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ABSTRACT

We provide the first direct estimates of the effects of minimum wages on low-wage workers in families at different points of the distribution of income-to-needs, using data from the Survey of Income and Program Participation, which oversamples low-income families. We find adverse – rather than beneficial – effects of minimum wages on the employment, hours, and earnings of initially-employed low-wage workers in poor and low-income families. Although we do not find a gradient indicating more adverse effects on the poorest low-wage workers, the adverse effects for poor and low-income low-wage workers help explain why minimum wages do not reduce poverty.

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Introduction

Most research on the effects of minimum wages in the United States focuses on effects on employment, with scores of papers written. However, the effect of minimum wages on poverty, or the distribution of family income more generally, is the more important policy question. Even if – as most research suggests – the minimum wage reduces employment of less-skilled workers (see the recent review by Neumark and Shirley, 2022), the minimum wage may increase incomes among low-income families, as long as the wage gains for these families outweigh any employment or hours declines.[1] There is a smaller – but growing – number of papers on the distributional effects of minimum wages. The most recent one of these (Burkhauser et al., forthcoming), like most prior work on this topic fails to find an impact of the minimum wage in reducing poverty.[2]

The research on minimum wages and poverty by and large simply estimates reduced-form effects of minimum wages on poverty rates (e.g., at the state-by-year level) or the probability that a family is poor. This research sometimes expands the lens to look at extreme poverty (below 0.5 times the poverty line) or near poverty (below 1.5 times the poverty line). However, we have relatively little understanding of why minimum wages seem to fail to help poor or low-income families. Clearly the answer to this question lies in understanding how low-wage workers in lower-income vs. higher-income families are affected by the minimum wage. But the existing evidence on this question is limited, and it is indirect.

One indirect type of analysis suggesting why minimum wages may have limited positive

[1] There is far more U.S. literature on employment effects than on hours effects. For the latter, Couch and Wittenburg (2001) report evidence of hours declines for teenagers, Neumark and Nizalova (2007) report evidence of negative longer-run effects on hours of exposure to high minimum wages when aged 16-24, but Zavodny (2000) does not find adverse effects on hours for teens.
[2] Dube (2019) is an exception, but Burkhauser et al. (forthcoming) provide evidence that his results are very fragile and that most evidence confirms that absence of beneficial distributional effects.

distributional effects has a long-standing history, beginning with Gramlich (1976). Gramlich demonstrated the basic fact that minimum wage workers are not very strongly concentrated in poor or low-income families. Subsequent research has confirmed this finding in more recent data (Burkhauser and Sabia, 2007; Sabia, 2014; Lundstrom, 2017), and explored it further. For example, in the typical kind of simulation done in this literature, where wages are topped up to a proposed minimum wage, assuming no other adjustments, Lundstrom (2017) estimated that only 16.8% of the increased "wage bill" from an increase in the minimum wage would flow to poor families. As highlighted by this research, a key reason that minimum wages do not target poor families well is that teenagers are a highly disproportionate share of minimum wage workers, but are not particularly strongly concentrated in the low-income families.[3] In addition, many poor families have no workers, and low-wage workers in poor families tend to work low hours (Burkhauser and Sabia, 2007).[4] This is not a controversial point; even Card and Krueger (1995) concede that the minimum wage is a "blunt instrument" for redistributing income to poor families (p. 285).

A second, even less direct approach to exploring how minimum wages might affect poor or low-income families relies on the estimated elasticity of employment with respect to the minimum wage. As Freeman (1996) pointed out, if the elasticity of employment with respect to the minimum wage – for workers affected by the minimum wage – is well below 1 in absolute

[3] This is confirmed in our data (Table 5, discussed below).
[4] This is confirmed in our data (Tables 3 and 4, discussed below). Strikingly, Burkhauser and Sabia (2007, Table 1) show that, historically, the minimum wage was much more effective at targeting poor families. In their simulation, in 1939, 85% of the benefits went to poor families – a percentage that declined steadily over the years, to 17% in 2003. They attribute the change to low-wage workers increasingly becoming "second or even third workers in nonpoor households;" in addition, increasingly over time, even when low-wage workers headed households, "the labor earnings of other household members as well as the income from other household sources usually pushed their household's income above the poverty line" (p. 266). Reflecting these points, they also show that the targeting of benefits from a higher minimum wage on *working* single mothers is far better; over half of the beneficiaries among this group have incomes below the poverty line.

value, then earnings of affected workers will be strongly positively affected by the minimum wage. While this does not speak to the distribution of these earnings effects across families at different points of the income distribution, it does provide a focus for what is at least a prerequisite for minimum wages to reduce poverty – that they increase earnings. Freeman asserted that because minimum wage-employment elasticities are often in the range of −.1 to −.2, we should expect strong positive effects of minimum wages on earnings. However, these elasticities are typically estimated for groups (such as teenagers or young adults) among whom a minority of workers are affected by the minimum wage. And the "legislated" minimum wage increase used to compute these elasticities is typically far greater than the induced wage increase for these groups, with, for example, wage elasticities of around 0.2 for restaurant workers (Jha et al., forthcoming) or teenagers (Manning, 2021). Thus, we need to divide the traditional estimated employment elasticity by the product of the wage elasticity and the proportion of workers directly affected, which can get the relevant elasticity much closer to −1 (Neumark and Wascher, 2008, Chapter 3). As more direct evidence that the right elasticity is in fact closer to −1, Clemens and Wither (2019) estimate the employment elasticity *only* for directly affected workers, and obtain an estimate very close to this value.[5] However, while this estimate would imply that, on average, minimum wages do not increase earnings of affected workers, it does not necessarily imply that there are not earnings gains concentrated among minimum wage workers in lower-income households.

Other reasons why the minimum wage fails to reduce poverty have not been explored – and cannot be using these prior indirect approaches. One possible reason is that job losses from

[5] This is roughly consistent with earlier research (Neumark et al., 2004), which found that higher minimum wages resulted in somewhat lower earnings for very low-wage workers (up to 1.1 times the minimum wage), owing more to hours than to employment declines.

higher minimum wages are concentrated on workers in the lowest-income families. Poverty may not increase because these workers are already poor, but it would not decline. What should we expect? Carrington and Fallick (2001) showed that most minimum wage workers are simply inexperienced workers who have recently left school, and quickly move out of minimum wage jobs. However, a small share of workers remain in minimum wage jobs longer term – disproportionately so for blacks, those with less education, and women with young children. The same characteristics are associated with higher poverty rates, so one way to frame the question is whether these "longer-term" minimum wage workers are more likely to experience job loss when the minimum wage increases. It is conceivable that longer-term minimum wage workers are perceived as less productive than new, young, and inexperienced minimum wage workers – who may have upward productivity potential. If so, then job loss from higher minimum wages may be more concentrated among poorer minimum wage workers. Alternatively (or additionally), some job loss may occur among families that are relatively low income but initially non-poor – as suggested by the evidence in Neumark et al. (2004) – and their resulting declines into poverty may offset positive earnings effects on workers in poorer families from higher minimum wages.

The implications of these alternative possibilities for families are different. In the first case – when job losses are concentrated among the poor, poor families are likely made worse off even though poverty does not increase.[6] In the second case, with job loss among families above the poverty line, the poor are not necessarily made worse off, although other families are. Thus, evidence on employment effects of minimum wages (and other margins of adjustment) in different parts of the family income distribution can be quite informative about the effects of the

[6] We say "likely made worse off" because the job loss is concentrated among poor families, even though some surely experience wage gains. This is a case where the application of a conventional employment elasticity like −0.2 would badly understate job loss among the poor.

policy, in addition to helping us understand the null effects of the minimum wage on poverty.

In this paper, we directly estimate the effects of minimum wages on low-wage workers in families at different points of the distribution of income-to-needs (I/N). To the best of our knowledge, we are the first to do this. One reason researchers may have not turned to this question before is that this kind of analysis requires both longitudinal data and very large sample sizes. We need to differentiate between effects of minimum wages on lower-wage and higher-wage workers at different points of the income distribution, and some of these combinations – perhaps most obviously higher-wage workers in lower-income families, and lower-wage workers in higher-income families – are less prevalent. Moreover, Gramlich's (1976) work and similar later work establishes that a large share of low-wage workers are in non-poor families – reinforced by the high share of poor families with no workers, and the much larger share of non-poor than poor families. This can limit the number of observations on low-wage workers in poor families, even though workers in poor families are more likely to earn lower wages.

Researchers who study the effects of minimum wages on employment, wages, earnings, etc., of low-skill workers have typically used monthly Current Population Survey (CPS) data, sometimes further restricted to the Outgoing Rotation Group (ORG) samples, which are one-quarter of the full monthly samples, when wages are needed. And researchers focused on poverty use the March Annual Social and Economic Supplements (ASEC) CPS files. Thus, marrying the two to have information on both hourly wages and family income would take us to one-quarter of the March files (the overlap with the ORGs).[7]

We substantially ameliorate the problem of low numbers of observations by using data

[7] In principle, the ASEC files can be matched to later rotation groups in April, May, and June, but there is attrition and the CPS does not follow individuals over time if they move (for details, see Neumark and Kawaguchi, 2004), resulting in even smaller samples (and possible selection bias, although Neumark and Kawaguchi do not find bias in the relationships they study).

from the Survey of Income and Program Participation (SIPP), which strongly oversamples low-income families and does not restrict wage information to a subsample. Within a primary sampling unit (a county or group of counties), the SIPP designates low-income (with a high concentration of households at less than 150 percent of the poverty threshold) strata and high-income strata. In the 2004 SIPP, low-income strata were sampled at 1.38 times the rate of high-income strata. In 2008, this rose to 1.44, and 1.47 in 2014.[8] To be more specific, we use 2004, 2008, and 2014 SIPP panels, covering the years 2003 (partial) through 2016, excluding part of 2008.

Table 1 provides comparisons of the observations available in the alternative data sets. For the three SIPP panels, there are typically around 500,000 or more observations per year. The ASEC files have roughly one-quarter that number or less, and the overlap with the ORG files cuts the number of observations substantially more.[9] Table 2 presents sample sizes for low-wage workers and by income-to-needs bins for some states of different sizes, to illustrate the advantage of the SIPP relative to the ASEC/ORG data.

Our approach is to classify workers by both their wages and their family incomes, and then to estimate models of minimum wage effects on low-wage workers in different parts of the distribution of family income relative to needs. We have a few key findings.

First, higher minimum wages lead to declines in the probability that low-wage workers

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[8] See: https://www.census.gov/programs-surveys/sipp/methodology/sampling.html; https://www2.census.gov/programs-surveys/sipp/guidance/2008-sipp-users-guide.pdf; https://www2.census.gov/programs-surveys/sipp/tech-documentation/source-accuracy-statements/2008/final.-stamped.-survey-of-income-and-program-participation-(sipp)-2008-panel-source-and-accuracy-statement-for-longitudina.pdf; https://www2.census.gov/programs-surveys/sipp/tech-documentation/methodology/2014-SIPP-Panel-Users-Guide.pdf.

[9] The CPS covers approximately 60,000 households, or 110,000 individuals each month (https://www.bls.gov/cex/cecomparison/cps_profile.htm). The ASEC numbers in Table 1 include the oversample of the ASEC. Without the oversample, there are around 85,000-100,000 respondents/month (aged 15-70). The ORG covers approximately one-quarter that number, but not all respondents are employed, so the values in the table reflect the roughly one-quarter eligible and 65% employed and eligible to give work information.

remain employed. These effects are negative for all of the income-to-needs bins we consider: 0-0.5, 0.5-1, 1-1.5, 1.5-2, 2-3, and 3+. While this evidence indicates that higher minimum wages reduce employment of workers below the poverty line, there is not a clear gradient between employment declines and income-to-needs, so the effects are not *more* adverse for the poorest individuals.

Second, higher minimum wages lead to hours declines for initially-employed low-wage workers. These declines, like for employment, are evident throughout the income-to-needs distribution. The implied hours reductions are larger than the employment reductions – measured in terms of elasticities. There is evidence of hours declines for the poorest individuals, and the hours declines appear in some cases to be larger among those in higher income-to-needs bins, rather than the reverse.

Third, higher minimum wages lead to earnings declines for initially-employed low-wage workers, throughout the income-to-needs distribution. There is evidence of earnings declines for the poorest individuals, but the earnings declines appear in some cases to be larger among those in higher income-to-needs bins. Earnings declines conditional on employment are sizable, and not that much smaller than the unconditional estimates. The negative earnings effects conditional on employment have to be attributable to the sizable hours declines we find. This evidence suggests that the failure of minimum wages to reduce poverty is less a consequence of the concentration of adverse effects among those with those with the lowest income-to-needs, and more a consequence of minimum wages failing to increase earnings even among those employed, and reducing earnings overall.

In terms of the questions that motivate this paper, our evidence helps explain why minimum wages do not reduce poverty. We do not find evidence of adverse effects of minimum wages that are strongest for the poorest workers. Nonetheless, we do find adverse effects of minimum wages on the employment, hours, and earnings of poor and low-income workers. Most importantly, none of our evidence points to beneficial effects of higher minimum wages for low-wage or low-skill workers in the lowest-income families – effects that, if present, would suggest that higher minimum wages can reduce poverty.

