Humanos, U.S. Department of Labor, and U.S. Department of Commerce	Time-series: −0.11 to −0.15 Panel: −0.54 for full sample period; 0.20 before 1974; −0.91 after 1974 (when U.S. law generated increases)	Krueger (1995) shows that results are fragile

Table 6.2 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Indonesia					
Rama (2001)	Cross-province variation in minimum wage changes over the early 1990s	Urban workers	Indonesia's 1993 Labor Force Survey; data for years 1988–1994 from multiple sources: national accounts, labor force survey, wage survey, survey of large manufacturing establishments and survey of small scale manufacturing industries	Elasticity for aggregate urban employment: using the log of the minimum wage, −0.04; using the minimum over labor productivity measures, −0.04 to 0.00 For ages 15–24: using log of the minimum wage, 0.02; using the minimum over labor productivity measures, −0.25 to 0.09 (all insignificant) Large firms: log of minimum, 0.20; minimum over productivity measures, 0.02 to 0.13 (all insignificant) Small firms: log of the minimum, −1.30 (significant); minimum over labor productivity measures, −0.77 to −0.82 (insignificant)	Strength of identifying information is unclear, given apparent lack of enforcement of provincial minimum wages as of 1989
Suryahadi et al. (2003)	Cross-province variation in minimum wage changes over the early 1990s	Urban workers	National Labour Force Surveys and Intercensal Population Survey, 1988–2000	Elasticity for aggregate urban employment: −0.06 (significant); males, −0.05; females, −0.16 (significant); adults, −0.04; youths, −0.12; educated, −0.03; less-educated, −0.09 (significant); white-collar, 1.00; blue-collar, −0.07; full-time, −0.06 (significant); part-time, −0.11	

Table 6.2 (*Continued*)

Study	Minimum wage variation	Group	Data	Estimated elasticities (or other effects), comments on methods	Criticisms
Harrison and Scorse (2005)	District level differences in the minimum wage, within the same province	Manufacturing firms overall and sub-group 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% (16%) 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: Results from studies we regard as more reliable tests of employment effects of minimum wages are highlighted.

6.2.1 Latin America and the Caribbean

6.2.1.1 Brazil

The effects of minimum wages in Brazil have attracted interest for a number of reasons, including this country's long reliance on the minimum wage, the extreme inequality in the country, the role of the minimum wage in coordinating centralized wage bargaining as part of anti-inflationary efforts in the 1980s and early 1990s, and recent efforts to increase the minimum wage substantially. Although minimum wages were originally set at the regional level, the government switched to a national minimum wage in 1984. Since then, there have been periods in which the nominal minimum wage has increased sharply, periods in which it was frozen, and periods in which it has been indexed to inflation. In general, however, the real value of the minimum wage has tended to decline over time. In addition, although all workers are legally covered by the minimum wage in Brazil, there is a sizable informal sector for which compliance is relatively low.

Fajnzylber (2001) takes an indirect approach to estimating the employment effects of the minimum wage in Brazil. Using matched data from the Brazilian Monthly Employment Survey – a CPS-type data set – for the period 1982–1997, he applies the same methods as in Neumark et al. (2004) to estimate minimum wage effects at different points of the wage distribution. However, he only estimates the effects of the minimum wage on income, and then backs out the employment effects by comparing the estimated effects on income for a sample working in both years and a sample that also includes those not working in the second year they are surveyed. Given that he still has to condition on initial employment to use this strategy, it is not clear why he did not estimate employment effects directly. Nonetheless, his indirect approach implies that for formal sector workers earning below or very near the minimum, the employment elasticity is around -0.10 in the first year, and -0.05 to -0.08 after allowing for lagged effects, with the moderation attributable, he argues, to some giveback in the wage effects. For the informal sector, the implied employment elasticities for those below or near the minimum range from -0.25 to -0.35 in the short-run and from -0.05 to -0.15 in the longer-run. He suggests that the stronger effects for the informal sector may reflect individuals leaving informal sector employment to queue for formal sector jobs, although there is some question as to whether this is a real phenomenon that can account for the differences. In addition, the implied disemployment effects are stronger for women and teenagers.[39]

