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ABSTRACT

Minimum Wages and the Distribution of Family Incomes*

Using the March Current Population Survey data from 1984 to 2013, I provide a comprehensive evaluation of how minimum wage policies influence the distribution of family incomes. I find robust evidence that higher minimum wages shift down the cumulative distribution of family incomes at the bottom, reducing the share of non-elderly individuals with incomes below 50, 75, 100, and 125 percent of the federal poverty threshold. The long run (3 or more years) minimum wage elasticity of the non-elderly poverty rate with respect to the minimum wage ranges between -0.22 and -0.55 across alternative specifications that subsume most of the approaches used in the literature to construct valid counterfactuals. Inverting the policy's effect on the cumulative distribution, I estimate minimum wage elasticities for unconditional quantiles of family incomes. The long run minimum wage elasticities for the 10th and 15th unconditional quantiles of equivalized family incomes range between 0.15 and 0.49 depending on specification. A reduction in public assistance partly offsets these income gains, which are on average 72% as large when using an expanded income definition including tax credits and non-cash transfers.

JEL Classification: J38, J88, D31

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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 employment effects of minimum wages—see Belman and Wolfson (2014) for a recent review—there are relatively fewer studies that empirically estimate the impact of the policy on family incomes. Compounding the problem, the existing papers often suffer from a number of key shortcomings including insufficient attention to the validity of the research design, and use of small samples sometimes with limited minimum wage variation—all of which tend to produce somewhat erratic and imprecise estimates.

In this paper, I use individual-level data from the March Current Population Survey (CPS) between 1984 and 2013 to provide a more complete assessment of how U.S. minimum wage policies shift the distribution of family incomes for the non-elderly population than has been to date.[1] This paper makes three important advances over the existing literature. First, I fully characterize how minimum wage increases shift the cumulative distribution of family incomes, and subsequently use this to estimate unconditional quantile partial effects (UQPE) of the policy. This provides a fuller picture of the distributional effects of the policy than available to date. Second, I show how the income effects by quantile varies when we consider broader income definitions to include tax credits and non-cash transfers. I am able to quantify the extent to which offsets through reduced public assistance affects the distributional impact of minimum wages. Third, I provide estimates that are more credibly causal than in the literature by utilizing a wide range of specifications with alternative sets of controls for time-varying heterogeneity, and by assessing the assumption of parallel trends using an array of falsification tests across the income distribution. Specifically, I use distributed lag specifications that both includes leads and up to three years of lags in the minimum wage, allowing for a delayed impact of the policy; and I allow for time varying heterogeneity by region, business cycles and state-specific trends.

Overall, I find robust evidence that higher minimum wages lead to increases in incomes at the bottom of the family income distribution. For the poverty rate, long-run minimum wage elasticities

[1] In this paper, when I refer to the 1984-2013 period, I am referring to the survey years for the March CPS. Note, however, that respondents in March 2013 CPS survey are asked about their income during the year 2012.

between -0.220 and -0.552 obtain from eight specifications ranging from the classic two-way fixed effects model to a model with rich set of controls for trends, regional shocks and business cycle heterogeneity. All of these estimates are all statistically distinguishable from zero at conventional levels.[2] There is a reduction in the shares below income cutoffs between 50 and 125 percent of the federal poverty threshold (FPT), with the largest proportionate reductions occurring around 75 percent of the poverty threshold (elasticities ranging between -0.186 and -0.652). When I look across demographic groups using the preferred (most saturated) specification, the poverty rate elasticities are similar when restricted to younger (under 30) and lower credentialed individuals without a high school degree. However, the elasticity is relatively larger for black and Latino individuals (-1.066), while relatively smaller for single mothers (-0.301).

By estimating how a minimum wage increase affects the shares of population with incomes below various multiples of the federal poverty threshold we identify the effect of the policy on the the cumulative distribution function (CDF) of equivalized family incomes. Subsequently, we can invert the impact of the policy on the CDF to estimate the impact on an income quantile, i.e., the UQPE. I use the recentered influence function (RIF) regression approach of Firpo et al. (2009), which performs this inversion using a local linear approximation to the counterfactual CDF. This local inversion entails rescaling the marginal effect of minimum wages on the share above an income cutoff by the probability density of income at that cutoff.[3]

I find clear, positive effects of minimum wages on family incomes below the 20th quantile. The largest impact occurs at the 10th and 15th quantiles, where estimates from most specifications are statistically significant, and the long run minimum wage elasticities for these family income quantiles range between 0.152 and 0.493 depending on control sets. In the preferred (most saturated) specification, the family income elasticities with respect to the minimum wage are around 0.426 and

[2] Most 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.

[3] 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, here we are 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.

0.407 for the 10th and 15th quantiles, respectively, and diminish close to zero by the 30th quantile. Since the conventional income definition used for official poverty calculations does not include tax credits such as the Earned Income Tax Credit (EITC), or non-cash transfers such as Supplemental Nutritional Assistance Program (SNAP), I also estimate the impact using an expanded income definition. After accounting for tax credits and non-cash transfers, the minimum wage effect on the level of family incomes (i.e., the semi-elasticities) are about 72% as large for the bottom 30 percent of the distribution. Overall, the evidence clearly points to at least moderate income gains for low income families resulting from minimum wage increases. At the same time, there is evidence for some substitution of government transfers with earnings as evidenced by the somewhat smaller income increase accounting for tax credit and non-cash transfers such as SNAP and EITC.

While this paper substantially improves upon existing research on the topic of minimum wages, family income distribution and poverty, the existing research is consistent with the proposition that minimum wages likely reduce poverty. In online Appendix A, I quantitatively assess estimates from the 13 key papers in the literature by collecting or constructing nearly every minimum wage elasticity for the poverty rate from those studies for a variety of demographic groups 72 out of the 78 elasticities have a negative sign, and a simple average of the 78 elasticities produces a poverty rate elasticity of -0.18, while the average of the median elasticities from each study is -0.17. For the 8 of these 13 studies that actually report an estimate for overall poverty (as opposed to for narrower subgroups), the average of poverty rate elasticities is -0.13. These averages are broadly consistent with the range of findings in this paper in pointing toward a poverty reduction effect of minimum wages, though they tend to be smaller in magnitude than what I find.

At the same time, the existing evidence is clouded by serious shortcomings in these studies: insufficient controls for state-level heterogeneity; short time periods, sometimes with little minimum wage variation; over-statement of precision due to improper methods of statistical inference; and the use of idiosyncratic sets of outcomes and target groups. In contrast, I use 30 years of data from a period with a large amount of cross-state minimum wage variation. I also pay close attention to the non-random nature of minimum wage policies (Allegretto et al. forthcoming). Since there is disagreement in the literature on the appropriateness of particular specifications, I show results using eight different specifications with alternative controls for state-level heterogeneity that subsumes much of the approaches used in the literature. Starting with the classic two-way fixed effects model, I progressively add regional controls, state specific trends, and state-specific business cycle effects—all of which have been shown to be important in the extant minimum wage literature (e.g., Allegretto et al. forthcoming, Zipperer 2016). The resulting set of estimates produces a credible range regardless of one's priors on, say, the desirability of using regional controls. At the same time, I assess the internal validity of various specifications using a host of falsification tests including estimating effects higher up in the income distribution, as well as analyzing leading effects (pre-existing trends) across specifications. 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 adds to a growing literature that estimates distributional effects of policies using a quasi-experimental design. For example, Havnes and Mogstad (2015) also use RIF regressions in a difference-in-difference setting to study the distributional impact of universal child care and find that a small average effect masks the more sizable increases in adult earnings at the bottom quantiles. The distributional analysis in this paper is also similar to Hoynes and Patel (2016), who show how EITC expansion shifted the lower end of the family income distribution. Finally, Autor et al. (2016) estimate the effect of minimum wages on the hourly wage distribution, reporting estimates by wage quantiles.

The rest of the paper is structured as follows. In section 2, I describe the data and research design, including the RIF estimation of unconditional quantile partial effects. Section 3 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 4 concludes with a discussion of the policy implications.

2 Data and research design

2.1 Data and sample construction

I use individual level data from the UNICON extract of the March Current Population Survey (CPS) between 1984 and 2013. I augment the CPS data with information on state EITC supplements,[4] state per-capita GDP, and state unemployment rates from the University of Kentucky Center for

[4]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.

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 30 year period studied in this paper. Figure B.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 there has been much more variation in minimum wages since 2000. 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 population as a whole.[5] 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 large part due to Social Security. For example, CPS data from March 2013 shows that 9.1 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 15.9 and 7.2 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). However, I also show that the overall estimates are quite similar if we include the elderly population.

As I discuss in Online Appendix A:, a number of researchers have studied the impact of minimum wages on children and single mothers (e.g., Morgan and Kickham 2001, DeFina 2008, Gundersen and Ziliak 2004). Several studies have also considered younger adults, or adults with limited education; these include Neumark (2016), Addison and Blackburn (1999), and Sabia and Nielsen (2015). 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

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

the literature. These include (1) all individuals without a high school degree (3) individuals younger than 30 without a high school degree, (4) individuals with high school or less schooling, (5) black or Latino individuals, (6) children under 18 years of age, (7) all adults and (8) single (unmarried) mothers with children.

2.2 Outcomes and research design

This paper focuses on equivalized[6] real family income, defined as multiples of the federal poverty threshold: $y_{it} = \frac{Y_{it}}{FPT(N_i, Children_i, t)}$. This is also sometimes referred to as the income-to-needs (ITN) ratio. 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 start with the conventional definition of family income as is used for official poverty measurement: pre-tax cash income which includes earnings and cash transfers, but does not include non-cash benefits or tax credits.[7] I use the term family to refer to primary families, sub-families, as well as single (non-family) householders. Family income and poverty thresholds are computed for each family type separately.[8]

The official poverty measure excludes non-cash transfers such as SNAP and housing assistance, as well as tax credits such as EITC. As Fox et al. (2014) shows, exclusion of these categories of income tends to understate the economic gains made by families in the lower end of the income distribution during the 1990s and 2000s. Moreover, there is evidence that minimum wages may reduce some types of transfers such as SNAP (Reich and West 2015). Therefore, I also show income quantile elasticities using an expanded income definition that includes monetized value of non-cash transfers (SNAP, housing assistance, school lunch) and tax credits (EITC, child tax credit, and additional child tax credit). The values for the non-cash transfers are self reported, while tax credits are calculated by the Census Bureau based on the respondents' reported income.

[6]Equivalized family income is adjusted for family size and composition.

[7]Eligible income includes earnings (excluding capital loss or gains), unemployment compensation, workers' compensation, Social Security, Supplemental Security Income, cash-based public assistance such as Temporary Assistance to Needy Families (TANF), 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 of cash income.

[8]I use the FAMINC and POVCUT variables from the UNICON extract.

Poverty rate and shares under multiples of federal poverty threshold

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

A distributed lag version of the classic two-way (state and time) fixed effects regression specification is as follows:

$$I_{cit} = \sum_{k=-1}^{3} \alpha_{ck} \ln(MW_{s(i)t+k}) + X_{it}\Lambda_c + W_{s(i)t}\Psi_c + \mu_{cs(i)} + \theta_{ct} + \epsilon_{cit} \tag{1}$$

Here $\mu_{cs(i)}$ 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 cutoff c as a multiple of the federal poverty threshold. The coefficient $\alpha_c^{LR} = \alpha_{c0} + \alpha_{c1} + \alpha_{c2} + \alpha_{c3}$ is the long run 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. The coefficient $\alpha_c^{MR} = \alpha_{c0} + \alpha_{c1} + \alpha_{c2}$ is the "medium run" semi-elasticity inclusive of the effect up to two years after the policy change. These are converted to elasticities by dividing by the sample mean of the population share below the cutoff, $F(c)$. For example, $\beta_c^{LR} = \frac{1}{F(c)} \times (\alpha_{c0} + \alpha_{c1} + \alpha_{c2} + \alpha_{c3})$ is the long run elasticity. 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 (e.g., Wolfers 2006, Meer and West 2015). Explicit inclusion of up to 3 years of lags in minimum wages mitigates this problem.

The inclusion of a leading minimum wage term allows us to net out any level differences in the outcome between treated and control states just prior to treatment. As a consequence, the medium and log run elasticities do not reflect such differences, unlike in a static model. Additionally, the "leading value" ($\beta_{c,-1}$) provides us with a falsification test to discern the reliability of a research design. A statistically significant or sizable leading value, $\beta_{c,-1}$, indicates that the specification may not be able to account for a violation of the parallel trends assumption prior to treatment, and hence may provide misleading estimates.[9] To clarify, the $\beta_{\tau,-1}$ term in not included in β_c^{MR} or β_c^{LR}. At the same time, a $\beta_{\tau,-1} \neq 0$ indicates that the treated and control units had some deviation in trends in the past, which raises questions about whether there may be additional deviations in trends in the future. For this reason, I subject all the specifications to the leading value falsification test, and use this information as a criteria for model selection. In some cases, I also show the cumulative response with 3 years of leads and 3 years of lags in minimum wages to better evaluate the assumption of parallel trends right before the minimum wage increases.

All regressions control for 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), as well as 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 shocks in the state that are unlikely to be affected by the policy, given the small share of minimum wage workers in the workforce. 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 two-way fixed effects model is that there are many potential time varying confounders when it comes to the distribution of family incomes. 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 also estimate specifications that allow for arbitrary regional shocks 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.

Given the importance of the business cycle as a determinant of family incomes and movements

[9] For example, Dube et al. (2010) and Allegretto et al. (forthcoming) show that the two-way fixed effects model often fails this falsification test when it comes to minimum wage impact on teen and restaurant employment.

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. As another illustration, Zipperer (2016) shows that low wage employment was much more reliant on the construction sectors in states bound by the federal minimum wage increase during 2007-2009; as a result, these states suffered a much steeper relative decline in low wage employment during this period quite independent of minimum wage policy. For these reasons, 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.[10] 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.[11]

The most saturated specification is as follows:

$$I_{cit} = \sum_{k=-1}^{3} \alpha_{ck} \ln(MW_{s(i)t+k}) + X_{it}\Lambda_c + W_{s(i)t}\Psi_c + \mu_{cs(i)} + \theta_{cd(i)t} + \rho_{cr(t)s(i)} + \sigma_{s(i)}t + \epsilon_{cit} \qquad (2)$$

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 effects, and state-specific linear trends), and discuss the full range of estimates.[12] Additionally,

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

[11]An added concern raised by Neumark et al. (2014) is that recessionary periods can influence the estimation of state-specific trends. As Allegretto et al. (forthcoming) argue, this too can be handled by the inclusion of state-specific recession-year dummies. In studying minimum wage effects on welfare caseloads, Page et al. (2005) also use state-specific business cycle controls, although they interact the unemployment rate with state dummies.

