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ALTERNATIVE LABOR MARKET POLICIES TO INCREASE ECONOMIC SELF-SUFFICIENCY:
MANDATING HIGHER WAGES, SUBSIDIZING EMPLOYMENT, AND INCREASING PRODUCTIVITY

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Alternative Labor Market Policies to Increase Economic Self-Sufficiency: Mandating Higher Wages, Subsidizing Employment, and Increasing Productivity

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ABSTRACT

I review evidence on alternative labor market policies that could potentially improve economic self-sufficiency via mandating higher wages, subsidizing employment, or increasing productivity. The evidence indicates that the minimum wage is an ineffective policy to promote economic self-sufficiency, entailing employment losses without any corresponding distributional benefits via higher wages. In contrast, living wage laws appear to present a more favorable tradeoff. Labor supply incentives, in particular the EITC, appear effective, as a more generous EITC boosts employment of single mothers and in so doing raises incomes and earnings of low-income families. There is some evidence that wage subsidies increase employment and earnings, but problems of stigmatization resulting from eligibility for wage subsidy programs can dissipate the gains, and wage subsidies entail substantial administrative difficulties. Finally, a newer but growing literature on school-to-work provides some evidence that school-to-work programs boost labor market attachment, skill formation, wages, and earnings.

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I. Introduction

The principal means by which individuals and families achieve economic self-sufficiency is through labor market earnings. As a consequence, it is natural for policy makers to look to interventions that increase the ability of individuals and families to achieve an adequate standard of living from participating in the labor market. This paper discusses some key policies that are used or can be used to increase economic self-sufficiency by increasing earnings.

Broadly speaking, the set of available policy options can be cast in the context of a simple supply and demand analysis. First, we can try to increase earnings via higher wages, by mandating a higher wage floor. Second, we can try to increase earnings by subsidizing employment on either the demand or the supply side, either raising the demand for labor or increasing the supply of labor. And third, we can encourage greater human capital accumulation, increasing productivity and thereby shifting the demand curve out and raising wages and employment.¹

Many public policies directed at the labor market can be viewed within this framework. In this paper, I emphasize policies for which I can bring my own expertise to bear, and for which I can provide what I view as the most valuable contribution to the policy debate by highlighting recent research. In particular, the paper focuses on mandated wage floors (minimum and living wages), employment incentives (the EITC and wage subsidies), and school-to-work policies. Obviously, the potential scope of this paper—what government policy can do to increase workers' and families' earnings—is far more extensive.² Innumerable papers and many books have been written, for example, about welfare reform, job training, the EITC, the minimum wage, and educational reform. I can neither touch on all of the potential policy levers,³ nor even fully treat those policies I do discuss.

Policy makers have attempted to increase earnings by mandating higher wages, via minimum wages and, more recently, living wages. Of course mandating higher wage floors has the potential to discourage employment of low-skill workers, which is the source of much of the controversy over minimum wage floors.

Perhaps the most prominent set of policy interventions and changes with respect to increasing

earnings in pursuit of economic self-sufficiency have targeted the supply side, by trying to change the incentives to work. Welfare reform has clearly aimed to increase employment of the target population—low-income households and low-skilled single mothers in particular—as have other policy changes including expansions of the Earned Income Tax Credit (EITC), revised income tax schedules, and modifications to Medicaid and the provision of public health insurance to children. These policy interventions have sought to change labor supply on either the extensive or the intensive margin. There seems to be little doubt that these policy interventions have contributed to higher employment among the target population (for example, see Meyer and Rosenbaum 2000; Blank and Schmidt 2001; Blank 2002). Viewed from the perspective of increased employment and reduced caseloads, the combined effects of welfare reform and these other changes appears to have been a successful effort to increase earnings, although that, of course, does not mean families are better off.⁴

An alternative policy that operates instead on the demand side is a wage subsidy program targeted toward low-skilled or disadvantaged individuals. A wage subsidy program can take many forms, depending in part on who is targeted, but all share the basic structure of subsidizing wages to increase demand for workers (shifting out the labor demand curve), thereby raising their employment and earnings (see Katz 1998). The current incarnation of wage subsidies in the United States is the Work Opportunity Tax Credit (WOTC), which targets young workers in disadvantaged families or who are “high-risk,” and members of families receiving TANF, as well as a few other groups.⁵

Of course, a key alternative to increasing incentives for work and increasing wages or demand for low-skilled or disadvantaged workers is to try directly to raise the productivity of such workers through training and education. Viewed through this lens, one could view much of the entire body of research on human capital as pointing to ways to increase earnings. For example, there is little disagreement that schooling increases earnings, even if there is continuing debate about the magnitude of the effect. And a simple policy goal of trying to increase schooling is even more compelling in light of increased returns to schooling in recent decades.⁶ In addition, one could view research by labor economists linking educational reform to earnings (for example, Betts 1995; Card and Krueger 1992; Hanushek 2006) as also

pointing to policy interventions to increase economic self-sufficiency via earnings.

I focus more narrowly on one dimension of schooling- and training-related policies for which policy experience is quite recent, and for which research findings have only recently begun to emerge—in particular, school-to-work. This focus is *not* intended to suggest anything about the weighting of the importance of alternative human capital-related policies for increasing earnings, but rather to add information on what we have learned from the recent experience with and research about school-to-work to the broader research literature on human capital and educational reform.

II. Mandating Higher Wages

The minimum wage has been a central component of the nation's efforts to help families achieve economic self-sufficiency since early in the last century. The federal minimum was instituted in 1938, and was predated by earlier efforts in some states. Until the early 1980s, increases in the federal minimum wage were sporadic but over the longer term largely preserved the real value of the minimum wage. Since then, federal increases have slowed, and instead states have picked up the ball; as of January 2007, twenty-nine states plus the District of Columbia had minimum wages above the federal level.⁷ A related development has been the advent of living wages since the mid-1990s, which have now spread to over 140 cities and other localities.

Do minimum wages and living wages enable individuals to earn more and families to exit and remain out of poverty? The research literature is enormous. William Wascher and I recently completed an extensive review of the evidence on the employment effects of minimum wages (Neumark and Wascher 2007) and a broader survey of the effects of minimum wages and living wages on a variety of outcomes (Neumark and Wascher 2008). Here, I discuss some of the main conclusions.

II.1. Minimum Wages and Employment

Much of the political debate surrounding proposed changes in the minimum wage concerns the potential effects on employment. Although that focus is not entirely appropriate, the potential disemployment effects are of course the channel that could dissipate or even outweigh the gains to low-skilled individuals from higher wages.

A large body of research on the employment effects of the minimum wage was conducted in the 1970s, focusing on the effects of the federal minimum wage and comprised mostly of time-series studies. The most widely-cited survey of this literature (Brown et al. 1982) concluded that the evidence established a consensus range of -0.1 to -0.3 for the elasticity of teenage employment with respect to the minimum wage; these elasticities measure the ratio of the percent change in employment to the percent change in the federal minimum wage. The state-level increases since the late 1980s, coupled with federal increases in 1990 and 1991, provided economists with additional “experiments” with which to reexamine the costs and benefits of minimum wages, and this new wave of research produced considerably more diverse results.

The best-known and most controversial of these new studies were conducted by David Card and Alan Krueger and summarized in their 1995 book *Myth and Measurement*. They argued that the consensus view that minimum wages reduce employment of less-skilled workers was wrong, and their most widely-cited results on the fast-food industry suggested that a higher minimum wage could lead to substantial *increases* in low-skilled employment.⁸ Other studies published during this period, sometimes from other countries, cast doubt on the conclusion that minimum wages reduce employment of low-skilled workers (for example, Machin and Manning 1994). This research was sometimes perceived as ending the consensus among economists that minimum wages reduce employment among the less-skilled.

However, the studies highlighted as challenging this consensus were part of a much larger body of research on the employment effects of minimum wages conducted since the early 1990s—including a number of studies I wrote with William Wascher. This larger body of new research includes more than one hundred studies, and encompasses an impressive variety of statistical techniques and datasets, including more sophisticated time-series analyses, case studies of particular minimum wage increases, and panel studies across states and years. Wascher and I recently undertook an exhaustive review in order to determine what can be learned from the wider body of research (Neumark and Wascher 2007).

This review led to two main conclusions. First, there certainly is a much wider range of estimates of the effects of the minimum wage on employment than was the case in the earlier time-series literature

reviewed by Brown et al. (1982). For example, few of the studies in the Brown et al. survey were outside of the consensus range of -1 to -3 for the elasticity of teenage employment with respect to the minimum wage. In contrast, even limiting the focus to studies of the effects of the minimum wage on teenagers in the United States, the range of estimates in the more recent research extends from well below -1 to well above zero. This wider range of estimates partly reflects the fact that the newer literature uses a variety of methods and data to identify the effects of minimum wages—including estimates for narrow subsets of workers and specific industries—whereas the earlier literature was for the most part based on aggregate time-series data that changed only by the addition of more data with the passage of time (although there were advances in statistical methods).

Second, despite the wider range of estimates, the oft-stated assertion that the new minimum wage research fails to support the view that the minimum wage reduces the employment of low-skilled workers is clearly incorrect. Indeed, the preponderance of the evidence points to disemployment effects. Of 102 studies on which we focus, 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, while only eight give a relatively consistent indication of positive employment effects. In addition, we identify thirty-three studies that we view as providing the most credible evidence. Among these, twenty-eight (85 percent) point to negative employment effects. Moreover, in research that focuses on the least-skilled groups most likely to be adversely affected by minimum wages, the evidence for disemployment effects seems especially strong, with minimum wages harming the least-skilled workers more than is suggested by the net disemployment effects estimated in many studies.⁹ In contrast, we see very few—if any—cases where a study provides convincing evidence of positive employment effects of minimum wages.¹⁰ Overall, this review of the newer literature largely solidifies the conclusion that minimum wages reduce employment of low-skilled workers.¹¹

II.2. The Distributional Effects of Minimum Wages

Despite minimum wages reducing employment among the less skilled, minimum wages could have offsetting beneficial effects because of income gains resulting from higher mandated wages. And it

is possible that these income gains tend to accrue to the lowest-income families, an argument sometimes made by those who advocate minimum wage increases.¹² Although the distributional question is central, it often receives short shrift in research and debates about the minimum wage, with critics of minimum wages ignoring the possibility that even if minimum wages *do* reduce employment they may have beneficial distributional effects, and advocates simply assuming that the distributional effects must be beneficial. In fact, as this section explains, research fails to establish that minimum wages have beneficial distributional effects that outweigh the employment losses.

It is commonly claimed that existing estimates of minimum wage employment elasticities imply that minimum wages must on average raise incomes of low-wage workers. The argument is that if the employment elasticity for these workers is in the -0.1 to -0.3 range suggested by many studies of teenagers and young adults, then because the elasticity is below one in absolute value a higher minimum wage must raise incomes of affected workers (Freeman 1996). However, this argument is likely incorrect. Teenagers or young adults are typically studied in research on the employment effects of minimum wages because a large share of them work at or near the minimum wage, so that the effects of minimum wages are more likely to be evident for these groups than for others. Nonetheless, many teenagers and young adults earn significantly more than the minimum wage. As a result, the reported elasticities from studies of teenagers will tend to understate the elasticity of demand with respect to the minimum wage for the least-skilled workers among them who are directly affected by the minimum wage. The same argument applies with greater force to the broader adult population, because the share of adult workers at the minimum is much smaller. The estimated elasticity from the usual minimum wage study also underestimates the relevant elasticity of demand for affected workers because, with some affected workers already earning more than the old minimum wage, when the minimum wage increases the size of the average wage increase associated with a minimum wage increase likely will be less than the minimum wage increase itself.¹³

To examine directly how minimum wages affect low-wage workers, Neumark et al. (2004) estimate various margins of minimum wage effects, including wages, employment, hours (conditional on remaining employed), and—most importantly for the purposes of this discussion—earned income, using

individual-level matched observations on those aged sixteen and older from the Current Population Survey (CPS) Outgoing Rotation Group (ORG) files for the years 1979-1997. For each outcome, we estimate 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 initial wage stands in relation to the minimum wage.¹⁴ With the additional controls included, minimum wage effects are identified from differential changes in outcomes for workers at similar points in the wage distribution who experience different minimum wage changes, and the approach generates estimates of the effects of minimum wage increases on these outcomes at various points of the wage distribution. We estimate both contemporaneous effects and cumulative effects that allow one-year lags.

The contemporaneous elasticity of wages with respect to the minimum is about .8 for workers at the minimum or below 1.1 times the minimum.¹⁵ The elasticity falls to about .4 for workers between 1.1 and 1.3 times the minimum, to about .25 for workers between 1.3 and 1.5 times the minimum, and to .15 for workers between 1.5 and 2 times the minimum, petering out higher in the wage distribution. However, the cumulative contemporaneous plus lagged effects are quite different, with the elasticity near the minimum wage falling to about .4, and declining for the cells slightly higher in the wage distribution. The smaller estimated effects once lags are included suggests that a substantial part of the wage gains caused by minimum wage increases are “given back” in the following year, likely because employers forego the usual nominal wage increases in subsequent years for workers whose wages were increased by the minimum wage, while workers at the same position in the wage distribution in states without minimum wage increases receive these nominal increases.

For workers initially earning close to the minimum wage, the estimated employment elasticities range from about $-.06$ to $-.15$ and are sometimes statistically significant. The estimated elasticities are close to the so-called consensus range of estimated disemployment effects for teenagers, even though the latter estimates are based on workers whose wages can be well above the minimum wage. However, the hours reductions are more severe. The cumulative estimates point to statistically significant hours reductions for workers initially paid at or just above the minimum wage, with elasticities near $-.3$.