Lemos has written a sequence of closely related papers that use the same data set as Fajnzylber, but with observations that extend through 2000. Lemos (2004) considers a variety of measures of the minimum wage and estimates the effects of the minimum wage on employment and hours from a cross-section time-series panel of regional data over the 1982–2000 period. She uses a standard panel specification that includes region and time fixed effects, as well as lagged employment terms to capture shorter-run and longer-run effects in some specifications. Because the nominal minimum wage is uniform across the country, the regional variation comes either from differences in regional price indexes (when the real minimum wage is used) or from differences across regions in the share of workers at or below the minimum. Nevertheless, wage and price levels vary substantially across regions, and so the bite of the national minimum wage may vary considerably by location. The estimates of the employment and hours effects are sometimes positive and sometimes negative, but rarely statistically significant. In addition, many of the estimated elasticities are near zero, although there are some outliers (both positive and negative).

Lemos does not address the question of why she obtains much smaller employment effects than Fajnzylber. His estimates, however, make clear that the (implied) employment effects are concentrated among workers in the lower part of the wage distribution. Given these prior results, it is not entirely clear why Lemos focuses on overall employment effects rather than on the segments of the workforce most likely to bear any employment impact. Some of her results discussed below try to get closer to lower-skill groups, but none build on Fajnzylber's work by focusing on those with wages near the minimum. More attention to this issue would clarify the extent to which her results differ because they are more aggregate, or because of her use of alternative

[39] These estimates do not take account of possible hours effects, however.

minimum wage measures, a longer sample period, and other differences in her analyses.

In two subsequent studies, Lemos considers whether the evidence of negative employment effects is more compelling for labor markets in which we might expect them to be stronger. In Lemos (forthcoming), for example, she estimates minimum wage effects separately for the private and public sectors. In particular, she speculates that public sector labor demand is more inelastic than in the private sector – either because the state can raise revenues to cover higher costs, or because of the necessity of providing public services. Lemos finds that the wage effects in the two sectors are roughly comparable. Her evidence on employment and hours effects suggests no impact or a slight (and insignificant) positive impact in the private sector (with long-run elasticities of zero to 0.01). For the public sector, she finds a weak positive effect on employment but a stronger (although still insignificant) negative effect on hours, resulting in an overall long-run elasticity of total hours of −0.07.

Lemos (2006) examines the formal and informal sectors separately to see whether the minimum wage causes wages to rise and employment to decline in the formal sector (where the minimum wage should be more binding), but wages to fall and employment to rise in the informal sector (where compliance is lower), as would be predicted by the standard two-sector model of the minimum wage. In contrast to the predictions of this model, she finds that the minimum wage raises wages at the low end of the wage distribution in both sectors, and no significant evidence of either positive or negative effects of the minimum wage on employment or hours in either sector; in all cases the estimates are small. Lemos does not try to explain these results, nor those for the public versus private sector.

Echoing concerns raised in Neumark et al. (2006) about the extent to which employers would respond to an increase in the nominal minimum wage in an environment of hyperinflation, Lemos (2006) also reports evidence on the effects of the minimum wage in high versus low inflation periods. In the low inflation periods, when minimum wage increases are more likely to be perceived as longer-lasting increases in the cost of low-wage labor, the estimated effect of the minimum wage on employment in the formal sector is more negative, although still not statistically significant. Lemos also restricts the sample to less-educated (four years of schooling or less) individuals to see if the adverse effects of the minimum wage show through more clearly for workers whose wages are most likely to be boosted by an increase. The estimated disemployment effects in the formal sector are larger for this subsample, although once again the estimates are insignificant.

