MINIMUM WAGES AND EMPLOYMENT:
A REVIEW OF EVIDENCE FROM THE NEW MINIMUM WAGE RESEARCH

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I. Introduction

For much of the past century, the minimum wage has been a controversial subject among policymakers and economists. From even before its inception as a major element of the 1938 Fair Labor Standards Act (1938), 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 much of the following three decades, a fierce debate raged between economists who claimed that the labor market 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). During the same period, other economists were beginning to accumulate research 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). Similarly, 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).

In May 1981 the Commission published its report, calling it "the most exhaustive inquiry ever undertaken into the issues surrounding the Act since its inception" (Minimum Wage Study Commission, 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 at that time. 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, legislators in an increasing number of states 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 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 exploit state-level variation in both minimum wages and in economic conditions as a way of estimating 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 (e.g., 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, and these state minimum wages cover a large share of the population.

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 paper. In contrast, there is no comprehensive review of the extensive literature that has developed over the past fifteen years, which, as documented in Tables 1-5, now comprises (to the best of our knowledge) more than 90 studies covering 15 countries.[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 report on research that examines more recent increases in minimum wage laws or otherwise extends the literature. We then complete 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 three-fold. First, much of the political debate surrounding proposed changes in the minimum wage revolves around the potential effects of an increase in the wage floor on employment. Although we do not view that focus as entirely appropriate, the fact that the "jobs" question takes on such importance suggests that the answers ought to 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. In this regard, where possible, we attempt to draw some general conclusions about the effects of the minimum wage on employment that are relevant to policymakers, pointing out, in particular, the predictions of the empirical research for when, how, and for whom raising the minimum wage will have consequences.

Second, we hope that our review will help the reader to assess the reasonableness of alternative models of the labor market. In particular, much of the recent empirical research has been characterized as rejecting the competitive model of the labor market in favor of the monopsony model (or vice versa). As we will note throughout the paper, economic theory often fails to make an unambiguous prediction about the direction or magnitude of 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,

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

the elasticities of substitution across different types of workers and cross-elasticities of demand across different types of goods. However, there are empirical tendencies that will tend to match up better with one model or the other, and we try to provide a sense of what these tendencies are.

Third, economists or policymakers perusing the existing literature might have a very hard time deciding what the evidence on minimum wages now says. Over 100 studies have been published on the effects of minimum wage increases on employment since the 1990s. And, of course, there are many discordant results. Finally, the findings from the newer research on minimum wages are summarized differently in different places. In some cases, the new 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 try to present a comprehensive review of the more-recent minimum wage literature that both

[2] In our view, groups advocating for minimum wage increases have—perhaps not surprisingly—made stronger statements. For example, Chapman, in 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.

provides an accurate accounting of the range of estimates in existing studies, and also attempts to assess these studies to see whether more firm conclusions can be drawn.

In putting together this review, we have intentionally foregone 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 in the literature, and the considerable variation in approaches and the variation in quality of the research, lumping the studies into one meta-analysis does not seem the best way to make sense of the literature. And 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 five tables covering different types of studies, including a brief summary of the minimum wage change studied, the methods and data used, the results, and what we regard as important criticisms.

II. 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.[3] Subsequently, these papers, along with an additional paper contributed by David Card 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 represent well 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

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

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, 1992a), an analysis of an increase in a particular state's minimum wage (Card, 1992b), and a survey of fast-food restaurants before and after an increase in the minimum wage (Katz and Krueger, 1992).[4] 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, 1992a) to a positive effect of the minimum wage on employment (Card, 1992b; Katz and Krueger). Given the relative newness of the methods employed in the papers, all of these researchers 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).

III. 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 initial 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 this literature, we find it useful to separate these papers into two broad categories: (1) panel data studies that employed 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.

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:

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[4] The symposium also included a paper by Bruce Smith and Wayne 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 this review.

(1) $$Y_{it} = \alpha MW_{it} + R_{it}\beta + \varepsilon_{it}$$

to a dataset consisting of state-year observations on an outcome variable (Y) such as employment, hours, or wages; a minimum wage variable (MW); and a vector of control variables (R) that may include state (i) and time (t) effects. The exact specification of the minimum wage variable varies 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, as 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 the outcome variable. 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 David Card's (1992a) 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; moreover, these results were robust to the inclusion of a control for overall labor market conditions. In contrast, when Card regressed the change in state employment-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 (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 a relatively long sample period. Specifically, we used an annual panel of state-specific observations from 1973 to 1989 for large states and from 1977 to 1989 for smaller states.[5] The minimum wage variable used in this study was similar to the Kaitz index that had populated 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, we concluded that 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.

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 particular states. Two papers in the ILRR symposium took this approach: Lawrence Katz and Alan Krueger's study of the effects of the 1991 increase in the federal minimum wage on fast-food restaurants in Texas, and David Card's study of the 1989 increase in California's minimum wage. More broadly, the details of the empirical approach in this subset of the literature have varied, with some studies focused on specific increases in the federal minimum wage and others focused on minimum

[5] Because of data limitations in the CPS prior to 1979, the data refer to May of each year. In addition, the different time periods across states reflect the fact that the CPS state identifiers did not have distinct codes for smaller states prior to 1977.

wage increases in particular state, and some studies using published data and others based on data collected by the authors. However, a unifying theme of this aspect of the literature was the authors' view 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 motivation—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 then 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 provides the identification used to measure 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 positive and statistically significant effect of the minimum wage on employment, with the estimated elasticities ranging 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 (1992b) took a different approach, using data from the 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, Card 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 change. 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 0.15. Card also finds a relative increase in employment in the retail trade industry in California between 1987 and 1989, and although he does find a small relative decline in the eating and drinking industry in California, he interprets it as more likely stemming from differences in longer-run trends than to the effect of the minimum wage increase. As did Katz and Krueger, Card raises the possibility that these results might indicate the presence of monopsony power in the low-wage labor market.

Given these provocative findings and renewed interest in using the natural experiment approach to studying 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, estimates of the employment response 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, the estimated employment effects were positive, although, again, the coefficient was not statistically significant. The authors interpreted these results, which were similar to those found by Katz and Krueger, as inconsistent with the conventional view of minimum wage effects.[6]

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 $5.15 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.[7] For example, stores that initially paid low starting wages showed significantly more employment growth between February and November than did stores that paid higher starting wages. Similarly, employment in the New Jersey sample rose over this period, while employment in the Pennsylvania sample fell. Putting this information

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[6] 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.

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

together, Card and Krueger constructed a wage gap measure equal to the difference between the initial starting wage and $5.25 per hour for stores in New Jersey and zero for stores in Pennsylvania. Again, the results showed 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).[8]

IV. Issues Raised in Subsequent Research

The divergent findings of this initial round of research stimulated a number of additional 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. Thus, much of the ensuing empirical literature attempted to uncover some of 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. More specifically, 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.

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 earlier literature in using a variant of the Kaitz index that included state minimum wages as our measure of the effective minimum wage, while Card (1992a) had used the percentage of teenagers earnings 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

[8] 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.

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, but it also seems less important, especially in the context of the recent literature.[9] We showed in our reply (Neumark and Wascher, 1994) that the employment elasticity with respect to changes in federal coverage was not significantly different from the employment elasticity with respect to changes in the level of the minimum wage for our relatively long sample period. Moreover, at the federal level there have not been any major changes to coverage since the early 1970s, and the combined coverage of federal and state laws has been persistently high for some time. Thus, for studies that begin in the late 1970s or later, which comprise the majority of the new minimum wage literature, changes in coverage are not likely to offer much in the way of identification, suggesting that it may be reasonable to ignore coverage altogether. Indeed, much of the literature in the past decade or so has followed this approach.[10]

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[9] Schiller (1994a) 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, Schiller finds that youth employment rates are significantly lower in states without any exemption than in other states.

[10] On the other hand, Schiller (1994b) argues that even at the federal level, coverage of teenagers is "far lower and less uniform than commonly assumed" (p. 132) and that the increase in the small business exemption (and its extension to construction, laundry, and dry cleaning businesses) that coincided with the April 1990 increase in the federal minimum may have mitigated the effects of the 1990-91 minimum wage increases on teenage employment. More generally, he speculates that the failure of the minimum wage literature to adequately account for teenage employment in the uncovered sector has resulted in an underestimate of the disemployment effects of the minimum wage.

In contrast, their 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 we showed in our reply, the Kaitz index should be (and is) positively correlated with changes in the relative teen wage, indicating that the Kaitz index is correctly classifying 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, Katz, and Krueger 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 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 thought to be a substitution away from lower-skilled minimum wage workers toward higher-skilled, higher-wage workers, it seems particularly important to include a relative minimum wage variable to capture this effect.[11] 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 (1992a), 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

[11] See Grant and Hamermesh (1981).

studied by Card. However, 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. [12]

Although it seems important to specify the minimum wage variable in real or relative terms in studies that use longer samples, a valid question that arises from the discussion of the appropriate choice of the minimum wage variable is whether the constraint embedded in the relative minimum wage variable—that changes in the adult wage should have the same size, but opposite-signed, effect on employment as changes in the nominal minimum wage[13]—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 (although with the opposite sign). 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). 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.

[12] Wessels (2006) argues that the fraction-affected specification used by Card (1992a) is an inappropriate measure of the minimum wage because it ignores some of the cross-state 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 1990-91 and 1996-97 (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.

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

Nevertheless, subsequent research has frequently relaxed this constraint. For example, Burkhauser et al. (2000a, 2000b) rewrite the relative minimum wage effect in an equation for log employment rates as:

$$\beta \ln MW / W = \beta_1 \ln MW + \beta_2 \ln W$$

where W is the average wage for adults and β_1 is interpreted as the minimum wage elasticity. [14] 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.[15] In contrast, Keil et al. (2001) rewrite the relative minimum wage effect as:

$$\beta \ln MW / W = \beta_1 \ln MW / W + \beta_2 \ln W / P,$$

which seems more natural to us. Again, the authors interpret β_1 as the minimum wage elasticity and generally report negative estimates of that coefficient.

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 (1985-92), 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 specifications seem problematic because the coefficients on the indicator variables may be capturing other influences as well.

Lagged Effects of the Minimum Wage

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[14] We should note that, in an appendix table, Burkhauser et al. (2000b) do 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 in this specification (−0.25 vs. −0.30 in the equation for teenagers), albeit still statistically significant.

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

The new minimum wage literature also resurrected questions about the 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 they 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 became effective, so that employers would be well prepared to adjust quickly when (or even before) the new law took effect. Card and Krueger (1995a) also argue that the industries that typically employ minimum wage workers (e.g., 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 from the minimum wage 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 nonlabor inputs (e.g., capital) slowly, which will tend to slow the adjustment of other inputs (including labor) as well. In this sense, the omission of lagged effects from the model may inappropriately exclude the possibility of a 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 presence 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 (1992a) 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 (1992a) produced minimum wage effects close to zero, similar to what was reported in his paper. In contrast, 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.[16]

Baker et al. (1999) take this analysis a step further in a study of the effects of the minimum wage on employment in Canada.[17] The authors begin by replicating some of the U.S. panel data estimates for teenagers with Canadian data.[18] They report that first-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. then investigate 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, explaining how 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

[16] 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 from minimum wages.

[17] Although we assess most of the international evidence later in the paper, we include several papers from Canada in this and the subsequent section, both because of its relevance for the discussion of the U.S. results and because of the similarity of U.S. and Canadian labor markets.

[18] As the authors explain, 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 setting is the far greater frequency (at least until recently in the United States) of minimum wage changes.

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 estimate the minimum wage elasticities separately at high and low frequencies.[19] 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 report 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 do not analyze U.S. data directly, they use the U.S. literature to demonstrate—through equations that relate estimated coefficients for alternative estimators to the implied elasticities at different frequencies—that their conclusions explain the different findings for the United States equally well.

Note that the importance of these findings are not so much to establish a particular answer (emphasizing high vs. low frequency) as "correct," but rather to use this perspective to better understand why the estimates in the U.S. literature vary with the length of differences and the inclusion of lags. Nevertheless, the 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 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 (rather than intrafirm) adjustment rather than as labor adjustment per se, which should be relatively quick in the low-skill labor market. In any event, the Baker et al. analysis clearly suggests that claims of an absence of a 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 have also tended to find evidence of lags in the effects of the minimum wage on employment. For example, Burkhauser et al. (2000a) re-

[19] The simplest filter they use is $MW_{it} = ½(MW_{it} - MW_{it-1}) + ½(MW_{it} + MW_{it-1})$, where the first term captures high frequency changes in the minimum wage variable and the second term captures low-frequency changes. They also employ a more precise finite Fourier transform with similar results.

estimated their specifications including lags and found that the coefficient on the lagged minimum wage variable was statistically significant even in specifications in which the contemporaneous coefficient was not significant. Moreover, estimates of the elasticities including the lagged effects were considerably larger than those calculated from specifications that included only contemporaneous terms.[20] Keil et al. (2001) also allow for the effects of minimum wage to show through with a lag, although they accomplish 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 estimate using a Factor-GLS procedure with instrumental variables, they report 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 thus 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, it seems to us that estimates of the elasticities relevant both to the testing of alternative theories of labor demand and to the public policy debate should always at least consider both contemporaneous and lagged responses to a change in the minimum wage.