[12]Using quadratic instead of linear trends in the saturated model produces similar results.

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 cutoffs. In the main tables, I report the impact of minimum wages on the shares below the following cutoffs: 0.5, 0.75, 1, 1.25, 1.5, and 1.75 times the federal poverty threshold. In the figures, I show the effects for each threshold between 0.5 and 3.5 times the poverty threshold in increments of 0.1. Note that 3.5 times the threshold exceeds the median income-to-needs ratio in the sample (3.04). Consequently, these estimates characterize the impact of the policy on the bottom half of the equivalized family income 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 semi-elasticities show the effect of a log point increase in the minimum wage on the cumulative distribution function (CDF) of family incomes. This is an example of *distribution regressions* as discussed in Chernozhukov et al. (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 1 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 occur were there to be 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 in the minimum wage context is of greater policy relevance 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 without a high school degree. However, when thinking about distributional effects of minimum wages, 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.[13] 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 marginal 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. (2011), we can instead invert the counterfactual distribution function using a local linear approximation. Figure 1 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 1. 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

[13] 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.

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 of taking a linear approximation to the counterfactual CDF 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 wages, 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 error.

To operationalize the estimation, following Firpo et al. (2009), I use as the dependent variable the recentered influence function of income. Since y is a multiple for the federal poverty threshold, for interpretational ease I rescale it by the average real value of the poverty threshold in my sample for the bottom 50% of the sample: $y'_{it} = \overline{FPT} \times y_{it} = \$22,476 \times y_{it}$. The RIF for the τ^{th} quantile, Q_τ, is as follows:

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

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 1.

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:

$$\begin{gathered} RIF(y'_{it}, Q_\tau) = \\ \sum_{k=-1}^{3} \gamma_{\tau,k} \ln(MW_{s(i),t+k}) + X_{it}\Lambda_\tau + W_{s(i)t}\Phi_\tau + \pi_{\tau s(i)} + \theta_{\tau d(i)t} + \sigma_{\tau s(i)}t + \rho_{\tau r(t)s(i)} + \epsilon_{\tau it} \end{gathered} \tag{4}$$

Here, $\gamma_\tau^{LR} = \sum_{k=0}^{3} \gamma_{\tau k}$ is the minimum wage long run semi-elasticity for the UQPE at the τ^{th}

quantile of equivalized family income. Note that $\gamma_\tau^{LR} = \frac{\overline{FPT}}{f(c(\tau))} \times \alpha_{c(\tau)}^{LR}$, 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 γ_τ^{LR} by $Q_\tau = c(\tau) \times \overline{FPT}$, so $\eta_\tau^{LR} = \frac{\gamma_\tau^{LR}}{c(\tau) \times \overline{FPT}}$. [14]

A number of features of the data make it attractive for the application of the RIF-UQPE approach. Table 2 and Figure B.2 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.[15] The linearity of the actual CDF (in combination with a continuous treatment) reduces the scope of the error from a linear approximation when inverting the counterfactual CDF using the RIF approach.

Additionally, Appendix Figure B.3 shows that the income quantiles at the bottom of the distribution have been fairly stationary over the past three decades, although they do exhibit cyclical tendencies. Appendix Figure B.3 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

[14] I find that the estimation error in $f(Q_\tau)$ contributes trivially to the overall variance of the η_τ^{LR}. Using the delta method, accounting for the estimation of the density around the cutoff increases the standard error of η_τ^{LR} typically by less than 0.1%. For this reason, the results in this paper do not explicitly adjust the standard errors for the estimation of $f(Q_\tau)$.

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

of both the treatment as well as the outcome facilitate the application of the RIF approach to a repeated cross-sectional setting.

2.3 Descriptive statistics

Table 1 shows shares of the non-elderly population, as well as eight key demographic groups under alternative income cutoffs, which range between 0.5 and 1.75 times the federal poverty threshold. To clarify, row 3 shows the poverty rates, while row 5 shows the share with income below 1.5 times the poverty threshold.

For non-elderly adults as a whole, the poverty rate averaged at 0.14 during the 1984-2013 period. Including elderly in the sample decreases it slightly to 0.13. Individuals without a high school degree (0.21) as well as those with high school or lesser schooling (0.18) had greater rates of poverty. The poverty rate for single mothers (0.22), black/Latino individuals (0.26), and children (0.20) were all higher than the average. Poverty rate among adults (0.11), on the other hand, is smaller than other groups. These patterns are as expected, and are qualitatively similar when we consider 50% or 150% of the poverty threshold.

3 Empirical findings

3.1 Main results for shares below multiples of poverty threshold

Table 3 provides the estimates for the impact of minimum wages on the shares below various multiples of the federal poverty threshold. For ease of interpretation, I report the estimates as elasticities β_c^{MR} and β_c^{LR} by dividing the sum of regression coefficients by the sample proportion under each cutoff; this is true both for the point estimate and the standard errors. I use eight different regression specifications that range from the classic 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-specific 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.

Overall, there is robust evidence that minimum wage increases reduce the share of individuals with low family incomes. For family income cutoffs between 0.50 and 1.25 (i.e., between 50 and 125 percent of the official poverty threshold), and across the eight specifications, all 32 of the long run estimates (sum of the contemporaneous plus one, two and three year lags in minimum wage) are negative in sign, and 28 are statistically significant at least at the 10 percent level. This includes all of the poverty rate elasticities, which are statistically significant at the 5 percent level. The medium run (sum of the coefficients for contemporaneous plus one and two years of lags) estimates tend to be qualitatively similar, and 31 or the 32 estimates are negative in sign. However in some cases (e.g., with the classic two-way fixed effects specification in column 1) the estimates are smaller in magnitude and/or less precise; consequently, 16 of the 32 estimates are statistically significant at conventional levels.

While all of the long run estimates are sizable, the more saturated specifications tend to produce estimates with somewhat larger magnitudes. For example, the long run poverty elasticity estimate of -0.220 from the two-way fixed effects estimate in column (1) is the smallest in magnitude among all eight specifications. In contrast, the most saturated specification in column (8) that controls for state-trends, division-period FE and state-specific recession year FE produces an elasticity of -0.533. Estimates from specifications with intermediate level of saturation are typically are somewhere in the middle.

The range of estimates raises the issue of model selection. There is an *a priori* case for using richer controls that better account for time-varying heterogeneity across states. Allowing for regional shocks and state-specific trends receives strong support in existing work. For example, Allegretto et al. (forthcoming) 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. One argument against using more saturated models is the loss of precision: that that they may lack the statistical power to detect an effect.[16] However, while the standard errors for the most saturated specifications 8 are larger, the 95% confidence intervals for that specification does not contain the point estimates using specification 1 for 75 and 100 percent

[16] 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 I address this issue by including three annual lags in minimum wage.

of the federal poverty threshold. In other words, there is strong indication that the differences in estimates across the two specifications are not driven primarily by the imprecision of the more saturated model.

Beyond this, I consider two types of falsification exercises for model selection. First, I use the leading values as a falsification test, analogous to tests used in Dube et al. (2010), Allegretto et al. (2011) and Allegretto et al. (2013). To be clear, the medium and long run estimates do not include the leading coefficient, so they net out any level differences in the outcome just prior to the minimum wage increase. However, since a non-zero leading coefficient provides an indication of the violation of parallel trends, such a test can help us assess the validity of particular specifications. The results are shown in Table 4. They indicate that specifications 1, 2, 5, 6 all produce positive leads for the proportion below one-half the poverty line, and these are statistically significant at the 5 percent level. Specification 1 also produces positive leads for 1.75 times poverty threshold. In contrast, specifications 3, 4 and 7 tend to produce negative leads for 0.75 times the poverty threshold. Considering the full range of cutoffs, the most saturated specification 8 usually performs the best when it comes to the leading values falsification test: it is the only specification where none of the leading coefficients are statistically significant; moreover, this is mostly driven be the small size of the coefficients and not from a greater imprecision. For example, the absolute value of the mean of all leads in specification 8 is 0.081, while the other specifications range between .083 and 0.191. Overall, this suggests the most saturated specification may be able to guard against pre-existing trends throughout the income distribution better than less saturated ones.

In addition, we should not expect minimum wages to affect the proportion earning under 3, 3.5 or 4 times the poverty threshold, which roughly corresponds to the 50th, 57th and 64th percentiles of the family income distribution in the national sample. Therefore, reliable specifications should produce estimates for these cutoffs that are small or close to zero. The bottom panel B of Table 4 reports the long run elasticities for shares below these middle and upper income cutoffs. In general, none of the specifications produce statistically significant long run effects, which is reassuring. However, the most saturated specification produces the smallest magnitudes: the absolute value of the mean for all cutoffs is 0.007 for specification 8, as compared to a range between 0.008 and 0.070 for the other specifications. Overall, the most saturated specification 8 performs very well on the falsification exercises, while the results from the intermediate specifications vary.[17]

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. At the same time, I recognize that reasonable observers may disagree on which specification is the most reliable, and 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. However, when I consider subgroup analysis or provide graphical evidence, I use the preferred, saturated specification for these purposes.

Figure 2 provides corresponding visual evidence on how minimum wages affect the bottom half of the family income distribution. The figure reports elasticities for each multiple of the poverty threshold between 0.5 and 3.5 in increments of 0.1—a total of 31 regressions—estimated using the preferred specification. The top panel shows that by the second year after the policy change, there are large, statistically significant reductions in the share below cutoffs between 0.5 and 1.2 times the poverty threshold with minimum wage elasticities exceeding -0.2 in magnitude. The elasticities for cutoffs exceeding 1.2 are mostly close to zero, and never statistically significant. The bottom panel of the figure shows that that long run (3+ year) minimum wage elasticities are sizable and statistically significant for cutoffs between roughly 0.6 and 1.4 times the poverty threshold. However, there is some additional reduction in the shares below cutoffs between 1.4 and 2 times the poverty threshold. The elasticities subsequently decline in magnitude, and are close to zero for cutoffs of 2.75 times the poverty threshold or greater. Overall, there is an indication that the medium run (2 year) effects are more concentrated at lower incomes than longer run effects. The lagged effects higher up in the family income distribution could be consistent with a lagged spillover effect of minimum wages on the wage distribution.

While the baseline specifications use 1 year of lead and 3 years of lags, to further assess the issue of parallel trends, I also estimate a longer distributed lag model with 3 leads and 3 lags for the preferred specification. Figure 3 reports the cumulative response elasticities by successively adding

[17] Additional results (not reported in the tables) also suggests that the saturated specification 8 is is more robust to inclusion of the state level controls. Excluding these controls produces a log run poverty elasticity (standard error) of -0.485 (0.166) for the saturated model, close to the original elasticity of -0.533 (0.130). In contrast, the two-way FE estimate is substantially smaller in magnitude when these controls are left out: -0.063(0.088) instead of -0.220 (0.084). To the extent unobservables are correlated with observable controls, these results suggests that more saturated specification is better able to account for them.

the leads and lags, normalizing the date -1 elasticity to be zero. The top panel shows the changes for the share below the federal poverty threshold: the share is stable in the 3 years prior to the policy change, but starts falling starting the year of the minimum wage increase, and reaches its maximum impact 2 years after the change. In contrast, the share below 3 times the federal poverty threshold shows no systematic movement: the cumulative response is close to zero for the three years prior to the policy change, and straddles zero subsequently. Overall, this evidence on timing from the longer distributed lag model provides additional validation for the preferred specification.

While the specifications in Table 3 include leads and lags in minimum wages, in much of the literature, estimates come from static models with just a contemporaneous minimum wage. To assess the difference this might make, in Table 5 I report estimates from a static model for the poverty rate. In every set of controls, the minimum wage elasticity from the static model is smaller in magnitude than either the medium or long run elasticities from the dynamic model. On average, the contemporaneous elasticity from the static model is less than half the size of the long run estimate. For example, the static two-way fixed effects specification (column 1) that is most commonly used in the literature produces an estimate of -0.074, not statistically distinguishable from zero. It also has a positive leading coefficient of 0.072, although not statistically significant. This subjects the static elasticity to a positive bias. Indeed, if we estimate a regression with a leading minimum wage in addition to the contemporaneous minimum wage, the magnitude of the minimum wage elasticity doubles to -0.144 with a standard error of 0.064, rendering it statistically significant (results not shown in the table). The addition of lags raise the magnitude further, with the long run estimate being -0.220. To summarize, the static two-way FE estimate appears to be biased towards zero both due to pre-existing trends, and due to missing some of the lagged effects of the policy.

What are the relative contributions of: (1) using a dynamic model, (2) including division-year effects, (3) including state-recession effects, and (4) including state-specific trends in explaining the gap between the the static, two-way fixed effects estimate (-0.074) and the long run estimate from the most saturated model (-0.533)? Any decomposition of these four factors will depend on the order in which they are implemented. There are exactly $4! = 24$ different orderings for incrementally changing each of these four factors going from the static version of specification 1 to the dynamic version of specification 8, and each of these orderings provides a different decomposition. Averaging across these 24 orderings, the average incremental contribution of these four factors (in the same order as above) are: (1) 45%, (2) 37%, (3) 21%, and (4) -3%. Use of the dynamic model (both netting out the lead, and considering long run effects) is the biggest source of difference, while controls for regional and business cycle heterogeneity are important as well. Use of state-specific trends appears to be the least important of these, at least when considering the poverty rate elasticity.

3.2 Effects for subgroups

Table 6 shows minimum wage elasticities for the shares below alternative family income cutoffs disaggregated by demographic groups using the preferred specification. For all groups, I find sizable reductions in the proportions under 50, 75, 100 and 125 percent of the poverty threshold. The long run estimates of 32 elasticities range between -0.294 and -1.066, and 26 are statistically significant at at least the 10 percent level. As compared to our previous estimates for non-elderly individuals (-0.533), the poverty rate elasticities are quite similar for children (-0.546), those without a high school degree (-0.542), those with high school or lesser schooling (-0.566), all individuals including elderly (-0.514), all adults (-0.476), and individuals under thirty without a high school degree (-0.481). In contrast, they are are somewhat smaller in magnitude for single mothers (-0.301), and substantially larger in magnitude for black and Latino individuals (-1.066). The reductions in low-income shares extend somewhat further up the distribution for black and Latino individuals, children under 18, and those under 30 without a high school degree; for these groups, 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 similar or somewhat greater; however, for another disadvantaged 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 Appendix Table A.1. First, if we take the poverty rate elasticities for groups under 20 years of age in the literature, Morgan and Kickham (-0.39) and Addison and Blackburn (average of -0.39 across specifications for teens) also find a substantial effect. While my estimate for all children are somewhat larger in magnitude (-0.546), both existing work and results in this paper point toward a substantial poverty reducing impact of minimum wages among children.