Finally, Lemos (2005) extends her analysis of aggregate employment effects to attempt to account for the endogeneity of minimum wage changes, noting that if minimum wages are increased in times of stronger economic growth, the estimated coefficients on the minimum wage variable will be biased upward. Lemos uses a variety of political measures as instruments for minimum wages, which she argues meet the standard criteria for validity. She then compares IV and OLS estimates for a number of different estimation approaches and disaggregations of the sample (for example, looking at teenagers or those with less education). We read her evidence as suggesting that the estimated disemployment effects tend to be somewhat more negative when the political instruments are used, although the standard errors increase enough that these estimates are never significant (her Table 3).

We have some doubts about the validity of these instruments, which, as Lemos notes, need to affect minimum wages but not be correlated with the error term in the employment equation. A natural source of such a correlation, however, is the influence of these political variables on other labor market policies that may have differential effects on employment across the regions of Brazil.[40] Thus, while Lemos concludes that there is no evidence to support the hypothesis that endogeneity of the minimum wage biases the estimated employment effect upward, we view the evidence as less informative.

Neumark et al. (2006) focus mainly on the distributional effects of minimum wages in Brazil, but they also report some employment effects for the 1996–2001 period, following the end of the hyper-inflation. Using the same data set as Lemos, they first estimate an employment effect for household heads, yielding a significant elasticity of -0.07 when lags

[40] Of course, one could also argue that other studies of minimum wages omit variation in other labor market policies, and that the problem in this paper is no different than in most others.

of the minimum wage are included. In contrast, the estimates imply positive effects of the minimum wage on employment and hours of other family members, which may reflect labor supply increases for these individuals (who are more likely to work in the informal sector) in response to the employment declines for household heads (who are more likely to work in the formal sector).

Overall, our sense is that the evidence for Brazil suggests that the effects of the minimum wage on employment are small in the aggregate. However, the evidence sometimes points to disemployment effects where we are more likely to find them – in low-inflation environments, and for less-skilled individuals and particularly lower-wage individuals.[41]

6.2.1.2 Mexico and Colombia

These 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 finds a small positive minimum wage elasticity (0.17) for wages and a small negative elasticity (-0.18) for employment; neither estimate is statistically significant. For Colombia, the estimated effects of the minimum wage are larger and statistically significant in both the wage and employment equations, with elasticities of 0.44 for wages and -0.34 for employment.

She then turns to firm-level panel data sets that allow her to focus on the 1980s period, when the divergence between the minimum wage

[41] An earlier review of minimum wage effects on employment in Brazil (Carneiro, 2001), which covers many unpublished papers circulating in Brazil (and written in Portuguese), argues that the evidence tends to point to disemployment effects.

changes in the two countries was especially large. For Mexico, specifications that include firm fixed effects yield elasticities of employment ranging from −0.03 to 0.03 for unskilled workers and −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 −0.03 to −0.24 for skilled workers. Bell attributes the differing results between Mexico and Colombia 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.

In contrast, Feliciano (1998) studies minimum wage effects for Mexico using data on all workers rather than just manufacturing workers. Her study uses data from the 1970, 1980, and 1990 Mexican Census of Population coupled with other sources. As a result, her sample period includes the 1980s when, as noted above, the minimum wage in Mexico declined sharply. But minimum wages also fell noticeably over the longer period she studied and became more uniform across states and regions within states as well.[42] Feliciano uses a standard panel data specification with a relative minimum wage variable, controls for the business cycle, and state and year fixed effects; the model is estimated at the state level, using the average minimum wage for regions within a state in cases when there was minimum wage variation within states.[43] She finds no minimum wage effects for men, with estimated elasticities typically close to and centered on zero, and generally insignificant. The one exception is for men aged 55 to 64, for which she finds a small but significant positive effect, which she suggests could reflect substitution toward these workers rather than direct effects of the minimum wage. For women, however, there is consistent evidence of disemploy-

[42] The number of geographic areas with a separately determined minimum wage declined from 111 in 1970 to 2 in 1990.