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 measure of school enrollment we used included only individuals who were 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

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[20] 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.

equation because that equation is essentially a labor demand function. Because a statistically significant disemployment effect for teenagers was only evident in our specifications that included the school enrollment rate, these questions about the correct specification of the basic model and about possible biases associated with the potential endogeneity of the enrollment rate led some to conclude that our results were consistent with no effect of the minimum wage on teen employment.

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 determined by both demand and supply. As a result, the specification of a model for the employment of all teenagers should also include factors that reflect exogenous shifts in the labor supply curve, including exogenous changes in the school enrollment rate.

Regarding the measurement of school enrollment, we agree that the definition of schooling used in our original paper was too narrow. As it turned out, however, incorporating broader measures of enrollment that do not exclude employed teenagers led to only minor differences in the results. For example, in our reply to Card et al. (Neumark and Wascher, 1994), 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. The resulting employment elasticity for teenagers fell to −0.11, toward the low end of the range we reported originally. However, using an even broader definition of enrollment that is calculated independently of employment, we found a statistically significant employment elasticity of −0.22 (Neumark and Wascher, 1996a).[21]

[21] 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 Turners 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 (1996a), which uses October observations for all of the variables in the model.

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 from the decision to work, the estimates from a version of equation (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 equation (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 −0.14 to −0.39. Thus, although the point estimates of the effects were sensitive to which enrollment measure we used, in general the IV estimates supported the view that minimum wages reduced employment among teenagers.

Second, and more fundamentally, in Neumark and Wascher (1995a) we extended equation (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 four possible states of employment/enrollment status (j) to the minimum wage and other controls (X):

(2) $$U_{kj} = \alpha_j MW + X\beta_j + \varepsilon_k, \qquad j = 1,\ldots,4.$$

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).[22]

Estimating the model using state-year data from 1977-1989, 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

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

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.

We noted that the results were consistent with a hypothesis in which a higher minimum wage causes 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), and that the resulting increase in the relative wages of higher-skilled teenagers induces some of them to leave school for employment. However, this hypothesis cannot be explicitly tested without information on the flows of teenagers across the different enrollment/employment states. Thus, in Neumark and Wascher (1995b and 1996b), we turned to individual panel-level data from matched CPS surveys.[23] In particular, 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 above-mentioned hypothesis, the results suggested that the employment effects of the minimum wage fall largely on the 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. 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 suggested that the teenage employment elasticities typically reported in the literature likely understate the size of the disemployment effects on the lowest-skilled workers, instead capturing net employment changes among a broader group of teenagers that mask labor-labor substitution.

[23] The sample design of the CPS permits a match of some individuals for the same month across two consecutive years. However, matches are not guaranteed, and for teenagers roughly 65 percent of the sample in each year could be matched to an observation in the following year.

As far as we can tell, 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 proportions on 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) use an approach similar to Neumark and Wascher (1995b) but focus, à la Card (1992a), on the early 1990s increases in the federal minimum wage. In particular, they examine 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 to $4.25 per hour. 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 movements among nonminority 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, especially if they lived in a central city. As in our earlier papers, this evidence 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 nonstudent populations in Canada.

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

Researchers also raised several other concerns about model specification in reaction to the first rounds of the new minimum wage research. For example, in reviewing Card's (1992a) analysis of the 1991 federal minimum wage increase, Deere et al. (1995) highlighted the possibility that differences in underlying trends in employment growth across states might bias estimates of the effects in short state-year panels like that used by Card. In the case of the 1991 increase in the federal minimum wage, they noted that the low-wage states where Card's fraction affected variable suggested that the effects of the minimum wage increase should be most pronounced also tended to be states where trend employment growth was faster. In particular, they showed that rates of employment growth for well-educated adult men were also higher in low-wage states than in high-wage states between 1989 and 1992, and that controlling for the 1985-1992 trend in employment and for business cycle developments 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, the minimum wage effect is taken from year dummies for the 1990 and 1991 minimum wage increases and thus should be interpreted somewhat cautiously. More broadly, however, 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).

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 the variation in the federal minimum wage. As a result, they assert 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 show that equations estimated without year effects consistently produce negative and statistically significant coefficients on the minimum wage variable, while specifications that include year effects consistently produce small and insignificant coefficients. The one exception is when a lagged minimum wage variable is included, in which case the estimated minimum wage effect is statistically significant even with year effects included, although smaller in absolute value. The elasticities from the equations estimated without year effects range from −0.4 to −0.6 for teenagers, somewhat larger than the earlier consensus, while the elasticity in the specification that includes both year effects and the lagged minimum wage is −0.22.[24] In a broader sense, they interpret their results 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.

Burkhauser et al. raise an important question—how to balance the loss of identification associated with including year effects with the potential bias associated with omitted macroeconomic effects that would be captured by the year dummies. The authors recognize that the possibility of omitted macroeconomic or other aggregate effects is a potential criticism of their specification. But, in response, they note 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, they report that when they add dummy variables corresponding to recessions to the specification, they find 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 the specification will permit some identification from variation in the federal minimum wage because of differential movements in state wages or prices. 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; indeed, 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. In addition, with the

[24] Burkhauser et al. (2000b) apply the same methodology to other demographic/education groups and find especially large negative effects for black youths and high-school dropouts aged 20-24.

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.

In this regard, a recent paper by Bazen and Gallo (2006) attempts 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. (2000a), they first use quarterly state-level data from 1984-1989, when the federal minimum wage was unchanged, and estimate the effect on teenage employment of changes in state-specific minimum wage increases over that period. The coefficient on the minimum wage in these specifications is close to zero and insignificant, whether or not year effects are included. They then re-estimate the model through 1992 and find 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 perform a similar analysis for the 1992-96 period, when the federal minimum was again unchanged, and also find 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 specify a more general model that includes 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 results in a state minimum wage level that exceeds 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 interpret their results as suggesting that small state-level increases may not have significant disemployment effects, although they admit that their tests are relatively weak.

Reactions to the Initial Round of State Case Studies

The case studies that comprised the other strand of the new minimum wage literature were also 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*—states 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) concludes that "even on its own grounds, CK's strongest evidence is fatally flawed" (p. 838).

In general, the criticisms of the case study approach focused on four 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 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 with geographic proximity, using one 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).

Hamermesh also questions whether the initial observation in each study was sufficiently prior to the effective date of the minimum wage change. He notes that for both Katz and Krueger's study of the effects in Texas of the federal minimum wage increase and Card and Krueger's study of the New Jersey minimum wage increase, the initial survey took place just a few months prior to the effective date of each minimum wage increase. Similarly, Card's study of California uses data for 1987, only six months prior to the minimum wage increase in July 1988. In each case, the law that led to the increase in the minimum wage was either already enacted or under discussion at the time of the initial observation, raising the possibility that some employers had already begun to adjust to the higher minimum wage. If so, the difference-in-difference estimates would tend to understate any employment effects.

A third, and related, criticism relates to our earlier discussion of the possibility that the effects of the minimum wage on employment occur with a lag. That is, not only may the initial observation be too close to the minimum wage change, but the post-treatment observation may be too close as well. In all three studies mentioned in the preceding paragraph, the second data point comes less than a year after the relevant minimum wage increase. As we noted earlier in this review, there is substantial empirical evidence that the disemployment effect of an increase in the minimum wage may occur with a lag one year or more, suggesting that these case studies may understate the full effect of a minimum wage hike. For the same reason, both Brown and Freeman, in their reviews of *Myth and Measurement*, speculate 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).

Fourth, some observers have questioned the reliability of the data used in these case studies. This obviously is less of a concern for Card (1992b), who used publicly available data from the Current Population Survey. 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) notes that these authors provide little documentation about the survey methodology or data collection process, and he expresses 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 document what seems to us to be an unusually high degree of volatility in the employment changes measured with Card and Krueger's data.

In light of these concerns, a number of researchers subsequently reexamined the results reported in the initial round of state-specific studies. Among the first of these reassessments was a paper by Taeil Kim and Lowell 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 that for the United States as a whole were negatively correlated with differences across industries in California-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 1988-89 period were more likely to be driven by the exogenous minimum wage increase in California in July 1988, the coefficient on the wage variable can be interpreted as a labor demand elasticity, which they estimated to −0.9. The authors conduct a similar analysis that relates county-level employment growth to county-level wage growth and again find a negative and statistically significant coefficient on wage changes only for the 1988-89 period, with an estimated demand elasticity of −0.7.[25]

As Card and Krueger (1995)—and later Kennan (1995)—pointed out, the main 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 variations in the average number of hours included in the pay period.[26] 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, as expected, 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 address the first point by showing that there is a negative correlation as well in the 1989-90 change (although in our view, this could also be viewed as evidence of a lagged effect from the 1988 increase in the minimum wage). In addition, they point out that the significant negative coefficient in the IV estimates relies on the inclusion of average firm size as an instrument, which they argue is inappropriate.

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 from a sample of similar fast-food establishments with which to compare the properties of the survey data. As suggested above, Card and Krueger's survey data exhibited considerably more variability in employment change than did the payroll data, with the pattern of variability indicative of severe measurement error. The Card-Krueger data were elicited from a survey that asked managers or assistant managers "How

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[25] As the authors point out, because they use 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.

[26] Although not especially relevant to the criticisms of the data discussed here, there seems to be some confusion in this literature about the reference period for employment. Kim and Taylor claim that it is employment during the last week of the quarter, while Card and Krueger (and Kennan) claim that it refers to employment as of March 31. In fact, the employment figures are supposed to include all employees who are on an establishment's payroll at any time during the pay period that includes March 12.

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 and suggested that, indeed, one measurement might sometimes be employment on a shift, with the subsequent measurement employment in a payroll period.

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 Kruegers' 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 that in 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 report results from a separate longitudinal sample of fast-food restaurants obtained from BLS records. In contrast both to their original study and to our replication, their reanalysis generally finds small and statistically insignificant effects of the increase in New Jersey's minimum wage on employment, and they conclude that "The increase in New Jersey's minimum wage probably had no effect on total employment in New Jersey's fast-food industry, and possibly had a small positive effect" (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 natural 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).

In this regard, it is also worth noting that the neoclassical model of the labor market does not predict that employment in every sub-sector of the economy will decline in response to a minimum wage increase, and thus 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) emphasizes, the effect on employment for any particular subgroup depends on relative factor intensities. For example, if fast-food restaurant chains are less intensive in low-wage labor than are their competitors (e.g., 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.

Hours vs. 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 per se and suggest that one potential reason for a small employment effect is that employers can also adjust the number of hours worked by their employees.[27] Michl (1996, 2000) speculated that the difference between the results we and Card and Krueger report with respect to New Jersey's minimum increase reflects the fact that Card and Krueger essentially measure employment, while we measure total hours.[28] To test this hypothesis, he compares the changes in employment and total hours in a subset of 52 observations from our payroll dataset 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 examines the ratio of full-time workers to total

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[27] As Couch and Wittenburg (2001) note, the use of employment rather than total hours could either understate or overstate the effect of the minimum wage of 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.

[28] As noted above, Card and Krueger attempt 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.

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 dismisses this result as irrelevant on the grounds that most fast-food workers are part-time employees and that reductions in their average hours worked could more than offset the larger 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 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.[29] 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 likely most relevant both for testing the validity of the competitive model of labor demand and for the policy question of how minimum wages affect opportunities for work.

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) investigate the effects of minimum wage changes on average hours in the state-level panel data framework described earlier. Zavodny uses the standard specification described in equation (1) above and includes both state and year effects in her analysis, which uses data from 1979-1993. 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 finds a small and insignificant effect of the

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

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 effect on total hours worked (e.g., unconditioned 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 augments these state-level results with an analysis of matched CPS individual-level data from 1979-80 to 1992-93. In this analysis, she identifies affected teenagers as those with an initial wage between the old and new minimum wage (in real terms), and calculates 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 regresses 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 cautions that these estimates do not incorporate the effects of the minimum wage on transitions from nonemployment to employment. Similarly, they do not capture whether the minimum wage affects the likelihood that those who make this transition work part-time or full-time.

In sharp contrast to Zavodny, Couch and Wittenburg (2001) find that minimum wages reduce both employment and total hours worked by teenagers. These authors follow the model put forth by Burkhauser et al. (2000a), using monthly data from January 1979 through December 1992 and excluding time dummies 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 interpret these results as suggesting that employers respond to a minimum wage increase by reducing both teen employment and average hours of those teenagers who remain employed. As we cautioned earlier, we are reluctant to place too much weight on estimates from specifications that exclude time effects. Nevertheless, the difference in the results reported by Zavodny and by Couch and Wittenburg indicates that the question of how employers adjust average hours in response to a minimum wage increase is not yet resolved.

