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Can the minimum wage reduce poverty and inequality in the developing world? Evidence from Brazil

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ABSTRACT

Even though there is growing social support for higher minimum wages as anti-poverty policy tools, very little is known about their effectiveness in reducing poverty or inequality in the developing world. Latin America's largest economy offers a fertile setting for shedding light on the issue, in being a large and data-rich country where frequent increases in the minimum wage can allow for direct estimation of influence on the distribution of income. Using a difference-in-difference estimator that takes advantage of substantial regional income variation and 21 increases in the Brazilian national wage floor, the study finds that within three months of these minimum wage hikes, poverty and inequality declined by 2.8% and 2.4%, respectively. Influence waned over time, particularly with respect to bottom-sensitive distribution measures, a development that is consistent with resulting job loses that fell more heavily among poorer households. The fact that the following annual hike in the minimum wage led to a renewed decline in poverty and inequality, suggests that potential unemployment costs were again overwhelmed by benefits in the form of higher wages among working individuals. However, evidence also establishes an inelastic relationship between wage floor hikes and changes in the incidence of poverty, as well as diminishing returns to the strategy when the legal minimum is high relative to median earnings.

1. Introduction

Income distribution has steadily gained prominence in the public policy agenda and there is growing social support for higher minimum wages as anti-poverty tools (Dickens, 2015). In contrast to other policy instruments like a participation income, high-quality preschool, or a global tax on capital (Atkinson, 2015; Heckman, 2013; Piketty, 2014), tackling poverty and inequality through the minimum wage appears straightforward and costless in budgetary terms. In some emerging economies, debate has turned into decisive action through wage floors that have risen by substantial extents; in Latin America by over 50% (ILO, 2013).

This policy change occurred even though there is ample evidence that a higher minimum wage does not cause meaningful changes in poverty or income inequality, partially as a result of the instrument's poor target efficiency, at least in the developed world.[1] This result may not necessarily extend to other settings, but research based on emerging country data has to date focused on identifying wage floor influence on employment or hours (Belman & Wolfson, 2016). The sparse evidence aimed at ascertaining impact on the distribution of income is mixed and does not extend to potential effects on inequality. Gindling and Terrell (2010) find that the minimum wage has a modest effect on poverty that is circumscribed to households with workers engaged in sectors where the wage floor is enforced. Based on 1996 to 2001 Brazilian data, Neumark, Cunningham, and Siga (2006) do not find positive minimum wage effects on income.

In 2002, a new government was elected in Brazil with a mandate to impact the distribution of income in one of the world's most unequal societies. Social safety net programs were expanded and the minimum wage was raised gradually and consistently during the election cycles when the left-of-center Workers' Party governed the country. Poverty fell by two-thirds and inequality declined by a fifth, falling consistently for the first time in recorded history.[2] Developments also coincided with a robust economic

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[1] See Burkhouser and Finegan (1989), Burkhouser, Couch, and Glenn (1996), Burkhouser and Sabia (2007), Campolieti, Gunderson, and Lee (2012), Card and Krueger (1995), Sabia (2008), Sabia (2014), and Sabia and Burkhauser (2010).

[2] See Barros, Henriques, and Mendonça (2001), Ferreira, Leite, and Ravallion (2010), Sotomayor (2008), and Section 4.

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upturn and this study aims to isolate wage floor effects through a difference-in-difference approach that exploits regional income differences in a continent-sized country and data spanning 21 increases in the national minimum wage.

Beyond addressing the question of whether the minimum wage played a positive role in Brazilian distribution trends, this study contributes to the literature examining the impact of the wage floor on poverty and income inequality, rather than the well-researched issues of its potential unemployment effects or influence on wage distribution. It does so using a methodological approach that offers advantages over other strategies employed in the limited existing research on the distributional impact of the minimum wage. First, it does not rely on a sectoral analysis. Therefore, endogeneity in the setting of sectoral minimum wages or potential wage floor impact on unemployment or uncovered sectors does not present a threat to the internal validity of results. Second, wage floor influence on family income as well as on poverty and income inequality can be estimated. Third, the approach can accommodate estimation of treatment effects tied to the policy instrument's position in the wage distribution, lessening the need for extrapolation and allowing for more policy-relevant results. This feature is particularly relevant since position in the wage distribution is likely to influence wage floor effects on income, poverty, or inequality.

Resulting evidence in the form of visual difference-in-difference estimations suggests clear influence that is supported by statistical results based on conventional and synthetic controls. Within three months of a minimum wage hike, poverty and inequality decline by 2.8% and 2.4%, respectively. Influence recedes over time, particularly with respect to bottom-sensitive distribution measures, a development that is consistent with resulting job loses that fall more heavily among poorer households. Moreover, there is evidence of diminishing returns to the strategy when the wage floor is high relative to the median wage.

These assertions are supported by research that begins, in Section 2, with a survey of the literature on the relationship between income distribution and the minimum wage, focused on evidence from the developing world. Section 3 discusses the nature of minimum wage setting in Brazil and Section 4 lays out the data that are applied, in Section 6, to the evaluation of potential wage floor effects through a visual difference-in-difference estimator. The empirical strategy posited to identify influence through statistical means is discussed in Section 5 and implemented in Section 7 to estimate wage floor effects on income, inequality, and poverty, including impact on the expected value of the consumption floor among the poor. Section 8 applies alternative controls to examine robustness of results and external validity, and Section 9 establishes elasticities and determinants of treatment effects. The last section concludes with a summary and a discussion of policy implications.