[43] She also presents estimates from a specification that instruments for the relative minimum wage variable with a real minimum wage variable (that is, divided by a price index rather than an average wage measure), 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.

ment effects for all age groups, with elasticities ranging from -0.41 to -0.76. Feliciano speculates that the differences between her results and Bell's arise because Bell focuses only on the manufacturing sector. She presumably could have checked this with her data, but did not. Nonetheless, this study seems to provide reliable evidence that the reductions in the minimum wage in Mexico increased employment of women and had little impact on men, consistent with overall disemployment effects.

In a related paper, Maloney and Nuñez Mendez (2004) examine the impact of minimum wages on wage distributions in 1998 for eight Latin American countries. Because the data reveal an especially pronounced impact of the minimum wage on the wage distribution in Colombia, paralleling Bell's findings, they narrow their focus to study the employment effects of minimum wages in that country. Using a rotating household panel data set with matched individual data across two quarters, they estimate the effects of two minimum wage increases during the 1997–1999 period covered by their data. Their sample consists only of men working 30 to 50 hours per week; it would be desirable to see evidence for other groups as well. As in much of the existing research using individual longitudinal data, they calculate transition rates from employment to non-employment for individuals who were employed as of the first observation and estimate how minimum wage increases affected these transition probabilities. In this particular study, the authors use the self-employed as a control group because they are not subject to the minimum wage. Because the minimum wage is often used as a numeraire for other wages, the specification also controls for each individual's initial position in the wage distribution, and separate estimates of the minimum wage effects on employment are reported at various wage levels (as in Neumark et al., 2004). The models also include individual characteristics, quarterly and regional dummy variables, and, in some specifications, lagged values of the minimum wage changes.

The estimated employment effects reported by the authors are large, negative, and statistically significant. In addition, these negative effects are evident at some of the higher locations in the wage distribution, suggesting that the numeraire function of the minimum wage also causes employment losses, although it is difficult to understand why this wage rigidity would persist at higher wage levels. The lagged effects are also negative and significant, suggesting a period of adjustment. The authors repeat the analysis excluding the dummies for location in the wage distribution to get average effects and calculate an elasticity of employment with respect to the minimum wage of −0.15. Overall, this research appears to confirm Bell's previous finding that minimum wages have negative consequences for employment in Colombia, albeit for a select group of workers.

6.2.1.3 Chile

Montenegro and Pagés (2004) estimate the effects of job security provisions and minimum wages in Chile on the relative employment of different groups, using a time-series of cross-sectional data sets for Santiago from 1960 to 1998. They estimate a model for employment at the individual level and include a set of variables for age, skill level, and sex, as well as these variables interacted with real minimum wage indexes (differentiating the minimum for those under age 18 and those aged 18 and over). Because the minimum wage varies by age, it is possible, in principle, to include year effects and still estimate an overall minimum wage effect on the level of employment. However, they indicate that the coefficient on the minimum wage variable was not robust in this specification, so they instead included only the interaction terms and the year dummy variables, and thus estimate relative employment effects.[44] This research design does not control for possible differences in trends for different demographic and skill groups.

For the most part, the authors' estimates are consistent with the competitive model, with the evidence indicating that a higher minimum wage reduces the employment of young workers and unskilled workers relative to that for older male skilled workers, with the disemployment effects particularly strong for workers who are both young and unskilled. Curiously, though, even though women earn lower wages,

[44] At the end of their paper, they attempt to estimate overall employment effects on older skilled male workers and to use these estimates to back out total effects on each age-sex-skill group. However, we do not see how this approach circumvents the problem just discussed.

minimum wages appear to increase their employment relative to that for older males.

6.2.1.4 Costa Rica

As pointed out by Gindling and Terrell (2004), Costa Rica provides fertile ground for studying minimum wages in a developing country context. In the period they study, the country moved from a system of over 500 minimum wages based on occupation and skill categories to a system of only 19 different levels. This sharp consolidation of the number of separate minimum wages generated a great deal of exogenous variation in minimum wages by occupation and skill category. Gindling and Terrell exploit this variation to study the effects of minimum wage changes on employment and hours, using individual-level data covering 1988–2000.