V. More Recent Evidence on Employment Effects on Less-Skilled U.S. Workers

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 raising their minimum wages above the federal level, studying the effects of minimum wages on employment has continued to be an active area of research. In addition, much of the recent literature on minimum wage effects has continued to look for ways to more effectively identify the economic consequences of minimum wage changes. In some cases, this research has attempted to sharpen 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.

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

We begin this section with a summary of 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. A recent study by Bernstein and Schmitt (2000) provides a typical example.[30] These authors compute changes in the average employment rates for teenagers and adults with less than a high school education over three overlapping periods: 1995-1996, 1995-1997, and 1995-1998. Following Card (1992a), they then regress these changes on the fraction of each group affected by the two federal minimum wage increases.[31] The increasingly longer differences are intended to capture more of the effects of the two federal increases, which took place in 1996 and 1997. 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 statistically significant) for the 1995-1996 period, and for the others it ranges from −0.1 to −0.4; these estimates fall by about half when the sample begins in 1994. [32] As noted above, this study also examines evidence on adults aged 20-54 with less than a high school education. Here, although the estimates for the 1995-1996 change are positive, those computed through 1997 or 1998 are negative, small (in terms of the elasticity), and

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[30] These researchers are from the Economic Policy Institute, which supports minimum wage increases. Despite the potential lack of objectivity from such organizations (including, as well, those opposing minimum wage increases), we discuss their research because it has sometimes been influential in the policy debate. For example, a different EPI study by Bernstein and Schmitt (1998), which is discussed below, is cited indirectly in the 1999 Economic Report of the President (p. 112), and such studies are frequently cited in Congressional hearings and in hearings in state legislatures (see, e.g., Sabia, 2006). For papers that are either circulating or published based on work that appeared earlier under the cover of advocacy organizations, we refer to the former, as these are the versions that either underwent or are likely to undergo peer review.

[31] 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.

[32] These results are consistent with those reported by Wessels (2006). In particular, Wessels regresses the log change in teenage employment in each state from the twelve-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 ten-percent level), somewhat larger than that reported by Bernstein and Schmitt.

insignificant. The authors also present results for different periods—e.g., beginning in 1996—and find further 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 sample 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 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, the analysis does not account for state-level variation in minimum 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 (see Neumark, 2001). 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 (1991-1997). 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.[33] For the first-year increase, the implied elasticity is about −0.61.[34] For females, the estimated positive employment effect is nearly twice as large (in absolute value).[35] 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), who found negative and significant effects for teenage males, teenage females, and teenage blacks (although those estimates were also quite large). Given this, 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 federal minimum wage increase. 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 who had written previous minimum wage studies and the answers they found (or "author effects"). As documented in Table 1 of the paper, perhaps the most pronounced tendency was for research by us to find negative employment effects (although not always) and for research by David Card and Alan 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. Although the journal's project would have been more valuable had more researchers involved in the minimum wage debate decided to participate, only one pre-specified research design was submitted and published.[36, 37]

[33] 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% employment rate for teenage males reported in Table 4.
[34] This calculation instead uses the estimate in Table 6, column 1.
[35] It is unclear why the authors do not report results for teenagers as a whole using this approach, as they did for their other analyses.
[36] Despite his submission of a paper, Neumark expressed initial reservations about the usefulness of this approach because it requires throwing away lots of information on previous minimum wage increases (2001, p. 124), paralleling some of the concerns we have about the Bernstein and Schmitt studies.

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, 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 dataset spans the period from October 1995 to December 1998, roughly one year before the first to one year after the second of the federal minimum wage increases in October 1996 and 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 far from 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 young adults (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 workers 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

[37] 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 −.14 to −.44, and centered on about −.3.

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 wage for young, unskilled workers, with the exception of teenagers; the absence of disemployment effects for teenagers parallels some of the research we discussed earlier, which suggested 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 using an extended sample period that includes the federal minimum wage increases in 1996 and 1997. In their baseline model, which includes the nominal minimum wage and nominal adult wage separately, they continue to find a negative effect of the minimum wage on teenage employment when year effects are excluded, although the elasticities are smaller than for the 1979-1992 period; moreover, the estimated elasticity is close to zero when year dummies are included in the specification. To isolate the effects of the 1996 and 1997 increases, they also include a specification that uses indicator variables to identify the effects of each of these minimum wage increases. In these specifications, the estimated elasticities are −.27 for the 1996 minimum wage increase and −.17 or 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, though, we have reservations about estimates based on this type of specification.

Studies Focused on Recent State Minimum Wage Increases

The last decade has witnessed an unprecedented number of states raising their minimum wages, generating substantial variation in state minimum wages. Although the federal minimum wage has remained at $5.15 per hour since 1997, as of August 2006, eighteen states and the District of Columbia had minimum wages that exceeded the federal wage floor, and more than a half dozen other states are currently considering increases or will implement them in 2007. 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 had previously often been relatively small.[38] 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 38.4 percent in 2005, and will climb above 50 percent with additional minimum wages set to take effect by January 2007 (based on CPS data).[39] Finally, many state minimum wages are currently quite high, including six states and the District of Columbia with minimum wages of at least $7. From a research perspective, this proliferation of and variation in state minimum wages provides an unparalleled opportunity.

Advocacy groups have circulated a few papers that examine the employment effects of some of these recent state increases in minimum wages. For example, another Economic Policy Institute study, by Chapman (2004), 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 percent and 120 percent of the state minimum wage in 2003.[40] Although the results suggest no relationship, it is unclear from the study why the proportion of the workforce earning near the minimum wage in 2003 would be expected to affect aggregate employment growth from an earlier year to 2003. 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 would expect a positive relationship between the low-wage share in 2003 and employment growth from 2000 to 2003. Moreover, most economists have focused on the effects of the minimum wage on the employment opportunities of the low-skilled workers 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.[41]

[38] For details on state minimum wages, see http://www.epinet.org/issueguides/minwage/table5.pdf.
[39] 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 Madison, San Francisco, and Santa Fe. For an up-to-date review of living wages and research on their effects, see Adams and Neumark (2005).
[40] This paper also discusses trends in specific states; we focus only on the regression analysis.
[41] See, e.g., www.epionline.org/oped_detail.cfm?oid=18.

Separately, 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 the 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 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 these 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. In this sense, 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.[42]

[42] 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.

Another example is a recent study by Sabia (2006), which was circulated by the Employment Policies Institute (which opposes minimum wages).[43] The main advantage of this study relative to that of the Fiscal Policy Institute is that it controls for other potential influences on employment, including the demographic characteristics of states' populations, aggregate state-level economic activity, and other factors. This study also uses 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 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, the results indicate statistically significant employment declines, with estimated elasticities of about −0.10. Sabia reports a larger range, up to −0.3, but this larger elasticity only results when the year effects are omitted. For overall 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.[44] 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

[43] Reflecting the contentious debate between those partisan "think tanks" that support minimum wage increases and those that oppose minimum wage increases, Sabia's study is presented as a rebuttal of the Fiscal Policy Institute study. However, because it presents a more serious empirical analysis than many such analyses, we discuss it in more detail.

[44] Sabia also present some results for overall teen employment. However, these findings are very similar to those in Burkhauser et al. (2000a and 2000b) and thus are not discussed here.

a minimum wage increase (keeping in mind the reservations we noted above 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. The elasticities for hours worked by teenagers in this sector are a bit above the upper end of this range, and lower when computed only for working teenagers. For teen employment and hours 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 towards 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.[45]

Additional Case Studies

A couple of additional studies have adopted the case study approach pioneered by Card, Krueger, and others. Orazem and Mattila (2002) examined the effects of a series of minimum wage increases that took place in Iowa beginning in 1990.[46] The authors begin with a county-level analysis of low-wage industries in the state using data from the QCEW program, the same dataset 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 dataset, the authors supplement these data with information on average hourly

[45] Of course, we are sometimes more interested in whether minimum wages reduce employment of the least-skilled workers. However, in this case, we might learn more about the implications of an increase in the minimum wage on 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 few substitution possibilities.

[46] Iowa first established a minimum wage of $3.85 per hour in January 1990 and subsequently raised the minimum to $4.25 in January 1991 and to $4.65 in January 1992. The federal minimum in this period went from $3.35 to $3.80 in April of 1990 and to $4.25 in April of 1991.

earnings by industry from the BLS's Current Employment Survey.[47] 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 combine the effects on both workers directly affected by the minimum wage increases and other employees, the authors supplement these results by collecting the 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 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 (e.g. 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

[47] These data, as well as the unemployment insurance tax forms that are the basis for the QCEW data, are collected by state employment agencies—in this case, the Iowa Department of Workforce Development.

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.

Singell and Terborg (2006) examine the effects of minimum wages on the eating and drinking sector, and the hotel and lodging industry in Oregon and Washington. In particular, they use data from 1994-2001, 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 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.[48] 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

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

from, population and per capita income growth, calendar month dummies and interactions between these and a dummy variable for Oregon (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 do a good deal to establish the robustness of their results. The results for the eating and drinking sector consistently indicate that the minimum wage increases 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 any evidence that the timing of minimum wage increases fits this explanation. One avenue worth exploring is whether tourism spending changed differently in the two states over this period; we would imagine that hotel and lodging employment is much more sensitive to this than is eating and drinking employment. 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.[49] 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.[50] 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 two 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

[49] Personal communication with Larry Singell, July 6, 2006.
[50] Of course, these jobs are not necessary specific to an industry. For example, we presume that a want-ad may be advertising for waitpersons in a hotel restaurant.

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. [51]

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 fifteen 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):

$$E_t = \alpha MW_t + R_t\beta + \varepsilon_t \quad (2)$$

In this specification, *E* 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.[52] 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 −.05 to −.09 for teenagers and from zero to −.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 dataset that included more recent observations 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

[51] 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. There is also evidence paralleling the results on separations 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 that are negotiated via centralized bargaining. Thus, the implications of the results for more traditional uniform minimum wage floors are not clear.

[52] 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.

data through 1993 and reported elasticities for the Kaitz index that center on about −0.07 and that are generally not statistically significant.[53] Likewise, Bernstein and Schmitt (2000) report results from time-series analyses that extend the data through 2000:Q1. 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 −.061 and a t-statistic (1.63) just below the ten-percent level of significance. These authors also report estimates that augment the earlier modeling by differencing the data to account for nonstationarity and treating the seasonality differently than simply seasonal dummy variables; in addition, they report results using annual data. The elasticities from these alternative analyses range from −.001 to −.052, with four of the five estimates reported nowhere near statistically significant.

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 published time-series studies reflected "publication bias." Using meta-analysis techniques, 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 that corresponded to their theoretical priors, and that reviewers and editors were more likely to publish such studies, concluding that the earlier literature was "biased in the direction of finding statistically significant results" (p. 194).

However, in Neumark and Wascher (1998), we showed that successive estimates 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-

[53] 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 −.083 that is significant at the five-percent level, although this estimate is still below those based on data through 1979.

analysis. This finding points to parameter instability rather than publication bias as a 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 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-median wages), leading to a growing downward bias 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.

We should also note, however, that 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 nonstationarity 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-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 estimated 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 1954-93 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.

Finally, 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 misspecified, 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.[54] In addition, they include the minimum wage and average manufacturing earnings in the model along with the Kaitz index to relax the constraint imposed in the Kaitz index that the effects of changes in the minimum wage and the average wage should be equal but of 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.[55] In addition, although the coefficient on the Kaitz index is not statistically significant, the coefficients on both the minimum wage and average manufacturing wage are significant, and the restriction that the minimum wage and average wage enter with equal but opposite-signed effects is rejected. 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 −.12 in the short-run and −.27 in the long-run.

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

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

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 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.[56]

Efforts to Identify the Effects of the Minimum Wage on the Lowest-Wage Workers

Much of the literature discussed thus far has focused on the effects of the minimum wage on the aggregate employment rates of teenagers. 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.

[56] Two papers (Wolfson and Belman, 2001 and 2004) study time-series data at the industry level. We focus on the most recent paper. These studies are included in the table along with other time-series studies because the authors present their results as a general test for disemployment effects. We have two major criticisms of this work. First, as noted earlier, we do not know how to interpret evidence on minimum wages for narrow industries—the same issue we discussed earlier with respect to studying minimum wages for the fast-food industry only. Conventional theory does not predict that employment should fall in every industry in response to a minimum wage increase. Second, the authors report that there was no stronger evidence of disemployment effects for the subset of industries for which the minimum wage has a larger effect on average industry wages, which they suggest further undermines the view that minimum wage increases reduce employment. However, industries where wages increase the most may be those where it was least possible to substitute away from the low-wage labor whose price was increased, so that average wages went up relatively more because of the legislated minimum wage hikes. Alternatively, they may be industries in which there was rather strong substitution toward more-skilled labor, as opposed to other inputs, also raising average wages. In either case, the authors' test would not be informative.

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. For example, one can think of the minimum wage elasticity for the teenage group as a whole as a weighted average of the effect on workers directly affected by a change in the minimum wage and the effect on 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 because 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:

$$e^A = (e / p^A) \cdot (\Delta MW / \Delta W^A),$$

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 researchers have used to estimate minimum wage elasticities for individuals most likely to be affected by the minimum wage has been to try to narrow down the sample to groups more likely to work at minimum wage jobs.[57] 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 taken an alternative approach to estimating elasticities for minimum wage workers by attempting to identify observations for which the minimum wage increase is binding. 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

[57] 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.