Finally, we turn to earned income. The contemporaneous effects are positive (and significant for most cells) for workers initially earning up to twice the minimum wage. However, the cumulative effects tell a much different story. Workers initially below the minimum, at the minimum, and up to 1.1 times the minimum experience income declines. The estimated elasticity for minimum wage workers is on the order of -0.6 and is statistically significant at the 5 percent level; it is about half that for workers just above the minimum wage. The reversal from the contemporaneous effects is attributable in part to the weaker wage gains once we get past the immediate effect of increases in the minimum wage. And overall, the fairly strong negative effects on earnings are in part due to the joint distribution of wage, employment, and hours effects across individuals. Overall, then, this analysis indicates that very low-wage workers are, on average, not helped by minimum wage increases, and instead are hurt, despite the wage increases among those who stay employed.

What about the effects on family incomes? Because poverty is defined based on family income, minimum wage workers need not be in poor families. Gramlich (1976), using data from the early 1970s, showed that there were many low-wage workers in non-poor and even above-median income families. More recent evidence (Burkhauser and Sabia 2007) echoes Gramlich's earlier conclusions. For example, in March 2003 CPS data, 4.2 percent of all workers were in poor families, while only 13.2 percent of workers earning a wage less than \$7.25 were in poor families. Alternatively, using a definition of low-wage workers based on half the average private sector wage, in the 2003 data 46.3 percent—or nearly one-half—of low-wage workers were in families with incomes three times the poverty line or higher, while 24.2 percent were in poor or near-poor families.¹⁶

These numbers clearly imply that many of the potential benefits of a higher minimum wage *could* flow to higher-income families. The key question, however, is how the distribution of family incomes is actually affected by minimum wage increases. Minimum wages undoubtedly create winners and losers, and neither the types of descriptive statistics just discussed, nor the fairly large number of studies that try to simulate the effects of minimum wages on the distribution of family incomes (for example, Horrigan and Mincy 1993; Card and Krueger 1995; Burkhauser et al. 1996), describe the actual distributional

effects of minimum wages and their incidence across families at different points in the income distribution.

In order to describe more fully how minimum wages affect the distribution of family income relative to needs, Neumark et al. (2005) develop a difference-in-differences non-parametric approach, applied to matched March CPS files from 1986-1995. This approach yields estimates of the effects of minimum wages on the proportion of families that are poor or near-poor, and of the extent to which minimum wages push families initially near-poor into poverty, or lift initially poor families out of poverty. Moreover, the non-parametric approach yields a rich description of the effects of minimum wages on family incomes at all points of the income-to-needs distribution.

The main results are displayed in Figure 1. I do not show the entire set of density estimations that are used to infer the effects of minimum wage increases on the distribution of income-to-needs, but rather just the final contemporaneous, lagged, and cumulative estimates of changes in the distribution for the treatment group of state-year observations with minimum wage increases relative to the control group of observations without increases.¹⁷ The difference-in-differences estimates of the effects of contemporaneous minimum wage increases on income-to-needs are shown in Panel A, with slight adjustment based on the need to account for the fact that, for example, some states with no contemporaneous increase had an increase in the previous year. The results indicate that the effect of contemporaneous minimum wage increases is to reduce the proportion of families with income-to-needs between 0 and about .6, to increase the proportion with income-to-needs between .6 and 1.5, and to reduce the proportion with income-to-needs from 1.5 to about 2.7. These results are consistent with minimum wages helping the poorest families, but they also suggest that some families with initial income-to-needs in the range from 1.5 to about 2.7 experience income losses.

Panel B of Figure 1 reports the difference-in-differences estimates of the lagged minimum wage effect. In contrast to the estimated effects of contemporaneous minimum wage increases, lagged increases unambiguously raise the proportion of families below about 1.3 times the poverty line, with corresponding decreases in the proportion of families with income-to-needs between 1.3 and 3.2. This

evidence, and the contrast with contemporaneous effects, is consistent with disemployment effects (or hours reductions) occurring with a lag, while the contemporaneous effect reflects more of the impact of immediate wage increases—which diminish fairly quickly according to the worker-based results discussed earlier.

The total effects of minimum wage increases, shown Panel C, are the sums of the contemporaneous and lagged effects. The estimated effect at each particular point of the income-to-needs distribution is given by the middle curve, while the upper and lower curves are the boundaries of the 95 percent confidence interval, calculated using a bootstrap procedure. The results are quite striking. There is essentially no change in the proportion of families with income-to-needs below .3, as the benefit associated with the contemporaneous increase is offset by the cost of the lagged increase. There is a marked increase in the proportion of families with income-to-needs between about .3 and 1.4, and a marked decrease in the proportion of families with income-to-needs between about 1.4 and 3.3. These results suggest that the overall effect of minimum wage increases is to push some families that are initially low-income but above the near-poverty line into poverty or near-poverty. On a point-by-point basis, the estimated increases in the proportions of families with income-to-needs from about .6 to 1.2 are statistically significant.

By integrating under the curves in Figure 1, and bootstrapping, we find—as reported in the first row of Table 1—that the minimum wage has essentially no effect on the proportion of families with income-to-needs between 0 and .5, but leads to significant increases in the proportion of families between .5 and 1 and the proportion below 1. There is also a significant (at the 10 percent level) increase in the proportion of near-poor families, and a statistically significant increase in the proportion of poor or near-poor families. The estimated elasticity of changes in the proportion poor or near-poor with respect to the minimum wage is approximately .41, and the average minimum wage increase in the sample increases the proportion of families in these categories (combined) by .013, and the proportion poor by .008. The estimates in the second row are based on non-parametric density estimates that adjust for state and year effects, and yield similar conclusions.

Table 2 illustrates more clearly how families with incomes initially above the poverty or near-poverty line might be affected by an increase in the minimum wage. Although minimum wage workers (those earning less than 1.1 times the minimum) account for a very small share of primary earners in families above 1.5 times the poverty line (Panel A), it is not unusual for the lowest-paid worker in higher-income families to be paid at or below the minimum wage (Panel B). And as shown in the Panel C, which presents the distribution of workers in each wage category across income-to-needs categories, there is nearly as large a proportion of minimum wage workers (including those below the minimum) in families with incomes between 1.5 and 3 times the poverty line as in families between 0 and 1.5 times the poverty line, and actually a greater proportion of minimum wage workers in families with incomes-to-needs between 1.5 and 3 than below the poverty line.

Thus, the evidence that minimum wage increases cause somewhat higher-income families to fall below the near-poverty line could easily reflect job losses among low-wage workers in these families, and calculations reported in Neumark et al. (2005) indicate that the numbers of such secondary workers make the estimated effects just discussed are quite plausible. At the same time, it is worth emphasizing that the research literature does not yet include evidence that directly estimates the effects of minimum wages on workers classified by *both* wage levels and family incomes. That is, the evidence just discussed suggests that the adverse effects of minimum wages tend to fall on low-wage workers in low-income families, but that is an inference from the effects of minimum wages on the distribution of family incomes, rather than a conclusion from direct analysis of how minimum wages affect particular subpopulations.

The failure to find beneficial distributional effects of minimum wages is consistent with the broader literature on this question, which is summarized in Table 3.¹⁸ All of these studies rely on CPS data, but they differ in terms of sample period studied, subgroups considered, the measurement of income (before- versus after-tax), and the measures of the income distribution (poverty rate, squared poverty gap, and income inequality measures). Overall, the evidence can be viewed as leading to one of two conclusions, depending on exactly what specifications and approaches one prefers. Either there is no evidence that the minimum wage has beneficial distributional effects, or the minimum wage harms those

at the bottom of the income distribution. In essentially no case, though, is there evidence that minimum wages help poor or low-income families.¹⁹

II.3. Living Wages

In the mid-1990s, political support for minimum wage floors was manifested in a new arena—local governments. In cities and other local jurisdictions across the country, campaigns arose in support of “living wages,” and governments adopted them by the score.²⁰ Table 4 provides illustrative examples of living wage laws, for eight of the largest cities where living wages have been implemented. Clearly many of these are quite higher than minimum wages in the respective states (column (2)), and there are considerably higher (and lower) living wages in other cities. On the other hand, living wage laws have much narrower coverage. As shown in column (3)—and as is true more generally of living wage laws—nearly all living wage laws cover city contractors, while about half also cover companies that receive financial assistance from cities (such as subsidies and tax abatements). In contrast, living wages rarely apply to city employees. Coverage estimates are very hard to come by, especially for living wage laws that cover financial assistance recipients, for which city-level information is typically decentralized. Estimates of coverage by city contractor provisions are typically below 1 to 2 percent, although there is considerable variation in these estimates, and in some cities coverage is higher because of how the law is specified;²¹ coverage by financial assistance provisions of living wage laws is even less clear.

There is ample evidence that living wages raise wages, and also evidence that they cause some employment losses, although not surprisingly there is some controversy about the latter conclusions; see Adams and Neumark (2004 and 2005b). Estimates of wage and employment effects from CPS data are reported in columns (1) and (2) of Table 5.²²

What about distributional effects? Results from CPS analyses are reported in column (3) of Table 5. The evidence yields negative point estimates (implying poverty reductions) for both contractor-only and the broader financial assistance living wage laws, but only the estimated effect of financial assistance living wage laws is statistically significant (at the 10 percent level). For the latter, the estimated coefficient of $-.024$ implies that a one log unit (100 percent) increase in the living wage reduces the

poverty rate by 2.4 percent.²³ Relative to an 18.6 percent poverty rate, this represents a 12 percent reduction, or an elasticity of $-.12$. This seems like a large effect, given a wage elasticity for low-wage workers below $.1$. However, the anti-poverty effects of living wages likely do not come from lifting families from well below the poverty line to well above it, but instead from nudging families over the poverty line; and these average wage effects are likely manifested as much larger gains concentrated on a possibly quite small number of workers and families.²⁴

Note that these positive distributional effects are different from the adverse (or perhaps simply non-existent) distributional effects of minimum wages. There is no necessary contradiction in these findings, however. Although economic theory predicts that raising mandated wage floors will lead to employment reductions—and the evidence from both minimum wages and living wages is consistent with this prediction—theory makes no predictions regarding the effects of mandated wage floors on the distribution of family incomes, or on poverty specifically. The distributional effects depend on both the magnitudes of the wage and employment effects (and other effects), and on their incidence throughout the family income distribution. The gains and losses from living wages may be of quite different magnitudes, and fall at different points in the distribution of family income, than do the gains and losses from minimum wages—depending in part on the types of workers who are affected by these alternative types of mandated wage floors.

Indeed there is evidence of significant differences in the populations of affected workers. Fairris et al. (2005) report descriptive statistics for workers directly affected by the living wage, based on their survey in Los Angeles. In this sample, 4 percent are teenagers (their Table 3.1). I extracted CPS data for Los Angeles for the same years (2002-2003). Overall, among workers the percentage of teenagers is 4.2, very similar to their living wage sample. However, when I restrict the sample to minimum wage workers, the share of teenagers is much higher. For example, among those earning between \$5.15 and \$7.25 (the state minimum wage was \$6.75, and the federal minimum \$5.15), 14.9 percent are teenagers; focusing only on those earning exactly \$6.75, the percentage is 14.1. Thus, these data suggest that workers affected by the living wage, although surely less skilled than the average, are quite similar to the overall workforce

in terms of the age distribution, with about 4 percent teenagers. In contrast, for workers likely affected by a minimum wage, the share of teenagers is more than three times as high. This presumably helps to explain why living wages have more beneficial distributional effects than do minimum wages, because in the former case there is less scope for the gains to be concentrated among teenagers (as well as young adults aged 20-24, who also represent a high percentage of minimum wage workers, and together with teens about one-third of minimum wage workers in Los Angeles). It would clearly be useful, however, to better understand how the different distributional effects arise.

II.4. Minimum Wages and Skills

The evidence on minimum wage (and living wage) effects discussed to this point focuses on short-run effects, typically looking at effects at most a year after minimum wage increases. There are, however, potential effects of minimum wages in the longer-run, through effects of minimum wages on the acquisition of skills via training, schooling, and work experience. Policymakers should be more concerned with how minimum wages affect long-run earnings than with their effects on young adult and especially teen employment, especially with respect to promoting economic self-sufficiency.

Although theoretical predictions are ambiguous, minimum wages may lower training among young workers (see Acemoglu and Pischke 2003; Hashimoto 1982; and Feldstein 1973). There is evidence from CPS data that minimum wages reduce formal training for 20-24 year-olds (Neumark and Wascher 2001a). Mixed evidence is report by Fairris and Pedace (2004), while Acemoglu and Pischke (2003) find no evidence of effects on training one way or the other. Overall, I regard the evidence on the effects of minimum wages on training as pointing to possible adverse effects, but hardly conclusive.

Minimum wages may also affect schooling. The impact could be positive or negative (see Cunningham 1981; and Ehrenberg and Marcus 1980), depending on how minimum wages affect the returns to searching for employment versus the returns to further education.²⁵ For example, if the minimum wage leads to a relative increase in the demand for more-skilled labor, the price of more-educated labor should rise, which may encourage some youths to stay in school. But the minimum wage also raises the wage of youths who leave school (if they find a job), so that the net effect of a minimum

wage increase could be to lower the return to an extra year of schooling, at the relevant margin.