Specifically, they create a pooled time-series cross-sectional data set on about 10,000 individuals per year, and estimate models for employment in the covered sector and hours worked by workers in each sector (as well as for wages and earnings). 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. Because of the need for information on occupation, the sample for the employment and hours analysis is restricted to those who have worked before.

The analysis first establishes that minimum wages affect wages, via inspection of histograms and estimates of similar regression models for wages. 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, unlike the others, is not significant. While the employment elasticity is of a similar magnitude to those estimated for teenagers in the United States, the implied employment effects are much bigger because far more workers are affected by the minimum wage in

Costa Rica. Given the significant variation in minimum wages in Costa Rica and the authors' ability to assign minimum wages to individuals, we regard this as one of the more convincing developing country studies.

6.2.1.5 Trinidad and Tobago

Strobl and Walsh (2003) examine the effects of the introduction of a national minimum wage in April 1998 in Trinidad and Tobago. They use a rotational household survey administered quarterly, which lets them construct short panels on individuals. Because their data set is limited to the 1996–1998 period, they can estimate only relatively short-run effects (at most eight months) of the minimum wage's introduction. Given the problem of large informal sectors in developing countries (and likely low enforcement), Strobl and Walsh focus mainly on compliance, and more tangentially on employment. Their evidence suggests substantial non-compliance, but also that the minimum wage is binding on some workers, and led to wage increases in large firms.

Strobl and Walsh estimate employment effects by asking whether workers initially employed below the new minimum wage were less likely to be employed after its introduction. They find that, for males, being caught by the minimum wage increases the probability of job loss by 0.09. Additionally, the larger the gap between the individual's initial wage and the minimum wage, the higher the probability of job loss. In contrast, the estimated effects for females are close to zero and insignificant (0.023 for being caught by the minimum), although large firms were more likely to lay off females than small firms in response to the minimum wage. The introduction of a new minimum wage in Trinidad and Tobago provides a nice – although rare – instance in which to study minimum wage effects, paralleling that for the United Kingdom. However, a valid control group is still necessary to control for aggregate trends. In the probit for job loss, the authors use all workers whose initial wage was above the new minimum wage as the control group. However, it might have been preferable to use a narrower group of workers who are relatively low wage but not bound by the minimum, although even in this case, aggregate trends that differ by skill level can invalidate estimated effects of introducing a national minimum wage.

6.2.1.6 Puerto Rico

Although a U.S. territory, Puerto Rico shares some similarities with the countries covered in this section because of its low wage levels. In 1938, when the FLSA was passed, the federal minimum wage applied to Puerto Rico as well. Recognizing that market wages were considerably lower in Puerto Rico than in the rest of the United States, the Congress passed subsequent amendments that allowed industry committees (including representatives from employers, labor, and to the public) to set lower minimum wages and to leave some important industries (like trade and services) uncovered. However, in 1974 the Congress reversed itself and enacted automatic increases to make the minimum uniform across industries, bring it to mainland levels, and to extend coverage as in the mainland; these objectives were accomplished by 1983. As a result, with average wages in Puerto Rico about half the average for the remainder of the United States, the minimum wage subsequently had much more bite in Puerto Rico than in the United States. As shown by Castillo-Freeman and Freeman (1992), wage distributions using Puerto Rican Census data on workers working over 20 hours per week clearly show the sharp impact of the federal minimum wage on wages in Puerto Rico.