2. Survey

Although there is no shortage of research on the minimum wage, a large share is dedicated to assessing unemployment effects in Europe and North America. One side of an important debate contends that increases in the minimum wage have little or no effect on employment (Allegretto, Dube, & Reich, 2011; Card, 1992a; Card & Krueger, 1994; Dickens, Riley, & Wilkinson, 2014; Dube, Lester, & Reich, 2010; Hoffman, 2016; Katz & Krueger, 1992), at least when they take place within a well-studied range of the wage distribution (Krueger, 2015). An opposing view argues that the wage floor raises unemployment among populations likely to be bounded by it–namely teens and the very low-skilled–and that findings of positive to no effects are generally associated with poor research designs (Neumark, Salas, & Wascher, 2014a; Neumark, Salas, & Wascher, 2014b). Much less controversial is the question of impact on the distribution of earnings, since the bulk of the evidence establishes an equalizing role (Autor, Manning, & Smith, 2016; DiNardo, Fortin, & Lemieux, 1996; Lee, 1999; Machin, Manning, & Rahman, 2003).

The same appears to be true in the developing world, where research also points to a link between the minimum wage and earnings distribution, as is the case in Mexico, where increased lower-tail inequality has been tied to an eroding wage floor (Bosch & Manacorda, 2010). Effects on wage levels and employment are, however, less certain. Harrison and Scorse (2010) conclude that an anti-sweatshop campaign in Indonesia led to lower employment and an increase in firm exits, but higher wages in surviving firms, a result that stands in some contrast to Alatas and Cameron (2008) who, exploiting wage floor differences in adjacent Indonesian counties, find that the minimum wage leads to employment loses only in small domestically-owned firms. Other studies find similarly nuanced results. Gindling and Terrell (2009) conclude that minimum wage hikes in Honduras lead to higher mean wages and employment losses in the formal sector, but no earnings changes in sectors where the wage floor is not effectively enforced.[3] Strobl and Walsh (2003) find gender-specific effects in Trinidad and Tobago, and Papps (2012) establishes disemployment effects in Turkey, but only in reference to one of three potential control groups. Another set of studies goes further in finding no evidence of negative employment consequences. Lemos (2004), Lemos (2007), and Lemos (2009) establish that Brazilian minimum wage hikes lead to higher and more equally distributed earnings without loss of employment, even among teenagers and other low-skilled populations. Similarly, introduction of an informal sector minimum wage in South Africa resulted in increased domestic worker wages without negative effects on hours or employment (Dinkelman & Ranchhod, 2012).

Getting a grip on employment costs versus the benefits of higher wages is one way of informing the debate on the effectiveness of the minimum wage as a policy tool. However, conclusions do not answer the questions of whether higher wage floors raise household income at the lower end of the distribution or whether they generate greater income equality. The issue is of particular interest in the developing world, where in the absence of comprehensive social safety nets, wage floor legislation is viewed as an important redistribution tool. Gindling and Terrell (2010) provide evidence regarding consequences on poverty in Honduras, in linking higher minimum wages to lower poverty among households with individuals engaged in sectors where the wage floor is enforced. This evidence stands in contrast to that established in Neumark et al. (2006), henceforth NCS, where the authors regress the 10th, 20th, and 30th percentiles of the distribution of per capita income on a minimum wage variable, state binaries, and year effects using a panel of Brazilian metropolitan areas and data from 1996 to 2001. No positive wage floor effects on household income are detected once minimum wage lags are included in the equation.

Conflicting evidence on the effects of the minimum wage should not be surprising, as results can be influenced by a number of factors, an important one being the wage floor's position in the distribution of earnings. In Brazil, for example, a minimum wage that almost doubled during the 2000s and 2010s could have led to greater positive (or negative) consequences on income, poverty, or inequality. Moreover, a finding of zero wage floor effects may signify that the minimum wage has no impact on distribution, but also that positive effects in, say, high-income regions of the

[3] Rather than becoming unemployed, workers who lose their jobs in the covered sector in Nicaragua tend to leave the labor force, become unpaid family workers, or find employment in the public sector (Alaniz, Gindling, & Terrell, 2011).

country are countered by negative effects in low-income ones. Latin America's largest economy offers a fertile setting for gathering evidence on the effectiveness of the minimum wage as a redistribution policy tool. It is a data-rich country where frequent increases in the wage floor can allow for direct estimation of minimum wage effects. A difference-in-difference estimator can exploit time series variation in comparable geographical areas to estimate causal effects. To the extent that impact is focused on policy-relevant ranges of the earnings distribution, research can produce evidence that is applicable beyond the case of Brazil.

3. Institutional setting

Brazil's constitution dictates that the wage floor be set at a level that "allows the worker to afford his or her family's basic necessities in the form of housing, nutrition, education, health, leisure, clothing, hygiene, transport, and retirement." Although the income requirements needed to conform to this directive are estimated by the *Departamento Intersindical de Estatística e Estudos Socioeconômicos*, the recommendations by this labor-backed entity are not binding and do not determine the size of minimum wage increases. Where they to be followed, the minimum wage in 2015 would be about four times as high as the prevailing floor.

