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The Effects of Minimum Wages on the Distribution of Family Incomes: A Non-parametric Analysis

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THE EFFECTS OF MINIMUM WAGES ON THE DISTRIBUTION OF FAMILY INCOMES: A NON-PARAMETRIC ANALYSIS

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The primary goal of a national minimum wage floor is to raise the incomes of poor families with members in the work force. We present evidence on the effects of minimum wages on family incomes from March CPS surveys. Using non-parametric estimates of the distributions of family income relative to needs in states and years with and without minimum wage increases, we examine the effects of minimum wages on this distribution, and on the distribution of the changes in income that families experience. Although minimum wages do increase the incomes of some poor families, the evidence indicates that their net effect is, if anything, to increase the proportions of families with incomes below or near the poverty line. Thus, it would appear that *reductions* in the proportions of families that are poor or near-poor should not be counted among the potential benefits of minimum wages.

JEL Classification: J18, I3, J23, D31
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I. Introduction

Debates over the merits of a higher minimum wage frequently focus on the employment effects of minimum wages. However, the existence of negative employment effects does not necessarily imply that minimum wages constitute bad social policy. Employment losses associated with a higher minimum wage may be acceptable if the increase in the minimum raises the incomes of poor or near-poor families. To quote Gramlich (1976): "Minimum wages do, of course, distort relative prices, and hence compromise economic efficiency, but so do all other attempts to redistribute income through the tax-and-transfer system. The important question is not whether minimum wages distort, but whether the benefits of any income redistribution they bring about are in some political sense sufficient to outweigh the efficiency costs" (p. 410). Our goal in this paper is to provide information that helps in assessing this tradeoff.

Two empirical questions underlie the potentially positive redistributive effects of minimum wages. First, how do minimum wages affect the total earnings of the low-wage workforce; that is, do the wage gains received by employed workers more than offset the earnings lost by those who lose or cannot find jobs?[1] Second, how do minimum wages impact workers in different parts of the family income distribution? Because not all minimum wage workers are in poor families (Gramlich, 1976; Card and Krueger, 1995; Burkhauser, et al., 1996), the incidence of gains and losses to workers in different parts of the family income distribution will have an important influence on the effects of minimum wages on low-income families.

In this paper, we provide non-parametric density estimates of the effects of minimum wages on the distribution of family incomes. Specifically, we use matched March CPS data on families to study how both the distribution of family incomes relative to needs and the distribution of changes in family incomes relative to needs are affected by an increase in the minimum wage. In a nutshell, our empirical strategy is to compute difference-in-difference estimates of the effects of minimum wages on the family income-to-needs distribution,

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[1]DiNardo, et al. (1996) focus on the effects of minimum wages on the distribution of wages of employed workers.

would a regression based approach based on arbitrarily specifying particular points of the distribution and asking whether the proportions of families above or below those points increase or decrease. On the other hand, as explained in detail in Section IV, the non-parametric estimation comes at some cost, including the complexities of recovering estimates of the combined effects of contemporaneous and lagged increases in minimum wages, and the inability to fully exploit continuous variation in the minimum wage. In our view, the advantages of the non-parametric approach outweigh the disadvantages. Regardless, it clearly provides complementary evidence to other parametric approaches.

The evidence on both the distributions of family income and of changes in incomes experienced by families in different parts of the distribution indicates that, if anything, raising the minimum wage tends to increase the proportions of families that are poor and near-poor and reduce the proportion of families with incomes above the "near-poverty" line but below about 3 times the poverty line. This evidence implies that reductions in the proportions of families that are poor or near-poor should not be counted among the potential benefits of minimum wages.

II. Distributional Effects of the Minimum Wage

For minimum wages to raise the incomes of low-income families, they must accomplish two things. First, they must redistribute earnings toward low-wage *workers*. Second, some of the low-wage workers who benefit must be in low-income families.

In regard to the first question, estimated employment elasticities from minimum wage studies in the −0.1 to −0.2 range for teenagers and young adults are sometimes interpreted as indicating modest disemployment costs and hence beneficial distributional effects for low-wage workers. In particular, because an elasticity of demand for minimum wage workers of –1 implies that total income of these workers is unchanged by a minimum wage increase, the smaller negative elasticities reported in the literature are often viewed as suggesting that minimum wages raise the incomes of low-wage workers.

However, the argument that "small" disemployment effects are indicative of a positive effect of the by comparing changes in this distribution over time in states in which minimum wages did and did not increase.

This approach has some important advantages relative to existing work on the effects of minimum wages on the income distribution. First, most of the well-known papers on this topic, including Gramlich (1976), Johnson and Browning (1983), Burkhauser and Finegan (1989), and Horrigan and Mincy (1993), do not directly estimate the consequences of minimum wage increases for family incomes, but rather make use of simulations that are based on assumptions about employment effects and other relevant parameters. In contrast, we conduct an actual "before and after" analysis of the effects of minimum wages on family incomes. Second, the few papers that do use actual changes in family income to infer the effects of minimum wages focus on a specific parametric question that is an isolated part of the whole picture. For example, Addison and Blackburn (1999) estimate the effects of minimum wages on state poverty rates for relatively narrow subsets of the population. Similarly, Card and Krueger (1995) estimate the effect of the minimum wage on state poverty rates and on weekly family earnings at a few specific centiles, Neumark and Wascher (2002) focus on transitions in and out of poverty, and Connolly and Segal (1997) estimate average earnings changes experienced by families in different ranges of income-to-needs.[2]

In contrast, our non-parametric approach provides a full picture (literally) of the effects of minimum wages on the shape of the family income distribution and on changes in incomes of families at different points in the income distribution. For example, we can examine the extent to which minimum wages push families initially near-poor into poverty, or push initially poor families out of poverty. Importantly, the non-parametric approach provides a far richer empirical description of the effects of minimum wages on family incomes than

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[2]This last paper is closest in spirit to ours, although it still looks at average changes. Curiously, although the authors conclude that minimum wage changes help the working poor, their tables indicate that minimum wages had similar percentage effects on family income for poor/near-poor families and for families with incomes more than 1.5 times the poverty line. This makes it difficult to interpret the evidence as reflecting minimum wage effects. Also, like most of the other papers mentioned in the text, the sample period used by Connolly and Segal is considerably more limited than in our paper.

minimum wage on earnings is flawed. For one thing, the conventional −0.1 or −0.2 elasticities used to make this argument are taken from studies of the employment effects of minimum wages for entire age groups, and thus are not equivalent to the elasticity of demand for minimum wage workers. Rather, an estimate of the effect of a minimum wage increase on total employment in any particular age group is the effect on the low-wage individuals in the group (for whom the new minimum wage raises wages), averaged over all workers in this age category; because high-wage workers are largely unaffected by changes in the minimum wage, the aggregate elasticity will likely understate the employment impact on affected workers. In addition, the conventional elasticity uses the legislated minimum wage increase as the denominator, whereas the average wage increases received by workers below the new minimum is likely to be smaller, because many of these workers previously were earning wages above the old minimum. Reducing the denominator in the elasticity also increases its absolute magnitude.

To illustrate this point, we consider the 1996-1997 legislation that raised the minimum wage from \$4.25 per hour to \$5.15 per hour, a 21.2 percent increase. Data from the 1995 CPS indicate that 6.2 percent of workers aged 16 to 24 were paid the old minimum wage in that year and another 15.1 percent were paid a wage between the old and new minimums, implying that a total of 21.3 percent of the youth work force was directly affected by the minimum wage increase. If everyone in these categories who retained a job saw their new wage rise to exactly \$5.15 per hour as a result of the increase in the minimum, the average wage increase received by a worker in this affected group would be 10.8 percent. If we further assume that all of the job loss resulting from the minimum wage increase occurred among these affected workers, then using an elasticity of −0.1 for the age group as a whole, we can calculate the demand elasticity for young minimum wage workers as:

$$(-0.1/0.213)/(10.8/21.2) = -0.92$$.[3]

[3]Dividing −0.1 by 0.213 adjusts the numerator of the conventional elasticity to obtain the percentage employment decline among affected workers. Dividing by (10.8/21.2) corrects the percentage wage increase in

This calculation suggests that appropriately adjusting the estimates taken from studies of the employment effects of minimum wages in order to obtain an elasticity of demand for minimum wage workers can easily produce an elasticity that is close to -1, the level at which minimum wage increases have essentially no effect on the average earnings of the low-wage workforce. And, if we start with a larger "baseline" disemployment elasticity (e.g., an "elasticity" of -0.2), then the effective elasticity of demand would imply that an increase in the minimum wage would lead to a reduction in the average earnings of low-wage workers.[4]

In addition to the ambiguous consequences of minimum wages for low-wage workers, any boost that a higher minimum wage might provide to low-income families is potentially weakened by the presence of low-wage workers in other parts of the income distribution. For example, only one-third of workers likely to be affected by the 1990 increase in the federal minimum wage were in poor or near-poor families (defined as those with family incomes up to 1.5 times the poverty line based on their family's size), and roughly another one-third were in families with incomes exceeding three times the poverty line (Burkhauser, et al., 1996). As a result, the redistributive effects of minimum wages depend on the location in the family income distribution of the "winners" and "losers" among low-wage workers. If the job losses from a minimum wage increase are concentrated among teenagers in relatively affluent families, while the wage gains are concentrated among single-parent heads of households, then it is considerably more likely that a minimum wage increase would help poor or low-income families. On the other hand, if job losses are concentrated among low-wage workers

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the denominator of the conventional elasticity to reflect the fact that the average wage increase for affected workers is smaller than the minimum wage increase itself.

[4]These elasticities consider only employment effects. There may be some positive distributional effects from wage increases for workers a bit above the minimum wage, whether stemming from relative demand shifts toward slightly higher-wage workers or from relative wage constraints faced by employers (Gramlich, 1976; Grossman, 1983). Such effects are potentially quite important in assessing the consequences of minimum wages for low-wage workers (and low-income families), because many workers earning above the minimum are still considered low-wage workers and a sizable proportion of them are in poor and near-poor families. The focus on employment effects also ignores potential changes in hours, which could increase or decrease.

in low-income families, poor families would be especially hurt by minimum wage increases. Finally, as noted by Addison and Blackburn (1999), changes in the minimum wage may induce labor supply responses by other family members or may lead to changes in family living arrangements, both of which could affect family income.

Thus, despite the relatively modest disemployment effects estimated from standard minimum wage studies, the direction (and magnitude) of the effects of minimum wages on low-income families, and on the family income distribution overall, are open questions. In addition, because of the possible roles played by employment and hours changes, wage increases for above-minimum wage workers, and the distribution of these effects throughout the family income distribution, these questions cannot be satisfactorily answered by the simulation exercises that are prevalent in the existing research.

III. The Data

The data we use come primarily from matched March CPS annual demographic files from 1986 through 1995. Using matched data from the CPS provides an important advantage relative to an analysis of the annual CPS cross-sections. In particular, the availability of two consecutive years of data for each family allows us to observe their transitions between various parts of the income distribution. As a result, when we observe a change in the income-to-needs distribution, we can more comfortably conclude that this change reflects the actual experiences of families rather than differences in the set of families sampled in each year. Statistically, the homogeneity of the samples before and after the minimum wage increase leads to more precise inferences.

Our choice of sample period was influenced by three factors. First, we are able to match successive March files for consecutive years over this ten-year time period. In particular, it is not possible to match CPS files from 1985 with those in 1986 because the two files are based on different Census sample designs. For similar reasons, it is also not possible to match the 1995 files with those in 1996. Second, for reasons discussed below it is desirable to use a sample period with considerable state variation in minimum wages, and it was in the late 1980s that such variation first emerged. Third, because the 1996 welfare reform legislation likely induced significant changes in labor market behavior among low-income families, the effects of minimum wages on the income distribution are arguably better isolated by focusing on the pre-reform period.