Recent evidence on the effects of minimum wages on schooling of teenagers in the United States points to negative effects. Card (1992) reports a significant negative difference-in-difference estimate of the California minimum wage increase in 1988 on the teenage enrollment rate, with or without demographic controls. Neumark and Wascher (1995) estimate a model of minimum wage effects on employment as well as enrollment, and find that a higher minimum wage leads to little change in the proportion enrolled but not employed, a significant negative effect on the proportion enrolled and employed (elasticity of $-.47$), a weak positive effect on the proportion not enrolled but employed (elasticity of $.14$), and a significant positive effect on the proportion idle (elasticity of $.64$). These estimates imply a negative effect of the minimum wage on the proportion enrolled in school. In updated estimates for a later sample period and using a better enrollment measure, Neumark and Wascher (2003) find robust evidence of negative effects of minimum wages on teenage enrollments.²⁶ Similar results are reported in Chaplin et al. (2003), based on data on the entire population of U.S. public schools.

To assess the overall longer-run influences of minimum wages via training, schooling, work experience, and so on, Neumark and Nizalova (2007) estimate the effects of exposure to higher minimum wages at younger ages—when minimum wages were most likely to be binding—on outcomes for somewhat older individuals (25-29 year-olds). The estimates indicate that adults exposed to minimum wages as teens or young adults subsequently have lower wages and earnings. The effects are not trivial. For example, exposure to the average higher minimum wage (for states with above-federal minimum wages) as a teenager is estimated to reduce adult earnings by .8 to 1.8 percent, and similar exposure as a 20-24 year-old to reduce adult earnings by 1.7 to 2.3 percent. This latter evidence comes from only one study, and it remains to be seen whether other studies reach similar conclusions. In the meantime, however, the findings suggest that in terms of boosting economic self-sufficiency in the longer-run, minimum wages may be particularly unhelpful, presumably by reducing the accumulation of skills that lead to higher earnings as an adult.

III. Subsidizing Employment

The minimum wage is often contrasted with the Earned Income Tax Credit (EITC) in discussions of policies to increase economic self-sufficiency. The comparison is natural, because the EITC subsidizes earnings for low-income working families and creates incentives for employment among families with no workers—pursuing much the same goals as suggested by the rhetoric, if not the reality, of minimum wages. (Of course it also affects income above and beyond earnings via the direct payments to families made under the program.) An alternative approach is to subsidize employers for hiring from particular groups of workers. I discuss these in turn.

III.1. The EITC

EITC payments are determined by four parameters. The earnings credit establishes a subsidy rate for earnings. Currently, the federal credit rate for a family with two or more qualifying children is 40 percent. There is a maximum benefit level, which for the same type of family was \$4,536 (in 2006—this is indexed). There is a “plateau,” or an income range over which the maximum benefit remains fixed (in 2006, from \$11,340 to \$14,810). And finally, there is a phase-out rate at which the credit is reduced as income rises (currently 21.05 percent).²⁷

The Earned Income Tax Credit has expanded considerably, along two dimensions. At the federal level, the credit rate increased sharply over the 1990s, rising from a rate of 14 percent (with two children) in 1990 to 40 percent in 1996, where it has remained since. In addition, a number of states introduced their own EITC programs, which typically specify a percentage supplement to the federal EITC that is provided to families by the state. The number of states with an EITC rose from seven in 1996 to nineteen (plus the District of Columbia) in 2007.²⁸

The EITC generates a variety of incentives regarding labor supply and other behavior. Good surveys are provided by Hoffman and Seidman (2003) and Hotz and Scholz (2003), and the discussion here is cursory. Regarding labor supply, for families in the phase-in range (that is, earning less than the maximum credit), theory predicts that the EITC has an unambiguous positive influence on employment, because the EITC raises the effective wage, and for those previously non-employed there is only a

substitution effect and no income effect.²⁹ Hours effects are more ambiguous. On the phase-in range, there are offsetting income and substitution effects. On the plateau, there is only a negative income effect. And on the phase-out range, both income and substitution effects create incentives for reduced hours. The research is fairly unambiguous in indicating that the EITC boosts employment of single mothers.³⁰ Hours effects for those already working but with somewhat higher income (perhaps a working spouse) appear to be modestly negative.

The implication of these findings is that the EITC is likely to boost the incomes of low-income families. Moreover, as the preceding discussion makes clear, the EITC targets *low-income families*; this contrasts sharply with the minimum wage, which of course targets *low-wage individuals*. Given the weak link between the two, we might expect that the EITC is more effective at reducing poverty and helping low-income families than is the minimum wage. At the same time, it is important to note that the EITC does not target poor families perfectly. Most importantly, the break-even point (at which benefits have fallen to zero) occurs well above the poverty line. Part of the reason for this is that if benefits were phased out too quickly, stronger labor supply disincentive effects (in terms of hours) would be created. In addition, because the EITC is based on income rather than wages, it is possible that it sometimes subsidizes workers who have high skills but work low hours.³¹ The EITC will fail to reach families with such low incomes that they do not file income tax returns, and they may sometimes subsidize higher-income families in which the adults are unmarried but cohabiting, whose joint income if they married would make them ineligible. Nonetheless, calculations suggest that the EITC targets reasonably well—with very few dollars going to families in excess of twice the poverty line (Liebman 1998) and nearly one-half of payments going to poor families (Scholz 1994)—although these calculations are based on simulated policy effects rather than before-and-after analyses.

Neumark and Wascher (2001b) use CPS data to estimate how changes in the EITC affect transitions into and out of poverty (and among other income-to-needs categories); the study exploits state policy variation, captured in the credit rate in the phase-in range, which varies by year and state and with the number of children. It would be unsurprising if the EITC lifts families out of poverty when both

earnings *and* EITC payments are considered. However, the study foregoes using estimated EITC payments (and other transfers), and instead just studies earned income; it therefore asks, for example, whether increased generosity of the EITC raises the probability that a family *earns its way* out of poverty. Such evidence would suggest that the incentives created (and not only the checks written) because of the EITC are pro-work and anti-poverty. Indeed, the evidence suggests that increases in EITC generosity raise the probability that family earnings rise to above the poverty level, and also raise the earnings, on average, of families with children that are initially below the poverty line.³² In contrast, there is no effect of pushing near-poor families into poverty (based on earnings), stemming from adverse effects on hours or on earnings of those initially near-poor. The estimates imply that the average increase in the federal credit rate over the sample period (4 percentage points) reduces the poverty rate by about .029, or nearly 3 percentage points.³³

Given the conclusions regarding minimum wages summarized earlier, it is fairly clear that the EITC has better beneficial distributional effects than the minimum wage, and direct comparisons in Neumark and Wascher (2001b) affirm this conclusion. The Wu et al. study (2006a) also compares the distributional effects of minimum wages and the EITC, as well as other policies, including income tax rates, unemployment insurance, Supplemental Security Income, Aid to Families with Dependent Children (AFDC), Disability Insurance, and Food Stamps. Their evidence points quite strongly to the conclusion that the maximum EITC benefit reduces inequality. Interestingly, though, they find statistically significant negative effects of the EITC benefit on inequality for all specifications *except* variants of the Atkinson index that place relatively more weight on the low end of the income distribution, which the authors suggest occurs because most of the benefits of the EITC actually accrue to families on the plateau and in the phase-out range. And for those families with no workers for whom the EITC does *not* induce employment, the EITC of course delivers no benefits, which is of course a potential shortcoming of the policy.^{34,35}

Finally, a few other issues merit comment. First, even the evidence that finds beneficial distributional effects of the EITC points out that for the very bottom of the family income distribution, the

EITC may not be effective, although that is almost surely true of the minimum wage as well. That is, both of these policies aim to raise incomes of those who work—although one encourages employment and one may have the unintended effect of reducing it. But other policies—more likely focused on disability—are needed to address the income needs of families with no workers or workers facing work limitations.

Second, as pointed out by Horrigan and Mincy (1993), among others, the EITC offers virtually no benefits to unrelated individuals (the maximum credit in 2007 was \$428, for those aged twenty-five to sixty-four). From a distributional perspective, we may be particularly concerned with increasing economic self-sufficiency among families with children. However, recent work has suggested reasons why we may want to consider expanding the EITC to single unrelated individuals, even if this policy seems “counterintuitive” (Berlin 2007) from the perspective of past anti-poverty efforts. These arguments are considered in more detail in the concluding section of this paper, which considers more fully alternatives to the policies already in place.

Third, I have posed the discussion so far in terms of comparing the minimum wage to the EITC as alternative policies to increase earnings. There are arguments suggesting that this “either/or” comparison is inappropriate, and that instead the minimum wage and the EITC may be complementary, with one making the other more effective. One such argument is that a higher minimum wage may reduce the distortionary impact of the EITC on labor supply. In particular, a higher minimum wage enables a family to achieve the same level of income (earnings plus EITC) at the maximum EITC credit with a smaller EITC payment. This, in turn, allows a lower marginal tax rate over the phase-out range of the credit, which could reduce the associated labor supply disincentives (Blank and Schmidt 2001). This argument and others about interactions between the minimum wage and the EITC are discussed in detail in Neumark and Wascher (2009). Some of the evidence quite indirectly suggests that a higher minimum wage might under some conditions enhance the positive distributional effects of the EITC. This is an intriguing possibility. However, the evidence is preliminary, and more research is needed to try to better establish the effects of minimum wage-EITC interactions. But it is worth emphasizing that this is one potential line of argument that could, depending on the results, lead to a more positive assessment of the

distributional effects of minimum wages under some conditions.

III.2. Wage Subsidies to Employers

The EITC subsidizes employment by adding to workers' or families' income an amount based on employment, hours worked, and the wage. Over the phase-in range, and with respect to the employment decision, this operates as a simple employee-based wage subsidy. An alternative policy, with which the United States has more limited experience, is employer-based wage subsidies—that is, making payments to employers for employing particular groups of workers. Although there is not a great deal of research on wage subsidies, evidence on their effectiveness has been reviewed by Katz (1998), who also presents some new evidence. And the two alternative approaches to subsidizing employment have been contrasted by Dickert-Conlin and Holtz-Eakin (2000).

A couple of key issues arise with using employer-based wage subsidies. First, the target group has to be identified. In principle, the targeting can be narrowly defined to improve upon that generated by the EITC, although of course simply targeting low-wage workers would run into the same distributional problems as with the minimum wage.³⁶ In the United States, wage subsidies have alternatively targeted hiring of young disadvantaged workers (Job Opportunities in the Business Sector, or JOBS), AFDC recipients (Work Incentives Tax Credit, or WINTC), and low-wage workers (New Jobs Tax Credit, or NJTC).³⁷ The Targeted Jobs Tax Credit (TJTC) targeted these groups and others, and was in effect from 1979 through 1994, before being replaced by the WOTC, which similarly targets multiple groups.³⁸ Under the Job Training and Partnership Act (JTPA), there were temporary wage subsidies to firms providing long-term employment for recipients of on-the-job training. This was the key wage subsidy for economically-disadvantaged adults who were not on welfare. JTPA was replaced by the Workforce Investment Act (WIA) in 2000, which does not include wage subsidies.

Both reviews of wage subsidies concur that the evidence suggests that narrow targeting is problematic, stigmatizing the intended beneficiaries and consequently making employers less likely to hire them (or at least dissipating the effects of the subsidies). For example, Katz discusses an experimental program in Dayton, Ohio, under the TJTC, for welfare recipients. The two groups that

received vouchers indicating their eligibility to present to employers were less likely to find employment than a third group of job seekers that did not receive vouchers, but technically had the same eligibility.

The second issue is program design. In particular, employer-based wage subsidy programs can create strong unintended incentive effects. If the subsidy seeks to have the most impact on generating new employment, then in some way it has to identify and reward either net (positive) changes in employment that would not have otherwise occurred, or new hires directly. The latter approach can create incentives for churning employees.³⁹ The former approach may not create incentives for churning, but it imposes the need for a good deal of information from firms (to try to determine, based on some rule, what employment would have been otherwise). In addition, this approach can generate incentives for employment variation (since increases are subsidized but reductions are not penalized). A simpler scheme can simply subsidize all employment (perhaps in targeted groups), but this would reward employers in large part for what they are doing already (“windfalls”), and hence entail much greater expense and likely less impact. In general, any employer-based subsidy is likely to be rather complicated, both in terms of identifying eligible workers and determining the subsidy paid to firms. As a consequence, subsidies entail serious administrative constraints on firms. These constraints likely explain the very low utilization of employer-based wage subsidies when they have been implemented.

How do wage subsidies compare with the EITC, which effectively pays workers to subsidize their employment (and hours)? In the simplest textbook world, the wage and employment effects of a wage subsidy do not depend on whether the subsidy is paid to employees or employers. However, as Dickert-Conlin and Holtz-Eakin (2000) emphasize, things are more complicated. In addition to the fact that the EITC is not a simple wage subsidy, they emphasize factors that likely diminish the effectiveness of wage subsidies, in particular. One issue is that of stigma, already discussed, which implies that any wage subsidy is less effective than the “posted” subsidy because of the negative information it may convey about potential employees. The EITC does not have stigma effects, since the employer typically has no idea whether an employee is eligible for or receiving the EITC,⁴⁰ and it has good but, as noted, not ideal targeting. Wage subsidies that do not target specific groups, but instead subsidize only an initial amount

of wages, reduce stigma, although of course the targeting then worsens.

A second issue is administrative and compliance costs. As already noted, these can be substantial for employer subsidies. The EITC, on the other hand, is easily administered through the tax code, although there is a sizable potential cost from fraudulent claims associated with claiming children on tax returns when they did not in fact reside with the filer for the half of the year required by the law.

