Minimum Wages and the Distribution of Family Incomes

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Abstract

I use data from the March Current Population Survey between 1990 and 2012 to evaluate the effect of minimum wages on the distribution of family incomes for non-elderly individuals. I find robust evidence that higher minimum wages moderately reduce the share of individuals with incomes below 50, 75 and 100 percent of the federal poverty line. The elasticity of the poverty rate with respect to the minimum wage ranges between -0.12 and -0.37 across specifications with alternative forms of time-varying controls and lagged effects; most of these estimates are statistically significant at conventional levels. For my preferred (most saturated) specification, the poverty rate elasticity is -0.24, and rises in magnitude to -0.36 when accounting for lags. I also use recentered influence function regressions to estimate unconditional quantile partial effects of minimum wages on family incomes. The estimated minimum wage elasticities are sizable for the bottom quantiles of the equivalized family income distribution. The clearest effects are found at the 10th and 15th quantiles, where estimates from most specifications are statistically significant; minimum wage elasticities for these two family income quantiles range between 0.10 and 0.43 depending on control sets and lags. I also show that the canonical two-way fixed effects model—used most often in the literature—insufficiently accounts for the spatial heterogeneity in minimum wage policies, and fails a number of key falsification tests. Accounting for time-varying regional effects, and state-specific recession effects both suggest a greater impact of the policy on family incomes and poverty, while the addition of state-specific trends does not appear to substantially alter the estimates. I also provide a quantitative summary of the literature, bringing together nearly all existing elasticities of the poverty rate with respect to minimum wages from 12 different papers. The range of the estimates in this paper is broadly consistent with most existing evidence, including for some key subgroups, but previous studies often suffer from limitations including insufficiently long sample periods and inadequate controls for state-level heterogeneity, which tend to produce imprecise and erratic results.

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1 Introduction

At least since Gramlich (1976), economists have recognized that the ability of minimum wage policy to aid lower-income families depends on the joint distribution of wage gains, potential job losses, and other sources of family income. However, while there is a large and active literature on the effects of minimum wages on employment, there are relatively fewer studies that empirically estimate the impact of the policy on family incomes. Compounding the problem, the existing papers suffer from a number of key shortcomings including small samples, the use of periods with limited minimum wage variation, and insufficient controls for state-level heterogeneity, all of which tend to produce somewhat erratic and imprecise estimates. Furthermore, these papers have evaluated the impact of the policy for disjoint sets of demographic groups and have focused attention on a limited set of outcomes. As a result, it is somewhat difficult to interpret the existing evidence on the topic and to assess the reliability of the findings.

In this paper, I use individual-level data from the March Current Population Survey (CPS) between 1990 and 2012 to estimate the effects of U.S. minimum wage policies on the distribution of family incomes for the non-elderly population.[1] I consider a wide range of distributional measures and demographic groups, and a utilize a rich set of controls for state-level time-varying heterogeneity. Overall, there is robust evidence that minimum wage increases lead to moderate increases in incomes at the lower tail of the family income distribution. For the poverty rate—the proportion of individuals under the federal poverty threshold—the minimum wage elasticity ranges between -0.12 and -0.30 across eight specifications, and most estimates are statistically distinguishable from zero at conventional levels.[2] The poverty-reducing effects generally extend between 50 and 125 percent of the federal poverty threshold, with the largest proportionate reductions occurring around 75 percent of the official threshold (elasticities ranging between -0.15 and -0.45). Accounting for the spatial heterogeneity in minimum wage policies suggests larger anti-poverty effects. The largest impact on the estimates comes from accounting for time-varying regional effects—which limits the identifying variation to within each of the nine Census divisions. The canonical two-way (state and year) fixed effects model—most commonly used in the literature—produces the smallest estimated magnitudes; but this model also fails some key falsification tests by implausibly suggesting income losses in the middle of the income distribution, as well as losses at the bottom prior to the minimum wage change. The most saturated model—with a separate set of year effects for each of the nine Census divisions, state specific recession controls, and state-specific linear trends—performs the best in terms of falsification tests, and estimates a poverty rate elasticity of -0.24. Allowing for lagged effects produces somewhat larger poverty rate elasticities ranging between -0.13 and -0.37, with a

[1] In this paper, when I refer to the 1990-2012 period, I am referring to the survey years for the March CPS. Note, however, that respondents in March 2012 CPS survey are asked about their income during the year 2011.

[2] All original results in this paper are for the non-elderly population; so when I refer to "the poverty rate," I am referring to the poverty rate among those under 65 years of age. Also, as a matter of terminology, in this paper virtually all elasticities are elasticities with respect to the minimum wage. For brevity, I will sometimes refer to "the elasticity of the poverty rate with respect to the minimum wage" as either "the minimum wage elasticity for the poverty rate" or simply "the poverty rate elasticity." The same is true for elasticities of other outcomes with respect to the minimum wage, such as family income quantiles, the proportion under one-half poverty line, etc.

preferred estimate of -0.36. Both the contemporaneous and lagged poverty rate elasticities from the preferred set of controls are statistically significant at conventional levels, as are the estimates from most of the other specifications. The finding that the poverty rate elasticities are larger in magnitude when controls for state-level heterogeneity are included is consistent with previous work on employment effects of minimum wages. As shown in Allegretto, Dube, Reich and Zipperer (2013), better controls for such heterogeneity tends to produce estimates of employment elasticities that are small in magnitude and often close to zero. These findings are mutually consistent with an explanation that higher minimum wages tend to be more prevalent at times and places with (relatively) worse economic outcomes.

I find evidence of poverty reduction for five demographic subgroups that have been studied in the literature. For the preferred specification, the poverty rate elasticities are somewhat larger in magnitude for black or Latino individuals (-0.4), and for children under 18 (-0.31). They are somewhat smaller for single mothers (-0.16) and for younger adults 21-44 years of age (-0.20). However, the elasticities are larger in magnitude for 21-44 year olds with no more than a high school degree (-0.27). The somewhat greater poverty reduction from minimum wage increases among disadvantaged racial minorities and those without college education is shown more clearly in this paper than in the existing literature, which provides somewhat contradictory or imprecise evidence on this matter. Finally, the elasticities are broadly similar in the 1990s (-0.29) and 2000s (-0.23), though the estimates are, as expected, less precise for the sub-samples.

Turning to alternative definitions of poverty, higher minimum wages also reduce the poverty gap and squared poverty gap, which measure the depth and severity of poverty. Using the preferred (most saturated) specification, the minimum wage elasticities for these two measures are -0.32 (poverty gap) and -0.96 (squared poverty gap), respectively. The large magnitude of the squared poverty gap elasticity is consistent with my finding that minimum wage increases lead to sizable reductions in the proportion with incomes less than one-half the poverty line: the squared gap measure is particularly sensitive to movements in very low incomes. Besides the implicit equivalence scale used by the Census Bureau for official poverty calculations, I also consider the square-root scale that is used in recent studies making international comparisons (e.g., OECD 2011, OECD 2008). For the preferred specification, the poverty rate elasticity estimate using the square root scale (-0.33) is somewhat larger than the baseline estimate (-0.24).

An additional contribution of the paper is to apply the recentered influence function (RIF) regression approach of Firpo, Fortin and Lemieux (2009) to estimate unconditional quantile partial effects (UQPEs) of minimum wages on the equivalized family income distribution. The UQPE measures how a unit increase in the minimum wage affects, say, the 10th quantile of the unconditional (or marginal) distribution of family incomes—after controlling for other covariates such as family and individual demographics, unemployment rate, state and time effects, etc. It is useful to contrast the UQPE with estimates from the more familiar (conditional) quantile regression. The quantile regression provides us with an estimate of the the impact of minimum wages on, say, the 10th conditional quantile of family incomes. This tells us how the policy affects those with unusually low income within their demographic group, e.g., a college graduate with an income that is low *relative* to others in her educational category. However, we are typically more interested in the effect of the policy on those with low incomes in an *absolute* (or unconditional) sense, while controlling for covariates such as education. This is exactly what UQPE measures.[3]

As I describe in section 3.2, there is a close link between how minimum wages affect the share of the population earning below certain income cutoffs (e.g., the poverty rate), and how they affect unconditional income quantiles. The key intuition underlying Firpo et al. (2009) is that we can invert the impact of the policy on the proportion under an income cutoff to estimate the effect of the policy on an income quantile. The RIF approach performs this inversion using a local linear approximation to the counterfactual cumulative distribution function. Estimating the RIF-UQPE essentially entails rescaling the marginal effect on the proportion above a cutoff by the probability density of the outcome at that cutoff.

I find positive effects of minimum wages on bottom quantiles of the equivalized family income distribution. The clearest impacts occur at the 10th and 15th quantiles, where estimates from most specifications are statistically significant, and the minimum wage elasticities for these family income quantiles range between 0.10 and 0.43 depending on control sets and lags. In the preferred (most saturated) specification, the family income elasticities with respect to the minimum wage are around 0.32 and 0.21 for the 10th and 15th quantiles, respectively, and diminish close to zero by the 30th quantile. When lagged effects are allowed, the long-run elasticities are slightly larger at 0.33 and 0.32 for the 10th and 15th quantiles, respectively. Overall, the evidence clearly points to moderate income gains for low income families resulting from minimum wage increases.

This paper substantially improves upon existing research on the topic of minimum wages, family income distribution and poverty. In section 2, I quantitatively assess estimates from the 12 key papers in the literature, and conclude that on balance, most of these studies point towards some poverty reducing effects from minimum wage policies. Considering nearly every extant estimate of minimum wage effect on the poverty rate, a simple “average of averages” of the 54 elasticities across 12 studies and a variety of demographic groups produces a poverty rate elasticity of -0.15; moreover, 48 of these estimates have a negative sign. Excluding the one study (i.e., Neumark et al. 2005) that, as I argue, uses a particularly unconventional and problematic methodology, the “average of averages” across the 11 other studies is -0.20. For the six of these 11 studies that actually report an estimate for overall poverty (as opposed to for narrower subgroups), the “average of averages” of poverty rate elasticities is -0.15. These averages are broadly consistent with the range of findings in this paper. However, the existing evidence is clouded by serious shortcomings in these studies: insufficient controls for state-level heterogeneity; short time periods; over-statement of precision due to improper methods of statistical inference; and the use of idiosyncratic sets of outcomes and

[3] In the case of the conditional mean, the law of iterated expectations implies that in expectation, the partial effect of an independent variable is the same on both the conditional and unconditional means of the outcome. This, however, is not true for quantiles. An alternative to the UQPE approach taken here would be to integrate the conditional quantile partial effects (CQPEs) over covariates in order to estimate the effect on the marginal (i.e., unconditional) distribution of the outcome. This route is taken in Machado and Mata (2005), who integrate over covariates via simulation.

target groups. In comparison, I use 23 years of data from a period with a tremendous amount of cross-state minimum wage variation. I also account for the fact that minimum wage variation is non-random by using a rich array of time-varying controls including division-specific time effects, state linear trends, and state-specific business cycle effects. Moreover, I assess the internal validity of various specifications using a host of falsification tests including estimating effects higher up in the income-to-needs distribution, as well as analyzing leading effects in a dynamic specification. I show that the inclusion of controls for such state-level heterogeneity tends both to improve performance on falsification tests and to increase the magnitude of the estimated elasticity of the poverty rate with respect to minimum wages.

This paper also adds to a small empirical literature on estimating distributional effects of policies by providing the first estimates of minimum wages on family income quantiles controlling for covariates. Card and Krueger (1995) estimate the impact of minimum wage changes on the 10th and 50th percentiles of family earnings using state-aggregated data and no individual-level controls. The only other paper that attempts at a full distributional analysis of minimum wages (Neumark, Schweitzer and Wascher 2005) makes much more restrictive and unrealistic assumptions about the changes in the family income distribution, and produces poverty rate elasticity estimates that are inconsistent with virtually all others in the literature, including ones from the authors' own subsequent work. Autor, Manning, and Smith (2010) estimate the effect of minimum wages on the hourly wage distribution. Unlike this paper, they do not include individual-level covariates, and for the most part use state-aggregated data.[4] There is a handful of other papers that have estimated UQPEs of policies in a difference in difference type setting. Frandsen (2012) reports effects of unionization on unconditional earnings quantiles using a regression discontinuity design. He finds that while the average effects of unionization on earnings is small, there is a sizable reduction in earnings dispersion, with large increases for bottom quantiles and some reductions at the top. Finally, Havnes and Mogstad (2012) also use RIF regressions in a difference-in-difference setting to study the distributional impact of universal child care and find that a small mean effect masks the more sizable increases in adult earnings at the bottom quantiles. To my knowledge, the latter study is the only other application of the Firpo et al. (2009) estimator to a repeated cross-sectional setting.

The rest of the paper is structured as follows. Section 2 reviews the existing literature. In section 3, I describe the data and research design, including the RIF estimation of unconditional quantile partial effects. Section 4 presents my empirical findings on the effect of minimum wages on the proportions below various low-income cutoffs as well as on income quantiles. Section 5 concludes with a discussion of the policy implications.

[4] They also estimate quantile regressions but do so without individual level covariates to avoid having to integrate the conditional quantile partial effects over the distribution of covariates.

2 Assessing the existing research on minimum wages, family incomes and poverty

In this section, I review the key papers on the topic of minimum wages and family income distribution based on U.S. data, and discuss their findings and limitations. My primary goal here is to provide a quantitative summary of the existing evidence, focusing on the poverty rate elasticity as the most commonly estimated distributional statistic. I begin by describing the process of selecting studies for this review. First, I only consider peer-reviewed publications since the early 1990s, i.e., the beginning of the "new economics of the minimum wage" literature. Second, I only include studies that report estimates for some statistic based on family incomes (such as poverty, quantiles, etc), and not other outcomes such as utilization of public assistance.[5] I review one additional paper (Neumark and Wascher 2002) that I do not include in my quantitative summary. As I explain below, their estimates on gross flows in and out of poverty do not have a clear implication for net changes in poverty. Third, studies are included only when they empirically estimate the effect of minimum wages, as opposed to simulate such effects. This selection process yields 13 studies, 12 of which are used in my quantitative summary. I note that there is also a forthcoming book by Belman and Wolfson on minimum wages, and they also provide a review of many of the same papers.[6] Finally, I note that seven of these 13 papers were also reviewed by Neumark and Wascher in their 2008 book, *Minimum Wages*; Dube (2011) discusses some of the shortcomings of that review.

As a way to quantify the existing evidence, Table 1 reports the key estimates from the 12 studies for which I could construct an elasticity of the poverty rate with respect to the minimum wage. When the original estimates are not reported as poverty rate elasticities, I use information in the paper to convert them (and standard errors) to that format for comparability.[7] To minimize the impact of subjective judgment, I have used the following guidelines for selecting estimates. (1.) I report estimates for all of the demographic groups studied in each paper; the sole exception is for workers, since minimum wages can affect who is in that group and lead to sample selection problems. (2.) When a study uses multiple econometric specifications, I include all of them in Table 1, except: (a.) the handful of estimates that did not include state and time fixed effects (or equivalent) as controls; (b.) estimates from sub-periods reported in a few of the papers, and (c.) specifications with lagged minimum wages reported in a few of the papers.[8] Overall, these guidelines lead me to

[5]I do not include Paige, Spetz and Millar (2005) in my quantitative summary as they do not consider the impact on family incomes generally, but rather only on welfare caseload. However, I note that this study stands out methodologically in using a wide array of specifications, some of which are similar to the ones used in this paper, such as state-specific trends and state-specific business cycle controls. The authors tend to find a positive impact of minimum wages on welfare caseload, which appears to go against the tenor of my findings. However, as they point out, their estimates seem to vary based on the sample period. Moreover, since the definition of family incomes used in this paper (and in official poverty estimates) include public assistance, it is possible for both poverty to fall and welfare caseload to rise.

[6]I thank Belman and Wolfson for sharing their pre-publication manuscript with me. They also discuss a number of papers which consider outcomes other than functions of family incomes, something I do not pursue here.

[7]For simplicity, I convert the standard errors to elasticities using the same conversion factor as the point estimate.

[8]The omission of lagged minimum wage estimates is solely due to space consideration, and not because I do not consider them relevant. However, including these long-run elasticities reported in three of the reviewed papers do not

report 54 elasticities in Table 1, which represent either all or nearly all of the estimates of minimum wage impact on the poverty rate available in each of the papers.[9] Finally, besides the poverty rate, I also report estimates for some of the other distributional statistics that are reported in the papers, including elasticities for proportions earning below cutoffs other than the official poverty line, family earnings quantiles, and the squared poverty gap.

In my discussion below, I mostly use a chronological order, except for the three papers by Neumark and Wascher which I discuss together at the end. After reviewing the individual papers, I provide summaries of the poverty rate elasticities in the literature. I also discuss and compare the individual estimates for specific demographic groups when I present results from my own subgroup analysis in section 4.3.

Card and Krueger (1995) consider the short run impact of the 1990 federal minimum wage increase on the poverty rate for those 16 years or older, and regress the change in the state-level poverty rate between 1989 and 1991 on the the proportion earning below the new federal wage in 1989 ("fraction affected"). While they do not report minimum wage elasticities *per se* (reporting instead the coefficient on "fraction affected"), I calculate the implicit elasticities for the poverty rate and family earnings percentiles with respect to the minimum wage for ease of comparability.[10] Their bivariate specification has an implied minimum wage elasticity for the poverty rate of -0.39, but controls for employment and regional trends reduce the overall elasticity in magnitude to the range (-0.36, -0.08), and the estimates are not statistically significant at the conventional levels. They also find that the 10th percentile of the (unadjusted) family earnings distribution responds positively to the minimum wage increase, with an implied elasticity between 0.28 (bivariate) and 0.20 (with controls); these are statistically significant at conventional levels.[11] A major problem with this analysis is that the estimates are imprecise. This is mainly due to the very short panel structure. For example, the 95 percent confidence interval associated with the poverty rate elasticity in their most saturated model is quite wide: (-0.65, 0.49). Other limitations include the use of the "fraction affected" measure of the treatment: it is possible that there were different latent trends in poverty across low- and high-wage states. Subsequent work has mostly used as the treatment measure the log of the effective minimum wage (originally suggested in Card, Katz and Krueger 1994).

Addison and Blackburn (1999) consider teens, young adults, and junior high dropouts between

alter the averages I provide below, or any of the conclusions drawn in this review.

[9] Due to space consideration, for one paper I omit two intermediate specifications that fall within the guidelines above (Addison and Blackburn 1999). These specifications did not include the unemployment rate as a control but the results were virtually identical for all three groups studied in that paper. Their exclusion also has no impact on any of the summaries I provide or conclusions I draw.

[10] The mean of "fraction affected" is 0.074, the minimum wage increased by 26.9% in 1990, and the average poverty rate in their sample is reported to be 10.6% during 1989-1991. Starting with a coefficient of -0.15 from a regression of "fraction affected" on the proportion under poverty, I multiply this coefficient by a conversion factor of $\frac{0.074}{0.269}$ to obtain a minimum wage semi-elasticity for the proportion under poverty, and then I further divide by 0.135 to obtain the minimum wage elasticity for the proportion under poverty: $-0.15 \times \frac{0.074}{0.269} \times \frac{1}{0.106} = -0.39$. I use the same conversion factor to obtain the standard errors, and perform analogous conversions for family earnings percentiles.

[11] Because they are using state-aggregated data from only two periods, these results are not subject to the criticism of using standard errors that are likely understated due to intraclass or serial correlation (Bertrand Duflo Mullainathan 2004), a problem which does affect numerous other papers in the literature as described in the text.

1983-1996. Using state-year aggregated data and two-way fixed effects, they find sizable poverty rate elasticities for teens and junior high dropouts in the range of (-0.61, -0.17), with an average of -0.43. They find more modest sized estimates for young adults (an average elasticity of -0.24). Their estimates for teens and junior high dropouts are often statistically significant, but the estimates are likely less precise than reported since they do not account for serial correlation. Additionally, their teen results are somewhat sensitive to the inclusion of state trends, as shown in Table 1. Morgan and Kickham (2001) study child poverty using a two-way fixed effects model with data between 1987 and 1996, and find a poverty rate elasticity of -0.39. Their estimate is statistically significant using panel-corrected standard errors (which however may be inadequate). Stevans and Sessions (2001) consider the overall poverty rate in the 1984-1998 period; their most comparable estimate is from a two-way fixed effects model, and appears to yield an elasticity of -0.28.[12] Gunderson and Ziliak (2004) consider the impact of a variety of social policies on the poverty rate and the squared poverty gap using both post and pre-tax income data between 1981 and 2000. For the population overall, they find a small overall poverty rate elasticity of -0.03, with a range of -0.02 to -0.06 across demographic groups. However, they specifically control for the wage distribution, including the ratio of 80th-to-20th percentile wages. This inclusion of the inequality measures is problematic, as it could block the key channel through which minimum wages would actually reduce poverty, namely raising wages at the lower end of the wage distribution.[13] Additionally, while their estimates are statistically significant, their standard errors are likely overstated since they do not account for serial correlation. DeFina (2008) uses state-aggregated data from 1991-2002 and finds that minimum wages reduce child poverty in female-headed families, including those headed by someone without a college degree. The estimated poverty rate elasticities are -0.42 and -0.35, respectively; while they are statistically significant, the standard errors also do not account for serial correlation.

Burkhauser and Sabia (2007) examine the effects on state-level poverty rates for 16-64 year olds and single mothers during the 1988-2003 period using specifications with two-way fixed effects. Depending on controls, their estimates of the poverty rate elasticity range between -0.08 and -0.19 for the population overall, and between -0.07 and -0.16 for single mothers. While none of the estimates are statistically significant, the point estimates are all negative, and the confidence intervals are consistent with sizable effects.[14] In a follow-up study, Sabia and Burkhauser (2010) consider the 2003-2007 period and income cutoffs of 100, 125, and 150 percent of the federal poverty line for the population of 16-64 year olds, and find little effect. This study is limited by a rather short sample

[12] I say “appears” because although Stevans and Sessions say they are estimating a log-log model, their Table 2 reports a “log of poverty rate” sample mean of 14.6, a “log of minimum wage” sample mean of 3.42, and a coefficient on the log minimum of -1.18. These three statistics suggest that the estimated specification was actually in levels, so that the implied elasticity is likely given by $-1.18 \times \frac{3.42}{14.6} = -0.28$. I note additionally that their standard errors also do not account for serial correlation.

[13] Another potentially problematic aspect of their methodology is the inclusion of lagged outcomes as controls along with state fixed effects; they do state in a footnote that their results are robust to various IV strategies to account for the bias. Furthermore, in contrast to other studies discussed here, Gunderson and Ziliak (2004) limit their sample to families with some positive income (not necessarily earnings).

[14] Moreover, their estimates’ precision is likely overstated due their use of conventional (as opposed to clustered) standard errors. Some of their estimates use a parametric serial correlation correction which may also be inadequate (see Bertrand Duflo Mullainathan 2004).

period. Since it is an update of their previous paper, it is unfortunate that they do not also report estimates using the full sample (1988-2007) instead of just considering a five year period. While their point estimate is small (-0.05), the 95 percent confidence interval is fairly wide (-0.34, 0.24).