Data

We use data from the 2004, 2008, and 2014 SIPP panels, which, as indicated in Table 1, cover the period from October 2003 to December 2016. The SIPP includes labor market information on a monthly basis. We begin with the 2004 SIPP because it is the first panel in which all 50 U.S. states and the District of Columbia are identified. We conclude our sample with the 2014 SIPP, because it is the last full panel prior to the COVID-19 pandemic.[10] In the 2004 and 2008 SIPP panels, respondents were contacted every four months and asked to report information for each of the preceding four months. In the 2014 SIPP panel, respondents were contacted once per year and asked to report information for each month of the preceding calendar year. This monthly structure allows us to link poverty, employment, and wage information over a period of several months (or more), which is essential for our analysis because we ask how labor market outcomes evolve for workers whose wages are initially below a higher minimum wage level that is imposed (i.e., who are "caught" by a higher minimum, in

[10] The format of the SIPP panel changes beginning with the 2018 panel, such that new households are added each year, rather than following the same set of households in each year of data. Since the 1996 panel, and prior to the 2018 panel, SIPP panels largely did not cover overlapping time periods, with the exception of some months in 2013 being covered in both the 2008 and 2014 panels. For the most part, prior to 2018, a "panel" consisted of the same set of households surveyed over the reference period, without overlap in reference periods across panels. The 2018 panel changed to an overlapping format, such that the 2018 SIPP would include wave 1 of the 2018 panel, the 2019 SIPP would include wave 2 of the 2018 panel and wave 1 of the 2019 panel, etc. Subsequent SIPP data thus includes both respondents from prior-year panels and new respondents from new panel years. For convenience, we use the last full panel prior to 2020. See the 2018 SIPP Users' Guide for further information (https://www2.census.gov/programs-surveys/sipp/tech-documentation/methodology/2018_SIPP_Users_Guide_SEP23.pdf).

the language – and framework – of Kramarz and Phillipon, 2001).

Respondents who report being paid by the hour (as opposed to a set annual salary or other calculation of pay) report their hourly wage. Respondents who are paid a set annual salary or some other form of compensation report usual monthly earnings and usual hours worked per week, which we use to calculate the hourly wage as usual monthly earnings divided by the product of usual weekly hours worked and the number of weeks in the month.

We only use respondents who report usual week hours, excluding those who report that weekly hours vary.[11] In cases where "usual hours vary," respondents are excluded from hours calculations, and hence we do not have a wage if it would have to be calculated; of course, if an hourly wage is reported, we use that value/observation. In the 2014 panel, more information is available on usual weekly hours for job #1, so that "hours vary" is never a response.[12] The 2014 panel also asks information about up to six additional jobs, while the earlier panels include information on up to two jobs total. Since we are most interested in the employment margin, which we can determine from job #1, for consistency and the cleanest information on employment, wages, and hours, we only use information on job #1 for all panels.

We calculate family poverty status by dividing total monthly family income by the family poverty threshold. Family income is the total reported monthly earning and income amounts received by all members of the family in the month, from all income and earnings sources.[13] The

[11] "Hours vary" is only a response in the 2004 and 2008 panels. 413,486 respondents (10.5%) indicated their usual weekly hours varied, out of 3,924,241 respondent observations on usual work hours in the two panels.

[12] In the 2014 panel, respondents are asked the average number of hours they worked each week in that month and up to three values of hours worked per week (for job #1). If work hours changed during the reference period, respondents can indicate multiple values of work hours. When respondents indicated that their usual hours worked in that month were 0, we instead used the first value of hours worked (this is the case for 14,509 (1.3%) of the 1,086,003 responses in the 2014 panel regarding usual work hours). In the 2004 and 2008 panels, respondents are only asked about usual work hours (and may report that "hours vary"), and do not provide multiple values of hours worked.

[13] We verified that our income definition lines up with the poverty ratio variables included in 2014 SIPP (though they begin at 0). (The 2004 and 2008 panels include the family poverty threshold for respondents each month, but

family poverty threshold is a SIPP-provided variable that assigns the dollar value used as a threshold for determining poverty based on the size of the family and number of children in the family.[14]

Empirical Approach

We study workers, classifying them by their position in the family income-to-needs distribution and by whether they are low-wage and affected by minimum wage increases. We then estimate impacts of minimum wage increases on employment, hours, and earnings of affected workers that vary across the income-to-needs distribution.

The identification strategy is to estimate the effects of the minimum wage in a set of bins for the income-to-needs distribution. The bins are: 0-0.5, 0.5-1, 1-1.5, 1.5-2, 2-3, and 3+.[15] Within each bin, we use variation in the "intensity" of treatment at the individual level, captured in *wgap*, which is the percentage difference between the minimum wage and the worker's wage three months ago. We are thus defining workers as those employed three months prior to any given minimum wage increase; we chose this window because the effects of minimum wage increases may not be immediate.[16]

Our specification can thus be interpreted as a difference-in-differences type of estimator for each income-to-needs bin, with the addition of the intensity of treatment measure. Suppose we only had one income-to-needs bin. Then our specification, for generic outcome Y, for

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they do not report the income-to-needs ratio.) When calculated income-to-needs ratios are negative, we retain the negative value rather than setting it to 0, but include it in the lowest poverty bin.

[14] See the threshold values used by the Census Bureau by year here: https://www.census.gov/data/tables/time-series/demo/income-poverty/historical-poverty-thresholds.html.

[15] In each case except the first and last, the ranges are defined as lower than the upper bound, e.g., $\geq .5$ and < 1.

[16] Individuals with business earnings can be included in our analysis of wages, employment, hours, and earnings regressions if they reported working in a wage or salary job in *t*-3. As a result, there is a very small percentage of cases with negative family income (who we include in the 0-0.5 bin), because we include self-employment income. There are 14,151 observations with negative family income, 2.3% of the 604,459 observations in the 0-0.5 income-to-needs bin.

individual *i*, in state *s*, in month *t,* would be:

$$(1) \qquad Y_{ist} = \alpha + \sum_{k=1}^{3} \rho^k DMWI_{s,t-k} + \sum_{k=1}^{3} \theta^k wgap_{is,t-3} \cdot DMWI_{s,t-k} + D_s \lambda_s + D_t \cdot \pi_s + \varepsilon_{ist}.$$

Putting aside the term $\sum_{k=1}^{3} \theta^k wgap_{is,t-3} \cdot DMWI_{s,t-k}$, this is a standard difference-in-differences specification, where the state dummy variables (D_s) stand in for "treated," the year dummy variables (D_t) stand in for "post," and *DMWI* is a vector of dummy variables for whether the minimum wage increased 3 months ago, 2 months ago, or 1 month ago, standing in for the interactions between "treated" and "post." The term $\sum_{k=1}^{3} \theta^k wgap_{is,t-3} \cdot DMWI_{s,t-k}$ captures variation in the effects of the latter with the gap between a worker's prior wage and the new minimum wage (if their wage is below this new minimum). The term $\sum_{k=1}^{3} \rho^k DMWI_{s,t-k}$ can then be viewed as controlling for period- and state-specific shocks that coincide with minimum wage increases that are common to workers regardless of the gap (if any) between their prior wage and the new minimum wage.[17]

We expand equation (1) to simultaneously estimate the effects in each bin of the income-to-needs distribution, estimating:

$$\begin{aligned}(2) \qquad Y_{ist} = {} & \sum_{b=1}^{B} \delta^b \, DIN_{is,t-3}^b + \sum_{b=1}^{B} \sum_{k=1}^{3} \rho^{bk} DMWI_{s,t-k} \cdot DIN_{is,t-3}^b \\ & + \sum_{b=1}^{B} \sum_{k=1}^{3} \theta^{bk} wgap_{is,t-3} \cdot DMWI_{s,t-k} \cdot DIN_{is,t-3}^b \\ & + \sum_{b=1}^{B} D_s \cdot DIN_{is,t-3}^b \, \lambda_{sb} + + \sum_{b=1}^{B} D_t \cdot DIN_{is,t-3}^b \pi_{sb} + \varepsilon_{ist}.\end{aligned}$$

DIN is a vector of dummy variables for the position in the family income-to-needs distribution at the same time employment is measured (a 3-month lag). Since *b* indexes all bins, the variables in equation (1) do not appear separately in equation (2), nor is there an intercept

[17] One might think this should then be a triple difference, with *wgap* entering the equation (1) directly, and interacted with the state dummies and the year dummies (yielding the usual eight moments that appear in a triple-difference specification when D_s represents "treated," D_t represents "post," and *DMWI* represents "treated x post"). But *wgap* is only non-zero in the periods after a minimum wage increase (defined as those when *DMWI* = 1), so these additional variables would be perfectly collinear with the *wgap·DMWI* terms.

(the dummy variables in the term $\sum_{b=1}^{B} \delta^{b}\, DIN_{is,t-3}^{b}$ are the bin-specific intercepts). The variables in the sum $\sum_{b=1}^{B} \sum_{k=1}^{3} \rho^{bk} DMWI_{s,t-k} \cdot DIN_{is,t-3}^{b}$ capture effects that coincide with minimum wage increases that are common to all workers in the same bin. Just like in equation (1), the key term is $\sum_{b=1}^{B} \sum_{k=1}^{3} \theta^{bk} wgap_{is,t-3} \cdot DMWI_{s,t-k} \cdot DIN_{is,t-3}^{b}$. The parameters θ^{bk} capture the effects of continuous minimum wage variation for affected workers in each income-to-needs bin.

One might think that if there is a concern about shocks correlated with minimum wage increases that are common to low-wage workers in all income-to-needs bins, then one could interpret the differences between the estimates of θ^{bk} between income-to-needs bins indexed by $b = 1, \ldots, B-1$ relative to bin B (I/N = 3+) as triple-difference type estimators (indexed by intensity as captured in *wgap*), under the assumption that there are not direct minimum wage effects on low-wage workers in the highest income-to-needs bin. But there does not appear to be a good rationale for that assumption, as low-wage workers could be affected wherever they are in the income-to-needs distribution, and a priori we do not have evidence that effects are even necessarily smaller in the highest income-to-needs bin. Indeed, our purpose in this paper is to estimate the effects of minimum wage on low-wage workers throughout the income-to-needs distribution. There is, of course, interest in how these effects vary across income-to-needs bins, if they do, which are measured by differences in the estimates of θ^{bk} across bins. But there is no reason to interpret the estimates relative to $b = B$ as more reliably causal.

We also report different computations based on these estimates. For employment, these include own-wage elasticities for the estimated effects on the probability of employment, and modifications of these that instead are based on the percentage change in the probability of remaining employed – which can differ by income-to-needs category regardless of minimum wage variation. We do many of the same calculations for hours and earnings.

We also compute "placebo" models for the effects of minimum wages on high-wage workers. As we show, we never find effects like those we find for low-wage workers, and nearly every "effect" for high-wage workers is small and statistically insignificant. We view these results as contributing evidence – beyond our core research design – that rules out spurious effects driven by prior trends associated with minimum wage changes.[18]

Our focus on how minimum wages impact affected (low-wage) workers parallels Kramarz and Phillipon (2001), Clemens and Wither (2019), as well as a large literature (including Gramlich, 1976, and the similar analyses that followed) that begins with an a priori classification of workers likely to be affected by minimum wages. By construction, this approach misses effects on those initially non-employed, who cannot be classified based on their prior wages. Given evidence that higher minimum wages reduce separations of existing workers but also reduce hiring (accessions) of new workers (e.g., Brochu and Green, 2013, for Canada; Dube et al., 2016, for the United States), limiting the analysis in this way misses a potential important channel of adverse effects of minimum wages via slowing transitions into employment – an effect that could, in principle, differ across the income-to-needs distribution. Given this concern, we explored estimating specifications similar to those explained above based on predicted low wages.[19] However, these estimates are relatively uninformative, likely because the ability to

[18] In addition, in some recent work minimum wage researchers have advocated for clean event-study designs and argued that other designs provide biased estimates of minimum wage effects that overstate negative employment impacts (Dube and Lindner, 2024). It would be very difficult if not impossible to implement such designs for our analysis. However, Neumark and Rodriguez-Lopez (2026) show that results for employment effects of minimum wages are generally not sensitive to using clean event studies, and, instead, the different results cited in this work stem from other choices researchers make about defining variables and the events used (and in fact it is a narrow and possibly incorrect set of choices that tend to weaken the evidence of adverse effects of minimum wages on employment).

[19] For the analysis of the initially non-employed based on predicted low wages, reported in the Appendix and described briefly below, we do something slightly different to characterize affected workers because we know predicted wages are not that accurate, and if we categorized a person as unaffected by the minimum wage because their predicted wage (at *t*-3) exceeds the minimum wage at *t*, this could be in error if their counterfactual wage would be below the minimum. Instead, we first restrict the sample of those not employed at *t*-3 to people with predicted wages between plus or minus 25 log points of the period *t* minimum wage. We then define the gap as the

predict this based on a limited set of human capital and other controls is very limited.[20]

Descriptive Statistics

Demographic and other characteristics of workers and non-workers at different points of the income-to-needs distribution

Prior to turning to estimates of the models described above, we present some relevant descriptive statistics to understand which kinds of people or workers are in lower income-to-needs bins. We begin by describing the demographic characteristics of people in different income-to-needs bins. There are no surprises here. As shown in Table 3, minorities are over-represented in low income-to-needs bins, as are those with less than a high-school degree, women, and the unmarried; and those in lower income-to-needs bins have more children. These are among the usual correlates of being poor or low-income. We also see evidence of the point made earlier, that employment rates and hours are lower (and the incidence of families with no workers are higher) in families in lower income-to-needs bins.