For each family, we extracted information on the amount and composition of family income, family size, and the family's state of residence.[5] We take a broad approach that does not distinguish among families based, for example, on whether family size changed, someone retired, or there was any earned income. Instead, we treat the family as the unit of observation and infer the total effect of minimum wage changes through any of these channels. Obviously the type of analysis we carry out here can be extended to study the mechanisms by which family incomes (relative to needs) are affected.

In all cases, the income data refer to the previous calendar year; although the state of residence is contemporaneous, the matching process ensures that only families living at the same address in two consecutive years are included in the data. We follow other research in this area in looking at total family income from all sources. Given the family income data, each family is classified in terms of its income-to-needs ratio (the ratio of total family income to the poverty line for that family). The estimation is conducted for families with non-negative incomes, up to a maximum income-to-needs ratio of six.

Each family-year record is also assigned the minimum wage level that prevailed in the state in May of the year for which family income is measured, as well as the minimum wage in the preceding year.[6] Because state minimum wage laws do not exempt employers of workers covered by the federal law from the federal minimum wage, and because coverage by the federal law is nearly complete, we use the higher of the federal

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[5]We use the CPS definition of a family, which is a group of two or more persons (one of whom is the householder) residing together and related by birth, marriage, or adoption. All persons satisfying these criteria, including related subfamily members, are considered members of one family.

[6]The bulk of the minimum wage increases in the sample occur in April (with the second greatest number occurring in January), so using May as the reference month works well in terms of defining the minimum wage that prevailed over most of the year for which income is measured.

minimum wage and the state minimum wage for each state and year.

Table 1 reports these minimum wages for each state for most of our sample period. The first column reports the minimum wage prevailing in 1987, while the remaining columns report the new minimum wage following an increase. With the exception of Minnesota, Pennsylvania, and New Jersey, all of the state increases have occurred in the New England and Pacific states. The most obvious feature of the table, however, is that a high fraction of the minimum wage increases in this sample period stem from changes in the federal law. Because increases in the federal minimum often coincided with sharp increases in overall unemployment rates (e.g. in 1991), we cannot treat minimum wages as "randomly assigned," but instead must attempt to account for the relationship between minimum wages and the business cycle to draw causal inferences regarding the effects of minimum wages on family incomes.

Finally, for weighting purposes we also retained the family-specific sampling weight. We then adjusted this weight to account for the possibility that certain types of families have a lower probability of being in the survey in consecutive years and thus are less likely to be included in our matched sample. In particular, although overall match rates were above 80 percent, families with younger heads and lower income-to-needs ratios were significantly less likely to be successfully matched. Using a logistic regression, including the age and race of the family head and the income-to-needs ratio as categorical variables, we estimated the probability of a successful match for each family, and divided the sampling weight for successfully matched families by this estimated match rate. The adjusted weight is an estimate of the inverse of the probability of being in our matched sample of families. Of course this procedure does not correct for non-random matching that, conditional on these observables, is correlated with changes in income-to-needs and therefore possibly also with minimum wage changes. Our conjecture is that, if anything, families most adversely affected by minimum wage increases tend to move away from areas where minimum wages have increased and toward areas where they have not. If so, the bias from non-random matching will tend to understate any adverse consequences of minimum wage increases.

IV. Empirical Methods

Basic Strategy

In this section, we describe our strategy for estimating the effects of minimum wage increases on the distributions of the levels and changes in the family income-to-needs ratio. Although we present our methods in relation to the density of income-to-needs, the approach carries over to the density of changes in income-to-needs as well.

Our basic approach is to construct a difference-in-difference estimator. The treatment group is defined as the set of families residing in states in which the minimum wage rose between years 1 and 2; the control group thus consists of families in states in which the minimum wage remained constant between years 1 and 2. Letting numbers in the subscripts denote years, and MW=1 and MW=0 denote the treatment and control groups, we use $f_{1,MW=1}(I)$ to denote the density of income-to-needs in year 1 in the treatment group and $f_{2,MW=1}(I)$ to denote the density in year 2 in the treatment group. The difference $f_{2,MW=1}(I) - f_{1,MW=1}(I)$ measures the change in the density at each point I for this group. Because the density of income-to-needs may be changing for reasons other than minimum wage increases, we subtract off the corresponding quantity for the control group, $f_{2,MW=0}(I) - f_{1,MW=0}(I)$. This yields the difference-in-difference estimator of the effect of minimum wage increases on the density at each income-to-needs ratio I:

$$\{f_{2,MW=1}(I) - f_{1,MW=1}(I)\} - \{f_{2,MW=0}(I) - f_{1,MW=0}(I)\} .$$

Although this approach does not provide explicit estimates of the influences of various regression controls, it potentially accounts for a wide range of factors that might alter the distribution of income. For example, cyclical GDP growth, unemployment rates, rising earnings inequality stemming from other sources, and demographic trends (all national phenomena) are controlled for if these effects are equally evident in the treatment and control groups; in the subsection that follows we ask whether this condition is likely to hold, and explain how we account for such factors if it does not. In addition, an advantage of the non-parametric approach is that, unlike in a regression model, the effectiveness of these controls does not rely on the linearity of any relationship with respect to the distribution of family income to needs.

To estimate each of the four densities in this expression, we use a kernel estimator. In particular, given a kernel K(z), the estimated density function for I is:

$$f_K^e(I) = \frac{1}{n}\sum_{j=1}^{n}\frac{\theta_j}{h}\ K[\frac{I - I_j}{h}] \quad ,$$

where n is the number of observations in the sample, h is the bandwidth, and θ_j is a sampling weight that has been normalized to sum to 1. The points at which the density is estimated are indicated by I, and the data by I_j. The initial bandwidth is chosen according to a normal rule of thumb procedure. Under this rule of thumb, if the data were generated from a normal distribution, the bandwidth used would be optimal (in a RMSE sense). In segments of the distribution with fewer observations, the bandwidths are adjusted to be wider using the adaptive bandwidth rule of Silverman (1986). In contrast, we allow for sharper fluctuations in the estimated density in ranges in which there are many observations (Härdle, 1991). As the peak of the family income-to-needs distribution is typically near 1 (i.e., the poverty line), this technique increases the accuracy of the kernel procedure in the area that may be of greatest interest.

When we analyze the densities of changes in income-to-needs, we study subsamples based on year 1 income-to-needs. To accommodate the widely differing sample sizes that result, we pool the data for initial bandwidth selection following Marron and Schmitz's (1992) approach. This keeps the level of smoothing equal for the analyses of families in different initial income-to-needs categories, whereas standard rules would result in more smoothed estimates for smaller sample sizes.

Controlling for Other Differences Between Treatment and Control Groups

There are two important complications that must be addressed in constructing this difference-in-difference estimator. First, as already mentioned, it is probably incorrect to assume that the treatment and control groups are randomly assigned. Table 2 displays separately the distribution of all observations and the distribution of observations with minimum wage increases, across cells defined by changes in state unemployment rates and changes in state poverty rates.[7] With respect to unemployment, minimum wage increases appear non-randomly distributed. For example, in the cells with declines in state unemployment rates of 2 to 3 percentage points, only 11 percent of the observations coincide with minimum wage increases. Conversely, in the cells with increases in unemployment rates of 1 to 2 percentage points, 39 percent of the observations coincide with minimum wage increases. The differences are more pronounced for more extreme changes in unemployment rates, although fewer observations are in those cells. Because minimum wage increases tend to be associated with increases in unemployment rates, and increases in the unemployment rate most likely disproportionately harm low-income families, failure to account for this relationship overstates any adverse effect of minimum wages on such families. On the other hand, as Table 2 shows, the pattern of minimum wage changes with respect to poverty changes is considerably less pronounced, and there is, if anything, a slight overrepresentation of minimum wage increases among observations with poverty declines. Of course, if changes in poverty stem from minimum wage increases, there is no sense in which we want to "control" for poverty changes.[8] But if there are other policy changes that reduce poverty and are correlated with minimum wage increases, then failing to account for these will likely overstate any beneficial effects of minimum wages on poor and low-income families. In this regard, our findings that minimum wages tend to increase the proportion of poor and near-poor families would be strengthened if we controlled for other sources of change in poverty rates.

The intuition behind our strategy for controlling for the correlation between minimum wage increases and changes in the unemployment rate follows DiNardo, et al. (1996). In particular, conditioning is converted

[7]We focus on changes in these rates, rather than levels, because we use a difference-in-difference estimator that is not influenced, for example, by persistent state differences in unemployment rates.

[8]This is potentially an issue for unemployment rates as well, but the effects of minimum wages on overall state unemployment rates are likely negligible. Also, controlling for changes in poverty is unappealing because the poverty rate is a transformation of the income-to-needs distribution.

to a reweighting problem in which we define a number of cells for the conditioning variable (the change in the unemployment rate), and then reweight the observations in the treatment group so that the distribution of observations across unemployment rate change cells is the same in the treatment and control groups. In our case, this can be thought of as creating an artificial sample in which observations with minimum wage increases are no longer concentrated in years with sharper increases in unemployment.[9]

To see how this works, suppose that we want to estimate the density of income-to-needs for observations for which the minimum wage increased, recognizing that the income-to-needs ratio (I) is also related to changes in the unemployment rate U. For the treatment group, this means that we need to estimate the (hypothetical) density of I for observations for which the minimum wage increased but for which unemployment rate changes were the same as observations for which the minimum wage *did not* increase. This is done for years 1 and 2, and we then compare the change in this hypothetical density with the observed change for the control group. Omitting the time subscripts, we write the conditional density for the treatment group as

$$f_{MW=1}(I \mid U_{MW=0}, I(U)) \quad ,$$

where $U_{MW=0}$ represents changes in unemployment rates for the observations for which the minimum wage did not increase, and I(U) is the relationship between income-to-needs and changes in unemployment for observations for which the minimum wage increased. What we observe, however, is

[9] In Neumark and Wascher (2002) we incorporate a wide variety of control variables into a parametric framework, including unemployment, the wage distribution, AFDC benefits, and welfare reform. These other controls had relatively little impact on the qualitative conclusions. However, adding controls for AFDC benefits and waivers (which reduce poverty) leads to slightly stronger adverse effects of minimum wages, confirming the argument that controlling for other sources of changes in poverty rates would tend to strengthen the adverse effects of minimum wages on poverty. The approach in that paper yields estimates of the effect of the minimum wage on the probability of leaving or entering poverty, but does not yield the rich information on the effects of minimum wages throughout the income distribution inherent in the non-parametric approach.

$$f_{MW=1}(I \mid U_{MW=1}, I(U)) \quad .$$

We define a reweighting function $\Psi(U)$ to capture the differences in the relative frequencies of particular ranges of unemployment rate changes in the treatment and control groups,

$$\Psi(U) = \frac{dG(U_{MW=0})}{dG(U_{MW=1})} \quad ,$$

where dG is the proportion of observations with each value of U in the population. The function $\Psi(U)$ is estimable using the proportions of observations at each value of U in the treatment and control groups.

This reweighting function generates the desired density because the reweighted observed density can be written as

$$\int f_{MW=1}(I,U)\Psi(U)dG(U_{MW=1}) \quad ,$$

which equals

$$\int f_{MW=1}(I,U)dG(U_{MW=0}) = f_{MW=1}(I \mid U_{MW=0}, I(U)) \quad ,$$

our desired density.

As DiNardo, et al. (1996) show, we can estimate each desired density using the kernel estimator

$$f_K^e(I \mid U_{MW=0}, I(U)) = \frac{1}{n}\sum_{j=1}^{n} \frac{\Psi^e \theta_j}{h} K[\frac{I - I_j}{h}] \quad ,$$

where Ψ^e is the estimate of Ψ.

As an alternative strategy, we also present results where we simply exclude from the analysis minimum wage increases that took effect in 1991 or 1992, years in which unemployment rates rose sharply as a result of the recession. This accounts–in a more extreme manner–for the principal problem in our sample of inferring the effects of minimum wages on family incomes, net of the business cycle.