Second, for single mothers, I find elasticities for the proportion under the poverty line of -0.301, and under one-half poverty line of -0.294, which as noted above are somewhat smaller than the population overall; they are also imprecise and not statistically significant at conventional levels. The implied elasticities in Neumark (2016) for 21-44 year old single females with kids range between -0.14 and -0.30 depending on specification. These estimates are broadly similar to what I find, though I note the imprecision in both sets of estimates. 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, Gundersen and Ziliak (2004) finds very small effects for female headed households (-0.02). If we take an average of the poverty rate elasticities for single mothers (or female heads of households) across these five studies, we get an average elasticity of -0.18, as compared to my estimate of -0.30.

The third comparison concerns heterogeneity in the effect by levels of education. I find poverty elasticities for those without a high school degree (-0.481), or with high school or lesser schooling (-0.566) that are comparable to the overall estimate (-0.533). Neumark (2016) provides estimates for those 21 years or age or older by education: the unrestricted estimates range between -0.11 and -0.15, while for those with high school or lesser education, they range between -0.03 and -0.12; none of these are statistically significant. Sabia (2008) finds somewhat larger reductions in the poverty rate for single mothers without a high school degree (-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 (2008), though they continue to be sizable (changing from -0.42 to -0.35). Sabia and Nielsen (2015)'s estimates are highly imprecise and the impact of conditioning on education levels is contradictory across specifications. Finally, while Addison and Blackburn (1999) do not provide comparable estimates by levels of education, averages across their specifications do suggest a somewhat large elasticity (-0.43) for individuals who did not complete junior high school. 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. That is consistent with what I find here.

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. Gundersen and Ziliak (2004) also find a slightly larger effect in the black population—though the magnitude is still very small (-0.06). Finally, Sabia and Nielsen (2015)'s estimates are, again, imprecise and qualitatively differ by specification. This paper provides sharper evidence than available in existing work that minimum wages tend to raise bottom incomes more strongly for African Americans and Latinos.

3.3 Effect on family income quantiles

We can use the impact of minimum wages on the cumulative distribution of family income to estimate the impact on family income quantiles. Figure 4 shows the sample average CDF of family income, along with the counterfactual distribution. In the top scale of the figure, income is shown as multiples of the federal poverty threshold, y. The bottom scale shows income in dollar amounts obtained by multiplying y by the sample average of the real federal poverty threshold ($22,476). Here the counterfactual distribution at income c is calculated as $F(c) + \alpha_c^{LR}$, where $F(c)$ is the CDF averaged over the entire sample, and α_c^{LR} is the 3+ year minimum wage semi-elasticity estimated from equation (2). In other words, it is the counterfactual CDF with a log point increase in the minimum wage. The figure is based on 31 separate regressions estimated for cutoffs c between 0.5 and 3.5 in increments of 0.1 using the preferred specification. This is just a different way of reporting the estimates shown in Figure 2, which instead showed the impact as elasticities. In both figures, we can see that there is a statistically significant and quantitatively substantial shift in the CDF for incomes between 0.5 and 1.75 times the poverty threshold. Overall, an increase in the minimum wage seems to improve the family income distribution in the sense of first order stochastic dominance. By juxtaposing the actual and counterfactual CDF, we can also visually assess the impact by quantiles (the vertical axis). By considering the horizontal distance between the actual and counterfactual CDF's, we can infer that there is clear, statistically significant, increases in incomes between the 10th and 20th percentiles. And that above the 40th percentile, the changes (horizontal distance) are mostly close to zero.

This exercise illustrates the key idea behind estimating the unconditional quantile partial effects. In principle, we can estimate the quantiles by globally inverting the CDF as we did visually when inspecting Figure 4. This is similar to the approach in Chernozhukov et al. (2013). While feasible, this requires estimating a large number of regressions, even when estimating the effect for a single quantile. Therefore, the global inversion can be computationally demanding, especially as we wish to estimate it for many specifications with a large set of fixed effects and for different income definitions, and using individual level data. For this reason, I use the RIF approach proposed by Firpo et al. (2009). We approximate the horizontal distance between the actual and counterfactual CDFs at quantile q by dividing the vertical distance between the two by the PDF at $q = F^{-1}(c)$. This requires us to estimate only a single regression per quantile. The unconditional quantile partial effects (β_τ) are estimated using equation 3, or analogous regressions for the less saturated specifications. To convert the UQPE's into elasticities (η_τ), they are subsequently divided by the income-to-needs cutoffs corresponding to a given quantile.

Figure 5 shows the long-run minimum wage elasticities for each family income quantile between 5 and 50 using the preferred specification. We find substantial and statistically significant effects for quantiles between the 7th and 20th, declining sharply by the 35th quantile. The elasticities around the median of the family income distribution straddle zero. The figure provides compelling visual evidence that minimum wage policies tend to raise income at the bottom of the distribution, consistent with evidence on the CDF in Figure 4.

Table 7 presents these equivalized family income elasticities for quantiles ranging from 5 through 30, in increments of 5, and shows robust evidence across all 8 specifications that minimum wages lead to at least moderate increases in incomes for the bottom 20 percent of the equivalized family income distribution. Considering the long run estimates in the bottom panel, of the 32 estimates, all are positive in sign, and 24 are statistically significant at least at the 10 percent level. The 16 estimates for the 10th and the 15th quantiles range between 0.152 and 0.493, and all are statistically significant at least at the 10 percent level. As before, the two-way fixed effects specification (column 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, and the rescaling factors are common across specifications. For the preferred specification (column 8), I find long run elasticities of 0.275, 0.426, 0.407 and 0.170 for the 5th, 10th, 15th and 20th quantiles of equivalized family incomes, respectively. The 10th and 15th quantiles are statistically significant at least at the 5 percent level.

The medium run estimates sometimes tends to be smaller and/or less precise (e.g., specifications 1 and 2). However, there is an indication across many specifications that the longer run effects extend somewhat higher up the distribution.

We can also estimate minimum wage elasticities for quantiles of family income inclusive of tax credits and transfers. Table 8 shows the corresponding results using an expanded income definition that includes non-cash transfer and tax credits. These minimum wage elasticities are generally smaller than those using pre-tax cash income in Table 7—especially for longer run estimates. Averaging across all specifications, the medium run estimates for 10th and 15th quantiles are 85% as large when using expanded income definition as when using cash income only. The long run estimates for these quantiles using the expanded definition are only about 59% as large as when using pre-tax cash income. The finding is similar when I consider the preferred specification (column 8), where these elasticities are 77% and 62% as large from using the expanded income definition for the medium and long run, respectively. Figure 6 shows the elasticities visually for each quantile between 5 and 50 using the preferred specification, where we continue to find sizable and statistically significant effects between 7th and 20th quantiles. However, the elasticities using expanded income are about 59% as large as those in Figure 5 for those quantiles.

While the elasticities are useful in understanding how responsive income is to minimum wages for various quantiles, they are less useful for assessing how much of the cash income gain is being offset by a loss in tax credits or non-cash transfers. This is because the proportionate change in cash versus expanded incomes is also driven by the fact that the expanded incomes are larger: so a $1 increase in income due to minimum wages is a smaller share of expanded income, just because the $1 is divided by a larger denominator. For example, the 15th quantile in cash income is $24,123; when we expand the income definition to include non-cash transfers and tax credits, it is $27,423. Therefore, even if the income gains using the two definition are identical, the elasticity using the expanded income definition would be around 12% smaller.

To better capture the extent of offset, Table 9 reports the minimum wage effect on the level of real incomes (i.e., the semi-elasticities, γ_τ) for progressively more expansive income definitions. When we consider all 5 quantiles (5th, 10th, 15th, 20th, 25th and 30th), the increase in expanded income including tax credits and non-cash transfers is around 72% as large. About half of the loss is due to reduced non-cash transfers (column 3) while the other half is due to reduction in tax credits (column 5). However, there is an interesting difference in where in the income distribution these two losses occur. To see this more easily, I plot the semi-elasticities from the three income definitions in Figure 7. The loss due to reduced non-cash transfers is highly concentrated between the 14th and 16th quantiles, around or just above the federal poverty threshold. This is consistent with the nature of eligibility requirements of non-cash assistance programs: for instance, typically only individuals with income at or lower than 130% of the federal poverty threshold are eligible for SNAP. As a result, some individuals with incomes just below the eligibility threshold can lose the benefits due to higher earnings. Figure 7 provides compelling evidence on the importance of these offsets: at the 15th quantile, there is a 40% offset to the income gain due to loss in public assistance, mostly due to reduced non-cash transfers. In contrast, at 10th or 20th quantiles, the offset in income gains due to loss in public assistance was only around 15%. Moreover, while the loss in non-cash transfers is more relatively concentrated around the 15th quantile, the loss due to tax credits is more uniform across the income distribution. This likely reflects the fact that the EITC phase-out range is more diffuse. The findings on offsets is also consistent with simulation results from Maag and Carasso (2013) who tend to find that the effective marginal tax rate (after accounting for transfers) on earnings is often quite high between 100 and 150% of the federal poverty threshold.[18]

Finally, Table 9 also shows that the offsets in public assistance appear to be larger in the long run. This could potentially reflect a behavioral response in utilization: for example, higher incomes could reduce SNAP take-up rate and not the eligibility rate, and changes in take-up behavior can take longer to adjust than the mechanical effects of changes in eligibility.

While use of the thirty years of data provides us with greater precision, one may wonder whether the patterns are similar in more recent times. In particular, when it comes to tax credits and transfers, EITC expansion of 1992 and welfare reform in 1996 are important, and may interact with the minimum wage policy. Moreover, there is evidence that target efficiency of minimum wages has improved over time (Lundstrom, 2016). For these reasons, in Table 10, I show the income

[18]That the income gains at the 15th quantile is 40% smaller when using an expanded income definition does not necessarily imply that an individual at the 15th quantile of the cash incomes experiences a 40% offset due to lost public assistance. Individuals at these quantiles need not be the same (a) before versus after the policy change (b) when considering different types of incomes. We would need to make a rank-invariance assumption to assure that our findings about quantiles can be interpreted as reflecting impacts on individuals. As an empirical matter, ranks in the conventional cash income and the expanded incomes are highly correlated. For those in the bottom half of the cash income distribution, the correlation coefficient between the two ranks is 0.99. However, this does not tell us whether the policy affects the rank of individuals in the two distributions.

elasticities by quantiles for cash income and expanded income including tax-credits and transfers for subsamples beginning in 1990 as well as 1996. The estimates are quite similar to those from the full sample. For the sample beginning in 1996, the long run elasticities for the 10th and 15th quantiles when considering cash incomes are 0.390 and 0.443, respectively, and both are statistically significant at the 5% level. For comparison, these elasticities for the full sample are 0.426 and 0.407, respectively (Table 7). When using the expanded income definition, the elasticities for these two quantiles are 0.291 and 0.218 in the 1996-2013 sample, as compared to 0.300 and 0.216 in the full sample. Estimates from the sample beginning in 1990 are similar. As expected, the estimates for the smaller samples are generally less precise, but the magnitudes are quite similar.

4 Discussion

There is robust evidence that minimum wage increases over the past 30 years have boosted pre-tax-and-transfer incomes at the bottom of the income distribution. Across specifications with alternative assumptions about the appropriate counterfactual, I find that minimum wages reduce the shares with family incomes below 50, 75, 100 and 125 percent of the federal poverty threshold. The long run poverty rate elasticities range between -0.220 and -0.552, and all of these are statistically significant. I also find that a higher minimum wage improves the family income distribution in the sense of first order stochastic dominance. The clearest increases are for the 10th and 15th quantiles, with elasticities ranging between 0.152 and 0.493 across alternative models. While these results hold across a range of specifications, the preferred specification with a rich set of controls suggest an elasticity of around 0.426 for the 10th quantile of family incomes. At the same time, I also find evidence that there are sizable offsets in public assistance in response to minimum wage gains. For those in the bottom fifth of the distribution, around 30% of the income gains are offset through reductions in non-cash-transfers as well as tax credits. These offsets seem to be the largest near the federal poverty threshold—consistent with losses in eligibility in programs such as SNAP.

What do these estimates imply about the likely impact on poverty from an increase in the federal minimum wage from the current $7.25/hour to, say, $12/hour? Taking into account the state minimum wages as of January 2017 suggests that this policy would raise the effective minimum wage—i.e., the maximum of federal or state standard—by 41 percent (or 0.34 log points). Using the long run minimum wage elasticity for the poverty rate of -0.53 from the preferred specification and a 13.5 percent poverty rate among the non-elderly population in 2016 suggests a 2.45 percentage point reduction in the poverty rate from this minimum wage increase. Given the roughly 270 million non-elderly Americans in 2016, this translates into 6.6 million fewer individuals living in poverty. We can also expect the same minimum wage increase to raise family incomes by 14.5 percent at the 10th quantile of the equivalized family income distribution in the long run. For the 10th quantile, this translates into an annual income increase of $2,538; after accounting for the offset due to reduced tax credits and transfers, this amounts to an increase of $2,140. [19] Therefore, the increase in the federal minimum wage can play an important role in reducing poverty and raising family incomes at the bottom. To put this in context, Hoynes et al. (2006) estimate that EITC reduces non-elderly poverty rate by around 1.7 percent, and cash transfers (means tested and non-means tested) cash transfers reduce it by around 3.8 percentage point, while non-cash transfers (other than Medicaid) reduce it by around 0.9 percentage point. In other words, a substantial increase in the minimum wage would likely have an impact on the non-elderly poverty rate comparable to means tested public assistance programs.

The income estimates in this paper do not factor in changes in prices due to minimum wage increase. However, the expected increase in the overall price level from this policy is quite small when compared to the income gains for those at the bottom of the income distribution. For example, in a simulation study using input-output tables, MaCurdy (2015) calculated that the bottom quintile faced a 0.5 percent increase in prices from the 21% increase in minimum wage in 1996—an elasticity of around 0.02. The preferred long run elasticity for the 10th quantile after public offsets is around 0.300, or more than an order of magnitude larger. Therefore, netting out the likely price increase would not substantially affect the estimates of the real income gains for the bottom quintile of the income distribution. However, it does mean that some (possibly much) of these income gains at the bottom are likely borne by middle and upper income consumers through small increases in prices.