Sabia (2008) uses individual level CPS data from 1992-2005, and a two-way fixed effects specification augmented with state-specific quadratic trends to study the effect on single mothers. He finds statistically insignificant but again mostly negative and often sizable estimates, with a poverty rate elasticity of -0.22 from his main specification; for single mothers without a high school degree, the estimate is larger in magnitude (-0.28) while still not statistically significant. Sabia and Nielsen (2013) use the SIPP between 1996-2007 and find an overall point estimate of -0.31 (without state-specific linear trends) or -0.03 (with trends). However, these are imprecise estimates, as the 95 percent confidence intervals are (-0.93, 0.30) and (-0.27, 0.22), respectively—the former set is consistent with nearly all other estimates in the literature. Their estimates also appear to be sensitive to the inclusion of state-specific trends, but again, the imprecision of the estimates makes it difficult to draw any firm conclusion. Overall, two of the four papers coauthored by Burkhauser and/or Sabia suggest small to modest negative effects, while the other two produce fairly imprecise or fragile estimates. However, the overall evidence from their papers does not actually rule out moderate sized poverty rate elasticities.

Neumark and Wascher have coauthored three papers that are of particular relevance. Neumark and Wascher (2002) consider movements in and out of poverty by forming two-year panels of families with matched March CPS data between 1986 and 1995. Because they do not directly estimate the effect of the policy on poverty rates, Table 1 does not include estimates from this paper. Their results seem to suggest that initially poor individuals are less likely to remain poor after a minimum wage increase, while the initially non-poor are slightly more likely to enter poverty. They interpret the greater churning as a negative attribute of minimum wages in creating "winners and losers." However, there are several major problems with the paper. First, the welfare implications of their findings on flows are far from clear. For example, the greater churning might be a positive attribute if it spreads both the gain and the pain more widely, and reduces the duration of poverty spells. Second, their estimated effects on net flows into poverty (the difference between inflows and outflows) are quite imprecise, and the standard errors are likely understated as they do not account for within-state correlations. They speculate that their results suggest that there was likely no effect on the overall poverty rate, but this would have been easy to check using a regression where the dependent variable is simply an indicator for being poor.[15]

Neumark, Schweitzer and Wascher (2005) is the only existing paper which attempts at an analysis of the impact of minimum wages on the entire distribution of family incomes. Like Neumark and Wascher (2002), they also use two-year panels of families between 1986 and 1995. They estimate the effect of discrete minimum wage treatments on the distribution of the income-to-needs ratio, and their estimates suggest that an increase in the minimum wage actually increases the fraction

[15] In general, looking at the impact of the treatment on year-to-year inflows and outflows does not tell us what its impact is on the stock. In the long run (i.e, reaching a new steady state) the effect of the treatment on the in- and outflows will have to be equal by definition, even if the stock is increased or decreased.

of the population in poverty: they report a poverty rate elasticity of +0.39. This is the only paper in the literature that I am aware of which finds such a poverty-increasing impact of the policy for the overall population, so it is important to compare its methodology to other papers on the topic as well my approach here. The authors are interested in estimating the counterfactual distribution of income-to-needs ratio for the treated state-years that experience a minimum wage increase. They implement a type of propensity score reweighting to adjust for demographic factors. Beyond this, however, there are numerous non-standard aspects of their research design. Their method does not properly account for state and year fixed effects. They "mimic" state and year fixed effects by shrinking all families' incomes by the proportionate change in the median income in that state (pooled over years) and also by analogously shrinking the median change in that year (pooled over states).[16] This constitutes an assumption that state and year effects are scale shifts that proportionately shrink the entire family income distribution. In other words, they impose the assumption that various counterfactual quantiles in states are moving proportionately to the median, which is an unattractive assumption, and much more restrictive than the inclusion of state and year dummies in a regression of the poverty rate on minimum wages.[17] Additionally, they use an *ad hoc* adjustment in the change in densities to account for the fact that some observations have both contemporaneous and lagged increases.[18] These non-standard techniques raise serious questions about the study, especially since it stands out in terms of producing a sizable positive poverty rate elasticity. To my knowledge, no one, including any of the authors, has used this methodology in any previous or subsequent paper.

In contrast, Neumark and Wascher (2011) uses a more conventional approach to study the interactive effects of EITC with minimum wages over the 1997-2006 period. Although their focus is mostly on wage and employment effects, they do provide some evidence of minimum wage effects on the share of 21-44 year olds with incomes below the poverty line and one-half the poverty line. They also report these estimates for sub-groups including single females, single females with no more than a high school degree, and single black/Hispanic females with high school or lesser education. Like most of the literature, they include state and year fixed effects; they also include demographic and state-level controls similar to this paper.[19] Unfortunately, the authors do not report an overall minimum wage effect, and instead focus on their interaction effects with EITC. However, we can use the regression coefficients along with other information provided in that paper to back out a poverty rate elasticity with respect to the minimum wage using straightforward calculations. For the broadest group that they considered—21-44 year old family heads or individuals—their results suggest a minimum wage elasticity of -0.29 for the proportion with an income under the

[16] They also report results from a specification without any time or state fixed effects at all, and the poverty rate elasticity from that specification was very similar. Since I screen on specifications to include (or attempt to include) state and time fixed effects, those estimates are not reported in Table 1.

[17] In this paper, my distributional analysis allows the shares under all income cutoffs to have arbitrary time-invariant differences by state and years, as well as time-varying differences by census divisions, state-specific recession years, and state-specific trends.

[18] Their statistical inference does not account for clustering of standard errors, which are likely understated.

[19] They mention that their estimates for the interaction between minimum wage and EITC, and minimum wage and kids are are robust to the inclusion of state-specific trends.

poverty line, and -0.45 for the proportion with an income less than half the poverty line ("extreme poverty").[20] For a group constituting the majority of non-elderly adults (and representing many children as well), the evidence from Neumark and Wascher (2011) suggests that minimum wages have a moderate-sized impact in reducing poverty and extreme poverty. These results seem to be qualitatively different from the findings in Neumark et al. (2005), and much more similar to rest of the literature. I also construct minimum wage elasticities for subgroups using estimates from Neumark and Wascher (2011), reported in Table 1. While there is not an indication of poverty reduction for single females or single mothers overall (elasticities range between 0.00 and 0.08), there is an indication of reduction in extreme poverty. There is also evidence of poverty reduction for single females and single mothers who are black/Hispanic, or without college education (elasticities range from -0.19 to -0.29).

To take stock, the results in this literature are varied and sometimes appear to be inconsistent with each other. But is it possible to filter out some of the noise and actually obtain a signal? First, I note that across these 12 studies, nearly all (48) of the 54 estimates of the poverty rate elasticity are negative in sign. Indeed, only one study by Neumark et al. (2005) suggests that minimum wages actually increase the overall poverty rate. Moreover, this study uses an unconventional methodology that is both different from all other studies, and is also problematic.

Second, if we take an "average of averages" of the poverty rate elasticities for the overall population across the seven studies that provide such an estimate so that (1) each study is weighted equally, and (2) within each study, all specifications reported in Table 1 are weighted equally as well, we obtain an average poverty rate elasticity of -0.07.[21] However, excluding Neumark et al. (2005), the "average of averages" of the poverty rate elasticities is -0.15. After excluding the one study that uses a highly unconventional technique, the existing evidence points towards a modest impact on the overall poverty rate.

Besides these seven studies, five additional studies reviewed here provide estimates for subsets of the population. If we take an "average of averages" of the poverty rate elasticities across all 12 studies, while (1) weighting each study equally, and (2) weighting each specification and group

[20] There are four minimum wage related variables included in their regression: MW, $MW \times kids$, $MW \times EITC$,$MW \times EITC \times kids$. However, since both MW and $EITC$ are demeaned, we can interpret the coefficients on MW and $MW \times kids$ as the average effects of minimum wages on adults without and with kids, respectively, evaluated at the sample average of state EITC rates. Therefore, we can ignore the $EITC$ interactions if we want to know the average impact of MWon the poverty rate. As shown in their Table 6a, for the broadest group considered in the paper (21-44 year old family head or individuals), the MWcoefficient (semi-elasticity) is -0.07 for the poverty rate (and statistically significant at the 5 percent level). For the adults with kids the relevant semi-elasticity for the poverty rate is the sum of the coefficients on MW and $MW \times kids$, and this is -0.04. From Table 1c, we know that 50 percent of this 21-44 year old family heads or individuals have kids, so the average semi-elasticity for the poverty rate is $0.5 \times (-0.07 - 0.04) = -0.055$. Again from their Table 1c, the proportion of 21-44 year olds under the poverty level is 0.19, so this translates into a poverty rate elasticity of $\frac{-0.055}{0.19} = -0.29$ for this demographic group. Analogous calculations were performed for sub-groups and for the proportion under one-half the poverty line. Because the implied elasticities involve linear combinations of coefficients, we unfortunately need more information than is reported in the paper to construct the implied standard errors.

[21] These seven studies are: Card and Krueger (1995), Stevans and Sessions (2001), Gunderson and Ziliak (2004), Neumark, Schweitzer, and Wascher (2005), Burkhauser and Sabia (2007), Sabia and Burkhauser (2010), and Sabia and Nielsen (2013). In the two studies authored by Burkhauser and Sabia, the overall poverty measure excludes those under 16 or over 64; Card and Krueger also exclude those under 16.

within study equally as well, we also obtain an elasticity of -0.15. If we exclude Neumark et al. (2005), the "average of averages" across the 11 studies is -0.20. There are, of course, other ways of aggregating estimates across studies.[22] However, when I consider the set of nearly all available estimates of the effect of minimum wages on poverty, the weight of the evidence suggests that minimum wages tend to have a small to moderate sized impact in reducing poverty.

While there is a signal in the literature that minimum wages tend to reduce poverty, it is also true that the existing evidence is clouded by serious limitations. These include (1) inadequate assessment of time-varying state-level heterogeneity, especially in light of the evidence in Allegretto et al. (2011, 2013) and Dube et al. (2010); (2) limited sample length and/or exclusion of more recent years that have experienced substantially more variation in minimum wages; (3) insufficient attention to serial and intra-group correlation in forming standard errors; (4) use of questionable estimators; and (5) frequent omission of demographic and other covariates. In this paper, I use more and better data along with more robust forms of controls to address these limitations in the existing literature.

3 Data and research design

3.1 Data and sample construction

I use individual level data from the March Current Population Survey (CPS) between 1990 and 2012. I augment the CPS data with information on state EITC supplements,[23] state per-capita GDP, and state unemployment rates from the University of Kentucky Center for Poverty Research, and state and federal minimum wages from the U.S. Department of Labor. I take the average of the effective minimum wage (maximum of the state or federal minimums) during the year for which respondents report incomes. For example, I match the the effective monthly minimum wage averaged over January through December of 2011 in a given state to respondents from that state in the 2012 March CPS.

There is extensive variation in minimum wages over the 23 year period studied in this paper. Figure 1 plots the nominal federal minimum wage, as well as 10th, 50th and 90th percentiles of the effective nominal minimum wages (weighted by population). As the figure shows, the effective minimum wage varied substantially over this period across different states. It is also the case that the last 10 years have seen much more variation in minimum wages than the previous decade. Therefore, the inclusion of more recent data is particularly helpful as it allows us to estimate the effects of the policy more precisely.

The primary goal of this paper is to characterize how minimum wage changes affect the entire distribution of family incomes; for this reason, most of the analysis is performed for the non-elderly

[22] Some other obvious candidates for aggregation point to a similar conclusion. The "median of median" elasticity across the 12 studies is -0.19. The simple mean of every elasticity in Table 1 is -0.17, while the median is -0.19.

[23] Many states specify a percentage of the federal EITC as a supplement to be paid to state taxpayers. I use this state EITC supplement rate in my analysis as a control variable.

population as a whole.[24] The exclusion of the elderly is motivated by the fact that they have much lower rates of poverty than the rest of the population, in part due to Social Security. For example, CPS data from March 2012 shows that 9.4 percent (2.7 percent) of the elderly had incomes under the poverty line (one-half the poverty line), whereas the corresponding proportions for the non-elderly population were 17.5 and 8.4 percent, respectively. For this reason, we are unlikely to learn very much about the impact of minimum wages on the bottom quantiles of the family income distribution from studying the elderly. Finally, a focus on the non-elderly is also common in the literature (e.g., Burkhauser and Sabia 2007, Sabia and Nielsen 2013).

Besides estimating the effect of minimum wages on the incomes of the non-elderly population overall, I also show key results by demographic groups similar to those that have been studied in the literature. These include (1) children under 18 years of age; (2) single (unmarried) mothers with children, (3) younger adults of 21-44 years of age, (4) 21-44 year olds with no more than a high school diploma, and (5) black or Latino individuals. As I discussed in section 2, a number of researchers have studied the impact of minimum wages on children and single mothers (e.g., Morgan and Kickham 2001, DeFina 2008, Gunderson and Ziliak 2004). Several studies have also considered younger adults, and adults with lesser education; these include Neumark and Wascher (2011), Addison and Blackburn (1999), and Sabia and Nielsen (2013). Unfortunately, the age and education categories are rarely aligned across studies. I have chosen the age group 21 to 44 primarily for the purpose of comparison with Neumark and Wascher (2011). The educational category of those with no more than a high school diploma similarly follows a number of other papers (Neumark and Wascher 2011, DeFina 2008). Finally, a number of studies (Neumark and Wascher 2011, Sabia and Nielsen 2013, Gunderson and Ziliak 2004) report results by race. My use of black or Latino individuals as a group again follows the categorization in Neumark and Wascher (2011).

3.2 Outcomes and research design

In this paper, I consider four classes of outcomes: the poverty rate, the poverty gap and the squared poverty gap, and family income quantiles. All of these are based on equivalized real family income, defined using the income-to-needs ratio, $y_{it} = \frac{Y_{it}}{FPT(N_i, Children_i, t)}$. As is standard, y_{it} is the ratio between family income, Y_{it}, and the federal poverty threshold $FPT(N_i, Children_i, t)$—which depends on family size (N_i) and the number of children, and varies by year (t). I use the same definition of family income as is used for official poverty measurement: pre-tax family income which includes earnings and cash transfers, but does not include non-cash benefits such as food stamps or housing subsidies.[25]

While most of the analysis in this paper uses the implied equivalence scale used for official

[24] Official poverty measures do not include unrelated individuals under 15 years of age; for this reason I exclude them from the sample as well.

[25] Eligible income includes earnings (excluding capital loss or gains), unemployment compensation, workers' compensation, Social Security, Supplemental Security Income, public assistance, veterans' payments, survivor benefits, pension or retirement income, interest, dividends, rents, royalties, income from estates, trusts, educational assistance, alimony, child support, assistance from outside the household, and other miscellaneous sources.

poverty calculations, there are conceptual problems with that measure. The poverty thresholds were created in 1965 by constructing minimally adequate food budgets for families of different sizes and compositions. For families of three or more individuals, the poverty threshold was defined as three times the minimal food budget. For families with less than three individuals, however, the threshold was defined as 3.7 times the food budget, to account for the smaller portion spent by these families on food. Among other issues, this creates an arbitrary threshold at three individuals. As a robustness check, I also report the results using the square root scale that is used in recent OECD publications for making international comparisons (e.g., OECD 2011; OECD 2008). Using the square root scale, the alternative federal poverty threshold, $F\tilde{P}T$, for a family with N individuals is defined simply as $F\tilde{P}T(N_i, t) = FPT(1, 0, t) \times \sqrt{N_i}$. Unlike the equivalence scale implicit in the official poverty measure, the returns to scale in household production are assumed to be smooth under this alternative.

Poverty rate and proportions under income-to-needs cutoffs

To estimate the impact of minimum wages on the proportion under a cutoff c of the income-to-needs ratio with individual data, I use a linear probability model where the dependent variable is simply an indicator for whether individual i is in a family whose income-to-needs ratio y_{it} falls below c: $I_{cit} = \mathbb{1}(y_{it} < c)$. As an example, the proportion under $c = 1$ corresponds to the official poverty rate.

The canonical two-way (state and time) fixed effects regression specification is as follows:

$$I_{cit} = \alpha_c \ln(MW_{s(i)t}) + X_{it}\Gamma_c + W_{s(i)t}\Psi_c + \mu_{cs(i)} + \theta_{ct} + \epsilon_{cit} \tag{1}$$

The coefficient α_c is a semi-elasticity of the proportion under the income-to-needs cutoff, c, with respect to the minimum wage, $MW_{s(i)t}$, indexed by the state of residence $s(i)$ of individual i and time t. Additionally, $\mu_{cs(i)}$ is the state fixed effect, θ_{ct} is the time fixed effect, and ϵ_{cit} is the regression error term. The regression coefficients and the error components are all indexed by c to clarify that they are from separate regressions for each income-to-needs cutoff c.

The vector of controls include individual-level covariates X_{it} (quartic in age, and dummies for gender, race and ethnicity, education, family size, number of own children, and marital status); and state-level covariates $W_{s(i)t}$ (unemployment rate, state EITC supplement, and per capita GDP). We can calculate the minimum wage elasticity for the proportion under c, γ_c, by dividing α_c by the sample proportion under c. Therefore, γ_1 corresponds to the elasticity of the poverty rate with respect to the minimum wage. The state-level unemployment rate and per-capita GDP are time-varying controls to account for aggregate economic trends in the state that are unlikely to be affected by the policy. All regressions and summary statistics in this paper are weighted by the March CPS sample weights. Finally, the standard errors are clustered by state, which is the unit of treatment.

A problem with the canonical model is that there are many potential time varying confounders.

As shown in Allegretto et al. (2013), high- versus low-minimum wage states over this period are highly spatially clustered, and tend be differ in terms of growth in income inequality and job polarization, and the severity of business cycles. To account for such confounders, I will report results from specifications that allow for arbitrary regional trends by the nine Census divisions, by incorporating division-specific year effects $\theta_{cd(i)t}$. This is motivated by the finding in Allegretto et al. (2011) and Dube et al. (2010) of the importance of spatial heterogeneity in estimating minimum wage effects on employment, and these papers utilize division-specific time effects as well. Additionally, I will consider specifications with state-specific linear trends, $\sigma_{s(i)}t$, to account for long run trend differences between states.[26]

Given the importance of the business cycle as a determinant of family incomes and movements in the poverty rate, I pay special attention to the issue in this paper. The inclusion of the state unemployment rate and year dummies are the usual means of accounting for cyclical factors. However, there are strong prior reasons to worry about business cycle heterogeneity across states when it comes to poverty and minimum wages. Allegretto et al. (2013) show that minimum wage increases are not uniformly distributed throughout the business cycle—they tend to occur more frequently during the second half of economic expansions. That paper also shows that states with higher minimum wages over the 1990-2012 period experienced sharper business cycle fluctuations. Moreover, states with higher minimum wages may systematically differ with respect to other attributes (such as unemployment insurance generosity) which may affect how a given change in the state unemployment rate translates into changes in family incomes or the incidence of poverty. For this reason, I also consider specifications that include state-specific recession-year indicators, $\rho_{cr(t)s(i)}$, whereby a dummy for each recessionary year is interacted with a dummy for the state: that is, state fixed effects interacted with separate dummies for each recessionary year: 1990, 1991, 2001, 2007, 2008, 2009.[27] This specification allows state level outcomes to respond arbitrarily to each recession, but as a consequence of the inclusion of the state-specific recession-year dummies, the identifying variation in such specifications is largely limited to non-recessionary periods. An added concern raised by Neumark et al. (2013) is that recessionary periods can influence the estimation of state-specific trends. As Allegretto et al. (2013) argue, this too can be handled by the inclusion of state-specific recession-year dummies.[28]

The most saturated specification is as follows:

$$I_{cit} = \alpha_c \ln(MW_{s(i)t}) + X_{it}\Gamma_c + W_{s(i)t}\Psi_c + \mu_{cs(i)} + \theta_{cd(i)t} + \rho_{cr(t)s(i)} + \sigma_{s(i)}t + \epsilon_{cit} \qquad (2)$$

Besides equations 1 and 2, I also show results from all of the six intermediate specifications with combinations of the three sets of controls (division-specific year effects, state-specific recession-year

[26] Using quadratic instead of linear trends produced virtually identical results.

[27] These correspond to CPS survey years 1991, 1992, 2002, 2008-2010.

[28] In studying minimum wage effects on welfare caseloads, Paige, Spetz and Millar (2005) also use state-specific business cycle controls, although they interact the unemployment rate with state dummies. My results using the unemployment rate interaction as opposed to recession-year interactions produced qualitatively similar results, as I discuss in footnote 34.

effects, and state linear trends), and discuss the full range of estimates. Additionally, I assess the relative contribution of each of the three sets of controls in explaining the difference between estimates from equations 1 and 2.

I estimate a series of regressions for alternative income-to-needs cutoffs. In the main tables, I report the impact of minimum wages on the proportions below the following cutoffs: 0.50, 0.75, 1.00, 1.25, 1.50, 1.75 and 2.00 times the federal poverty threshold. In the figures (and appendix tables), I show the effects between 0.50 and 3.00 times the threshold, which is close to the median income-to-needs ratio in the sample (3.04). I consider a wide range of income cutoffs for several reasons. First, the official poverty line may inadequately account for costs associated with a minimally acceptable standard of living, and alternative approaches define hardship considerably more broadly (e.g., Allegretto 2006). Second, there is an inherent arbitrariness in choosing any specific threshold. And third, the goal of this paper is to provide a full picture of how minimum wage policies affect the cumulative distribution of family earnings. For this reason, the figures show the impact (and confidence bounds) on proportions below all cutoffs between 0.50 and 3.00 times the federal poverty threshold in intervals of 0.25. Together, these estimates characterize the impact of the policy on the bottom half of the income-to-needs distribution. The estimates for cutoffs near the middle of the distribution are also useful as falsification tests, since we do not expect the minimum wage to substantially affect incomes in that range.

Unconditional quantile partial effects

When we estimate the impact of a policy on the proportion of individuals below various income cutoffs, and do so for a large number of such cutoffs, the results summarize the effect of the policy on the cumulative distribution function (CDF) of family incomes. This is an example of *distribution regressions* as discussed in Chernozhukov, Fernandez-Val and Melly (2013). Moreover, if we have estimates for the impact of the policy on the CDF for all values of an outcome y, we can then invert the impact of the policy on the CDF to estimate the effect of the policy on a particular quantile Q_τ of y. Figure 2 illustrates the concept: $F_A(y)$ is the actual CDF of the outcome y, say equivalized family income. The function $F_B(y)$ represents the counterfactual CDF, showing the distribution that would have occurred absent the treatment—say, a small increase in the minimum wage. Under the assumption of conditional independence of the treatment, $F_B(y)$ is estimable using distribution regressions such as equations 1 or 2 of the outcome $I_c = \mathbb{1}(y)$ on the treatment, along with a set of covariates, for every value of c. The resulting estimates would fully characterize the impact of the treatment on the CDF of y, i.e., $F_B(y) - F_A(y)$, and hence form an estimate of the counterfactual distribution $F_B(y)$.

Say we are interested in the effect of the policy on the τ^{th} quantile of the outcome y. The unconditional quantile partial effect (UQPE) estimand is defined as: $Q_{B,\tau} - Q_{A,\tau} = F_B^{-1}(\tau) - F_A^{-1}(\tau)$. It is a partial effect of minimum wages, since the distribution regressions used to estimate the counterfactual, $F_B(y)$, hold other covariates constant. It is an unconditional quantile effect because it measures the impact of the policy on quantiles of the unconditional (or marginal) distribution of y, which is more directly economically interesting than the conditional quantile partial effect (CQPE) that is the estimand associated with the quantile regression (Koenker and Bassett 1978). The latter represents the impact of the treatment on the τ^{th} quantile of the distribution of y conditional on covariates. For example, the CQPE informs us of the impact of minimum wages on those with low family incomes within their educational group—be they college graduates or junior high dropouts. However, when thinking about distributional effects, we are not as interested in the impact of minimum wages on college graduates with unusually low family incomes–i.e., who are poor relative to other college graduates. We are more interested in the impact on those with low incomes in an absolute (or unconditional) sense.[29] We do wish to *control for* factors like education, but do not wish to *condition* the distributional statistic on (e.g., define "low income" based on) those factors. The UQPE, $Q_{B,\tau} - Q_{A,\tau}$, controls for covariates, but does not define the quantiles based on them; hence, it captures the effect of the policy on the bottom quantiles of the unconditional distribution.