As in the case of many developing economies, the minimum wage is more widely referenced in monthly rather than in hourly terms. Wage hikes take place annually at regular intervals, with some variation. That is, during the latter half of the 1990s, revisions became effective in May, but between 2000 and 2007 they were effective either in April or in May. Between 2008 and 2010 the effective month for the revisions transitioned to January, where it stood until the end of the period under consideration. The wage floor has no demographic or occupational exceptions and appears largely binding in the informal sector (Lemos, 2009).

Wage increases are neither determined by a minimum wage board nor by a panel of experts. Instead, they are enacted by Congress and are informed by the rate of inflation as well as by other considerations. For example, a large real wage hike in 1995 corresponded to the first increase after the country began a successful transition from hyperinflation in 1994. Thereafter, large increases are observed in the year preceding the 2002, 2006, and 2010 elections, and in the 2006 and 2010 election years. Beginning in 2012, the floor has been set in a more deterministic fashion through a formula that accounts for inflation during the year prior to a wage floor increase as well as for GNP growth, with a two-year lag. Since 2010 was a banner year for economic growth, this led to a sizable increase in the real minimum wage in 2012. These instances account for the largest real wage hikes during the period under consideration. The median increase of 9% is six times as large as the median increase over all other years.

Policy-makers view minimum wage revisions through the lens of real wages rather than position in the distribution–the more relevant metric used by expert panels. For this reason, substantial hikes relative to median earnings can be observed in other than the alluded years, particularly in periods of slow growth or contraction at the median. This is the case in 2003, 2009, and 2013 when increases relative to median earnings were not too far off from those taking place in election years. Although hikes appear to respond more to political than other considerations, the potentially non-stochastic nature of wage floor increases is addressed by the approach laid out in Section 5.

4. Data

Data are drawn from the 1995–2015 Monthly Employment Surveys (PMEs) conducted by the Brazilian Institute of Geography and Statistics (IBGE) in approximately 40,000 housing units located in the country's main metropolitan areas. The survey's design is similar to that of the Current Population Survey, where information is collected using rotation groups that are interviewed for four consecutive months and for another four months one year after first entering the sample, with some attrition associated with the fact that the sampling unit is the dwelling rather than its occupants. Individuals or families cease to be followed if they move, and partially as a result, the PME is characterized by high levels of attrition (Ribas & Soares, 2008). As a research tool, it is likely best regarded as a long and rich sequence of monthly cross-sections rather than a potential source of panel data.

In 2002, the number of rotation groups was increased from four to eight, variables were added, the content of some questions was modified, but variables of interest to this study did not suffer substantive changes. These include household size, composition, and earnings from primary employment and from all jobs for each member of the household. These variables are used to construct measures of "equivalent household income," arrived at by dividing household income by an equivalence scale of the form $(A_i+kC_i)^s$. A_i and C_i refer to the numbers of adults and children, respectively, in the i-th household, k to the resource cost of children relative to adults, and s is a parameter reflecting scale economies in the production of household goods. k and s are set to 0.4 and 0.9, respectively, where the former choice is the upper bound of the cost of children relative to adults as estimated by Deaton and Muellbauer (1986) using data from poor countries. The larger share of food costs relative to other expenses limits the extent of scale economies in developing countries, and the choice of s attempts to reflect the restriction. In comparison, the equivalence scale of the UK implies the parameter values k = 0.53 and s = 0.77 (Jenkins & Cowell, 1994) while an alternative US poverty measure explicitly chose k = 0.7 and s = 0.65–0.75 (Citro & Michael, 1995).

Equivalent income is weighted by the sum of the sample weights of household members, thus deriving a personal distribution of income under the premise that each member receives an equal share of the household's equivalent income. Since the assumption may be untenable for certain individuals such as boarders and domestic employees and their relatives, they are excluded from calculations. Incomes are deflated using the National Consumer Price Index (INPC) corresponding to the month when the PME was carried out, with a base period of August 1994–the month in which the precursor of the current monetary unit was introduced. The policies that accompanied the new currency dealt a decisive blow to the near hyper-inflationary conditions that characterized the 1980s and early 1990s. Research establishes a link between inflation, real wages, poverty, and inequality in Brazil (Ferreira et al., 2010; Hoffmann, 1995; McIntyre & Pencavel, 2004; Neri, 2006; Sotomayor, 2006), and to avoid interference from a potentially confounding factor, analysis is based on data from the macroeconomically stable period encompassed by the 1995–2015 PMEs. The upper bound reflects the last year of available PME data before the survey underwent major changes in geographical scope, among other modifications.

There are no official poverty thresholds in Brazil, and in their place the study adopts those developed by Rocha (1997); Rocha (2003), based on expenditures required to comply with a nutritional norm of 2,100 calories per person per day and augmented to incorporate non-food consumption. They take into account cost of living differences across Brazil's main metropolitan areas. At the higher end, metropolitan São Paulo has a poverty threshold of R $95.7, at the lower end Recife's threshold is R$82.5, the larger figure equivalent to a poverty line of about $3 per day at the dollar exchange rate prevalent at the time. Since the PME does not collect data on non-labor income, focus will be placed on changes rather than levels in distribution.