The estimates in this paper tend to suggest bigger increases in bottom incomes in response to

[19] If we take the range of estimates from all specifications, the proposed minimum wage changes can be expected to reduce the poverty rate among the non-elderly population by 1.00 and 2.53 percentage points, hence reducing the number of non-elderly individuals living in poverty by somewhere between 2.7 and 6.8 million. For the 10th quantile of family incomes, this translates to an annual income increase ranging between 5.2 and 16.8 percent, or between $905 and $2,937. After accounting for offsets due to lost public assistance, the income increases would range between $657 and $2,790.

minimum wage increases than some simulation based studies such as Sabia and Burkhauser (2010) or CBO (2014), or MaCurdy (2015). 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. Most obviously, we would need to make assumptions about how behavior changes: this includes employer hiring and firing behavior, as well as workers' job search behavior and labor force participation, which could vary by family income and other characteristics. In addition, simulations such as 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.[20] 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 et al. 2011). Moreover, as

[20]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.

Autor et al. (2016) show, effects of the minimum wage extend up to the 20th percentile of the wage distribution, which would be unlikely absent some spillovers.[21] Therefore, results from simulation studies 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.

While minimum wages tend to raise family incomes at the bottom, they also tend to substitute earnings for public assistance. Were we to assess public policies strictly based on their efficacy in reducing post-tax and transfer poverty, the offsets through reduced tax credits and non-cash transfers further suggests a lower effectiveness of minimum wages in raising post-tax-and-transfer incomes. Policies like cash transfers, SNAP, and tax credits are better targeted to raise incomes for those at the very bottom of the income distribution. As many researchers, including Card and Krueger (1995), have pointed out, the minimum wage is a somewhat blunt tool when it comes to fighting poverty. In comparison, the EITC is better targeted at those with very low incomes.[22]

At the same time, there are positive aspects of a policy that reduces public assistance via increased earnings. First, a reduction in public benefits like SNAP can be efficiency enhancing, since in principle these programs are funded using taxation which can have deadweight losses. Hendren (2014) finds that that lower pre-tax incomes at the bottom has an efficiency cost due to such transfers: a $1 of surplus accruing to the bottom quintile of the income distribution can be transformed into a $1.15 surplus for everyone. A corollary to that argument is that by reducing such transfers, minimum wages can have an efficiency-enhancing attribute.[23] Additionally, there is increasing recognition that individuals may not see labor earnings and tax credits/transfers as perfect substitutes, and may prefer to receive a higher compensation for their work than what is often perceived as a government transfer. Relatedly, voters may prefer "pre-distributive" policies like minimum wages over "re-distributive" ones using tax and transfers even when they are concerned

[21] 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.

[22] 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 may call for combining tax and transfers like the EITC with a minimum wage (Lee and Saez 2012).

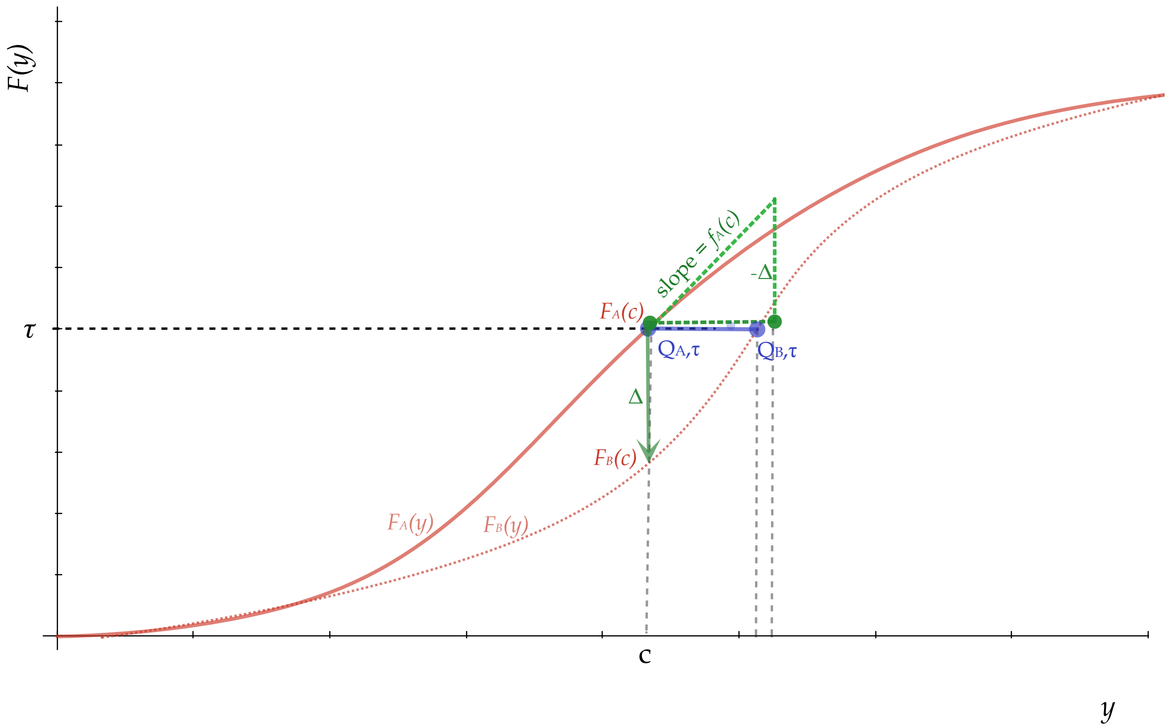
[23] Alternatively, the public savings afforded by a higher minimum wage could be used to expand program generosity, further enhancing income at the bottom.

with inequality—in part because of low levels of trust in government (Kuziemko et al. (2015)). All of these factors add to the cost of pre-tax inequality and raises the benefits of a policy like minimum wage that affects the pre-tax-and-transfer income distribution.

Overall, motivations of voters and policy makers in raising minimum wage policies tend to 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 et al. 1986). The findings from this paper suggest that attaining these goals is also consistent with at least a moderate increase in family incomes, and a reduced reliance on public assistance for those at the bottom of the income distribution.

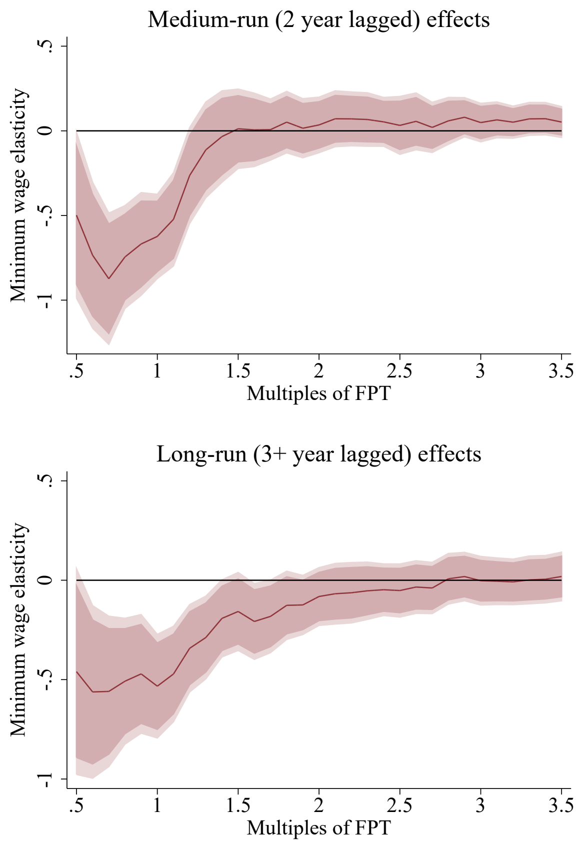
Figures and Tables

Figure 1: Unconditional quantile partial effects: locally inverting the counterfactual distribution



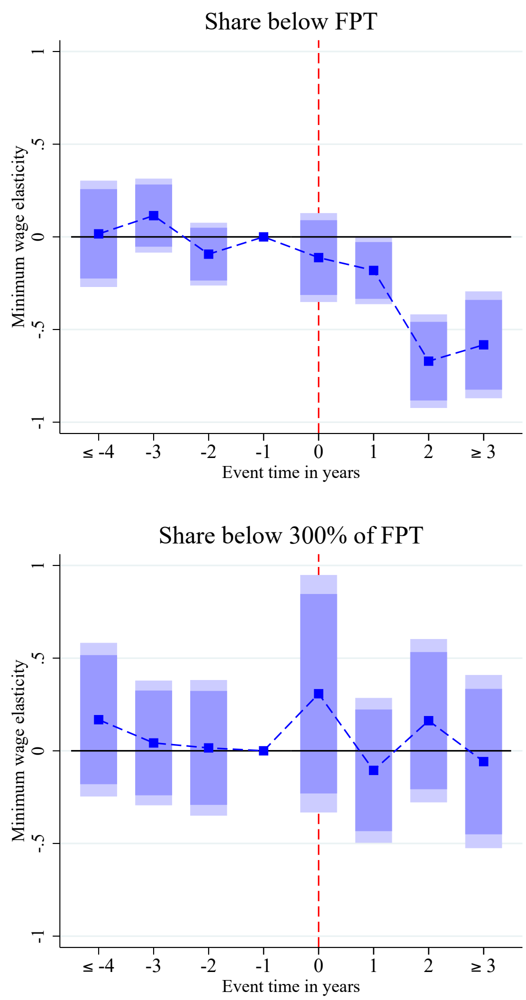
Notes. The figure shows how the unconditional quantile partial effect (UQPE) is approximately estimated for a treatment such as a small increase in the minimum wage. $F_A(y)$ represents the actual distribution of outcome y, while $F_B(y)$ is the counterfactual distribution with a higher level of treatment. 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 2: Minimum wage elasticities for share under alternative family incomes in multiples of federal poverty threshold



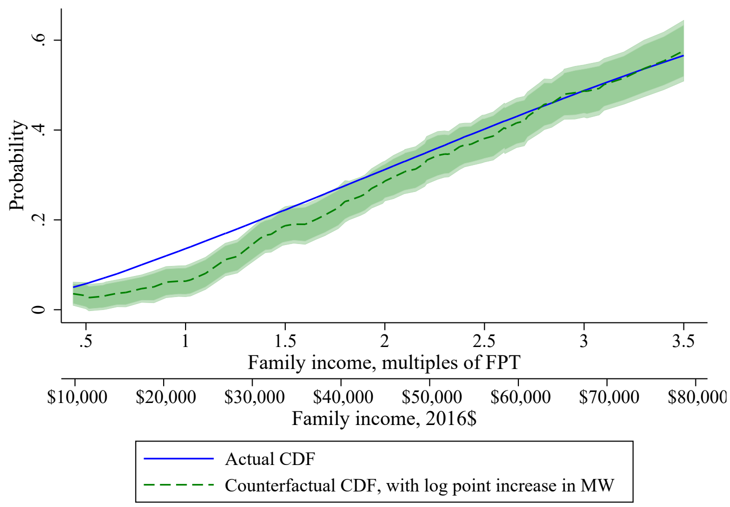
Notes. The reported estimates are long run minimum wage elasticities for share with family income under various multiples of the federal poverty threshold. These estimates are from a series of linear probability models that are estimated by regressing an indicator for having family income below cutoffs between 0.5 and 3.5 times the federal poverty threshold (in increments of 0.05) on distributed lags of log minimum wage and covariates. The medium-run (2 year), and long-run (3+ year) elasticities are calculated by dividing the respective coefficients (or sums of coefficients) by the sample proportion under the cutoff. All specifications include state fixed effects, division-by-year fixed effects, state specific linear trends, state specific indicator for each recession year, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, martial status, family size, number of children, education level, Hispanic status, gender), and are weighted by March CPS person weights. Dark shaded area represents 90%, light shaded area 95% state-cluster-robust confidence intervals.

Figure 3: Cumulative response of share below poverty threshold, and three times poverty threshold to a log point increase in minimum wages



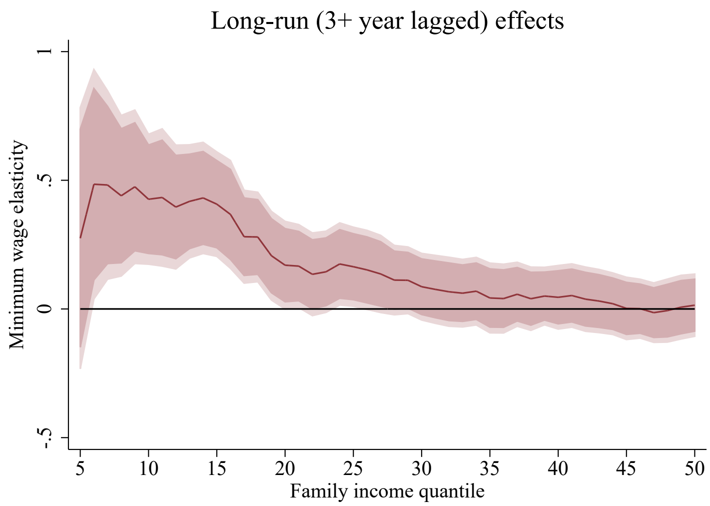
Notes. The reported estimates are minimum wage elasticities for share with family income below 1 or 3 times the federal poverty threshold, by event date. The elasticities are from linear probability models estimated by regressing an indicator for having family income below cutoffs of 1 and 3 times the federal poverty threshold on covariates and a distributed lags window including up to 3 leads and lags of log minimum wage. The elasticity at event date -1 is normalized to 0. The remaining elasticities are calculated by summing up the joint effect and divide it by the sample proportion under the cutoff. Specifications include state fixed effects, division-by-year effects, state specific linear trends, state specific indicator for each recession year, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, martial status, family size, number of children, education level, Hispanic status, gender).. Regressions are weighted by March CPS person weights. Dark shaded area represents 90%, light shaded area 95% state-cluster-robust confidence intervals.

Figure 4: Actual and counterfactual cumulative distribution functions of family income in multiples of federal poverty threshold



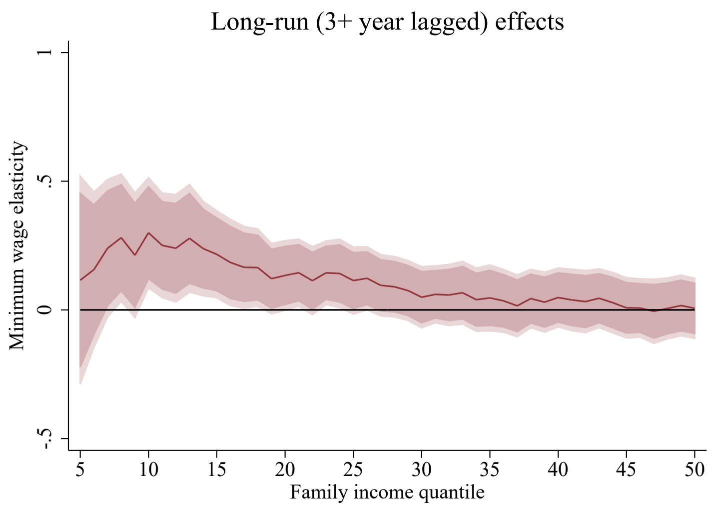
Notes. The solid (blue) line shows the actual cumulative distribution function of family income, estimated as the sample average over the full period. The dashed (green) line shows the counterfactual CDF of family income with a log point increase in the minimum wage. The counterfactual is calculated by adding to the actual CDF the regression-based long-run effects of minimum wage on the share below cutoffs. These long run estimates come from a series of linear probability models that are estimated by regressing an indicator for having a family income below cutoffs (between 0.40 and 3.50) on distributed lags of log minimum wage and covariates. The long-run estimates are calculated as the sum of the contemporaneous up to three-year lagged log minimum wage coefficients. All specifications include state fixed effects, division-by-year fixed effects, state specific linear trends, state specific indicator for each recession year, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, martial status, family size, number of children, education level, Hispanic status, gender), and are weighted by March CPS person weights. Dark shaded area represents 90%, light shaded area 95% state-cluster-robust confidence intervals. The additional x-axis shows the average family income values in 2016 dollars corresponding to the multiples of federal poverty threshold.