It is possible to estimate the UQPE for the τ^{th} quantile by (1) estimating the effect of the policy on the proportions under a large set of cutoffs, c, and forming an estimate for the counterfactual distribution $F_B(\tau)$, and then (2) globally inverting that distribution function and obtain an estimate for $F_B^{-1}(\tau)$ and hence an estimate for $F_B^{-1}(\tau) - F_A^{-1}(\tau)$. This procedure is feasible, and outlined in Chernozhukov et al. (2013). However, it is computationally demanding as it requires estimating a very large number of distribution regressions to globally invert $F_B(y)$ and estimate the quantile effects. As described in Firpo et al. (2009) and Fortin et al. (2010), we can also invert the counterfactual distribution function using a local linear approximation. Figure 2 provides the intuition behind this approach. We begin by defining a cutoff c associated with quantile τ such that $F_A(c) = \tau$ using the actual distribution. Next, we estimate the effect of the policy on the proportion below c using a single distribution regression. The effect on the proportion is graphically represented as $\Delta = (F_B(c) - F_A(c))$ in Figure 2. Now, the quantity $Q_{B,\tau} - Q_{A,\tau}$ can be locally approximated by the product of the vertical distance $-\Delta = -(F_B(c) - F_A(c))$ divided by the slope of the distribution function at $F_A(c) = \tau$, which is just the PDF of y at the τ^{th} quantile: $f_A(F_A^{-1}(\tau))$. The green dashed triangle shows the geometry of this local linear approximation, which can be written as $UQPE \approx -\frac{F_B(c) - F_A(c)}{f_A(c)}$. While the global inversion would require us to estimate a large number of regressions for different values of c in order to obtain the estimate for a single quantile Q_τ, only one regression is needed for each quantile when inverting locally.

The key simplification here is taking a linear approximation to the counterfactual CDF which greatly simplifies the problem of inverting the counterfactual distribution function. This linearization works well for a relatively continuous treatment with a substantial variation in treatment intensity, and less well for lumpy or discrete treatments. Given the fairly continuous variation in minimum wage changes, the approximation error is unlikely to be a major concern here. Later in this section, I discuss a few additional features of the data that further reduce the scope of the approximation

[29]To be clear, both the UQPE and CQPE measure the effect of the treatment on low income quantiles, and not specifically on people who would have earned low incomes (in either a conditional or an unconditional sense) absent the policy. The two concepts coincide only under the additional assumption of rank invariance, i.e., that the treatment does not alter the ranking of individuals.

error.

To operationalize the estimation, Firpo et al. use as the dependent variable the recentered influence function of y. The RIF for the τ^{th} quantile, Q_τ, is as follows:

$$RIF(y_{it}, Q_\tau) = \left[Q_\tau + \frac{\tau}{f(Q_\tau)}\right] - \frac{\mathbb{1}(y_{it} < Q_\tau)}{f(Q_\tau)} = k_\tau - \frac{\mathbb{1}(y_{it} < Q_\tau)}{f(Q_\tau)} \quad (3)$$

Since the first term in the bracket is a constant, the regression estimate for the UQPE at the τ^{th} quantile is simply a rescaled effect of the impact on the proportion under $c(\tau) = Q_\tau$, where the scaling factor is $-\frac{1}{f_A(Q_\tau)}$. This corresponds to the graphical demonstration of the technique in Figure 2.

I estimate a series of regressions for alternative quantiles, Q_τ. Again, I use a range of controls for time-varying heterogeneity across eight different specifications. The most saturated specification is as follows:

$$RIF(y_{it}, Q_\tau) = \beta_\tau \ln(MW_{s(i)t}) + X_{it}\Gamma_\tau + W_{s(i)t}\Phi_\tau + \pi_{\tau s(i)} + \theta_{\tau d(i)t} + \sigma_{\tau s(i)}t + \rho_{\tau r(t)s(i)} + \epsilon_{\tau it} \quad (4)$$

β_τ is the minimum wage semi-elasticity for the UQPE at the τ^{th} quantile of equivalized family income. Note that $\beta_\tau = \alpha_{c(\tau)} \frac{1}{f(c(\tau))}$, so there is a one-to-one correspondence between the estimates from equations 2 and 4. To obtain the minimum wage elasticity for the τ^{th} income quantile, we divide β_τ by $Q_\tau = c(\tau)$, so $\eta_\tau = \frac{\beta_\tau}{c(\tau)}$. Since both Q_τ and $f(Q_\tau)$ are estimated, in principle, the standard errors can be computed using bootstrapping. However, I find that the additional contribution of these estimations to the overall variance of the $\hat{\beta}_\tau$ to be small, and for this reason the results here report standard errors without accounting for the estimation of Q_τ and $f(Q_\tau)$ due to computational reasons.[30]

A number of features of the data make it attractive for the application of the RIF-UQPE approach. Table 2 and Figure 3 show the cumulative distribution function for the income-to-needs ratio. I note that the CDF is nearly linear in the bottom half of the distribution, especially between income-to-needs ratios of 0.75 and 2.50, which roughly correspond to the 10th and 40th percentiles: in this range the PDF is essentially flat.[31] This is an useful feature of the data when it comes to the estimation of the UQPE , since the linearity of the actual CDF (in combination with a continuous treatment) reduces the scope of the approximation error when inverting the counterfactual CDF using the RIF approach, which is based on a linear approximation.

Additionally, Figure 4 shows that the income quantiles at the bottom of the distribution have been fairly stationary over the past two decades, although they do exhibit pro-cyclical tendencies.

[30] Using block-bootstrapping by state, I find that accounting for the estimation of the density around the cutoff increases the standard error by less than 3% in the case of the $c = 1$ or right at the poverty threshold. Given the sample sizes, the large number of specifications and cutoffs, and the large number of covariates due to division-year dummies and state trends, the computational burden from using bootstrapped standard errors is substantial, and I do not pursue this strategy here.

[31] The kernel density estimation uses an Epanechnikov kernel and the STATA default bandwidth based on Silverman's rule-of-thumb.

This is corroborated in Table 3, which shows that the proportions below various income-to-needs cutoffs were quite similar in the 1990s and the 2000s. Figure 4 also shows that the probability densities at the associated income-to-needs cutoffs ($f_A(c(\tau))$ have also been fairly stable over time, with the possible exception of the 5th quantile. The relative stability of the income-to-needs quantiles and densities is relevant for interpreting the UQPE estimates. The estimation of the UQPE for a particular quantile, τ, is based on changes in the proportion below the income-to-needs cutoff $c(\tau)$ associated with that quantile, along with the probability density of the income-to-needs ratio at that cutoff, $f_A(c(\tau))$. Both $c(\tau)$ and $f_A(y)$ are calculated by averaging over the entire sample. The relative stability of the mapping between c and τ over this period suggests that the estimated impact on income around a given cutoff c is referring to roughly the same quantile over this full period.

Finally, the use of the full-sample distribution to estimate the cutoff $c(\tau)$ and the density $f_A(c)$ may be an issue if the treatment and control units had very different income distributions. However, all states receive treatment at some point during the sample, and the variation in minimum wages is fairly continuous and widespread; therefore, the the sample-averaged cutoffs and densities are broadly representative of where the minimum wage variation is coming from. Overall, the nature of both the treatment as well as the outcome facilitate the application of the RIF approach to a repeated cross-sectional setting.

Other distributional measures: gap and squared gap indices

An attractive feature of the RIF approach is that it allows us to use individual level data to estimate the impact of of minimum wages on a variety of distributional statistics. For example, an additional statistic for measuring poverty is the poverty gap index, which measures how much we would need to increase incomes to bring everyone up to the poverty line. As such, it is more sensitive to the depth of poverty than is the poverty rate. As shown in Essama-Nssah and Lambert (2012), for a given cutoff, c, we can define the recentered influence function for the gap index as:

$$RIF(y_{it}, gap(c)) = \begin{cases} (1 - \frac{y_{it}}{c}) & if \;\; y_{it} < c \\ 0 & if \;\; y_{it} \geq c \end{cases}$$

Similarly, we can also estimate the impact of the policy on the squared poverty gap, which is used to measure the severity of poverty. The squared gap measure is more sensitive to income movements far below the cutoff, c. As also shown in Essama-Nssah and Lambert, the recentered influence function for the squared gap index is simply $RIF(y_{it}, squared\, gap(c)) = [RIF(y_{it}, gap(c))]^2$. I show the impact of minimum wages on these two additional poverty related indices for income-to-needs cutoffs ranging between 0.50 and 2.00.

Dynamic effects

I also estimate dynamic specifications with a one-year lead and a one-year lag of log minimum wage, in addition to the contemporaneous value. I do so for both the poverty rate and the unconditional quantile regressions. For example, for the UQPE regressions, I estimate:

$$RIF(y_{it}, Q_\tau) = \sum_{k=-1}^{1} \beta_{\tau,k} \ln(MW_{s(i),t+k}) + X_{it}\Gamma_\tau + W_{s(i)t}\Phi_\tau + \pi_{\tau s(i)} + \theta_{\tau d(i)t} + \sigma_{\tau s(i)}t + \rho_{\tau r(t)s(i)} + \epsilon_{\tau it} \tag{5}$$

In this distributed lag specification, I define $(\beta_{\tau,-1})$ as the "leading value", and $(\beta_{\tau,0} + \beta_{\tau,1})$ as the "long term effect" on quantile Q_τ. There are two distinct motivations behind the dynamic specification. First, the "leading values" provide us with a falsification test to discern the reliability of a research design. A statistically significant or sizable leading value, $\beta_{\tau,-1}$, indicates that the specification may not be able to account for pre-existing trends, and hence may provide misleading estimates. For example, Dube et al. (2010) and Allegretto et al. (2011, 2013) show that the canonical two-way fixed effects model often fails this falsification test when it comes to minimum wage impact on teen and restaurant employment. During the past 25 years, minimum wage increases have tended to occur at times and places where low-wage employment was unusually low or falling, and the two way fixed effects model is unable to account for these pre-existing trends. For this reason, I subject all the specifications to the leading value falsification test, and use this information as a criteria for model selection.

A second motivation for the dynamic specification is to allow for lagged effects from the policy change, and the $(\beta_{\tau,0} + \beta_{\tau,1})$ term better captures the longer run effect of the policy. Previous work such as Addison and Blackburn (1999), Sabia (2008) and Sabia and Nielsen (2013) also consider lagged effects, although their conclusions do not appear to be substantially affected by the inclusion of lags. The explicit inclusion of the lagged treatment variable may be of particular relevance when the specification includes a state-specific linear trend. With state trends, but without lagged treatment included as a regressor, a delayed impact can lead to a mis-estimation of the state trends, attenuating the measured effect of the treatment (Wolfers 2006). Explicit inclusion of lagged minimum wages mitigates this problem. A few of the papers reviewed in section 2 have shown results using state trends or with lagged minimum wages, but not with both.

3.3 Descriptive statistics

Table 3 shows various distributional measures for the non-elderly population, as well as the five key demographic groups, using alternative income-to-needs cutoffs between 0.50 and 3.00. For these groups I calculate the proportions below the cutoffs using the standard equivalence scale (columns 4 through 11), and the square root scale (column 1). For the overall non-elderly population, I also show the gap index, and the squared gap index for the same cutoffs (using the standard equivalence scale). To clarify, for income-to-needs cutoff of 1.00, the columns 1, and 4 through 11, show the headcount poverty rates; column 2 shows the poverty gap index, and column 3 the squared poverty gap index.

For non-elderly adults as a whole, the poverty rate stayed stable at 0.15 over the 1990s and 2000s. The poverty rate for single mothers (0.38), black/Latino individuals (0.28), and children (0.21) were all higher than the average. Among adults 21-44 year old, those with high school or lesser education had greater rates of poverty (0.21) than all adults of that age (0.14). These patterns are as expected, and are qualitatively similar when we consider income-to-needs cutoffs of 0.50 and 1.50 instead of 1.00. Moreover, the overall poverty rates do not differ substantially if we use the square root equivalence scale. Finally, the gap and squared gap indices tend to be somewhat less sensitive to the choice of income-to-needs cutoff than the headcount rate. For example, whereas moving the cutoff from 50 to 150 percent of the official poverty line increases the headcount rate by a factor of 3.4, it increases the gap index by a factor of 2.8, and the squared gap index by a factor of 2.3. This pattern reflects the greater sensitivity of the two gap measures to income changes further below the cutoff, as compared to the headcount rate, which only measures income movements near the cutoff.

4 Empirical findings

4.1 Main results for the poverty rate, and proportions below low-income cutoffs

Table 5 provides the estimates for the impact of minimum wages on the proportions under alternative income-to-needs cutoffs. For ease of interpretation, I report the estimates as elasticities ($\hat{\gamma_c}$) by dividing the regression coefficients ($\hat{\beta_c}$) by the sample proportion under each cutoff; this is true both for the point estimate and the standard errors.[32] The underlying regression coefficients, or semi-elasticities, and standard errors are reported in Appendix Table A1. I use eight different regression specifications that range from the canonical two-way fixed effects model in column (1) to the most saturated specification in column (8) which includes (a) division-specific year effects, (b) state-specific recession-year dummies, and (c) state linear trends. The six specifications in columns (2) through (7) exhaust all intermediate combinations of controls and provide us with evidence on how the inclusion of various types of time-varying controls affects the estimates.

First, I note that there is robust evidence that minimum wage increases reduce the share of individuals with very low family incomes. For income-to-needs cutoffs between 0.50 and 1.25 (i.e., between 50 and 125 percent of the official poverty threshold), and across the eight specifications, 30 out of the 32 estimates are negative in sign, and 22 are statistically significant at least at the 10 percent level. The canonical model in specification 1 stands out as the only one where none of the estimates for these income-to-needs cutoffs are statistically significant. Moreover, in the range where there are the strongest effects (i.e., income-to-needs cutoffs between 0.50 and 1.25), the point estimates from specification 1 are uniformly the smallest in magnitude. For example, specification 1 suggests a poverty rate elasticity of -0.12, which is similar to the average estimate of -0.13 in Burkhauser and Sabia (2007). However, for all other specifications (2-8), we find statistically significant poverty rate elasticities between -0.13 and -0.30. Moreover, we generally find evidence of reductions in the share under 75, 100 and 125 percent of the federal poverty threshold across

[32]Since I divide both the regression coefficient and the standard error by the sample proportion under the cutoff, I am not accounting for the estimated nature of the sample proportion. However, I note that doing so would increase the estimated standard errors for the elasticities by a very small amount.

specifications 2-8. The share under 50 percent of the poverty threshold is also estimated to fall substantially when using within-division variation as in specifications 5-8.

Figure 5 provides corresponding visual evidence on how minimum wages affect the bottom half of the income-to-needs distribution. The most saturated specification 8 suggests that the distribution of family incomes with higher minimum wages first-order stochastically dominates the distribution with a lower value of the minimum. The shares below cutoffs are smaller for cutoffs up to 2.00 or so, and unchanged thereafter. Specification 1 suggests a different (and anomalous) pattern, with a rise in the share below cutoffs in the middle of the distribution. However, analogous graphs for most intermediate specifications, as shown in Figure A1, also corroborate the evidence that minimum wages tend to reduce shares of individuals with low incomes without significantly affecting the rest of the distribution.

The range of estimates raises the issue of model selection. There is an *a priori* case for using more saturated specifications that better account for time-varying heterogeneity across states. Allowing for time-varying regional effects and state-specific trends makes both intuitive sense, and receives strong support in existing work. For example, Allegretto et al. (2013) show that the inclusion of these controls mitigates contamination from pre-existing trends when it comes to estimating the effect of minimum wages on teen employment. They also provide evidence that synthetic control methods tend to put substantially more weight on nearby states in constructing a control group, providing additional validity to the intuition that nearby states are better controls. They further show that the amplitude of business cycles tend to be greater in states with higher minimum wages, suggesting that business cycle heterogeneity may be an important factor to control. The main argument against using more saturated models would be that they lack the statistical power to detect an effect.[33] In reality, however, for the relevant range of income-to-needs cutoff, the point estimates in specifications 2-8 are larger in magnitude than the canonical specification 1, while the standard errors are not necessarily so. Based on both *a priori* and *a posteriori* considerations, it is difficult to argue for the least saturated specification, while there is a strong case for preferring the most saturated model.[34]

Beyond this, I consider two types of falsification tests for model selection. First, I consider higher income thresholds falsification tests: these are minimum wage elasticities for proportion earning below 2.50 or 3.00 times the poverty line. It is safe to say that we should not expect minimum wages to affect the proportion earning under 3.00 times the poverty threshold, which roughly corresponds

[33] A second rationale for excluding covariates is that some of them are "bad controls" in the sense of blocking a causal pathway between the treatment and the outcome. As I discussed above, the state-specific linear trends may constitute a problem if there are delayed effects of the policy, but this can be mitigated by including lagged treatment variables. I assess this issue later in this section.

[34] Additional variations in the control set did not qualitatively affect the findings here. As noted earlier, Paige, Spetz and Millar(2005) account for state-specific business cycle controls by interacting the unemployment rate with state dummies, and also use state-specific trends. When I estimate that model, the elasticities (and standard errors) for the proportions under 0.50, 0.75 and 1.00 times the poverty line are -0.221(0.073), -0.173(.076) and -0.161(0.056), respectively. For comparison, my analogous specification 4 (with recession year interactions) produces broadly similar estimates of -0.138(0.091), -0.202(0.105) and -0.146(0.070), respectively. The same is true when division-specific time effects are included in each of the previous two specifications. Finally, quadratic instead of linear trends by state produced virtually identical results.

to the median equivalized family income in the national sample. Therefore, reliable specifications should produce estimates for these cutoffs that are small or close to zero. Appendix Figure A1 plots the elasticities and the 95 percent confidence intervals for income-to-needs cutoffs between 0.50 and 3.00 for all eight specifications. We find that the estimates from the canonical specification 1 suggest that minimum wages *increase* the proportion of families with incomes under cutoffs ranging between 2.00 and 3.00 times the poverty threshold, and all of these estimates are statistically significant at the 5 percent level. These suggest that the state and year fixed effects (and the control variables) are not sufficiently capturing the non-random nature of minimum wages, which seem to be higher at times and places with an unusually large fraction of the state population with family incomes below the national median. In general, the inclusion of state-specific recession-year dummies and state-specific linear trends both tend to incrementally improve the performance when it comes to higher income thresholds falsification tests. By and large the best performance occurs for the most saturated specification 8, where the elasticities for thresholds of 2.00 or greater are virtually identical to zero. While the canonical specification 1 is the only one where there are statistically significant estimates at the middle of the distribution, some of the intermediate specifications (e.g., 3 and 5) also have non-negligible point estimates (see Appendix Figure A1).

Second, I consider the dynamic estimates from models similar to equation (5) which include as regressors a one-year leading and one-year lagged log minimum wage in addition to the contemporaneous value. I use the leading values as a second falsification test, analogous to tests used in Dube et al. (2010), Allegretto et al. (2010) and Allegretto et al. (2013). The results are shown in Table 5 and Appendix Figure A2. They indicate that specifications 1-4 without division-year controls all produce spurious positive estimates for the proportion below one-half the poverty line, and these are statistically significant at the 5 percent level.[35] Some of the specifications (especially specification 5) tend to produce spurious negative estimates at the income-to-needs cutoff of 0.75. Considering the full range of cutoffs, the most saturated specification 8 usually performs the best when it comes to the leading values falsification test, much like the higher income threshold falsification test.

Overall, the canonical two-way fixed effects model used in most of the existing studies fails falsification tests across the board when it comes to effects prior to the wage increase, as well as effects in the middle of the income distribution. Moreover, it does so in the same direction, suggesting higher minimum wages are correlated with negative economic outcomes unrelated to the policy. This is also consistent with the results from similar falsification tests in the context of employment effects from this period, which also suggest pre-existing trends contaminate the estimates from this canonical model (e.g., Allegretto et al. 2013). Moreover, the most saturated specification 8 performs very well on the falsification exercises, while the results from the intermediate specifications vary. Therefore, based both on *a priori* grounds as including the richest set of controls for time-varying heterogeneity, as well as its performance on the falsification tests, I consider 8 to be the preferred specification. However, I recognize that reasonable observers may disagree on exactly

[35] The spurious positive leading minimum wage estimate for income-to-needs cutoff of 0.50 suggests that the lack of finding a reduction in the proportion under that cutoff in specifications 1-4 (see Table 4) may be driven by pre-existing trends.

which specification is ideal, or may place somewhat different weights on the evidence associated with each specification. For this reason, in this paper I often report the range of estimates across all eight specifications.

We can also use the dynamic models to study longer term impact of minimum wages. In Table 6, I report the "long-run" effect, which is the sum of the contemporaneous and one-year lagged log minimum wage coefficients, again converted to elasticities. (The actual sums of the coefficients, which are semi-elasticities, are reported in Appendix Table A2.) These estimated effects beyond the first year of policy change are typically as large or larger in magnitude as the estimates without lags. Among the 32 estimates for proportions below the income-to-needs cutoffs between 0.5 and 1.25, 24 of the long-run minimum wage elasticities are statistically significant at least at the 10 percent level. Of these 32 cases, in 23 the estimates with lagged effects are larger than their counterparts in Table 4, while most of the rest are similar. Of the 16 cases from specifications that include a state linear trend, 14 are larger when lagged minimum wages are included. In contrast, for the 16 cases without state trends, 9 are larger while the other 7 are not. Therefore, the inclusion of lags appears to mitigate the attribution of delayed effects to the estimation of state-specific trends, similar to Wolfers (2006).

For the proportion under one-half the poverty line, the long-run elasticities range between -0.28 and -0.40. Unlike the estimates in Table 4 without lags, now even specifications 1-4 suggest a clear reduction in the share below this cutoff. For the proportion under the poverty line, the long-run elasticities range between -0.13 and -0.37, as compared to the elasticities between -0.12 and -0.30 in Table 4. The preferred specification 8 suggests a long-run poverty rate elasticity of -0.36, somewhat larger than the elasticity of -0.24 without lags. Importantly, the long-run effects from the canonical model suggest sizable and statistically significant reductions in the proportions below 50 and 75 percent of the poverty threshold; even the poverty rate elasticity of -0.13 is statistically significant at the 10 percent level. Finally, as shown visually in Figure 7, the long-run elasticities for specification 1 are somewhat better behaved (i.e., closer to zero) for higher-income cutoffs. The preferred specification 8 continues to show sizable reductions at the bottom, tapering off to close to zero by 200 percent of the poverty threshold. The long run elasticities are plotted for all eight specifications in Appendix Figure A3. Although the estimates tend to be less precise than those from models without lags (Appendix Figure A1), the point estimates almost uniformly point toward moderate sized reductions in the low income shares, coupled with typically small and statistically insignificant effects at two or three times the poverty threshold.

4.2 Source of heterogeneity—trends, regions and business cycles

As the previous section shows, there are substantial differences in the minimum wage elasticities for low-income shares from the least and most saturated specifications (i.e., specifications 1 and 8, respectively). Since the most saturated specification includes three additional sets of time-varying controls—division-specific time effects, state-specific recession-year effects, state linear trends—it is somewhat difficult to disentangle their relative contributions. In this section, I provide some additional evidence on this question by decomposing the difference between these two specifications into components attributable to each set of controls.