The simplest model also breaks down when we consider incentives on the extensive (employment) versus intensive (hours) margins. A program that does more on the extensive margin likely delivers more of the intended impact, but Dickert-Conlin and Holtz-Eakin suggest that employer subsidies may have more impact on the intensive margin, for which they suggest that supply is less elastic and demand more elastic, in which case more of the benefits of the subsidy go to employers.

Despite problems with employer-based wage subsidies, Katz (1998) presents some evidence indicating that the TJTC did boost employment of disadvantaged youths, and discusses evidence indicating positive and persistent program impacts from JTPA when the training was combined with job search assistance, especially for adult female welfare recipients. This leads him to conclude that wage subsidies combined with training and job development assistance can help disadvantaged adults, but based on the evidence on stigma and low utilization, to express more skepticism (while still suggesting modest benefits) of other narrowly-targeted, stand-alone programs. In more recent evidence on the WOTC, Hamersma (2005b) concludes that any employment effects are small, if they exist at all, and are hard to establish based on the existing evidence. She does find positive effects on earnings (of around 10 percent), although only on the job paying the credit, and not over the course of the year after starting a subsidized job. Dickert-Conlin and Holtz-Eakin (2000) favor the EITC over wage subsidies, based on evidence on the positive labor supply and poverty-reducing effects of the EITC, as well as the limited evidence of effectiveness and utilization of employer-based subsidies.

I would suggest that we do not yet have sufficient evidence on the impact (and costs) of wage subsidy programs on the basis of which to make definitive statements, and both reviews are similarly cautious. However, based on the considerations discussed here, it seems that there is little compelling

basis for preferring employer- to employee-based subsidies, unless we can identify not only effective employer-based wage subsidy programs, but also demonstrate that for particular groups they may be more effective than the EITC, or enhance the positive benefits of the EITC. Note that some of the evidence Katz discusses suggests possible cases like these, in particular for marginalized groups such as disadvantaged adults in need of training, or welfare recipients. (This echoes some of the concerns raised earlier regarding the inability of the EITC to provide much benefit at the bottom end, as it delivers no benefits to the non-employed.) His suggestion that employer-based wage subsidies appear most promising when combined with other job-related services may point to the need for particular interventions to provide training and job skills to those for whom simply strengthening financial incentives to work may not be enough.

IV. Increasing Skills via School-to-Work

I now turn to discuss public policy that seeks to improve earnings and hence economic self-sufficiency through increasing skills. As noted in the Introduction, my discussion focuses on school-to-work policies. Because school-to-work policies are, by construction, effective mainly for young people, an important element omitted from this discussion is efforts to increase skills among adults already in the labor market. As already noted, there is an extensive literature on training programs, some targeted on adults. Although I do not discuss the training literature in any detail, it is worth pointing out that evidence compiled in that literature tends to find that traditional job training programs have some beneficial effects in terms of increasing earnings for disadvantaged adults, but quite limited effectiveness for less-advantaged youths.⁴¹ This may provide an additional motivation for focusing on school-to-work programs, as an alternative and possibly more effective means of intervening with teenagers to increase skills and thus earnings.

IV.1. School-to-Work Background and Policies

School-to-work policies were encouraged to reduce the “churning” or “milling about” experienced by some youths in the United States upon their entry into the labor market, reflected in initial periods of joblessness or a series of “dead-end” jobs (U.S. General Accounting Office 1990). Researchers

and others advocated that the United States adopt a more orderly school-to-work system, like that of the German apprenticeship system or the informal contracts between Japanese schools and employers (for example, Commission on the Skills of the American Workforce 1990; Hamilton 1990). This policy debate provided much of the impetus for the 1994 School-to-Work Opportunities Act (STWOA), which provided \$1.5 billion in federal funding to support the creation of an integrated system of youth education, job training, and labor market information, to provide a faster and more successful transition from school to stable employment in higher-paying jobs.⁴² In addition, school-to-work practitioners and advocates commonly argue that school-to-work programs like those encouraged by the STWOA are especially helpful for the “forgotten half”—the non-college bound among whom the less-advantaged are concentrated. However, in studying school-to-work programs it is important not to focus solely on the STWOA. As described in more detail in Neumark (2007, Chapter 1), school-to-work policies culminating in the STWOA developed from a rather long history of policies addressing the school-to-work transition. What distinguishes the STWOA is its efforts to integrate academic and vocational education for the traditional target audience of those bound for at most two-year degrees as well as those bound for four-year colleges and universities (Cohen and Besharov 2002).⁴³ A second effort toward integrating academic and vocational skills was the development of “career academies,” which are “schools within schools” that integrate academics with general job readiness and preparation in a particular career area.

The STWOA was not re-authorized after its initial five years, and the federal effort was essentially abandoned by the Bush Administration in favor of No Child Left Behind (NCLB), which emphasizes measurable academic outcomes and assigns a central role to standardized testing in grades K-12. Although educational reforms focusing on school quality can be viewed as seeking to enhance labor market success via better educational preparation, there is some basis for questioning whether test-based reforms do as much to prepare students for careers as do school-to-work efforts, especially for the forgotten half. This section of the paper addresses the effectiveness of school-to-work efforts in improving prospects for economic self-sufficiency. A comparison of school-to-work efforts and test-based educational reforms is more challenging and is not taken up here.

IV.2. The Effects of School-to-Work

IV.2.a. Earlier Research

Stern et al. (1995) provide a thorough compendium of earlier research on school-to-work programs. The research summarized in that volume offers little persuasive evidence of positive impacts of these programs on adult labor market outcomes. First, few studies have focused on labor market outcomes more than a year or two after completion of the programs, and those that do provide limited evidence that over a period of a few years beneficial effects of some types of school-to-work programs dissipate, as comparison group members find good jobs on their own. Second, many of these studies do not construct a reasonable comparison group, let alone consider the problem of selection into programs on the basis of unobserved characteristics that might also be correlated with outcomes. Third, even those studies that attempt to construct a good comparison group find no beneficial short-term labor market effects, with the possible exception of those students who remained with the employer with whom they “apprenticed” during the program. Finally, some of the evidence suggests that school-to-work programs may discourage post-secondary education.

A major report on the STWOA by Mathematica (Hershey et al. 1999) did little to advance our understanding of program impacts; in fact, the main goal of this report was not to provide a program evaluation. Nonetheless, the report does present some evidence that is intended to speak to the effects of school-to-work programs. For example, the report notes that students in paid positions arranged as part of school-to-work programs are employed in a wider array of industries and receive more training than other students in paid positions, and concludes that “Schools develop positions in a wide range of industries, increasing the chances that students can work in a setting relevant to their career interests” (p. 89). However, nothing in the evidence implies that students who found these jobs as part of school-to-work programs would not have found the same types of jobs absent such programs; students most likely to do so may simply have sorted into school-to-work programs. Students do, however, report that school-to-work activities helped them sharpen their career goals; whether this translates into concrete gains was not addressed. Paralleling this view of the earlier evidence, a subsequent survey of published academic

research on school-to-work across the United States generally supported the claim that little progress had been made in estimating the causal effects of school-to-work programs (Hughes et al. 2001).

IV.2.b. Evidence from the 1997 National Longitudinal Survey of youth (NLSY97)

The NLSY97 offers researchers opportunities to improve significantly upon the existing research. The NLSY97 covers respondents who were in high school during the period in which the STWOA was in effect, and surveys individuals about “programs schools offer to help students prepare for the world of work.” The school-to-work programs covered in the NLSY97 include: job shadowing; mentoring (matching students to an individual in an occupation); cooperative education (combining academic and vocational studies); work in a school-sponsored enterprise; Tech Prep; and internships or apprenticeships. However, the NLSY97 has not until recently permitted much follow-up as the respondents age into adulthood.

In two papers with Donna Rothstein (Neumark and Rothstein 2006, 2007), we explore the effectiveness of school-to-work programs using the NLSY97. The analysis in the first paper is based on data from the first four rounds of the NLSY97. When the first round was administered, in 1997, respondents were aged twelve to seventeen. With the second round, we could begin to observe some respondents who have left high school, but we get many more such observations with the third and fourth rounds, and therefore focus on educational and employment outcomes measured as of the third or fourth rounds (and in the 2007 paper, the fifth round).

The core empirical framework is estimation, at the individual level, of the relationship between employment or enrollment in the post-high-school period, and participation in school-to-work during high school. The analysis is based on dichotomous choice models for employment and enrollment; both of these activities are considered important to later labor market success, in particular relative to the alternative of being neither in school nor employed. We estimate models for employment and enrollment outcomes as determined by participation in a number of different school-to-work programs, as well as a vector of individual, family, or school-level controls.

However, this research has to confront the potential for endogenous selection into school-to-work

participation. For example, individuals with the highest expectations of work after high school may choose to participate in these programs, perhaps because they are more interested in learning about the job market. When actual work behavior is observed, then, we would find that those who participated in school-to-work have higher employment rates, but the positive relationship between school-to-work participation and employment is at least partly non-causal. Alternatively, schools with student populations that tend not to go to college may be more likely to offer school-to-work programs and to have students with lower college enrollment, in which case, again, the association between school-to-work participation and later outcomes might not be causal. A common approach to the endogenous selection problem with longitudinal data is to implement a first-difference estimator based on observations before and after program participation. But because we are studying program effects on individuals' *first* labor market experiences, or on further school enrollment of those already enrolled, there are no meaningful observations on the outcomes of interest prior to participation, and hence such estimators are inapplicable.

Instead, we use an extensive set of controls for the factors that might affect the dependent variables and also be correlated with school-to-work participation. In addition to standard demographic information, the NLSY97 includes data on living arrangements and family structure and income, test scores (from the Armed Services Vocational Aptitude Battery, or ASVAB), and self-reported measures of school behavior (whether the respondent was threatened at school, or had gotten into a physical fight at school, and information on absences or tardiness at school). Even more useful are the respondent's self-reported subjective probabilities for future education and employment, including: receipt of a high school diploma by age twenty; obtaining a four-year college degree by age thirty; and working over twenty hours per week at age thirty. Including these expectations variables can be viewed as mimicking the longitudinal estimator that we would like to have, as these controls capture some measures of an individual's propensity for later enrollment or employment *prior to* participation in school-to-work.⁴⁴

We also found that school offerings of school-to-work programs were correlated with characteristics of schools and their student bodies, such as truancy, pregnancy, and alcohol and drug use. These associations suggest that if we use individual-level school-to-work participation to identify the

effects of school-to-work, but the variation in participation stems partly from variation across schools, then we may obtain biased estimates. The NLSY97 has one additional feature that we can exploit to address this problem. Specifically, there are data on multiple students in the same school, allowing school fixed effects to be added to the equations to control for unobserved factors that are common to students within a school. Of course, since individuals within schools differ from one another, in the within-school estimation attention must still be paid to individual-level heterogeneity.

Key results are presented in Table 6. As it turns out, the estimates are quite similar across the alternative specifications—including just the basic demographic controls, the more detailed ones listed in the table, the proxy variables measuring respondents' work and schooling expectations during high school, and the school fixed effects. Consequently, I simply summarize the overall results. Looking first at college education, the estimates reveal considerable heterogeneity in the effects of different types of school-to-work programs. There is some evidence that job shadowing and mentoring programs are associated with a significantly higher likelihood of some college education, and robust evidence of a positive effect of school enterprise programs.⁴⁵ The estimates are often quite sizable. For example, in Panel A, with the basic controls, the estimated differentials for mentoring and school enterprise programs range from about .07 to .11; relative to the sample proportion of .50 with some college, these estimates imply increases of 14 to 22 percent in the probability of college attendance. On the other hand, Tech Prep programs are associated with a significantly lower likelihood of college education.

The employment results are more robust across specifications and samples, generally pointing to statistically significant positive effects of coop programs, with effects near .06 to .10, and also evidence of positive effects of internship/apprenticeship programs. Relative to a sample proportion currently employed of .63, the estimates imply that coop programs are associated with approximately 10-16 percent increases in the probability of post-high school employment, and internship/apprenticeship programs with increases of about 8 to 18 percent.⁴⁶

A key finding is that the school-to-work programs that boost college attendance (job shadowing, mentoring, and school enterprise) do not adversely affect employment, and conversely those programs

that boost employment (coops and internships/apprenticeships) do not reduce college attendance. This suggests that, on net, these school-to-work programs increase skill formation; if, in contrast, they tended to reduce employment when enrollment rises, or vice versa, the conclusions would be more ambiguous. The one piece of evidence on school-to-work programs that appears in a more negative light is the estimated negative impact of Tech Prep on college enrollment. Paralleling the earlier discussion, if there is any tradeoff with employment so that work increases, this negative enrollment effect might be viewed less negatively. It turns out that there is a positive, although only weakly significant, effect of Tech Prep on the likelihood of full-time work conditional on employment, which is roughly the same size (but opposite sign) as the estimated effect on enrollment. This suggests that the negative effect of Tech Prep on schooling is roughly offset by a higher incidence of full-time work. Nonetheless, as the returns to schooling in the form of higher wages typically outweigh the returns to experience—and even more so with regard to the return to full-time versus part-time experience—it is difficult not to view the negative effect of Tech Prep on schooling in a somewhat adverse light. Finally, it bears repeating the reservation that these estimates are only suggestive of longer-term beneficial effects; longer-term analyses await more data.