Income distributions are summarized by Atkinson- and Foster-Greer-Thorbecke-class distribution measures. Both classes of measures satisfy a number of attractive properties and are defined by a parameter that generates indices with diverse sensitivities to income changes at different points in the distribution. FGT(α) measures defined by parameter values over 0 satisfy the monotonicity axiom and those defined by values over 1 result in indices that also respect the transfer principle (Foster, Greer, & Thorbecke, 1984). In practice, the most common choices for α have been 0, 1, and 2 (Foster, Greer, & Thorbecke, 2010), producing measures that have been termed to reflect the incidence, depth, and severity of poverty, respectively (Ravallion, 1994). Ravallion (2016) proposes an additional summary measure intended to gauge success in raising income among the poorest households. The index aims to identify the lower bound of the distribution of consumption, where the expected value of the consumption floor (ECF) is derived to be equal to the product of the poverty threshold and the term $1\text{-}\frac{FGT(2)}{FGT(1)}$. Necessary and sufficient conditions for a rising consumption floor are larger proportionate declines in the FGT(2) index relative to those in the FGT(1) measure.

Atkinson-class inequality indices are also defined by a parameter that explicitly incorporates aversion to inequality, with higher values generating measures that are more sensitive to income changes at the lower end of the distribution. Amiel, Creedy, and Hurn (1999) find that a parameter value of 0.25 is consistent with elicited attitudes towards inequality; therefore, it is adopted for use in this study. Nonetheless, two other indices reflecting greater aversion to inequality and defined by parameters 0.5 and 0.75 are also incorporated to evaluate robustness of results.

Fig. 1 lays out estimates of income, poverty, and inequality for all 252 months in the 1995–2015 PMEs, with the top left panel presenting trends in equivalent income by quintile in the distribution. Besides spikes corresponding to a legally-mandated December bonus, income trends exhibit a close relationship to the business cycle, as reflected by short periods of expansion and contraction during the second half of the 1990s and during the early 2000s, followed by a robust upturn between 2004 and 2013, briefly interrupted in 2009 when the economy contracted slightly. Income gains evident during the expansion are substantial and, moreover, negatively correlated with position in the distribution. That is, equivalent income grew by 56% at the 80th percentile, but by 72%, 86%, and 97% at the 60th, 40th, and 20th percentiles, respectively. Developments translated to sharp reductions in poverty, whose incidence fell by 39%, its intensity by 36%, and its severity by 30%, with larger declines in the FGT(1) measure relative to those in the FGT(2) index consistent with income changes that benefited households closer to the poverty line than at the bottom of the distribution. During the 2000s and early 2010s, inequality fell by as much as 19%. In 2015 the economy entered a recession and all measures reflect some reaction to the downturn, particularly those most sensitive to income changes at the bottom of the distribution.

Clearly, economic growth–or its absence–can go a long way towards explaining income trends, but it does not explain the disproportionate benefits accruing to households at the lower end of the distribution of income or the unprecedented drop in inequality. A rising minimum wage could have played a role (Fig. 1), and information in Table 1 can help evaluate the policy tool's potential for impacting poverty and inequality. Columns 2–5 contrast characteristics of the working population with those of individuals earning no more than the minimum wage, where samples are constructed by retaining information from household members engaged in work for a positive wage. Columns 6–7 display coefficient estimates of a regression of the probability that a worker is bounded by the minimum wage on a set of binaries representing the traits in column 1. To provide insight on potential changes over time, information is disaggregated by time period, defined as pertaining to data from the 1995–97 or the 2013–15 PMEs.

Results establish that the wage floor is more likely to be binding among dependents, the young, and the low-skilled. An overlap