A challenge for such a decomposition is that the results depend on the order in which the controls are added. There are exactly six different orderings for incrementally adding the three sets of controls going from specification 1 to specification 8, and each of these orderings provides a different decomposition.[36] In Appendix Table A3, I report the incremental contributions of these three sets of controls averaged over all six orderings. I do so for the four income-to-needs cutoffs between 0.5 and 1.25, which constitutes the range where the minimum wage appears to have an effect. The top panel A presents the results from regressions with contemporaneous minimum wages only. The first section of the panel reports the contributions of each control set in terms of the actual elasticity estimates in Table 4; the second section converts these into proportions of the total difference between estimates from specifications 1 and 8. The last row of the panel further averages these proportions over the four income-to-needs cutoffs to provide an overall decomposition. What is clear from panel A is that the biggest impacts come from the inclusion of division-specific year effects (74 percent), followed by the state-specific recession-year effects (39 percent). Indeed, these two sum up to 113 percent, as they are offset by the average incremental impact of the state trends (-13 percent) which actually tend to, on average, slightly reduce the magnitudes of the elasticities.

We can also do an analogous decomposition for the long term effects, which are reported in Panel B of Appendix Table A3. Here, we find that it is the state-specific recession-year controls that make the most difference (77 percent) followed by division-year effects (25 percent). The inclusion of state-specific trends does little, on average, to explain the gap between the two specifications for any of the income cutoffs.[37]

To be sure, there are other reasonable ways to quantify the relative contributions of these sets of controls. However, the take-away from this exercise (and from a casual inspection of Tables 4 and 6, or Appendix Figures A1 and A3) is that the inclusion of time-varying regional effects, and controls for heterogeneous impacts of the business cycle, matter substantially. And unlike state-specific trends—which have smaller and more ambiguous impact on the size of the estimates—these two sets of controls have not been used in the existing literature on minimum wages and family incomes. It is an interesting question why the inclusion of business cycle heterogeneity seems to matter relatively more when lagged effects are included. One possible explanation concerns the timing of minimum wage increases, which tend to occur more frequently in later parts of economic expansions (Allegretto et al. 2013). As a consequence, the estimation of lagged effects may be more likely to encounter the heterogeneity of business cycles. However, further research is needed to gain insight

[36] Denoting the three sets of controls as D (Division-specific time effects), R (state-specific recession-year effects), and L (state linear trends), the six orderings are as follows: DRL, DLR, RDL, RLD, LDR, LRD. There are four unique incremental contributions of each set of controls, but the contributions associated with orderings where a given set either comes first or last are weighted twice, because they appear in two different orderings.

[37] Although I do not report the results here, we can also decompose the differences between the specifications 1 and 8 for the leading values, and higher income thresholds, falsification tests. For outcomes where there is a non-trivial gap between the two specifications, all three of the control sets contribute towards the difference. This suggests all three sets of controls "matter."

into this issue.

4.3 Effect for subgroups

In Table 7, I use the preferred specification 8 to show minimum wage elasticities for the proportions under alternative income-to-needs cutoffs disaggregated by time periods and demographic groups. First, I find that the poverty rate elasticities were comparable in the 1990-1999 sample (-0.29) as in the 2000-2012 sample (-0.23). The reductions in proportions were substantially larger in the 1990s for lower cutoffs (i.e., severe poverty), but somewhat smaller for higher cutoffs (i.e., near poor). However, as expected, the precision is lower when we disaggregate by periods.

Turning to the five demographic subgroups, for all of them I find sizable reductions in the proportions under 50, 75 and 100 percent of the poverty threshold. The 15 elasticities range between -0.16 and -0.57, and 13 are statistically significant at at least the 10 percent level. The poverty rate elasticities are larger than average for children (-0.31), black and Latino individuals (-0.40), and 21-44 year olds with high school or lesser education (-0.27). They are somewhat smaller for single mothers (-0.16) and 21-44 year olds generally (-0.20). The reductions in low-income shares extend somewhat further up the distribution for black and Latino individuals as well as for children under 18, for whom there are substantial and statistically significant reductions for up to 175 percent of the poverty threshold. The key conclusion from these findings is that when we focus on disadvantaged groups such as black or Latino individuals, or those with lesser education, the anti-poverty impact of minimum wages appears to be somewhat greater; however, for another group (single mothers) the impact is somewhat smaller.

Next, I compare my findings with what the existing research suggests about heterogeneous impact by age, single mother status, education, and race, as summarized in Table 1. First, if we take the poverty rate elasticities for groups under 20 years of age in the literature, my estimate for children (-0.31) is similar to Morgan and Kickham (-0.39) and Addison and Blackburn (average of -0.39 across specifications for teens). Therefore, both existing work and results in this paper point toward a greater poverty reducing impact of minimum wages among children than the population as a whole.

Second, for single mothers, I find elasticities for the proportion under the poverty line of -0.16, and under one-half poverty line of -0.32, which as noted are somewhat somewhat smaller than the population overall. The implied elasticities in Neumark and Wascher (2011) for 21-44 year old single females with kids are +0.08 (poverty) and -0.45 (half-poverty). However, their results suggest stronger anti-poverty effects when they consider single mothers who are either black/Hispanic (-0.20) or have at most a high school diploma (-0.22). Sabia (2008) finds a range of elasticities between -0.28 and -0.17 for single mothers, depending on the mother's education level. Burkhauser and Sabia (2007) find poverty rate elasticities for single mothers between -0.21 and -0.07 depending on specification. DeFina (2008) finds poverty rate elasticities in female headed households with kids of -0.42 (-0.35 when restricting to mothers without a college education). Finally, Gunderson and Ziliak finds very small effects for female headed households (-0.02). If we take an "average of averages" of poverty rate elasticities for single mothers (or female heads of households) across these five studies, we get an average elasticity of -0.18, which is not very different from my estimate of -0.16.

The third comparison concerns heterogeneity in the effect by levels of education. Recall that among 21-44 year olds, I find that the poverty rate elasticity rises somewhat in magnitude from -0.20 to -0.27 when I restrict to those with high school or lesser education. First, I note that my estimated poverty rate elasticity for 21-44 year olds (-0.20) is somewhat smaller than the implied elasticity in Neumark and Wascher (2011) of -0.29. Second, while they do not provide estimates for 21-44 year olds by education, they do so for single females who are 21-44 year old. Within that category, the implied poverty rate elasticity for those with only a high school diploma is -0.19, as opposed to 0.00 for the group overall. A similar pattern obtains for single mothers as well, and these results are qualitatively similar to the findings in this paper. Sabia (2008) also finds larger reductions in the poverty rate for single mothers with less than a high school diploma (-0.28) than those with (-0.17), although neither estimate is statistically significant. In contrast, restricting to those with less education tends to slightly diminish the effects in DeFina, though they continue to be sizable (changing from -0.42 to -0.35). Sabia and Nielsen's estimates are highly imprecise and the impact of conditioning on education levels is contradictory across specifications. Finally, while Addison and Blackburn do not provide comparable estimates by levels of education, averages across their specifications do suggest a somewhat large elasticity (-0.43) for junior-high dropouts. While the estimates in the literature do not paint to a clear picture, on balance they do not suggest that the poverty reducing effect of minimum wages is smaller among those with less education. A contribution of this paper is to show more clearly that the minimum wage effects on poverty are somewhat *larger* among adults without any college education.

The fourth, and final, comparison concerns heterogeneity by race. Here, I find clear evidence of substantially stronger reduction in poverty, and near poverty, among black or Latino individuals as compared to the population as a whole. This is consistent with the implied estimates from Neumark and Wascher (2011), which suggest that that among single females, the poverty rate elasticity rises in magnitude from an average of 0 to -0.19 when they restrict the sample to black or Hispanic individuals. A similar pattern obtains for single mothers in their paper as well. Gunderson and Ziliak also find a slightly larger effect in the black population—though the magnitude is still very small (-0.06). Finally, Sabia and Nielsen's estimates are, again, imprecise and qualitatively differ by specification. Similar to the case of education, this paper provides sharper evidence than available in existing work that the impact of minimum wages on poverty rates tends to be somewhat greater among African Americans and Latinos.

4.4 Alternative measures of poverty

Table 8 shows that the minimum wage elasticities for proportions under 0.50, 0.75, and 1.00 times the poverty threshold are either similar or somewhat larger when using the square root equivalence scale, as compared to the implicit scale used for official poverty calculations. The poverty rate elasticity rises in magnitude to -0.33 from the original -0.24, and is statistically significant at the 1 percent level. The estimates for cutoffs above the poverty line are slightly smaller in magnitude. Overall, the use of the square root scale continues to show a moderate reduction in poverty in response to minimum wage increases.

Table 9 also considers two other outcomes besides the headcount rate, namely the gap and squared gap indices. For the official poverty line, the poverty gap elasticity is somewhat larger in magnitude at -0.32 than the poverty rate elasticity of -0.24. The squared poverty gap is substantially larger in absolute value, with an elasticity of -0.96. Both the gap and squared gap elasticities are statistically significant at the 1 percent level. The findings for the gap and squared gap measures show that minimum wage increases do not reduce poverty by merely pushing some families above the poverty line, but rather by increasing incomes substantially and further below the poverty line. This finding is consistent with sizable reductions in the proportion below 50 and 75 percent of the poverty line, as shown in Table 8 and also in previous tables. Moreover, it is also consistent with findings on family income elasticities by quantile that I present below in section 4.5.

I want to make two additional points about the squared poverty gap elasticities. First, I note that the elasticity close to -1 is in sharp contrast to the near zero effect Gunderson and Ziliak found in their study, which mirrors my findings of larger anti-poverty effects of minimum wages generally than those found in that paper. Since I tend to find substantial effects not just at the poverty line, but also at 75 percent and 50 percent of the poverty line, it is not surprising that the disjuncture between the two studies is particularly large for the squared poverty gap measure, which is more sensitive to changes far below the poverty line.

Second, I also report estimates for the gap and squared gap indices using cutoffs above the poverty line. I find that the gap elasticities continue to be sizable and statistically significant for these higher income cutoffs, though they diminish in magnitude. This is as expected, since the gap index for a cutoff c is more sensitive to increases in incomes substantially below c than is the headcount rate measure for that same cutoff c. For example, if all the increases in incomes for families due to a higher minimum wage occur at or below 125 percent of the poverty line, the proportion under 150 percent of the poverty line will not be affected. However, such income gains will still affect the gap index when using a cutoff of 150 percent of the poverty line. Table 9 shows that the squared gap elasticities actually *increase* in magnitude at higher cutoffs. While this may seem surprising, it is not for a similar reason: the squared gap index is even more sensitive to income gains substantially below the cutoff. A given increase in income for, say, families around 75 percent of the poverty line is much more influential for the squared gap index when the cutoff is 150 percent of the poverty line than when the cutoff is the poverty line itself.

Overall, when considering alternative poverty measures, I continue to find substantial anti-poverty effects from minimum wage increases. The minimum wage effects are somewhat larger when it comes to the depth of the poverty as measured by the poverty gap. And the effects are substantially larger when I consider the severity of poverty as indicated by the squared poverty gap.

4.5 Effect on family income quantiles

As discussed above, we can use the impact of minimum wages on the proportions below alternative income cutoffs to estimate the impact on equivalized family income quantiles. The unconditional quantile partial effects (β_τ) are estimated using equation 3, or analogous regressions for the less saturated specifications. To convert the UPQE's into elasticities (η_τ), they are subsequently divided by the income-to-needs cutoffs corresponding to a given quantile. In Table 9, I present these equivalized family income elasticities for quantiles ranging from 5 through 50, in increments of 5. Recall that the 15th quantile is essentially at the poverty line during the sample period.

Consistent with the evidence on proportions, I find robust evidence that minimum wages lead to moderate increases in incomes for the bottom 20 percent of the equivalized family income distribution. Of the 32 estimates, 30 are positive in sign, and 19 are statistically significant at least at the 10 percent level. The 16 estimates for the 10th and the 15th quantiles range between 0.10 and 0.43, and 13 are statistically significant at least at the 10 percent level. As before, the two-way fixed effects specification 1 provides the smallest estimated magnitudes, and the inclusion of division-specific year effects and state-specific recession controls tend to increase the size of the estimates. These patterns are as expected, since the elasticities for the family income quantiles are simply rescaled semi-elasticities for the proportions below alternative income-to-needs cutoffs.

For the preferred estimate from specification 8, I find elasticities of 0.47, 0.32, and 0.21 for the 5th, 10th, and 15th quantiles of equivalized family incomes, respectively; all are statistically significant at least at the 5 percent level. I note that the minimum wage elasticity for the 10th percentile of family earnings in Card and Krueger (1995) ranges between 0.2 and 0.28. This is only slightly smaller than the family income elasticity for the 10th quantile from my preferred specification (0.32). Moreover, their estimate is well within the range of estimates across the eight specifications considered here, (0.13, 0.43).[38]

The minimum wage elasticities for family income quantiles from specifications 1 and 8 are also plotted in Figure 8, which shows that while the two-way fixed effects specification 1 produces smaller estimates at the bottom, it also implausibly suggests a statistically significant income elasticity of -0.09 at the median, indicating a failure of a falsification test. In contrast, we find substantial and statistically significant effects for the preferred specification 8 up to the 15th quantile, declining to close to zero by the 30th quantile. Corresponding figures showing the elasticities for family income quantiles using intermediate specifications are provided in Appendix Figure A1.

Table 10 and Figure 10 show the long-run elasticities for the income quantiles, based on the dynamic specifications (e.g., equation 5). All of the 32 estimates for the 5th, 10th, 15th, and 20th quantiles are positive, and 16 of them are statistically significant at the 10 percent level. Of these 32 estimates, 21 are larger when lagged effects are included as compared to the corresponding

[38]fHowever, I should note that the outcomes in the two papers are somewhat different. First, Card and Krueger's estimate relates to family *earnings,* while I am considering family *incomes,* a broader category. Second, and more subtly, they are estimating the effect on the 10th percentile of a *state's* family earnings distribution. In contrast, the effects in this paper are the unconditional effect on the 10th quantile of the *national* family income distribution. In other words, there are differences in both the definition of income, and the nature of the distributional statistic.

estimates from specifications without lags reported in Table 10; and the rest are mostly similar.[39] However, the precision is lower when we consider the long-run effects, and 27 of the 32 estimates noted above have larger standard errors. The clearest evidence of income increases come from the 10th and 15th quantiles, where the 16 estimates range between 0.11 and 0.39, and 13 of these are statistically significant at least at the 10 percent level. For the preferred specification, I find that the elasticities of 0.36, 0.33, 0.32 for the 5th, 10th, and 15th quantiles, with the latter two being statistically significant at the 5 percent level. Even the canonical specification (1) shows statistically significant long-run effects for the 10th and 15th quantiles, with elasticities of 0.18 and 0.11, respectively. I also note that while the estimates for the 5th quantile vary substantially across specifications without lagged minimum wages (elasticities ranging between -0.19 and 0.61 in Table 9), the long-run elasticities are more alike (ranging between 0.01 and 0.46, with six of the eight estimates larger than 0.2).

Overall, there is clear evidence that minimum wage increases raise family incomes at the bottom of the distribution, with the clearest effects at the 10th and 15th quantiles. When lagged effects are accounted for, the best performing specification 8 suggests that minimum wage elasticities for both of these quantiles slightly exceed 0.30. Across all models, the minimum wage elasticities for these family income quantiles range between 0.10 and 0.43 depending on the set of controls and the inclusion of lags.

5 Discussion

In a recent report, David Neumark concluded that "[T]he existing research literature provides no solid evidence of beneficial distributional effects of minimum wages for poor or low-income families on the whole. As a result, there is no basis for concluding that minimum wages reduce the proportion of families living in poverty or near poverty" (Neumark 2012). However, a careful look at the existing research does not seem to support this conclusion. The totality of evidence from the 12 published studies for which I could obtain or construct minimum wage elasticities point towards some poverty reduction from minimum wage increases. Only one study I reviewed stands out as suggesting that minimum wages actually increase poverty (Neumark, et al. 2005). However, as noted above, that study uses an unconventional methodology and makes a number of problematic assumptions; and its results seem to be qualitatively inconsistent with the rest of the literature. Indeed, the estimates I construct using Neumark's own research with William Wascher from 2011 suggests that on net, minimum wages reduce the incidence of poverty for 21-44 year old adults, with an implied elasticity of -0.29. Excluding the one problematic study that appears to be an outlier (i.e., Neumark et al. 2005), a simple "average of averages" of 53 minimum wage elasticities across the 11 other studies and various demographic groups produces an estimate of -0.20; 48 of these elasticities are negative in sign. For the six of these 11 studies that actually report an estimate for the overall poverty rate (as opposed to for narrow subgroups), the "average of averages" produces a minimum wage elasticity

[39] As before, the inclusion of lags is somewhat more likely to increase the magnitude of the coefficient when the specification includes state trends.

of -0.15. While averages across studies with different groups and specifications should always be taken with a grain of salt, they nevertheless contradict the claim that the literature does not provide evidence that minimum wages reduce the proportion of families living in poverty.

What *is* true about the existing studies is that they often suffer from serious limitations. These include imprecision owing to short sample periods, as well as inadequate controls for the type of state-level heterogeneity that I show to be quite important in this paper, and that have been shown to important elsewhere with regard to minimum wages (e.g., Allegretto et al. 2013). However, the imperfection of the evidence does not constitute evidence of its absence. In this paper, I address these key imperfections by using a 23-year sample, a battery of controls for time-varying heterogeneity, a wide range of distributional statistics, and an array of falsification tests to assess the reliability of the models. I find robust evidence that minimum wages tend to reduce the incidence of poverty, and also proportions with incomes under one-half or three-quarters of the poverty line. Across all 16 specifications with alternative controls and lag structures, I find poverty rate elasticities ranging between -0.12 and -0.37, and most of these are statistically significant. Some of these specifications include ones that are very similar to ones used by Neumark and Wascher (2011), Burkhauser and Sabia (2007), and Sabia and Burkhauser (2010), except that I use more data.

An additional contribution of this paper is to estimate unconditional quantile partial effects of minimum wages on family incomes using the RIF regression approach of Firpo et al. (2009). I find moderate positive effects on the bottom quantiles of the equivalized family income distribution. The clearest increases are for the 10th and 15th quantiles, with elasticities ranging between 0.10 and 0.43 depending on controls and lags; my preferred specification suggests an elasticity of around 0.3 for the 10th quantile of equivalized family incomes.

I do find that the inclusion of time-varying regional controls and state-specific recession controls suggests larger anti-poverty effects of the policy, consistent with existing evidence on the non-random nature of minimum wage variation (e.g., Allegretto et al. 2013). Most notably, the canonical two-way fixed effects model that is used in most (though not all) of the literature both finds the smallest anti-poverty effects and also fails two types of falsification tests.[40] First, the canonical model suggests that minimum wages reduce the median family income (with an statistically significant elasticity of around -0.09), which is implausible; and this is true even with the inclusion of state per-capita GDP and unemployment rates as covariates. Second, the canonical model suggests that the share under one-half the poverty line rises *prior* to the minimum wage increase, even though the share is subsequently reduced after the increase. This pattern, too, is implausible. In contrast, the most saturated specification passes both of these falsification tests, lending additional support to the importance of controlling for spatial heterogeneity in minimum wage variation. I consider the most saturated specification to be the preferred one based both on its performance in these falsification tests, as well on *a priori* grounds of including a rich set of controls for the kind of heterogeneity that have been found to be important when studying employment effects. However, I recognize that

[40] Overall, the inclusion of state-specific trends (the one form of time-varying effects that has been used in the existing minimum wage-poverty literature) does not appear to affect the estimates as much, especially when lags are included.

reasonable observers may disagree on exactly which specification is ideal, or on the relative weight to place on the evidence associated with each.[41] For this reason, I have shown much of my results using a wide range of specifications, and have reported and discussed the range of estimates across specifications, lags and cutoffs. It is important to note that Dube et al. (2010) and Allegretto et al. (2011, 2013) find that the inclusion of these time-varying controls tend to *reduce* the magnitude of estimated employment effects, while this paper finds that such controls *increase* the magnitude of the estimated effects on family incomes at the lower end of the distribution. These findings are mutually consistent with an explanation that higher minimum wages tend to more prevalent in times and places with worse economic outcomes—an interpretation that is further supported by the results from the falsification tests on the median income. These joint findings, however, are much less consistent with an explanation that the inclusion of spatial controls "throws out" too much identifying variation to be informative, as has been advanced by Neumark, Salas, and Wascher (2013).[42]

How do these moderate sized estimates of minimum wage impact on poverty and bottom income quantiles accord with cross-sectional evidence on the relationship between wages and family incomes? At least since Gramlich (1976), it has been recognized that the link between low wages and low family incomes is imperfect. First, it is true that workers in poverty disproportionately report earning wages at or below the minimum wage. Consider workers earning under \$10.10/hour, which is the proposed federal minimum wage under legislation currently in Congress, authored by Senator Tom Harkin and Congressman George Miller. Based on the March 2013 CPS, 63.2 percent of workers in poor families report hourly earnings of under \$10.10/ hour, as compared to 21.8 percent in the overall population. In other words, we expect minimum wages to affect earnings at the bottom of the family income distribution much more than elsewhere in the distribution, consistent with the results in this paper. At the same time, it is also true that many workers who report earning at or below the minimum wage are not in families below the official poverty line (e.g., Card and Krueger 1995, Sabia and Burkhauser 2010). For example, also from March 2013 CPS, I find that 18.9 percent of workers reporting earnings of under \$10.10/hour are in poverty, and 46.0 percent are under two times the poverty line. However, there are a number of problems in using the cross sectional relationship between reported wages and family incomes to simulate how the gains from a minimum wage increase will be distributed, as is done, for example, in Sabia and Burkhauser (2010). Most obviously, we would need to make assumptions about how behavior changes: this concerns not only employer activities on hiring and firing, but also worker actions including job search behavior, which could be vary by family income and other characteristics. In addition, simulations such as

[41] One limitation of the preferred specification is that, for the most part, it does not use variation in minimum wages during recessionary periods. To the extent there may be heterogeneous impact by the phase of the business cycle, the estimates from the preferred specification are valid primarily for non-recessionary years.

[42] In their conclusion, they state the following. "We think the central question to ask is whether, out of their concern for avoiding minimum wage variation that is potentially confounded with other sources of employment change, [Allegretto et al. 2011] and [Dube et al. 2010] have thrown out so much useful and potentially valid identifying information that their estimates are uninformative or invalid. That is, have they thrown out the 'baby' along with – or worse yet, instead of – the contaminated 'bathwater'? Our analysis suggests they have."

these face a number of challenges which tend to suggest a weaker link between low wages and low family income than is truly the case. A key concern is measurement error in both wages and other sources of incomes (which includes wage and salary incomes of other family members). It is a straightforward point that measurement error in reported wages leads to an attenuation in the measured relationship between workers' wages and family incomes.[43] As a result, simulating wage changes for those earning around the minimum wage will typically suggest smaller effects on poverty and smaller income increases at the bottom quantiles than would occur in reality. This is because (1) some of the individuals with high reported wages in low income families are actually low wage earners, and (2) some of the low wage earners reporting high levels of other sources of income (including spousal wage and salary income) in reality are in poorer families. A related practical issue that arises from this is the treatment of sub-minimum wage workers. For example, in their simulations of raising the minimum wage from \$5.70 to \$7.25, Sabia and Burkhauser (2010) assume that all those with reported hourly earnings below \$5.55 will receive no wage increases because they are in the "uncovered sector." Moreover, they assume that no one above \$7.25 will get a raise. These particular assumptions seem implausible due to both measurement error issues, as well as the well known "lighthouse effect" phenomenon whereby even uncovered sector workers' wages are affected by minimum wages (Card and Krueger 1995; Boeri, Garibaldi, and Ribeiro 2011). Moreover, as Autor, Manning and Smith (2010) show, effects of the minimum wage extend up to the 20th percentile of the wage distribution, which would be unlikely absent some spillovers.[44] Therefore, results from simulation studies—such as those conducted by Sabia and Burkhauser (2010)—may not provide reliable guidance in assessing the impact of minimum wages on bottom incomes, making it critical for us to consider actual evidence from past minimum wage changes when analyzing policy proposals.