Finally, we implement a procedure that mimics more closely a regression approach to controlling for factors generating state-specific or year-specific shifts in the income-to-needs distribution that are potentially correlated with the incidence of minimum wage increases. The goal is to mimic the inclusion of fixed state and year effects in a difference-in-difference regression framework. We are ultimately interested in differences in the changes in the distributions of income-to-needs in our treatment and control groups. What we would like to avoid, then, is attributing to minimum wages shifts in income-to-needs distributions that are common to states or years, but correlated with minimum wage changes. A prime example, as just discussed, is the aggregate business cycle. But other possibilities include changes in federal policies affecting the income distribution, aggregate influences on wages or incomes other than the business cycle, or state-specific differences in movements of families through the income-to-needs distribution, perhaps also stemming from policy differences.

To do this, we first estimate the median proportional change in income-to-needs by state (across all years). We then adjust each family's income-to-needs in year 2 so that the common state shift is taken out of the change in the family's income-to-needs from year 1 to year 2. We make a parallel adjustment for the median proportional change by year (across all states).[10] The difference between the adjusted data on income-to-needs for each family in year 2 and the year 1 income-to-needs ratio is the deviation around the average

[10]We use the proportional change because the first difference of the level of income-to-needs is unlikely to apply very well to either tail of the distribution. In addition, using the proportional change controls for the possibility that income-to-needs distributions shift because of changes in the price of skill that affect incomes multiplicatively. In particular, changes in the price of skill that are common across all states would "hollow out" the left-hand tail of the distribution relatively more in higher-wage, higher-income states, which could in turn bias our estimated minimum wage effects to the extent that minimum wage increases in higher-wage, higher-income states provide relatively more (or less) identifying information. Because we use proportional changes, we estimate state-specific and year-specific medians rather than means to avoid outliers caused by very high or low income-to-needs values for either of the two observations on a family.

state change over all years in the sample and the average year change over all states in the sample. We then perform the same analysis described above (and elaborated upon below) using these adjusted data.

Contaminated Treatment and Control Groups

A second concern arises in our analysis because previous research has found that the effects of minimum wages are often stronger at a lag of one year (see Neumark and Wascher, 1992 and 2002, and Baker, et al., 1999), and thus we are interested in estimating both contemporaneous and lagged minimum wage effects on the densities of levels and changes of family income-to-needs. This creates complications because the observations for the treatment group (or the control group) may be contaminated by the effects of minimum wage increases not directly captured by the difference-in-difference estimator. For example, when we estimate $f_{2,MW=1}(I)$ for the treatment group for the lagged effect, there could also be a contemporaneous effect in year 2. Similarly, when we estimate the density for the treatment group for the contemporaneous effect, there could be a lagged effect (from a contemporaneous increase in year 1). Of course, we could drop all of the observations in which the treatment is contaminated. But as Table 1 shows, that would entail the loss of many observations.

Instead, we employ a procedure that uses all of the observations and distributes the overall effects into "pure" contemporaneous and "pure" lagged effects correcting for the incidence of contaminated treatment and control groups. To explain the procedure, we first define the following terms:

C(I) = the estimated change in the density from year 1 to year 2 for observations with a contemporaneous increase in year 2, vs. the estimated change for observations with no contemporaneous increase,

L(I) = the similar estimate for lagged increases,

in(I)= the change in the density from year 1 to year 2 for observations with a contemporaneous increase in year 2 and no lagged increase in year 2,

ni(I)= the change in the density from year 1 to year 2 for observations with a lagged increase in year

2 and no contemporaneous increase in year 2,

ii(I)= the change in the density from year 1 to year 2 for observations with a contemporaneous increase in year 2 and a lagged increase in year 2,

nn(I)= the change in the density from year 1 to year 2 for observations with no contemporaneous increase in year 2 and no lagged increase in year 2,

Then C(I) is a weighted average of the estimated changes in densities over four groups:

$$C(I) = \alpha_1\{in(I) - nn(I)\} + \alpha_2\{in(I) - ni(I)\} + \alpha_3\{ii(I) - nn(I)\} + \alpha_4\{ii(I) - ni(I)\} \ ,$$

where the first term corresponds to a change in densities estimated from uncontaminated treatment and control groups, the second term corresponds to an estimate with a contaminated control group only, the third term corresponds to an estimate with a contaminated treatment group only, and the fourth term corresponds to an estimate with a contaminated treatment and control group; the α_k (which sum to 1) are the probabilities that the estimate comes from each of these groups.[11] Similarly, L(I) can be written as

$$L(I) = \beta_1\{ni(I) - nn(I)\} + \beta_2\{ni(I) - in(I)\} + \beta_3\{ii(I) - nn(I)\} + \beta_4\{ii(I) - in(I)\} \ .$$

Adding and subtracting nn(I) in the second and fourth terms in the equation for C(I), and assuming that

$$\{ii(I) - nn(I)\} = \{in(I) - nn(I)\} + \{ni(I) - nn(I)\} \ ,$$

we can rewrite the expression for C(I) as

$$C(I) = \{in(I) - nn(I)\} + (\alpha_3 - \alpha_2)\cdot\{ni(I) - nn(I)\} \ .$$

This expression makes intuitive sense. First, the final term in the equation for C(I) drops out because both the treatment and control group are contaminated, leaving the difference-in-difference estimate

[11]Define γ as the probability that observations with a contemporaneous increase have a lagged increase as well (i.e., the probability that the treatment group is contaminated); this is estimated from the data. Similarly, define δ as the probability that observations with no contemporaneous increase have a lagged increase (i.e., the probability that the control group is contaminated). Then assuming independence of the two types of contamination (because observations are either in the treatment or control group), $\alpha_1 = (1-\gamma)(1-\delta)$, $\alpha_2 = (1-\gamma)\delta$, $\alpha_3 = \gamma(1-\delta)$, and $\alpha_4 = \gamma\delta$.

unaffected. Similarly, if $\alpha_2 = \alpha_3$, so that there are equal likelihoods that the estimate comes from a contaminated treatment group (only) and a contaminated control group (only), the contamination again does not matter, and C(I) is the estimated change in the density for uncontaminated treatment and control groups. On the other hand, if $\alpha_2 < \alpha_3$, so that there is a relatively higher probability of a contaminated treatment group, lagged effects {ni(I) - nn(I)} will be added to C(I), relative to the correct estimate of in(I) - nn(I). Conversely, if $\alpha_2 > \alpha_3$, so that there is a relatively higher probability of a contaminated control group, lagged effects will be subtracted from C(I).

In parallel fashion, L(I) can be rewritten as

$$L(I) = \{ni(I) - nn(I)\} + (\beta_3 - \beta_2)\cdot\{in(I) - nn(I)\} \quad .$$

We can solve the two equations C(I) and L(I) for the unknowns {in(I) - nn(I)} and {ni(I) - nn(I)}, which are the "pure" contemporaneous and lagged treatment effects, respectively, in which we are interested. Adding the "pure" lagged and contemporaneous effects together yields the combined "long-run" or "one-year-out" effect of minimum wage increases on the income-to-needs density. In a regression framework with contemporaneous and lagged increases as independent variables, this would be equivalent to the sum of the contemporaneous and lagged effects.

The assumption embodied in the equation for {ii(I) - nn(I)} above merits some discussion. For the case in which all minimum wage increases are of equal magnitude, this assumption means that the effect of two successive minimum wage increases (relative to no increases for two years) is equal to the sum of a pure contemporaneous and a pure lagged increase. In a regression context, this is equivalent to the assumption that there is not an interaction between contemporaneous and lagged effects.[12]

[12]Over the long-run, the average increase corresponding to the ii(I) term would equal the sum of the average increases corresponding to the in(I) and ni(I) terms. For example, taking a state with only federal minimum wages, 1990 would correspond to the in(I) term, with an increase of \$.45, 1991 would correspond to the ii(I) term, with an increase of \$.90, and 1992 would correspond to the ni(I) term, with an increase of \$.45. These averages only differ in the sample because the sample period sometimes cuts off observations corresponding to some of these terms; for example, for Maine, where the minimum rose in 1985, 1986, and

If, however, minimum wage increases are of different sizes, as is actually the case, this assumption does not hold exactly. For example, many of the "ii" observations—that is, those with a contemporaneous and lagged increase—are in 1991 and entail minimum wage increases of $.45. In contrast, the $.90 increase in California contributes "in" and "ni" observations (in 1989 and 1990, respectively), but no "ii" observations. On the other hand, there are also some smaller minimum wage increases (e.g., Oregon in 1994) that contribute "in" or "ni" observations. This problem reflects an inherent limitation of the non-parametric approach, which requires us to classify observations as belonging to the treatment or control group, and does not permit us to exploit information on the size of the treatment. In the ensuing empirical analysis, we carry out a few alternative analyses that help to assess whether this problem is generating any substantial biases.

V. Results

Difference-in-Difference Estimates of Minimum Wage Effects on Income-to-Needs Densities

Figure 1 displays the entire set of density estimations that we use to infer the effects of minimum wage increases on the distribution of income-to-needs. This figure shows results for the full matched data set. The first row presents evidence on changes in the income-to-needs distribution in states with contemporaneous minimum wage increases compared to states with no contemporaneous minimum wage increases. The left-hand panel presents estimates of the densities in year 1 and year 2 for the treatment group (observations with increases), and the middle panel for the control group. The vertical axis shows the proportion of families at each income-to-needs level. Because the differences between the densities in each panel are small relative to the scale (and therefore hard to distinguish visually), the right-hand panel summarizes the information by

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1987, we get the ii(I) term in 1987, but not the in(I) term for the first year of the run-up in that state's minimum wage to its value in 1987.

As a check on the assumption of no interaction, we estimated parametric regression models for changes in the proportion of families poor or near-poor, using data aggregated to the state-year level. These specifications indicated contemporaneous and lagged minimum wage effects that roughly parallel the richer non-parametric estimates described below, and revealed no statistically significant interactions between contemporaneous and lagged effects. Results are available from the authors upon request.

plotting–for the treatment and control groups, with a different scale–the vertical distance between the year 1 and year 2 densities. The difference-in-difference estimate of the effects of contemporaneous minimum wage increases is the vertical distance between these two lines. After applying the methods described in the previous subsection, we extract the "pure" effect of a contemporaneous minimum wage increase, which is displayed in the left-hand panel of the bottom row of Figure 1. Aside from the adjustment made to remove the influence of contaminated contemporaneous effects, this panel depicts the vertical distance between the solid and hatched graphs in the upper right-hand panel.[13] The results indicate that the effect of contemporaneous minimum wage increases is to reduce the proportion of families with income-to-needs of 0 to about 0.6, to increase the proportion with income-to-needs of 0.6 to 1.5, and to reduce the proportion with income-to-needs of 1.5 to about 2.7. These results are consistent with minimum wages helping the poorest families, but they also are suggestive of some income loss among families with initial income-to-needs in the range of approximately 1.6 to 2.7.

The panels in the second row of the figure report similar estimations, but with the treatment group defined as those observations for which there was a lagged minimum wage increase. The right-hand panel again reports the vertical distances between the year 1 and year 2 densities in the treatment and control groups. The difference-in-difference estimate of the pure lagged minimum wage effect is reported in the middle panel of the bottom row. In contrast to the estimated effects of contemporaneous minimum wage increases, lagged increases unambiguously raise the proportion of families below about 1.3 times the poverty line, with corresponding decreases in the proportion of families with income-to-needs between 1.3 and 3.2. This evidence, and the contrast with contemporaneous effects, is consistent with disemployment effects (or hours reductions) occurring with a lag.

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[13]Visual inspection of the graphs reveals that the adjustment has little effect on the difference-in-difference estimate. Consequently, further attempts to rectify some of the approximation errors associated with the assumptions underlying this adjustment would likely have no detectable effect. The same findings hold for the lagged effects discussed below.