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 movements along 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 nonbinding 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 nonbinding 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 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 −0.2 to −0.33, somewhat larger then 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.[58] In particular, these authors calculate a "wage gap" for each employed individual as the difference between the individual's wage in year t-1 and the minimum wage in year t for workers whose wage in year t-1 was between the old and new minimum wage, and zero otherwise. They then use a fixed-effects 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 by those increases. The results show clear signs of a negative and statistically significant disemployment effect even after controlling for other unobservable individual differences. Their

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

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* for individuals who were not employed prior to the minimum wage hikes.[59]

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.[60] Another concern, which seems more relevant to us, is Currie and Fallick's use of above-minimum wage workers as the control group in their study.

In a series of papers, Abowd et al. (1999, 2000a, 2000b) attempt to improve on this approach by respecifying the variable in real terms and attempting to identify individuals who were "freed" by a decline in the real minimum wage as well as those who were newly bound by a nominal increase in the wage floor. 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 variables used by Currie and Fallick (as well as Cards' "fraction affected" variable) except that it is expressed in real terms. 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

[59] The authors report that they attempted to identify nonemployed workers likely to be affected by the minimum wage by imputing their wage rates from a hedonic regression of 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.

[60] 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.

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 erosion of the minimum wage 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 mixed.[61] 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 negative elasticity of employment with respect to increases in the minimum wage. However, Abowd et al. (2000b) use data from 1981-1991, thus including both decreases and increases in the real value of the federal minimum wage, and found 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 Current Population Survey ORG files for the years 1979-1997 and extend the analysis to include 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. However, our incorporation of state changes in minimum wages allows us to address a criticism of the difference-in-differences studies that use higher-wage workers as a control group (e.g., Currie and Fallick; Abowd et al.)—that the experience and behavior of higher-wage workers may not provide a good counterfactual for what would happen to low-wage workers absent the minimum wage. In particular, analyses that condition on initial employment—which is needed to classify workers based on their initial wage—identify the effect of minimum wages on the probability of a transition from employment to non-employment, not on the probability of employment overall. If lower-wage workers are less stable, and regardless of the minimum wage are more likely to leave employment, then comparisons to higher-wage workers may not be picking up minimum wage effects.

[61] The results for France are discussed in Section VI.

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 so that the minimum wage effect can differ across the wage distribution. 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 the minimum wage and estimate the equation for wages, employment, hours (conditional on employment), and weekly labor income.[62] As in Currie and Fallick, we restrict the analysis to individuals employed in year t because we do not have an initial wage for those initially nonemployed. The model is estimated for several outcome variables—wages, employment, hours (conditional on employment), and weekly labor income.

Not surprisingly, the results indicate that workers whose wage is 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 −0.07 to −0.15 and are often statistically significant. The effect on hours is even more 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. Our results, which are based on samples that include both teenagers and adults (but are similar for adults only), are consistent with the findings for teenagers reported by Couch and Wittenburg (2001) discussed above and contrary to those reported by Zavodny (2000).

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

There has also been research along these lines using Canadian data. For example, Yuen (2003) looks for evidence of minimum wage effects using the Canadian Labor Market Activity Survey, which contains weekly longitudinal labor market data. However, since many important variables, such as 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. He excludes individuals from the Yukon or Northwest Territories due to missing control variables. 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 estimated employment effects in the recent literature. Yuen estimates models for the employment effects of minimum wages in two ways—first, by using high-wage workers from the same province as well as 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 where there is 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 $0.25 above.

Like the papers just discussed, Yuen limits his sample to those who are initially employed and therefore only addresses a possible transition 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, finding 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.[63] 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. Estimates using only the low-wage control group, however, lead to employment effects that are insignificant and near zero for both teens and young adults. These finding contrast with those in Neumark et al. (2004), which also uses workers in other 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 over the study. The author motivates this distinction by arguing that "transitory" low-wage workers more 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" 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" 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,

[63] The authors provide an average minimum wage increase (8.4%), which is used to calculate the elasticities. This average is calculated as the average percentage increase across all 19 provinces weighted by the number of at-risk individuals at the time of each provincial increase.

and in that sense the negative employment results for the permanent low-wage workers are potentially significant from a policy perspective.[64]

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 Labour and Income Dynamics to examine the effect of provincial changes in minimum wage on the transitions from employment to nonemployment 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-1 and year t 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 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 addition 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 (ranging from elasticities of −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

[64] Moreover, the different results for more 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 more permanent low-wage workers are likely to be in low-income families.

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.[65] Using SIPP data, she finds that the probability that a minimum wage worker (defined as 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 increases. She concludes that "the low-educated minimum wage workers benefit proportionally more than the high-educated" (p. 17) But 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 increase and their wages are bid up. Indeed her difference-in-difference 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.[66]

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[65] 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 towards teenagers and students, who may well be more skilled than adults at minimum wage jobs.

[66] 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.

Other Issues and Approaches

We close by highlighting a few new avenues for research on the employment effects of minimum wages. First, relatively recent changes in the low-wage labor market may have complicated the analysis of the minimum wage. In particular, the expansion of the earned-income tax credit during the 1990s provided many workers from low-income families with additional monetary incentives to work, while the welfare reforms enacted in the mid-1990s included a number of work-requirement provisions. Because these developments often coincided with minimum wage increases, disentangling their effects can be difficult. Moreover, such policy-related changes in the low-wage work environment may have changed the way in which minimum wages influence employment of low-wage workers with particular characteristics. That is, there may be important policy interactions between minimum wages and other policies affecting the low-wage labor market. We view this as a promising research agenda that has, as of yet, received little attention.[67]

Second, there is a budding literature on the effects of minimum wages in structural equilibrium search models; prominent examples include van den Berg and Ridder (1998), Flinn (2006), and Ahn et al. (2005).[68] 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" (i.e., 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

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Also, although not the focus of her paper, Connolly also 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.

[67] See Neumark and Wascher (in progress).

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

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 it, 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 latter types of studies, and it is a sufficiently daunting challenge to summarize and synthesize this literature, and in part because we find the results from this approach more defensible, while recognizing the limitations of what this approach can tell us. We also feel that, given the types of results described above, the search 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. Finally, to be honest, we know our comparative advantage and disadvantage. Clearly researchers better versed in these methods can provide a much more informative discussion of the alternative types of results one gets from these models, the key assumptions underlying the results, and the evidence—to the extent that it exists—on their validity.

We are struck, however, by the extent to which at least some of this work appears to be primarily motivated by a sense that the new minimum wage research establishes positive employment effects of minimum wages and that this evidence seriously undermines the standard competitive model (which is not based on an equilibrium search framework). For example, Flinn describes the recent evidence as “indicating that the “textbook” competitive model of the labor market, which has been used as an interpretive framework for the bulk of the empirical work performed using aggregated time series data, may have serious deficiencies in accounting for minimum wage effects on labor market outcomes when confronted with disaggregated information” (2006, pp. 1013-4). 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.

Third, some very recent research has focused on testing for monopsony via evidence on prices, which is an indirect approach to exploring whether minimum wages could lead to employment increases. In particular, Aaronson et al. (2005) and Aaronson and French (forthcoming) consider such evidence for the restaurant industry, for which some evidence suggesting positive employment effects has prompted some researchers to suggest monopsony explanations. Aaronson et al. (2005) present evidence that minimum wage increases lead to price increases. In the standard competitive model, minimum wage increases cause prices to rise and employment to fall. The authors consider alternative monopsony models as well as efficiency wage models that have a similar flavor (e.g., Rebitzer and Taylor, 1995), and show that these models generally imply that prices either fall or do not change in response to a minimum wage increase, if employment rises. One exception is in monopsony models where there is a good deal of firm exit in response to a minimum wage increase, in which case prices and employment can rise. However, they discount this possibility based on what they claim are small observed exit rates in this sector, although they do not present direct evidence on exit or on how much would be needed to overturn their result.[69]

Based on their findings, Aaronson and French (forthcoming) 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—few employers increase employment in response to the minimum wage. This results in only slightly smaller employment elasticities. Together, they suggest that their results provide evidence against the view that

[69] Another exception, which they do not address, is Wessels (1997), who 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 wage for relatively low levels of the minimum wage.

monopsony power can explain findings of employment effects near zero in studies of the restaurant industry. This, in turn, either casts further doubt on such findings, or, if such findings are correct, suggests that the model is incorrect.

Of course, the standard monopsony model implies that if the minimum wage is set high enough we will see higher prices and reduced employment, just like in the competitive model. In this case, however, the monopsony model cannot be used to explain apparent evidence of 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 rejecting arguments that monopsony power in the restaurant industry can explain zero or positive effects of minimum wages on employment. Of course, as the earlier discussion suggest, the evidence for the restaurant industry is, on balance, more consistent with negative effects of minimum wages on employment.

VI. International Evidence

The international evidence on minimum wages is large and growing, and covers 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.[70] Our review of the international evidence may therefore provide a less reliable description of the "distribution" of estimated employment effects of minimum wages across studies. In this section, we begin with a review of the evidence for the industrialized countries, and then turn to studies of developing countries.[71] As we have already discussed the available evidence for Canada and Sweden, we do not repeat that material here, although we do include the findings in the summary table.

Industrialized Countries

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[70] 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.

[71] There is also some emerging work on the effects of the minimum wage in transition economies (e.g., Eriksson and Pytlikova, 2004), which we do not cover in this survey.

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 across time within a country.[72] The study begins by summarizing minimum wage setting and levels in OECD countries that have a national minimum wage.[73] The authors then construct a measure of the relative minimum wage by dividing the nominal minimum wage by median earnings of full-time workers.[74] This ratio, which varied in 1997 from 0.36 in Spain to 0.69 in France, is used as the minimum wage variable in a set of pooled regressions using data for seven or nine countries from 1975 to 1996.[75] 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 −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 showing no effect of the minimum wage on their employment.

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[72] See also Hamermesh (2002).
[73] Bazen (2000) also provides details on minimum wage setting in various OECD countries.
[74] For a few countries, median earnings are not available
[75] 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.

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 the 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 (seventeen) of the OECD countries (Neumark and Wascher, 2004), taking account of variation in a variety of labor market policies and institutions. 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. But it also increases the 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 the employment model, 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. However, 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-1970's through about

2000.[76] 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.

The models are then augmented to control for institutional differences in other characteristics of 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 possibility that a higher minimum wage might actually induce substitution toward young workers in such cases. 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 (e.g., legislated working time rules, worker representation rights, and restrictions on the use of contract workers), the strength of employment protection regulations, the use of active labor market policies by the government, union density, and the generosity of unemployment insurance. In accordance with expectations, minimum wages have more adverse effects when labor standards are more restrictive, presumably because in the presence of rigidities more adjustment comes through employment (although the differences are typically not significant). Conversely, there is quite strong evidence that

[76] The precise years as well as the age ranges vary slightly across countries.

when employment protection is high, the disemployment effects of minimum wages are muted.[77] 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.[78]

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 the 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 clearly 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.

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

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[77] Dolado et al. (1996) also discuss some of these issues, pointing out, for example, that with higher firing costs, adjustments of employment to minimum wages may be smaller or slower.

[78] 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.

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 a number of specific European countries. Because most European 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, including new evidence that Dolado et al. report.[79] We review 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).

United Kingdom. The United Kingdom experienced two policy changes that provide potentially useful variation for identifying the effects of minimum wages. Prior to the early 1990's the United Kingdom had a system of Wages Councils, which consisted of equal numbers of employer and worker representatives as well as 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 U.K. Subsequently, in 1999, a national minimum wage was introduced.

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[79] The paper appears to present analyses for a number of countries each conducted by a subset of the paper's eight authors.

In their 1994 paper and a later paper co-authored with Richard Dickens (Dickens et al., 1999), Stephen Machin and Alan Manning study the period from 1979-1992, during which minimum wages declined relative to average wages in the eighteen industries covered by the Wages Councils.[80] These authors first establish that 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 questions 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 when conditions in the industry are (or are projected to be) good and not when conditions are bad, 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 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 be 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).

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[80] 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.

The Dickens et al. paper acknowledges the endogeneity problem. 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 Machin and Manning's 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 towards a monopsony interpretation of their findings, presumably based on their significant positive estimates, the specifications that show the largest positive effects are also 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 the main 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.[81] However, their reduced-form estimates, which are generally smaller and not statistically significant, strike us as more meaningful.