What does the evidence from this paper suggest about the likely impact on poverty from an immediate increase in the federal minimum wage from the current \$7.25/hour to \$10.10/hour, similar to the change proposed in the legislation by Senator Harkin and Congressman Miller? For my preferred specification, the estimated minimum wage elasticity for the poverty rate is -0.24, while

[43] Consider the relationship between own wage income, W, and family income $F = W + I$, where I represents other incomes (possibly others' wages). The linear approximation to the true relationship is represented by the population regression $F = \beta W + u$. Note that $\beta = \frac{Cov(W+I,W)}{V(W)} = 1 + \frac{\sigma_{WI}}{\sigma_W^2}$. So if wages are at all positively correlated with other sources of family incomes, I, as is likely, then $\beta > 1$.

Now consider the case where W is measured with error, so that $\tilde{W} = W + e$, and $\tilde{F} = W + I + e$ are the observed wage and family income. This is slightly different from the textbook classical measurement error case because the measurement error, e, affects both the independent and dependent variables. Substituting the reported values into the true regression equation produces $\tilde{F} - e = \beta(\tilde{W} - e)$. Rearranging, we have $\tilde{F} = \beta\tilde{W} + (1-\beta)\, e = \beta\tilde{W} + \tilde{u}$.

Note that $\tilde{\beta} = \frac{Cov(\tilde{F},\tilde{W})}{V(\tilde{W})}$ is the estimate from a population regression of $\tilde{F}$ on $\tilde{W}$. Substituting $\tilde{F} = \beta\tilde{W} + (1-\beta)\, e$ into the expression for $\tilde{\beta}$ we have $\tilde{\beta} = \beta + (1-\beta)\frac{\sigma_e^2}{\sigma_w^2+\sigma_e^2}$, which will be attenuated towards zero if $\beta > 1$, which is true if wages are at all positively correlated with other sources of family incomes.

[44] Autor, Manning and Smith also highlight how measurement error in wages and wage spillovers have similar implications about the effects of minimum on the observed wage distribution. This is an interesting point which affects the interpretation of the effects on higher wage quantiles. But for our purposes here, regardless of the interpretation of these effects as true spillovers or measurement error spillovers, ignoring them will tend to downward bias the predicted effects of minimum wages on poverty in simulation studies.

the elasticity accounting for lagged effects is -0.36. Starting from the current 17.5 percent poverty rate among the non-elderly population, the estimates suggest a 1.7 percentage point reduction in the poverty rate from a 39 percent increase in the minimum wage as proposed in the legislation. When we take lagged effects into account, the estimates suggest a somewhat larger reduction of 2.5 percentage points. Given the roughly 275 million non-elderly Americans in 2013, the proposed minimum wage increase is projected to reduce the number of non-elderly living in poverty by around 4.6 million, or by 6.8 million when longer term effects are accounted for. We can also expect the same minimum wage increase to raise family incomes by 12 percent at the 10th quantile of the equivalized family income distribution. For the average family near the 10th percentile in 2013, this translates into an annual increase of $1,700.[45] Therefore, the increase in the federal minimum wage currently under consideration can play a modest but important role in reducing poverty and raising family incomes at the bottom. To put this in context, the poverty rate among the non elderly rose by as much as 3.4 percentage points during the Great Recession; so the proposed minimum wage change can reverse at least half of that increase.

To be clear, if we were to assess public policies strictly based on their efficacy in reducing poverty, we should prefer more targeted policies like cash transfers, food stamps, and programs that raise the employment rate for highly disadvantaged groups. As many researchers, including Card and Krueger (1995), have pointed out, the minimum wage is a blunt tool when it comes to fighting poverty. In comparison, the EITC is better targeted at those with very low incomes. It is important to point out, however, that as currently structured, the EITC provides only minimal assistance to adults without children, and may hurt some of them through a negative incidence on wages (Rothstein 2011). More generally, in the presence of such incidence effects due to increased labor supply, the optimal policy calls for combining tax and transfers like the EITC with a minimum wage (Lee and Saez 2012).

However, motivations behind minimum wage policies go beyond reducing poverty. The popular support for minimum wages is in part fueled by a desire to raise earnings of low and moderate income families more broadly, and by concerns of fairness that seek to limit the extent of wage inequality (Green and Harrison 2010), or employers' exercise of market power (Fehr and Fischbacher 2004; Kahneman, Knetsch and Thaler 1986). The findings from this paper suggest that attaining such goals through increasing minimum wages is also consistent with a modest reduction in poverty, and moderate increases in family incomes at the bottom quantiles. Ultimately, this conclusion does not differ markedly from that reached by Card and Krueger (1995), or by Gramlich (1976) before them.

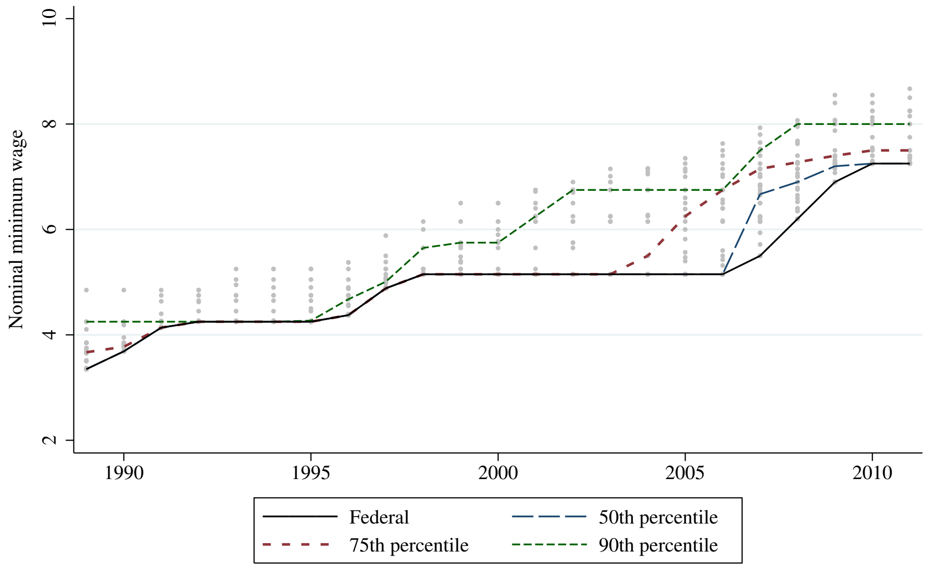
There are a number of outstanding issues that I did not address in this paper. The first set of issues concerns the definition of family income used in this analysis. Following official poverty

[45] If we take the range of estimates from all specifications and lag structures, the proposed minimum wage changes can be expected to reduce the poverty rate among the non-elderly population by 0.8 and 2.9 percentage points, hence reducing the number of non-elderly individuals living in poverty by somewhere between 2.3 and 8.1 million. For the 10th quantile of family incomes, this translates to an annual income increase ranging between 5 and 17 percent, or between $700 and $2,400.

calculations, my family income definition includes both pre-tax earnings and cash transfers, and I have not decomposed the increase in income following minimum wage increases into component parts. At the same time, the estimates here do not capture the impact of minimum wages on non-cash transfers such as food stamps or housing, or on the receipt of tax credits such as EITC. Second, and relatedly, while my estimates control for state EITC supplements, I have not directly evaluated the interaction of EITC (or other policies) and minimum wages in this paper. As such, the minimum wage estimates I provide are the average effects over the sample period. At least for the poverty rate estimates, however, the effects appear to be qualitatively similar during the 1990s—a period with with less generous EITC—as compared to the 2000s with more generous EITC. And while existing work by Neumark and Wascher (2011) points to interactive effects of the two policies for some groups, this work does not directly show how the interactions affect the distribution of post-tax income that includes the tax credits themselves—which are of first order importance. Better understanding the source of income gains from minimum wage increases, as well as understanding the interactions of various policies in shaping the post-tax-and-transfer family income distribution, seem fruitful directions for future research.

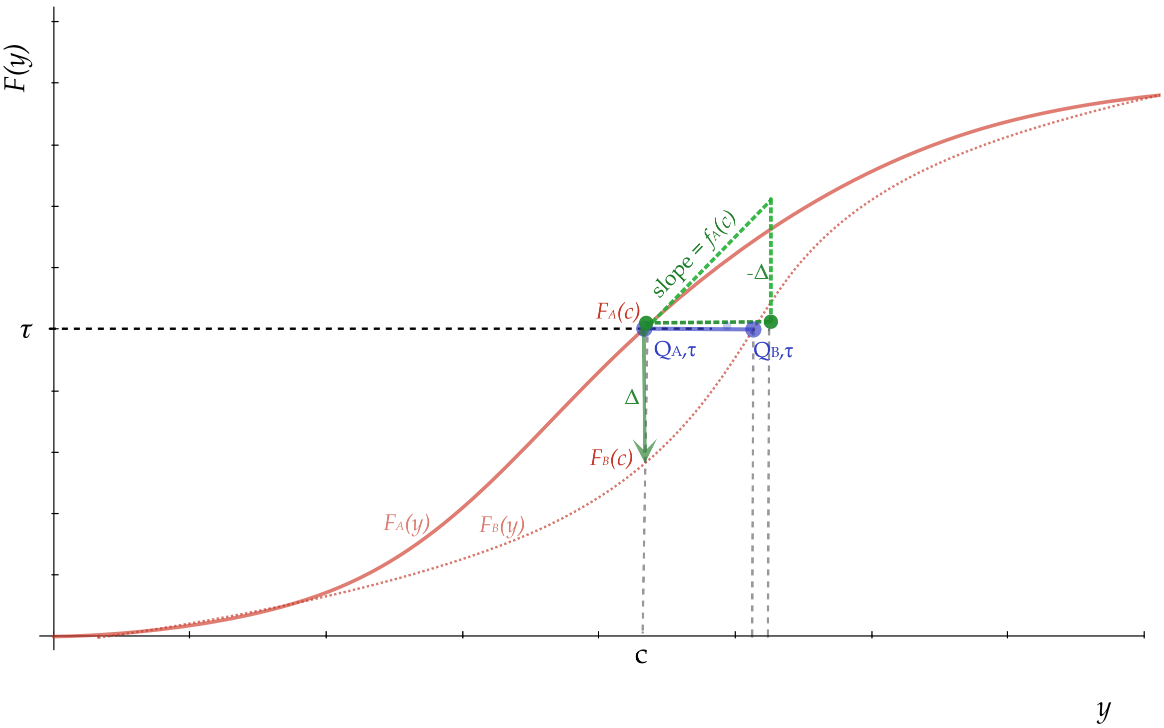
Figures and Tables

Figure 1: Minimum wage variation over time



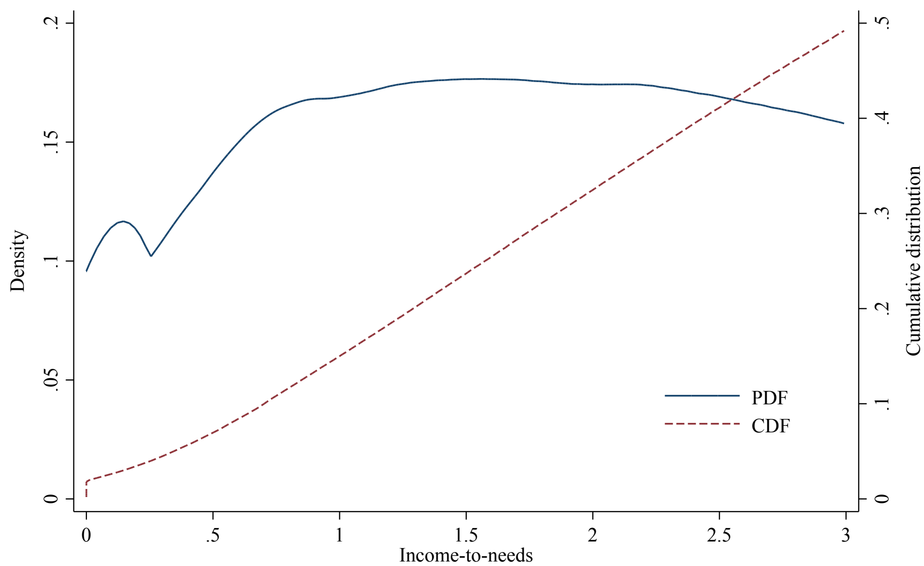
Notes: Annualized state-level minimum wages are constructed by averaging the effective nominal minimum wage (higher of the state or federal minimums) during the twelve months in a given year. Annualized minimum wage data from year t is matched with the CPS survey from March of year $t+1$. The years in the horizontal axis represents year t, and not the CPS survey year $t+1$. Minimum wage percentiles are weighted by the non-elderly population in the state using 1990-2012 March CPS surveys and person weights. The grey dots in the scatter plot represent annualized effective minimum wages in each state.

Figure 2: Unconditional quantile partial effects: locally inverting the counterfactual distribution



Notes. The figure shows how the unconditional quantile partial effect (UQPE) is approximately estimated for a treatment such as a small increase in the minimum wage. $F_A(y)$ represents the actual distribution of outcome y, while $F_B(y)$ is the counterfactual distribution absent the treatment. Under the assumption of conditional independence, the counterfactual distribution can be estimated using distribution regressions of the impact of the policy on the share below cutoffs c for all cutoffs. The UQPE for the τ^{th} quantile is $Q_{B,\tau} - Q_{A,\tau}$, represented as the solid (blue) segment. The recentered influence function (RIF) regression approximates the UQPE by inverting the counterfactual CDF $F_B(y)$ using a local linear approximation. After defining a cutoff c such that $F_A(c) = \tau$ using the actual distribution $F_A(y)$, it uses the impact on the proportion below c, i.e., $F_B(c) - F_A(c)$, and the slope of the CDF, $f_A(c)$, to estimate UQPE $\approx -\frac{F_B(c)-F_A(c)}{f_A(c)}$. The dashed (green) triangle shows the geometry of the RIF approximation to the UQPE, with is represented by the length of the triangle's base.

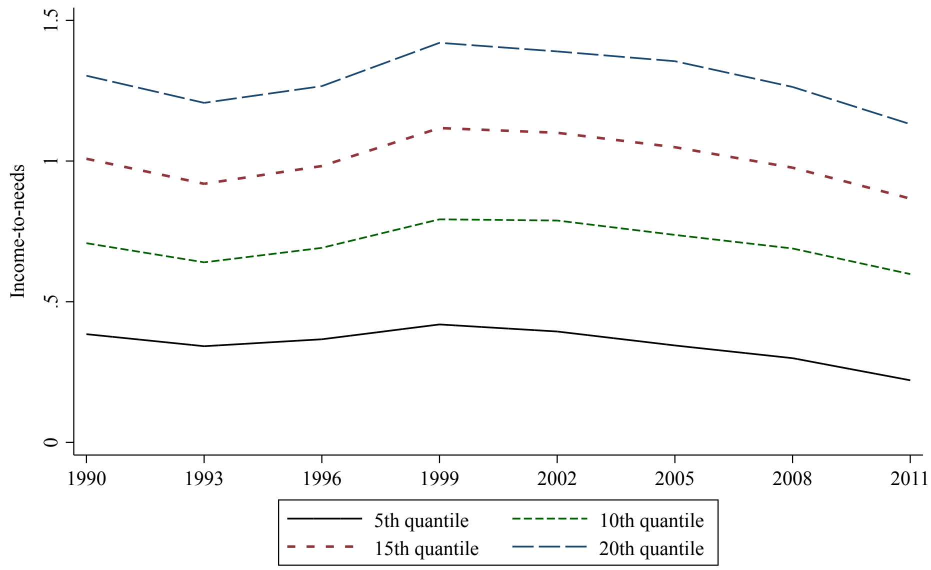
Figure 3: Probability density and cumulative distribution of income-to-needs: averages over 1990-2012 March CPS samples



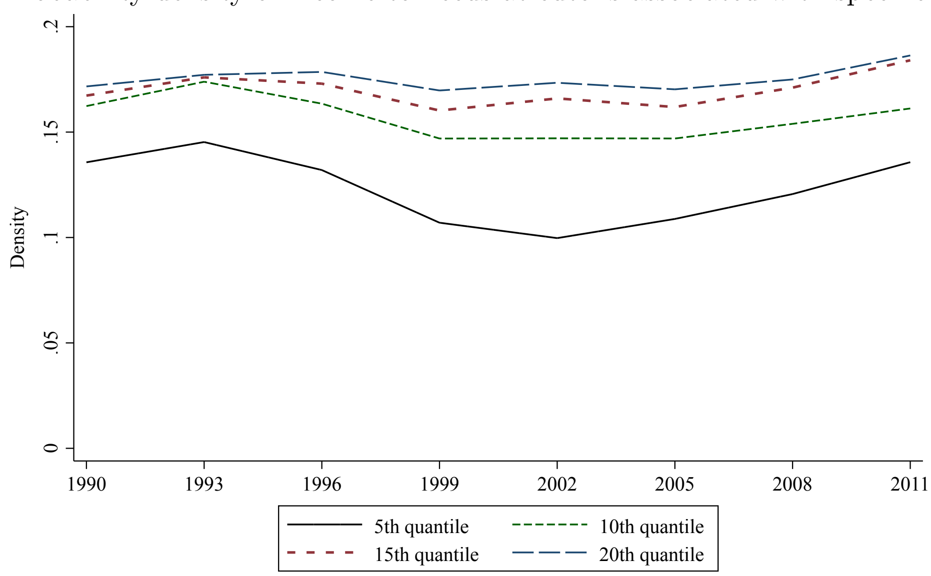
Notes: Both the probability density and cumulative distribution function are estimated using March CPS person weights for survey years 1990-2012 for the non-elderly population. The probability density is estimated using an Epanechnikov kernel and the STATA default bandwidth based on Silverman's rule-of-thumb.

Figure 4: Income-to-needs quantiles, and probability density at associated cutoffs over time

Panel A: Income-to-needs quantiles

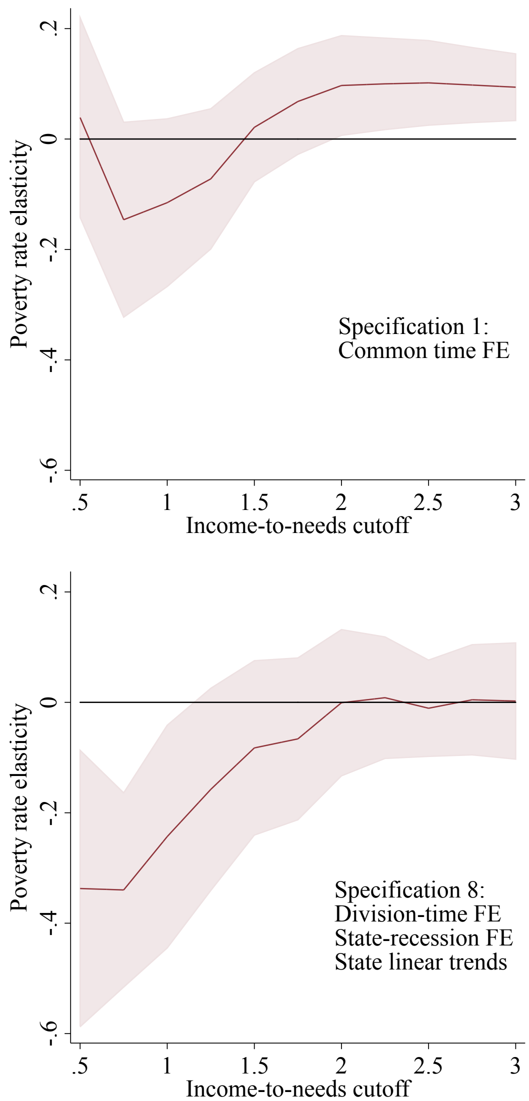


Panel B: Probability density of income-to-needs at cutoffs associated with specific quantiles



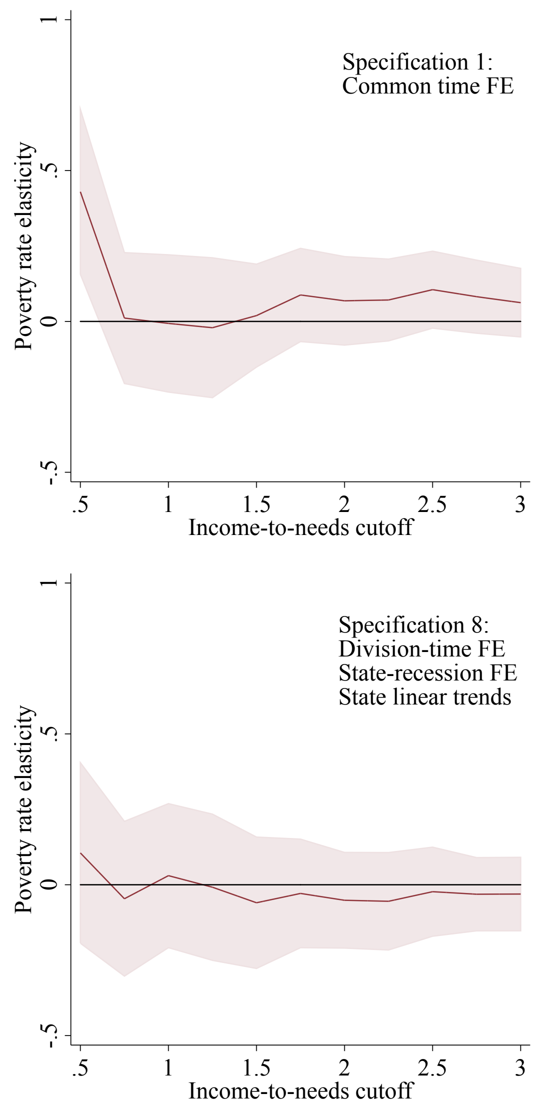
Notes: Panel A plots the values of the 5th, 10th, 15th and 20th quantiles of income-to-needs over time. Panel B plots the probability density of income-to-needs at specific cutoffs associated with each of these quantiles over time. Both panels are calculated for non-overlapping three-year intervals using March CPS person weights, where the horizontal axis indicates the beginning year of the interval. The final interval consists only of two years (2011, 2012). The probability density is estimated using an Epanechnikov kernel and the STATA default bandwidth based on Silverman's rule-of-thumb.