The authors use this variation to study the effects of the U.S. federal minimum wage on employment in Puerto Rico. In particular, they first estimate time-series regressions for the log of the employment-to-population ratio for the period 1956–1987. Using two different measures of the ratio of the minimum wage to the average wage, they estimate minimum wage elasticities for aggregate employment of -0.11 and -0.15, both of which are statistically significant. In addition to this aggregate time-series approach, they implement a cross-section time-series analysis for 37 manufacturing industries in order to exploit the variation in minimum wages by industry, estimating a model that includes fixed year and industry effects. For the entire sample period, they report a statistically significant employment elasticity of −0.54.[45] They then estimate the model separately for the 1956–1973 and 1974–1987 subperiods. The estimated elasticities are 0.20 and −0.91, respectively, indicating that all of the effect occurred after Congress mandated that the minimum wage in Puerto be realigned with the U.S. federal minimum. The contrast between the pre- and post-1974 estimates may also suggest that the industry committees that set minimum wages in Puerto Rico prior to 1974 took local labor market conditions into account in deciding on the appropriate levels of the minimum.

Krueger (1995) re-examined the evidence for Puerto Rico using the same data as in the original study. His findings generally indicate that the conclusions reached by Castillo-Freeman and Freeman are quite fragile, with different answers (especially for the cross-industry analysis) emerging from specifications that differ in terms of weighting or functional form. Krueger also suggests, as noted earlier, that labor demand elasticities could vary across industries in such a way as to generate spurious disemployment effects, although the force of this argument would be stronger were it accompanied by evidence that this is responsible for the results that Castillo-Freeman and Freeman report. Nevertheless, we are left with somewhat inconclusive evidence for Puerto Rico.

6.2.2 Indonesia

In the early 1990s, international pressure led Indonesia to increase minimum wages sharply, and they tripled in nominal terms (and doubled in real terms) over the first half of the decade. Two papers (Rama, 2001, Suryahadi et al., 2003) exploit this sudden and arguably exogenous increase to study the effects of minimum wages in a developing country context, and other papers followed.

Rama (2001) exploits province-level differences in minimum wage increases in the early 1990s that stemmed from the fact that minimum wages varied considerably across provinces prior to 1989, but

[45] Note that these cross-industry elasticities should be larger than those from the time-series analysis, as employment may shift among industries with less of a change in overall employment.

converged during the early 1990s in response to legislation passed in 1989.[46] In particular, he uses data for the years 1988 to 1994 to estimate a standard panel data regression of the urban employment rate on a minimum wage variable, province and year fixed effects, and other controls. The minimum wage variable is measured in a variety of ways, although most of the specifications use the ratio of the minimum to measures of average or aggregate wages or labor productivity. The estimated elasticity of aggregate urban employment with respect to the minimum wage is small, ranging from zero to −0.04. The range is larger for 15–24 year-olds, extending from 0.02 to −0.25. However, the estimates are all insignificant. Rama also presents results disaggregated by firm size, suggesting that small firms (without providing a definition of the size cutoff) would be expected to conform more to the competitive model, while larger firms could exhibit monopsony power. His evidence is consistent with positive (but insignificant) elasticities for large manufacturing firms, and negative, much larger, and sometimes significant elasticities (ranging from −0.77 to −1.30) for small firms.

Suryahadi et al. (2003) extend this analysis through 2000. They also present results for various subgroups of workers among the urban, formal-sector workforce, including men and women, adults and youths, more- and less-educated workers, full-time and part-time workers, and white-collar and blue-collar workers.[47] The estimates in this paper provide stronger evidence of disemployment effects than does Rama's analysis, and evidence from wage distributions suggests that the larger negative employment effects occur among individuals more likely to be bound by the minimum wage. In particular, for overall employment, the authors estimate a significant negative employment elasticity of −0.06. For women, the estimated elasticity is −0.16, and for less-educated workers −0.09; in both cases these estimates are significant. The point estimates are negative, but generally insignificant and

[46] There is some question as to how much real variation in minimum wages across provinces was generated by this change, given that, as Rama notes, "minimum wages were not enforced in practice" (p. 866).