If we condition on employment (Table 4), many of these differences are less pronounced (e.g., the differences by gender and education are dissipated), reflecting the over-representation in lower-income families of non-workers. This table also shows the distribution of primary workers in the family (based on highest wages). We find that a larger share of workers in families

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percentage difference between the predicted wage and the maximum predicted wage (in sample) in the corresponding state and month and use this in place of $wgap$ in the equation above. In addition, to provide comparable estimates for the initially employed, we estimate the same models for them but based on having predicted rather than actual low wages. For these models using predicted low-wage workers, rather than low wages per se, the estimating equation is expanded to include all the triple difference terms, because *wgap* is not only nonzero in the periods after a minimum wage increase, so the perfect collinearity referenced in the previous footnote does not hold. (Specifically, we add *wgap* and its interactions with the poverty bin x state and poverty bin x year interactions that appear in equation (2).)

[20] In log wage regressions controlling for education, race/ethnicity, gender, age, marital status, children and their ages, state, and a full set of unique month-year dummy variables, the R-square is 0.32 (and a good deal of the explanatory power comes from the state and time dummy variables). For completeness and transparency, we report these results (and other auxiliary results) in the Online Appendix, but we focus nearly exclusively on the analysis of workers who have low actual wages and hence can be classified as directly affected by minimum wage increases.

in low income-to-needs bins are the primary workers in their family based on wages, and this declines monotonically across the income-to-needs categories, consistent with there generally being only one worker in the lowest income families, and more workers in higher income families.

In Table 5, we condition further, looking at employed workers earning no more than 125% of the minimum wage. Here we find low-wage workers are more likely to be in lower income-to-needs bins if they are unmarried or have more children, or if they are black or have less education (although this latter relationship does not hold consistently across the board). In Table 5, the share of workers who are primary drops much more sharply across higher income-to-need bins than in Table 4, which makes sense because if the highest earner earns a low wage (and Table 5 is restricted to low-wage workers), the family is very likely to be low-income. In contrast to other factors related to low wages, teenagers are a good deal less likely to be in low income-to-needs bins, with, e.g., only 11.5% of low-wage workers in the poorest families (income-to-needs 0 to 0.5) aged 15-19, vs. 24.6% in families with income-to-needs 3+. This is consistent with the one of the key points from prior work about the blunt targeting of the poor by the minimum wage, as a high share of teens earn the minimum wage but are in higher-income families.

Next, in Table 6, we compare the characteristics of the employed and the non-employed in each bin of the income-to-needs distribution. (The results for the employed are the same as in Table 3; including them in Table 5 makes the comparison with the non-employed easier.) We find, not surprisingly, that regardless of income-to-needs bin, the non-employed are more likely to be disabled or enrolled in school. The non-employed are also more likely to have lower education. In the lower income-to-needs ranges, the non-employed are more likely to be black or

Hispanic, but these relationships do not hold in the higher income-to-needs ranges. The relationship with single motherhood is not consistent. Among the poorest families (income-to-needs in the 0-0.5 range) single mothers are less likely to be employed, but between 0.5 and 2, single mothers are more likely to be employed.

The empirical relationships documented in Tables 3-6 reinforce the evidence that higher minimum wages may not target the most disadvantaged very well. We see this, for example, in the concentration of teens, among low-wage workers, in higher-income families, and in the over-representation of single mothers and minorities in low-income families among the non-employed rather than the employed. Of course, the purpose of this paper is to move beyond simplistic predictions of the effects of minimum wages at different points in the income-to-needs distribution based on characteristics of people in ranges of the income-to-needs distribution prior to minimum wage changes. We first present descriptive evidence on employment transitions, before turning to the new evidence on how minimum wage effects for low-wage workers differ across the family income-to-needs distribution.

Employment transitions

Our analysis focuses on employment transitions, so we now turn to descriptive information on these transitions. Table 7, Panel A, shows employment to non-employment transitions, for those employed at *t*-3.[21] Given that the employment rate at *t*-3 is 1, 1 minus the rate shown at *t*-2, *t*-1, and *t* yields the rate of transition to non-employment. The table shows a monotonic increase in the likelihood of remaining employed as income-to-needs increases, or equivalently, a monotonic decline in the rate of transition to non-employment as income-to-needs increases – i.e., workers in the lowest income-to-needs bins have the highest rates of transition to

[21] The approach described in these tables is also used for our elasticity calculations discussed below.

non-employment, and the differences are sizable. For example, this rate is 0.085 for those in extreme poverty (income-to-needs between 0 and 0.5) but only one-third as large for those with income-to-needs above 2. We will of course have to control for these differences in baseline transition rates in estimating how minimum wage effects differ across income-to-needs bins.

Table 7, Panel B, shows the rates of transition for those initially non-employed. Here, baseline employment rates are 0 at *t*-3. The results indicate that those in the lowest income-to-needs bin (0 to 0.5) have much higher transition rates to employment by period *t* (0.12), whereas the rates are lower and similar (0.06 or 0.07) for all of the other income-to-needs groups. This reflects, presumably, the more precarious employment of the poorest workers, cycling both in and out of employment at a fairly high frequency.

Table 8 presents similar statistics, but limited to low-wage workers for the employed, and low predicted wage workers for the non-employed (as described in the table notes). In Panel A, we see a similar pattern of higher transitions to non-employment in lower income-to-needs bins (or lower persistence in employment). But here the relationship is less linear/monotonic than for all workers in Table 6, Panel A, and the main difference is a higher transition rate to non-employment (lower persistence in employment) for those in the lowest income-to-needs bin (0 to 0.5). In Panel B, for transitions from non-employment to employment, there is no clear difference across income-to-needs bins. Presumably the weaker (or much weaker) relationships across income-to-needs bins is because now we are looking only at low-wage (or low predicted wage) workers, for whom employment is precarious regardless of income-to-needs bins. Put differently, the differences we saw in Table 6 were driven by the presence of higher-wage (predicted wage) workers in higher income-to-needs families.

Preliminary and Replication Regressions

In part because the SIPP data have not been used as often as the CPS to study the impacts of minimum wages, we begin by replicating more basic results from the minimum wage literature before moving onto the effects of minimum wages across the family income-to-needs distribution. We begin, in Table 9, with verification that we detect effects of minimum wage changes on wages – a prerequisite for any of our analyses to be meaningful. We estimate a model for the percent change in the wage from *t*-3 to *t* (for those who remain employed) as a function of the percent change in the minimum wage over the same period, including state and month-year fixed effects. We estimate this for low-wage workers in *t*-3, as explained in the table notes – defined as being between the minimum wage at *t*-3 and the prevailing wage (which may or may not have increased) at *t*. We find an elasticity of 0.14, consistent with prior evidence.

We also replicate the prevailing result from simpler analyses of the impact of the minimum wage on poverty. These estimates are reported in Table 10. We estimate the effect of three lags of the minimum wage on the probability that a person is poor, extremely poor (I/N < 0.5) or near-poor (I/N < 1.5). Like most prior work, we find no impact of the minimum wage on poverty.

Results for Effects of Minimum Wages by Income-to-Needs Bins

We now turn to our core analysis of the effects of minimum wages on low-wage workers in the different bins of the income-to-needs distribution. We begin by looking at employment effects, but given that our goal is to understand the different channels by which minimum wages affect incomes of workers in poor, low-income, and other families, we also look at effects on hours and earnings.

Employment Effects

The estimates for employment to non-employment transitions of low-wage workers are reported in Table 11. Columns (1)-(3) report estimated employment effects of a 10% increase in *wgap* induced by a minimum wage increase. Note that this can be larger than a 10% minimum wage increase for workers whose initial wages are above the old minimum wage in *t*-3, but below the new minimum wage. Columns (1)-(3) report the estimates for *t*-2, *t*-1, and *t*, and column (4) reports the average of the coefficients across all three periods. These estimates point to significant negative effects of higher minimum wages on the probability of remaining employed, in all but one of the income-to-needs bins (1.5-2). There is evidence that these effects occur with some lag, as the estimates are generally larger at lags of two or three months.

Importantly with respect to our key question, the effects are more adverse in the lowest income-to-needs bin, while the pattern for bins above 0.5 is not so clear. The average estimates in column (4) range from about a −0.038 to −0.066 lower probability of remaining employed following a 10% increase in *wgap* induced by a minimum wage increase. With regard to variation across the income-to-needs bins, the effect is largest in the lowest bin: 0 to 0.5, or "extreme poverty." The estimates for all bins above 1 are quite close – hovering around −0.4; all the estimates are statistically significant except for the 1.5-2 bin.

Given that we compute the effects of a 10% increase in *wgap* induced by a minimum wage increase, and given that we start with those initially employed, the corresponding elasticities for column (4) range from −0.38 to −0.66 (simply dividing the estimates by 0.1). These are larger than conventional employment elasticities with respect to the minimum wage, for two reasons. First, they are computed for low-wage workers. (For example, Clemens and Wither (2019) report employment elasticities near −1 for workers with wages near the minimum.) Second, we are computing these elasticities relative to increases in the gap between the new minimum wage and the worker's prior wage, which is smaller than the legislated minimum wage increase for all workers except those previously paid the old minimum wage (see, e.g., Neumark and Wascher, 2008, Chapter 3).

In that sense, these elasticities are more akin to own-wage elasticities (which divide the conventional elasticity of employment with respect to a legislated minimum wage increase by the elasticity of wage with respect to a legislated minimum wage increase). We show this more clearly in column (5), where we compute own-wage elasticities by dividing the estimates in column (4) by the elasticity of wages with respect to the 10% increase in *wgap*.[22] These range from −0.2 to −0.34, with the largest negative value for the lowest income-to-needs bin (0 to 0.5), although the difference relative to the 1-1.5 bin is negligible (because the wage elasticity for the 0-0.5 bin is larger).

Finally, these particular own-wage elasticities are a bit unusual, because they pertain to the percent decline in the probability of employment. As shown earlier (Table 8), workers in different income-to-needs bins have different baseline probabilities of transitions to non-employment, so we obtain more comparable percentage effects if we divide the own-wage elasticities by the employment "continuation rate" – i.e., the baseline probability of remaining employed. Because those in the 0-0.5 bin have a lower continuation rate, this increases the implied effect more for that group. The negative impact is largest for this lowest income-to-needs bin, although the relationship is not monotonic as income-to-needs increases.[23]

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[22] The wage elasticities are reported in Appendix Table A1. We compute the percentage change in wages resulting from "topping-up" wages to 10 percent above the minimum wage in *t*-3 when the minimum wage increases between *t*-3 and *t*. In Appendix Table A2, we compute this for the subsample of workers with wages in *t*-3 at or above the minimum wage in *t*-3 and below 1.1X the minimum wage in *t*-3. We verify that this group of workers between the two minimum wages have wage elasticities approximately equal to 0.5.

[23] Appendix Table A3 reports the results for predicted low-wage workers, for those initially non-employed (Panel A) and those initially employed (Panel B). There is no own-wage elasticity computation in this case, because we cannot

In general, then, our estimation yields evidence of substantial adverse effects of minimum wages on the probability that low-wage workers remain employed. There is some indication that the effects are more adverse in the poorest families, but there is not very strong evidence of heterogeneity of this nature, and certainly not of a monotonic diminution of the adverse effects as family income-to-needs rises. Rather, perhaps a stronger conclusion is that there are adverse employment effects for all of the income-to-needs bins.[24]

Hours and Earnings Effects

Although evidence on employment effects has dominated the minimum wage literature, in this paper we are ultimately more interested in how minimum wages affect income. We thus turn to other margins: first hours, which is another dimension of how much people work; and then earnings, which capture all the relevant effects – employment, hours, and wages. We report estimates both conditional on remaining employed and unconditional. The former are useful to

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estimate wage changes for those initially not working. However, column (5) reports the estimate from column (4) divided by the rates of transition to employment from Table 8 – as in Table 11, to compute the percent change in the probability of becoming employed relative to the baseline transition rate in the corresponding income-to-needs category. In Panel A, in general, we do not find adverse effects of minimum wages on the probability of transitions to employment, and in one of the income-to-needs cells (1-1.5) the estimated impact is positive (column (4)). To get a sense of the implication of using predicted wages – which we have to do for those initially non-employed – Panel B of Appendix Table A3 reverts to estimating effects of minimum wage increases on employment-to-non-employment transition, but using predicted wages as in Panel A, rather than actual wages as in Table 11, to try to identify affected workers (and, as a result, also considering the effect of a 10% increase in the minimum wage). The estimated effects in Panel B are much closer to zero than the estimates in Table 11, and do not particularly tend to be negative (and are never negative and significant). However, in this case we are computing the implied effects of a 10% increase in the minimum wage, and not the effects of a 10% increase in *wgap*, hence we expect a somewhat smaller effect – but not this large a difference. Of course, the other difference is that we are less definitively identifying low-wage workers than in Table 11, since we are only predicting wages. Thus, the employment effects (column (4)) should be smaller, and the fact that the estimates are near zero may simply indicate that we do not identify affected workers very well. As a consequence, we do not put much store in our results using predicted low-wage workers.