IV.2.c. School-to-Work Programs and the “Forgotten Half”

Neumark and Rothstein (2007) use the NLSY97 data to explore the differential effects of school-to-work program participation on disadvantaged and minority youths. The analysis proceeds in two steps. First, to operationalize the “forgotten half,” we estimate a reduced form model for attending college. We do this without incorporating information on school-to-work participation, to establish the ex ante probabilities of college attendance (on the basis of which schools or policymakers might target school-to-work efforts). We use the estimates of this model to distinguish between those in the top and bottom halves of the distribution of the predicted probability of college attendance, interpreting the latter as the “forgotten half.”⁴⁷ We then estimate regression models for the effects of participation in various school-to-work programs on a number of post-secondary education- and employment-related outcomes, allowing for separate effects of program participation for those in the top and bottom halves of the predicted

probability of college attendance—in other words, separate effects for the forgotten half. These estimates are then used to test which types of school-to-work programs are particularly effective at boosting post-secondary outcomes for the forgotten half.

We use the same data as in the earlier paper, but extended through the fifth round. In addition, we explore a richer set of education and work-related outcomes. Because the findings from Neumark and Rothstein (2006) suggested little evidence of endogenous selection into school-to-work programs in a manner that biases the estimates of program effects, for this analysis the regressions with the detailed controls, but without either the subjective expectations data (available for only a subsample) or school fixed effects, were used.

Table 7 provides a summary of the results. The table displays the estimated signs of effects for those in the forgotten half, showing all cases for which the estimates are significantly different from zero only for the forgotten half (or significant for both, but with the opposite sign for the forgotten half). The estimates are also broken up into those indicating that school-to-work participation increases skills, and the opposite case. Thus, the entries in the “skill increasing” panels highlight the school-to-work programs that appear to increase schooling, employment, and earnings *only* for those in the forgotten half.

Looking first at the results for women, there is little indication of positive effects on schooling-related outcomes for those in the forgotten half. However, the conclusion is quite different for work-related outcomes, with the most striking finding that internship/apprenticeship programs appear to be particularly effective at boosting wages and earnings for the forgotten half. For men, the situation is somewhat different. In general, there is more evidence that school-to-work programs are particularly beneficial for the forgotten half. Moreover, there is evidence of beneficial effects on both schooling-related and work-related outcomes. With regard to schooling, for five of the six programs there is evidence of positive effects on education, and for the work-related outcomes, there is particularly strong evidence that internship/apprenticeship programs boost employment and decrease idleness among men in the forgotten half, with similar results for school enterprise programs.

Overall, then, there is evidence that school-to-work programs are particularly advantageous for

men in the forgotten half with respect to both schooling and work-related outcomes, but for women only with respect to work-related outcomes. Thus, the combined evidence from the NLSY97 points to some beneficial effects of school-to-work programs, and suggests that especially for men participation in some school-to-work programs increases education and employment among the forgotten half.

IV.2.d. Evidence on Career Academies

“Career academies” have three elements. First, they combine academic courses that meet college entrance requirements and technical classes that relate to a specific theme, which is typically organized around an industry such as finance, travel and tourism, public service, or information technology. Second, academies engage in partnerships with employers in the industry to provide internships and other work-related experiences outside the classroom. Finally, career academies are typically structured so that groups of students take the same classes together in each grade, and stay with the same group of teachers for at least two years.⁴⁸ Career academies can be thought of as relatively intensive versions of school-to-work programs.

Even some recent studies of career academies (Orr et al. 2007; Stern et al. 2007) suffer from two problems—an inability to address endogenous selection into these academies, and a lack of data following students after they leave high school. Nonetheless, these studies provide descriptive information on the content of career academy programs, and some suggestive evidence that career academies associated with fields that generally require a college degree may boost college attendance.

However, the ongoing evaluation of career academies by the Manpower Demonstration Research Corporation (Kemple and Snipes 2000; Kemple 2001; Kemple 2004) addresses explicitly the problem of endogenous program participation, *and* includes later labor market outcomes.⁴⁹ The strength of this study is that it is based on random assignment of students to career academies, as participants were chosen randomly from applicants to the career academies in the study, with participants and non-participants followed for four years (thus far) after leaving high school.

The most recent report (Kemple 2004) finds beneficial labor market effects for male participants, but not for female participants. For example, for male participants relative to non-participants the

probability of any employment and full-time employment is higher in each of the four follow-up years, with differences of 3.6-11.8 percentage points (mostly significant). Average weekly hours in each year are significantly higher, by about four hours, the average wage is significantly higher in most years, by about \$.80 (although less in year two), and average monthly earnings are higher in most years, by \$166-\$268 (significant in all years but year three, and highest in the last year). There was also some evidence that participants were in better jobs; as of the last quarter, among those employed, participants were more likely to have a health plan, sick leave, paid vacation days, and a retirement plan, although only the vacation day differential was significant. Comparing the estimated differences in employment, hours, earnings, and so on, across each of the four follow-up years, there is no evidence of increasing (or decreasing) differentials between the participants and the controls. The earnings differential, for example, is \$206 in year one, \$202 in year two, \$166 in year three, and \$268 in year four. The wage, hours, and employment differentials are similarly relatively stable. This does raise an interesting question of what the nature of the effect of career academies is. The evidence does *not* point to greater career progress of participants. Rather, the differentials that emerge in the first year after leaving high school are largely persistent.

For women, interestingly, none of these differences emerged. The estimated differences were much smaller and statistically insignificant, although the point estimates tended to favor participants over non-participants. Kemple points out that the young women in the sample were more likely to have children than the young men, with no differences between women who participated and the controls. He suggests that the higher incidence of childbearing “may have limited the extent to which the young women were able to capitalize on the career development experiences that they had in the career academies” (2005, p. 16). Finally, with respect to labor market outcomes, the results indicated that the beneficial effects were concentrated on those who came from higher-risk groups, defined as either those with a high likelihood of dropping out, based on characteristics measured prior to random assignment, or those who were not highly engaged in school.⁵⁰

Of course it is possible that the labor market gains for men could come at the expense of

schooling, which might deliver more long-run returns. The evidence for men suggests many negative schooling-related differentials for participants relative to non-participants, although only one is significant, for ever enrolling in post-secondary education (a 6.8 percentage point differential). Other shortfalls for male participants, although none are significant, include completion of a post-secondary credential, receipt of a bachelor's degree, or receipt of a skills training certificate or license; the estimates range from about 2.3 to 4.2 percentage points. Kemple argues that the evidence suggests that the "labor market outcomes for young men did not come at the expense of reducing the prospects for post-secondary education" (2005, p. 21). I am a bit less convinced of this given the sign patterns of the estimates, although clearly in terms of statistical significance the evidence of positive labor market effects is stronger. Moreover, as the study reports, for the higher-risk group defined in terms of likely drop out, there was a significant negative effect on post-secondary educational attainment.⁵¹ For women, there is no evidence of effects on education. Given the potential negative schooling effects, it will be important, if possible, to follow up participants even later, to see whether the positive effects for male participants decline as non-participants leave school, although it is highly unlikely that this could erase the reported gains.

Finally, one slightly troubling aspect of this study is that the non-academy control group did exceptionally well in terms of high school completion. The rates for the treatment and control groups, respectively, were 81.3 and 83.3 percent, compared with 61.2 percent in a comparable random sample of students in a general curriculum, which Kemple suggests is the group from which academy students typically come. (The study does not attempt to provide similar comparisons for the other outcomes it covers.) In my view, the high achievement of the control group raises questions about the generalizability of this study. In addition, these comparisons put the estimated effects of career academies in perspective, suggesting that even though there were significant gains relative to the control group, the gains may have been relatively small compared to the low achievement and performance of the general population that career academies are intended to help.

Nonetheless, this study does provide evidence of positive gains from career academy

participation. The evidence from the MDRC study clearly represents a major addition to the evidence of beneficial effects of career academies. It also to some extent parallels the findings from the NLSY97 for school-to-work more generally, indicating that school-to-work programs deliver labor market gains for disadvantaged men.

IV.2.e. School-to-Work at Post-Secondary Institutions

All of the research on school-to-work programs discussed to this point, and most of the existing research literature, focuses on high schools. This reflects the policy emphasis. Person and Rosenbaum (2007) provide a provocative perspective that suggests that this policy emphasis should be at least partially redirected. In particular, they begin by arguing that linkages between high schools and labor markets are relatively weak, with employers having little regard for high school achievement, and high school staff having little trust of or interaction with employers. Of course school-to-work programs seek to change this. Nonetheless, echoing Grubb (2001), among others, Person and Rosenbaum emphasize the potentially important role played by community colleges, and occupational colleges in particular, in the workplace preparation of lower-skilled adults. Person and Rosenbaum point out that many students enter post-secondary education, with nearly half of these at community colleges. This fact, coupled with the occupational focus of many community colleges—based in part on the ability of these local institutions to collaborate with local employers and government—suggests that we should look to community colleges as playing a potentially important role in the school-to-work transition.

Person and Rosenbaum's research seeks to explore the role of two-year post-secondary institutions in creating linkages between school and work. Much of their focus is on differences between private occupational colleges and public community colleges, which is potentially informative about what the latter institutions might do better. More generally, this work can be viewed to some extent as testing the idea that these institutions are a potentially fertile ground for policy efforts focused on the school-to-work transition. The authors provide a mixed qualitative and quantitative study of labor market linkages among faculty at two-year colleges, comparing seven public community colleges and seven private occupational colleges; the latter are hardly representative, and may instead point to "best practices" at

such schools.

The qualitative evidence from interviews of program chairs points to a number of dimensions along which labor market linkages are taken more seriously at the private occupational colleges, even if there are formal responsibilities for labor market linkages at the public community colleges. These include greater contact and integration between faculty/teaching and career services, greater involvement with advisory committees of local employers, fewer bureaucratic obstacles to changing curriculum to respond to new developments, more individualized and intensive job placement efforts, and a mission more focused on workforce training rather than general education and transfer to four-year colleges.

Analysis of data from a survey of over 4,000 students at these colleges tries to examine quantitatively whether labor market linkages on the part of teachers or the institutions (as perceived by students) had beneficial effects. Person and Rosenbaum found that labor market linkages are greater at private colleges, and lead to increased effort in school and greater confidence about degree completion. This conclusion is reinforced by evidence from national samples that job placement services at private community colleges are positively associated with degree attainment. Clearly more evidence is needed of the actual effects of school-to-work type interventions at two-year and community colleges. But this work points to potential importance of focusing policy efforts on two-year colleges in addition to high schools. And one critical question is how two-year and community colleges can better contribute to improving skills among adults already in the labor market.

V. Conclusions and Discussion of Policy Options Moving Forward

This paper does not consider all of the labor market policies that could potentially improve economic self-sufficiency via increasing earnings. It focuses on mandated wage floors, the EITC and wage subsidies, and school-to-work programs. It excludes key issues such as welfare reform, training, and education reform including early childhood investments, without in any way suggesting that these are less important.

As noted in the introduction, the policies I examine can each be considered in the context of a simple supply and demand analysis. A minimum wage mandates a movement up the labor demand curve,

toward higher wages and lower employment. The EITC pays subsidies to workers to increase employment, and hence shifts the labor supply curve outward, raising employment, potentially lowering the market wage, but increasing the combined wage including the EITC (as long as the supply curve slopes upward and the demand curve downward). Wage subsidies instead subsidize hiring by employers, shifting the demand curve out and also raising employment and the wage (part of which is paid in the form of the subsidy), under the same conditions. And finally school-to-work policies aim to increase productivity, also shifting the demand curve out and raising wages and employment. Even in a simple textbook setting, it is unclear which of these policies will do the most to increase income. And once a more realistic view is adopted, with heterogeneous workers who can be affected differently by the alternative policies, predictions about which policies will work become even more complicated, as questions of who gains and loses, and by how much, come to the fore. What are the main conclusions from the empirical analysis of each of these policies?

First, the minimum wage is an ineffective policy to promote economic self-sufficiency through higher earnings. It reduces employment of the least-skilled individuals it is trying to help. That, in itself, does not imply that minimum wages do not on net help. The more telling evidence is that minimum wages do not deliver beneficial distributional effects to poor or near-poor families, and may make them worse off. In addition, minimum wages appear to have deleterious longer-run effects on earnings, presumably through reducing the accumulation of skills. Putting the case succinctly but strongly, it is extraordinarily difficult to discern any case for higher federal or state minimums in order to improve economic self-sufficiency.

Interestingly, though, living wages, which target different workers, present a more favorable tradeoff. They still entail disemployment effects, but appear to deliver more beneficial distributional effects. Of course the implication of the research on minimum wages is that sharply expanding the coverage of living wage laws would take us into the territory of minimum wages, with their concomitant adverse effects. Moreover, even if the findings on living wages might imply that at much higher levels minimum wages could have some beneficial distributional effects, the costs of raising the wage floor from

current levels of minimum wages to the much higher levels typical of living wages could be very high.

Labor supply incentives, in particular the EITC, appear effective. There seems to be fairly compelling evidence that a more generous EITC boosts employment of single mothers and in so doing raises incomes and earnings of low-income families.

Wage subsidies are the flip side of trying to strengthen employment incentives, but provide the incentives to employers rather than employees. There is some evidence that these subsidies increase employment and earnings. (Certainly if I am convinced that employment for the low-skilled falls in response to a higher minimum wage, I should also think that subsidizing wages boosts employment.) However, as Dickert-Conlin and Holtz-Eakin (2000) emphasize, problems of stigmatization resulting from eligibility for wage subsidy programs can offset some of the gains. Coupling such programs with training and job search assistance may reduce problems associated with stigma and hence increase the benefits of wage subsidies. Another possible means to avoid stigmatization is to pay the subsidy to workers instead of to firms (Scholz 2007)—a policy more like the EITC but based only on low wages.⁵² Assuming that the low effectiveness of existing subsidy programs is principally due to low participation by firms, but that the effects of a wage subsidy paid to employees would parallel those estimated for the EITC, the employment effect could be sizable (Scholz 2007). Although wage subsidies paid to employees are worth considering, at present there does not seem to be a great deal of political support for expanding them. Moreover, a major effort in this direction entails substantial administrative difficulties. Thus, it may be that increasing the generosity of the EITC for unrelated individuals is a more realistic option for further extending subsidies to employment, especially if the goal is to increase earnings (and incomes) among those not currently eligible for the EITC.