Figure 5: Minimum wage elasticities for unconditional family income quantiles



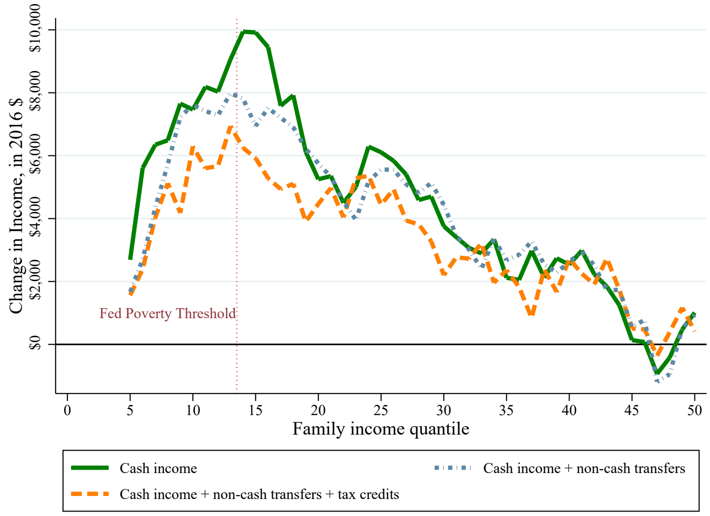
Notes. The reported estimates are long run minimum wage elasticities by unconditional quantiles of family income. The estimates are from a series of linear probability models that are estimated by regressing an indicator for having a family family income below cutoffs associated with alternative quantiles (between 5 and 50) on distributed lags of log minimum wage and covariates. The long-run coefficients are calculated as the sum of the contemporaneous up to three-year lagged log minimum wage coefficients. Unconditional quantile partial effects (UQPE) are calculated by dividing the coefficient on log minimum wage by the negative of the family income density at the appropriate quantile. The UQPE estimates are subsequently divided by the family income cutoff for the quantile to transform the estimates into elasticities. All specifications include state and year fixed effects, division-by-period fixed effects, state specific linear trends, state specific indicator for each recession year, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, martial status, family size, number of children, education level, Hispanic status, gender), and are weighted by March CPS person weights. Dark shaded area represents 90%, light shaded area 95% state-cluster-robust confidence intervals.

Figure 6: Minimum wage elasticities for unconditional family income quantiles; expanded income with tax credits and non-cash transfers



Notes. The reported estimates are long run minimum wage elasticities by unconditional quantiles of an expanded definition of family income which includes tax credits (EITC, child tax credit) and non-cash transfers (SNAP, NSLP, housing subsidy). The estimates are from a series of linear probability models that are estimated by regressing an indicator for having an expanded family family income below cutoffs associated with alternative quantiles (between 5 and 50) on distributed lags of log minimum wage and covariates. The long-run coefficients are calculated as the sum of the contemporaneous up to three-year lagged log minimum wage coefficients. Unconditional quantile partial effects (UQPE) are calculated by dividing the coefficient on log minimum wage by the negative of the family income density at the appropriate quantile. The UQPE estimates are subsequently divided by the expanded income cutoff for the quantile to transform the estimates into elasticities. All specifications include state and year fixed effects, division-by-period fixed effects, state specific linear trends, state specific indicator for each recession year, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, martial status, family size, number of children, education level, Hispanic status, gender), and are weighted by March CPS person weights. Dark shaded area represents 90%, light shaded area 95% state-cluster-robust confidence intervals.

Figure 7: Minimum wage semi-elasticities for unconditional quantiles of family incomes, for alternative income definitions



Notes. The reported estimates are semi-elasticities, showing the increase in income (in 2016 $) from a log point increase in the minimum wage. The solid line is for the change in cash income, the dash-dot line augments it by adding non-cash transfers (SNAP, NSLP and housing subsidies) to the income definition, and the dashed line is for the expanded income that includes both non-cash transfers and tax credits (EITC and child tax credits). The estimates are from a series of linear probability models that are estimated by regressing an indicator for having family income below cutoffs associated with alternative quantiles (between 5 and 50) on distributed lags of log minimum wage and covariates. The long-run coefficients are calculated from the sum of the contemporaneous up to three-year lagged log minimum wage coefficients. Unconditional quantile partial effects (UQPE) are calculated by dividing the coefficient on log minimum wage by the negative of the family income density at the appropriate quantile. The UQPE estimates are subsequently multiplied by the average cash income in 2016 dollars corresponding to the federal poverty threshold to transform the estimates into changes in income. All specifications include state and year fixed effects, division-by-period fixed effects, state specific linear trends, state specific indicator for each recession year, state-level covariates (GDP per capita, EITC supplement, unemployment rate), and individual demographic controls (quartic in age, as well as dummies for race, martial status, family size, number of children, education level, Hispanic status, gender), and are weighted by March CPS person weights. The vertical dotted line shows the quantile corresponding to the sample averaged federal poverty threshold.

Table 1: Population shares below multiples of federal poverty threshold - by demographic group

Family income (multiples of FPT)	Age<65	All, including elderly (65+)	<HS, under 30	<HS	$\leq$ HS	Black and Latino	Children	Adults	Single mothers
0.50	0.058	0.054	0.091	0.091	0.076	0.114	0.085	0.047	0.100
0.75	0.095	0.090	0.148	0.152	0.126	0.188	0.139	0.077	0.163
1.00	0.136	0.132	0.206	0.214	0.179	0.263	0.195	0.112	0.224
1.25	0.178	0.178	0.264	0.276	0.233	0.335	0.250	0.148	0.284
1.50	0.222	0.225	0.322	0.337	0.289	0.405	0.305	0.188	0.342
1.75	0.267	0.273	0.377	0.395	0.344	0.469	0.360	0.229	0.398
Observations	4,662,781	5,238,862	1,646,917	1,947,133	3,019,445	1,211,366	1,500,559	3,162,220	965,820

Notes. Each cell contains the proportion of the sample (or the mean of a poverty measure) under the family income cutoff. The sample consists of all non-elderly individuals in March CPS surveys for years between 1984 and 2013. All columns use the implicit equivalence scale used to calculate the official poverty rate. Columns 1 through 9 report the proportions for the following subsamples: non-elderly; all individuals, including elderly; individuals younger than 30 without a high school degree; all individuals without a high school degree; individuals with high school or lesser education; black and Latino individuals; children under 18; adults; and single mothers. All calculations use the March CPS person weights. All calculations use the March CPS person weights.

Table 2: Quantiles and densities of family income in multiples of federal poverty threshold

Quantile	Family income (multiples of FPT)	Family income (in 2016 $)	Density
Panel A: Cash Income			
5	0.436	$9,789	0.120
10	0.779	$17,509	0.160
15	1.083	$24,350	0.168
20	1.374	$30,873	0.176
25	1.656	$37,215	0.179
30	1.933	$43,443	0.180
Panel B: Cash Income + Non-cash transfers + Tax credits			
5	0.601	$13,516	0.118
10	0.934	$20,990	0.166
15	1.221	$27,436	0.183
20	1.483	$33,333	0.196
25	1.735	$38,988	0.198
30	1.990	$44,727	0.193

Notes. Family income quantiles, and kernel densities at cutoffs associated with the quantiles, are estimated for the nonelderly population using March CPS data from 1984-2013 and person weights. Income definition in Panel A only includes the cash income; whereas income definition in Panel B includes non-cash transfers (SNAP, NSLP and housing subsidy) and tax credits (EITC and child tax credits) as well. Kernel density estimates use an Epanechnikov kernel and the STATA default bandwidth based on Silverman's rule-of-thumb.

Table 3: Minimum wage elasticities for share of individuals with family income below multiples of federal poverty threshold

Family income (multiples of FPT)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Medium-run (2 year lagged) estimates								
0.50	-0.272	-0.299*	-0.558**	-0.458*	0.044	-0.090	-0.610**	-0.498**
	(0.176)	(0.166)	(0.234)	(0.228)	(0.238)	(0.212)	(0.256)	(0.244)
0.75	-0.147	-0.141	-0.446***	-0.383**	-0.221	-0.331**	-0.872***	-0.826***
	(0.124)	(0.122)	(0.158)	(0.154)	(0.144)	(0.146)	(0.172)	(0.170)
1.00	-0.161	-0.145	-0.302**	-0.274**	-0.303**	-0.307**	-0.682***	-0.624***
	(0.101)	(0.099)	(0.129)	(0.124)	(0.129)	(0.130)	(0.155)	(0.124)
1.25	-0.062	-0.051	-0.115	-0.116	-0.133	-0.056	-0.258	-0.193
	(0.089)	(0.087)	(0.118)	(0.110)	(0.155)	(0.135)	(0.163)	(0.128)
1.50	0.017	0.022	-0.004	-0.015	0.003	0.102	-0.039	0.012
	(0.074)	(0.071)	(0.103)	(0.101)	(0.139)	(0.120)	(0.135)	(0.117)
1.75	-0.035	-0.035	-0.004	-0.033	-0.035	0.056	-0.027	-0.011
	(0.057)	(0.054)	(0.084)	(0.081)	(0.111)	(0.090)	(0.107)	(0.090)
Panel B: Long-run (3+ year lagged) estimates								
0.50	-0.219	-0.506***	-0.599**	-0.443	-0.186	-0.470***	-0.670***	-0.460*
	(0.137)	(0.134)	(0.295)	(0.283)	(0.124)	(0.127)	(0.235)	(0.258)
0.75	-0.186**	-0.243***	-0.407**	-0.327*	-0.277***	-0.372***	-0.652***	-0.558***
	(0.092)	(0.084)	(0.189)	(0.187)	(0.089)	(0.093)	(0.173)	(0.174)
1.00	-0.220**	-0.227***	-0.310**	-0.296**	-0.309***	-0.337***	-0.552***	-0.533***
	(0.084)	(0.077)	(0.143)	(0.147)	(0.071)	(0.074)	(0.135)	(0.130)
1.25	-0.188***	-0.235***	-0.164	-0.213*	-0.243***	-0.298***	-0.293**	-0.344***
	(0.068)	(0.063)	(0.109)	(0.109)	(0.076)	(0.070)	(0.116)	(0.109)
1.50	-0.071	-0.152**	-0.048	-0.112	-0.091	-0.156**	-0.099	-0.157
	(0.064)	(0.061)	(0.087)	(0.088)	(0.067)	(0.070)	(0.093)	(0.098)
1.75	-0.067	-0.162***	-0.028	-0.122*	-0.088	-0.166**	-0.078	-0.174*
	(0.063)	(0.054)	(0.076)	(0.073)	(0.063)	(0.064)	(0.091)	(0.089)
Observations	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781
Division-period FE			Y	Y			Y	Y
State trends		Y		Y		Y		Y
State-recession FE					Y	Y	Y	Y

Notes. The reported estimates are minimum wage elasticities for share with family income under various multiples of the federal poverty threshold. These estimates are from linear probability models that regress an indicator for having family income below multiples (between 0.50 and 1.75) of the federal poverty threshold on distributed lags of log minimum wage and covariates. The medium-run and long-run elasticities are calculated by summing the contemporaneous up to two- and three-year lagged log minimum wage coefficients, respectively, and then dividing by the sample proportion under the family income 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 division-by-year fixed effects, state specific linear trends and state specific indicators for each recession year are indicated in the table. State-cluster-robust standard errors in parentheses.

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

Table 4: Leading estimates and upper income falsification tests: share of individuals with family income below multiples of federal poverty threshold

Family income (multiples of FPT)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 1-year leading estimates								
0.50	0.496***	0.367***	-0.093	0.003	0.469***	0.388***	-0.056	0.066
	(0.151)	(0.115)	(0.137)	(0.147)	(0.147)	(0.121)	(0.144)	(0.153)
0.75	0.041	-0.011	-0.298***	-0.262**	0.132	0.113	-0.215**	-0.166
	(0.098)	(0.116)	(0.090)	(0.107)	(0.104)	(0.106)	(0.104)	(0.116)
1.00	0.072	0.033	-0.134*	-0.142	0.162*	0.130	-0.053	-0.058
	(0.084)	(0.097)	(0.072)	(0.085)	(0.092)	(0.095)	(0.078)	(0.088)
1.25	0.070	0.009	-0.095	-0.128	0.111	0.026	-0.067	-0.111
	(0.085)	(0.090)	(0.080)	(0.087)	(0.097)	(0.094)	(0.098)	(0.100)
1.50	0.097	0.022	-0.082	-0.122*	0.108	0.008	-0.078	-0.126
	(0.081)	(0.081)	(0.069)	(0.070)	(0.097)	(0.095)	(0.089)	(0.087)
1.75	0.161**	0.077	-0.022	-0.081	0.163*	0.055	-0.030	-0.093
	(0.074)	(0.073)	(0.061)	(0.059)	(0.081)	(0.078)	(0.078)	(0.072)
Panel B: Long-run (3+ year lagged) estimates of upper income								
3.00	0.055	-0.020	0.071	0.038	0.037	-0.040	0.017	-0.002
	(0.038)	(0.038)	(0.051)	(0.053)	(0.038)	(0.036)	(0.053)	(0.061)
3.50	0.055	0.007	0.069	0.052	0.027	-0.024	0.026	0.019
	(0.038)	(0.037)	(0.052)	(0.050)	(0.038)	(0.039)	(0.057)	(0.061)
4.00	0.022	-0.016	0.069	0.048	-0.007	-0.048	0.013	0.005
	(0.031)	(0.030)	(0.043)	(0.039)	(0.029)	(0.034)	(0.049)	(0.049)
Observations	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781
Division-period FE			Y	Y			Y	Y
State trends		Y		Y		Y		Y
State-recession FE					Y	Y	Y	Y

Notes. The reported estimates are either leading minimum wage elasticities (Panel A) or long run minimum wage elasticities (panel B) for share with family income under various multiples of the federal poverty threshold. These estimates are from linear probability models that regress an indicator for having family income below multiples (between 0.50 and 1.75) of the federal poverty threshold on distributed lags of log minimum wage and covariates. The leading elasticities are calculated by dividing the one-year leading minimum wage coefficients by the sample proportion under the family income cutoff. The long-run elasticities are calculated by dividing the sum of the contemporaneous up to three-year lagged log minimum wage coefficients by the sample proportion under the family income 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 division-by-year fixed effects, state specific linear trends and state specific indicators for each recession year are indicated in the table. State-cluster-robust standard errors in parentheses.