[47] In some specifications they include the share of workers earning above the minimum wage as a measure of compliance. This is potentially problematic because it is endogenous with respect to employment. However, the authors also report results without this control variable (in Table 3), and we emphasize those for comparability.

smaller for men, adults, youths, those with more education, blue-collar workers, and both full-time and part-time workers.[48] The one exception to the evidence of negative employment effects is for white-collar workers, for whom the authors find a large positive elasticity. They interpret this result as evidence that employers substitute away from blue-collar labor in response to minimum wage increases. Regardless of whether this interpretation is correct, white-collar wages are much higher and are not directly influenced by the minimum wage.[49]

Other studies for Indonesia attempt to use more compelling information to identify the effects of minimum wages. Harrison and Scorse (2005) analyze both the effects of increases in the minimum wage and the effects of the U.S.-driven anti-sweatshop campaigns on wages and employment in Indonesia, using firm-level data from the Annual Manufacturing Survey of Indonesia over the years 1990–1996. In contrast to the above studies, they exploit variation in minimum wages by districts within provinces. In addition, the authors are able to separate the effects of the minimum wage from those stemming from anti-sweatshop activism by recognizing that the latter effects 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 estimates of the elasticity of employment with respect to the minimum wage ranging from −0.12 to −0.18; the estimates are generally statistically significant and robust to a variety of specification changes. Only in small firms were the estimated effects insignificant (and smaller), which the authors suggest

[48] The estimated disemployment effects are considerably larger and significant for youths and both part-time and full-time workers when the compliance measure is included.

[49] Islam and Nazara (2000) also revisit Rama's evidence using data through 1998. Their reporting of results is rather unclear, but it appears that they find negative and significant employment effects for overall formal employment in models that include region and period dummy variables, but positive effects if the region and period dummy variables are dropped. They describe the negative estimates, therefore, as "model-specific." However, as this review makes clear, it is quite standard to include some version of region and period effects, and it is well-understood that models without these effects may be mis-specified. The case for including these effects in Indonesia is particularly strong, as economic conditions differ considerably across regions and the East Asian financial crisis struck in the latter part of the sample period.

may be the result of lower compliance among small firms. Using annual differences instead, they find smaller elasticities of -0.05. They also examine the effects of the minimum wage on firm closings and find weak evidence that the minimum wage increased the probability of exit, although they note that the effect of the minimum wage on exit rates could be larger over a longer time period.

Finally, Alatas and Cameron (2003) also try to use a sharper identification strategy. They 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. Alatas and Cameron also provide evidence that, in these provinces, improved enforcement throughout the 1990s coupled with the large minimum wage changes led to detectable shifts in wage distributions.

Using a panel of all Indonesian manufacturing firms with 20 or more employees, the authors implement a matched difference-in-differences approach to estimate employment effects for production workers, who are typically less skilled, identifying the minimum wage effect from changes in otherwise similar firms in Botabek relative to Jakarta. The authors estimate the model separately for small (20–150 workers) domestic, large domestic, and large foreign firms because they believe that different cost structures across these categories may 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. Other evidence reported in the paper suggests that higher minimum wages did not increase exit rates.

Overall, the evidence for Indonesia is mixed, with the results dependent upon research design and firm size. As a result, we do not think one can draw firm conclusions, although 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. Either way, the minimum wage changes and availability of data in Indonesia suggest that further research on the effects of the minimum wage in this country could be especially informative in a developing country context.

7

Conclusions and Discussion

Our lengthy review of the new minimum wage research documents the wide range of estimates of the effects of the minimum wage on employment, especially when compared to the review of the earlier literature by Brown et al. (1982). For example, few of the studies in the Brown et al. 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 of teenagers in the United States, the range of 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. And, the range would be considerably wider if we were to include estimates for narrower subsets of workers and industries or estimates from other countries.

Nonetheless, the oft-stated assertion that the new minimum wage research fails to support the conclusion that the minimum wage reduces the employment of low-skilled workers is clearly incorrect. Indeed, in our view, the preponderance of the evidence points to disemployment effects. For example, the studies surveyed in this monograph correspond to 102 entries in our summary tables.[1] Of these, nearly two-thirds give a relatively consistent (although by no means always statistically significant) indication of negative employment effects of minimum wages, while only eight give a relatively consistent indication of positive employment effects. In addition, we have highlighted in the tables 33 studies (or entries) that we regard as providing the most credible evidence, and 28 (85 percent) of these point to negative employment effects.[2] Moreover, when researchers focus on the least-skilled groups most likely to be adversely affected by minimum wages, 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 predicts disemployment effects.