Moreover, even taking their positive estimates at face value, it is not apparent to us that the evidence favors the monopsony model. As we have documented, a number of papers that report zero or

[81] We are skeptical of this approach in dynamic panel data models because the identifying assumptions are conditional on the exclusion of lagged values from the equation of interest, yet theory provides little guidance in specifying the appropriate lag length of the underlying model. The same issue arises when they instrument for minimum wages with lags. They note that there is reason—as discussed above—to believe that minimum wages are driven by past minimum wages. However, that does not imply that lagged minimum wages can be excluded from the employment equation.

positive effects of minimum wages appeal to monopsony in a general way. However, putting aside questions about the empirical methods used in some of these papers, non-negative estimates can arise by statistical chance, and even in a competitive setting, an increase in the minimum wage can raise employment in a specific industry. As a result, we think that more convincing evidence in favor of the monopsony model requires the researcher to tie more explicitly the variation in minimum wage effects to monopsony power—i.e., 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, forthcoming). 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.[82]

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 pronounced. As the authors point out, however, employers may have been reluctant to cut nominal wages for their current employees, and

[82] As noted above, we presented some 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 that the monopsony model, in general, does not fit the data significantly better than a competitive model.

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. Using data three quarters prior to the abolition of the Wages Councils and two quarters afterwards, they conclude that “There is no noticeable change in the behaviour 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 (Table 10). Moreover, a similar conclusion 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 rose sharply to 3.67 percentage points, while the difference in exit rates rose only negligibly to 2.88 percentage points (Table 11). Because the difference in hiring rates increased by more than the difference in exit rates, a simple difference-in-difference 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 (as well as the employment data by industry).

Finally, a number of recent studies have examined the effects of minimum wages using the variation associated with 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.[83] 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. The authors then 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.[84] 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, 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

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

[84] 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.

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. For example, Stewart (2002) follows the approach taken by Card (1992a) 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 increased more following the introduction of the minimum wage in areas that had a larger share of workers paid below the new minimum.[85] Stewart then uses a "fraction affected" variable to test for the presence of employment effects. He also presents results from a difference-in-difference 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 estimate in the first row of Table 3 indicates that the difference-in-difference employment effect between high- and low-impact areas is a 0.02 reduction in the employment rate, while the wage estimates indicate that wages at the 5th and 10th percentile rose about five percent following the introduction of the minimum wage. If we treat this wage increase as the effective increase in the minimum, then the implied elasticity is −0.4.[86] The fact that an estimate this large is not detectable as statistically significant suggests that the data Stewart uses, coupled with his research design, is uninformative.

[85] Because there are relatively small samples of workers 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 did not emerge 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 national minimum wage being imposed.

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

Stewart also estimates employment models for a variety of low-wage groups of workers (women, those with less tenure, less-skilled occupations, etc.), 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.[87] This short-term focus is not different from many other studies, but given the evidence for 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 (2004a) 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 with the change in transition probabilities for workers who were initially paid just above the new minimum wage; the analysis of multiple periods is needed because lower-wage workers may generally have higher transitions to non-employment for reasons unrelated to the 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 these with year effects, in contrast to what one can do when there is regional minimum wage variation.

The empirical analysis uses three different data sources. Stewart first establishes, using the same empirical framework, that the introduction of the minimum wage boosted wages of those whose wages were initially below the minimum. Turning to the results for employment, he consistently finds small and

[87] 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.

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 eleven months beyond it, raising questions about whether the sample period is too short to adequately pick up the full impact of the minimum wage.[88] Of course, as we emphasized earlier, the papers that use this approach fail to provide evidence on the effects of minimum wages on transitions *into* employment, and hence on the overall employment effects of the minimum wage.

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 the 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 (2004a), 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.[89] However, the evidence of lagged minimum wage effects on hours raises the question of whether there might also be lagged effects on

[88] In a brief note, Stewart (2004b) 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 may 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.

[89] 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 three to seven percent. Using our earlier calculation that places the effective minimum wage increase at about five percent, we get implied elasticities ranging from about −0.6 to roughly −1.4.

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.

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. The authors employ a very similar empirical approach to Stewart (and Swaffield) and use many of the same datasets (although they include a slightly longer time frame). Their results are mixed. 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 introduced, but negative effects two and three years after the minimum wage was introduced; these results more closely resemble those reported by Stewart and Swaffield. The source of the differences between the estimates from the two datasets 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).

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 either has no effect on, or raises, 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 for 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, especially with regard to the national minimum. 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 bias in those estimates and because of the focus of this research on narrow industries. And 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 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.

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.[90] 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

[90] 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.

given to the potential endogeneity of such increases, absent a more complete solution to the endogeneity problem.

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 0.014 to −0.805; in addition, the four largest estimates (ranging from −.381 to −.805) are statistically significant.[91] Leigh also estimates a pooled model that combines these observations and finds an overall employment elasticity of −0.29, which is again statistically significant. He also estimates models that disaggregate by age and sex, to try to isolate those more bound by the minimum wage. When he disaggregates by age, the estimated elasticity is −1.01 for 15-24 year-olds and ranges from −.032 to −.141 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 (−1.426) than for young men (−0.681), although both are statistically significant. The estimated elasticity for 25-34 year-old men is also sizable (−0.238) and significant, while the estimates for older ages are generally not significant or positive. The elasticities that Leigh reports for aggregate employment are quite large relative to those found for other industrialized countries, especially given his estimate that only about four 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, at least, might be regarded skeptically.

A report by Harding and Harding (2004) estimates the effects of states' increase of their minimum wage 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

[91] 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 he 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).

a short-run elasticity for minimum wage workers of −0.2. However, it is unclear whether we should view employers' responses regarding the employment change due to minimum wage increases as reliable.[92]

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.

Maloney (1995) reports estimates from relatively standard time-series specifications for teens and young adults. He uses time-series data from the quarterly Household Labour Force Survey, a CPS-style dataset, for the period 1985 through 1993. This short sample period is necessitated by the differences in minimum wage setting in earlier years, although it may limit the confidence with which we can infer the effects of minimum wages, even aside from the usual problems of time-series analysis of this question.

[92] 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 that of employment, with estimates that are generally significant. However, this study is quite sketchy on the details, with little or no detail 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 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.

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 towards 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 he never presents comparable results for the more-educated young adults).

Although these results are quite consistent with expectations from the standard competitive model, results in a subsequent paper (Maloney, 1997) are somewhat less so. In this paper, Maloney extends the dataset 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. 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 inexplicably 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.[93] It also is 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 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. Unfortunately, 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

[93] 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.

from aggregate minimum wage changes. Finally, 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 estimates indicate significant negative employment effects, with elasticities of −0.06 to −0.10, which seem rather large given that these are for the entire industry rather than for just unskilled workers. 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. As it turns out, though, in this case the estimated coefficients of the minimum wage and the price deflator are both negative, rather than the equal and opposite-signed effects we might expect if the minimum wage and an average wage were entered into the equation. In addition, the estimates are extremely sensitive to splitting the sample into two periods. For example, for the latter half of the sample, the elasticity of industry employment to the minimum wage is −0.48 in one specification and 0.66 in another. Finally, Chapple also estimates time-series models industry by 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, the time-series evidence parallels Maloney's, as Chapple acknowledges, and the industry-level estimates are not robust, although in the basic pooled model he does obtain negative estimates. 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 (2004) 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 two and four percent, respectively, in these two years).

They also use the Household Labour Force survey, with data covering the 1997-2003 period, and conclude that the minimum wage had no effect or a positive effect on teen employment.

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 1997-2001:Q1 to 2001:Q2-2003:Q3. The simple difference-in-differences one can compute 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 reduces employment.[94] However, it is not clear why one would necessarily want to average over these relatively long periods. First, the minimum wage increase took place in March 2001, but was announced nearly a year in advance. Thus, at a minimum, one might want to cut off the "pre" period somewhat earlier. Second, 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. 15). However, although short-terms responses may be misleading, 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 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 are positive in almost every case, the estimated changes for 20-21 year-olds are almost always larger, and sometimes considerably so. For example, in 2001, the

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

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 thing researchers allow for in a triple-differenced estimate), these results are perfectly consistent with what the standard model would predict. Of course, part of the 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. Thus, in the end, we view the evidence for New Zealand as more consistent with negative employment effects of minimum wages.

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. present evidence from two analyses. First, they compare wage and employment changes over two periods—1981-1985 (when minimum wages rose sharply) and 1985-1989 (when it did not). In particular, they estimate the changes over these periods in hourly wages and 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. Despite this outcome, they then estimate similar regressions for the change in employment. Perhaps surprisingly given the wage results, the results suggest that the unemployment rate rose more for groups with a greater proportion of workers paid at or below the SMIC 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 less convincing, but go in the same 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 1967-1992.[95] 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 positive relationship between regional wage growth and the level of the initial wage during the period in which the relative minimum wage was rising, consistent with the rise in the SMIC boosting wages more in low-wage regions. However, a regression of the change in employment on the initial wage 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 aggregate 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 workers along any particular dimension, and as for the United States, the effects of the minimum wage may be difficult to detect with respect to aggregate employment.

In contrast, Bazen and Skourias (1997) study the effects of the 1981-1984 increases in the French minimum wage on youth employment (15-24 year-olds). In particular, they split their sample into 32 sectors, and calculate the ratio of youth employment to total employment in each sector and the

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

proportion of workers in the sector who were below the new 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 increase 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 those 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, 2000b), 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 (1982-89), they 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. 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 at −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, echoing issues we raised earlier, the "answer" about the effects of minimum wages is quite different when one focuses on directly-affected workers rather than on a broader group.

The evidence for France regarding overall employment effects of minimum wages on young, 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. paper, on those under age 25, appear to confirm this. At the same, however, the results reported by Abowd et al, point to disemployment effects of the minimum wage among low-skilled workers less protected by these institutions, and the evidence from France, on balance, is consistent with disemployment effects. [96]

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 and a reduction in the nominal adult minimum wage in 1984. Dolado et al. (1996) study the effects of the declines in youth subminimums, which, for example, fell from 77.5 percent to 61.5 percent of the adult minimum wage for 20 year-olds and from 47.5 percent to 34.5 percent of the adult minimum wage for 16 year-olds).[97] The authors first verify that relative wages of young workers declined following this change, indicating that the youth subminimum wages were a

[96] Kramarz and Philippon (2001) extend this analysis to study the effects of the combination of minimum wages and other labor costs, with a focus on payroll taxes and exemptions. 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-Wise approach to estimating the employment effects of minimum wages (discussed below).

[97] 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 note 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 the severe recession in the Netherlands at that time may have adversely affected youth employment disproportionately, 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 two percentage points in one, remained constant in one, and rose by two and four 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." However, in all four industries the youth share in employment rose both relative to the overall youth share of employment (which fell by three 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 among less-skilled workers.[98]

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 gradually 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

[98] There are also some papers studying the effects of the minimum wage on employment in the Netherlands using either structural search models (Koning et al., 1994) 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 resulting from this approach are quite sensitive to assumptions about the distribution of wages and the wage above which the minimum has no effect.

sector-specific changes in employment and youth employment (16-19 year-olds) on changes in the relative minimum wage, cyclical controls, and fixed sector effects.[99] The results show a positive and statistically significant elasticity of 0.08 for total employment, but a negative elasticity of −0.154 for youths (significant at the ten-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 16-19 year-old workers after the increase in the minimum wage for 16-17 year-olds. The same relationship does not appear for 20-24 year-olds (indeed it 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 youth employment.[100]

<u>Portugal</u>. 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 event to study the effects of the

[99] 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.

[100] 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 in which positive effects are possible, we would presumably expect such effects to show up most strongly for workers directly affected by the minimum wage. In contrast, the evidence for Spain points to negative employment effects for the most affected workers.

minimum wage on teenage employment in Portugal in 1987.[101] In particular, she uses a firm-level panel dataset for the period 1986-1989, with observations pertaining to March of each year. From this dataset, 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 for the effects of minimum wages on employment. First, she compares changes in employment (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; this approach identifies 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.[102]

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

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[101] 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 this age range 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).

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

from the most-affected firms. In addition, the evidence 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.

Curiously, Portugal and Cardoso (2006) study the same minimum wage change but reach a different conclusion. Using the same data set, although sometimes a different sample, their aggregate employment figures (i.e., without any regression controls) indicate faster employment growth for teenagers relative to older groups of workers, except for the comparison with those in their early 20's in the year encompassing the minimum wage increase.[103] Unfortunately, they do not adequately reconcile the differences. Although they claim that Pereira's sample is nonrandom and "severely biased with respect to the actual trend in employment for the affected group of workers" (p. 995), they offer no explanation or evidence regarding this claim. Moreover, they note that even when they tried to replicate Pereira's sample, they found opposite results from hers, which suggests that the use of different samples is not the explanation for the difference in results. A potential difference they do not explore is the inclusion of control variables in Pereira's analysis. Thus, in the end, we are left with an incomplete analysis that does not establish which results are correct. At this point, however, the evidence on employment effects from this particular episode in Portugal has to be viewed as inconclusive.[104]

Greece. The only evidence we have uncovered for Greece comes from a time-series study that uses annual data from 1974-2001 (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.

[103] They also present employment figures from another household level dataset, but report only that employment of teenagers grew (and unemployment fell) from 1986 to 1989 rather than 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.