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

Table 5: Comparing minimum wage elasticities for the poverty rate: static and dynamic specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Static Specification:								
MW elasticity	-0.074	-0.069	-0.250**	-0.196**	-0.066	-0.057	-0.291***	-0.214**
	(0.062)	(0.058)	(0.095)	(0.094)	(0.068)	(0.070)	(0.092)	(0.101)
Dynamic specification:								
1 year leading MW elasticity	0.072	0.033	-0.134*	-0.142	0.162*	0.130	-0.053	-0.058
	(0.084)	(0.097)	(0.072)	(0.085)	(0.092)	(0.095)	(0.078)	(0.088)
2 year lagged MW elasticity	-0.161	-0.145	-0.302**	-0.274**	-0.303**	-0.307**	-0.682***	-0.624***
	(0.101)	(0.099)	(0.129)	(0.124)	(0.129)	(0.130)	(0.155)	(0.124)
3+ year lagged MW elasticity	-0.220**	-0.227***	-0.310**	-0.296**	-0.309***	-0.337***	-0.552***	-0.533***
	(0.084)	(0.077)	(0.143)	(0.147)	(0.071)	(0.074)	(0.135)	(0.130)
Observations	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781
Division-period FE			Y	Y			Y	Y
State trends		Y		Y		Y		Y
State-recession FE					Y	Y	Y	Y

Notes. The reported estimates are minimum wage elasticities for share with family income under the federal poverty threshold. The estimates are from linear probability models where the outcome is an indicator for having a family income below the federal poverty threshold. The static specification includes only the contemporaneous log minimum wage. The elasticity is calculated by dividing the log minimum wage coefficient by the sample proportion under the family income cutoff. The dynamic specification includes a distributed lags window of one lead and up to three lags of log minimum wage. The leading, medium-run, and long-run elasticities are calculated from the leading log minimum wage coefficient, the sum of the contemporaneous up to two-year lagged log minimum wage coefficients, and the sum of the contemporaneous up to three-year lagged log minimum wage coefficients, respectively, and then 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 division-by-year fixed effects, state specific linear trends and state specific indicators for each recession year are indicated in the table. State-cluster-robust standard errors in parentheses.

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

Table 6: Minimum wage elasticities for share of individuals with family income below multiples of federal poverty threshold by demographic subgroup

Family income (multiples of FPT)	All, including elderly (65+)	<HS, under 30	<HS	≤HS	Black and Latino	Children	Adults	Single mothers
Panel A: Medium-run (2 year lagged) estimates								
0.50	-0.458*	-0.494	-0.710**	-0.643**	-1.109**	-0.427	-0.447*	-0.521
	(0.237)	(0.298)	(0.282)	(0.248)	(0.498)	(0.274)	(0.246)	(0.465)
0.75	-0.780***	-0.880***	-1.017***	-0.986***	-1.386***	-0.840***	-0.724***	-0.831***
	(0.171)	(0.220)	(0.204)	(0.172)	(0.292)	(0.213)	(0.155)	(0.270)
1.00	-0.538***	-0.607***	-0.738***	-0.672***	-1.089***	-0.534***	-0.604***	-0.549**
	(0.115)	(0.155)	(0.140)	(0.131)	(0.178)	(0.147)	(0.128)	(0.216)
1.25	-0.161	-0.203	-0.266**	-0.205	-0.698***	-0.102	-0.181	-0.176
	(0.111)	(0.149)	(0.129)	(0.132)	(0.143)	(0.152)	(0.126)	(0.191)
1.50	0.066	-0.055	-0.055	-0.032	-0.379**	0.007	0.075	0.048
	(0.094)	(0.158)	(0.141)	(0.129)	(0.153)	(0.157)	(0.105)	(0.207)
1.75	0.038	-0.038	-0.002	-0.037	-0.354**	0.018	0.019	-0.035
	(0.078)	(0.118)	(0.110)	(0.106)	(0.140)	(0.120)	(0.088)	(0.142)
Panel B: Long-run (3+ year lagged) estimates								
0.50	-0.477*	-0.411	-0.477	-0.548*	-0.894**	-0.462	-0.389*	-0.294
	(0.247)	(0.317)	(0.308)	(0.288)	(0.354)	(0.327)	(0.222)	(0.403)
0.75	-0.564***	-0.470**	-0.533***	-0.615***	-1.045***	-0.518**	-0.527***	-0.326
	(0.166)	(0.207)	(0.192)	(0.190)	(0.243)	(0.225)	(0.169)	(0.270)
1.00	-0.514***	-0.481***	-0.542***	-0.566***	-1.066***	-0.546***	-0.476***	-0.301
	(0.124)	(0.154)	(0.131)	(0.147)	(0.151)	(0.181)	(0.126)	(0.210)
1.25	-0.333***	-0.350***	-0.348***	-0.320**	-0.915***	-0.385**	-0.279**	-0.293*
	(0.106)	(0.126)	(0.114)	(0.121)	(0.116)	(0.145)	(0.121)	(0.162)
1.50	-0.121	-0.218*	-0.171	-0.149	-0.685***	-0.269*	-0.053	-0.114
	(0.098)	(0.124)	(0.119)	(0.121)	(0.096)	(0.138)	(0.103)	(0.139)
1.75	-0.110	-0.207*	-0.140	-0.166	-0.649***	-0.228*	-0.116	-0.171
	(0.096)	(0.109)	(0.107)	(0.109)	(0.099)	(0.114)	(0.100)	(0.120)
Observations	5,238,862	1,646,917	1,947,133	3,019,445	1,211,366	1,500,559	3,162,220	965,820

Notes. The reported estimates are minimum wage elasticities for share with family income under various multiples of the federal poverty threshold. These estimates are from linear probability models that regress an indicator for having family income below multiples (between 0.50 and 1.75) of the federal poverty threshold on distributed lags of log minimum wage and covariates. The medium-run and long-run elasticities are calculated by summing the contemporaneous up to two- and three-year lagged log minimum wage coefficients, respectively, and then dividing by the sample proportion under the family income cutoff. The regression specification includes state fixed effects, division-specific year effects, state-specific indicator for each recession year, 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. <HS refers to individuals without a high school degree, while <HS under 30 further restricts to individuals under 30 years of age. ≤HS refers to individuals with high school or lesser schooling. State-cluster-robust standard errors in parentheses.

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

Table 7: Minimum wage elasticities for unconditional quantiles of family incomes

Family income quantile	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Medium-run (2 year lagged) estimates								
5	0.092	0.116	0.339	0.259	-0.066	-0.050	0.400*	0.345
	(0.173)	(0.161)	(0.221)	(0.221)	(0.207)	(0.192)	(0.225)	(0.215)
10	0.122	0.114	0.348***	0.303**	0.188*	0.241**	0.703***	0.648***
	(0.092)	(0.090)	(0.118)	(0.115)	(0.110)	(0.116)	(0.132)	(0.127)
15	0.123	0.108	0.244**	0.231**	0.230*	0.207	0.507***	0.459***
	(0.085)	(0.082)	(0.105)	(0.100)	(0.131)	(0.131)	(0.135)	(0.113)
20	0.005	-0.003	0.014	0.017	0.047	-0.037	0.089	0.032
	(0.070)	(0.067)	(0.094)	(0.090)	(0.123)	(0.102)	(0.135)	(0.113)
25	0.005	0.005	0.003	0.027	-0.014	-0.098	0.018	-0.004
	(0.052)	(0.052)	(0.075)	(0.071)	(0.097)	(0.078)	(0.093)	(0.076)
30	-0.002	0.002	-0.071	-0.046	0.048	-0.027	-0.014	-0.035
	(0.045)	(0.040)	(0.061)	(0.057)	(0.097)	(0.082)	(0.085)	(0.070)
Panel B: Long-run (3+ year lagged) estimates								
5	0.024	0.314***	0.357	0.244	0.026	0.271**	0.413*	0.275
	(0.119)	(0.110)	(0.268)	(0.269)	(0.124)	(0.118)	(0.232)	(0.253)
10	0.152**	0.183***	0.295**	0.246*	0.219***	0.272***	0.493***	0.426***
	(0.071)	(0.063)	(0.142)	(0.136)	(0.065)	(0.071)	(0.125)	(0.126)
15	0.158**	0.158**	0.216**	0.224*	0.227***	0.243***	0.396***	0.407***
	(0.064)	(0.060)	(0.106)	(0.112)	(0.057)	(0.059)	(0.103)	(0.101)
20	0.099*	0.154***	0.061	0.106	0.133**	0.179***	0.130	0.170*
	(0.057)	(0.052)	(0.080)	(0.079)	(0.061)	(0.058)	(0.087)	(0.085)
25	0.065	0.149***	0.045	0.124*	0.077	0.149***	0.082	0.164**
	(0.048)	(0.045)	(0.071)	(0.067)	(0.049)	(0.049)	(0.078)	(0.076)
30	0.024	0.132***	-0.022	0.061	0.042	0.128**	0.008	0.086
	(0.050)	(0.042)	(0.054)	(0.052)	(0.055)	(0.053)	(0.061)	(0.064)
Observations	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781
Division-period FE			Y	Y			Y	Y
State trends		Y		Y		Y		Y
State-recession FE					Y	Y	Y	Y

Notes. The reported estimates are minimum wage elasticities for unconditional quantiles of equivalized family income. These estimates are from linear probability models that regress an indicator for having family income below cutoff associated with a quantile (between 5 and 30) on distributed lags of log minimum wage and covariates. Medium and long run unconditional quantile partial effects (UQPE) for family incomes are calculated by summing the contemporaneous up to two- and three-year lagged log minimum wage coefficients, respectively, and then dividing by the negative of the family income density at the appropriate quantile. The UQPE estimates are subsequently divided by the family income 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 division-by-year fixed effects, state specific linear trends and state specific indicators for each recession year are indicated in the table. State-cluster-robust standard errors in parentheses.

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

Table 8: Minimum wage elasticities for unconditional quantiles of expanded family incomes with tax credits and non-cash transfers

Family income quantile	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Medium-run (2 year lagged) estimates								
5	0.010	0.019	0.111	0.094	0.242	0.081	0.268	0.195
	(0.100)	(0.097)	(0.139)	(0.146)	(0.170)	(0.152)	(0.232)	(0.218)
10	0.144*	0.138*	0.321***	0.276***	0.241***	0.255***	0.629***	0.549***
	(0.083)	(0.082)	(0.102)	(0.097)	(0.090)	(0.086)	(0.127)	(0.115)
15	0.062	0.055	0.135	0.111	0.224**	0.154	0.394***	0.316***
	(0.073)	(0.070)	(0.086)	(0.085)	(0.106)	(0.104)	(0.102)	(0.082)
20	0.010	0.005	0.027	0.027	0.068	-0.002	0.099	0.054
	(0.058)	(0.055)	(0.075)	(0.075)	(0.101)	(0.084)	(0.104)	(0.084)
25	0.018	0.018	-0.006	0.015	0.035	-0.054	0.027	-0.009
	(0.047)	(0.041)	(0.065)	(0.062)	(0.100)	(0.085)	(0.082)	(0.065)
30	-0.001	0.002	-0.065	-0.044	0.039	-0.037	-0.027	-0.052
	(0.043)	(0.037)	(0.059)	(0.057)	(0.091)	(0.078)	(0.079)	(0.062)
Panel B: Long-run (3+ year lagged) estimates								
5	-0.012	0.167	0.095	0.114	0.060	0.167	0.144	0.115
	(0.097)	(0.103)	(0.167)	(0.188)	(0.100)	(0.103)	(0.176)	(0.201)
10	0.092	0.128	0.245**	0.196*	0.131	0.168**	0.377***	0.300***
	(0.089)	(0.081)	(0.118)	(0.114)	(0.082)	(0.073)	(0.111)	(0.106)
15	0.046	0.082*	0.096	0.077	0.121***	0.145***	0.251***	0.216**
	(0.051)	(0.048)	(0.086)	(0.099)	(0.045)	(0.043)	(0.084)	(0.084)
20	0.045	0.101**	0.044	0.077	0.086**	0.132***	0.110	0.134*
	(0.044)	(0.042)	(0.068)	(0.074)	(0.040)	(0.044)	(0.068)	(0.067)
25	0.016	0.096**	-0.015	0.060	0.046	0.106**	0.041	0.114*
	(0.043)	(0.039)	(0.064)	(0.059)	(0.046)	(0.047)	(0.068)	(0.065)
30	0.023	0.120***	-0.028	0.044	0.038	0.111**	-0.018	0.049
	(0.044)	(0.037)	(0.050)	(0.050)	(0.047)	(0.048)	(0.056)	(0.059)
Observations	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781
Division-period FE			Y	Y			Y	Y
State trends		Y		Y		Y		Y
State-recession FE					Y	Y	Y	Y

Notes. The reported estimates are minimum wage elasticities for unconditional quantiles of equivalized family income using an expanded income defintion. Expanded income includes tax credits (EITC, child tax credit) and non-cash transfers (SNAP, NSLP, housing subsidy). The estimates are from linear probability models that regress an indicator for having an expanded family income below cutoff associated with a quantile (between 5 and 30) on distributed lags of log minimum wage and covariates. Medium and long run unconditional quantile partial effects (UQPE) for family incomes are calculated by summing the contemporaneous up to two- and three-year lagged log minimum wage coefficients, respectively, and then dividing by the negative of the family income density at the appropriate quantile. The UQPE estimates are subsequently divided by the family income 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 division-by-year fixed effects, state specific linear trends and state specific indicators for each recession year are indicated in the table. State-cluster-robust standard errors in parentheses.