[24] Appendix Table A8 reports estimates of "placebo" regressions for employment effects, for workers with high wages, paralleling Table 11. High wages are defined as at least 75% of 2.5X the minimum wage, and high predicted wages are defined as 25 log points below 2.5X the minimum wage or higher. (Note that *wgap* is defined here the same way as in the low-wage regressions (the minimum wage in *t* minus the wage in *t*-3 all divided by the wage in *t*-3), except that we do not set *wgap* = 0 for workers above the minimum wage. So, for high-wage workers, *wgap* is negative and becomes closer to 0 when the minimum wage increases.) We do not find any systematic pattern of negative estimates like those in Table 11; if we did, we might interpret the evidence of adverse employment effects in Table 11 as spurious. (We do not report own-wage elasticities because minimum wages are not predicted to affect wages of high-wage workers.)

understand how minimum wages affect workers who keep their jobs, but the latter are ultimately more important in studying how minimum wages affect individual earnings and incomes.

Table 12 reports results for unconditional hours, hence combining both extensive and intensive margins. As the table shows, for those initially employed at low wages, we find sizable hours reductions in all bins of the income-to-needs distribution. The average declines are not larger in the lowest bins, and, if anything, appear a bit smaller. This is also true when we look at elasticities or own-wage elasticities, despite the lower hours worked in the lowest income-to-need bins. As before, these are our most compelling estimates, because we can directly identify low-wage workers.

Estimates for hours conditional on employment in period *t* are reported in Table 13. In general, the results are similar in pointing to negative hours effects in all or at least most of the income-to-needs bins. Overall, then, the evidence indicates hours declines for minimum wage/low-wage workers, which are if anything less severe for the lowest income-to-needs categories.[25]

Tables 14 and 15 turn to earnings – first unconditional, and then conditional. The analyses are otherwise the same as for hours. For unconditional earnings, the estimates in Table 14 point to declines across all of the income-to-needs bins for those initially employed. There is not evidence that the effects are largest in the lower bins, and the evidence points more in the opposite direction, regardless of how we do the calculation. The estimates conditional on employment, in Table 15, are very similar.[26] Perhaps the most striking evidence from the analysis

[25] Appendix Tables A4 and A5 report the results for unconditional and conditional hours for predicted low-wage workers. We generally find much smaller effects, and little consistent evidence of negative hours effects. Appendix Tables A9 and A10 report the placebo estimates corresponding to Tables 12 and 13. The estimated effects are near zero, as for employment, bolstering a causal interpretation of the estimated effects on hours of low-wage workers in Tables 12 and 13.
[26] Appendix Tables A6 and A7 report the corresponding estimates for predicted low-wage workers. In Panel A, for the non-employed with predicted low wages, there is some evidence of earnings declines in the 0-0.5 and 3+

of earnings is that, even conditioning on employment, there is no evidence of earnings gains from higher minimum wages, and if anything evidence of earnings declines – consistent with the negative conditional hours effects in Table 13.[27]

Conclusions

Our goal in this paper was to drill down more deeply into the effects of minimum wages. Specifically, we wanted to bridge the research literature on the effects of minimum wages on workers – which largely focuses on employment but also on hours and earnings – with the research literature on the effects of minimum wages on families. The research on workers focuses on low-skilled or low-wage workers, and finds that: (1) minimum wages increase wages (not surprisingly); and (2) generally, but by no means always, minimum wages reduce employment. This literature is fairly silent on hours and earnings (an exception is Neumark et al., 2004). To be sure, these results are contested, with some researchers – and many economists more generally – quite convinced that minimum wages help low-wage workers.[28] The research on families largely focuses on whether minimum wages lift families out of poverty – or related outcomes with regard to helping low-income families – and generally finds little positive impact.

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income-to-needs bins and increases in the 0.5-1 and 1-1.5 bins. In Panel B, using predicted low-wage workers but conditioning on initial employment, there is also some evidence of earnings increases in these intermediate bins and declines in the 3+ bin. Again, though, we put the most store in the Table 14 and 15 estimates for initially employed, low-wage workers.

[27] Appendix Tables A11 and A12 report the placebo estimates corresponding to Tables 14 and 15. We find little or no impact of minimum wages on wages of high-wage workers, bolstering a causal interpretation of the estimated effects on earnings of low-wage workers in Tables 14 and 15.

[28] For example, in testimony before the U.S. House Education and Labor Committee, Ben Zipperer (2019) argued: "The bulk of recent economic research on the minimum wage, as well as the best scholarship, establishes that prior increases have had little to no negative consequences and instead have meaningfully raised the pay of the low-wage workforce." And a 2019 letter signed by scores of economists, including many prominent academics such as Sandra Black, David Cutler, Emmanuel Saez, Juliet Schor, and Nobel Laureates Daron Acemoglu, Angus Deaton, and Peter Diamond, says that: "The last decade has seen a wealth of rigorous academic research on the effect of minimum wage increases on employment, with the weight of evidence showing that previous, modest increases in the minimum wage had little or no negative effects on the employment of low-wage workers." (See https://www.epi.org/economists-in-support-of-15-by-2024/.)

We estimate the "standard" effects of minimum wages on low-wage or low-skill workers, but we leverage the very large samples available in the SIPP to estimates the effects separately for workers in different bins of the income-to-needs distribution. This allows us to see whether, for example, the effects of minimum wages are more beneficial for low-wage or low-skill workers with low income-to-needs – e.g., in poor families – or whether the effects are more harmful for workers in the lowest-income families. This inquiry can provide the link that is missing in the vast research literature on the minimum wage: understanding how minimum wage effects on workers, along many margins, map into the distribution of family income. Given that the main rationale for the minimum wage is to reduce poverty or otherwise help low-income families, but minimum wage laws target low-wage work rather than low family income, understanding this mapping from the legislated effects on wages to the ultimate effects on families is crucial.

We can summarize our findings with two key conclusions. First, we generally find adverse effects of higher minimum wages on the employment, hours, and earnings of low-wage or low-skill workers.[29] Second, there is no clear and consistent "gradient" indicating either more positive (or more negative) minimum wage effects for low-wage/low-skill workers in families with low income-to-needs. Most importantly, perhaps, nothing in our deeper dive into the interrelationships between minimum wages, low-wage work, and low family income provides any indication that minimum wage impacts are beneficial – or particularly beneficial – for low-wage or low-skill workers in poor or low-income families.

[29] As we have noted, our estimates are more comparable to those that condition on workers having very low wage (e.g., Clemens and Wither, 2019), than to those that condition on low-skill groups broadly (like teenagers, or high-school dropouts under age 25). This almost surely explains our larger negative employment elasticities than much of the literature, and thus likely also accounts for the adverse earnings effects.

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Table 1: Sample Sizes in SIPP and CPS Files

	2004 SIPP	2008 SIPP	2014 SIPP	ASEC	ASEC/ORG
2003 (Oct. to Dec.)	114,750			147,897	15,134
2004	856,883			146,345	14,907
2005	796,742			144,360	14,410
2006	581,260			143,861	14,236
2007	300,489			142,780	14,198
2008 (May to Dec.)		470,677		143,373	14,295
2009		788,667		144,658	13,585
2010		735,783		145,975	13,461
2011		684,279		143,048	13,406
2012		647,405		140,808	13,480
2013		424,830 (Jan. to Nov.)	615,094	141,650	13,353
2014			477,106	139,487	13,464
2015			390,972	139,205	13,307
2016			345,494	129,655	12,216

The 2004 and 2008 SIPP Panels consist of four-month "waves," where a portion of survey households are interviewed each month and asked about activities in the current and three preceding months. At the beginning of the panel, household respondents are rotated in. For example, in January 2004, the first set of households were asked about October 2003, November 2003, December 2003, and January 2004. In February 2004, the second set of households were asked about November 2003, December 2003, January 2004, and February 2004. At the end of the sample, households are rotated out. For this reason, sample sizes are smaller in the first and last months of the 2004 and 2008 SIPP Panels. The 2014 SIPP uses a 12-month reference period without a rotation pattern. Attrition in the 2014 SIPP Panel was faster than in previous panels. This was possibly due to the long interval between interviews (average 12 months), participant unwillingness, and an inability to locate and interview prior participants, leading to a particularly strong rise in the non-response rate between Waves 2 and 3 (National Academies of Sciences, 2018). Eggleston and Gideon (2017) also note that the first wave of the 2014 SIPP has a higher nonresponse rate than the first wave of the 2008 SIPP, and that nonresponse rates for survey have been increasing over time. Tolliver et al. (2019) note the particular challenge of following respondents who moved between waves and that during wave 2 of the 2014 panel, efforts to follow these respondents were inconsistent enough that some never received a single recorded contact attempt.

Table 2: Sample Sizes in SIPP and CPS ORG Files – Low-Wage Workers, 2016

	0 to 0.5		0.5 to 1		1 to 1.5		1.5 to 2		2 to 3		3 or more		Total	
	SIPP	ASEC /ORG	SIPP	ASEC /ORG	SIPP	ASEC/ ORG	SIPP	ASEC/ ORG	SIPP	ASEC/ ORG	SIPP	ASEC/ ORG	SIPP	ASEC/ ORG
California	210	13	422	39	566	59	615	46	786	69	1,329	102	3,928	328
DC	4	2	4	10	6	3	44	7	4	11	12	13	74	46
Mississippi	106	3	165	4	180	8	164	5	198	8	366	5	1,179	33
Texas	134	9	250	23	339	9	344	15	357	16	585	28	2,009	89
Wyoming	3	0	15	0	0	1	12	0	0	1	11	2	41	4

The SIPP, ASEC, and ORG 2016 samples are all limited to employed individuals aged 15-70, with wages at or below 1.25 times the minimum wage.

Table 3: Demographic Characteristics of People in Different Income-to-Needs Bins

	Family Poverty Bin (*t*-3)					
	[0, 0.5)	[0.5, 1)	[1, 1.5)	[1.5, 2)	[2,3)	3+
Non-Hispanic White	53.3	46.0	49.3	53.9	61.7	74.9
Hispanic	20.4	26.7	26.2	24.0	18.8	9.1
Black	18.2	20.0	17.2	15.2	12.7	8.4
Asian	4.1	3.5	3.9	3.8	3.8	5.1
Other Race	4.0	3.8	3.5	3.1	2.9	2.5
Less than HS Diploma	24.6	33.1	27.6	22.5	17.4	8.3
HS Diploma	29.4	30.1	32.5	32.3	30.6	20.4
Some College	30.8	27.7	30.0	32.3	34.9	32.3
Bachelor's Degree	10.9	6.7	7.5	9.7	12.5	24.1
Master's Degree	4.3	2.4	2.4	3.3	4.6	14.9
Enrolled in School	49.4	43.1	41.9	41.6	41.0	42.3
Age:						
15-19	12.2	12.7	11.6	10.6	9.9	7.5
20-24	16.0	11.7	11.5	11.1	9.8	7.5
25+	71.8	75.6	76.9	78.3	80.3	85.0
Male	45.1	42.9	46.4	48.4	49.4	50.9
Married	24.8	33.4	39.3	44.0	48.8	61.5
Number of Children Under 18 (mean)	0.8	1.0	0.9	0.8	0.7	0.5
Single Mother	16.6	15.5	12.3	9.9	7.6	3.9
Employed	38.9	43.4	53.3	61.8	68.2	77.7
Family Has No Employees	47.1	34.4	22.6	15.4	10.9	5.8
Weekly Hours (mean)	8.8	11.4	16.1	20.1	23.2	28.1
N	521,815	547,886	652,014	657,943	1,222,130	3,650,567

Sample includes individuals aged 15-70, employed and non-employed. Weighted using SIPP person weights. The values presented are percentage shares, with the exception of number of children under 18 and usual weekly hours, which are mean values.

Table 4: Demographic Characteristics of Employed Workers in Different Income-to-Needs Bins

	Family Poverty Bin (*t*-3)					
	[0, 0.5)	[0.5, 1)	[1, 1.5)	[1.5, 2)	[2,3)	3+
Non-Hispanic White	60.0	47.8	49.0	53.4	61.3	75.3
Hispanic	18.5	28.4	28.2	25.3	19.7	9.1
Black	14.5	17.2	16.1	14.7	12.7	8.5
Asian	3.6	3.4	3.7	3.78	3.7	4.8
Other Race	3.4	3.1	3.0	2.8	2.7	2.3
Less than HS Diploma	16.6	24.2	20.7	16.8	12.4	4.7
HS Diploma	27.8	30.8	33.6	32.9	30.9	19.8
Some College	35.0	33.0	33.7	35.4	37.7	32.9
Bachelor's Degree	14.4	8.7	9.1	11.0	14.0	26.2
Master's Degree	6.1	3.3	3.0	3.9	5.06	16.4
Enrolled in School	37.2	36.9	35.7	35.9	35.5	38.1
Age						
15-19	5.5	5.8	5.0	4.6	4.5	3.5
20-24	15.9	15.2	13.8	12.5	10.5	7.0
25+	78.7	79.0	81.2	82.9	85.0	89.4
Male	52.8	47.6	50.4	52.3	52.8	53.5
Married	30.4	39.3	42.2	45.0	49.4	63.2
Number of Children Under 18 (mean)	0.8	1.2	1.0	0.9	0.8	0.5
Single Mother	14.0	19.0	14.4	10.6	7.6	3.2
Primary Worker (wages)	90.0	85.4	80.7	76.2	71.4	64.0
Weekly Hours (mean)	24.1	28.1	31.7	33.6	34.9	36.7
N	201,399	228,346	339,244	398,081	823,453	2,831,851

See notes to Table 2, except that sample is restricted to individuals employed in *t*. Weighted using SIPP person weights. The Primary Worker calculations are based on observations with non-missing wages; the percentages reported are the shares of workers in each income-to-needs bin who are the primary worker in the family. (If there is a tie, we count only one primary worker in the family.)