Figure 5: Minimum wage elasticities for proportions under alternative income-to-needs cutoffs



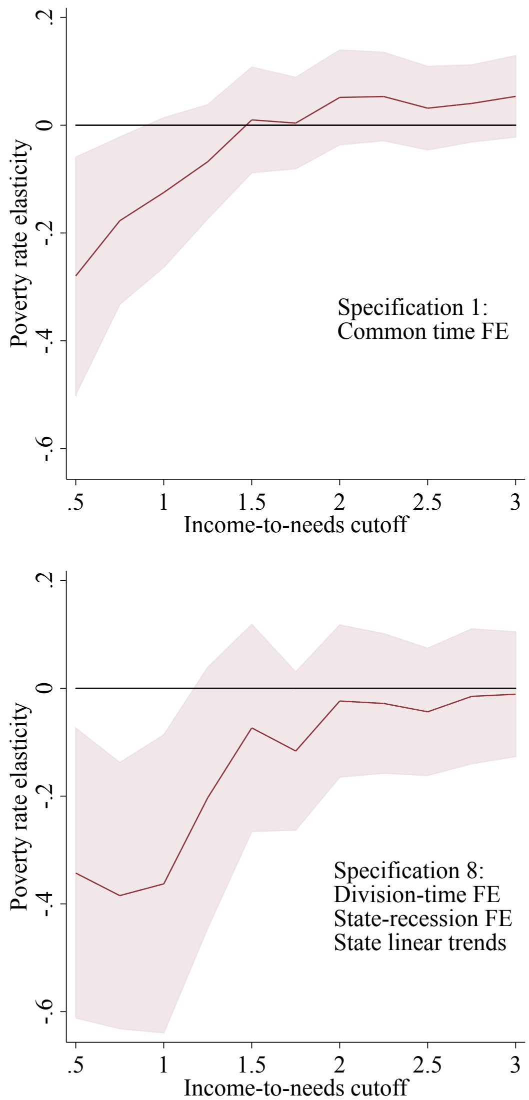
Notes. A series of linear probability models are estimated by regressing an indicator for being under alternative income-to-needs cutoffs (between 0.50 and 3.00) on log minimum wage and covariates. Elasticities are calculated by dividing the coefficient on log minimum wage by the sample proportion under the income-to-needs cutoff. All specifications include state and year fixed effects, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. Additional controls are indicated in the figure. Shaded area represents 95 percent state-cluster-robust confidence intervals.

Figure 6: One-year leading minimum wage elasticities for proportions under alternative income-to-needs cutoffs



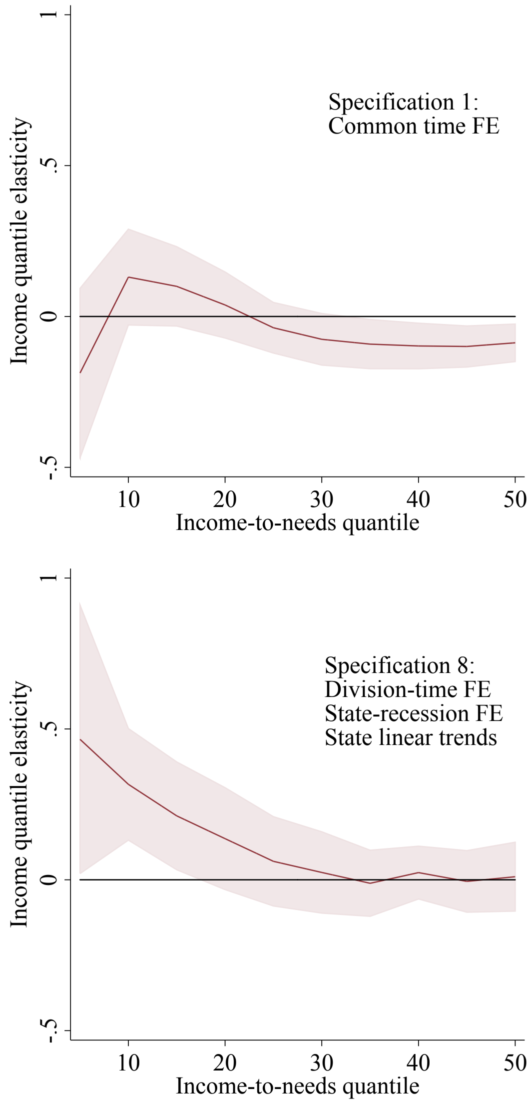
Notes. A series of linear probability models are estimated by regressing an indicator for being under alternative income-to-needs cutoffs (between 0.50 and 3.00) on distributed lags of log minimum wage and covariates. The leading elasticity is the one-year leading minimum wage coefficient divided by the sample proportion under the cutoff. All specifications include state and year fixed effects, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. Additional controls are indicated in the figure. Shaded area represents 95% state-cluster-robust confidence intervals.

Figure 7: Long-run minimum wage elasticities for proportions under alternative income-to-needs cutoffs



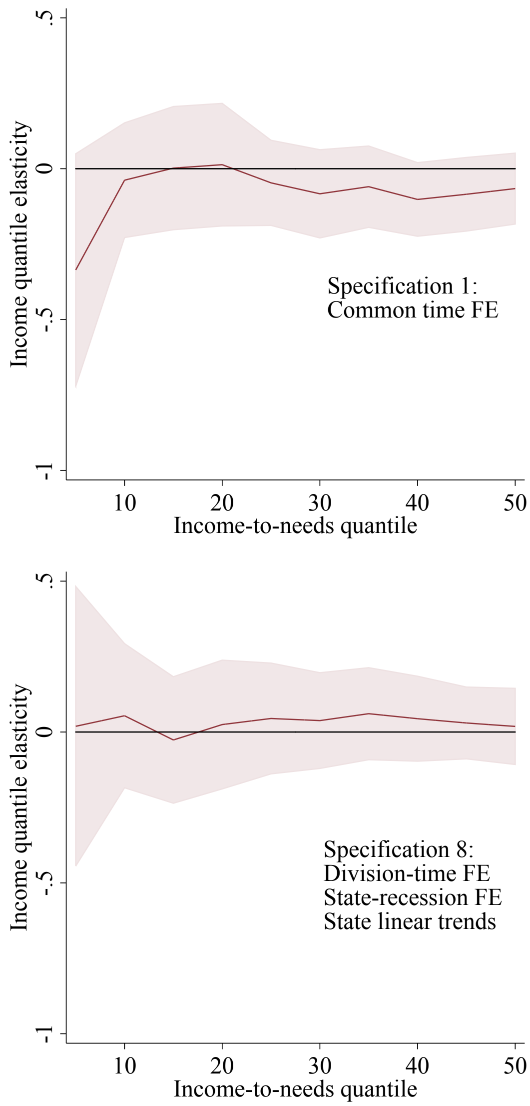
Notes. A series of linear probability models are estimated by regressing an indicator for being under alternative income-to-needs cutoffs (between 0.50 and 3.00) on distributed lags of log minimum wage and covariates. The long-run elasticity is calculated from the sum of the contemporaneous and one-year lagged log minimum wage coefficients, divided by the sample proportion under the cutoff. All specifications include state and year fixed effects, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. Additional controls are indicated in the figure. Shaded area represents 95% state-cluster-robust confidence intervals.

Figure 8: Minimum wage elasticities for unconditional family income quantiles



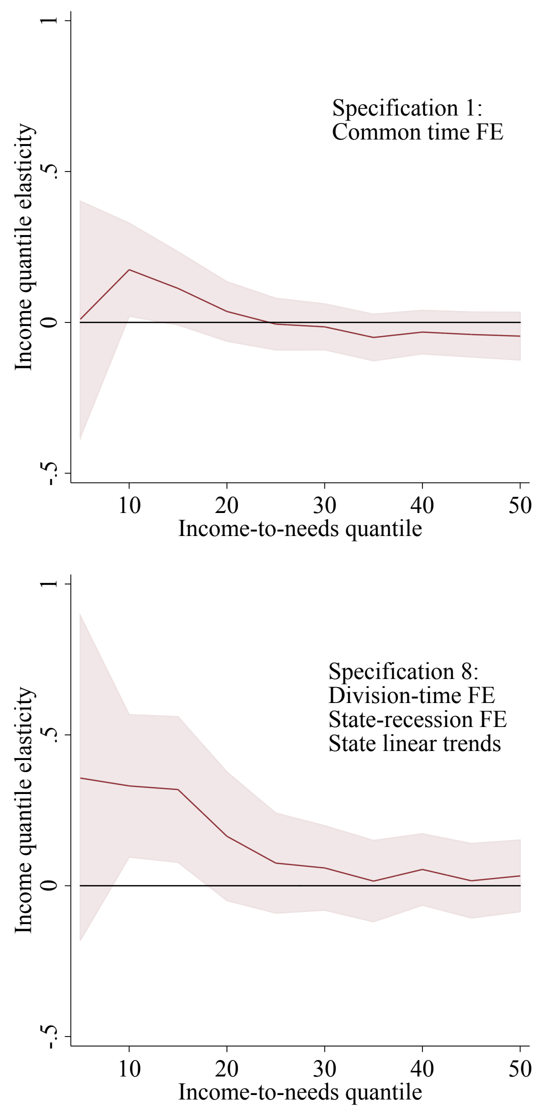
Notes. A series of linear probability models are estimated by regressing an indicator for being under income-to-needs cutoffs associated with alternative quantiles (between 5 and 50) on log minimum wage and covariates. Unconditional quantile partial effects (UQPE) for equivalized family income are calculated by dividing the coefficient on log minimum wage by the negative of the income-to-needs density at the appropriate quantile. The UQPE estimates are subsequently divided by the income-to-needs cutoff for the quantile to transform the estimates into elasticities. All specifications include state and year fixed effects, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. Additional controls are indicated in the figure. Shaded area represents 95% state-cluster-robust confidence intervals.

Figure 9: One year leading minimum wage elasticities for unconditional family income quantiles



Notes. A series of linear probability models are estimated by regressing an indicator for being under income-to-needs cutoffs associated with alternative quantiles (between 5 and 50) on distributed lags of log minimum wage and covariates. Unconditional quantile partial effects (UQPE) for the leading effect is calculated by dividing the coefficient on one-year leading log minimum wage by the negative of the income-to-needs density at the appropriate quantile. The leading UQPE estimates are subsequently divided by the income-to-needs cutoff for the quantile to transform the estimates into elasticities. All specifications include state and year fixed effects, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. Additional controls are indicated in the figure. Shaded area represents 95% state-cluster-robust confidence intervals.

Figure 10: Long-run minimum wage elasticities for unconditional family income quantiles



Notes. A series of linear probability models are estimated by regressing an indicator for being under income-to-needs cutoffs associated with alternative quantiles (between 5 and 50) on distributed lags of log minimum wage and covariates. Unconditional quantile partial effects (UQPE) for the long-run effect is calculated by dividing the the sum of the contemporaneous and one-year lagged log minimum wage coefficients by the negative of the income-to-needs density at the appropriate quantile. The long-run UQPE estimates are subsequently divided by the income-to-needs cutoff for the quantile to transform the estimates into elasticities. All specifications include state and year fixed effects, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. Additional controls are indicated in the figure. Shaded area represents 95% state-cluster-robust confidence intervals.

Table 1: Poverty related minimum wage elasticities in the existing literature

Study & Sample	Poverty Rate Elasticity	Other Elasticity	Data	Controls
Addison & Blackburn (1999)				
Ages 16 - 19	-0.50 (0.22)		1983-1996 March CPS; S-Y	S,Y FE; LM; GLS
Ages 16 - 19	-0.61 (0.28)			S, Y FE; LM; PA Pov; GLS
Ages 16 - 19	-0.39 (0.22)			S,Y FE; LM; PA Pov
Ages 16 - 19	-0.17 (0.28)			S,Y FE; S-tr; LM; PA Pov; GLS
Ages 20 - 24	-0.33 (0.22)			S,Y FE; LM; GLS
Ages 20 - 24	-0.11 (0.28)			S, Y FE; LM; PA Pov; GLS
Ages 20 - 24	-0.22 (0.22)			S,Y FE; LM; PA Pov
Ages 20 - 24	-0.28 (0.22)			S,Y FE; S-tr; LM; PA Pov; GLS
Age > 24, Ed < 10 yrs	-0.50 (0.15)			S,Y FE; LM; GLS
Age > 24, Ed < 10 yrs	-0.31 (0.19)			S, Y FE; LM; PA Pov; GLS
Age > 24, Ed < 10 yrs	-0.46 (0.15)			S,Y FE; LM; PA Pov
Age > 24, Ed < 10 yrs	-0.46 (0.15)			S,Y FE; S-tr; LM; PA Pov; GLS
Burkhauser & Sabia (2007)				
Ages 16-64	-0.19 (0.12)		1988-2003 March CPS; S-Y	S,Y FE
Ages 16-64	-0.15 (0.11)			S,Y FE; LM
Ages 16-64	-0.11 (0.10)			S,Y FE; LM
Ages 16-64	-0.08 (0.12)			S,Y FE; LM; GLS
SF HH w/ kids	-0.21 (0.13)			S,Y FE
SF HH w/ kids	-0.16 (0.13)			S,Y FE; LM
SF HH w/ kids	-0.16 (0.13)			S,Y FE; LM
SF HH w/ kids	-0.07 (0.13)			S,Y FE; LM; GLS
*Card & Krueger (1995)**				
Age $\geq$ 16	-0.39 (0.21)	Fam earn p10: 0.28 (0.05)	1989-1991 March CPS; S-Y	FD
Age $\geq$ 16	-0.16 (0.26)	Fam earn p10: 0.20 (0.06)		FD; LM
Age $\geq$ 16	-0.36 (0.31)			FD; LM
Age $\geq$ 16	-0.08 (0.29)			FD; LM; Region
DeFina (2008)				
Fem HH w/ kids	-0.42 (0.15)	Post-tax: -0.46 (0.15)	1991-2002 March CPS; S-Y	S,Y FE
Fem HH w/ kids < Col.	-0.35 (0.16)	Post-tax: -0.40 (0.16)		S,Y FE

Notes. All estimates expressed as elasticities, with standard errors in parentheses when available. For papers marked by a *, implied elasticities and/or their standard errors were calculated using reported coefficient estimates and sample means, rather than the elasticities being reported directly. Sample abbreviations: Ed = education; HS = high school degree; Col.=college; SF = single female; Fem = female; HH = head of household; Bl = Black; Hisp = Hispanic. Other elasticity categories: Fam inc p10 = family earnings elasticity at the 10th percentile; Post-tax = poverty rate elasticity using post-tax and transfer income; Pov Gap^2 = squared poverty gap elasticity; N% Pov = poverty rate elasticity using N% of poverty as the threshold. Data abbreviations (for unit of observation): S = state; S-Y = state-year; NY MA = N-year moving average; Ind. = individual. Controls abbreviations: S = State; Y = Year; FE = fixed effects; FD = first difference; LDV = lagged dependent variable; S-tr, S-tr^2 = linear, quadratic state-specific trends; Scale Sh. = Scale shifts in the income-to-needs distribution; LM = labor market controls; PA Pov = Prime age poverty rate control; WQ = wage quantiles; Emp = employment.

Table 1: Poverty related minimum wage elasticities in the existing literature (continued)

Study & Sample	Poverty Rate Elasticity	Other Elasticity	Data	Controls
Gunderson & Ziliak (2004)				
All	-0.03 (0.01)	Pov gap^2: -0.04 (0.01)	1980-1999 March CPS; 3Y MA S-Y	S,Y FE; WQ; LM; S-tr; LDV
Fem HH	-0.02 (0.01)	Pov gap^2: -0.03 (0.01)		S,Y FE; WQ; LM; S-tr; LDV
Married H	-0.03 (0.03)	Pov gap^2: -0.01 (0.03)		
Black	-0.06 (0.03)	Pov gap^2: -0.01 (0.02)		S,Y FE; WQ; LM; S-tr; LDV
White	-0.04 (0.10)	Pov gap^2: -0.05 (0.02)		
*Morgan & Kickham (2001)**				
Kids	-0.39 (0.08)		1987-1996 March CPS; 5Y MA	S FE; LM
*Neumark, Schweitzer, & Wascher (2005)**				
All	0.39 (0.22)	150% Pov: 0.41 (0.13)	1987-1996 March CPS; Ind	S,Y Scale Sh.
*Neumark & Wascher (2011)**				
Ages 21-44	-0.29	50% Pov: -0.42	1997-2006 March CPS; Ind.	S,Y FE; LM
SF, 21-44	0.00	50% Pov: -0.51		S,Y FE; LM
SF, 21-44 $\leq$ HS	-0.19	50% Pov: -0.45		S,Y FE; LM
SF, 21-44, Bl/Hisp	-0.19	50% Pov: -0.11		S,Y FE; LM
Ages 21-44 w/kids	-0.21	50% Pov: -0.27		S,Y FE; LM
SF, 21-44 w/kids	0.08	50% Pov: -0.45		S,Y FE; LM
SF, 21-44 $\leq$ HS w/kids	-0.22	50% Pov: -0.40		S,Y FE; LM
SF, 21-44, Bl/Hisp w/kids	-0.20	50% Pov: -0.31		S,Y FE; LM
Sabia (2008)				
SF HH, 18-55, w/kids	-0.22 (0.17)		1991-2004 March CPS; Ind.	S,Y FE; S-tr^2; LM
SF HH, 18-55, $<$ HS w/kids	-0.28 (0.39)			S,Y FE; S-tr^2; LM
SF HH, 18-55, $\geq$ HS w/kids	-0.17 (0.23)			S,Y FE; S-tr^2; LM

Notes. All estimates expressed as elasticities, with standard errors in parentheses when available. For papers marked by a *, implied elasticities and/or their standard errors were calculated using reported coefficient estimates and sample means, rather than the elasticities being reported directly. Sample abbreviations: Ed = education; HS = high school degree; Col.=college; SF = single female; Fem = female; HH = head of household; Bl = Black; Hisp = Hispanic. Other elasticity categories: Fam inc p10 = family earning elasticity at the 10th percentile; Post-tax = poverty rate elasticity using post-tax and transfer income; Pov Gap^2 = squared poverty gap elasticity; N% Pov = poverty rate elasticity using N% of poverty as the threshold. Data abbreviations (for unit of observation): S = state; S-Y = state-year; NY MA = N-year moving average; Ind. = individual. Controls abbreviations: S = State; Y = Year; FE = fixed effects; FD = first difference; LDV = lagged dependent variable; S-tr, S-tr^2 = linear, quadratic state-specific trends; Scale Sh. = Scale shifts in the income-to-needs distribution; LM = labor market controls; PA Pov = Prime age poverty rate control; WQ = wage quantiles; Emp = employment.

Table 1: Poverty related minimum wage elasticities in the existing literature (continued)

Study & Sample	Poverty Rate Elasticity	Other Elasticity	Data	Controls
Sabia & Burkhauser (2010)				
Ages 16-64	-0.05 (0.15)	150% Pov: 0.18 (0.14)	2003-2007 March CPS; S-Y	S,Y FE; LM
*Sabia & Nielsen (2013)**				
Ages 16-64	-0.31 (0.31)	150% Pov: -0.20 (0.17)	1996-2007 SIPP; Ind	S,Y FE
Ages 16-64	-0.03 (0.12)	150% Pov: 0.03 (0.07)		S,Y FE; S-tr
Ages 16-29,$<$ HS	-0.52 (0.63)	150% Pov: -0.57 (0.35)		S,Y FE
Ages 16-29, $<$ HS	1.21 (0.63)	150% Pov: 0.86 (0.25)		S,Y FE; S-tr
Ages 16-24, Bl	0.60 (0.72)	150% Pov: 0.07 (0.30)		S,Y FE
Ages 16-24, Bl	-0.46 (0.63)	150% Pov: -0.23 (0.30)		S,Y FE; S-tr
Ages 30-54, $\geq$ HS	-0.18 (0.18)	150% Pov: -0.23 (0.18)		S,Y FE
Ages 30-54, $\geq$ HS	0.18 (0.18)	150% Pov: -0.23 (0.30)		S,Y FE; S-tr
*Stevans & Sessions (2001)**				
All	-0.28 (0.17)		1984-1998 March CPS; S-Y	S,Y FE
Average of averages: Every group				
All 12 studies:	-0.15			
11 studies excluding Neumark et al. (2005):	-0.20			
Average of averages: Overall population				
All 7 studies:	-0.07			
6 studies excluding Neumark et al. (2005):	-0.15			

Notes. All estimates expressed as elasticities, with standard errors in parentheses when available. For papers marked by a *, implied elasticities and/or their standard errors were calculated using reported coefficient estimates and sample means, rather than the elasticities being reported directly. Sample abbreviations: Ed = education; HS = high school degree; Col.=college; SF = single female; Fem = female; HH = head of household; Bl = Black; Hisp = Hispanic. Other elasticity categories: Fam inc p10 = family earnings elasticity at the 10th percentile; Post-tax = poverty rate elasticity using post-tax and transfer income; Pov Gap^2 = squared poverty gap elasticity; N% Pov = poverty rate elasticity using N% of poverty as the threshold. Data abbreviations (for unit of observation): S = state; S-Y = state-year; NY MA = N-year moving average; Ind. = individual. Controls abbreviations: S = State; Y = Year; FE = fixed effects; FD = first difference; LDV = lagged dependent variable; S-tr, S-tr^2 = linear, quadratic state-specific trends; Scale Sh. = Scale shifts in the income-to-needs distribution; LM = labor market controls; PA Pov = Prime age poverty rate control; WQ = wage quantiles; Emp = employment. Average of Averages = equally weighted average acrross studies of the within-study average elasticity, either for every demographic group, or just for the overall population (defined as 16-64 year olds or broader).

Table 2: Income-to-needs quantiles and densities

Quantile	Income-to-needs cutoff	Density
5	0.345	0.115
10	0.702	0.160
15	1.000	0.169
20	1.290	0.175
25	1.574	0.177
30	1.857	0.175
35	2.144	0.174
40	2.433	0.170
45	2.732	0.164
50	3.042	0.157

Notes. Income-to-needs quantiles, and kernel densities at cutoffs associated with the quantiles, are estimated for the nonelderly population using March CPS data from 1990-2012 and person weights. Kernel density estimates use an Epanechnikov kernel and the STATA default bandwidth based on Silverman's rule-of-thumb.

Table 3: Distributional statistics by sample group and income-to-needs cutoffs

	Prop. below: Sq root scl	Gap Index	Squared gap Index	Proportions below: Standard equiv scale							
Income-to -needs cutoff	Overall	Overall	Overall	Overall	1990-99	2000-12	Children	Single mothers	Black & Latino	Ages 21-44	Ages 21-44, HS
0.50	0.071	0.041	0.033	0.070	0.070	0.069	0.104	0.207	0.135	0.062	0.095
0.75	0.110	0.057	0.042	0.108	0.111	0.106	0.160	0.302	0.209	0.097	0.150
1.00	0.153	0.075	0.053	0.150	0.154	0.148	0.216	0.383	0.284	0.135	0.209
1.25	0.198	0.094	0.064	0.193	0.197	0.190	0.271	0.458	0.355	0.176	0.270
1.50	0.243	0.114	0.077	0.237	0.241	0.235	0.325	0.524	0.424	0.220	0.334
1.75	0.289	0.135	0.089	0.281	0.286	0.278	0.378	0.585	0.487	0.265	0.395
2.00	0.334	0.156	0.103	0.325	0.331	0.321	0.428	0.636	0.543	0.311	0.455
2.25	0.379	0.177	0.116	0.368	0.376	0.364	0.476	0.682	0.594	0.357	0.512
2.50	0.423	0.199	0.130	0.411	0.421	0.405	0.523	0.725	0.639	0.402	0.565
2.75	0.467	0.220	0.144	0.453	0.465	0.445	0.567	0.762	0.680	0.447	0.615
3.00	0.509	0.241	0.157	0.493	0.508	0.484	0.608	0.794	0.716	0.490	0.660
Observations	3,646,565	3,646,565	3,646,565	3,646,565	1,271,657	2,374,908	1,173,395	189,454	979,360	1,408,250	622,515

Notes. Each cell contains the proportion of the sample (or the mean of a poverty measure) under the income-to-needs cutoff, using the indicated poverty measure. The sample consists of all non-elderly individuals in March CPS surveys for years between 1990 and 2012. Columns 1, and columns 4 through 9 report the proportions below alternative income-to-needs cutoffs. Column 1 uses the square root equivalence scale, while all others use the implicit equivalence scale used to calculate the official poverty rate. Column 2 reports the gap index, and column 3 the squared gap index. Columns 7 through 11 report the proportions for the following subsamples: children under 18; single mothers; black or Latino individuals; 21-44 year old adults; and 21-44 year old adults with high school or lesser education. All calculations use the March CPS person weights.