To calculate the total effect of minimum wage increases, the bottom right-hand panel reports the sum of the contemporaneous and lagged effects. The estimated effects are given by the middle curve, while the upper and lower curves are the tails of the 95-percent confidence interval for the estimated change at the particular point of the income-to-needs distribution, calculated using a bootstrap procedure for the non-parametric estimation. The result is quite striking. There is essentially no net change in the proportion of families with income-to-needs below 0.3, as the benefit associated with the contemporaneous increase is essentially offset by the cost of the lagged increase. There is a marked increase in the proportion of families with income-to-needs between about 0.3 and 1.4, and a marked decrease in the proportion of families between about 1.4 and 3.3. These results suggest that the overall net effect of minimum wage increases is to push some families that are initially low-income but above the near-poverty line into poverty or near-poverty. For the range of income-to-needs from about 0.6 to 1.2, the estimated increase in the proportion of families is statistically significant.

Table 3 provides some summary information about the changes in densities displayed in Figure 1. The first row of the table reports the implied changes (and corresponding standard errors from the bootstrap) occurring in some of the more "meaningful" ranges of the income-to-needs distribution. As indicated in column (1), an increase in the minimum wage elicits essentially no change in the proportion of families with income-to-needs between 0 and 0.5. In contrast, as shown in columns (2) and (3), minimum wage hikes lead to an increase of 0.0079 in the proportion with income-to-needs between 0.5 and 1 and an increase of 0.0083 for the 0-1 category as a whole. The proportion of poor families in the sample is approximately 0.18, so that the change in the proportion poor corresponds to a 4.6 percent increase in the overall number of poor families. As indicated by the standard errors, the change in the proportion between 0 and 0.5 is not statistically significant, while the changes in the proportion between 0.5 and 1 and the proportion of poor families are statistically significant.[14] As was apparent in Figure 1, column (4) shows a sizable increase in the proportion of near-poor families (.0046, or 3.6 percent) following minimum wage changes, an estimate that is significant at the 10-percent level. Column (5) aggregates over the preceding categories and shows that minimum wage increases raise the proportion of poor and near-poor families by 0.013, an estimate that is statistically significant. Columns (6)-(8) indicate that minimum wage increases lead to declines in the proportion of families with income-to-needs in the 1.5-2 or 2-3 category of 0.0049 and 0.0071, respectively, while the overall decline in the proportion of families with income-to-needs between 1.5 and 3 is 0.012 (3.4 percent); the latter two estimates are statistically significant at the 5-percent level, and the first at the 10-percent level. To interpret the magnitudes in Table 3, the average minimum wage increase in our sample is 43 cents, or about 10 percent. Thus, the elasticity of changes in the proportion poor or near-poor with respect to the minimum wage is approximately 0.41, and the elasticity of the proportion with income-to-needs in the 1.5-3 range is about -.34.

Are We Detecting "Real" Effects of Minimum Wages?

As in any empirical study that tries to estimate the causal effect of a policy change, it is worthwhile to endeavor to establish that real effects of the policy change are being estimated, rather than a spurious relationship. We do this in several ways. First, we checked for minimum wage effects in parts of the income-to-needs distribution where there should be no effects. Figure 1 and Table 3 present detailed results for the part of the income-to-needs distribution below three times the poverty line. In addition, though, Figure 1 shows the estimated changes in the density from three to five times the poverty line. Over this range the changes are small and, as depicted by the confidence intervals, not statistically significant. Furthermore, although not reported in Table 3, the estimated minimum wage effects on the density defined over the 3-5 range (as well as over the 3-4 and 4-5 ranges individually) are always small and statistically insignificant, a

[14]The implied t-statistics are asymptotically normally distributed. Unless otherwise noted, statements regarding statistical significance are for two-sided tests at the 5-percent level.

result that also generally holds in the ensuing analyses. The fact that the estimates do not indicate an effect of the minimum wage on the incomes of higher-income families suggests that the changes in the income-to-needs distribution that we find for lower-income families can be attributed to increases in minimum wages.

As a further check on whether we are detecting real effects of minimum wages, we also investigated whether states with larger minimum wage increases experienced bigger changes in their income-needs distributions. In particular, while the "treatment" in our initial analysis is based simply on whether a minimum wage increase occurred, Table 1 shows that some minimum wage increases are quite small (e.g., 10 cents in Minnesota in 1990) and that others are much larger (e.g., 80 cents in New Jersey in 1992). The non-parametric procedure we use makes it difficult to take explicit account of continuous variation in the size of the minimum wage increase. However, we can provide a rough approximation of the importance of the size of a minimum wage increase by dividing the sample of state-year observations with minimum wage increases into those with small increases (less than the median increase of 45 cents) and those with larger increases (greater than or equal to 45 cents), and then recomputing our estimates for these two treatment samples, relative to the sample of state-year observations with no minimum wage increases. As we would expect if we are capturing real effects of minimum wages, the estimated effects are much stronger in the subsample with large minimum wage increases.[15,16]

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[15]The numbers below give some of the key comparisons, for the estimates corresponding to the first row of Table 3:

	0-1, In Poverty	0-1.5, Poor/Near-Poor	1.5-3
Increases < \$.45	-.0006	.0076	-.0105
Table 3	.0083	.0130	-.0120
Increases ≥ \$.45	.0120	.0157	-.0121

[16]In addition, this analysis helps reassure us that the problem of variation in the size of minimum wage effects with respect to extracting "pure" contemporaneous and lagged effects, discussed in Section IV, does not lead to substantial biases. In particular, when we disaggregate by size of minimum wage increase the estimation method appears to be sensitive in the appropriate direction to the actual sizes of minimum wage increases.

Finally, we carried out two more substantive analyses. First, one potential problem with any difference-in-difference estimator is that a different trend in the treatment group than in the control group can lead to a spurious inference about the treatment effect. One way to test for this problem in our analysis is to estimate "lead" effects of minimum wages. If the difference-in-difference procedure shows that future minimum wage increases are associated with the same types of effects on the income-to-needs density that we obtain from the contemporaneous and lagged effects, we might conclude that the estimates we reported in Figure 1 are picking up differential changes over time in the treatment and control groups that are not truly attributable to minimum wage increases.[17] The results using one-year leads are displayed in the lower right-hand panel of Figure 2,[18] and reveal no "effect" of future minimum wage increases on the income-to-needs density. Interestingly, while the estimated leading effects are virtually flat over the part of the distribution that is less than twice the poverty line, the results hint at leading effects for higher ranges, matching those depicted in Figure 1 (although these are always insignificant, as noted above). This result suggests that the contemporaneous and lagged effects at these higher ranges may reflect common trends in states where minimum wages increased, rather than causal effects of minimum wages. If so, this further reinforces our conclusion that minimum wage effects are concentrated in the lower parts of the income-to-needs distribution.

Second, in order to provide an assessment of whether our statistical methods tend to create spurious evidence of minimum wage effects, we recomputed our basic estimates after replacing the actual observations on minimum wage changes with randomly assigned minimum wage increases (and allowing for contemporaneous and lagged effects). In this case, an appropriate estimation procedure would reveal no effects of minimum wages. To carry out this test, we executed 1000 replications of randomly assigning

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[17]Of course, an observed effect of a future policy is conceivable, but these have never been established in the minimum wage employment literature.

[18]Given that we found the extraction of pure contemporaneous and lagged effects made little qualitative difference, we do not extract the pure from the contaminated effects for this robustness analysis.

minimum wage increases (to the same proportion of observations as in the real data), and then averaged the estimated effects. This exercise reveals no systematic tendency of our procedure to indicate minimum wage effects, as the graph of the average of the combined contemporaneous and lagged effects is essentially flat at no change. This graph is displayed in Appendix Figure 1.

Overall, the analyses in this subsection strengthen the argument that our methods are detecting real effects of minimum wages. We next go on to consider the role of other influences on the income-to-needs density, sample issues, and the interpretation of our evidence.

Controlling for Other Influences on the Income-to-Needs Distribution

By comparing changes in the income-to-needs density between those state/year pairs with minimum wage increases and those without such increases, the difference-in-difference estimates account both for fixed state differences in the density of the income-in-needs distribution and for changes in the density over time that are common across all states. However, the analysis to this point does not take explicit account of the possibility that minimum wage increases are correlated with other changes in economic conditions that may have influenced the distribution of family income, yet varied by state.

Estimates that take account of this problem are reported in Figure 3 and the remainder of Table 3. We first explore alternative methods of accounting for the relationship between minimum wage changes and changes in unemployment rates. In particular, we first exclude from the analysis minimum wage increases that took effect in 1991 or 1992, years in which the aggregate unemployment rate rose sharply as a result of the 1990-1991 recession.[19] As the federal minimum wage rose in both 1990 and 1991, this exclusion drops the 1991 increase from the analysis of contemporaneous effects of minimum wages, and *all* federal increases from the analysis of lagged effects. As a more extreme version of this exclusion rule, we also eliminated all years in which there was a contemporaneous or lagged federal minimum wage increase, which means that we drop

[19]That is, we exclude observations in both the treatment and control groups corresponding to these increases.

1992 as well. This of course throws out all common variation across states where the federal minimum wage is binding, but also throws out variation from differences in minimum wage changes that result from the federal minimum catching up to state minimums in high minimum wage states. It thus does more than simply the equivalent of including year fixed effects in a regression framework. Finally, we use the reweighting method described above, which we prefer because it accounts for the relationship between changes in the minimum wage and unemployment rates without excluding observations altogether. We also control more generically for factors generating state-specific or year-specific shifts in the income-to-needs distribution, using the method described in Section IV to remove state and year effects in the proportional shifts in income-to-needs distributions.

For each of these analyses, the relevant row of Figure 3 shows the difference-in-difference estimate that is conceptually equivalent to the last row of Figure 1; the first graph in each row shows the contemporaneous effect on the income-to-needs density, the second the lagged effect, and the third the total effect, again with the confidence intervals. As can be seen in the second row, excluding all years with contemporaneous or lagged federal minimum wage increases widens the confidence intervals considerably (note that the scale in the right-hand side panel is more condensed) and leads to much larger point estimates of the changes in the income-to-needs distribution, which are probably unreasonable. In each of the other analyses reported in Figure 3 (and even to some extent in the second row), the qualitative conclusions are similar to the results reported in Figure 1. The contemporaneous effect of minimum wage increases–displayed in the graphs in the left-hand column–is always beneficial for the families at the very bottom of the income-to-needs distribution. In addition, although the exact shape of the difference-in-difference estimate of contemporaneous effects on the density varies, there generally is an increase in the proportion of families in the range from about 0.6 or 0.7 to about 1.5 or 1.6, and a decline in the proportion of families with income-to-needs in at least some part of the 1.5 to 3 range.

On the other hand, the estimated lagged effects–displayed in the graphs in the middle column–systematically show a net increase in the proportion of families in the 0 to 0.5 range in response to a higher minimum wage, and, more broadly, a net increase in the proportion of families below the poverty line. In addition, estimates of the lagged effects indicate a net reduction in the proportion of families in the 1.5 to 3 range; this is presumably the range from which the additional poor and near-poor families are drawn.