Table 5: Demographic Characteristics of Employed Workers with Wages Less Than 125% of Minimum Wage in Different Income-to-Needs Bins (Percentages)

	Family Poverty Bin (t-3)					
	[0, 0.5)	[0.5, 1)	[1, 1.5)	[1.5, 2)	[2,3)	3+
Non-Hispanic White	51.4	43.5	44.4	46.8	53.1	70.4
Hispanic	23.2	31.3	32.2	32.2	27.2	13.0
Black	18.5	18.9	16.0	13.9	12.3	8.9
Asian	3.2	3.3	4.3	4.1	4.5	4.0
Other Race	3.6	2.9	3.0	2.9	2.9	3.7
Less than HS Diploma	22.8	27.5	25.7	24.1	22.5	17.4
HS Diploma	30.5	32.7	35.8	35.3	33.6	28.2
Some College	35.6	31.7	30.4	31.3	33.6	39.1
Bachelor's Degree	8.6	6.2	6.4	7.3	7.9	11.3
Master's Degree	2.5	2.0	1.7	2.0	2.3	4.1
Enrolled in School	48.8	44.9	43.9	46.6	50.8	62.0
Age						
15-19	11.5	9.4	9.8	12.0	16.8	24.6
20-24	26.2	20.7	20.2	20.3	21.2	25.8
25+	62.3	70.0	70.1	67.6	62.0	49.6
Male	40.5	40.1	43.4	45.8	42.6	43.1
Married	19.7	29.7	29.5	33.4	37.3	32.9
Number of Children Under 18 (mean)	0.9	1.1	0.8	0.7	0.6	0.5
Single Mother	22.1	22.8	14.6	10.7	10.3	10.3
Primary Worker (wages)	87.1	80.2	69.0	53.4	35.0	20.0
Weekly Hours (mean)	27.7	30.9	33.1	33.4	32.0	28.8
N	61,697	97,858	116,655	98,584	141,128	243,946

See notes to Tables 4, except sample is restricted to workers who have calculated wages less than or equal to 125% of the minimum wage in t. Weighted using SIPP person weights.

Table 6: Characteristics of the Employed and Non-Employed in Different Income-to-Needs Bins (Percentages)

	Family Poverty Bin (t-3)											
	[0, 0.5)		[0.5, 1)		[1, 1.5)		[1.5, 2)		[2, 3)		3+	
	NE	E	NE	E	NE	E	NE	E	NE	E	NE	E
Non-Hispanic White	49.1	60.0	44.6	47.8	49.6	49.0	54.7	53.4	62.5	61.3	73.5	75.3
Hispanic	21.6	18.5	25.4	28.4	24.0	28.2	21.9	25.3	17.0	19.7	9.1	9.1
Black	20.5	14.5	22.2	17.2	18.4	16.1	15.9	14.7	12.9	12.7	8.4	8.5
Asian	4.5	3.6	3.6	3.4	4.1	3.7	3.9	3.8	4.2	3.7	5.9	4.8
Other Race	4.3	3.4	4.2	3.1	4.0	3.0	3.5	2.8	3.4	2.7	3.0	2.3
Less than HS Diploma	29.7	16.6	39.9	24.2	35.4	20.7	31.8	16.8	28.2	12.4	21.0	4.7
HS Diploma	30.4	27.8	29.5	30.8	31.2	33.6	31.2	32.9	30.0	30.9	22.7	19.8
Some College	28.2	35.0	23.7	33.0	25.8	33.7	27.1	35.4	29.0	37.7	30.3	32.9
Bachelor's Degree	8.6	14.4	5.2	8.7	5.7	9.1	7.7	11.0	9.3	14.0	16.7	26.2
Master's Degree	3.1	6.1	1.7	3.3	1.8	3.0	2.3	3.9	3.5	5.1	9.4	16.4
Enrolled in School	57.1	37.2	47.9	36.9	49.0	35.7	50.8	35.9	52.6	35.5	56.8	38.1
Age												
15-19	16.6	5.5	18.0	5.8	19.0	5.0	20.2	4.6	21.3	4.5	21.5	3.5
20-24	16.1	15.9	9.1	15.2	9.0	13.8	8.9	12.5	8.4	10.5	8.9	7.0
25+	67.4	78.7	73.0	79.0	72.0	81.2	70.9	82.9	70.3	85.0	69.6	89.4
Male	40.2	52.8	39.4	47.6	41.8	50.4	42.0	52.3	42.2	52.8	41.6	53.5
Disabled	9.9	2.4	13.8	2.1	12.1	1.9	11.0	1.7	9.5	1.4	7.1	1.1
Married	21.3	30.4	28.9	39.3	36.1	42.2	42.3	45.0	47.6	49.4	55.3	63.2
Number of Children Under 18	0.8	0.8	0.8	1.2	0.7	1.0	0.7	0.9	0.6	0.8	0.5	0.5
Single Mother	18.3	14.0	12.8	19.0	9.9	14.4	8.8	10.6	7.6	7.6	6.4	3.2
N	320,416	201,399	319,540	228,346	312,770	339,244	259,862	398,081	398,677	823,453	818,716	2,831,851

See notes to Table 2. Weighted using SIPP person weights. Employment status is defined in t.

Table 7: Employment Transitions in Different Income-to-Needs Bins

A: Employment to non-employment

	Family Poverty Bin (*t*-3)					
	[0, 0.5)	[0.5, 1)	[1, 1.5)	[1.5, 2)	[2,3)	3+
Share employed in:						
t-2	96.4	97.6	98.2	98.6	98.9	99.1
t-1	93.7	95.4	96.5	97.2	97.7	98.2
t	91.5	93.4	94.9	95.9	96.5	97.3
N	178,588	224,729	337,111	397,814	825,519	2,849,517

B: Non-employment to employment

	Family Poverty Bin (*t*-3)					
	[0, 0.5)	[0.5, 1)	[1, 1.5)	[1.5, 2)	[2,3)	3+
Share employed in:						
t-2	4.1	2.1	2.2	2.3	2.4	2.6
t-1	7.9	4.1	4.4	4.7	4.7	5.1
t	11.6	6.1	6.5	6.8	7.0	7.5
N	343,227	323,157	314,903	260,129	396,611	801,050

Sample in Panel A includes all individuals employed in *t*-3. Sample in Panel B includes all individuals not employed in *t*-3. N is the number of observations in time *t*. Sample sizes are similar in *t*-1 and *t*-2, and missing values are due to absence from the sample in that month. Weighted using SIPP person weights.

Table 8: Employment Transitions in Different Income-to-Needs Bins for Workers with Wages ≤ 125% of Minimum Wage

A: Employment to non-employment

	Family Poverty Bin (t-3)					
	[0, 0.5)	[0.5, 1)	[1, 1.5)	[1.5, 2)	[2,3)	3+
Share employed in:						
t-2	94.8	97.5	97.9	98.0	97.8	97.3
t-1	91.2	94.9	95.7	95.9	95.7	94.7
t	88.0	92.5	93.7	93.8	93.6	92.2
N	53,405	101,458	120,913	100,796	142,744	242,746

B: Non-employment to employment

	Family Poverty Bin (t-3)					
	[0, 0.5)	[0.5, 1)	[1, 1.5)	[1.5, 2)	[2,3)	3+
Share employed in:						
t-2	3.2	2.3	2.4	2.7	2.9	3.2
t-1	6.4	4.4	4.8	5.3	5.6	6.3
t	9.5	6.5	7.2	8.0	8.3	9.3
N	91,248	79,803	73,389	59,394	85,799	161,775

Sample in Panel A includes workers who have calculated wages in t-3 less than or equal to 1.25X minimum wage in t. Sample in Panel B includes all individuals not employed in t-3 with predicted log wages within 25 log points of the minimum wage in t. N is the number of observations in time t. Sample sizes are similar in t-1 and t-2, and missing values are due to absence from the sample in that month. Weighted using SIPP person weights.

Table 9: Estimated Effect of Minimum Wage Increases on Wages of Low-Wage Workers

	Wage (Percent Change)
% change in minimum wage	0.139***
	(0.051)
N	490,937

Sample restricted to workers with wages at *t*-3 between the minimum wage at *t*-3 and 125% of the minimum wage at *t* and wages at *t* greater than or equal to the minimum wage and less than or equal to 100 dollars, inflated. Percent changes in the wage and the minimum wage are computed from *t*-3 to *t*. State and month-year fixed effects included, and standard errors clustered by state. Weighted using SIPP person weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Estimated Effects of Minimum Wage on the Probability of Poverty

Log minimum wage:	*t*-1	*t*-2	*t*-3	Average
I/N < 0.5				
Log minimum wage lagged 1-3 months	0.005	0.002	-0.007	-0.000003
	(0.009)	(0.007)	(0.011)	(0.004)
I/N < 1				
Log minimum wage lagged 1-3 months	0.004	-0.004	-0.001	-0.0004
	(0.019)	(0.005)	(0.018)	(0.008)
I/N < 1.5				
Log minimum wage lagged 1-3 months	0.0002	-0.005	0.003	-0.0005
	(0.034)	(0.009)	(0.026)	(0.010)
Observations		7,252,355		

Sample restricted to all individuals with non-missing family poverty multiple in *t*-3. State and month-year fixed effects included, and standard errors clustered by state. Weighted using SIPP person weights. The estimates in the first three columns are from a single regression, and the average estimate is calculated from the three coefficients.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Estimated Effects of Minimum Wage on Employment Transitions by Income-to-Needs Bins

Probability of employment at t, conditional on employment at t-3, low-wage workers

Effect of 10% *wgap* increase, lagged 1-3 months	(1) *t*-1	(2) *t*-2	(3) *t*-3	(4) Average	(5) Own-wage elasticity	(6) Col. (5)/employment continuation rate
I/N: 0-0.5	-0.035	-0.077*	-0.086*	-0.066**	-0.341**	-0.395**
	(0.037)	(0.041)	(0.051)	(0.029)	(0.150)	(0.174)
I/N: 0.5-1	-0.031	-0.013	-0.071**	-0.038**	-0.200**	-0.218**
	(0.024)	(0.019)	(0.033)	(0.019)	(0.097)	(0.106)
I/N: 1-1.5	-0.049*	-0.106***,†	-0.005	-0.054**	-0.334**	-0.358**
	(0.025)	(0.033)	(0.023)	(0.020)	(0.125)	(0.134)
I/N: 1.5-2	-0.038*	-0.065*	-0.014	-0.039	-0.266	-0.285
	(0.021)	(0.035)	(0.031)	(0.027)	(0.183)	(0.196)
I/N: 2-3	-0.029	-0.019	-0.065**	-0.038**	-0.227**	-0.244**
	(0.017)	(0.022)	(0.025)	(0.018)	(0.106)	(0.114)
I/N: 3+	-0.069***	-0.045**	-0.025	-0.046***	-0.267***	-0.291***
	(0.018)	(0.019)	(0.023)	(0.017)	(0.099)	(0.108)

Observations: 562,777. Columns (1)-(4) report estimates in terms of a 10% increase in *wgap*. Column (5) reports the estimates of column (4), divided by the wage elasticity computed similarly (see Appendix Table A1). Column (6) reports the estimates of column (5) divided by the likelihood of remaining employed in *t* in states with minimum wage equal to the federal minimum wage (estimates paralleling Table 7). The sample is restricted to workers with wages in *t*-3 at or above the minimum wage in *t*-3, and either not employed or with a wage between the minimum wage and 125% of the minimum wage at *t*. State and month-year fixed effects by poverty bin in *t*-3 included, and standard errors clustered by state. Model is fully interacted (family poverty bins are interacted with minimum wage timing, *wgap* values, the interaction of minimum wage timing and *wgap*, state dummy variables, and month-year dummy variables). Weighted using SIPP person weights.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The † symbols indicate the same levels of significance for effects relative to those of the 3+ bin.