[104] Portugal and Cardoso then move on to the more ambitious aspect of their research, which is to examine how the underlying flows of worker separations and accessions and firm entry and exit responded to the minimum wage increase. Their main results are that the minimum wage appeared to lead to a reduction in the share of teens separating from their jobs, but also a reduction in the share of teens among newly-hired workers.

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, for young adults the estimated effect of the aggregate unemployment rate on young adult employment is positive and significant, which is suggestive of model misspecification. Third, the author does not really try to explain the extraordinarily large positive minimum wage effects for teenagers. Although he suggests that part of the explanation may be that teens and youths 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, which the author does not provide since the employment equation for each group contains only that group's minimum wage relative to an adult average; as noted above, there seems to be little variation in the two groups' relative minimum wages, so it may not be possible to test this conjecture. Finally, there are some institutional details left unexplained, such as "incentives to employers who particularly employ teenage trainee workers in certain periods of the year" (p. 61). 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.

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. 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. And fifth, in some countries the minimum wage also directly influences the value of other government benefits. 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 use that many of the complexities involved in thinking about the evidence for each country were better left to scholars of those particular countries' economies.[105] We therefore provide a much briefer overview of this evidence, more as a guide to the literature than as a critical review.

Latin America and the Caribbean

<u>Brazil</u>. 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 the anti-inflationary efforts in the 1980's and early 1990's, 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,

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

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 effect of the minimum wage on income, and then backs out the employment effects by comparing the estimated effects on income for a sample working at both observations and a sample that also includes those not working. 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.[106]

Sara Lemos has written a sequence of closely-related papers that use the same dataset as Fajnzylber, but with observations that extend through 2000. Lemos (2004) considers a variety of measures of the minimum wage, and presents strong evidence that increases in the minimum wage generally compressed wages over the 1982-2000 period. The increases generated by the minimum wage were largest at the bottom of the wage distribution, but the data also suggests that a rise in the minimum wage led to increases for wages up to about the median wage. Part of the effects higher up in the wage distribution may be attributable to the hyper-inflationary period when wages were often set at “multiples”

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

of the minimum wage (e.g., Fajnzylber, 2001). As evidence, similar regressions for the post-hyper-inflation period do not find effects of the minimum wage above the 30^{th} percentile (Neumark et al. 2006). Evidence of a spike at the minimum in the informal sector is also suggestive of these "numeraire" effects.

Based on these results, Lemos then estimates the effects of the minimum wage on employment and hours from a cross-section time-series panel of regional data. 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 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 effects on those 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 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 0.011). 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 compresses the wage distribution in both sectors (although the positive effects of minimum wages on wages in the informal sector reach higher up into the wage distribution), and no significant evidence of either positive or negative effects of the minimum wage on employment or hours in either sector; and in all cases the estimates are small.

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) workers 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 (e.g., 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 (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.[107] 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 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 dataset as Lemos, they first estimate an employment effect for household heads, yielding a significant elasticity of −0.07 when lags 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).

[107] 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.

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.[108]

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 that decade 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 datasets that allow her to focus on the 1980s period, when the divergence between the minimum wage 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 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

[108] 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.

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 studies and became more uniform across states and regions within states as well.[109] 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 estimate at the state level, using the average minimum wage for regions within a state in cases when there was minimum wage variation within states.[110] She finds no minimum wage effects for males, with estimated elasticities typically close to and centered on zero, and generally insignificant. The one exception is for males aged 55 to 64, for which she find a small but significant positive effect, which she suggests could reflect substitution towards these worker rather than direct effects of the minimum wage. For females, however, there is consistent evidence of disemployment effects for all age groups, with elasticities ranging from −0.41 to −0.76. Feliciano speculates that the 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 (Argentina, Bolivia, Brazil, Chile,

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

[110] She also presents estimates from a specification that instruments for the relative minimum wage variable with a real minimum wage variable (i.e., 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.

Colombia, Honduras, Mexico, and Uruguay). 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 dataset 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 the 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 of the minimum wage is often used as a numeraire for other wages (as discussed in the context of Brazil), 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 in some of the higher locations in the wage distribution, suggesting that the function of the minimum wage also causes employment losses, although it is difficult to understand why this wage rigidity would persist. 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 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.

Chile. Montenegro and Pagés (2004) estimate effects of job security provisions and minimum wages on the relative employment of different groups, using a time-series of cross-sectional data sets for

Santiago from 1960-1998. They estimate a model for employment at the individual level and include a set of variables to capture 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.[111] Of course 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, minimum wages appear to increase their employment relative to that for older males.

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

[111] 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.

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 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.

Trinidad and Tobago. Strobl and Walsh (2003) examine the effects of the introduction of a national minimum wage in April of 1998 in Trinidad and Tobago. They use a rotational household survey administered quarterly, which lets them construct short panels on individuals. Because their dataset is limited to the 1996-1998 period, they can estimate only relatively short-run effects (at most 8 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.[112] 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.

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 applied as in the mainland. 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 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 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 now has 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.

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[112] Given that there was no minimum wage initially, there is no meaningful way to convert this into an elasticity.

The authors use this variation to study the effects of the U.S. federal minimum wage in Puerto Rico. In particular, they 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.[113] 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) reexamined 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.

Indonesia

[113] 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.

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; and 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 converged during the early 1990s in response to legislation passed in 1989.[114] 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, on the idea 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.[115] 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

[114] 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).

[115] 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.

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 females, 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 insignificant and generally smaller for males, adults, youths, those with more education, blue-collar workers, and both full-time and part-time workers.[116] 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.[117]

Other studies for Indonesia attempt use more compelling information to identify the effects of minimum wages. Harrison and Scorse (2005), for example, 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.[118] 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.

[116] 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.

[117] 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 very standard to include some version of region and period effects, and it is well-understood that models without these effects may be misspecified. 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.

[118] Rama (2001) acknowledges this variation, but claims that "minimum wage variance across districts within the same province was low" (p. 866).

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 −0.118 to −0.184; the estimates are generally statistically significant and robust to a variety of specifications changes. Only in small firms were the estimated effects insignificant (and smaller), which the authors suggest 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 the 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 effects is negative overall, indicating significantly faster employment growth in Jakarta in this period, but it becomes insignificant when the control group is narrowed to a small strip just along the border (to hold economic conditions more similar) or when a higher-wage control group from Botabek is used. The authors conclude that the disemployment effects of the minimum wage increases in Indonesia were, overall, not very large. Other evidence reported in the paper suggests that 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 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.

VII. Conclusions and Discussion

What is likely most striking to the reader who has managed to wade through our lengthy review is the wide range of estimates of the effects of the minimum wage on employment, especially when compared to the review by Brown et al. in 1982. For example, few of the studies in BGK's 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 well below −1 to well above zero. This wider range for the United States undoubtedly reflects both the new sources of variation used to identify minimum wage effects—notably increased state-level variation in minimum wages—and the new approaches and methods used to perform the relevant hypothesis tests. 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.

This wide range of estimates makes it difficult for us to draw broad generalizations about the implications of the new minimum wage research. Clearly, no consensus now exists about the overall effects on low-wage employment of an increase in the minimum wage. However, the oft-stated assertion that this recent research fails to support the traditional view that the minimum wage reduces the employment of low-wage workers is clearly incorrect. The studies surveyed in this paper lead to 91 entries in our summary tables (in some cases covering more than one paper). Of these, by our reckoning nearly two-thirds give a relatively consistent (although by no means always statistically significant) indication of negative employment effects of minimum wages—where we sometimes focus on results for the least-skilled—and fewer than 10 give a relatively consistent indication of positive employment effects. In addition, we have highlighted in the tables 20 studies that we view as providing more credible evidence, and 16 (80 percent) of these point to negative employment effects. Correspondingly, we have indicated in our narrative review that, in our view, many of the studies that find zero or positive effects suffer from various shortcomings.

Moreover, as the summary table suggests, the evidence tends to point to disemployment effects of minimum wages for many other countries as well, either based on a single study or two, or what we view as the more compelling evidence when there are many studies. There are, of course, important exceptions—cases where a convincing study fails to find disemployment effects even when other studies for the same country find such evidence.

Finally, two potentially more important conclusions emerge from our review. First, we see very few—if any—cases where a study provides convincing evidence of positive employment effects of minimum wages, especially from studies that focus on broader groups (rather than a narrow industry) for which the competitive model predicts disemployment effects. Second, and related, when researchers focus on the least-skilled groups most likely to be adversely affected by minimum wages, we regard the evidence as relatively overwhelming that there are stronger disemployment effects for these groups, even if they are not always statistically significant (which we would not expect).

Some broader themes that emerge from our review also help to highlight some of the important considerations that economists and policymakers should keep in mind when assessing the empirical evidence from studies of the disemployment effects of minimum wages that—if recent experience is any guide—are likely to continue to be produced.

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. In contrast, the majority of the U.S. studies that have found zero or positive effects of the minimum wage on low-skill employment were either short panel data studies or case studies of a specific change in the minimum wage in a particular state. 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. Indeed, when they are considered, 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 "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 about the adequacy of the neoclassical model or about the broader implications of changes in either the federal minimum wage or state minimum wages.

Third, even aside from the estimates of the effects on the minimum wage on low-wage 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 this literature largely reflects 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, and, 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. As a result, 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. Thus, minimum wages may harm the 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. First, a question that is relevant to much of the literature is how to address the potential endogeneity of minimum wage policy 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 cross-sectional fixed effects, the researcher needs a set of instrumental variables that vary over time. Second, it will be useful to bring more direct evidence to bear on whether the monopsony model or the competitive model better characterizes the low-wage labor market. The evidence presented in much of the existing research is indirect in nature, and given the ambiguous predictions of the competitive model in specific circumstances, more structural approaches to this question may ultimately prove more informative. Third, 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 per se. A few studies have attempted to disentangle the implications for aggregate hours from those for employment, and the differences in results reported in rather similar studies suggests that this remains an area for further research. Finally, given the variation of estimates across studies, a systematic assessment of the sources of differences in the estimates across studies using meta-analysis techniques focused on this question (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 the end, our view is that the weight of the empirical evidence is generally consistent with the view of traditional economists that the low-wage labor market can be reasonably approximated by the neoclassical competitive model. As a result, we also believe that raising the minimum wage leads to economic distortions and often has unintended adverse consequences for the employment opportunities of low-skilled workers. Of course, as we have argued elsewhere, the effects 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, 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.

Of course, if minimum wages do not generate disemployment effects among low-skilled workers, it is far more likely that they have beneficial effects at the bottom of the income distribution. But given that the weight of the evidence points to disemployment effects, the wisdom of pursuing higher minimum wages hinges in potentially complex ways on the tradeoffs between the effects of minimum wages on different workers and other economic agents, and whether other policies present more favorable tradeoffs.

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Table 1: Studies of General Minimum Wage Employment Effects in the United States

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
Federal variation						
Card (1992a)	1990 federal minimum wage increase	Difference-in-differences; fraction affected	Teenagers	CPS, 1989-1990	−.06 to +.19; not significant	No allowance for lagged effects
Deere, Murphy, and Welch (1995)	1990 and 1991 federal minimum wage increases	Differences-in-differences	Teens and adult high school dropouts by sex and race	CPS, 1985-1992	Teens: −.27 to −.56 Adult high school dropouts: −.11 to −.33 Largest elasticities for blacks	Indicator variables may pick up other influences
Currie and Fallick (1996)	1980-81 increases in federal minimum wage	Wage gap	Workers with initial wage between old and new minimum	NLS, 1979-1987	−.19 to −.24; significant	Control group includes all workers above minimum wage
Bernstein and Schmitt (1998)	Federal	Difference-in-differences	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	Ignores state variation in minimum wages
Bernstein and Schmitt (1998)	1996 and 1997 federal minimum wage increases	Difference-in-differences (Deere, et al., specification)	Teens	CPS ORG's, 1993-1998	Male teens: −.45 to −.61; estimates sometimes significant Female teens: .32 to .86	Ignores state variation in minimum wages; differences from Deere, et al., results not explored
Abowd et al. (1999)	Change in real federal minimum wage	Real wage gap	Low-wage workers (ages 16-60) freed by decline in real minimum wage relative to those marginally above	Matched CPS, 1981-82 to 1986-87	Varies by age. Male average: −.42 Female average: −1.57 (Conditional on employment in t+1)	No actual change in minimum wage
Abowd et al. (2000a)	Change in real federal minimum wage	Real wage gap	Low-wage young workers (ages 16-30) freed by decline in real minimum wage relative to those marginally above	Matched CPS, 1981-82 to 1986-87	Varies by age. Male average: −2.23 Female average: −1.87 (Conditional on employment in t+1)	No actual change in minimum wage