One argument for extending the EITC to individuals is to offset some presumed adverse consequences of lower earnings for the less advantaged—including declining employment rates—stemming in large part from the long-run increase in wage inequality (Berlin 2007). Berlin also points out that there have been declines in marriage and increases in out-of-wedlock childbearing and childrearing, and that these changes may have occurred in part because declining earnings of men made them less

attractive marriage partners. Berlin argues that rising earnings from an expanded EITC might therefore deliver benefits in addition to direct income, including higher marriage rates, decreased relative attractiveness of illicit sources of income, and so on. In addition, Gitterman et al. (2007) suggest that an expanded EITC for individuals without qualifying children would also recognize that many non-custodial parents have responsibility for children, especially in light of high out-of-wedlock childbearing and divorce rates.⁵³

There is evidence that declining wages for less-skilled men have reduced employment (for example, Juhn 1992). Consistent with this evidence, Berlin (2007) cites evidence from experimental evaluations of programs offering earnings supplements to low-wage workers conditional on working thirty hours or more per week, which points to positive effects on earnings (net of the income supplement). However, most of the evidence pertains to single mothers. One program offered supplements to single men (the New Hope community group in Milwaukee), and it generally produced positive effects on employment, earnings, and family income of men, even up to five years after the program, although many of the estimated effects are not significant (Duncan et al. 2007). However, regarding the broader link between higher earnings and the encouragement of marriage and discouragement of crime, the evidence is certainly more sparse. Scholz (2007), who also advocates making the EITC for unrelated individuals more generous, discusses some of the research linking lower wages to higher crime and lower marriage rates, while acknowledging that this evidence is fairly limited, especially in the case of marriage. Finally, there appears to be no evidence on how an expanded EITC for individuals might translate into more resources for children of non-custodial parents. Clearly, though, if such a policy would lead to major reductions in crime, for example, then the benefits could outweigh the costs, as Scholz (2007) argues.

However, a number of issues arise in considering this policy. To begin, it is useful to think about how the EITC affects wages and the margins on which it operates in order to try to clarify who might gain and lose from an expanded EITC. The current EITC boosts employment among those who would not work in its absence, which should increase competition with those already in the labor market and reduce

the market wage for low-skilled workers.⁵⁴ Indeed, Rothstein (2007) reports such evidence. Aside from implying that employers get some of the gains from an expanded EITC, this evidence also points to some important considerations with regard to expanding the EITC for unrelated individuals. In particular, if expanding the EITC for unrelated individuals brings more such individuals into the labor market—which is part of the argument (Berlin 2007; Gitterman et al. 2007)—then this would lower low-skilled wages, potentially shifting some of the benefits of the EITC program as a whole away from families, as the eligible participants with children face increased labor supply from unrelated individuals, while at the same time encouraging employment and increasing earnings and income among less-skilled men who are not in families. There may be an argument for shifting the benefits of the EITC in this way, but if so this should be made explicit, rather than suggesting that we can simply do more to increase incomes at the bottom of the distribution by extending the EITC to unrelated individuals, with no tradeoffs. On the other hand, if it can be established that an expanded EITC for unrelated individuals has substantial impacts on the employment margin, the potential benefits from returns to labor market experience that eventually lead to increased earnings and reduced reliance on the EITC might prove a substantial boon to economic self-sufficiency.⁵⁵ Also, if the unrelated individuals who are the target of proposals to expand the EITC are already working, an expanded EITC would likely reduce their labor supply but increase their total income (earnings plus EITC), and this reduction in labor supply would increase low-skilled wages, likely benefiting other EITC recipients as well.

It is also useful to think about the main goals of expanding the EITC for unrelated individuals, and asking whether doing so is likely to be the most efficient policy option. In particular, to the extent that social goals relating to marriage and child support figure prominently in proposals for a more generous EITC for unrelated individuals, it may make more sense to try to reduce directly some of the incentives of the EITC with respect to marriage and to pursue other measures to increase support of unmarried parents for children, as it seems at least possible that “buying” changes in these behaviors through an expanded EITC for unrelated individuals might prove very inefficient. The same argument could be made about crime, although I admit that arguments that we need to do more to increase the costs of crime, rather than

trying to increase the benefits of participating in the licit economy, seem far-fetched in the current U.S. context of massive incarceration.

Aside from the idea of extending the EITC to unrelated individuals, there have also been proposals to structure the EITC differently, to focus on low-wage workers (MaCurdy 2004). As noted earlier, part the motivation for this structure is to try to reduce the labor supply disincentives associated with the EITC for higher-income families that are still eligible. But this idea is also intended to better target families with low-wage workers that cannot, even with full-time work, achieve an acceptable standard of living, and to increase work incentives. MaCurdy discusses two options: a “wage-based” EITC that pays a share of the maximum EITC benefit based on the share of full-time work in a family; and a “wage-subsidy” EITC that pays the difference between a target wage and the worker’s market wage (when it is lower), multiplied by hours worked (averaged for the family). In the context of state add-ons to the federal EITC (in data for California), he finds that the wage-based policy provides similar incentives and benefits for low-wage workers, but greater work incentives for higher-wage workers, while lowering expenses by reducing benefits for families with high-wage and part-time workers, and that the wage-subsidy policy targets families with low-wage workers, and provides stronger work incentives. As MaCurdy notes, however, these alternative EITC programs pose greater administrative challenges, because of the necessity of measuring family labor supply. Nonetheless, these ideas for modifying the EITC merit further consideration because of their potential for boosting earnings among low-income families.

Finally, a new but growing literature on school-to-work provides some support for the potential benefits of school-to-work institutions and programs. Although there is an absence of evidence on longer-run effects, it appears that institutions and programs to improve the school-to-work transition deliver benefits in terms of labor market attachment, skill formation, and higher wages and earnings. However, there are a number of missing pieces of evidence, in particular with regard to the longer-term effects of specific programs. In the past decade, school-to-work efforts were largely dismantled, in favor of test-based educational reforms. The evidence suggests that explicit school-to-work programs—such as those

encouraged by the STWOA, as well as career academies—deliver benefits. This suggests that policymakers concerned with improving education to enhance labor market success would likely do better by pursuing a more balanced mix of strengthening school-to-work institutions and programs along with other educational reforms than has been the case with the either/or approach of federal policy in the last decade and a half.⁵⁶ At the same time, there are two qualifications to this suggestion simply to pursue both types of policies. First, under any circumstances, there will be tradeoffs between school-to-work efforts and a greater focus on academic preparation, as there is a fixed amount of time that students can allocate during the school day. Indeed, concerns that NCLB has cut into the time available for course electives generally, and work-related courses specifically, have already been voiced (for example, Stone and Aliaga 2007, p. 82).⁵⁷ And second, it is essential to remain cognizant of the limitation of school-to-work programs as well as education reform efforts, which serve current students who will work in the future, but not older individuals already in the labor market.⁵⁸

Because I have striven to discuss the evidence on policies to increase economic self-sufficiency via higher earnings, I have of necessity focused on policies already in place (or that were in place recently). Including both those I discussed in detail, and those to which I gave shorter shrift, these policies focus on either mandating higher wages, encouraging work, or increasing skills. I have suggested that we have (or had) in place successful policies to encourage work (the EITC) and to increase skills (school-to-work). On the other hand, I have argued that mandating higher wage floors is ineffective or even counter-productive.

An obvious question is whether there are new policies that have been tried on a much less extensive basis that might hold promise. One I discussed in some detail is the expansion of work subsidies (in the form of the EITC, perhaps) to unrelated individuals, although I have suggested that this may have undesirable distributional effects. Focusing instead on skill formation, Holzer (2007) has advocated a broad-based system of federal grants to encourage skill formation—which he labels Worker Advancement Grants for Employment in States (WAGES). The call for this broad-based program is based in part on fairly compelling evidence of beneficial effects (such as for school-to-work programs or

community college enrollment), and in part on less rigorous evidence pointing to isolated examples of other types of programs that appear to be particularly effective, even if in other locations or settings they are less effective. Recognizing that the research evidence on the effects of these other programs is weak, Holzer has proposed simultaneous adoption of a competitive process for states to receive federal funds, based on program evaluation. There are legitimate questions as to whether building evaluation requirements into the program will be effective in creating a process whereby the most effective programs emerge and “crowd out” the least effective.⁵⁹ Moreover, I think it is likely that Holzer’s back-of-the-envelope calculations about the social returns to investments in these programs are overoptimistic.⁶⁰

Nonetheless, it is probably true that, in the long-run, policies that increase skills are the “holy grail” that can give individuals the capacity to achieve economic self-sufficiency through their own earnings, and that we should be open to some of the new ideas (labor market intermediaries, sectoral training, career ladders, and so on) that Holzer discusses. Especially in light of what is probably a cultural bias in the United States towards meeting the goals of economic self-sufficiency through earnings rather than public support, it is hard to argue against the merit, in principle, of efforts devoted to trying to raise skills of the less-skilled and less-advantaged population. And if a structure can be successfully built that encourages innovative programs, while at the same time being hard-nosed about program evaluation—discarding unsuccessful programs, promoting the successful ones, and killing the whole project if it does not deliver programs that are cost effective and scalable—then we may ultimately learn something useful and make some progress regarding policies to raise skills and increase the economic self-sufficiency of individuals and families through labor market earnings.

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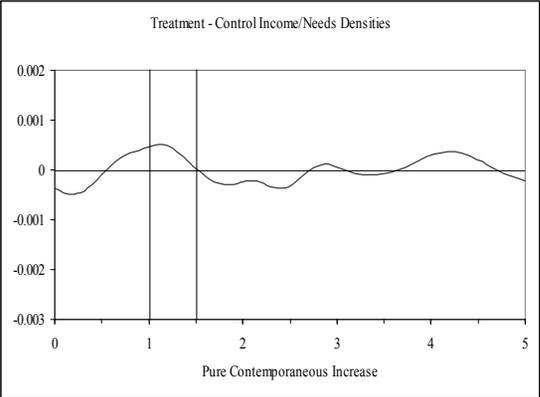
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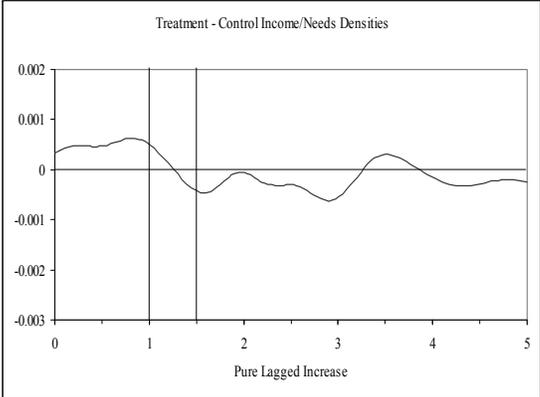
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Figure 1: Estimated Effects of Minimum Wages on Distribution of Family Income/Needs, 1986-1995

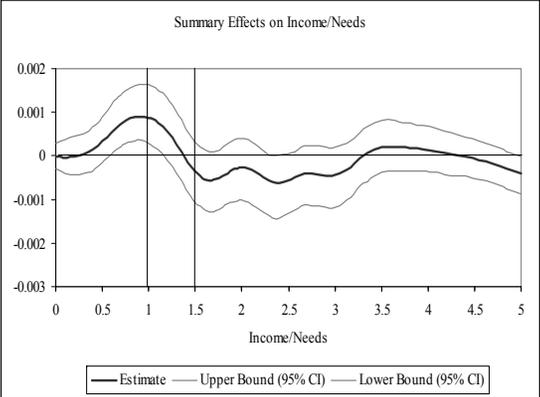
A. Contemporaneous Effects



B. Lagged Effects



C. Combined Effects



Source: Neumark et al. (2005).

Note: The estimates are based on data taken from matched CPS March files for 1986-1995. See the text for explanation.

Table 1: Estimated Effects of Minimum Wage Increases on Proportions in Income-to-Needs Ranges

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

Source: Neumark et al. (2005).

Notes: The data come from matched CPS March files, from 1986-1995. Estimates are constructed by integrating under the densities like those reported in Figure 1. The total sample size for the analysis, including families with income-to-needs up to six, is 196,270. Standard errors are bootstrapped, based on 500 repetitions, with implied t-statistics asymptotically normally distributed. +, *, and ** indicate that estimate is statistically significant at the 10, 5, or 1 percent level.