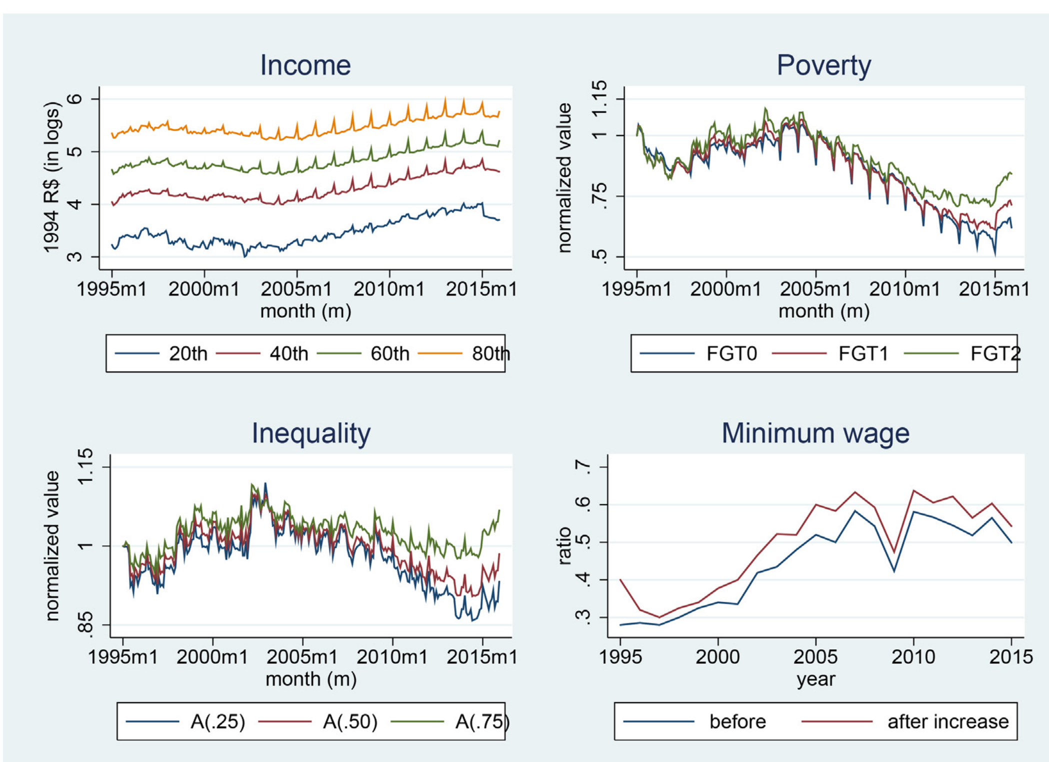

Fig. 1. .

Table 1
Distribution (%) of the working population and probit coefficients estimating probability of working for the minimum wage.

Characteristic	All wage earners 1995–97	All wage earners 2013–15	MW earners 1995–97	MW earners 2013–15	Probit β's 1995–97	Probit β's 2013–15
Aged < = 18	6	2.3	19.3	7.5	0.67	0.76
19–24	15.6	11.1	19.9	15	0.19	0.29
25–44	55.7	50.7	41	40.8	−0.01	0†
45–64	21.2	32.9	17.4	32.4	–	–
65+	1.6	3	2.4	4.4	0.41	0.21
Male	61.4	54.1	45.6	39	–	–
Female	38.6	45.9	54.4	61	0.47	0.48
Primary education	33	10.3	54.2	22.4	0.93	0.7
Intermediate	27.4	19	32.7	29.9	0.56	0.41
High School	24.6	42.4	12.2	42.9	–	–
College	14.9	28.3	0.9	4.7	−0.79	−0.88
Householder	50	47.9	28.2	39.2	–	–
Spouse	17.7	23.5	22.1	25	0.2	0.05
Son/daughter	26.3	23.5	41.1	28.6	0.47	0.27
Other relative	6.1	5	8.7	7.2	0.32	0.19
Formal sector	66.5	80.3	39	57.3	–	–
Informal	33.5	19.7	61	42.7	0.39	0.38
Recife resident	6.3	6.5	16.9	15.9	1.2	1.07
Salvador	5.7	7.3	17.2	20.2	1.32	1.17
Belo Horizonte	10.2	10.4	13.5	11.4	0.6	0.43
Rio de Janeiro	24.7	23.8	27	26.1	0.59	0.52
São Paulo	44.6	43.7	19.1	21.5	–	–
Porto Alegre	8.5	8.3	6.2	5	0.36	0.08

Source: Author's calculations from person-based PME data.
Note: †Not significant at the 95% confidence level.

between these populations could limit the policy tool's targeting effectiveness, as is the case in the US, where the bulk of minimum wage workers are second- and third-income earners in non-poor households (Sabia, 2014). However, regression coefficients in columns 6–7 suggest that this is less likely to be the case in Brazil, as the link between minimum wage work and education is shown to go beyond a correlation with age and dependent status. Further bolstering the wage floor's potential for impacting the Brazilian distribution of income is the large size of the low-skilled population. At the beginning of the period under analysis, for example, only 40% of the metropolitan area workforce had more than eight years of schooling (column 2).

Geography is another important factor associated with earning no more than the Brazilian wage floor, with residents of Salvador and Recife accounting for 12% of the country's metropolitan workforce, but over a third of its minimum wage workers. The phenomenon is not strictly a consequence of the regions' lower schooling and higher levels of informality, since it is also established by large and positive probit coefficients that reflect incidence relative to the base region of São Paulo. Race could be playing a role, but assessment of its impact is hindered by the fact that the variable is unavailable in pre-2002 PME data.

Statistically significant declines in probit coefficient values between 1995–97 and 2013–15 establish a weakening in the relationship between minimum wage work, low skill, geography, and dependent status. Information in columns 4–5 reflects this development, in showing an expansion in the share of minimum wage workers with a high school education, residing in São Paulo, and classified as heads of household. This last development is supported by a statistically significant fall in the incidence of minimum wage work among spouses and dependents relative to incidence among heads of household, and an increase, from 28% to 39%, in the prevalence of minimum wage workers reported to be heads of household.[4]

[4] All changes are statistically significant at the 95% confidence level.

5. Empirical approach

Identification of minimum wage effects involves comparisons of income distributions observed after increases in the wage floor with distributions depicting what would have obtained in the absence of the hikes. Differences between these distributions define impact on poverty, inequality, income deciles, or other measures of interest. Research aimed at estimating effects on unemployment, for example, has taken advantage of regional wage floors to arrive at the required counterfactuals. If the legal minimum were to be increased in one region of a country but not in others, subsequent disparities in changes in unemployment rates across areas could be associated with the rise in the wage floor. Large income disparities that characterize a continent-sized country such as Brazil can also provide for an effective identification strategy, as increases in the national wage floor will be more binding in lower-income regions than in higher-income ones (Card, 1992b). If differencing controls for factors than can confound an evaluation of the policy instrument's impact on the distribution of income, dissimilarities in changes in outcome variables would follow from variation in the degree of the 'bite' of the minimum wage.