Table 12: Estimated Effects of Minimum Wage on Unconditional Usual Weekly Hours by Income-to-Needs Bins

	Conditional on employment at t-3, low-wage workers					
	(1)	(2)	(3)	(4)	(5)	(6)
Effect of 10% *wgap* increase, lagged 1-3 months	*t*-1	*t*-2	*t*-3	Average	Col. (4)/average hours in *t*-3	Own-wage elasticity
I/N: 0-0.5	-1.975	-4.124***	-3.996**	-3.365***	-0.108***	-0.562***
	(1.547)	(1.219)	(1.614)	(0.953)	(0.031)	(0.159)
I/N: 0.5-1	-1.716†	-2.248**,†	-3.071**	-2.345***,†	-0.076***,†	-0.400***,††
	(1.768)	(0.847)	(1.160)	(0.773)	(0.025)	(0.132)
I/N: 1-1.5	-3.322**	-5.382***	-4.420***	-4.375***	-0.144***	-0.904***
	(1.492)	(1.429)	(1.451)	(1.208)	(0.040)	(0.250)
I/N: 1.5-2	-4.116***	-5.803***,†	-1.339	-3.753***	-0.124***	-0.845***
	(1.241)	(0.659)	(1.231)	(0.785)	(0.026)	(0.177)
I/N: 2-3	-5.912***	-4.450***	-5.048***	-5.136***	-0.169***	-1.018***
	(1.095)	(1.003)	(1.583)	(1.077)	(0.036)	(0.213)
I/N: 3+	-4.840***	-4.228***	-3.526***	-4.198***	-0.132***	-0.756***
	(0.646)	(0.689)	(0.767)	(0.630)	(0.020)	(0.113)

Observations: 503,260. Columns (1)-(4) report estimates in terms of a 10% increase in *wgap*. Column (5) reports the estimates of column (4), divided by the mean usual weekly hours in *t-3* in states where the minimum wage changed between *t-3* and *t*. Column (6) reports the estimates of column (5) divided by wage elasticity of the minimum wage. The sample is restricted to workers with wages in *t*-3 at or above the minimum wage in *t*-3, and either not employed or with a wage between the minimum wage and 125% of the minimum wage at *t*. State and month-year fixed effects by poverty bin in *t*-3 included, and standard errors clustered by state. Model is fully interacted (family poverty bins are interacted with minimum wage timing, *wgap* values, the interaction of minimum wage timing and *wgap*, state dummy variables, and month-year dummy variables). Weighted using SIPP person weights.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The † symbols indicate the same levels of significance for effects relative to those of the 3+ bin.

Table 13: Estimated Effects of Minimum Wage on Conditional Usual Weekly Hours by Income-to-Needs Bins

Conditional on employment at t-3, low-wage workers

Effect of 10% *wgap* increase, lagged 1-3 months	(1) *t*-1	(2) *t*-2	(3) *t*-3	(4) Average	(5) Col. (4)/average hours in *t*-3	(6) Own-wage elasticity
I/N: 0-0.5	-0.964	-2.343**	-1.350	-1.552**	-0.049**	-0.263**
	(1.031)	(0.910)	(0.955)	(0.723)	(0.023)	(0.122)
I/N: 0.5-1	-0.493	-1.471*	-0.902	-0.956†	-0.031†	-0.168†
	(1.893)	(0.778)	(0.798)	(0.882)	(0.028)	(0.155)
I/N: 1-1.5	-1.711	-2.353**	-4.507***	-2.857***	-0.093***	-0.593***
	(1.115)	(1.036)	(1.026)	(0.899)	(0.029)	(0.187)
I/N: 1.5-2	-2.812**	-3.786**	-0.899	-2.499**	-0.082**	-0.575**
	(1.203)	(1.422)	(1.129)	(0.954)	(0.031)	(0.220)
I/N: 2-3	-5.213***	-4.089***	-3.204**	-4.169***	-0.136***	-0.841***
	(0.914)	(0.750)	(1.448)	(0.914)	(0.030)	(0.184)
I/N: 3+	-3.348***	-3.222***	-2.976**	-3.182***	-0.099***	-0.579***
	(1.023)	(1.126)	(1.338)	(1.115)	(0.035)	(0.203)

Observations: 463,157. Columns (1)-(4) report estimates in terms of a 10% increase in *wgap*. Column (5) reports the estimates of column (4), divided by the mean usual weekly hours in *t-3* in states where the minimum wage changed between *t-3* and *t*. Column (6) reports the estimates of column (5) divided by wage elasticity of the minimum wage. The sample is restricted to workers with wages in *t*-3 at or above the minimum wage in *t*-3, and either not employed or with a wage between the minimum wage and 125% of the minimum wage at *t*. State and month-year fixed effects by poverty bin in *t*-3 included, and standard errors clustered by state. Model is fully interacted (family poverty bins are interacted with minimum wage timing, *wgap* values, the interaction of minimum wage timing and *wgap*, state dummy variables, and month-year dummy variables). Weighted using SIPP person weights.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The † symbols indicate the same levels of significance for effects relative to those of the 3+ bin.

Table 14: Estimated Effects of Minimum Wage on Unconditional Monthly Earnings by Income-to-Needs Bins

	(1)	(2)	(3)	(4)	(5)	(6)
	Conditional on employment at t-3, low-wage workers					
Effect of 10% *wgap* increase, lagged 1-3 months	*t*-1	*t*-2	*t*-3	Average	Col. (4)/average earnings in *t*-3	Own-wage elasticity
I/N: 0-0.5	-135.557***	-214.297***	-163.113**	-170.989***	-0.154***	-0.797***
	(42.198)	(58.454)	(64.055)	(39.500)	(0.036)	(0.184)
I/N: 0.5-1	-140.662*	-212.181***	-166.598***	-173.147***	-0.159***	-0.831***
	(73.388)	(60.468)	(40.621)	(44.423)	(0.041)	(0.213)
I/N: 1-1.5	-194.132***	-133.985	-130.828	-152.982**	-0.145**	-0.903**
	(59.380)	(86.550)	(98.909)	(68.924)	(0.065)	(0.407)
I/N: 1.5-2	-423.100***,†	-412.881***,††	-301.746***	-379.242***,††	-0.364***,††	-2.478***,†††
	(82.539)	(52.680)	(110.981)	(70.563)	(0.068)	(0.461)
I/N: 2-3	-375.342***,†	-345.466***	-264.118***	-328.309***	-0.316***	-1.896***
	(56.886)	(60.785)	(73.420)	(52.266)	(0.050)	(0.302)
I/N: 3+	-220.736***	-243.116***	-220.458***	-228.103***	-0.226***	-1.305***
	(63.681)	(64.998)	(50.862)	(55.472)	(0.055)	(0.317)

Observations: 562,777. Columns (1)-(4) report estimates in terms of a 10% increase in *wgap*. Column (5) reports the estimates of column (4), divided by the mean monthly earnings in *t-3* in states where the minimum wage changed between *t-3* and *t*. Column (6) reports the estimates of column (5) divided by wage elasticity of the minimum wage. The sample is restricted to workers with wages in *t*-3 at or above the minimum wage in *t*-3, and either not employed or with a wage between the minimum wage and 125% of the minimum wage at *t*. State and month-year fixed effects by poverty bin in *t*-3 included, and standard errors clustered by state. Model is fully interacted (family poverty bins are interacted with minimum wage timing, *wgap* values, the interaction of minimum wage timing and *wgap*, state dummy variables, and month-year dummy variables). Weighted using SIPP person weights.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The † symbols indicate the same levels of significance for effects relative to those of the 3+ bin.

Table 15: Estimated Effects of Minimum Wage on Conditional Monthly Earnings by Income-to-Needs Bins

Conditional on employment at t-3, low-wage workers						
	(1)	(2)	(3)	(4)	(5)	(6)
Effect of 10% *wgap* increase, lagged 1-3 months	*t*-1	*t*-2	*t*-3	Average	Col. (4)/average earnings in period t-3	Own-wage Elasticity
I/N: 0-0.5	-137.937***	-225.505***	-154.863*	-172.768***	-0.153***	-0.804***
	(48.546)	(69.497)	(89.428)	(53.280)	(0.047)	(0.248)
I/N: 0.5-1	-128.198	-215.915***	-145.077***	-163.063***	-0.147***	-0.793***
	(79.739)	(68.773)	(50.941)	(52.930)	(0.048)	(0.257)
I/N: 1-1.5	-138.764*	-16.479	-126.983	-94.075	-0.087	-0.550
	(78.032)	(108.551)	(95.047)	(81.132)	(0.075)	(0.474)
I/N: 1.5-2	-410.102***,††	-361.194***,†	-275.058***	-348.785***	-0.327***,††	-2.296***,†††
	(75.544)	(47.699)	(75.835)	(50.842)	(0.048)	(0.335)
I/N: 2-3	-364.678***,††	-349.487***,†	-204.368**	-306.178***,††	-0.288***	-1.772***,†
	(57.154)	(59.864)	(79.038)	(52.418)	(0.049)	(0.303)
I/N: 3+	-171.979***	-211.953***	-202.503***	-195.478***	-0.189***	-1.114***
	(52.999)	(54.411)	(42.648)	(42.030)	(0.041)	(0.240)

Observations: 522,674. Columns (1)-(4) report estimates in terms of a 10% increase in *wgap*. Column (5) reports the estimates of column (4), divided by the mean monthly earnings in *t-3* in states where the minimum wage changed between *t-3* and *t*. Column (6) reports the estimates of column (5) divided by wage elasticity of the minimum wage. The sample in Panel A is restricted to workers with wages in *t*-3 at or above the minimum wage in *t*-3, and either not employed or with a wage between the minimum wage and 125% of the minimum wage at *t*. State and month-year fixed effects by poverty bin in *t*-3 included, and standard errors clustered by state. Model is fully interacted (family poverty bins are interacted with minimum wage timing, *wgap* values, the interaction of minimum wage timing and *wgap*, state dummy variables, and month-year dummy variables). Weighted using SIPP person weights.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The † symbols indicate the same levels of significance for effects relative to those of the 3+ bin.

Appendix A: Additional Tables

Appendix Table A1: Wage Elasticity by Bin

Effect of 10% increase in *wgap*:	Average
I/N: 0-0.5	
Log minimum wage lagged 1-3 months	0.194
	(0.006)
I/N: 0.5-1	
Log minimum wage lagged 1-3 months	0.192
	(0.004)
I/N: 1-1.5	
Log minimum wage lagged 1-3 months	0.160
	(0.003)
I/N: 1.5-2	
Log minimum wage lagged 1-3 months	0.147
	(0.003)
I/N: 2-3	
Log minimum wage lagged 1-3 months	0.167
	(0.003)
I/N: 3+	
Log minimum wage lagged 1-3 months	0.174
	(0.002)
Observations	55,629

We compute the percentage change in wages resulting from "topping-up" wages to 10 percent above the minimum wage in t-3 when the minimum wage increases between t-3 and t. We use the subsample of observations from the employment regression (Table 11, Panel A) where the minimum wage is increasing between t-3 and t. Weighted using SIPP person weights. Standard errors reported in parentheses.

Appendix Table A2: Wage Elasticity by Bin (for *t*-3 Wages Between Old and New Minimum Wage)

	Average
I/N: 0-0.5	
Log minimum wage lagged 1-3 months	0.588
	(0.009)
I/N: 0.5-1	
Log minimum wage lagged 1-3 months	0.584
	(0.007)
I/N: 1-1.5	
Log minimum wage lagged 1-3 months	0.537
	(0.006)
I/N: 1.5-2	
Log minimum wage lagged 1-3 months	0.555
	(0.007)
I/N: 2-3	
Log minimum wage lagged 1-3 months	0.589
	(0.006)
I/N: 3+	
Log minimum wage lagged 1-3 months	0.589
	(0.004)
Observations	16,805

See Appendix Table A1 notes. Means presented here are calculated using the sub-sample of observations from the employment regression where the minimum wage is increasing between *t*-3 and *t*, and wages in *t*-3 are at and above the minimum wage in *t*-3 and below 1.1X the minimum wage in *t*-3. Weighted using SIPP person weights. Standard error reported in parentheses.

Appendix Table A3: Estimated Effects of Minimum Wage on Employment Transitions by Income-to-Needs Bins, Predicted Low-Wage Workers (Parallels Table 11)

A: Probability of employment at t, conditional on non-employment at t-3, predicted low-wage workers					
	(1)	(2)	(3)	(4)	(5)
Effect of 10% predicted *wgap* increase, lagged 1-3 months:	*t*-1	*t*-2	*t*-3	Average	Col. (4)/rate of transition to employment
I/N: 0-0.5	-0.001	0.005	0.002	0.002	0.019
	(0.006)	(0.007)	(0.006)	(0.005)	(0.049)
I/N: 0.5-1	-0.003	0.003	0.008	0.003	0.047
	(0.005)	(0.007)	(0.005)	(0.004)	(0.060)
I/N: 1-1.5	0.004†	0.012**,†††	0.006	0.007**,†††	0.096**,†††
	(0.005)	(0.005)	(0.005)	(0.003)	(0.045)
I/N: 1.5-2	0.002	0.018**,†††	-0.001	0.006††	0.081††
	(0.006)	(0.008)	(0.006)	(0.005)	(0.061)
I/N: 2-3	0.012**,†††	0.009††	-0.001	0.007††	0.085††
	(0.006)	(0.007)	(0.005)	(0.004)	(0.054)
I/N: 3+	-0.010	-0.010*	0.0001	-0.007	-0.074
	(0.006)	(0.006)	(0.005)	(0.004)	(0.049)
B: Probability of employment at t, conditional on employment at t-3, predicted low-wage workers					
	(1)	(2)	(3)	(4)	(5)
Effect of 10% predicted *wgap* increase, lagged 1-3 months:	*t*-1	*t*-2	*t*-3	Average	Col. (4)/employment continuation rate
I/N: 0-0.5	0.021†	0.012	0.022*	0.018†	0.021†
	(0.018)	(0.016)	(0.013)	(0.011)	(0.013)
I/N: 0.5-1	0.023	-0.010	-0.017	-0.001	-0.002
	(0.015)	(0.012)	(0.011)	(0.010)	(0.011)
I/N: 1-1.5	0.001	-0.009	0.014	0.002	0.002
	(0.007)	(0.010)	(0.009)	(0.007)	(0.008)
I/N: 1.5-2	-0.010	0.017*,†	0.021***,†††	0.009*	0.010*
	(0.007)	(0.009)	(0.006)	(0.005)	(0.006)
I/N: 2-3	-0.002	-0.011	-0.006	-0.006	-0.007
	(0.007)	(0.008)	(0.007)	(0.005)	(0.006)
I/N: 3+	-0.005	-0.005	0.002	-0.002	-0.003
	(0.009)	(0.006)	(0.005)	(0.005)	(0.005)

Observations: Panel A, 551,408; Panel B, 337,032. The notes from Table 11 apply to this table, except that in Panel A, the sample includes all individuals not employed in *t*-3 with predicted log wages within plus or minus 25 log points of the minimum wage in *t*; in Panel B, sample includes all individuals employed in *t*-3 with predicted log wages within 25 log points; in both panels, there is no equivalent of column (5) from Table 11 because there is no wage.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The † symbols indicate the same levels of significance for effects relative to those of the 3+ bin.