Table 4: Minimum wage elasticities for proportions under alternative income-to-needs cutoffs

Income-to-needs cutoff	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0.50	0.039	-0.131	0.002	-0.138	-0.374***	-0.248**	-0.430***	-0.337***
	(0.090)	(0.091)	(0.076)	(0.091)	(0.127)	(0.120)	(0.093)	(0.125)
0.75	-0.146	-0.151	-0.217**	-0.202*	-0.332***	-0.215**	-0.450***	-0.340***
	(0.088)	(0.109)	(0.082)	(0.105)	(0.089)	(0.097)	(0.076)	(0.088)
1.00	-0.115	-0.127*	-0.165**	-0.146**	-0.212**	-0.166*	-0.299***	-0.243**
	(0.076)	(0.075)	(0.064)	(0.070)	(0.083)	(0.098)	(0.079)	(0.100)
1.25	-0.072	-0.085	-0.123**	-0.106**	-0.131*	-0.123	-0.188**	-0.158*
	(0.063)	(0.053)	(0.056)	(0.051)	(0.074)	(0.084)	(0.079)	(0.091)
1.50	0.021	-0.030	-0.009	-0.025	-0.063	-0.078	-0.083	-0.083
	(0.049)	(0.045)	(0.042)	(0.043)	(0.062)	(0.070)	(0.066)	(0.079)
1.75	0.068	-0.016	0.021	-0.035	0.000	-0.037	-0.039	-0.066
	(0.048)	(0.040)	(0.041)	(0.040)	(0.056)	(0.064)	(0.059)	(0.073)
2.00	0.097**	-0.001	0.049	-0.022	0.051	0.009	0.020	-0.001
	(0.045)	(0.034)	(0.041)	(0.036)	(0.055)	(0.058)	(0.059)	(0.066)
Observations	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525
Division × Time FE					Y	Y	Y	Y
State × Recession FE			Y	Y			Y	Y
State linear trends		Y		Y		Y		Y

Notes. Linear probability models are estimated by regressing an indicator for being under alternative income-to-needs cutoffs (between 0.50 and 2.00) on log minimum wage and covariates. Elasticities are calculated by dividing the coefficient on log minimum wage by the sample proportion under the income-to-needs cutoff. All specifications include state and year fixed effects, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. Additional controls are indicated in the table. State-cluster-robust standard errors in parentheses.

** $p < 0.10$, ** $p < 0.5$, *** $p < 0.01$*

Table 5: Leading minimum wage elasticities for proportions under alternative income-to-needs cutoffs

Income-to-needs cutoff	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0.50	0.430***	0.246**	0.472***	0.380***	-0.132	-0.016	-0.068	0.105
	(0.136)	(0.115)	(0.161)	(0.120)	(0.141)	(0.133)	(0.154)	(0.149)
0.75	0.011	-0.004	0.129	0.167	-0.257**	-0.148	-0.199	-0.046
	(0.108)	(0.131)	(0.105)	(0.113)	(0.097)	(0.098)	(0.121)	(0.128)
1.00	-0.007	-0.003	0.110	0.141	-0.130	-0.067	-0.077	0.030
	(0.113)	(0.119)	(0.101)	(0.103)	(0.086)	(0.089)	(0.109)	(0.119)
1.25	-0.021	-0.014	0.046	0.047	-0.100	-0.066	-0.073	-0.008
	(0.115)	(0.105)	(0.105)	(0.101)	(0.089)	(0.087)	(0.115)	(0.121)
1.50	0.019	-0.008	0.058	0.024	-0.097	-0.097	-0.088	-0.060
	(0.085)	(0.079)	(0.089)	(0.092)	(0.072)	(0.074)	(0.096)	(0.108)
1.75	0.088	0.026	0.124	0.047	-0.023	-0.051	-0.009	-0.029
	(0.077)	(0.068)	(0.075)	(0.075)	(0.063)	(0.058)	(0.084)	(0.090)
2.00	0.068	-0.005	0.093	0.004	-0.032	-0.060	-0.033	-0.051
	(0.073)	(0.062)	(0.075)	(0.073)	(0.064)	(0.057)	(0.079)	(0.079)
Observations	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525
Division × Time FE					Y	Y	Y	Y
State × Recession FE			Y	Y			Y	Y
State linear trends		Y		Y		Y		Y

Notes. Linear probability models are estimated by regressing an indicator for being under alternative income-to-needs cutoffs (between 0.50 and 2.00) on distributed lags of log minimum wage and covariates. The leading elasticity is the one-year leading minimum wage coefficient divided by the sample proportion under the cutoff. All specifications include state and year fixed effects, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. Additional controls are indicated in the table. State-cluster-robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.5$, *** $p < 0.01$

Table 6: Long-run minimum elasticities for proportions under alternative income-to-needs cutoffs

Income-to-needs cutoff	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0.50	-0.279**	-0.326***	-0.287*	-0.383**	-0.360**	-0.275	-0.402***	-0.343**
	(0.110)	(0.110)	(0.144)	(0.150)	(0.169)	(0.168)	(0.113)	(0.134)
0.75	-0.177**	-0.179**	-0.334***	-0.337***	-0.255**	-0.212	-0.441***	-0.385***
	(0.077)	(0.085)	(0.107)	(0.118)	(0.113)	(0.128)	(0.099)	(0.123)
1.00	-0.125*	-0.142*	-0.292***	-0.288***	-0.171	-0.170	-0.366***	-0.363**
	(0.069)	(0.075)	(0.093)	(0.097)	(0.109)	(0.127)	(0.107)	(0.138)
1.25	-0.068	-0.090	-0.199**	-0.181**	-0.073	-0.096	-0.201*	-0.203*
	(0.053)	(0.061)	(0.077)	(0.073)	(0.097)	(0.108)	(0.107)	(0.120)
1.50	0.010	-0.028	-0.073	-0.067	0.002	-0.030	-0.049	-0.074
	(0.049)	(0.054)	(0.067)	(0.063)	(0.079)	(0.086)	(0.087)	(0.096)
1.75	0.004	-0.045	-0.090	-0.106**	0.021	-0.017	-0.069	-0.116
	(0.042)	(0.044)	(0.055)	(0.051)	(0.065)	(0.072)	(0.068)	(0.073)
2.00	0.052	-0.010	-0.036	-0.063	0.083	0.041	0.014	-0.024
	(0.044)	(0.045)	(0.057)	(0.054)	(0.059)	(0.065)	(0.068)	(0.070)
Observations	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525
Division × Time FE					Y	Y	Y	Y
State × Recession FE			Y	Y			Y	Y
State linear trends		Y		Y		Y		Y

Notes. Linear probability models are estimated by regressing an indicator for being under alternative income-to-needs cutoffs (between 0.50 and 2.00) on distributed lags of log minimum wage and covariates. The long-run elasticity is calculated from the sum of the contemporaneous and one-year lagged log minimum wage coefficients, divided by the sample proportion under the cutoff. All specifications include state and year fixed effects, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. Additional controls are indicated in the table. State-cluster-robust standard errors in parentheses.

** $p < 0.10$, ** $p < 0.5$, *** $p < 0.01$*

Table 7: Minimum wage elasticities for proportions under alternative income-to-needs cutoffs, by subgroup and sample period

Income-to-needs cutoff	Overall	1990-99	2000-12	Children	Single mothers	Black & Latino	Ages 21-44	Ages 21-44, HS
0.50	-0.337***	-0.977***	-0.175	-0.465***	-0.324*	-0.246	-0.215	-0.403**
	(0.125)	(0.170)	(0.158)	(0.149)	(0.168)	(0.235)	(0.136)	(0.169)
0.75	-0.340***	-0.749***	-0.280**	-0.425***	-0.232**	-0.566***	-0.313***	-0.481***
	(0.088)	(0.127)	(0.118)	(0.119)	(0.097)	(0.151)	(0.078)	(0.085)
1.00	-0.243**	-0.294*	-0.226*	-0.313**	-0.164*	-0.401***	-0.197*	-0.268**
	(0.100)	(0.165)	(0.123)	(0.127)	(0.087)	(0.116)	(0.108)	(0.105)
1.25	-0.158*	-0.126	-0.189*	-0.230**	-0.073	-0.316***	-0.120	-0.165*
	(0.091)	(0.170)	(0.111)	(0.103)	(0.069)	(0.074)	(0.101)	(0.088)
1.50	-0.083	0.000	-0.110	-0.192*	-0.093	-0.138***	-0.026	-0.081
	(0.079)	(0.176)	(0.093)	(0.106)	(0.082)	(0.049)	(0.081)	(0.083)
1.75	-0.066	0.009	-0.099	-0.181**	-0.145	-0.171***	0.005	-0.004
	(0.073)	(0.157)	(0.078)	(0.089)	(0.090)	(0.048)	(0.083)	(0.085)
2.00	-0.001	0.028	-0.048	-0.089	-0.114	-0.060	0.076	0.082
	(0.066)	(0.168)	(0.067)	(0.082)	(0.077)	(0.046)	(0.067)	(0.067)
Observations	3,646,525	1,271,617	2,374,908	1,173,388	189,452	979,334	1,408,230	622,507
Division × Time FE	Y	Y	Y	Y	Y	Y	Y	Y
State × Recession FE	Y	Y	Y	Y	Y	Y	Y	Y
State linear trends	Y	Y	Y	Y	Y	Y	Y	Y

Notes. Linear probability models are estimated by regressing an indicator for being under alternative income-to-needs cutoffs (between 0.50 and 2.00) on log minimum wage and covariates. Elasticities are calculated by dividing the coefficient on log minimum wage by the sample proportion under the income-to-needs cutoff. The regression specification includes state fixed effects, division-specific year effects, state-specific recession year effects, state-specific linear trends, and state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. State-cluster-robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.5$, *** $p < 0.01$

Table 8: Minimum wage elasticities for alternative poverty measures

Income-to-needs cutoff	Poverty rate: alternative equivalence scales		Alternative poverty measures	
	Standard scale	Square root scale	Poverty gap	Squared poverty gap
0.50	-0.337***	-0.369***	-0.326*	-0.593
	(0.125)	(0.125)	(0.166)	(0.356)
0.75	-0.340***	-0.359***	-0.336***	-0.767**
	(0.088)	(0.100)	(0.125)	(0.370)
1.00	-0.243**	-0.328***	-0.323***	-0.964**
	(0.100)	(0.102)	(0.101)	(0.376)
1.25	-0.158*	-0.131	-0.284***	-1.129***
	(0.091)	(0.082)	(0.091)	(0.390)
1.50	-0.083	-0.079	-0.232***	-1.238***
	(0.079)	(0.085)	(0.085)	(0.414)
1.75	-0.066	-0.015	-0.186**	-1.288***
	(0.073)	(0.075)	(0.078)	(0.441)
2.00	-0.001	0.036	-0.148**	-1.301***
	(0.066)	(0.073)	(0.073)	(0.470)
Observations	3,646,525	3,646,525	3,646,525	3,646,525
Division × Time FE	Y	Y	Y	Y
State × Recession FE	Y	Y	Y	Y
State linear trends	Y	Y	Y	Y

Notes. Linear probability models are estimated by regressing an indicator for being under alternative income-to-needs cutoffs (between 0.50 and 2.00) on log minimum wage and covariates. Elasticities are calculated by dividing the coefficient on log minimum wage by the sample proportion under the income-to-needs cutoff. The regression specification includes state fixed effects, division-specific year effects, state-specific recession year effects, state-specific linear trends, and state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. State-cluster-robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.5$, *** $p < 0.01$

Table 9: Minimum wage elasticities for unconditional quantiles of equivalized family incomes

Income-to-needs quantile	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
5	-0.187	0.056	-0.026	0.119	0.476**	0.333	0.613***	0.466**
	(0.140)	(0.116)	(0.141)	(0.144)	(0.184)	(0.209)	(0.163)	(0.222)
10	0.131	0.139	0.184**	0.182*	0.315***	0.202*	0.430***	0.316***
	(0.079)	(0.104)	(0.074)	(0.101)	(0.089)	(0.101)	(0.072)	(0.092)
15	0.100	0.111*	0.144**	0.127**	0.190**	0.147*	0.264***	0.212**
	(0.065)	(0.066)	(0.056)	(0.062)	(0.074)	(0.087)	(0.070)	(0.089)
20	0.038	0.067	0.089*	0.087*	0.102	0.118	0.149**	0.136
	(0.054)	(0.045)	(0.050)	(0.044)	(0.068)	(0.075)	(0.073)	(0.084)
25	-0.037	0.020	-0.006	0.025	0.040	0.055	0.058	0.061
	(0.042)	(0.039)	(0.036)	(0.038)	(0.057)	(0.064)	(0.062)	(0.074)
30	-0.076*	0.010	-0.038	0.024	-0.031	0.008	-0.002	0.024
	(0.043)	(0.034)	(0.038)	(0.036)	(0.055)	(0.058)	(0.058)	(0.067)
35	-0.092**	0.002	-0.041	0.026	-0.060	-0.026	-0.018	-0.012
	(0.041)	(0.028)	(0.039)	(0.029)	(0.046)	(0.050)	(0.047)	(0.055)
40	-0.098**	0.001	-0.041	0.032	-0.036	0.004	0.009	0.024
	(0.038)	(0.023)	(0.039)	(0.026)	(0.043)	(0.043)	(0.042)	(0.044)
45	-0.099***	-0.010	-0.047	0.017	-0.051	-0.012	-0.012	-0.005
	(0.034)	(0.024)	(0.036)	(0.028)	(0.039)	(0.046)	(0.039)	(0.051)
50	-0.087***	-0.009	-0.035	0.030	-0.050	-0.008	-0.009	0.010
	(0.031)	(0.025)	(0.033)	(0.030)	(0.042)	(0.049)	(0.044)	(0.057)
Observations	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525
Division × Time FE					Y	Y	Y	Y
State × Recession FE			Y	Y			Y	Y
State linear trends		Y		Y		Y		Y

Notes. Linear probability models are estimated by regressing an indicator for being under the income-to-needs cutoff associated with a quantile (between 5 and 50) on log minimum wage and covariates. Unconditional quantile partial effects (UQPE) for equivalized family incomes are calculated by dividing the coefficient on log minimum wage by the negative of the income-to-needs density at the appropriate quantile. The UQPE estimates are subsequently divided by the income-to-needs cutoff for the quantile to transform the estimates into elasticities. All specifications include state and year fixed effects, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. Additional controls are indicated in the table. State-cluster-robust standard errors in parentheses.
** $p < 0.10$, ** $p < 0.5$, *** $p < 0.01$*

Table 10: Long-run minimum wage elasticities for unconditional quantiles of equivalized family incomes

Income-to-needs quantile	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
5	0.010	0.089	0.228	0.274	0.391	0.269	0.462*	0.357
	(0.195)	(0.189)	(0.267)	(0.263)	(0.256)	(0.273)	(0.238)	(0.268)
10	0.175**	0.168**	0.285**	0.303**	0.214*	0.170	0.393***	0.331***
	(0.077)	(0.082)	(0.112)	(0.122)	(0.110)	(0.127)	(0.095)	(0.117)
15	0.113*	0.126*	0.258***	0.253***	0.157	0.150	0.324***	0.319**
	(0.060)	(0.066)	(0.078)	(0.081)	(0.095)	(0.112)	(0.093)	(0.120)
20	0.036	0.077	0.158**	0.151**	0.046	0.098	0.151	0.164
	(0.049)	(0.056)	(0.071)	(0.065)	(0.083)	(0.093)	(0.096)	(0.106)
25	-0.006	0.035	0.063	0.072	-0.005	0.023	0.044	0.075
	(0.043)	(0.047)	(0.059)	(0.055)	(0.068)	(0.075)	(0.076)	(0.083)
30	-0.015	0.038	0.055	0.075	-0.043	-0.002	0.019	0.059
	(0.038)	(0.041)	(0.050)	(0.048)	(0.061)	(0.065)	(0.066)	(0.070)
35	-0.050	0.006	0.029	0.049	-0.084	-0.052	-0.006	0.015
	(0.038)	(0.036)	(0.047)	(0.044)	(0.053)	(0.059)	(0.061)	(0.067)
40	-0.032	0.024	0.044	0.065*	-0.058	-0.023	0.023	0.054
	(0.036)	(0.031)	(0.038)	(0.034)	(0.047)	(0.051)	(0.055)	(0.059)
45	-0.040	0.018	0.036	0.054	-0.072	-0.033	0.002	0.016
	(0.037)	(0.033)	(0.037)	(0.036)	(0.046)	(0.055)	(0.051)	(0.061)
50	-0.046	0.004	0.033	0.061	-0.079	-0.040	0.007	0.033
	(0.039)	(0.037)	(0.042)	(0.043)	(0.049)	(0.054)	(0.049)	(0.059)
Observations	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525
Division × Time FE					Y	Y	Y	Y
State × Recession FE			Y	Y			Y	Y
State linear trends		Y		Y		Y		Y

Notes. Linear probability models are estimated by regressing an indicator for being under the income-to-needs cutoff associated with a quantile (between 5 and 50) on distributed lags of log minimum wage and covariates. Unconditional long-run quantile partial effects (UQPE) for equivalized family incomes are calculated by dividing the sum of the coefficients on the current and one-year lagged log minimum wage by the negative of the income-to-needs density at the appropriate quantile. The long-run UQPE estimates are subsequently divided by the income-to-needs cutoff for the quantile to transform the estimates into elasticities. All specifications include state and year fixed effects, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. Additional controls are indicated in the table. State-cluster-robust standard errors in parentheses.
** $p < 0.10$, ** $p < 0.5$, *** $p < 0.01$*

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Appendix: Additional Figures and Tables

This appendix contains additional figures and tables that are referred to in the text. Figure A1 plots the minimum wage elasticities for proportions under alternative income-to-needs cutoffs between 0.5 and 3 in increments of 0.25. It does so for all eight specifications, with the control sets as indicated in each of the figure panels. The panels with specifications 1 and 8 are repeated from Figure 5 in the paper.

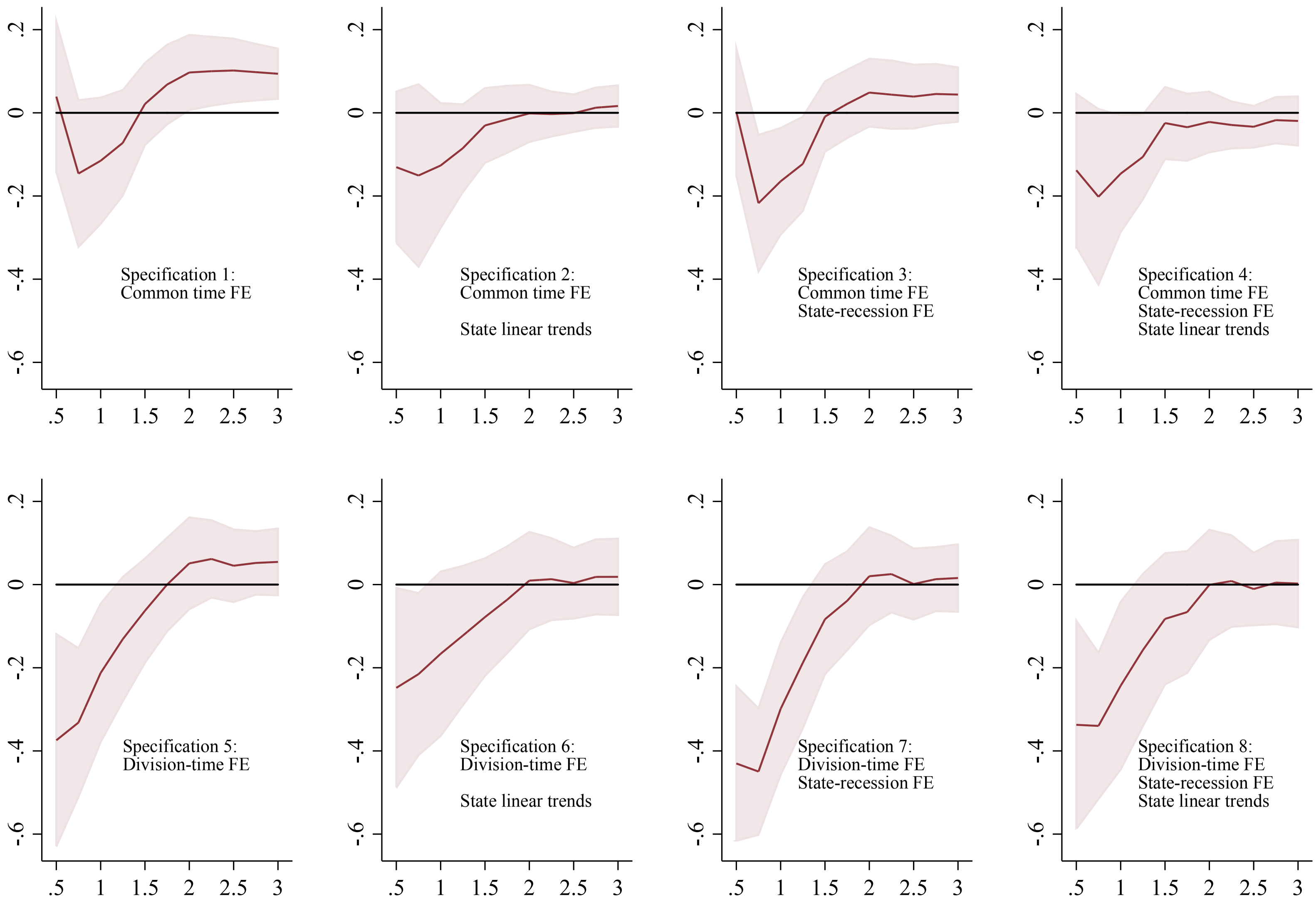
Figures A2 and A3 report results from the dynamic specification with one-year leading and one-year lagged log minimum wages, along with the contemporaneous log minimum wage. Figure A2 shows the elasticities for proportions under alternative cutoffs with respect to the one-year leading log minimum wage for all eight specifications. Figure A3 shows the long-run elasticities for proportions under alternative cutoffs: they are constructed as sums of the regression coefficients associated with the contemporaneous and one-year lagged log minimum wages, and divided by the sample proportion under the cutoff. The panels with specifications 1 and 8 are repeated from Figures 6 and 7 in the paper.

Figure A4 shows the minimum wage elasticities for the unconditional quantiles of family incomes for all eight specifications, between the 5th and 50th quantiles in increments of 5. First, unconditional quantile partial effects (UQPE) for equivalized family incomes are calculated from the linear probability model by dividing the coefficient on log minimum wage by the negative of the income-to-needs density at the appropriate quantile. The UQPE estimates are divided by the income-to-needs cutoff for the quantile to transform the estimates into elasticities. The panels with specifications 1 and 8 are repeated from Figure 8 in the paper.