Finally, the total effects are displayed in the graphs in the right-hand column. Again, the analyses lead to conclusions that parallel our initial analysis; indeed, the estimated total effects appear more similar across the first, third, and fourth rows than are the estimated contemporaneous or lagged effects separately. In particular, raising the minimum wage appears to have little net effect on the proportion of families in the lowest income-to-needs range (approximately 0 to 0.5) and raises the proportion of families in the 0.5 to 1.4 or 1.5 range; together, these effects imply that a higher minimum results in a net increase in the proportion of families that are poor or near-poor. In the last two rows, which show results that use all the data but with either reweighting or fixed effects, the point estimates of the increases in the proportion of families are statistically significant in a range surrounding the poverty line. Finally, all of the graphs indicate a reduction in the proportion of families in the range from about 1.5 to 3. Thus, the evidence points in the direction of minimum wages increasing the number of poor and near-poor families, with these families coming from the ranks of lower-income, non-poor (and non-near-poor) families.[20]

In Table 3, these results are translated into changes in the proportion of families in various income-to-needs ranges. Although the qualitative evidence generally points in the same direction as the baseline estimates shown in the first row, the magnitudes and the statistical strength of the evidence varies. In all four cases,

[20]In the estimations that do not throw out years of data (Figure 1; Figure 3, Panels C and D), the magnitudes of the estimated effects displayed in the figures are largest below the near-poverty line, while those above three times the poverty line are quite small and nearly always insignificant. However, this is no longer the case in the estimations in which many years of data are dropped (Figure 3, Panels A and B). We suspect that the problems in the latter estimations reflect the loss of identifying information caused by excluding many minimum wage increases, and hence we interpret this as pointing to the importance of using all of the available information, rather than as suggesting that we are identifying something other than minimum wage effects.

column (1) shows trivial (and statistically insignificant) changes in the proportion of families with income-to-needs from 0 to 0.5. Similarly, columns (2)-(5) consistently indicate positive effects from minimum wage increases on the proportions of families that are poor or near-poor, while columns (6)-(8) indicate that minimum wages lead to corresponding reductions in the proportion of families with income-to-needs from 1.5 to 3. For the estimation that excludes the high unemployment years (and therefore most of the identifying information from federal minimum wage increases), the estimated changes are not statistically significant. In contrast, the inclusion of controls (via reweighting) for changes in state unemployment rates, which is intended to capture the year-to-year variation in economic conditions that could potentially bias the estimated effects, generally leads to statistically significant estimates. Finally, for the estimation including fixed state and year effects, the statistical significance of the estimated effects is nearly the same as for the baseline estimates in the first row of the table. In particular, the estimated increases in the proportions below the poverty line or the near-poverty line are statistically significant, as is the estimated decrease in the proportion with income-to-needs between 1.5 and 3.[21]

Characteristics of Affected Families

Most of the results presented thus far suggest that minimum wage increases push families that are initially non-poor into poverty (and families above the near-poverty line below this line). This requires that these families initially have some rather low-wage workers, since we would generally expect low-wage workers to suffer the brunt of the disemployment or reduced hours effects of minimum wages. Table 4 provides indirect evidence on this and other issues, by documenting characteristics of families in the various income-to-needs categories in year 1.

Columns (1) and (2) present information on poor families. More than half of poor families have no workers, a relatively high proportion have one worker (.4), and an additional small proportion have 2 workers.

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[21]For this estimation, the bootstrapping encompasses the estimation of the fixed state and year effects.

In addition, these families are unlikely to have one or more teenage workers. Mean income is only about 2000 dollars in families with income-to-needs in the 0 to 0.5 range, and is about 5200 dollars in families with income-to-needs of 0.5 to 1 (all figures are 1982-1984 dollars). For near-poor families, described in column (3), the modal number of workers is 1, and 15 percent of families have 2 workers; of course, mean income is considerably higher.

Families with income-to-needs in the 1.5 to 3 range, some of whom appear to be pushed into poverty or near-poverty by minimum wage increases, are relatively more likely to have 2 or more adult workers, although again relatively few have teenage workers. Mean income is of course considerably higher for these families. However, the bottom panel of the table shows that incomes of the non-primary earners in these families are often quite low, with means of 5700 to 7500 dollars, and 25th centiles of 2200 to 3400 dollars. Thus, it is entirely possible that families with income-to-needs initially in the 1.5 to 3 range have a second worker earning a wage at or near the minimum wage,[22] and that minimum wage increases could have sizable adverse consequences for such families. In the next subsection, we present more direct evidence of such declines.

Direct Evidence on Changes in Income-to-Needs

In order to examine the changes in income-to-needs induced by minimum wage increases, we apply our difference-in-difference procedure for estimating the effects of minimum wages to the distribution of *changes* in income-to-needs. The analysis is performed separately for families initially in each of the following four (not mutually exclusive) income-to-needs categories: 0 to 1.5, 1.5 to 3, 1.5 to 2, and 2 to 3. As most of the alternative estimates reported in Figure 3 and Table 3 are qualitatively similar, we report estimates only for the analysis using the entire matched data set without controls, reweighting, etc.

Figure 4 first reports the density estimates for the 0 to 1.5 and 1.5 to 3 group. The horizontal axis

[22]The March CPS files have wages only for the outgoing rotation groups, which is why we look at earnings rather than wages.

denotes the change in the income-to-needs ratio and the vertical axis shows the proportion of families experiencing a given change. As before, we estimate the densities separately for contemporaneous and lagged increases, versus the control group of no increases. The top row is for those families with income-to-needs initially in the 0 to 1.5 range, and the bottom row for those initially in the 1.5 to 3 range. Figure 5 then reports the difference-in-difference estimates, which are the vertical distances between the graphs in Figure 4.

The first row of Figure 5 shows the results for families initially in the 0-1.5 income-to-needs category. The left-hand graph is the estimated effect of a contemporaneous minimum wage increase. Consistent with our previous finding that the wage increase is the dominant contemporaneous effect, the most notable feature of this graph is the positive mass to the right of zero. This indicates that the contemporaneous effect of a minimum wage increase is a greater proportion of families experiencing increases in income-to-needs than would otherwise be the case. The middle graph displays the estimated lagged effects. This graph is more suggestive of disemployment effects, with the positive mass to the left of zero indicating an increase in the proportion of families experiencing declines in income-to-needs ratios, and the trough to the right of zero indicating a decline in the proportion of families experiencing increases in income-to-needs. Finally, the right-hand panel displays the total effects of minimum wage increases. The picture is relatively unambiguous, with its most prominent feature being the positive mass to the left of zero. This implies that the net effect of minimum wage increases on poor and near-poor families is a decline in income-to-needs.

The graphs in the second row report a similar analysis for families initially in the 1.5 to 3 income-to-needs range. Focusing on the total effects displayed in the right-hand graph, we again see that minimum wage increases result in a net increase in the proportion of families experiencing declines in income-to-needs, and a net decrease in the proportion experiencing increases in income-to-needs.

Note that the bulk of the positive mass to the left of zero in this graph is for declines in income-to-needs of less than 1. This suggests that relatively few families with income-to-needs initially above 2 are falling into poverty. What may be happening instead is that some families with income-to-needs of about 2 are falling to 1.5 or so, and others with income-to-needs of 1.5 are falling into poverty. (Thus, what we ultimately observe can be thought of as the cumulative effect of many families making small movements to the left in the income-to-needs distribution.) To explore this further, the last two rows of graphs in Figure 5 break out the results for those with initial income-to-needs of 1.5 to 2, and 2 to 3. The same qualitative pattern of a positive mass at small declines in income-to-needs, and a trough at small increases, appears for both groups. The declines for those with income-to-needs initially in the 2-3 range tend to be relatively small (less than 0.5), suggesting that the declines into poverty or near-poverty are generally coming from families that are initially in the 1.5 to 2 range, or just above the near-poor cutoff.

Table 5 provides a summary description of these results. Paralleling Table 3, we report the change in the proportion experiencing particular changes in income-to-needs (the area under the graphs in Figure 5 in the specified range of changes in income-to-needs). Thus, for example, the 0.023 figure in the row for initial income-to-needs of 0-1.5, in column (3), indicates that, as a result of minimum wage increases, the proportion of families in this initial income-to-needs category experiencing a decline of 0 to 0.5 in their income-to-needs is raised by 0.023. More generally, the table clearly indicates increases in the proportions of families experiencing declines in income-to-needs, and decreases in the proportions of families experiencing increases in income-to-needs, as a result of minimum wage increases. These estimated effects of minimum wage increases on the changes in income-to-needs experienced by families explain the changes underlying the estimated effects of minimum wage increases on the income-to-needs distribution that were documented in the earlier tables and figures.

Comparison to Simulated Effects

We began this paper by arguing that there is little basis in the existing research for drawing strong priors as to the distributional consequences of minimum wages. Our direct analysis of the effects of minimum wages on the distribution of income-to-needs indicates that minimum wages do not reduce poverty, and, if anything, appear to increase it. To further assess the plausibility of the estimates, and to attempt to understand what the estimates might imply about the underlying disemployment effects, in this final subsection of our analysis we report results from some simulations of the link between disemployment effects of minimum wages and the family income distribution. To keep things simple, we focus only on disemployment effects, ignoring hours effects and effects on other workers' wages.

We begin by looking at the data in the control sample—the set of states and years with neither contemporaneous or lagged minimum wage increases. For this sample, we computed a baseline transition matrix among the family income-to-needs cells that we used in the earlier tables (0-1, 1-1.5, 1.5-2, 2-3, and 3 plus); denote this transition matrix T. Some of these transitions occur because families add minimum wage workers, so we first simulate the negative effects of minimum wages on the number of families adding a minimum wage worker. In particular, we use the actual data to calculate what the transition matrix would look like if none of these families added a minimum wage worker; denote this transition matrix T_a. Similarly, some of the transitions in the control sample occur because families lose a minimum wage worker. To simulate the distributional effects of additional job losses stemming from a minimum wage increase, we again use the actual data, calculating what T would look like if every family with a minimum wage worker in the first year lost this worker's income; denote this matrix T_l. Of course, these calculations reflect extreme assumptions about minimum wage effects. For the actual simulations, we assume an elasticity of employment of minimum wage workers with respect to the minimum wage, allowing this elasticity to sometimes take on different values in different cells of the income-to-needs distribution. Assuming that the disemployment effects are randomly distributed among minimum wage workers in each cell, we can rescale the differences $T - T_a$ or $T - T_l$ by the proportions of minimum wage workers who, conditional on the assumed elasticities, are affected by the increase in the minimum. This imposes an overall decline in minimum wage employment that corresponds to the assumed elasticity and enables us to recover predictions of the changes in the proportions of families in each cell of the income-to-needs distribution.

Some simulation results that illustrate the key points are reported in Table 6. These are calculated for a 10 percent increase in the minimum wage, which corresponds to the average increase in the sample period underlying the estimates in the preceding tables. The first panel reports simulations based on an elasticity for minimum wage workers of −0.2, which is assumed to be the same across all income-to-needs categories. The panel reports the changes in the proportion of families in each income-to-needs category owing to increased transitions out of minimum wage jobs, followed by the changes attributable to decreased flows into minimum wage jobs. Finally, the last row of the panel (and the other panels) reports the combined effects.

The figures in this last row of the first panel indicate an increase in the proportion of families that are poor of 0.0006, or 0.06 percentage point. Coupled with an even smaller (absolute) decline in the proportion of families above poverty but near-poor, the simulation indicates an increase in the proportion of poor or near-poor families of 0.05 percentage point. The remaining columns indicate that these families come from a decline in the proportions of families with income-to-needs between 1.5 and 3, although that proportion does not fall by quite as much because there are also families that flow from above 3 into the 1.5-3 range. These estimates should be compared with those reported in Table 3. Clearly, they are smaller by an order of magnitude; for example, the simulated 0.06 percentage point increase in the proportion of poor families is less than one-tenth as large as the estimated change of 0.71 percentage point in the last row of Table 3.

Of course, as we argued in Section II, an elasticity of −0.2 for minimum wage workers may be much too low. As an alternative, the second panel of Table 6 reports simulations with an elasticity for minimum wage workers (again assumed equal across income-to-needs categories) of −1. For some parts of the distribution, the simulated results are closer to our empirical estimates. In particular, the simulated increase in the proportion of poor families (0.3 percentage point) is now just under one-half of the (preferred) estimated increase in Table 3. However, in other parts of the distribution the simulated estimates do not match the empirical estimates very well. For example, the simulation indicates a decrease in the proportion of families with income-to-needs between 1 and 1.5, in contrast to the estimates.