Appendix Table A4: Estimated Effects of Minimum Wage on Unconditional Usual Weekly Hours by Income-to-Needs Bins, Predicted Low-Wage Workers (Parallels Table 12)

A: Conditional on non-employment at t-3, predicted low-wage workers					
	(1)	(2)	(3)	(4)	(5)
Effect of 10% predicted *wgap* increase, lagged 1-3 months:	*t*-1	*t*-2	*t*-3	Average	Col. (4)/average hours in *t*
I/N: 0-0.5	-0.133	$0.118^{\dagger}$	-0.141	-0.052	-0.023
	(0.211)	(0.152)	(0.207)	(0.144)	(0.065)
I/N: 0.5-1	-0.071	$0.292^{*,\dagger\dagger}$	0.325^{*}	$0.182^{\dagger\dagger\dagger}$	$0.137^{\dagger\dagger}$
	(0.175)	(0.171)	(0.185)	(0.118)	(0.089)
I/N: 1-1.5	$0.339^{*,\dagger\dagger}$	$0.383^{**,\dagger\dagger\dagger}$	0.146	$0.290^{**,\dagger\dagger\dagger}$	$0.171^{**,\dagger\dagger\dagger}$
	(0.192)	(0.144)	(0.166)	(0.121)	(0.072)
I/N: 1.5-2	-0.072	$0.350^{\dagger\dagger}$	-0.134	0.048	0.027
	(0.198)	(0.232)	(0.178)	(0.147)	(0.083)
I/N: 2-3	0.170	$0.087^{\dagger}$	-0.058	0.066	0.039
	(0.215)	(0.189)	(0.151)	(0.131)	(0.077)
I/N: 3+	-0.173	-0.285^{**}	0.039	-0.140	-0.083
	(0.162)	(0.135)	(0.131)	(0.107)	(0.064)
B: Conditional on employment at t-3, predicted low-wage workers					
	(1)	(2)	(3)	(4)	(5)
Effect of 10% predicted *wgap* increase, lagged 1-3 months:	*t*-1	*t*-2	*t*-3	Average	Col. (4)/average hours in *t*
I/N: 0-0.5	0.055	0.055	-0.172	-0.021	-0.001
	(0.722)	(0.787)	(0.612)	(0.434)	(0.024)
I/N: 0.5-1	0.530	-0.905	-0.462	-0.279	-0.011
	(0.475)	(0.552)	(0.431)	(0.383)	(0.016)
I/N: 1-1.5	0.182	0.115	0.224	0.174	0.006
	(0.595)	(0.408)	(0.540)	(0.411)	(0.015)
I/N: 1.5-2	-0.265	$1.414^{***,\dagger\dagger\dagger}$	1.031^{**}	$0.727^{\dagger}$	$0.025^{\dagger}$
	(0.534)	(0.489)	(0.508)	(0.441)	(0.015)
I/N: 2-3	0.236	-0.077	-0.162	-0.001	-0.00004
	(0.338)	(0.374)	(0.398)	(0.267)	(0.010)
I/N: 3+	-0.039	-0.075	0.100	-0.005	-0.0002
	(0.257)	(0.238)	(0.210)	(0.163)	(0.007)

Observations: Panel A, 545,495; Panel B, 299,705. The notes from Table 12 apply to this table, except that in Panel A, the sample includes all individuals not employed in *t*-3 with predicted log wages within plus or minus 25 log points of the minimum wage in *t*; in Panel B, sample includes all individuals employed in *t*-3 with predicted log wages within 25 log points; column (5) reports the estimates of column (4), divided by the mean usual weekly hours (unconditional on employment) in *t* in states with minimum wage equal to the federal minimum wage; in both panels, there is no equivalent of column (5) from Table 11 because there is no wage.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The † symbols indicate the same levels of significance for effects relative to those of the 3+ bin.

Appendix Table A5: Estimated Effects of Minimum Wage on Conditional Usual Weekly Hours by Income-to-Needs Bins, Predicted Low-Wage Workers (Parallels Table 13)

A: Conditional on non-employment at t-3, predicted low-wage workers					
	(1)	(2)	(3)	(4)	(5)
Effect of 10% predicted *wgap* increase, lagged 1-3 months:	*t*-1	*t*-2	*t*-3	Average	Col. (4)/average hours in *t*
I/N: 0-0.5	-1.449††	-0.462	-2.375**,†	-1.429*,†	-0.056*,†
	(0.945)	(0.894)	(1.003)	(0.789)	(0.031)
I/N: 0.5-1	2.411*	3.015***,†	2.255	2.560***,††	0.100***,††
	(1.272)	(1.079)	(1.356)	(0.917)	(0.036)
I/N: 1-1.5	2.730***,††	1.150	0.334	1.405	0.054
	(1.018)	(1.430)	(1.467)	(1.127)	(0.043)
I/N: 1.5-2	-1.454	-0.903	-0.818	-1.059	-0.042
	(1.253)	(1.527)	(0.680)	(0.893)	(0.035)
I/N: 2-3	-2.109**,††	-1.456	-1.260	-1.609*,††	-0.069*,††
	(0.974)	(0.983)	(1.056)	(0.818)	(0.035)
I/N: 3+	0.696	0.379	-0.248	0.276	0.013
	(0.595)	(0.603)	(0.545)	(0.383)	(0.018)
B: Conditional on employment at t-3 and t, predicted low-wage workers					
	(1)	(2)	(3)	(4)	(5)
Effect of 10% predicted *wgap* increase, lagged 1-3 months:	*t*-1	*t*-2	*t*-3	Average	Col. (4)/average hours in *t*
I/N: 0-0.5	-0.453	-0.075	-0.586	-0.371	-0.017
	(0.661)	(0.829)	(0.517)	(0.385)	(0.017)
I/N: 0.5-1	-0.063	-0.684	0.153	-0.198	-0.007
	(0.394)	(0.512)	(0.377)	(0.266)	(0.010)
I/N: 1-1.5	0.241	0.587	-0.173	0.218	0.007
	(0.585)	(0.398)	(0.500)	(0.410)	(0.014)
I/N: 1.5-2	0.184	1.022***,†††	0.377	0.527	0.017
	(0.474)	(0.372)	(0.465)	(0.380)	(0.012)
I/N: 2-3	0.359	0.349	-0.028	0.227	0.008
	(0.309)	(0.296)	(0.349)	(0.228)	(0.008)
I/N: 3+	0.030	0.001	0.082	0.038	0.001
	(0.230)	(0.225)	(0.203)	(0.140)	(0.005)

Observations: Panel A, 40,089; Panel B, 268,055. The notes from Table 13 apply to this table, except that in Panel A, the sample includes all individuals not employed in *t*-3 with predicted log wages within plus or minus 25 log points of the minimum wage in *t*; in Panel B, sample includes all individuals employed in *t*-3 with predicted log wages within 25 log points; column (5) reports the estimates of column (4), divided by the mean usual weekly hours (conditional on employment) in *t* in states with minimum wage equal to the federal minimum wage; in both panels, there is no equivalent of column (5) from Table 11 because there is no wage.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The † symbols indicate the same levels of significance for effects relative to those of the 3+ bin.

Appendix Table A6: Estimated Effects of Minimum Wage on Unconditional Monthly Earnings by Income-to-Needs Bins, Predicted Low-Wage Workers (Parallels Table 14)

A: Conditional on non-employment at t-3, predicted low-wage workers

	(1)	(2)	(3)	(4)	(5)
Effect of 10% predicted *wgap* increase, lagged 1-3 months:	*t*-1	*t*-2	*t*-3	Average	Col. (4)/average earnings in *t*
I/N: 0-0.5	-8.271	-8.251	-16.017**,††	-10.846**	-0.159**
	(9.246)	(6.668)	(7.361)	(4.632)	(0.068)
I/N: 0.5-1	-6.881	6.559††	23.663***,††	7.781††	0.207††
	(6.740)	(8.217)	(7.836)	(5.264)	(0.140)
I/N: 1-1.5	20.169*,†††	12.609†††	12.686*	15.155**,†††	0.313**,†††
	(10.469)	(8.802)	(7.037)	(6.995)	(0.144)
I/N: 1.5-2	-16.282	8.413††	-2.375	-3.415	-0.065
	(10.903)	(9.862)	(9.008)	(8.261)	(0.156)
I/N: 2-3	22.801	12.114	-12.266	7.550	0.147
	(22.303)	(20.391)	(9.883)	(14.148)	(0.275)
I/N: 3+	-14.134**	-16.615***	1.229	-9.840**	-0.185**
	(6.598)	(5.418)	(6.772)	(4.373)	(0.082)

B: Conditional on employment at t-3, predicted low-wage workers

	(1)	(2)	(3)	(4)	(5)
Effect of 10% predicted *wgap* increase, lagged 1-3 months:	*t*-1	*t*-2	*t*-3	Average	Col. (4)/average earnings in *t*
I/N: 0-0.5	1.073	5.355	-6.887	-0.153	-0.0003
	(30.132)	(29.612)	(29.640)	(21.321)	(0.047)
I/N: 0.5-1	30.894†	54.547††	3.590	29.677†††	0.041††
	(23.565)	(38.697)	(23.877)	(19.056)	(0.026)
I/N: 1-1.5	29.994††	22.005††	3.670	18.556††	0.020††
	(26.843)	(23.265)	(21.390)	(19.196)	(0.021)
I/N: 1.5-2	28.311	45.123**,†††	63.044**,††	45.493**,††	0.043**,††
	(48.913)	(19.778)	(28.683)	(22.057)	(0.021)
I/N: 2-3	7.937	1.185	-2.791	2.111	0.002
	(19.107)	(22.816)	(15.448)	(12.104)	(0.011)
I/N: 3+	-33.257	-30.146*	-34.852	-32.752*	-0.034*
	(23.157)	(16.957)	(25.717)	(16.672)	(0.017)

Observations: Panel A, 551,408; Panel B, 337,032. The notes from Table 14 apply to this table, except that in Panel A, the sample includes all individuals not employed in *t*-3 with predicted log wages within plus or minus 25 log points of the minimum wage in *t*; in Panel B, sample includes all individuals employed in *t*-3 with predicted log wages within 25 log points; column (5) reports the estimates of column (4), divided by the mean monthly earnings (unconditional on employment) in *t* in states with minimum wage equal to the federal minimum wage; in both panels, there is no equivalent of column (5) from Table 11 because there is no wage.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The † symbols indicate the same levels of significance for effects relative to those of the 3+ bin.