Based on our review of the literature, we would also highlight some important considerations that economists and policymakers should keep in mind when assessing the empirical evidence from studies of the employment effects of minimum wages. First, 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, while 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. This raises the question, highlighted in the reviews of *Myth and Measurement* by both Brown (1995) and Hamermesh (1995), of whether the latter analyses encompass too short of a time period with which to capture the full effects of minimum wage changes given the

[1] We do not include every single paper we have discussed. In particular, a few papers that use very similar data and estimators to other papers included in the tables, but that largely comment on or replicate the latter, or present a narrower set of estimates, are not included.

[2] Note that we have left out of this calculation some of our studies that use similar specifications and data to other studies we have done, and which instead explore other issues; were these included, the percentage finding negative employment effects would be higher.

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, and, in our view, the need to allow for sufficient time to observe the consequences of a minimum wage change is an important lesson for researchers and policymakers.

Second, the concerns raised in the literature about the case study approach seem especially problematic. Even aside from the question of whether the authors' own surveys 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, aside from the estimates of the effects of the minimum wage on low-skilled workers 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 wage 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 least-skilled workers more than is suggested by the net disemployment effects estimated in many studies.

Finally, our review of the literature leads us to suggest a number of areas where additional research may prove to be especially fruitful. One question that is relevant to much of the literature is how to address the potential endogeneity of minimum wage policy 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. A principal difficulty is that in specifications that include state or region fixed effects, the researcher needs a set of instrumental variables that vary over time. Second, although much of the literature has focused on the employment effects of the minimum wage, the predictions of theory tend to be about overall labor input rather than employment specifically. A few studies have attempted to disentangle the implications for hours from those for employment, and the differences in results reported in rather similar studies suggest that this remains an area for further research. Third, although we view the evidence as largely consistent with the competitive model of low-wage labor markets, given the ambiguous predictions of the competitive model in specific circumstances, and of the monopsony model more generally, there is scope for bringing more direct evidence to bear on whether the monopsony model or the competitive model better characterizes the low-wage labor market. And fourth, a systematic assessment of the sources of differences in the estimates across studies using meta-analysis techniques (rather than simply combining estimates across studies to obtain one "meta" estimate) could provide complementary evidence to this survey and improve our understanding of how to interpret the literature.

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. Of course, as we have argued elsewhere, 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. In particular, a more comprehensive review that includes the implications of the minimum wage for the levels and distributions of wages, employment and hours, incomes, and human capital accumulation, as well as consideration of alternative policies, is ultimately needed to assess whether raising the minimum wage is good economic policy. Given that the weight of the evidence points to disemployment effects, any argument in favor of pursuing higher minimum wages would appear to require that the benefits of a higher minimum wage outweigh the costs of the employment losses for those workers who are adversely affected.

Acknowledgments

Neumark is Professor of Economics at the University of California at Irvine, a Research Associate at the National Bureau of Economic Research, and a Research Fellow at IZA. Wascher is Deputy Associate Director in the Division of Research and Statistics at the Board of Governors of the Federal Reserve System. The authors thank Jennifer Graves for research assistance, and Dan Aaronson, Dale Belman, Jared Bernstein, Mike Campolieti, Ana Rute Cardoso, Kenneth Couch, Wendy Cunningham, Harry Holzer, Lazaros Karageorgiou, Andrew Leigh, William Maloney, Tom Michl, Pedro Portugal, Nicholas Potter, Michael Reich, Joseph Sabia, Bradley Schiller, Per Skedinger, Walter Wessels, David Wise, and an anonymous reviewer for their helpful comments. The views expressed are the authors' and do not necessarily reflect those of the Federal Reserve Board.

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