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

Table 9: Minimum wage effects on unconditional quantiles of family incomes, for alternative income definitions

Income:	Cash Income		+ Non-cash transfers		+ Non-cash transfers + Tax credits	
Family income quantile	Semi-Elasticity	Elasticity	Semi-Elasticity	Elasticity	Semi-Elasticity	Elasticity
Panel A: Medium-run (2 year lagged) estimates						
5	\$3,381	0.345	\$2,720	0.215	\$2,636	0.195
	(2,108)	(0.215)	(2,791)	(0.221)	(2,942)	(0.218)
10	\$11,347***	0.648***	\$10,308***	0.531***	\$11,515***	0.549***
	(2,221)	(0.127)	(2,425)	(0.125)	(2,404)	(0.115)
15	\$11,166***	0.459***	\$7,680***	0.301***	\$8,678***	0.316***
	(2,753)	(0.113)	(2,410)	(0.094)	(2,259)	(0.082)
20	\$981	0.032	\$1,991	0.063	\$1,784	0.054
	(3,478)	(0.113)	(3,041)	(0.096)	(2,800)	(0.084)
25	-\$141	-0.004	\$183	0.005	-\$347	-0.009
	(2,824)	(0.076)	(2,854)	(0.076)	(2,545)	(0.065)
30	-\$1,514	-0.035	-\$1,374	-0.031	-\$2,314	-0.052
	(3,037)	(0.070)	(3,039)	(0.069)	(2,793)	(0.062)
Panel B: Long-run (3+ year lagged) estimates						
5	\$2,690	0.275	\$1,670	0.132	\$1,560	0.115
	(2,472)	(0.253)	(2,345)	(0.185)	(2,717)	(0.201)
10	\$7,464***	0.426***	\$7,615***	0.392***	\$6,295***	0.300***
	(2,199)	(0.126)	(2,347)	(0.121)	(2,235)	(0.106)
15	\$9,916***	0.407***	\$6,940***	0.272***	\$5,921**	0.216**
	(2,460)	(0.101)	(2,198)	(0.086)	(2,309)	(0.084)
20	\$5,251*	0.170*	\$5,752**	0.182**	\$4,465*	0.134*
	(2,615)	(0.085)	(2,359)	(0.075)	(2,240)	(0.067)
25	\$6,110**	0.164**	\$5,558*	0.148*	\$4,443*	0.114*
	(2,844)	(0.076)	(2,788)	(0.074)	(2,528)	(0.065)
30	\$3,757	0.086	\$4,434	0.101	\$2,202	0.049
	(2,798)	(0.064)	(2,751)	(0.063)	(2,647)	(0.059)
Observations	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781	4,662,781

Notes. The reported estimates are minimum wage elasticities for unconditional quantiles of equivalized family income using alternative income defintions. The estimates are from linear probability models that regress an indicator for having a family income below cutoff associated with a quantile (between 5 and 30) on distributed lags of log minimum wage and covariates. The first two columns only consider pre-tax cash income, columns 3 and 4 augment it by adding non-cash transfers (SNAP, NLSP and housing subsidies), and the last two columns further add tax credits (EITC and child tax credits) to the family income definition. Medium and long run unconditional quantile partial effects (UQPE, or semi-elasticities) for family incomes are calculated by summing of the contemporaneous up to two- and three-year lagged log minimum wage coefficients, respectively, and dividing by the negative of the family income density at the appropriate quantile. To calculate elasticities, the UQPE estimates are subsequently divided by the family income cutoff for the quantile. The regression specification includes state fixed effects, division-specific year effects, state-specific indicator for each recession year, 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 10: Minimum wage elasticities for unconditional quantiles of family incomes for samples starting in 1990 and 1996

	1990				1996			
	Cash Income		+ Non-cash transfers + Tax credits		Cash Income		+ Non-cash transfers + Tax credits	
Family income quantile	Medium-run	Long-run	Medium-run	Long-run	Medium-run	Long-run	Medium-run	Long-run
5	0.330	0.121	0.295	0.162	0.250	-0.247	0.181	0.042
	(0.228)	(0.258)	(0.217)	(0.191)	(0.292)	(0.321)	(0.248)	(0.258)
10	0.671***	0.415***	0.581***	0.297**	0.649***	0.390**	0.573***	0.291*
	(0.135)	(0.137)	(0.122)	(0.114)	(0.156)	(0.191)	(0.141)	(0.152)
15	0.481***	0.446***	0.300***	0.223**	0.446***	0.443**	0.221*	0.218*
	(0.127)	(0.127)	(0.090)	(0.099)	(0.148)	(0.170)	(0.116)	(0.129)
20	0.036	0.231**	0.062	0.164**	-0.042	0.187	-0.023	0.138
	(0.111)	(0.100)	(0.083)	(0.081)	(0.110)	(0.119)	(0.085)	(0.088)
25	-0.033	0.143	-0.024	0.120	-0.046	0.103	-0.021	0.095
	(0.088)	(0.090)	(0.074)	(0.077)	(0.084)	(0.106)	(0.071)	(0.088)
30	-0.061	0.082	-0.070	0.046	-0.100	0.025	-0.118*	0.030
	(0.083)	(0.074)	(0.071)	(0.070)	(0.072)	(0.089)	(0.065)	(0.074)
Observations	3,836,210	3,836,210	3,836,210	3,836,210	3,021,984	3,021,984	3,021,984	3,021,984

Notes. The reported estimates are minimum wage elasticities for unconditional quantiles of equivalized family income using alternative income defintions. The estimates are from linear probability models that regress an indicator for having a family income below cutoff associated with a quantile (between 5 and 30) on distributed lags of log minimum wage and covariates. The first four column starts the estimation samples from 1990, and the last from 1996. Columns 1, 2, 5, 6 only consider cash income; and columns 3, 4, 7, 8 includes non-cash transfers (SNAP, NLSP and housing subsidies) and tax credits (EITC and child tax credits). Medium and long run unconditional quantile partial effects (UQPE) for family incomes are calculated by summing of the contemporaneous up to two- and three-year lagged log minimum wage coefficients, respectively, and dividing by the negative of the family income density at the appropriate quantile. The UQPE estimates are subsequently divided by the family income cutoff for the quantile to transform the estimates into elasticities. The regression specification includes state fixed effects, division-specific year effects, state-specific indicator for each recession year, 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.

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

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Online Appendix A: Assessing the existing research on minimum wages, family incomes and poverty (not for publication)

In this online appendix, 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.[24] 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 14 studies, 13 of which are used in my quantitative summary. I note that a recent book by Belman and Wolfson (2014) provides a review of many of the same papers. Finally, I note that seven of these 14 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 A.1 reports the key estimates from the 13 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.[25] 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

[24] I do not include Page et al. (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, especially the reductions in non-cash transfers. However, as they point out, their estimates seem to vary based on the sample period. Moreover, since cash income 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.

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

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 A.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 Addison and Blackburn (1999). Overall, these guidelines lead me to report 78 elasticities in Table A.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. Finally, besides the poverty rate, the table also reports 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 3.2.

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.[26] 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

[26]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.

0.20 (with controls); these are statistically significant at conventional levels.[27] A major limitation of 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 et al. 1994).

Addison and Blackburn (1999) consider teens, young adults, and junior high dropouts between 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 -0.21). 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.[28] Additionally, their teen results are somewhat sensitive to the inclusion of state trends, as shown in Table A.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.[29] Gundersen 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,

[27] 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 et al. 2004), a problem which does affect numerous other papers in the literature as described in the text.

[28] The standard errors of the long-run estimates are derived from the reported p-values.

[29] 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.

namely raising wages at the lower end of the wage distribution.[30] 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.[31] 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 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 (2015) 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. The long-run point estimate is

[30] 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, Gundersen and Ziliak (2004) limit their sample to families with some positive income (not necessarily earnings).

[31] 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).

negative (-0.245) with an even larger 95% confidence interval (-1.21, 0.71).[32] 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 rule out moderate sized poverty rate elasticities.

Sabia (2014) responds to an earlier version of this paper, and reports estimates for 16-64 year olds using state-year aggregated data from 1979-2012. He estimates a static two-way fixed effects model, as well as a static model with state-specific trends, division-period effects, and state-specific recession effects: these controls are similar to what I use in this paper (and in the previous version). His estimates for the static two-way fixed effects model is 0.06, while from the more saturated model is -0.16; neither is statistically significant. His exclusion of children is one likely reason his estimates are smaller in magnitude than what I find. But more importantly, the fact that the elasticity from the static two-way fixed effects model is small is similar to what I show in Table 5; not accounting for leading effects produces a positive bias, especially for the two-way fixed effects model.

David Neumark has authored three papers (two with coauthors) 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

[32]Standard error of the long-run estimate is calculated by assuming the 1- and 2-year lagged minimum wages are independent, as they do not report it in the paper.

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.[33]

Neumark et al. (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 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).[34] 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.[35] 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.[36] These non-standard techniques raise serious questions about the study, especially since

[33] 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.

[34] 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 A.1.

[35] 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.

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

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 (2016) uses a more conventional approach to study the effect of minimum wages over the 1997-2006 period; this is a follow up to an earlier paper original paper Neumark and Wascher (2011), which used a similar methodology to study minimum wage and EITC effects on 21-44 year olds.[37] He also reports 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, he includes state and year fixed effects. He also includes demographic and state-level controls similar to this paper, and reports estimate with and without state-specific trends.

For the original group studied in Neumark and Wascher (2011) considered—21-44 year old family heads or individuals—his results suggest a poverty rate elasticity of -0.22 without state-trends, and -0.35 with trends. For a group constituting the majority of non-elderly adults, the evidence from Neumark (2016) suggests that minimum wages have a moderate-sized impact in reducing poverty and extreme poverty. When he adds older individuals (but not children), i.e., those 21 or older, the estimates are -0.15 and -0.11, respectively. For single mothers, the estimates are -0.30 and -0.14, respectively, without and with state trends. Most of the estimates are not statistically significant; however, it is important to recall that these estimates are from a relatively short 9 year period. Importantly, however, these results are sharply different from the findings in Neumark et al. (2005), and much more similar to rest of the literature.

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? Figure A.1 plots a histogram of all 72 elasticities. First, I note that across these 13 studies, nearly all (72) of the 78 estimates of the poverty rate elasticity are negative in sign. Table A.2 reports that the simple average of elasticities across all these 78 estimates is -0.18. If we take the median estimate from each study, the simple average of these median estimates is -0.17. Indeed, only one

[37]Neumark (2016) follows up on an original paper Neumark and Wascher (2011), which mostly focused on the interaction of EITC and minimum wages on on wage and employment effects. However, in Neumark and Wascher (2011), 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. In an earlier draft of this paper, I had backed out the implied minimum wage elasticities implied in Neumark and Wascher (2011). In response, Neumark (2016) went back and directly estimated and reported these estimates.

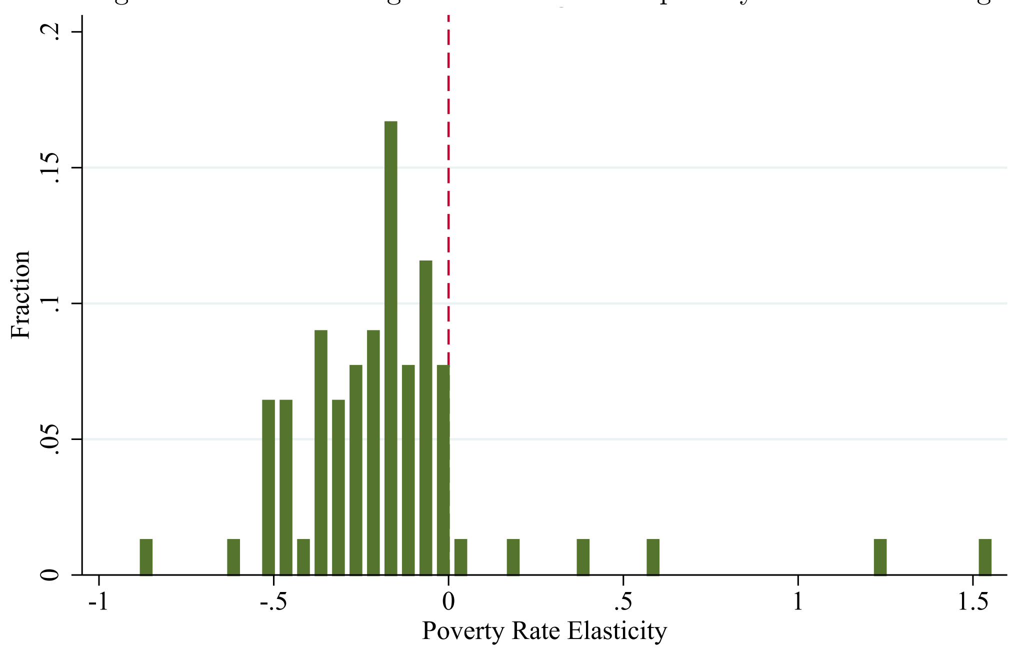
study by Neumark et al. (2005) suggests that minimum wages actually increase the overall poverty rate. As I discussed above, this study uses an unconventional methodology that is both different from all other studies, and makes problematic distributional assumptions.

Second, if we take the average of the poverty rate elasticities for the overall population across the eight studies that provide such an estimate, we obtain an average poverty rate elasticity of -0.13.[38] There are, of course, other ways of aggregating estimates across studies. 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.

[38]These eight studies are: Card and Krueger (1995), Stevans and Sessions (2001), Gundersen and Ziliak (2004), Neumark et al. (2005), Burkhauser and Sabia (2007), Sabia and Burkhauser (2010), and Sabia and Nielsen (2015), and Sabia (2014). 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.

Figure A.1: Histogram of minimum wage elasticities for the poverty rate from existing estimates



Notes: The histogram plots all 78 existing estimates of the minimum wage elasticity for the poverty rate in the existing literature, as summarized in Table A.1. All the estimates are expressed as elasticities. . The vertical red dash line indicates a poverty rate elasticity of zero.