Table 2: Wages and Family Income-to-Needs

	Income-to-Needs Ranges				
	0-0.5	0.5-1	1-1.5	1.5-2	2-3
	(1)	(2)	(3)	(4)	(5)
<i>A. Distributions of primary earners in family income-to-needs category by hourly earnings:</i>					
Less than 90 percent of minimum	0.49	0.27	0.12	0.06	0.03
90 - 110 percent of minimum	0.17	0.18	0.12	0.05	0.02
110 - 200 percent of minimum	0.25	0.43	0.53	0.50	0.29
More than 200 percent of minimum	0.09	0.12	0.23	0.39	0.66
<i>B. Distributions of lowest earner in family in family income-to-needs category by hourly earnings:</i>					
Less than 90 percent of minimum	0.57	0.52	0.41	0.34	0.25
90 - 110 percent of minimum	0.20	0.16	0.18	0.17	0.14
110 - 200 percent of minimum	0.17	0.26	0.32	0.40	0.45
More than 200 percent of minimum	0.06	0.06	0.08	0.10	0.16
<i>C. Distributions of workers by family income-to-needs:</i>					
Less than 90 percent of minimum	0.13	0.15	0.12	0.11	0.18
90 - 110 percent of minimum	0.08	0.14	0.15	0.11	0.19
110 - 200 percent of minimum	0.03	0.08	0.12	0.14	0.23
More than 200 percent of minimum	0.01	0.01	0.03	0.05	0.16
N	2,979	5,980	8,852	10,741	24,420

Source: Neumark et al. (2005).

Notes: Income-to-needs categories and income measures are reported for year one for each family. Hourly earnings are calculated using annual wage and salary income / {(weeks worked last year)·(usual hours worked last year)}; this way the full March files, rather than only the ORG files, are utilized. In the first and second panels the columns sum to one; in the third panel the rows sum to one but entries are not shown for income-to-needs greater than three. The second panel is restricted to families with at least two earners.

Table 3: Evidence on the Distributional Effects of Minimum Wages

Study	Data	Sample	Findings	Comments
Card and Krueger (1995)	March 1990 and 1992 CPS files	Workers, all families	Larger minimum wage increases associated with poverty reductions, but never significant with controls for overall state employment or unemployment. Slightly stronger evidence of anti-poverty effects for sample of workers only (but still often insignificant).	Conditioning on employment by studying workers masks potential adverse effects of minimum wages.
Burkhauser and Sabia (2007)	Update of Card and Krueger analysis, 1988-2003	Workers, all families	Larger minimum wage increases associated with poverty reductions, but evidence never significant with state unemployment controls. Even for workers, estimated effects near zero and insignificant.	Specifications with state employment controls not included, although these entered more strongly in Card and Krueger's analysis.
Sabia (2006)	March CPS files, 1990-2005	Employed single mothers aged 15-55,	No evidence of effects of minimum wages on poverty.	Burkhauser and Sabia (2007) extend analysis to all single female heads of household, with no significant evidence that minimum wages affect poverty.
Gunderson and Ziliak (2004)	March CPS files, 1981-2000	All families, and subgroups (female-headed households, married couples, white families, black families)	Mixed evidence: some estimates point to minimum wages reducing poverty; but for preferred specification (after-tax income, using squared poverty gap) estimated effect varies in sign and is never significant.	
Wu et al. (2006a)	March CPS files, 1981-1997	All families	For a wide variety of inequality measures (but not all), using after-tax income, minimum wages increase inequality. Evidence strongest for inequality measures that place more weight on transfers at low end of income distribution. Using pre-tax income, minimum wages always estimated to increase inequality.	No year effects included in specifications

Table 4: Living Wage Laws in the Eight Largest Cities, as of 2006

	Level	Prevailing minimum wage	Coverage
	(1)	(2)	(3)
New York	\$10	\$5.15	Service contractors
Los Angeles	\$9.39	\$6.75	Service contractors, financial assistance recipients
Chicago	\$10	\$6.50	For-profit contractors in specific categories of workers
Philadelphia	150% of higher of federal or state minimum wage	\$5.15	Contractors, business with city leases/franchises/concessions, city employees
San Diego	\$10	\$6.75	Contractors, financial assistance recipients
San Antonio	70% of employees in new jobs: \$11.14 (services involving durable goods); \$10.86 (services involving non-durable goods); minimum for all workers is \$9.62	\$5.15	Financial assistance recipients (tax abatements)
Detroit	\$10	\$5.15	Service contractors, financial assistance recipients
San Jose	\$12.27	\$6.75	Service contractors in specific categories, financial assistance recipients

Source: <http://www.livingwagecampaign.org/index.php?id=1958> (viewed November 11, 2006).

Notes: In most cases, the required wage level is higher if health insurance benefits are not provided. The living wage if such benefits are provided is reported. The prevailing minimum wage is the higher of the state or federal minimum.

Table 5: Estimated Effects of Living Wage Laws

Dependent variable:	Log wages, lowest decile of wage distribution (elasticity) (1)	Employment, lowest decile of predicted wage distribution (2)	Probability that family income below poverty (3)
<i>All living wage laws:</i>			
Log living wage, lagged 12 months	0.040	-0.053**	-0.035**
<i>Financial assistance living wage laws:</i>			
Log living wage, lagged 12 months	0.067*	-0.076**	-0.024*
<i>Contractor-only living wage laws:</i>			
Log living wage, lagged 12 months	-0.006	-0.027	-0.038
N	46,374	116,466	142,421

Source: Adams and Neumark (2004).

Notes: The data on labor market outcomes and other worker-related characteristics come from the Current Population Survey (CPS) monthly Outgoing Rotation Group files (ORGs), from January 1996 through December 2002, and the CPS Annual Demographic Files (ADFs), from 1996 through 2002, for individuals or families residing in MSA's, in city-month cells with twenty-five or more observations. The data for the first two columns cover 1996-2002, and for the last column cover 1995-2001. The regressions include controls for city, year, month, minimum wages, and other individual-level controls in the wage and employment specifications, and controls for city, year, and minimum wages in the poverty specification. All specifications also allow differential linear time trends for cities passing or not passing living wage laws, or passing different types of laws. The entries in the first row are from a specification with a single living wage variable, and the entries in the second and third rows are from a specification interacting the living wage variable with dummy variables for the type of living wage. The coefficients for the log wage equation are from log-log specifications, and hence are elasticities. The coefficients from the employment and poverty regressions measure the change in the share employed or poor in response to a one-unit increase in the log living wage (or a 100 percent increase). '***' ('*') superscript indicates estimate is statistically significant at 5 percent (10 percent) level. Reported standard errors are robust to nonindependence (and heteroscedasticity) within city cells.

Table 6: Linear Probability Estimates of Effects of School-to-Work Participation on College Attendance and Employment

<i>A. Detailed Control Variables</i>	Some college		Employment	
	(1)	(2)	(1')	(2')
Job shadowing	.015 (.026)	.037 (.023)	.006 (.025)	-.000 (.025)
Mentoring	.066* (.036)	.026 (.031)	-.035 (.033)	-.029 (.033)
Coop	-.019 (.028)	.007 (.026)	.079*** (.028)	.078*** (.028)
School enterprise	.112*** (.037)	.088*** (.033)	.025 (.037)	.016 (.037)
Tech prep	-.059** (.030)	-.042 (.030)	-.000 (.028)	-.007 (.028)
Internship / apprenticeship	.045 (.032)	.021 (.030)	.053* (.030)	.059* (.030)
Demographic controls included	X	X	X	X
Controls for living arrangements/family structure, ASVAB, and school behaviors included		X		X
<i>B. Expectations Proxies</i>				
Job shadowing	.024 (.028)	.014 (.027)	.017 (.030)	.018 (.030)
Mentoring	.019 (.039)	-.008 (.038)	-.007 (.041)	.008 (.041)
Coop	.021 (.031)	.030 (.030)	.055* (.033)	.052 (.033)
School enterprise	.113*** (.040)	.104*** (.039)	-.025 (.048)	-.019 (.049)
Tech prep	-.046 (.038)	-.016 (.035)	.031 (.033)	.030 (.033)
Internship / apprenticeship	.012 (.036)	.016 (.035)	.052 (.037)	.052 (.037)
High school diploma by age 20		.010 (.072)		.025 (.090)
4-year degree by age 30		.428*** (.035)		-.101** (.043)
Work over 20 hours/week at age 30		.054 (.064)		.226*** (.079)
<i>C. School Fixed Effects</i>				
Job shadowing	.035 (.027)	.063** (.030)	-.019 (.028)	-.026 (.035)
Mentoring	.018 (.034)	.048 (.039)	-.031 (.037)	-.057 (.047)
Coop	.004 (.031)	-.013 (.035)	.075** (.033)	.102*** (.037)
School enterprise	.091** (.038)	.133*** (.048)	-.002 (.047)	-.018 (.056)
Tech prep	-.070** (.036)	-.095** (.040)	.011 (.032)	.036 (.041)
Internship / apprenticeship	.038 (.036)	.055 (.041)	.116*** (.035)	.073* (.043)
Hausman test for excluding school fixed effects, p-value		.18		.24
School fixed effects		X		X

Source: Neumark and Rothstein (2006).

Notes: School and work outcomes are measured as of the post-high school interview (1999 or 2000). The standard errors allow for general heteroscedasticity and non-independence within schools. The sets of control variables are detailed in Neumark and Rothstein (2006). All of the specifications in Panels B and C include the demographic, living arrangement/family, ASVAB, and school behavior variables that are included in columns (2) and (2') in Panel A. Asterisks denote statistical significance of the coefficient estimates, with ***, **, and * indicating that the estimate is significantly different from zero at the one-, five-, and ten-percent levels, respectively.

Table 7: Summary of Effects of STW Participation on Forgotten Half

	Schooling-related	Work-related
	(1)	(2)
Females		
<i>Skill increasing</i>		
Job shadowing		Idle: --
Mentoring		
Coop		Hours: +
School enterprise		
Internship/ apprenticeship	Attended 2-year college: ++	Earnings, uncond.: +++ Wage, uncond.: ++ Earnings, cond.: ++ Wage, cond.: ++
<i>Skill decreasing</i>		
Tech prep	Any college: -	
Internship/ apprenticeship		Training: -
Males		
<i>Skill increasing</i>		
Job shadowing	Weeks in school: ++	Earnings, cond.: +
Mentoring	Any college: + Currently enrolled: + Attended 4-year college: ++	
Coop	Any college: ++ Currently enrolled: +++ Attended 2-year college: +	Idle: ----
School enterprise		Weeks working: + Weeks idle: --
Tech prep	Weeks in school: +	Weeks idle: -
Internship/ apprenticeship	Attended 2-year college: ++	Currently working: +++ Weeks idle: -
<i>Skill decreasing</i>		
Internship/ apprenticeship	Attended 4-year college: --	

Source: Neumark and Rothstein (2007).

Notes: The results shown are those for which the estimated effect was statistically significant at the ten-percent level or better *only* for the forgotten half (or significant with the opposite sign for the forgotten half). The sign is as indicated, appearing three, two, or one times to indicate that the estimate for the indicated group is significantly different from zero at the one-, five-, or ten-percent level, respectively. In all cases, effects that increase schooling, work, skills, or earnings are included in the rows labeled “skill increasing,” and vice versa.

Endnotes

- ¹ Of course shifts in aggregate demand will have the same effect, but my focus is on labor market policies.
- ² Aside from what the government can do, there is a large and growing literature on the effects on firms and workers of what are often called “high-road” work practices that lead to more productive and higher-wage jobs (see, for example, Cappelli and Neumark 2001; Handel and Levine 2004). However, although policies to encourage firms to adopt such practices are often advocated (for example, Osterman et al. 2002), it is unclear exactly what policies would achieve this. One argument that is sometimes made is that a higher minimum wage would encourage employers to adopt such practices, by raising the productivity level at which it is profitable to hire a worker (Bernstein 2000; Fitzgerald 2006), in which case high-road practices might lead to skill upgrading. Of course a higher minimum wage increases the productivity of labor in the neoclassical model with no necessary implications for what practices firms adopt, as firms move up the labor demand curve; and there is no direct evidence that minimum wages change firms’ practices. Another tactic that has emerged is corporate responsibility campaigns by community groups or labor unions to encourage the creation of high-wage jobs; see, for example, <http://www.workingforamerica.org/documents/Journal4/regional.htm> (viewed May 2, 2007).
- ³ Regarding welfare reform, see, for example, Blank (2002); regarding training, see, for example, Heckman et al. (1999). For a very recent discussion of adult training and other workforce development policies, see the volume edited by Holzer and Nightingale (2007).
- ⁴ In addition, Bitler et al. (2006) point out the importance of heterogeneity in these effects.
- ⁵ See <http://www.us.es.doleta.gov/wotcdata.cfm> (viewed May 2, 2007).
- ⁶ For evidence on efforts to increase schooling levels at the post-secondary level, see, for example, Cornwell et al. (2006). One policy effort to increase primary or secondary schooling of disadvantaged youths is the “learnfare” program, implemented in a handful of states. Under learnfare, welfare benefits can be cut if children of recipient families are not in school.
- ⁷ See <http://www.dol.gov/esa/minwage/america.htm> (viewed May 2, 2007).
- ⁸ See Card and Krueger (1994), and also Neumark and Wascher (2000).
- ⁹ See Abowd et al. (2000), Currie and Fallick (1996), and Neumark et al. (2004).
- ¹⁰ The review also discusses in some detail our efforts to explain some of the conflicting findings in the literature.
- ¹¹ Since this review was published, additional research on both sides of the debate has continued to appear. A couple of provocative papers that continue to challenge the conclusion that minimum wages reduce employment among the least skilled are Allegretto et al. (2008) and Dube et al. (2008).
- ¹² For example, in response to a presentation of the findings from the minimum wage review just discussed, Jared Bernstein of the Economic Policy Institute, which advocates a much higher minimum wage, responded “The minimum wage increase will invariably hurt some of its intended beneficiaries ... [But] the benefits will often outweigh the costs, even for narrowly-affected workers.” See http://www.aei.org/events/eventID.1430,filter.all/event_detail.asp# (viewed June 11, 2007).
- ¹³ See Neumark and Wascher (2002) for a more-detailed discussion and illustrative calculations. The same arguments would apply to hours effects.
- ¹⁴ The sample conditions on the initial wage, and hence the models are estimated for those initially working.
- ¹⁵ See Figure 1 of Neumark et al. (2004).
- ¹⁶ Near-poor families are defined as those between 1 and 1.5 times the poverty line.
- ¹⁷ For the full set of figures, see Figure 1 of Neumark et al. (2005).
- ¹⁸ A more detailed discussion is provided in Neumark and Wascher (2008). I omit from Table 3 a study by Addison and Blackburn (1999) that focuses only on narrow subsets of families and therefore does not speak to the overall effects of minimum wages on poverty or the income distribution.
- ¹⁹ There are some related questions about the effects of minimum wages on the family income distribution for different types of families. For example, minimum wages may have different effects on rural and urban families. Wu et al. (2006b) report evidence suggesting that minimum wages increase pre-tax and post-tax family income inequality in urban areas, and more so when more weight is put on the lower end of the distribution. These results for urban areas are consistent with their aggregated results presented in Table 3. However, they find very small and insignificant distributional effects of minimum wages in rural areas. Gunderson and Ziliak (2004) contrast some of their results for female-headed versus married-couple families. Across their different specifications, there is no clear pattern of differences across family structure in how the minimum wage affects the poverty rate or the squared poverty gap, and none of the estimates by family structure are significant.
- ²⁰ The most recent development with regard to living wages has been the advent of city-level minimum wages—that is, broad minimum wage floors just like state minimum wages, but enacted at the city level. Santa Fe and San Francisco enacted a minimum of \$8.50 in 2003 and 2004, respectively, with both set to rise through indexation and (in Santa Fe) additional planned increases in the legislation. Madison and other smaller towns in Wisconsin

also recently passed minimum wage laws, but they were subsequently repealed by state laws. A city minimum wage in New Orleans was approved by voters in 2002, but subsequently blocked by a state law. (Washington, DC has its own minimum wage, but is often treated as a state in state-level analyses of minimum wage effects like those described earlier.)