To shed more light on how minimum wage increases affect the simulated proportions in different regions of the income-to-needs distribution, we experimented with letting the elasticities vary across the cells of the distribution that we use, while keeping the average elasticity the same (at −1).[23] The next two panels in Table 6 (which report only the combined effects) show that we need to assume a larger (absolute) elasticity in the 1.5-2 and 2-3 cells of the income-to-needs distribution to get a simulated increase in the proportion of families in the 1-1.5 category, because these families come from higher cells. On the other hand, the simulations indicate a greater increase in the proportion of poor families when sharper elasticities are assumed for lower income-to-needs categories. Finally, the simulation results indicate decreases in the proportion of families in the 1.5 to 3 category that are considerably smaller than the estimates in Table 3. But in simulations not shown, if smaller (absolute) elasticities are used for families with income-to-needs above 3, the decreases in the 1.5 to 3 category become larger.

These simulations are intended to be only suggestive. As noted above, they ignore hours and wage effects, and they impose untested assumptions about how the disemployment effects of minimum wages operate. As such, they should not be viewed as a substitute for the actual estimates we obtain from the data, even if those estimates (as in many policy evaluations) have a "black box" flavor to them. Indeed, we could in principle easily construct simulations that reproduce our estimates by specifying more narrowly where the disemployment effects occur (i.e., near which boundaries of the cells of the income-to-needs distribution). However, our intention is simply to give some rough idea of the size and incidence of the disemployment effects that are needed to rationalize the income effects we find in the data. Although there is no single answer to this question, the simulations clarify that the elasticities of employment for *minimum wage workers* with respect to the minimum wage would have to be larger than the elasticities for young workers reported in the literature. Perhaps more than anything else, these simulations, coupled with our estimates, point to the value

[23]The elasticities are larger than the −0.5 figure that is taken as a consensus in the labor demand literature (Hamermesh, 1993). However, whereas this consensus figure is for labor overall, the minimum wage employment elasticities refer to the response of employment of one narrow category of labor to the price of that labor, so substitution possibilities may be considerably greater.

of research aimed at gaining a more complete understanding of the effects of minimum wages on all of the behaviors and outcomes that feed into the impact of minimum wages on the income distribution.

VI. Conclusions

This paper presents a non-parametric analysis of the effects of minimum wages on the family distribution of income-to-needs. In particular, we attempt to address the central question regarding the wisdom of the minimum wage as social policy: Do minimum wage increases raise the incomes of families at the lower end of the income distribution? Although modest disemployment effects of minimum wages have often been interpreted as implying that minimum wages are likely to achieve this goal, there is little basis for this conclusion in the absence of direct evidence on the effects of the minimum wage on family incomes. The evidence we present comes from non-parametric difference-in-difference estimates of the effects of minimum wages on the income-to-needs distribution and on the distribution of changes in income-to-needs, which provide a complete characterization of the changes in these distributions induced by increases in the minimum wage.

Our results offer no empirical support for the hypothesis that minimum wage increases reduce the proportions of poor and low-income families. The evidence on both family income distributions and changes in incomes experienced by families indicates that minimum wages raise the incomes of some poor families, but that the net effect of higher minimum wages is, if anything, to increase the proportions of families that are poor and near-poor. Thus, it would appear that *reductions* in poverty or near-poverty should not be counted among the potential benefits of minimum wages. Returning to Gramlich's question posed in the Introduction, this suggests that the efficiency and equity effects of minimum wages point in the same negative direction.

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Table 1: Minimum Wages by State and Year

	1987	1988	1989	1990	1991	1992	1993	1994	1995
ME	3.65	...	3.75	3.85	4.25	...	...	...	...
NH	3.45	3.55	3.65	3.80	4.25	...	...	...	...
VT	3.45	3.55	3.65	3.85	4.25	...	...	...	4.50
MA	3.55	3.65	3.75	3.80	4.25	...	...	...	...
RI	3.55	3.65	4.00	4.25	4.45	...	...	...	...
CT	3.37	3.75	4.25	...	4.27	...	...	...	...
NY	3.35	...	...	3.80	4.25	...	...	...	...
NJ	3.35	...	...	3.80	4.25	5.05	...	...	...
PA	3.35	...	3.70	3.80	4.25	...	...	...	...
OH	3.35	...	...	3.80	4.25	...	...	...	...
IN	3.35	...	...	3.80	4.25	...	...	...	...
IL	3.35	...	...	3.80	4.25	...	...	...	...
MI	3.35	...	...	3.80	4.25	...	...	...	...
WI	3.35	...	...	3.80	4.25	...	...	...	...
MN	3.35	3.55	3.85	3.95	4.25	...	...	...	...
IA	3.35	...	...	3.85	4.25	4.65	...	...	...
MO	3.35	...	...	3.80	4.25	...	...	...	...
ND	3.35	...	...	3.80	4.25	...	...	...	...
SD	3.35	...	...	3.80	4.25	...	...	...	...
NE	3.35	...	...	3.80	4.25	...	...	...	...
KS	3.35	...	...	3.80	4.25	...	...	...	...
DE	3.35	...	...	3.80	4.25	...	...	...	...
MD	3.35	...	...	3.80	4.25	...	...	...	...
VA	3.35	...	...	3.80	4.25	...	...	...	...
WV	3.35	...	...	3.80	4.25	...	...	...	...
NC	3.35	...	...	3.80	4.25	...	...	...	...
SC	3.35	...	...	3.80	4.25	...	...	...	...
GA	3.35	...	...	3.80	4.25	...	...	...	...
FL	3.35	...	...	3.80	4.25	...	...	...	...
KY	3.35	...	...	3.80	4.25	...	...	...	...
TN	3.35	...	...	3.80	4.25	...	...	...	...
AL	3.35	...	...	3.80	4.25	...	...	...	...
MS	3.35	...	...	3.80	4.25	...	...	...	...
AR	3.35	...	...	3.80	4.25	...	...	...	...
LA	3.35	...	...	3.80	4.25	...	...	...	...
OK	3.35	...	...	3.80	4.25	...	...	...	...
TX	3.35	...	...	3.80	4.25	...	...	...	...
MT	3.35	...	...	3.80	4.25	...	...	...	...
ID	3.35	...	...	3.80	4.25	...	...	...	...
WY	3.35	...	...	3.80	4.25	...	...	...	...
CO	3.35	...	...	3.80	4.25	...	...	...	...
NM	3.35	...	...	3.80	4.25	...	...	...	...
AZ	3.35	...	...	3.80	4.25	...	...	...	...
UT	3.35	...	...	3.80	4.25	...	...	...	...
NV	3.35	...	...	3.80	4.25	...	...	...	...
WA	3.35	...	3.85	4.25	...	...	...	4.90	...
OR	3.35	...	...	4.25	4.75	...	...	...	...
CA	3.35	...	4.25	...	...	...	...	...	...
AK	3.85	...	...	4.30	4.75	...	...	...	...
HI	3.35	3.85	...	...	4.25	4.75	5.25	...	...

The higher of the state or federal minimum wage prevailing in May of each year is reported. To highlight changes minimum wages are shown only in the year of each increase, except for the first year. In the six years prior to 1987, Alaska and Connecticut had minimum wages above the federal minimum in all years, at $3.85 and $3.37 respectively. Maine raised its minimum to $3.45 in 1985 and $3.55 in 1986. All other states with a minimum higher than $3.35 in 1987 raised their minimum in

1987.

Table 2: Distributions of Observations

	Proportion of Total Number of Observations in Each Cell	Proportion of Observations in Each Cell with Minimum Wage Increases
	(1)	(2)
Overall sample	1.0	.25
Cells based on:		
Annual change in state unemployment rate		
Decline of 3% or more	.05	.05
Decline of 2-3%	.09	.11
Decline of 1-2%	.15	.20
Decline of 1% - Increase of 1%	.46	.22
Increase of 1-2%	.13	.39
Increase of 2-3%	.07	.36
Increase of 3-4%	.04	.41
Increase of 4% or more	.02	.73
Annual change in percentage of families in poverty in state		
Decline of 4% or more	.04	.15
Decline of 3-4%	.06	.25
Decline of 2-3%	.09	.36
Decline of 1-2%	.21	.21
Decline of 1% - Increase of 1%	.45	.25
Increase of 1-2%	.07	.27
Increase of 2-3%	.05	.28
Increase of 3% or more	.03	.18

Cells were chosen to provide a high level of disaggregation, while ensuring that each cell included observations both with and without minimum wage increases. In each range the lower limit is excluded and the upper limit included. The reweighting used in the following tables applies a weight to the treatment group in each cell to equalize the proportions in that cell in the treatment and control group.

Table 3: Estimated Effects of Minimum Wage Increases on Proportions in Income-to-Needs Ranges

	Income-to-Needs Categories							
	0-.5	.5-1	***0-1, In Poverty***	***1-1.5, Near-Poor***	***0-1.5, Poor/Near-Poor***	1.5-2	2-3	1.5-3
	(1)	(2)	***(3)***	***(4)***	***(5)***	(6)	(7)	(8)
Changes in proportions:								
No controls	.0005	.0079	***.0083***	***.0046***	***.0130***	-.0049	-.0071	-.0120
	(.0018)	(.0025)	*(.0035)*	*(.0027)*	*(.0040)*	(.0028)	(.0031)	(.0040)
No controls, exclude minimum wage increases in high unemployment years (1991 & 1992)	-.0013	.0049	***.0037***	***.0034***	***.0070***	-.0045	-.0025	-.0070
	(.0030)	(.0047)	*(.0064)*	*(.0053)*	*(.0084)*	(.0051)	(.0063)	*(.0084)*
No controls, exclude years with current or lagged federal minimum wage increases (1990-1992)	.0055	.0121	***.0176***	***.0102***	***.0278***	-.0028	-.0300	-.0328
	(.0040)	(.0058)	*(.0086)*	*(.0065)*	*(.0108)*	(.0071)	(.0088)	*(.0114)*
Reweighted to control for:								
Annual state unemployment rate changes	-.0001	.0078	***.0077***	***.0034***	***.0111***	-.0057	-.0057	-.0115
	(.0018)	(.0025)	*(.0035)*	*(.0027)*	*(.0040)*	(.0028)	(.0031)	(.0040)
Fixed state and year effects (proportional shifts)	.0002	.0069	***.0071***	***.0033***	***.0104***	-.0072	-.0074	-.0146
	(.0022)	(.0028)	*(.0039)*	*(.0034)*	*(.0046)*	(.0033)	(.0037)	(.0048)

The ranges of unemployment rate changes used to reweight are reported in Table 2. The numbers in parentheses are bootstrapped standard errors, based on 500 repetitions.

Table 4: Profiles of Families

	Income-to-Needs Categories				
	0-.5	.5-1	1-1.5	1.5-2	2-3
	(1)	(2)	(3)	(4)	(5)
Number of adult workers, proportions:					
0	.57	.51	.39	.28	.19
1	.39	.40	.44	.48	.46
2	.04	.08	.15	.22	.32
3	.002	.005	.008	.014	.025
4+	.000	.001	.001	.003	.005
Number of teenage workers, proportions:					
0	.91	.94	.95	.94	.92
1	.09	.05	.05	.05	.06
2	.003	.004	.005	.007	.009
3+	.000	.000	.000	.001	.001
Earnings of primary earners, households with at least 1 earner:					
Mean	2039	5205	8216	11070	15360
(Std. dev.)	(1832)	(2874)	(3973)	(5069)	(6798)
25th centile	830	3660	5885	8339	11290
Average earnings of non-primary earners, households with at least 2 earners:					
Mean	1621	3382	4600	5729	7547
(Std. dev.)	(2310)	(3136)	(3993)	(4651)	(5637)
25th centile	424	1015	1591	2257	3360

Income-to-needs categories and income measures are reported for year 1 for each family. All estimates are weighted. Incomes are measured in 1982-1984 dollars.