Table A.1: Minimum wage elasticities for the poverty rate from the existing literature

Study & Sample	Poverty Rate Elasticity	Other Elasticity	Data	Specification
Addison & Blackburn (1999)				
Ages 16 - 19	-0.50 (0.22)		1983-1996 March CPS; S-Y	S,Y FE; GLS
Ages 16 - 19	-0.50 (0.22)			S,Y FE; LM; GLS
Ages 16 - 19	-0.61 (0.28)			S, Y FE; LM; PA Pov
Ages 16 - 19	-0.39 (0.22)			S,Y FE; PA Pov; GLS
Ages 16 - 19	-0.39 (0.22)			S,Y FE; LM; PA Pov; GLS
Ages 16 - 19	-0.17 (0.28)			S,Y FE; S-tr; LM; PA Pov; GLS
Ages 16 - 19	-0.33 (0.23)			S,Y FE; LM; PA Pov; GLS; Lag-1 year
Ages 20 - 24	-0.28 (0.22)			S,Y FE; 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
Ages 20 - 24	-0.22 (0.22)			S,Y FE; LM; PA Pov; GLS
Ages 20 - 24	-0.22 (0.22)			S,Y FE; PA Pov; GLS
Ages 20 - 24	-0.28 (0.22)			S,Y FE; S-tr; LM; PA Pov; GLS
Ages 20 - 24	-0.06 (0.34)			S,Y FE; LM; PA Pov; GLS; Lag-1
Age $>$ 24, Ed $<$ 10 yrs	-0.50 (0.15)			S,Y FE; 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
Age $>$ 24, Ed $<$ 10 yrs	-0.46 (0.15)			S,Y FE; PA Pov; GLS
Age $>$ 24, Ed $<$ 10 yrs	-0.46 (0.15)			S,Y FE; LM; PA Pov; GLS
Age $>$ 24, Ed $<$ 10 yrs	-0.46 (0.15)			S,Y FE; S-tr; LM; PA Pov; GLS
Age $>$ 24, Ed $<$ 10 yrs	-0.46 (0.18)			S,Y FE; LM; PA Pov; GSL; Lag-1 year
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

Table A.1: Minimum wage elasticities for the poverty rate from the existing literature (continued)

Study & Sample	Poverty Rate Elasticity	Other Elasticity	Data	Specification
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
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)		1987-1996 March CPS; Ind	S,Y Scale Sh.
Neumark (2016)				
Ages 21-44	-0.22 (0.15)	Earnings: -0.27 (0.12)	1997-2006 March CPS; Ind.	S,Y FE; LM
Ages 21-44	-0.35 (0.22)	Earnings: -0.29 (0.13)		S,Y FE; LM; S-tr
Age $\geq$ 21	-0.15 (0.13)	Earnings: -0.04 (0.04)		S,Y FE; LM
Age $\geq$ 21	-0.11 (0.14)	Earnings: -0.06 (0.08)		S,Y FE; LM; S-tr
Age $\geq$ 21, w/kids	-0.18 (0.14)			S,Y FE; LM
Age $\geq$ 21, w/kids	-0.21 (0.22)			S,Y FE; LM; S-tr
Age $\geq$ 21, $\leq$ HS	-0.19 (0.17)			S,Y FE; LM
Age $\geq$ 21, $\leq$ HS	-0.01 (0.22)			S,Y FE; LM; S-tr
Age $\geq$ 21, Bl/Hisp.	-0.15 (0.12)			S,Y FE; LM
Age $\geq$ 21, Bl/Hisp.	-0.12 (0.16)			S,Y FE; LM; S-tr
SF, Age $\geq$ 21, w/kids	-0.30 (0.11)			S,Y FE; LM
SF, Age $\geq$ 21, w/kids	-0.14 (0.24)			S,Y FE; LM; S-tr
SF, Age $\geq$ 21, $\leq$ HS	-0.12 (0.14)			S,Y FE; LM
SF, Age $\geq$ 21, $\leq$ HS	-0.03 (0.154)			S,Y FE; LM; S-tr
SF, Age $\geq$ 21, Bl/Hisp.	-0.07 (0.137)			S,Y FE; LM
SF, Age $\geq$ 21, Bl/Hisp.	-0.26 (0.182)			S,Y FE; LM; S-tr

Table A.1: Minimum wage elasticities for the poverty rate from the existing literature (continued)

Study & Sample	Poverty Rate Elasticity	Other Elasticity	Data	Specification
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
Sabia (2014)				
Ages 16-64	0.05 (0.07)		1980-2013 March CPS; S-Y	S,Y FE; LM
Ages 16-64	-0.16 (0.14)			S,Y FE; LM; Div-per
Fem HH, w/kids	-0.13 (0.20)			S,Y FE; LM; Div-per
Sabia & Burkhauser (2010)				
Ages 16-64	-0.05 (0.15)		2003-2007 March CPS; S-Y	S,Y FE; LM
Sabia & Nielsen (2015)				
Ages 16-64	-0.31 (0.31)	Post-tax: -0.21 (0.28)	1996-2007 SIPP; Ind	S,Y FE
Ages 16-64	-0.03 (0.12)	Post-tax: -0.06 (0.14)		S,Y FE; S-tr
Ages 16-64	-0.25 (0.48)	Post-tax: -0.20 (0.53)		S,Y FE; Lag-2 years
Ages 16-29,$<$ HS	-0.52 (0.63)	Post-tax: -0.60 (0.63)		S,Y FE
Ages 16-29, $<$ HS	1.21 (0.63)	Post-tax: 1.04 (0.53)		S,Y FE; S-tr
Ages 16-29, $<$ HS	-0.89 (1.83)	Post-tax: -1.10 (1.79)		S,Y FE; Lag-2 years
Ages 16-24, Bl	0.60 (0.72)	Post-tax: -0.90 (0.82)		S,Y FE
Ages 16-24, Bl	-0.46 (0.63)	Post-tax: -0.65 (0.70)		S,Y FE; S-tr
Ages 16-24, Bl	1.52 (1.18)	Post-tax: 1.88 (1.33)		S,Y FE; Lag-2 years
Ages 30-54, $\geq$ HS	-0.18 (0.18)	Post-tax: -0.21 (0.21)		S,Y FE
Ages 30-54, $\geq$ HS	0.18 (0.18)	Post-tax: -0.10 (0.21)		S,Y FE; S-tr
Ages 30-54, $\geq$ HS	-0.05 (0.15)	Post-tax: 0.46 (0.147)		S,Y FE; Lag-2 years
Stevans & Sessions (2001)				
All	-0.28 (0.17)		1984-1998 March CPS; S-Y	S,Y FE

Notes. All estimates are expressed as elasticities, with standard errors in parentheses when available. 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; Lag-X year = Specification includes up to X years of lags of minimum wage.

Table A.2: Averages of minimum wage elasticities for the poverty rate from the existing literature

Study & Sample	Poverty Rate Elasticity
Averages: Every group	
All 13 studies:	-0.18
Averages: Overall population	
All 8 studies:	-0.13
Average of medians	
All 13 studies:	-0.17

Notes. The table reports various averages of the minimum wage elasticities for the poverty rate as summarized in the literature review. "Averages: Every group" is a simple average across all 78 estimated elasticities from 13 studies, either for every demographic group, or just for the overall population (defined as 16-64 year old or broader). "Averages: Overall population" is a simple average across estimated elasticities for the overall population (defined as 16-64 year old or broader) reported in 8 studies. "Average of medians" is a simple average of the median elasticities from each of the 13 studies.

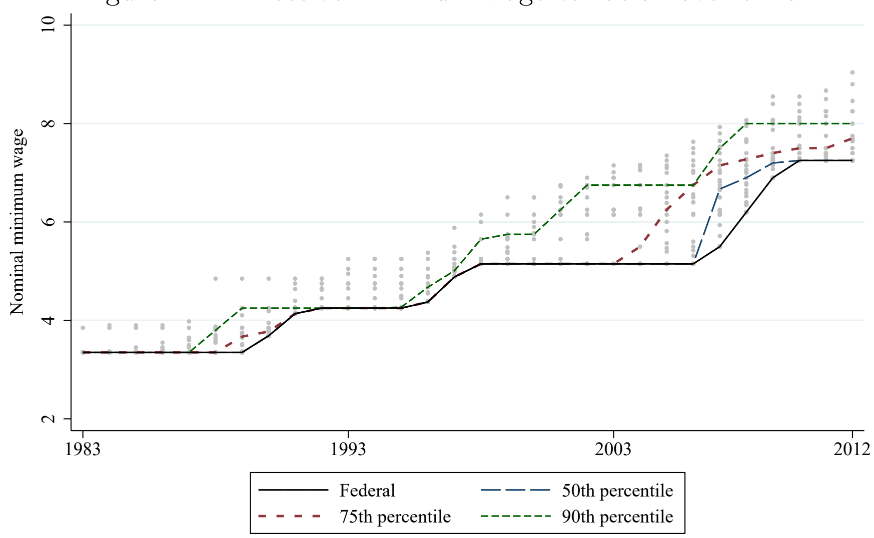
Online Appendix B: Additional figures and tables (not for publication)

This appendix contains additional figures and tables that are referenced in the text.

Appendix Figure B.1 shows the variation in effective (maximum of state or federal) minimum wage over time between 1984 and 2013. For each year, the figure plots the federal minimum wage, as well as the 50th, 75th, and 90th percentiles of the effective minimum wage distribution. The figure shows substantial variation in minimum wage across states over the sample period, especially during the 2000s.

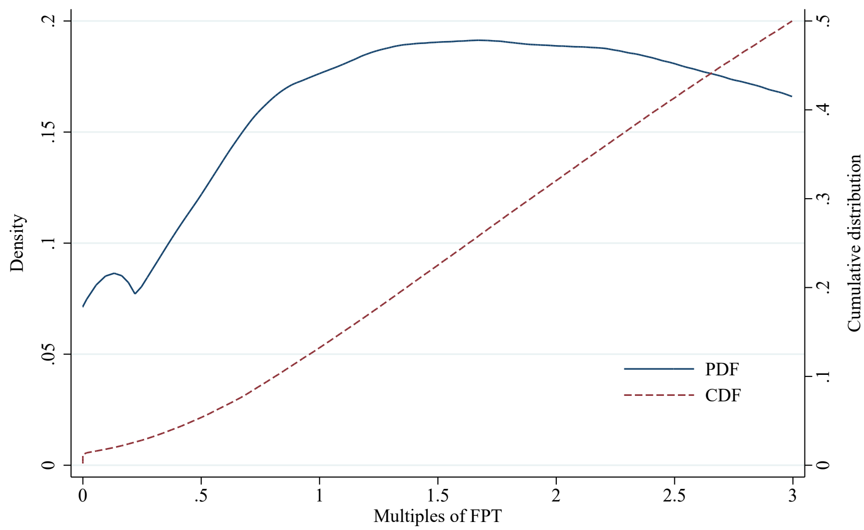
Appendix Figure B.2 reports the PDF and CDF of the family income distribution for the non-elderly population, in multiples of the FPT. This distributions are estimated using the full sample between 1984-2013. Panel A of Figure B.3 plots the family income quantiles (in multiples of FPT) over time. The figure shows that the income quantiles at the bottom of the distribution have been fairly stationary over the past three decades, although they do exhibit cyclical tendencies. Panel B of Appendix Figure B.3 shows the probability densities associated with family income cutoffs ($f_A(c(\tau))$), which 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.

Figure B.1: Effective 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 1984-2013 March CPS surveys and person weights. The gray dots in the scatter plot represent annualized effective minimum wages in each state.

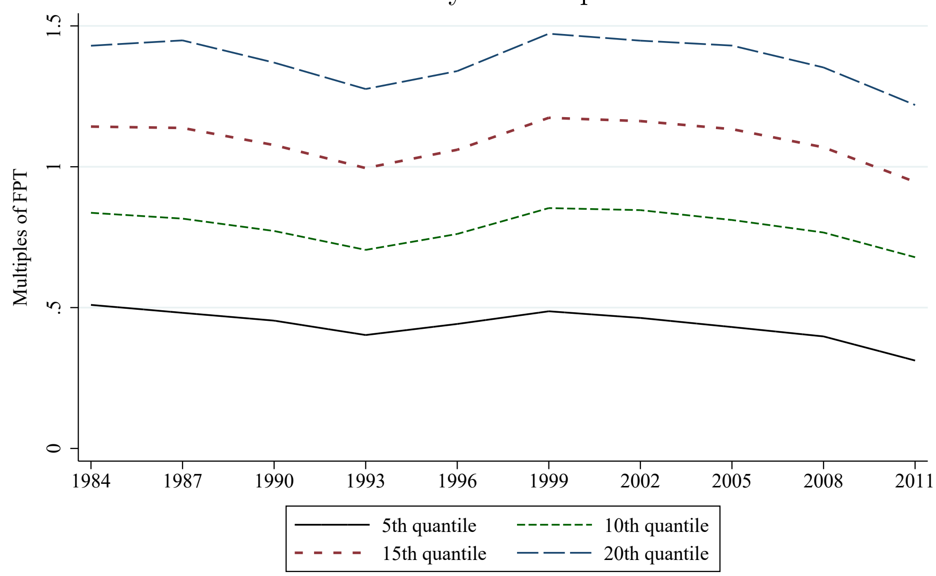
Figure B.2: Probability density and cumulative distribution of family income: averages over 1984 2013 March CPS samples



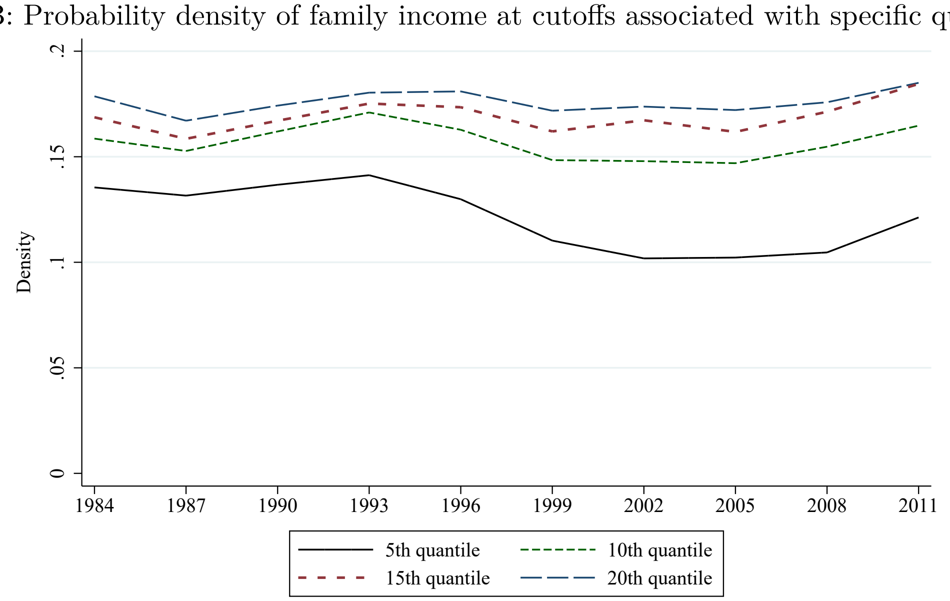
Notes: Both the probability density and cumulative distribution function are estimated using March CPS person weights for survey years 1984-2013 for the non-elderly population. The probability density is estimated using an Epanechnikov kernel and the STATA default bandwidth based on Silverman's rule-of-thumb.

Figure B.3: Family income quantiles, and probability density at associated cutoffs over time

Panel A: Family income quantiles



Panel B: Probability density of family income 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 family income 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.