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
Neumark (2001)	Cross-state variation generated by 1996 and 1997 federal minimum wage increases	Difference-in-differences, fraction affected and relative minimum wage specifications	Teens, young adults (16-24), non-enrolled young adults 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: −.02 to −.22; insignificant** **Non-enrolled, 16-24, high school or less: −.11 to −.53; significant** **Non-enrolled, 20-24, high school or less: −.09 to −.15; sometimes significant** **Non-enrolled, 16-24, less than high school: −.21; significant** **Non-enrolled, 20-24, less than high school: −.11 to −.12; insignificant**	Limited period and small number of minimum wage increases
Chapman (2004)	Cross-state variation in share between 100 and 120% of state minimum in 2003	Difference-in-differences	Total workforce	Payroll Survey	N/A; estimated effect of −.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	Simple comparisons of employment growth	Overall employment, and employment in retail, in small businesses, and in small retail businesses	Payroll Survey, County Business Patterns	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-difference estimates are centered on 0	Ignores variation in timing of state minimum wage increases; no controls for other factors affecting employment growth; simple difference-in-difference calculations undermine most evidence of positive effects
Aggregate Time-Series Studies						
Wellington (1991)	Federal	Kaitz Index	Teenagers and 20-24 year olds	1954-1986	Teenagers: −.052 to −.089 20-24 year olds: +.002 to −.017	
Card and Krueger (1995a)	Federal	Kaitz Index	Teenagers	1954-1993	−.050 to −.087; only a few significant	
Bernstein and Schmitt (2000)	Federal	Kaitz Index	Teenagers	Data from Card and Krueger (1995), 1954 -1993, updated through 2001:Q1	Teenagers: −.001 to −.061	

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
Williams and Mills (2001)	Federal	Kaitz Index, relative minimum wage, and real minimum wage	Teenagers	Data from Card and Krueger (1995), 1954-1993	Teenagers: −.3 to −.5 after two years	
Bazen and Marimoutou (2002)	Federal	Kaitz Index, and separate minimum wage and real wage	Teenagers	Data from Card and Krueger (1995), 1954-1993, updated through 1999:Q2	**Teenagers: −.11 (short-run), −.266 (long-run), significant, and similar estimates for subperiods**	
Aggregate Panel Data Studies						
Neumark and Wascher (1992)	Federal and state	Panel data analysis; Kaitz Index	Teenagers and young adults	CPS, 1973-1989	Teenagers: −.1 to −.2 Young adults: −.15 to −.2	For teens, significant negative effects only when enrollment is included. Enrollment rate too narrow
Williams (1993)	Federal and state	Panel data analysis; Relative minimum wage, difference coefficients in different regions	Teenagers	CPS, 1977-89	+.09 (New England) to −.62 (Pacific)	
Neumark and Wascher (1994)	Federal and state	Panel data analysis; Kaitz Index; IV for enrollment	Teenagers and 16-24 year olds	CPS, 1973-1989	**Teenagers: −.17 to −.25** **Youth: −.12 to −.16**	
Evans and Turner (1995)	Federal and state	Panel data analysis; Kaitz Index	Teenagers and 16-24 year olds	CPS, 1978-1989	Teens: −.09 Youth: −.04 Not significant	Mixes October employment data with May minimum wage data
Neumark and Wascher (1995a)	Federal and state	Conditional logit analysis; Kaitz Index	Teenagers	CPS, 1977-1989	Non-employed and enrolled: −.13 Employed and enrolled: −.40 Employed and not enrolled: +.28 Idle: +.64	
Neumark and Wascher (1995b)	Federal and state	Multinomial logit analysis; Relative minimum wage	Teenagers	Matched CPS, 1979-1992	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	

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
Neumark and Wascher (1996)	Federal and state	Panel data analysis; Kaitz Index	Teenagers and young adults	October CPS, 1980-1989	Teenagers: −.22 Young adults: −.14; significant	
Abowd et al. (2000b)	Change in real federal and state minimum wages	Real wage gap	Low-wage workers affected by a change in the real minimum wage relative to those marginally above them	Matched CPS, 1981-82 to 1990-91	Many results reported for exit and entry elasticities. Generally small (of both signs) and not significant	
Burkhauser et al. (2000a)	Federal and state	Panel data analysis; nominal minimum wage and indicator variables	Teenagers	CPS, 1979-1997; monthly data	1979-1997: −.2 to −.6 1996-1997: −.17 to −.27 Estimates generally significant	Excludes year effects
Burkhauser et al. (2000b)	Federal and state	Panel data analysis; nominal minimum wage	Teenagers and young adults, by race and educational attainment	CPS, 1979-1997; SIPP, 1990-1992; monthly data	Teens: −.3 to −.6 Youths: −.20 to −.25 Black youths: −.85 Nonblack youths: −.18 High school dropouts (20-24): −.85 High school grads (20-24): −.16	Excludes year effects
Zavodny (2000)	Federal and state	Panel data analysis; real or relative minimum wage; wage gap	Teenagers	CPS, 1979-1993 Matched CPS, 1979-80 to 1992-93	*Aggregate results* Employment: −.02 to −.12; Total hours: +.24 to −.11 *Individual results* Employment: −.08 to −.10 Total hours: positive but not significant	
Couch and Wittenburg (2001)	Federal and state	Panel data analysis; nominal minimum wage	Teenagers	CPS, 1979-1992	Employment: −.44 to −.58 Total hours: −.51 to −.59 Estimates significant	Excludes year effects
Turner and Demiralp (2001)	Federal minimum wage increase in April 1991	Multinomial logit analysis; Relative minimum wage; indicator for bound observations; wage gap	Teenagers by race and location (city vs. non-city)	CPS, Jan-Apr. 1991 to Jan-Apr. 1992	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	Panel data analysis; relative minimum wage; dynamic model	Aggregate and youth employment (not defined)	CPS, 1977-1995	Aggregate: −.11 (short-run); −.19 (long-run) Youths: −.37 (short-run); −.69 (long-run)	

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
Pabilonia (2002)	State	Cross-section probit analysis	14-16 year olds	NLSY97; data for 1996	Males: −.6 Females: −1.3 Some estimates significant	
Neumark and Wascher (2002)	Federal and state	Switching regressions with state-year panel	16-24 year olds in the binding regime	CPS; 1973-1989	−.13 to −.21; significant	
Neumark and Wascher (2003)	Federal and state	Conditional logit; update of Neumark and Wascher (1995a)	Teenagers	October CPS, 1980-1998	**Non-employed and enrolled: −.11** **Employed and enrolled: −.09** **Employed and not enrolled: +.41** **Idle: +.18**	
Neumark et al. (2004)	Federal and state	Difference-in-differences across state-year observations	Workers at different points in the wage distribution	Matched CPS, 1979-80 to 1996-97	**Employment: −.12 to −.17 for workers between 1 and 1.3 times the old minimum wage** **Hours: −.3 for workers between 1 and 1.3 times the old minimum wage**	
Bazen and Le Gallo (2006)	Federal and state	Panel data analysis; nominal minimum wage	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 test of lagged effects

The Bernstein and Schmitt papers also look at young adults aged 20-54 with less than a high school education, but the results are not reported here because they fail to find any effects of minimum wages on wages for this group. Results from studies we regard as more reliable tests of employment effects of minimum wages are highlighted.

Table 2: Studies of Minimum Wage Employment Effects in Specific Sectors in the United States

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
Studies of Specific Sectors						
Katz and Krueger (1992)	1991 federal minimum wage increase	Difference-in-differences. Minimum wage gap	Fast-food employment in Texas	Survey of restaurants in Dec. 1990 and July 1991	+1.7 to +2.65; significant	Potentially large amount of measurement error
Card (1992b)	1988 California minimum wage increase	Difference-in-differences	Teen employment and retail trade employment	CPS	Teens: +.15; significant Retail trade: −.04; not significant	Questionable control groups
Spriggs and Klein (1994)	1991 federal minimum wage increase	Difference-in-differences. Minimum wage gap	Fast-food employment in Mississippi and North Carolina	Survey of restaurants in March 1991 and April 1991	Estimates centered on zero, not significant	Potentially large amount of measurement error; short period over which to observe effects
Card and Krueger (1994)	1992 New Jersey minimum wage increase	Difference-in-differences. Minimum wage gap	Fast-food employment in New Jersey and Pennsylvania	Survey of restaurants in Feb. 1992 and Nov. 1992	FTEs: +.63 to +.73; some estimates significant	Potentially large amount of measurement error
Kim and Taylor (1995)	1988 California minimum wage increase	Difference-in-differences	Retail trade employment	QCEW	−.4 to −.9; some estimates significant	No direct measure of hourly wages
Partridge and Partridge (1999)	Federal and state	Panel data analysis; minimum wage level	Retail trade employment	CPS and BLS establishment survey; 1984-1989	Retail: −.08 to −.25 Eating and drinking: −.05 to −.2 Other retail: −.09 to −.26	
Neumark and Wascher (2000)	1992 New Jersey minimum wage increase	Difference-in-differences.	Fast-food employment in New Jersey and Pennsylvania	Payroll data collected from establishments	**FTEs: −.1 to −.25; some estimates significant**	
Card and Krueger (2000)	1992 New Jersey minimum wage increase	Difference-in-differences. Minimum wage gap	Fast-food employment in New Jersey and Pennsylvania	BLS establishment-level data	**FTEs: +.005 to +.15; not significant**	
Michl (2000)	1992 New Jersey minimum wage increase	Difference-in-differences.	Fast-food employment in New Jersey and Pennsylvania	Neumark-Wascher payroll data (subsample of observations reporting employment	Employment: +0.044 Total hours: −.018 Hours per worker: −.062	Small sample

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
Orazem and Mattila (2002)	1990-1992 increases in Iowa minimum wage	Minimum wage-gap	Retail and non-professional services	QCEW; Establishment UI records and author survey, 1989-1992	*County level* Employment: −.06 to −.12; mostly significant *Firm level* Employment: −.22 to −.85 Hours: −1.01 to −1.50 All estimates significant	
Sabia (2006)	State	Panel data analysis	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: −.09 to −.29 Share of 16-64 year-olds employed in small businesses: −.08 to −.12 Share of teens employed in retail sector: −.27 to −.43 Average retail hours worked by teens: −.53 Average retail hours worked by employed teens: −.05 to −.28 Share of teens employed in small businesses: −.46 to −.89 Average small business hours worked by teens: −.48 to −.88 Average small business hours worked by employed teens: −.54 to −.70 Teen employment: −.19 to −.33 Average hours worked by teens: −.37 to −.45 Average hours worked by employed teens: −.01 to −.29; almost all estimates significant	Focus on teen employment in low-wage sectors generates ambiguous results; declines imply neither overall declines in these sectors, nor overall decline in teen employment
Singell and Terborg (2006)	Oregon and Washington minimum wage increases at different times	Difference-in-differences	Eating and drinking workers; hotel and lodging workers	BLS monthly employment data, 1997-2001; help-wanted ads, 1994-2001	**Eating and drinking employment: −.2; significant** **Hotel and lodging employment: .15 to .16, significant** **Want-ads: negative and significant for all restaurant jobs except cooks, and for hotel housekeepers**	Hotel and lodging employment could be sensitive to variation in tourism; want-ad specifications different from industry employment specifications

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
Time-series Studies of Industries						
Wolfson and Belman (2001)	Federal	Time-series	Low-wage industries and industries employment large share of young adults	BLS payroll survey, various years through 1997	Preponderance of employment elasticities are negative, although more often than not insignificant; including minimum wage in time-series models generally does not improve one-step ahead employment forecasts, even in lower-wage industries or industries with larger share of young workers	Theory does not predict employment declines in all industries, or even all low-wage industries. Unclear why one-step ahead forecasts provide right criterion.
Wolfson and Belman (2004)	Federal	Time-series	Low-wage industries and industries employment large share of young adults	BLS payroll survey, various years through 1997	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 more evidence of disemployment effects in industries where minimum wages increased average wages more	Theory does not predict employment declines in all industries, or even all low-wage industries. Industries with larger wage increases may be those with less ability to substitute away from low-wage labor, or greater ability to substitute towards more-skilled labor, and in either case disemployment effects would be smaller, not larger.

Results from studies we regard as more reliable tests of employment effects of minimum wages within a sector are highlighted.

Table 3: Studies of Minimum Wage Effects on the Distribution of Employment in the United States

Study	Minimum wage variation	Method	Group	Data	Results	Criticisms
Lang and Kahn (1998)	State and federal	Difference-in-differences	Workers in food service occupations in eating and drinking establishments	CPS, 1988-1992	Shift in employment from adults to teenagers and students	
Connolly (2005)	State minimum wage increases	Differences-in-differences	High school dropouts and high school graduates	SIPP, 1986-1996	Increase in minimum raises share of minimum wage workers with less than high school degree, interpreted as substitution toward less-educated workers; but results for transitions indicate that minimum wage increases associated with relatively more transitions from minimum wage jobs to higher wage jobs for male high school graduates versus high school dropouts, and relatively fewer transitions from higher wage jobs into minimum wage jobs	Results do not establish substitution towards less-educated workers

Results from studies we regard as more reliable tests of employment effects of minimum wages for subset of workers are highlighted.