Table 5: Estimated Effects of Minimum Wage Increases on Changes in Income-to-Needs Ratios

	Income-to-Needs Ratio Change					
	< -1	-1 to -.5	-.5 to 0	0 to .5	.5 to 1	> 1
	(1)	(2)	(3)	(4)	(5)	(6)
Changes in proportions:						
Initial income-to-needs						
0-1.5	.004	.001	.023	-.010	-.011	-.007
1.5-3	.011	.001	.017	-.010	-.006	-.011
1.5-2	.015	.003	.016	-.004	-.014	-.016
2-3	.009	.000	.015	-.014	-.002	-.008

The panel reports the changes in the proportions experiencing the income-to-needs change implied by the density estimates.

Table 6: Simulated Effects of 10 Percent Minimum Wage Increase on Proportions in Income-to-Needs Ranges

	Income-to-Needs Categories					
	0-.1	1-1.5	0-1.5	1.5-2	2-3	1.5-3
	(1)	(2)	(3)	(4)	(5)	(6)
Standard elasticities (-.2), equal across income-to-needs categories:						
Employment elasticity	-.2	-.2		-.2	-.2	
Absolute changes in proportion						
From increased transitions out of minimum wage jobs	.0004	-.0001	.0003	-.0000	-.0001	-.0001
From decreased transitions into minimum wage jobs	.0003	-.0000	.0002	-.0000	-.0001	-.0001
Combined effect	.0006	-.0001	.0005	-.0001	-.0001	-.0002
Larger average elasticity (-1), equal across low income-to-needs categories:						
Employment elasticity	-1	-1		-1	-1	
Absolute changes in proportion						
From increased transitions out of minimum wage jobs	.0020	-.0005	.0015	-.0002	-.0004	-.0006
From decreased transitions into minimum wage jobs	.0013	-.0001	.0012	-.0001	-.0003	-.0004
Combined effect	.0032	-.0006	.0026	-.0003	-.0007	-.0010
Larger average elasticity (-1), elasticity smaller in low income-to-needs categories:						
Employment elasticity	-.4	-.4		-1.35	-1.35	
Absolute changes in proportion						
Combined effect	.0023	.0010	.0033	-.0002	-.0008	-.0011
Larger average elasticity (-1), elasticity larger in low income-to-needs categories:						
Employment elasticity	-1.5	-1.5		-.7	-.7	
Absolute changes in proportion						
Combined effect	.0040	-.0019	.0021	-.0003	-.0006	-.0009

The same elasticity is assumed for income-to-needs exceeding 3 as for income-to-needs of 1.5 to 3.

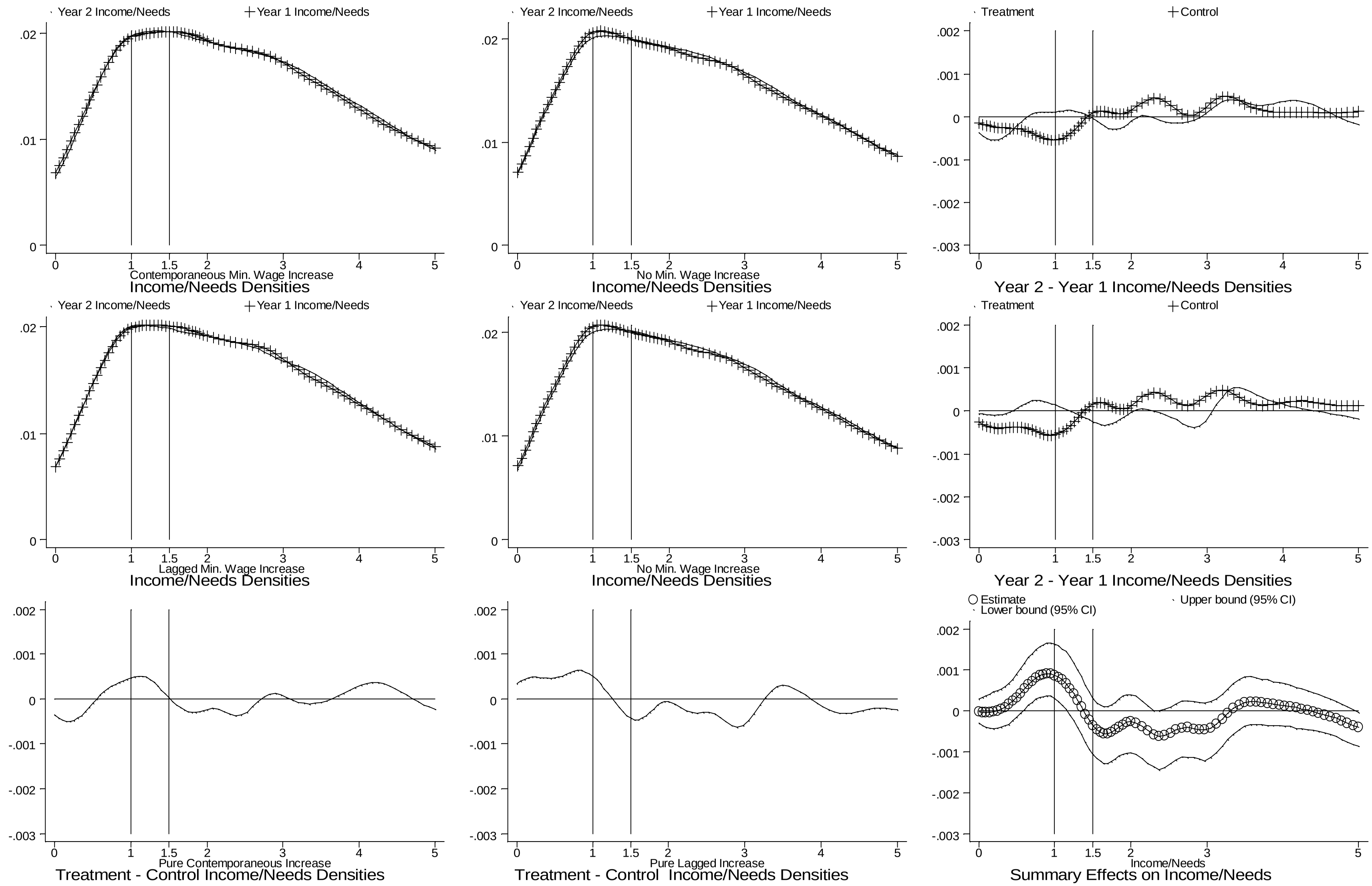


Figure 1: Min. Wage Effects on Income/Needs

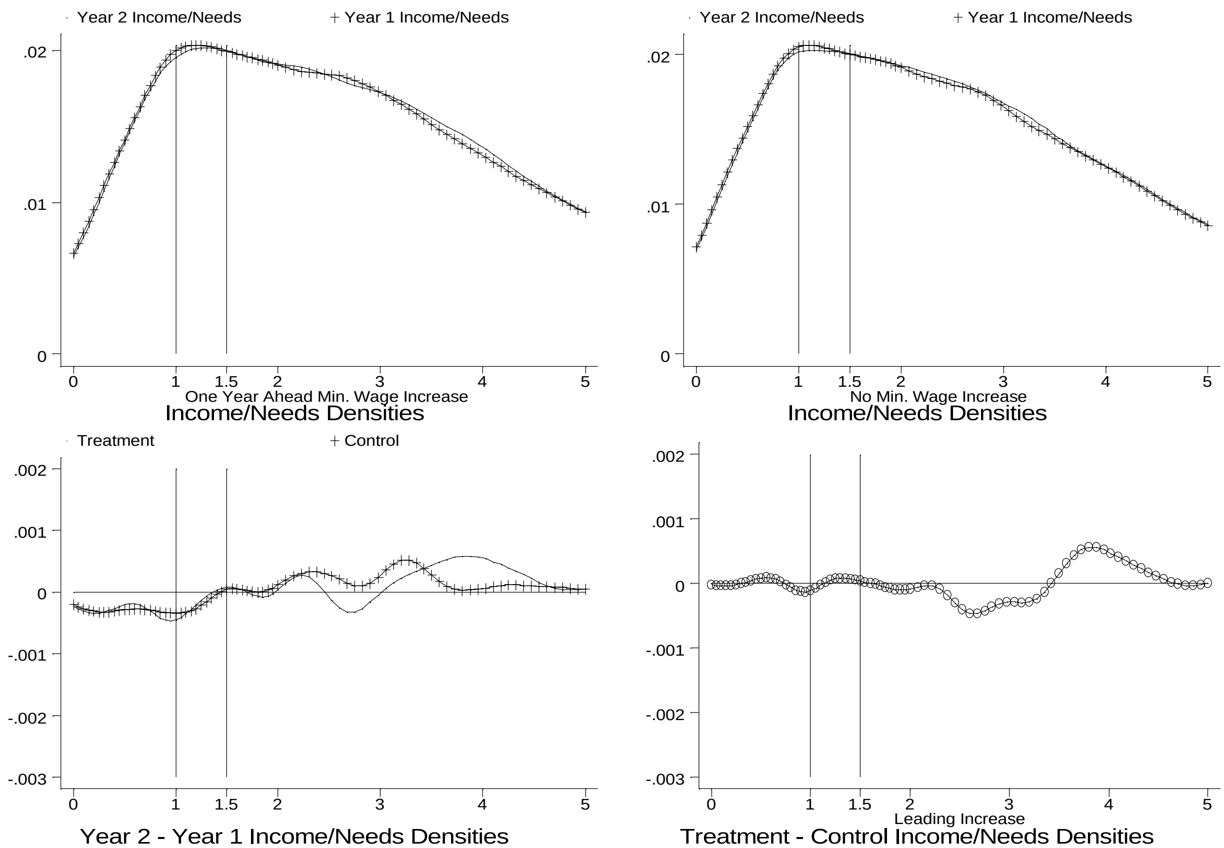
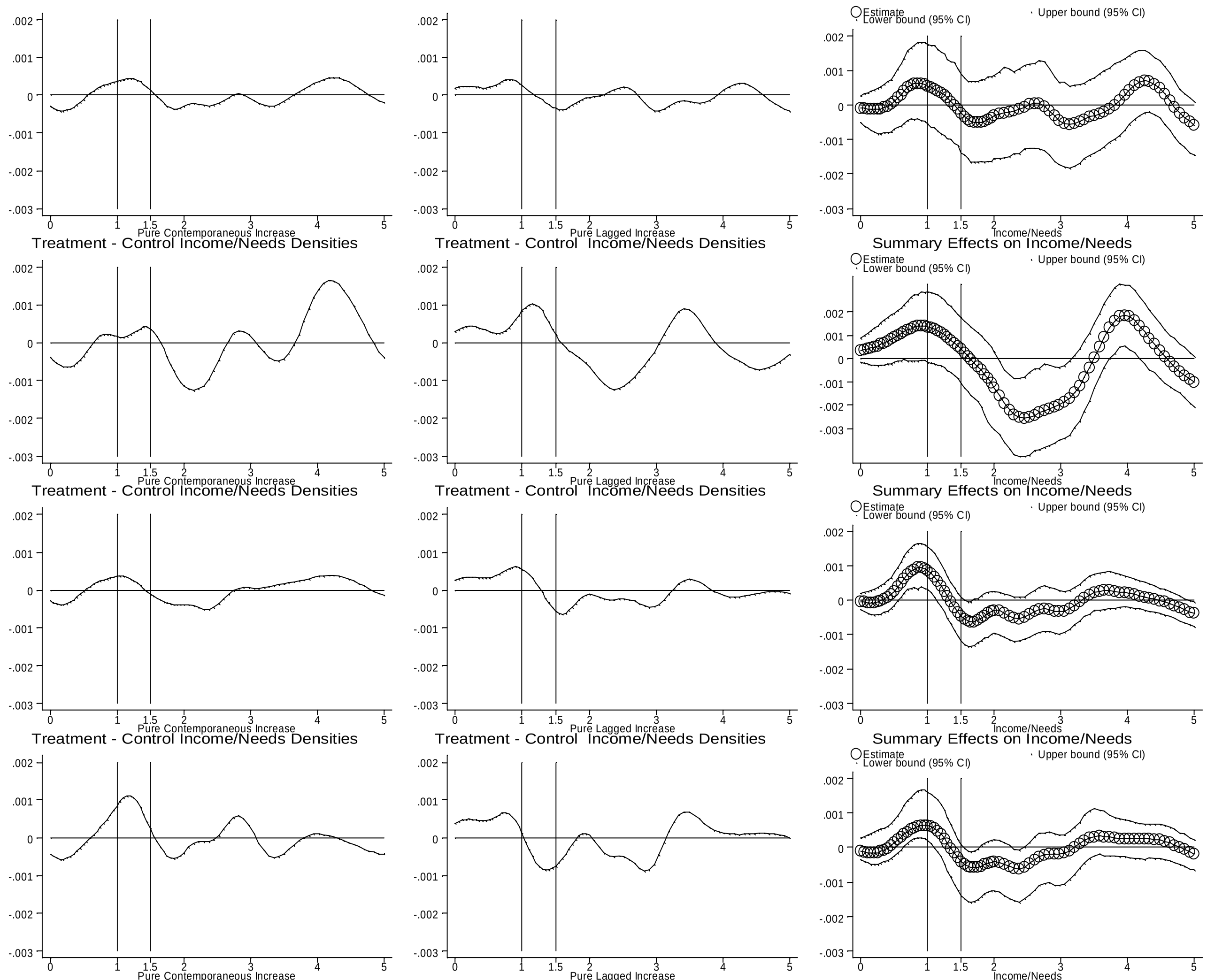