- ²¹ See the summary of coverage estimates in Adams and Neumark (2005a) and in Freeman (2005). For more systematic estimates of coverage by the living wage laws in Los Angeles and San Francisco, respectively, see Fairris et al. (2005) and Alunan et al. (1999).
- ²² A very recent review of the living wage literature, which also discusses some other studies, is provided in Holzer (2008).
- ²³ It might be viewed as curious that the estimate for contractor-only living wage laws, although insignificant, is larger, in light of the smaller wage and employment effects for contractor-only laws reported in columns (1) and (2). But the offsetting positive wage effects and negative employment effects of financial assistance living wage laws imply that these laws need not have a stronger effect on poverty. Nonetheless, the estimate for contractor-only laws is puzzling in light of the absence of wage or employment effects of these narrower laws.
- ²⁴ The size of some of these estimated effects may seem surprisingly large, given relatively low coverage by living wage laws. With respect to poverty reductions, Adams and Neumark (2004) explain that their estimates are of the same order of magnitude suggested by Brenner's (2005) calculations based on detailed data for Boston. With respect to wage and employment effects, the estimates are large given coverage estimates. One possibility is that there are non-neoclassical influences, so that living wage laws affect community norms for wages; one channel for this may be that firms desirous of future contracts or development subsidies believe it is advantageous to pay higher wages (Bartik 2004). At the same time, some critics have grossly overstated the implications of these estimates for employment declines. Fairris and Reich (2005, p. 10) incorrectly calculate that the Adams and Neumark estimates imply huge employment losses. In fact, the 35 percent living wage increase that they consider, for a financial assistance living wage law, is estimated to lead to a 6 percent employment decline among those in the bottom tenth of the skill distribution (.35 multiplied by the employment effect of $-.076$ reported in column (2) of Table 5, divided by the .44 employment rate in the bottom tenth of the skill distribution). This contrasts with the 91 percent employment decline that Fairris and Reich claim is implied by the estimates.
- ²⁵ Teenagers may also be myopic, putting too much emphasis on the potential higher earnings in the minimum wage sector following a minimum wage increase, with insufficient attention to the cost in terms of foregone higher earnings from more education.
- ²⁶ As confirmatory evidence, this negative effect was present for observations in states with compulsory schooling ages less than eighteen (where teenagers have more choice about leaving school), but not in states with a compulsory schooling age of eighteen (for which the estimates were smaller and insignificant, although still negative).
- ²⁷ These parameters determine the level of income at which the credit falls to zero, which was \$36,348 in 2006, for a family with two children.
- ²⁸ The nineteen states with EITC supplements in 2007 were Delaware, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Massachusetts, Minnesota, Nebraska, New Jersey, New Mexico, New York, Oklahoma, Oregon, Rhode Island, Vermont, Virginia, and Wisconsin, and the supplemental EITC in those states ranged from 4 to 43 percent. See Neumark and Wascher (2009). In addition, EITC supplements became effective in 2008 in Louisiana, Michigan, and North Carolina.
- ²⁹ The usual substitution effect occurs as the higher wage raises the price of leisure and induces more labor supply, in the case of those originally non-employed leading some to enter the labor market. The income effect, however—which typically induces a labor supply reduction as a result of the higher wage leading to more consumption of all normal goods, including leisure—does not have an impact on those originally working zero hours.
- ³⁰ A rare exception that fails to find employment (or hours) effects is Cancian and Levinson (2005).
- ³¹ For this reason, MaCurdy (2004) considers EITC alternatives that target low-wage workers. These alternatives are discussed more in the concluding section.
- ³² This evidence emerges in specifications that rely on the state-level variation in EITC parameters; the paper argues that federal variation is confounded with other policy changes. Grogger (2003) reports confirming evidence, finding that higher EITC payments increase employment, hours, and earnings of female-headed households.
- ³³ This effect is perhaps a little implausibly large. As reported in the paper, the estimated effect tapers off considerably with increased generosity.
- ³⁴ Wu et al. conclude that higher marginal tax rates have larger beneficial redistributive effects than the EITC. They find adverse effects of AFDC and Food Stamps, which they attribute to work disincentives. They also find that a higher phase-out rate increases inequality for nearly every inequality measure, presumably because a higher

- phase-out rate reduces labor supply among moderate-income families.
- ³⁵ Gunderson and Ziliak (2004) find mixed evidence on the distributional effects of the EITC. Their evidence is more consistent with the EITC increasing the poverty rate, using the before- or after-tax measure (the latter incorporates the credit), although only some of the estimates are statistically significant. For the squared poverty gap, with the before-tax measure there are no significant effects of the EITC, whereas for the after-tax measure there is some evidence consistent with a reduction in the gap, although curiously this emerges for married-couple families, which is unexpected. Their specification may be problematic because it does not appear to allow the effects of the EITC and other policies to vary with the number of children in the family, which in Neumark and Wascher (2001b) appeared to be quite important. They use the difference between the logs of the state and federal maximum EITC benefit, although it is not clear for what type of family, or whether this is averaged over families in each state-year cell.
- ³⁶ And Dickert-Conlin and Holtz-Eakin (2000) present evidence that offering wage subsidies for low-skill workers is also not particularly effective at targeting poor families.
- ³⁷ The NJTC did not target specific workers, but rather new employment. But it created stronger incentives to hire low-wage workers by applying only to the first \$4,200 of wages per employee (in 1977 and 1978).
- ³⁸ There are also similar efforts tied to welfare reform—at the federal level, with the Welfare to Work Tax Credit.
- ³⁹ However, Hamersma (2005a) summarizes a 2001 GAO report suggesting little evidence of such churning, as very few workers receiving subsidies stayed long enough to get the maximum subsidy, and hiring and training costs appeared to render such a strategy cost ineffective.
- ⁴⁰ Many researchers have noted that most EITC recipients choose to take their payment as a lump sum at the end of the year rather than in each paycheck. It is conceivable that part of the explanation for this is the avoidance of stigma effects.
- ⁴¹ Heckman et al. (1999) provide an extensive review of the literature documenting this general result, based on data for both the United States and Europe. (See also the brief summary in Holzer (2007), who concurs with this summary of the findings from the literature on training programs.) Heckman et al. summarize cost-benefit calculations indicating that training programs for adults sometimes appear to deliver positive social returns. Recent evidence from evaluations of the Job Corps program appear to indicate gains for twenty to twenty-four years olds, but not those younger (see Krueger 2006).
- ⁴² The federal funding was intended to serve as seed money to establish school-to-work transition systems that included formal partnerships among secondary and post-secondary institutions and employers. Research has established that in many states the legislation did spur the development of such systems (see, for example, Hershey et al. 1999; Neumark and Allen 2003; Neumark 2006).
- ⁴³ In contrast, vocational education (later called career and technical education) was characterized by its isolation from more comprehensive high school curricula (Hayward and Benson 1993), while Tech Prep pursued limited integration of vocational and academic education by introducing some vocational education into comprehensive high school curricula while sequencing vocational education during high school with two years of further related study at post-secondary institutions (Ryan 2001).
- ⁴⁴ As explained in Neumark and Rothstein (2006), these expectations variables can also be interpreted as ideal proxies, the inclusion of which may eliminate bias from endogenous selection into school-to-work participation. The idea is that any remaining variation in unobservables net of these expectations is only forecast error, which should be uncorrelated with the exogenous variables in the information set.
- ⁴⁵ The link between school enterprise and college enrollment is not obvious. However, research by Stern (1984) and Stern et al. (1994) reports that enterprise-based jobs are more closely related to students' education than are out-of-school jobs, and that enterprise-based jobs provide more opportunity to apply what students are learning in school; so school enterprises may be a particular type of school-to-work program that enhances the educational experience and therefore encourages higher education.
- ⁴⁶ We found that very few variables were predictive of participation in school-to-work programs; the one important exception was that blacks were more likely to participate in coop, school enterprise, and Tech Prep programs. These results suggest that there may be little systematic selection into school-to-work programs, which would explain why the results are insensitive to adding controls.
- ⁴⁷ Of course this share is not necessarily one-half of the appropriate population. But the “forgotten half” phrase was introduced by the William T. Grant Foundation (1988), and this characterization has stuck in subsequent work. (See, for example, Donahoe and Tienda 1999.)
- ⁴⁸ Kemple (2005) reports that there are approximately 2,500 career academies across the country. He does not report any direct enrollment numbers, but suggests that academies typically include 150-200 students. These numbers suggest that there are around 440,000 students in career academies, out of approximately sixteen million high school students. (The latter number comes from Digest of Education Statistics,

- http://nces.ed.gov/programs/digest/d04/tables/dt04_002.asp, viewed May 14, 2007; Kemple does not give a date (or source) for his estimate.) The National Center for Education Statistics (NCES) reports 4,800 secondary schools with career academies, nearly twice Kemple's estimates (<http://nces.ed.gov/surveys/ctes/tables/h14.asp>, viewed May 14, 2007). However, it is not clear what the differences are in the definitions of career academies.
- ⁴⁹ The study covers nine schools across the country, all located in or near urban areas.
- ⁵⁰ These results appear to be for men and women combined.
- ⁵¹ Note that this evidence for men contrasts with the findings in Orr et al. (2007) pointing to positive effects of the National Academy Foundation (NAF) career academies on four-year college attendance. It is unclear how much this difference is attributable to the random assignment, or to differences in the types of academies studied. In particular, Orr et al. suggest that the NAF academies cover particular fields (especially finance, which was the type that boosted college attendance), and may differ in terms of other program characteristics as well, given that the academies they study were relatively homogeneous and met criteria for implementation of the NAF model.
- ⁵² Scholz proposes targeting lower-income families with this subsidy by restricting eligibility to individuals living in federally-designated Renewal Communities, Empowerment Zones, or Enterprise Communities. However, his analysis suggests that a fairly high share of the gains would go to families above the poverty line.
- ⁵³ A modest version of this policy was recently enacted as the "Making Work Pay Tax Credit."
- ⁵⁴ It is also worth pointing out, in relating the literature on minimum wage employment effects to the EITC, that the less elastic one thinks the demand for low-skilled labor is, the more one would expect wages for low-skilled workers to fall in response to an expanded EITC.
- ⁵⁵ This is speculative, but is in some sense the flip-side of the results in Neumark and Nizalova (2007) suggesting that minimum wages may have long-term adverse effects on wages and employment, stemming in part from lost work experience.
- ⁵⁶ Although difficult to establish rigorously, it is also my impression that the \$1.5 billion in federal funds for the STWOA seems to have had rather dramatic policy impacts in terms of spurring the creation of school-to-work programs in the states, presumably because the seed money provided by the STWOA interacted with other incentives for states to create school-to-work programs.
- ⁵⁷ Figlio (2007) discusses a number of studies that address the issue of tradeoffs between resources devoted to accountability programs and other activities in schools.
- ⁵⁸ This qualification is even more relevant to discussions of the potential higher efficacy of early (that is, childhood) human capital investment (for example, Carneiro and Heckman 2002).
- ⁵⁹ Neumark (2006) presents an analysis of program evaluation of school-to-work programs in California that raises warning flags about the implementation of evaluation mandates. On the other hand, Blank (2002) argues that the process of experimentation and evaluation was very successful in the arena of welfare reform.
- ⁶⁰ In particular, his calculations are based on estimates of gains from programs that are apparently the highest among the estimates he surveys, and in each case come from an isolated program when estimates of the earnings gains from similar programs implemented elsewhere were smaller and often insignificant (see p. 20 of his paper). Whether these high estimates reflect best practices or simply statistical outliers, it is probably unreasonable to presume that gains of these magnitudes will be replicated across-the-board, even with the best institutional structure in place.