Table 4: International Studies of Minimum Wage Employment Effects, Industrialized Countries

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
Panel Studies						
OECD (1998)	Time-series variation across countries	Panel data analysis	Teens (15-19), young adults (20-24), and adults (25-54)	National sources	Teens: −.07 to −.41 20-24: −.03 to −.10 25-54: .0 to +.01 Estimates for teens mostly significant	
Neumark and Wascher (2004)	Time-series variation across countries	Panel data analysis	Teens (15-19) and youths (15-24)	OECD and various sources, mid-1970s through 2000 (varies by country)	**Employment** **Standard models: Teens, −.18 to −.24 to; youths, −.13 to −.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**	
United Kingdom						
Machin and Manning (1994); Dickens, et al. (1999)	Wages Councils	Panel data analysis	All workers in covered (low-wage) industries	New Earnings Survey, Employment Gazette, 1978-1992	.05 to .43	Change in institutional setting of minimum wages in 1986 is ignored; questions about exogeneity of minimum wages
Dolado, et al. (1996)	Abolition of Wages Councils	Difference-in-differences	Workers in Council and non-Council sectors	Quarterly Labour Force Survey Micro Data	Relative increases in hiring rate and employment in Council sector after Councils abolished (i.e., evidence consistent with disemployment effects of minimum wages)	Questions about exogeneity of minimum wage Increases chosen by Wages Councils, and hence of variation created by abolition of Wages Councils
Machin, et al. (1999)	Introduction of national minimum wage in 1999	Difference-in-differences	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: −.08 to −.38; for hours: −.15 to −.39**	

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
Stewart (2002)	Variation across local areas in effect of imposition of national minimum wage in 1999	Difference-in-differences	All workers and various lower skill groups	New Earnings Survey, 1998, 2000; Labour 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
Stewart (2004a)	Variation across workers at different points of the wage distribution	Difference-in-differences	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	Difference-in-differences	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
Canada						
Baker, et al. (1999)	Across provinces and over time	Panel data analysis, with attention to frequency domain	Teenagers (15-19)	Special tabulations from Statistics Canada, 1979-1993	**Within-group: −.267 (−.471 with one lag)** **First difference: .074 (−.227 with one lag)** **Second difference: −.127** **Third difference: −.306** **Fourth difference: −.398** **Within-group estimates and longer-difference estimates significant; similar result reflected in lower-frequency filters**	

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
Yuen (2003)	Across provinces and over time	Panel data analysis	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)	Provincial	Multinomial logit analysis; Relative minimum wage	Teenagers	Survey of Labour and Income Dynamics (1993-1999)	−0.12 to −0.50 No effects on schooling	
Campolieti, et al. (2005b)	Provincial	Difference-in-differences. Minimum sage gap	Youths (16-24)	Survey of Labour and Income Dynamics (1993-1999)	−0.33 to −0.54	
Campolieti, et al. (2006)	Provincial	Panel data analysis	16-19, 20-24, and 16-24 year-olds, including full-time vs. part-time and non-enrolled	April Labor Force Surveys, 1981-1997	**Teens: −.17 to −.44** **20-24 year-olds: −.14 to −.43** **16-24 year-olds: −.17 to −.44**	
Australia						
Leigh (2004a)	Minimum wage increases in Western Australia relatively to rest of country\	Difference-in-differences	Aggregate, and disaggregated by age and sex	Labour Force Survey, 1994-2001	Aggregate: −.25 to −.40 15-24: −1.01 15-24, male: −.68 15-24, female: −1.44 Older groups: −.03 to −.14 (mostly insignificant)	Very large elasticities for aggregate employment and for 15-24 year-olds
Harding and Harding (2004)	State minimum wage increases	Survey of employers	Minimum wage workers	2003	−0.2	Employer attributions of employment changes to minimum wage increases may not be reliable
Mangan and Johnston (1999)	State minimum wage differences (over time and cross-sectionally)	Panel data regressions, and cross-sectional model	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: −.21 to .08, almost all estimates negative, none significant Cross-section analysis: full-time, −.05 to −.31, generally significant	Source of minimum wage variation unclear; model should include non-teen minimum wage

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
Junankar et al. (2000)	Time-series variation in youth minimum wages	Time-series regressions by industry, age, and sex	16-20 year-olds	Quarterly data, 1987-1997 (source unspecified)	Effects insignificant and often positive, except for retail where there is some evidence of disemployment effects; elasticities 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 20 and over, and introduction of teen minimum wage	Time-series	Young adults (20-24) and teenagers (15-19)	Household Labour Force Survey (HLFS), 1985-1996	Effect of adult minimum: 20-24: −.1 to −.4 15-19: −.1 to .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
Chapple (1997)	National minimum wage for workers 20 and over	Time-series, cross-industry	Young adults (20-24)	Time-series: HLSF, 1985-1997 Cross-industry: Labour and Employment Gazette and Statistics New Zealand Quarterly Employment Survey, 1980-1997	Time-series: 20-24: −.17 to −.34 Cross-industry: −.06 to −.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 (2004)	Introduction of higher minimums for 16-17 and 18-19 year-olds		Teens, 20-21, and 20-25 year-olds	HLSF, 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	Difference-in-differences	All	Enquête Emploi, 1981-1989	No 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

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
	2. Differential impact of national minimum wage increases across regions with varying initial wages	Difference-in-differences	All	Declaration Annuelle de Salaires, 1967-1992	Regions with low initial wages experienced greater employment growth	Labor demand variation could explain results; not restricted to low-skill workers
Bazen and Skourias (1997)	National minimum wage increases across sectors with different percentages of minimum wage workers	Difference-in-differences	Youths (under age 25)	French Labor Force Survey, 1980-1984	Youth employment fell more in sectors where minimum wage was more binding	Does not address overall changes in youth employment or most affected industries; questions about specification
Abowd, et al. (1999)	Differences between workers "caught" by national minimum wage increases and workers with slightly higher wages	Difference-in-differences	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, 25-29: −4.6** **Women, 25-29: −1.38** **Men, 20-24: −.77** **Women, 20-24: −1.21** **Men, 16-19: −.08** **Men, 16-19: .46**	Elasticities larger than conventional estimates because based on comparison to those with slightly higher wages
Netherlands						

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
Dolado, et al. (1996)	Declines in youth subminimums relative to adult minimum in 1981 and 1983	Difference-in-differences	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	
Spain						
Dolado, et al. (1996)	1. National minimum wage, and variation in effects across industries where minimum wage more or less binding	Panel data analysis	Teens (16-19)	Contalilidad Nacional Sectorial	Teens: −.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	Difference-in-differences analysis across regions in share initially low-paid	Teens (16-19)	Contalilidad Nacional Sectorial	**Negative relationship 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	Difference-in-differences	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 −.2 to −.4; substitution towards 20-25 year-olds	Firm size controls may be inappropriate

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
Portugal and Cardoso (2006)	Abolition of teenage subminimum wage in 1987	Comparisons of aggregate changes	16-65 year-olds	Quadros de Pessoal, 1986-1989; Labor Force Survey, 1986-1989	Teen employment grew faster than employment of 20-25 year-olds or older workers following minimum wage increase	No controls; no reconciliation with Pereira's results
Greece						
Karageorgiou (2004)	Variation in teen and young adult minimum wages relative to average adult wages	Time-series	15-19 and 20-24	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: .22 to .63 (larger estimates significant) Young adults: −.05 to −.12 (insignificant)	Little real time-series variation, unexpected results for other controls, failure to account for minimum wages for other groups
Sweden						
Skedinger (2006)	Union negotiated minimum wages in hotels and restaurants	Difference-in-differences	All workers	Surveys from Confederation of Swedish Enterprise, 1979-1999	Job separations in response to minimum wage increases: elasticity .58 overall; .36 to 1.00 for 20-65 year-olds; .77 to .80 for teenagers (although −.12 to −.14 and insignificant for the 1993-1998 subperiod). Job accessions in response to minimum wage decreases: .84 overall; .45 to .55 for teenagers,	

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

Table 5: International Studies of Minimum Wage Employment Effects, Developing Countries

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
Brazil						
Fajnzylber (2001)	Largely time-series variation	Backed out from estimates of effects of minimum wages on income throughout the wage distribution	All, but effects differ based on initial wage	Brazilian Monthly Employment Survey, 1982-1997	Formal sector, below and near minimum wage: −.05 to −.08 Informal sector, below and near minimum wage: −.05 to −.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	Panel data analysis	All ages, many other comparisons (public vs. private, less- vs. more-educated, formal vs. informal sector)	Brazilian Monthly Employment Survey, 1982-2000	Aggregate employment: centered on zero for hours and employment Employment, formal sector: 0 Hours, formal sector: −.02 Employment, informal sector: −.02 Hours, informal sector: .02 Employment, private sector: 0 Hours, private sector: .01 Employment, public sector: .03 Hours, public sector: −0.09	Some estimates difficult to explain or reconcile
Lemos (2005)	Largely time-series variation, with different impact across regions based on different wage levels	Panel data analysis, instrumenting for minimum wage variables with political variables	All ages	Brazilian Monthly Employment Survey, 1982-2000	OLS: −.12 to .02 (most negative) IV: large number of estimates, ranging from −.29 to .12 (most negative)	Political variables may influence other labor market policies as well
Mexico/Colombia						

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
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	Time series and panel data analyses	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: (insignificant) for Mexico, −.18, and (significant) for Colombia, −.34. Panel data: OLS (insignificant) for Mexico skilled, −1.08, and unskilled, −1.52; (significant) for Colombia skilled, −2.93, and unskilled, −2.29. FE (insignificant) for Mexico skilled, −.01 to .05 , and unskilled, −.03 to .03; (significant) for Colombia skilled, −.03 to −.24, and unskilled, −.15 to −.33**	
Mexico						
Feliciano (1998)	Sharp consolidation of regional minimum wages and decline in average minimum wage	Panel data analysis	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: .005 to .014 (−.002 to .037 by age group) Females: −.426 to −.575 (−.406 to −.764 by age group)**	
Colombia						
Maloney and Nuñez (2001)	Two federal minimum wage increases during 1997-1999.	Difference-in-differences, using self-employed as a control group	Men working 30-50 hours per week	National Household Survey, 1997-1999	Employment elasticity: −.15. Stronger effects near minimum wage but effects also present higher in wage distribution	
Chile						
Montenegro and Pagés	Time-series variation in real minimum wage, and variation in teen relative to adult minimum wage	Pooled time-series cross-section	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

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
Costa Rica						
Gindling and Terrell (2004)	Sharp consolidation of occupation-skill-specific minimum wages	Pooled cross-section time-series	All	Household Surveys for Multiple Purposes, industry data from Costa Rican Central Bank, 1988-2000	**Covered-sector employment: −.11** **Covered-sector hours of employed: −.06**	
Trinidad and Tobago						
Strobl and Walsh (2001)	Implementation of national minimum wage in 1998	Difference-in-differences in job loss between those bound and those not bound by new minimum wage	Females and males working in small (fewer than 10 employees) and large firms	Continuous Sample Survey of Population (CSSP) 1996-1998	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	Time-series and panel data	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	Elasticities of employment, time-series: −0.11 to −0.15; panel: −0.54 for full sample period, 0.20 1974; −.91 after 1974 (when U.S. law generated increases)	Krueger (1995) shows that results are fragile.
Indonesia						

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
Rama (2001)	Cross- province variation in minimum wage changes over the early 1990s	Panel data analysis	Urban workers	Indonesia's 1993 Labor Force Survey; data for years 1988-1994 from multiple sources: national accounts, the labor force survey, the wage survey, the survey of large manufacturing establishments and the survey of small scale manufacturing industries	Elasticity for aggregate urban employment using the log of the minimum wage, −.035; using the minimum over labor productivity measures, −.038 to .000. For ages 15-24: using log of the minimum wage, .015; using the minimum over labor productivity measures, −.25 to .086. All insignificant. Large firms: log of minimum, .197; minimum over productivity measures, .018 to .132. All insignificant. Small firms: log of the minimum −1.302 (significant); minimum over labor productivity measures, −.767 to −.821 (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	Panel data analysis	Urban workers	Indonesia's 1993 Labor Force Survey; data for years 1988-2000	Elasticity for aggregate urban employment, −.06 (significant); males, −.047; females, −.155 (significant); adults, −.044; youths, −.123; educated, −.025; less-educated, −.087 (significant); white-collar, 1.00; blue-collar, −.073; full-time, −.058 (significant); part-time, −.109	
Harrison and Scorse (2005)	District level differences in the minimum wage, within the same province	Difference-in differences	Manufacturing firms overall and sub-group of textiles, apparel, and footwear factories	Indonesia's Annual Survey of Manufacturing Firms (1990-1996)	**Diff-in-diffs, elasticity for manufacturing employment, −.047 for all firms; −.051 for balanced panel. Other specifications, −.118 to −.184 (all significant); insignificant only when done separately for small firms, −.021.**	

Study	Minimum wage variation	Method	Group	Data	Estimated elasticities (or other effects)	Criticisms
Alatas and Cameron (2003)	Differences in minimum wage changes between a province, Jakarta, and a grouping of districts, Botabek, across the border between adjacent provinces.	Difference-in-differences	Manufactur-ing 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.**	

Results from studies we regard as more reliable tests of employment effects of minimum wages for subset of workers are highlighted.