Estimation of impact through this approach could be as straightforward as differencing changes in outcomes in treated and control areas relative to differences in the minimum wage. Alternatively, a regression approach could be used to arrive at the same answer, with added benefits. That is, a statistical approach would allow for introduction of controls necessary for generating valid counterfactuals as well as estimation of standard errors.

$$Y_{gt} = \alpha + \beta_1 P_t + \beta_2 T_g + \beta_3 P_t \times T_g + \sum_{y=1996}^{2015} \beta_y year_t + \varepsilon_{gt} \quad (1)$$

Difference-in-difference effects arrived through regressions can be estimated using Eq. (1), where the dependent variable Y_{gt} measures income, poverty, or inequality in region g at time t, and where observations of regions being 'treated' with a higher minimum wage are stacked over the corresponding ones from a suitable control group. T refers to a binary variable identifying the treatment group, the post-treatment variable P equals zero on the month prior to a minimum wage hike and one a number of months thereafter, say, three or nine months after a wage floor increase. These take place on an annual basis in Brazil and the equation adds binary variables indexed by y = 1996, 1997, 1998, ... 2015. When estimation is carried out using observations where P equals either zero or one, the coefficient of the interaction term $P \times T$ yields the treatment effect over the 21 minimum wage hikes encompassed by the 1995–2015 PMEs. With only one post-treatment period observation, standard errors can be large and to arrive at more precise estimates of effects, impact can be evaluated using a more flexible specification that allows for a greater number of post-treatment period observations.

$$Y_{gt} = \alpha + \sum_{j=1}^{4}\beta_j P_{jt} + T_g + \sum_{j=5}^{8}\beta_j P_{(j-4)t} \times T_g + \sum_{y=1996}^{2015}\beta_y year_t + \sum_{y=1996}^{2015}\zeta_y(year_t \times T_g) + \sum_{m=1}^{11}\gamma_m month_t + \sum_{m=1}^{11}\delta_m(month_t \times T_g) + \varepsilon_{gt} \quad (2)$$

Rather than assuming that the impact of the minimum wage is observed within three months or within a longer span that allows more time for unemployment effects to be manifested, Eq. (2) evaluates influence at a range of points over time. Post-treatment is defined using dummies (P_1 to P_4) indexing the months encompassed by the first, second, third, and fourth trimesters following a minimum wage increase, respectively.[5] When the regression is based on a dataset containing all monthly observations, the coefficients of $P_1 \times T$ to $P_4 \times T$ (β_5 to β_8) yield minimum wage effects by trimester. The remaining variables in Eq. (2) add year and month effects that are also allowed to vary across regions.

Since there are no variables with within-group variation, the model, specified with the equivalent of collapsed data by metropolitan area and month, is analogous to the Donald and Lang (2007) estimator when the number of cluster samples is small. As with any estimation method, identification of causal effects depends on adequate controls, and in a difference-in-difference setting, it translates to a common trend assumption. Its chances of holding true can be improved by basing estimates on metropolitan areas that are more likely to react to structural and cyclical factors in a similar manner, and Salvador and Recife appear to be natural comparison groups. They are the main metropolitan areas of the Brazilian northeast. Compared to all other areas surveyed by the PME, they are much poorer, labor force participants are more likely to be younger, non-white, less educated, and working in the informal sector. Rio de Janeiro and São Paulo also stand out as comparable units, in being Brazil's largest metropolitan areas and the country's centers of economic power. Earnings in Rio de Janeiro (Recife) are lower than in São Paulo (Salvador).[6]

Abadie, Diamond, and Hainmüeller (2010) follow an alternative control group approach motivated by an evaluation of the effects of anti-smoking initiatives implemented in California. In the absence of an obvious control state, the authors propose an estimator for arriving at the weighted combination of US states that best matches California's baseline characteristics. The policy instrument's effects are then determined by comparing post-treatment smoking behavior in California with that in the synthetic control. In the context of this study, the approach can provide a more objective basis for comparison group selection, and potentially, for improving on controls. With these objectives in mind, synthetic control groups for Rio de Janeiro and Recife are estimated based on characteristics that are deemed to be important determinants of income distribution. These include household size and composition, labor force participation, human capital and gender makeup of the workforce, and institutional and social factors such as size of the informal sector as well as racial and gender gaps.

Results confirm priors in terms of comparability of proposed treatment and control regions. That is, the synthetic control for Recife turns out to be 60% Salvador. São Paulo is an even more important donor in Rio de Janeiro's synthetic control, that is also 22% Northeastern Brazil, 2% Belo Horizonte, and 7% Porto Alegre. Rio de Janeiro and São Paulo account for some two-thirds of the population surveyed by the PMEs, and primary results will therefore be based on Rio de Janeiro as the treated region and São Paulo as the control. However, Section 8 will also examine effects based on alternative selections, including a synthetic control. In all cases, the common trend assumption is relaxed in Eq. (2) by including state-specific trends. Also, falsification tests will evaluate the validity of causality claims associated with estimated minimum wage effects.