Appendix Table A7: Estimated Effects of Minimum Wage on Conditional Monthly Earnings by Income-to-Needs Bins, Predicted Low-Wage Workers (Parallels Table 15)

A: Conditional on non-employment at t-3, predicted low-wage workers					
	(1)	(2)	(3)	(4)	(5)
Effect of 10% predicted *wgap* increase, lagged 1-3 months:	*t*-1	*t*-2	*t*-3	Average	Col. (4)/average earnings in *t*
I/N: 0-0.5	-96.074	-94.850**	-145.894***,†	-112.273***	-0.159***
	(90.442)	(42.818)	(44.719)	(41.365)	(0.058)
I/N: 0.5-1	38.882	45.435	208.814***,†††	97.710**,††	0.157**,††
	(69.780)	(77.103)	(65.357)	(41.065)	(0.066)
I/N: 1-1.5	223.645***,†††	54.655	74.319	117.540*,††	0.179*,††
	(75.622)	(88.978)	(80.386)	(66.793)	(0.102)
I/N: 1.5-2	-197.672**,†	-31.655	0.022	-76.435	-0.115
	(80.358)	(76.993)	(57.666)	(53.963)	(0.081)
I/N: 2-3	60.031	4.796	-129.624*	-21.599	-0.035
	(138.163)	(144.016)	(72.939)	(91.985)	(0.148)
I/N: 3+	-33.236	-23.046	-35.682	-30.655	-0.051
	(48.940)	(33.067)	(40.395)	(29.307)	(0.049)
B: Conditional on employment at t-3 and t, predicted low-wage workers					
	(1)	(2)	(3)	(4)	(5)
Effect of 10% predicted *wgap* increase, lagged 1-3 months:	*t*-1	*t*-2	*t*-3	Average	Col. (4)/average earnings in *t*
I/N: 0-0.5	-4.277	3.938	-19.609	-6.649	-0.012
	(30.734)	(32.734)	(33.632)	(23.055)	(0.043)
I/N: 0.5-1	13.660	69.565††	18.911	34.046*,†††	0.042*,†††
	(24.728)	(44.236)	(24.448)	(19.489)	(0.024)
I/N: 1-1.5	36.402††	37.992††	-8.902	21.831††	0.022††
	(26.925)	(23.764)	(22.359)	(20.372)	(0.020)
I/N: 1.5-2	47.003	31.437††	40.224†	39.554*,††	0.035*,††
	(50.408)	(20.356)	(28.046)	(21.859)	(0.019)
I/N: 2-3	11.442	17.261	-2.086	8.872†	0.008†
	(18.520)	(23.318)	(19.138)	(12.167)	(0.010)
I/N: 3+	-35.287	-29.575*	-37.523	-34.128**	-0.032**
	(23.460)	(16.702)	(27.078)	(16.500)	(0.015)

Observations: Panel A, 46,002; Panel B, 305,382. The notes from Table 15 apply to this table, except that in Panel A, the sample includes all individuals not employed in *t*-3 with predicted log wages within plus or minus 25 log points of the minimum wage in *t*; in Panel B, sample includes all individuals employed in *t*-3 with predicted log wages within 25 log points; column (5) reports the estimates of column (4), divided by the mean monthly earnings (conditional on employment) in *t* in states with minimum wage equal to the federal minimum wage; in both panels, there is no equivalent of column (5) from Table 11 because there is no wage.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The † symbols indicate the same levels of significance for effects relative to those of the 3+ bin.

Appendix Table A8: Estimated Effects of Minimum Wage on Employment Transitions by Income-to-Needs Bins – Placebo Estimates for High-Wage Workers (Parallels Table 11)

	Probability of employment at t, conditional on employment at t-3, high-wage workers				
	(1)	(2)	(3)	(4)	(5)
Effect of 10% predicted *wgap* increase, lagged 1-3 months:	*t*-1	*t*-2	*t*-3	Average	Col. (4)/employment continuation rate
I/N: 0-0.5	-0.005	0.008	0.026	0.009	0.001
	(0.012)	(0.015)	(0.019)	(0.011)	(0.001)
I/N: 0.5-1	0.018	$0.034^{**,\dagger\dagger}$	0.035^{*}	$0.029^{**,\dagger\dagger}$	0.003^{**}
	(0.012)	(0.015)	(0.021)	(0.013)	(0.001)
I/N: 1-1.5	-0.013	$-0.016^{**,\dagger\dagger}$	$-0.013^{**,\dagger\dagger}$	$-0.014^{**,\dagger\dagger}$	$-0.001^{**,\dagger\dagger}$
	(0.010)	(0.007)	(0.007)	(0.006)	(0.001)
I/N: 1.5-2	$-0.016^{***,\dagger\dagger\dagger}$	-0.002	-0.004	$-0.007^{*,\dagger\dagger}$	$-0.001^{*,\dagger\dagger}$
	(0.004)	(0.005)	(0.006)	(0.004)	(0.0004)
I/N: 2-3	-0.001	-0.002	0.0003	-0.001	$-0.0001^{\dagger\dagger}$
	(0.003)	(0.002)	(0.002)	(0.002)	(0.0002)
I/N: 3+	0.001	0.001	0.002^{*}	0.001	0.0001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.0001)

Observations: 2,438,245. Columns (1)-(4) report estimates in terms of a 10% increase in *wgap*. Column (5) reports the estimates of column (4) divided by the likelihood of remaining employed in *t* in states with minimum wage equal to the federal minimum wage (estimates paralleling Table 7). The sample is restricted to all individuals with hourly wages in *t*-3 at least 75% of 2.5X the minimum wage in *t*. State and month-year fixed effects by poverty bin in *t*-3 included, and standard errors clustered by state. Model is fully interacted (family poverty bins are interacted with minimum wage timing, *wgap* values, the interaction of minimum wage timing and *wgap*, state dummy variables, and month-year dummy variables). Weighted using SIPP person weights.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The † symbols indicate the same levels of significance for effects relative to those of the 3+ bin.

Appendix Table A9: Estimated Effects of Minimum Wage on Unconditional Usual Weekly Hours by Income-to-Needs Bins – Placebo Estimates for High-Wage Workers (Parallels Table 12)

	Conditional on employment at t-3, high-wage workers				
	(1)	(2)	(3)	(4)	(5)
Effect of 10% predicted *wgap* increase, lagged 1-3 months:	*t*-1	*t*-2	*t*-3	Average	Col. (4)/average hours in *t*
I/N: 0-0.5	-0.869	-0.336	1.050	-0.052	-0.001
	(1.119)	(1.201)	(1.201)	(0.921)	(0.022)
I/N: 0.5-1	1.963	1.636	0.774	1.457	0.036
	(1.172)	(1.202)	(1.163)	(0.974)	(0.024)
I/N: 1-1.5	0.197	-1.010†	0.114	-0.233	-0.006
	(0.521)	(0.606)	(0.682)	(0.495)	(0.012)
I/N: 1.5-2	-1.336***,†††	-0.176	0.122	-0.463	-0.011
	(0.447)	(0.432)	(0.413)	(0.383)	(0.009)
I/N: 2-3	0.110	0.163	0.194	0.156	0.004
	(0.216)	(0.189)	(0.211)	(0.172)	(0.004)
I/N: 3+	0.125*	0.136*	0.183***	0.148**	0.004**
	(0.070)	(0.071)	(0.061)	(0.062)	(0.002)

Observations: 2,335,959. Columns (1)-(4) report estimates in terms of a 10% increase in predicted *wgap*. Column (5) reports the estimates of column (4), divided by the mean usual weekly hours (unconditional on employment) in *t*-3 in states where the minimum wage changed between *t*-3 and *t*. The sample is restricted to all individuals with hourly wages in *t*-3 at least 75% of 2.5X the minimum wage in *t*. State and month-year fixed effects by poverty bin in t-3 included, and standard errors clustered by state. Model is fully interacted (family poverty bins are interacted with minimum wage timing, wgap values, the interaction of minimum wage timing and wgap, state dummy variables, and month-year dummy variables). Weighted using SIPP person weights.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The † symbols indicate the same levels of significance for effects relative to those of the 3+ bin.

Appendix Table A10: Estimated Effects of Minimum Wage on Conditional Usual Weekly Hours by Income-to-Needs Bins – Placebo Estimates for High-Wage Workers (Parallels Table 13)

	Conditional on employment at t-3, high-wage workers				
	(1)	(2)	(3)	(4)	(5)
Effect of 10% predicted *wgap* increase, lagged 1-3 months:	*t*-1	*t*-2	*t*-3	Average	Col. (4)/average hours in *t*
I/N: 0-0.5	-0.693	-0.687	-0.005	-0.462	-0.011
	(0.894)	(0.976)	(0.819)	(0.722)	(0.018)
I/N: 0.5-1	1.515	0.483	-0.204	0.598	0.015
	(0.993)	(1.024)	(1.125)	(0.841)	(0.020)
I/N: 1-1.5	0.660	-0.525	0.626	0.254	0.006
	(0.517)	(0.648)	(0.676)	(0.526)	(0.013)
I/N: 1.5-2	-0.722	-0.092	0.311	-0.168	-0.004
	(0.474)	(0.410)	(0.474)	(0.410)	(0.010)
I/N: 2-3	0.143	0.224	0.173	0.180	0.004
	(0.204)	(0.184)	(0.200)	(0.169)	(0.004)
I/N: 3+	0.106^{*}	0.107	0.114^{**}	0.109^{*}	0.003^{*}
	(0.061)	(0.074)	(0.049)	(0.058)	(0.002)

Observations: 2,290,776. Columns (1)-(4) report estimates in terms of a 10% increase in predicted *wgap*. Column (5) reports the estimates of column (4), divided by the mean usual weekly hours (conditional on employment) in *t*-3 in states where the minimum wage changed between *t*-3 and *t*. The sample is restricted to all individuals with hourly wages in *t*-3 at least 75% of 2.5X the minimum wage in *t*. State and month-year fixed effects by poverty bin in t-3 included, and standard errors clustered by state. Model is fully interacted (family poverty bins are interacted with minimum wage timing, *wgap* values, the interaction of minimum wage timing and *wgap*, state dummy variables, and month-year dummy variables). Weighted using SIPP person weights.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The † symbols indicate the same levels of significance for effects relative to those of the 3+ bin.

Appendix Table A11: Estimated Effects of Minimum Wage on Unconditional Monthly Earnings by Income-to-Needs Bins – Placebo Estimates for High-Wage Workers (Parallels Table 14)

	Conditional on employment at t-3, high-wage workers				
	(1)	(2)	(3)	(4)	(5)
Effect of 10% predicted *wgap* increase, lagged 1-3 months:	*t*-1	*t*-2	*t*-3	Average	Col. (4)/average earnings in *t*
I/N: 0-0.5	197.115	290.770	195.418	227.768	0.044
	(218.520)	(247.123)	(267.390)	(207.914)	(0.040)
I/N: 0.5-1	502.733*,†	-147.822	219.186	191.366	0.037
	(286.713)	(327.007)	(232.689)	(173.545)	(0.034)
I/N: 1-1.5	-60.819	-84.616	-65.725	-70.386	-0.014
	(124.632)	(91.044)	(109.499)	(96.280)	(0.019)
I/N: 1.5-2	33.587	165.631*,††	73.260	90.826	0.017†
	(161.492)	(89.744)	(137.016)	(113.520)	(0.022)
I/N: 2-3	-36.395	-21.479	-25.075	-27.650	-0.005
	(64.128)	(46.752)	(44.323)	(48.613)	(0.009)
I/N: 3+	-64.031	-46.607	-67.513	-59.384	-0.022
	(45.231)	(39.057)	(42.402)	(37.379)	(0.014)

Observations: 2,438,245. Columns (1)-(4) report estimates in terms of a 10% increase in predicted *wgap*. Column (5) reports the estimates of column (4), divided by the mean monthly earnings (unconditional on employment) in *t*-3 in states where the minimum wage changed between *t*-3 and *t*. The sample is restricted to all individuals with hourly wages in *t*-3 at least 75% of 2.5X the minimum wage in *t*. State and month-year fixed effects by poverty bin in t-3 included, and standard errors clustered by state. Model is fully interacted (family poverty bins are interacted with minimum wage timing, *wgap* values, the interaction of minimum wage timing and *wgap*, state dummy variables, and month-year dummy variables). Weighted using SIPP person weights.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The † symbols indicate the same levels of significance for effects relative to those of the 3+ bin.

Appendix Table A12: Estimated Effects of Minimum Wage on Conditional Monthly Earnings by Income-to-Needs Bins – Placebo Estimates for High-Wage Workers (Parallels Table 15)

	Conditional on employment at t-3, high-wage workers				
	(1)	(2)	(3)	(4)	(5)
Effect of 10% predicted *wgap* increase, lagged 1-3 months:	*t*-1	*t*-2	*t*-3	Average	Col. (4)/average earnings in *t*
I/N: 0-0.5	224.877	296.384	156.149	225.803	0.044†
	(213.355)	(256.385)	(284.926)	(216.409)	(0.042)
I/N: 0.5-1	485.713†	-228.066	136.047	131.231	0.025
	(296.166)	(323.633)	(236.147)	(168.570)	(0.033)
I/N: 1-1.5	-27.279	-29.217	-40.457	-32.317	-0.006
	(126.566)	(86.707)	(113.638)	(97.027)	(0.019)
I/N: 1.5-2	93.928	165.448*,††	86.834	115.403	0.022†
	(164.172)	(89.317)	(143.562)	(116.491)	(0.022)
I/N: 2-3	-30.703	-13.011	-26.236	-23.316	-0.004
	(64.394)	(49.420)	(47.603)	(50.430)	(0.009)
I/N: 3+	-69.870	-52.341	-80.121*	-67.444*	-0.025*
	(46.980)	(40.897)	(44.779)	(39.626)	(0.015)

Observations: 2,393,062. Columns (1)-(4) report estimates in terms of a 10% increase in predicted *wgap*. Column (5) reports the estimates of column (4), divided by the mean monthly earnings (conditional on employment) in *t-3* in states where the minimum wage changed between *t-3* and *t*. The sample is restricted to all individuals with hourly wages in *t*-3 at least 75% of 2.5X the minimum wage in *t*. State and month-year fixed effects by poverty bin in t-3 included, and standard errors clustered by state. Model is fully interacted (family poverty bins are interacted with minimum wage timing, *wgap* values, the interaction of minimum wage timing and *wgap*, state dummy variables, and month-year dummy variables). Weighted using SIPP person weights.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The † symbols indicate the same levels of significance for effects relative to those of the 3+ bin.