Figure 2: Leading Min. Wage Effects on Income/Needs



A. Excluding increases in high unemployment years

B. Excluding years of federal min. wage increases

C. Reweighted using unemployment changes

D. Fixed state and year effects

Figure 3: Minimum Wage Effects, Alt. Estimates

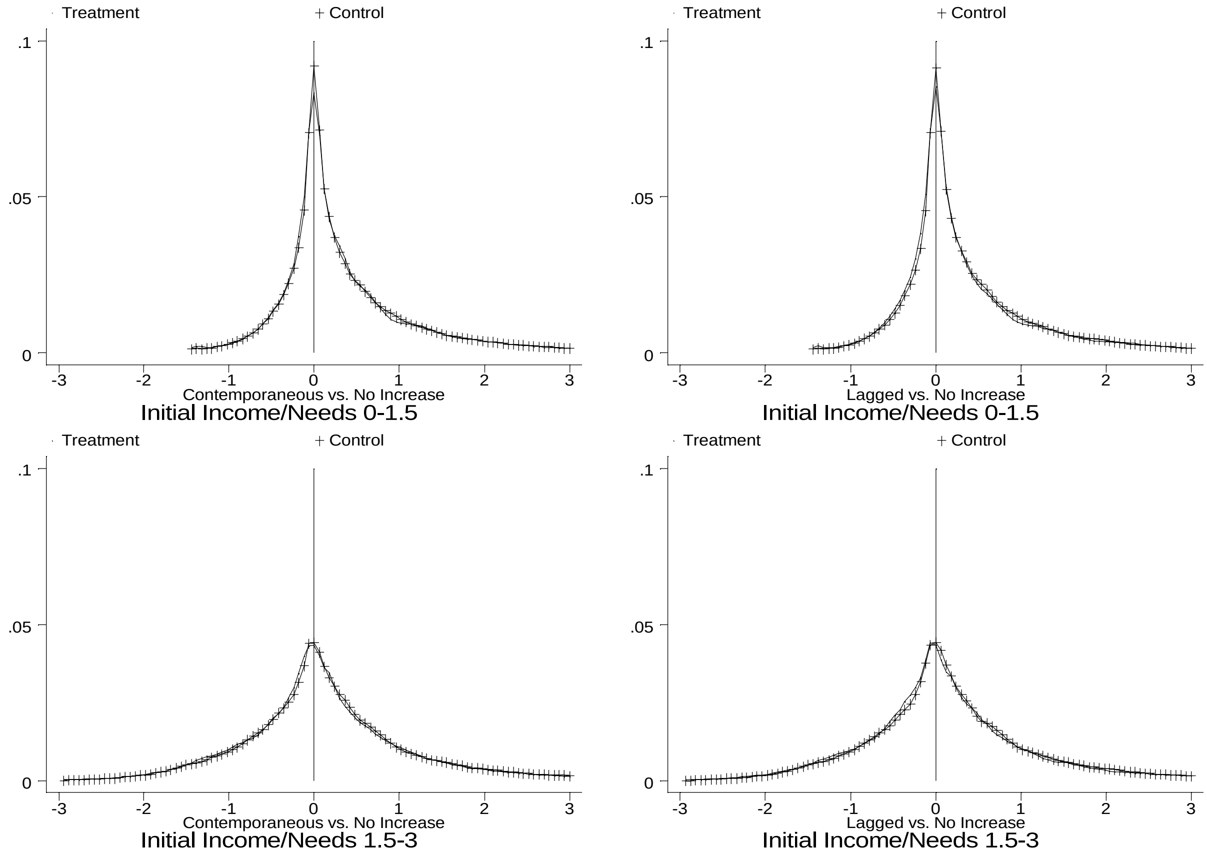


Figure 4: Densities of Change in Income/Needs

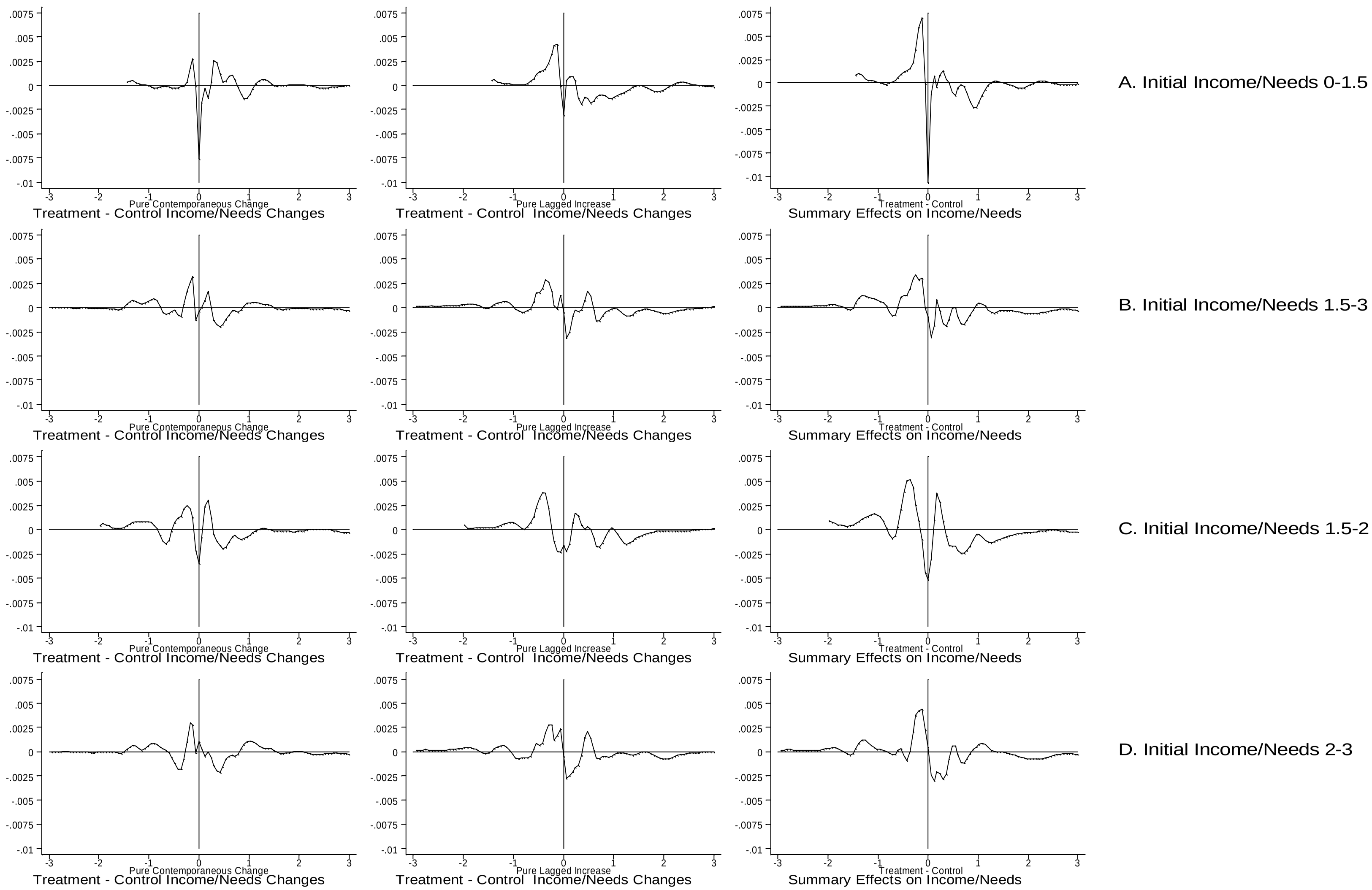
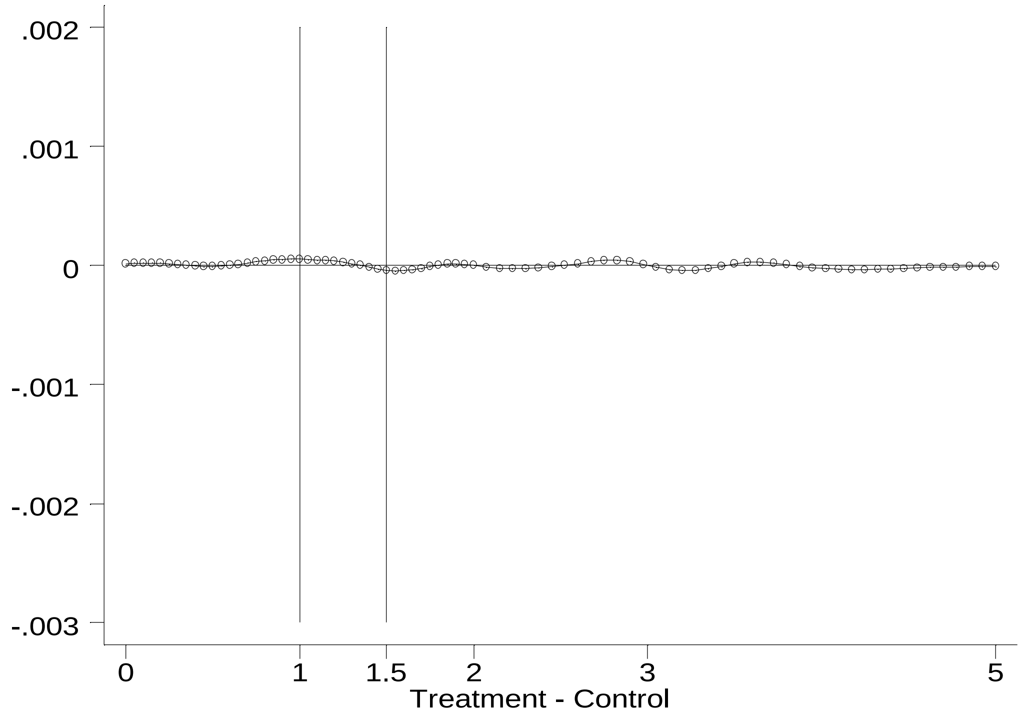


Figure 5: Min. Wage Effects on Changes in Income/Needs



App. Fig. 1: Mean of 1000 Random Assignment Estimates

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