Alternative strategies such as those based on a minimum wage variable reflecting treatment intensity across a panel of states could also identify a treatment effect, assuming that conditions required for unbiased estimation were to be met. In Brazil, however, influence will reflect an average across a large and diverse country. As a result, a finding of zero effects can signify that the minimum wage has no impact on distribution, but may also reflect positive effects in some regions that are countered by negative ones in others. Wage floor influence is also likely to vary by the wage floor's position in the earnings distribution. The proposed difference-in-difference approach can directly identify impact in policy-relevant ranges, where one is embodied by estimation of the consequences of raising the minimum wage within the range observed in São Paulo and Rio de Janeiro. In the former region, the wage floor is equivalent to 43% of median earnings among full-time salaried workers and over the period under examination, as compared to a figure of 49% in the latter region.

6. Preliminary evidence

Another attractive feature of the proposed empirical strategy is that it lends itself to visual representation that can complement and direct statistical analysis. A first test can involve ascertaining whether wage floor hikes have a potentially meaningful influence anywhere on the distribution of earnings, an effect that can then translate to impact on the household. To arrive at an answer, probability density functions of earnings are derived from samples constructed by retaining wage information from all household members engaged in full-time paid work. Density functions corresponding to the month prior to a minimum wage increase are subtracted from those manifested six months later. The data span 21 wage floor increases and to conserve space, differences are presented in two-year intervals and for the main treatment unit, Rio de Janeiro. Earnings are normalized using their median on the month prior to the wage floor increase and initial and revised wage floors are shown in vertical lines.

[5] The base period is the month prior to a wage floor increase. Hence, the fourth trimester dummy will encompass two months if a wage floor increase takes place 12 months after the previous one. During the period under analysis, 5, 15, and 1 wage floor increases took place within the space of 11, 12, and 13 months, respectively. Of the 21 increases, seven took place in the month of May, six in January and in April, and one in February and in March.

[6] Over the period under analysis, the median wage in Rio de Janeiro (Recife) among full time workers is equivalent to 86% (95%) of the prevailing one in São Paulo (Salvador).

Visual evidence in Fig. 2 demonstrates that minimum wage hikes have appreciable impacts on the distribution of earnings, especially when increases are large or when the wage floor is high relative to median earnings. Except in the late 1990s, when minimum wage hikes were small, density mass clearly falls around the initial value of the wage floor and increases around the revised one, with changes that overshadow those taking place elsewhere in the distribution. Moreover, there is evidence of spillover effects. That is, density mass increases are evident around the revised wage floors, but also around twice their values, a phenomenon likely associated with the fact that the legal minimum can act as a numéraire during times of high inflation. After years of macroeconomic stability, the practice of expressing wages as multiples of the minimum wage has gradually fallen out of use, providing a potential explanation for the diminishing influence of spillover effects over time that is also evident in the graphs.

To evaluate whether these changes may translate to impact on poverty and income inequality, probability density functions of household income on the month prior to a minimum wage increase are subtracted from those manifested six months later. Total rather than equivalent household income is used as the unit of observation in order to facilitate portrayal of a minimum wage that is expressed in total currency units rather than in equivalent adult terms. Incomes are normalized using their median on the month prior to the wage floor increase and initial and revised wage floors are depicted in vertical lines. To allow examination of potential spillover effects or impact among households with two minimum wage earners, the figures also display vertical lines corresponding to twice initial and revised wage floor values.

Two features of the differenced densities in Fig. 3 stand out. The first is clear evidence of changes in density mass around income levels corresponding to initial and revised minimum wage values. During the late 1990s, some minimum wage impact is observed among households with incomes close to the bottom decile of the distribution, whereas during the 2000s and the early 2010s, clear positive effects are observed closer to the 20th percentile. The second feature of the density changes evident in Fig. 3 is the presence of minimum wage influence at higher levels of the distribution. That is, density mass increases are also observed among households with incomes equivalent to twice the minimum wage, a phenomenon that generates change at the 35th-40th percentile range and is either product of spillover effects or of household units with multiple minimum wage earners.

Causal inference requires counterfactuals derived from appropriate control groups, and density changes are therefore differenced using corresponding changes taking place in São Paulo to produce visual difference-in-difference estimates presented in Fig. 4. In general, these demonstrate that use of the control group reduces the amplitude of density mass changes, suggesting that income growth manifested in simple differences cannot be entirely attributed to the minimum wage. Still, appreciable impact remains after controlling for factors that can play a role in distribution other than wage floor hikes. The following section ascertains the direction, size, and statistical significance of effects.

7. Results

Table 2 presents estimates of treatment effects based on Eq. (1), Rio de Janeiro as the treated region, and São Paulo as the control. These establish that minimum wage hikes lead to reductions in poverty as defined by all three measures proposed in Section 4. Relative to levels evident on the month prior to a wage floor hike, poverty falls by 3.8% and 4% three and nine months after an increase, respectively, when treatment effects are averaged over all three measures of poverty. In contrast, impact on inequality is more subdued, reflecting declines of 1.7% three and nine months after treatment, when averaged across the three measures of inequality. As expected, some standard errors are large. Tests also indicate that they are potentially biased by autocorrelation and

Fig. 2. .

Fig. 3. .

Fig. 4. .

heteroskedasticity that is detected in most time series, and as a result, inference in Tables 3–6 is based on autocorrelation- and heteroskedasticity-robust standard errors.

Estimates of treatment effects based on Eq. (2), shown in Table 3, establish that within three months of a minimum wage increase, poverty falls by an average of 2.8% across all three poverty

Table 2
Minimum wage treatment effects 3 and 9 months after an increase and average baseline levels of poverty and inequality.