The main tables in the paper (e.g., Table 4) report the estimates for proportions below income-to-needs cutoffs as minimum wage elasticities, and show results for cutoffs between 0.50 and 2.00 in increments of 0.25. In Table A1, I report the underlying regression coefficients on log minimum wage (semi-elasticities), and for a wider range of cutoffs (between 0.50 and 3.00). In Table A2, I report the sum of regression coefficients (semi-elasticities) for the contemporaneous and one-year lagged log minimum wages, also for income-to-needs cutoffs between 0.50 and 3.00. (The associated long-run elasticities are reported in main Table 6 for cutoffs up to 2.00.)

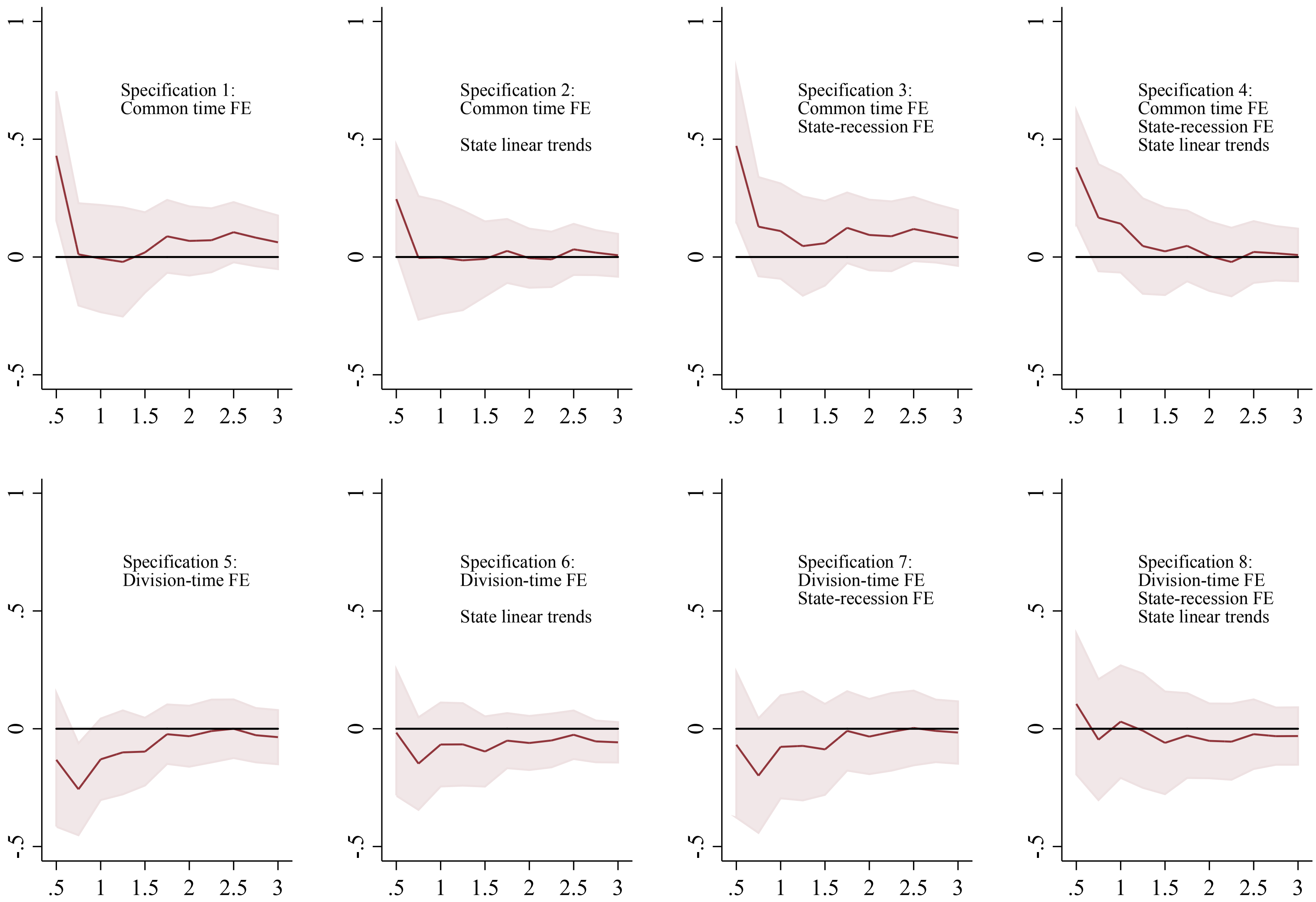
Table A3 decomposes the differences in the minimum wage elasticities for proportions under cutoffs between the least saturated specification 1 and the most saturated specification 8. Panels A and B provide decompositions for the contemporaneous estimate from the regressions without lagged minimum wages, and long-run estimates from the distributed lag regressions, respectively.

Figure A1: Minimum wage elasticities for proportions under alternative income-to-needs cutoffs



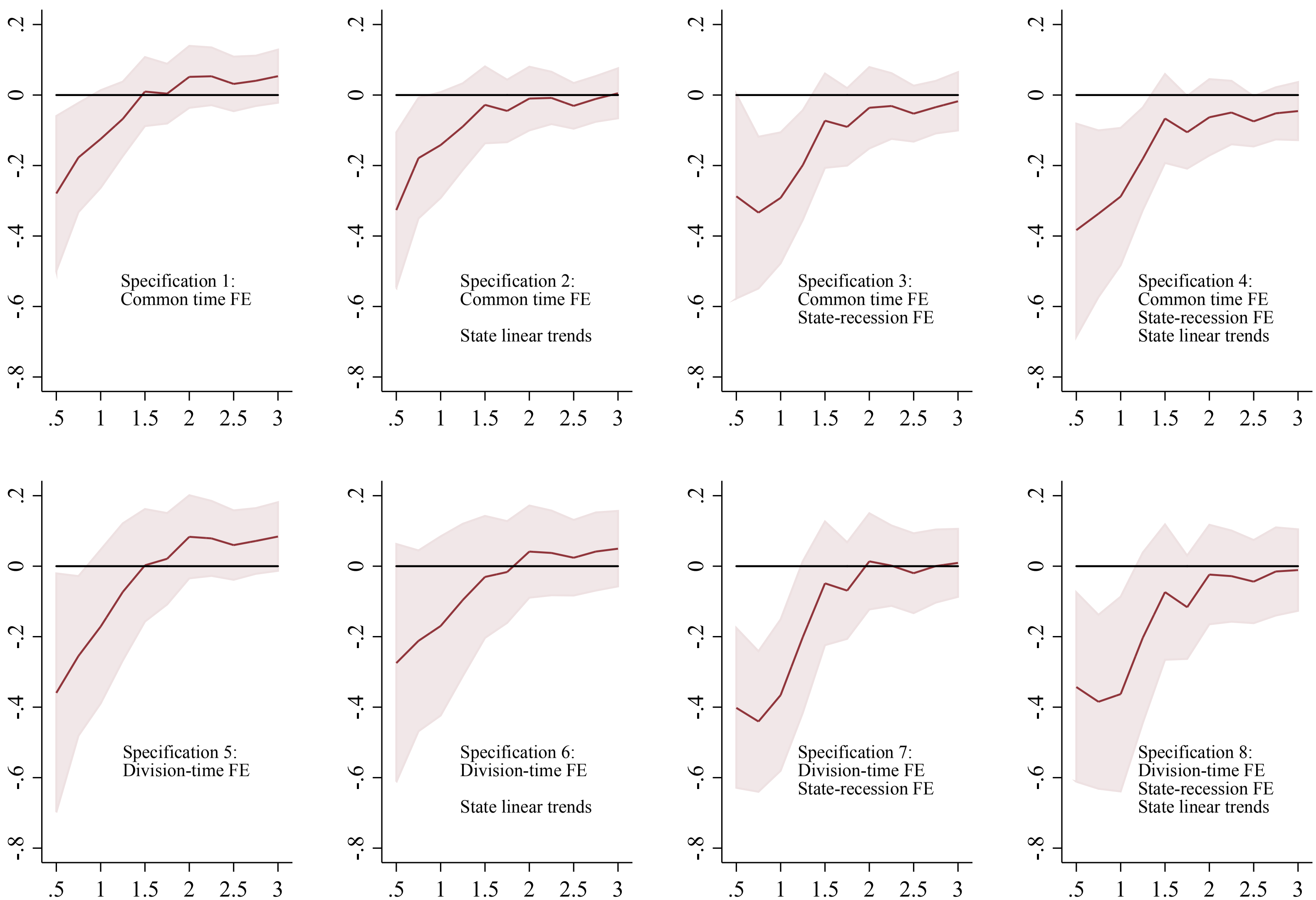
Notes. Horizontal axes indicate the income-to-needs cutoffs, and vertical axes measure the minimum wage elasticity for the proportion under the cutoff. A series of linear probability models are estimated by regressing an indicator for being under alternative income-to-needs cutoffs (between 0.5 and 3) on log minimum wage and covariates. Elasticities are calculated by dividing the coefficient on log minimum wage by the sample proportion under the income-to-needs cutoff. All specifications include state and year fixed effects, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. Additional controls are indicated in the figure. Shaded area represents 95% state-cluster-robust confidence intervals.

Figure A2: One-year leading minimum wage elasticities for proportions under alternative income-to-needs cutoffs



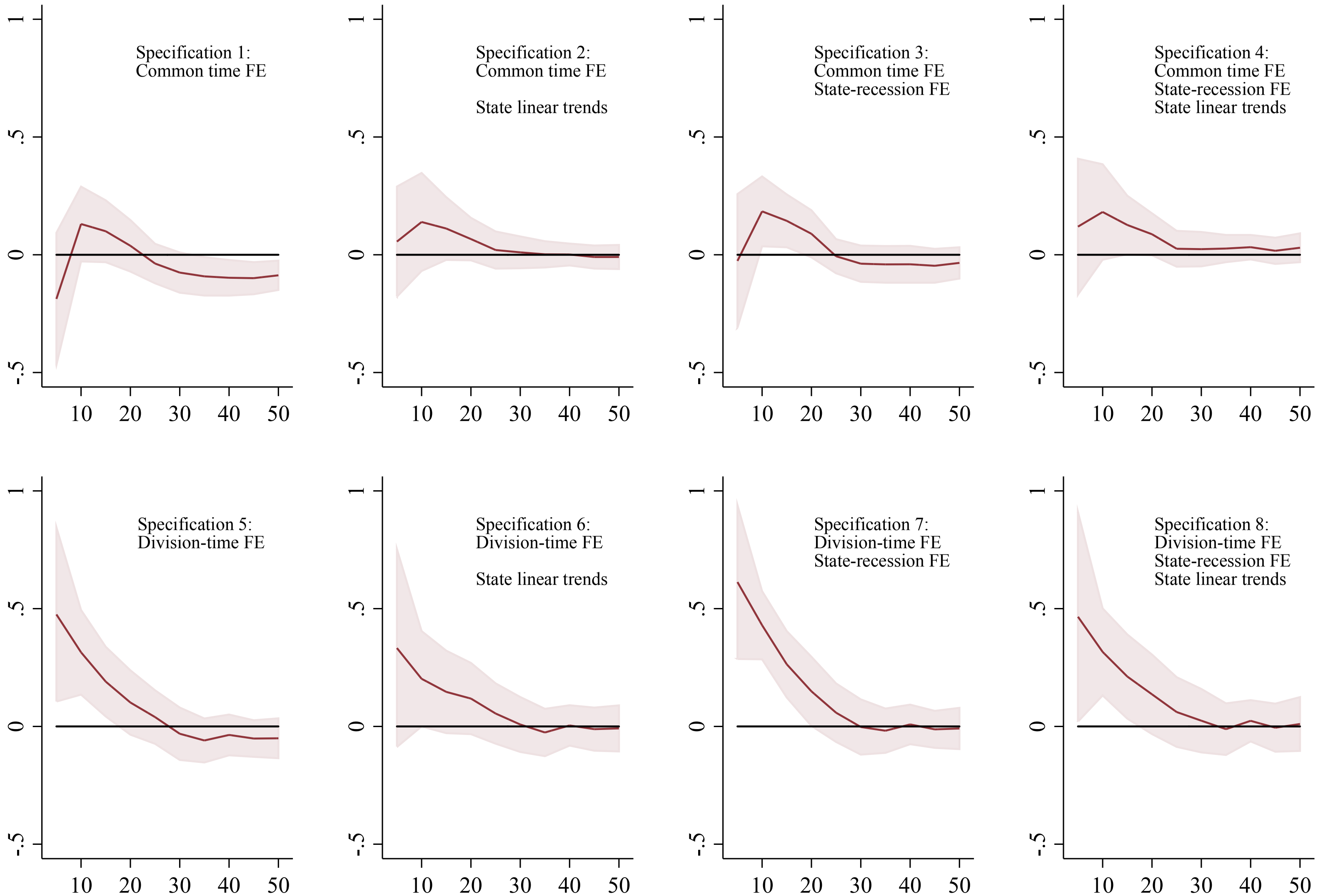
Notes. Horizontal axes indicate the income-to-needs cutoffs, and vertical axes measure the minimum wage elasticity for the proportion under the cutoff. A series of linear probability models are estimated by regressing an indicator for being under alternative income-to-needs cutoffs (between 0.50 and 3.00) on distributed lags of log minimum wage and covariates. The leading elasticity is the one-year leading minimum wage coefficient divided by the sample proportion under the cutoff. All specifications include state and year fixed effects, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. Additional controls are indicated in the figure. Shaded area represents 95% state-cluster-robust confidence intervals.

Figure A3: Long-run minimum wage elasticities for proportions under alternative income-to-needs cutoffs



Notes. Horizontal axes indicate the income-to-needs cutoffs, and vertical axes measure the minimum wage elasticity for the proportion under the cutoff. A series of linear probability models are estimated by regressing an indicator for being under alternative income-to-needs cutoffs (between 0.5 and 3.00) on distributed lags of log minimum wage and covariates. The long-run elasticity is calculated from the sum of the contemporaneous and one-year lagged log minimum wage coefficients, divided by the sample proportion under the cutoff. All specifications include state and year fixed effects, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. Additional controls are indicated in the figure. Shaded area represents 95% state-cluster-robust confidence intervals.

Figure A4: Minimum wage elasticities for unconditional quantiles of equivalized family income



Notes. Horizontal axes indicate the equivalized family income quantile, and vertical axes measure the minimum wage elasticity for the unconditional quantile of equivalized family incomes. A series of linear probability models are estimated by regressing an indicator for being under income-to-needs cutoffs associated with alternative quantiles (between 5 and 50) on log minimum wage and covariates. Unconditional quantile partial effects (UQPE) for equivalized family income are calculated by dividing the coefficient on log minimum wage by the negative of the income-to-needs density at the appropriate quantile. The UQPE estimates are subsequently divided by the income-to-needs cutoff for the quantile to transform the estimates into elasticities. All specifications include state and year fixed effects, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. Additional controls are indicated in the figure. Shaded area represents 95% state-cluster-robust confidence intervals.

Table A1: Minimum wage semielasticities for proportions under alternative income-to-needs cutoffs

Income-to-needs cutoff	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0.50	0.003	-0.009	0.000	-0.010	-0.026***	-0.017**	-0.030***	-0.023***
	(0.006)	(0.006)	(0.005)	(0.006)	(0.009)	(0.008)	(0.006)	(0.009)
0.75	-0.016	-0.016	-0.024**	-0.022*	-0.036***	-0.023**	-0.049***	-0.037***
	(0.010)	(0.012)	(0.009)	(0.011)	(0.010)	(0.011)	(0.008)	(0.009)
1.00	-0.017	-0.019*	-0.025**	-0.022**	-0.032**	-0.025*	-0.045***	-0.036**
	(0.011)	(0.011)	(0.010)	(0.011)	(0.012)	(0.015)	(0.012)	(0.015)
1.25	-0.014	-0.016	-0.024**	-0.020**	-0.025*	-0.024	-0.036**	-0.030*
	(0.012)	(0.010)	(0.011)	(0.010)	(0.014)	(0.016)	(0.015)	(0.018)
1.50	0.005	-0.007	-0.002	-0.006	-0.015	-0.019	-0.020	-0.020
	(0.012)	(0.011)	(0.010)	(0.010)	(0.015)	(0.017)	(0.016)	(0.019)
1.75	0.019	-0.004	0.006	-0.010	0.000	-0.010	-0.011	-0.019
	(0.013)	(0.011)	(0.012)	(0.011)	(0.016)	(0.018)	(0.017)	(0.021)
2.00	0.031**	-0.000	0.016	-0.007	0.017	0.003	0.007	-0.000
	(0.015)	(0.011)	(0.013)	(0.012)	(0.018)	(0.019)	(0.019)	(0.021)
2.25	0.037**	-0.001	0.016	-0.011	0.023	0.005	0.009	0.003
	(0.015)	(0.010)	(0.015)	(0.010)	(0.017)	(0.018)	(0.017)	(0.020)
2.50	0.042**	-0.000	0.016	-0.014	0.019	0.001	0.001	-0.004
	(0.016)	(0.009)	(0.016)	(0.010)	(0.018)	(0.017)	(0.018)	(0.018)
2.75	0.044***	0.006	0.020	-0.008	0.024	0.008	0.006	0.002
	(0.015)	(0.011)	(0.016)	(0.013)	(0.017)	(0.020)	(0.017)	(0.023)
3.00	0.046***	0.008	0.022	-0.010	0.027	0.009	0.008	0.001
	(0.015)	(0.012)	(0.016)	(0.015)	(0.020)	(0.023)	(0.020)	(0.026)
Observations	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525
Division × Time FE					Y	Y	Y	Y
State × Recession FE			Y	Y			Y	Y
State linear trends		Y		Y		Y		Y

Notes. Linear probability models are estimated by regressing an indicator for being under alternative income-to-needs cutoffs (between 0.5 and 3) on log minimum wage and covariates. Semielasticities are the coefficient on the log minimum wage. All specifications include state and year fixed effects, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. Additional controls are indicated in the table. State-cluster-robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.5$, *** $p < 0.01$

Table A2: Long-run minimum wage semielasticities for proportions under alternative income-to-needs cutoffs

Income-to-needs cutoff	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0.50	-0.019**	-0.023***	-0.020*	-0.027**	-0.025**	-0.019	-0.028***	-0.024**
	(0.008)	(0.008)	(0.010)	(0.010)	(0.012)	(0.012)	(0.008)	(0.009)
0.75	-0.019**	-0.019**	-0.036***	-0.036***	-0.028**	-0.023	-0.048***	-0.042***
	(0.008)	(0.009)	(0.012)	(0.013)	(0.012)	(0.014)	(0.011)	(0.013)
1.00	-0.019*	-0.021*	-0.044***	-0.043***	-0.026	-0.025	-0.055***	-0.054**
	(0.010)	(0.011)	(0.014)	(0.015)	(0.016)	(0.019)	(0.016)	(0.021)
1.25	-0.013	-0.017	-0.038**	-0.035**	-0.014	-0.018	-0.039*	-0.039*
	(0.010)	(0.012)	(0.015)	(0.014)	(0.019)	(0.021)	(0.021)	(0.023)
1.50	0.002	-0.007	-0.017	-0.016	0.001	-0.007	-0.012	-0.017
	(0.012)	(0.013)	(0.016)	(0.015)	(0.019)	(0.020)	(0.021)	(0.023)
1.75	0.001	-0.013	-0.025	-0.030**	0.006	-0.005	-0.019	-0.033
	(0.012)	(0.012)	(0.015)	(0.014)	(0.018)	(0.020)	(0.019)	(0.021)
2.00	0.017	-0.003	-0.012	-0.020	0.027	0.013	0.004	-0.008
	(0.014)	(0.015)	(0.019)	(0.018)	(0.019)	(0.021)	(0.022)	(0.023)
2.25	0.020	-0.003	-0.011	-0.018	0.029	0.014	0.001	-0.010
	(0.015)	(0.014)	(0.017)	(0.017)	(0.020)	(0.022)	(0.021)	(0.024)
2.50	0.013	-0.013	-0.022	-0.031**	0.025	0.010	-0.008	-0.018
	(0.016)	(0.013)	(0.016)	(0.015)	(0.020)	(0.022)	(0.023)	(0.024)
2.75	0.018	-0.005	-0.016	-0.024	0.032	0.019	0.000	-0.007
	(0.016)	(0.015)	(0.017)	(0.017)	(0.021)	(0.025)	(0.023)	(0.028)
3.00	0.026	0.002	-0.009	-0.022	0.041*	0.025	0.005	-0.005
	(0.019)	(0.017)	(0.020)	(0.020)	(0.024)	(0.026)	(0.024)	(0.028)
Observations	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525	3,646,525
Division × Time FE					Y	Y	Y	Y
State × Recession FE			Y	Y			Y	Y
State linear trends		Y		Y		Y		Y

Notes. Linear probability models are estimated by regressing an indicator for being under alternative income-to-needs cutoffs (between 0.5 and 3) on distributed lags of log minimum wage and covariates. Semielasticities are the coefficient on the sum of the coefficients of the contemporaneous and one-year lagged log minimum wage. All specifications include state and year fixed effects, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, marital status, family size, number of children, education level, Hispanic status, and gender), and are weighted by March CPS person weights. Additional controls are indicated in the table. State-cluster-robust standard errors in parentheses. ** $p < 0.10$, ** $p < 0.5$, *** $p < 0.01$*

Table A3: Decomposing the difference between the most and the least saturated specifications: minimum wage elasticities for proportions under alternative income-to-needs cutoffs

Panel A: Estimates from Table 4 without lags

	State linear trends	State × Recession	Division × Time	Total Difference: Spec 8 - Spec 1
Income-to-needs cutoff	*Average Incremental Contribution (Elasticities)*			
0.50	-0.028	-0.052	-0.296	-0.376
0.75	0.057	-0.093	-0.157	-0.194
1.00	0.026	-0.060	-0.094	-0.128
1.25	0.010	-0.041	-0.054	-0.085
	Average Incremental Contribution (Proportion of Total)			
0.50	7.4%	13.9%	78.7%	100.0%
0.75	-29.4%	48.2%	81.2%	100.0%
1.00	-20.0%	46.7%	73.3%	100.0%
1.25	-11.7%	48.5%	63.2%	100.0%
Overall	-13.4%	39.3%	74.1%	100.0%

Panel B: Long-run estimates from Table 6

	State linear trends	State × Recession	Division × Time	Total Difference: Spec 8 - Spec 1
Income-to-needs cutoff	*Average Incremental Contribution (Elasticities)*			
0.50	0.002	-0.042	-0.024	-0.063
0.75	0.025	-0.167	-0.065	-0.208
1.00	-0.004	-0.177	-0.057	-0.238
1.25	-0.009	-0.116	-0.011	-0.135
	Average Incremental Contribution (Proportion of Total)			
0.50	-3.4%	66.0%	37.4%	100.0%
0.75	-11.9%	80.5%	31.4%	100.0%
1.00	1.6%	74.3%	24.1%	100.0%
1.25	6.5%	85.7%	7.8%	100.0%
Overall	-1.8%	76.6%	25.2%	100.0%

Notes. This table provides decompositions of the estimates between the least saturated specification (1) and most saturated specification (8) for: the estimates from the specification without lags (Panel A), and long-run estimates from the dynamic models (Panel B). For each of the four income-to-needs cutoffs, column 5 reports the total difference in the estimated elasticities between the most and the least saturated specifications, as reported in columns 8 and 1 in Table 4 (Panel A) and Table 6 (Panel B), respectively. This total difference can be decomposed using 6 different paths between specifications 1 and 8 that incrementally add Division × Time effects, State × recession effects, and state linear trends in different orders. The first section reports the average the incremental contributions of each of these three sets of controls along the six paths. The second section reports these average incremental contributions as a proportion of the total difference between estimates from specifications 1 and 8. The last row averages these contributions further across the four income-to-needs cutoffs.