Measure	After 3 months		After 9 months		Pre-treatment
	TE $\times 10^3$	s.e.$\times 10^3$	TE$\times 10^3$	s.e.$\times 10^3$	$\times 10^3$
FGT(0)	−13.2	6.9	−12.4	9.8	391
FGT(1)	−9.5	4.1	−10	4.6	237
FGT(2)	−7.7	3.4	−8.7	3.4	193
ECF	−30.8	354	183	530	16982
A(.25)	−3.4	2	−3.1	2.4	176
A(.50)	−5.9	3.3	−6	3.6	351
A(.75)	−8.8	4.7	−10.3	4.5	584

Source: Author's calculations from household-based PME data.

Table 3
Minimum wage treatment effects $\times 10^3$ by quarter after increase (robust s.e. $\times 10^3$ in parentheses).

Measure	Quarter			
	First	Second	Third	Fourth
FGT(0)	−11.0	−10.8	−11.8	−5.9
	(5.0)	(5.6)	(5.2)	(4.4)
FGT(1)	−6.9	−6.7	−6.0	−2.6
	(3.4)	(3.6)	(3.2)	(2.9)
FGT(2)	−5.1	−4.5	−3.7	−1.1
	(2.9)	(3.0)	(2.7)	(2.5)
ECF	−245	−383	−623	−601
	(290)	(342)	(345)	(317)
A(.25)	−5.4	−5.7	−3.9	−4.6
	(2.0)	(2.2)	(2.2)	(2.5)
A(.50)	−8.6	−8.4	−5.9	−6.6
	(3.0)	(3.3)	(3.3)	(3.8)
A(.75)	−9.7	−8.3	−5.9	−5.3
	(4.1)	(4.4)	(4.1)	(4.5)

Source: Author's calculations from household-based PME data.

Table 4
Minimum wage treatment effects by quarter after increase and percentile in the distribution of equivalent income (robust s.e. in parentheses).

Percentile	Quarter			
	First	Second	Third	Fourth
90th	1.4	−1.0	2.9	−7.4
	(7.6)	(8.1)	(9.0)	(10.0)
80th	5.7	5.6	8.9	0.9
	(3.7)	(4.1)	(5.0)	(5.5)
70th	5.4	5.3	9.0	6.3
	(2.7)	(3.1)	(3.7)	(3.9)
60th	3.2	3.6	5.7	3.5
	(2.0)	(2.3)	(2.9)	(2.9)
50th	2.6	3.7	5.6	3.4
	(1.7)	(1.9)	(2.3)	(2.3)
40th	3.3	3.9	5.4	3.5
	(1.4)	(1.6)	(1.7)	(1.8)
30th	3.0	3.4	4.3	2.3
	(1.2)	(1.3)	(1.4)	(1.5)
20th	2.9	3.1	3.7	2.3
	(1.2)	(1.3)	(1.2)	(1.3)

Source: Author's calculations from household-based PME data.

measures. Estimates also point to an erosion of influence over time that is particularly pronounced when poverty is aggregated by the income- and distribution-sensitive FGT(2) measure. According to this index, the reduction in poverty associated with a wage floor hike recedes from 2.6% to 2.3% between the first and second quarter after a wage floor increase, and drops from 1.9% to 0.6% between the third and fourth quarter. This compares to impact on the headcount ratio that falls from 2.8% in the first quarter to 1.5% in the fourth. The precision with which influence is estimated is also associated with the sensitivity of the distribution measure to income changes at the lower end of the distribution. That is, when poverty is aggregated through use of the headcount ratio, minimum wage impact is statistically significant three quarters after a hike. In contrast, use of the FGT(1) index leads to treatment effects that are statistically significant at the 95% confidence level one quarter after an increase and at the 90% confidence level two quarters thereafter. First-quarter effects reflected by the FGT(2) index are significant only at the 90% level of confidence. Influence that recedes more quickly and becomes statistically indistinguishable from zero in relation to a measure's aversion to poverty is consistent with a policy tool that is more likely to help households cross the poverty line than impact the income of the poor, or that

Table 5
Concurrent and lagged minimum wage effects ×10 by percentile in the distribution of per capita income, 1996–2001 (robust s.e. ×10 in parentheses).

	Lags (in quarters)			
Percentile	0	1	2	3
		All metro areas		
20th	−17.8	−76.3	−29.3	43.2
	(33.3)	(33.3)	(31.8)	(30.9)
30th	−6.9	−94.0	−47.3	23.9
	(36.3)	(36.4)	(33.9)	(33.0)
		Salvador and Recife		
20th	−45.7	−105.1	0.2	4.3
	(38.1)	(38.7)	(37.9)	(37.2)
30th	−61.2	−112.4	−9.4	−16.4
	(38.9)	(39.4)	(38.4)	(38.7)
		São Paulo and Rio de Janeiro		
20th	231.1	−70.9	−65.7	51.9
	(148.6)	(146.3)	(107.4)	(89.3)
30th	122.2	−47.5	−183.0	−21.3
	(166.2)	(163.6)	(120.6)	(100.6)
		All metro areas		
40th	−44.6	−99.7	−75.6	33.0
	(46.1)	(45.9)	(42.0)	(41.0)
50th	8.3	−134.9	−113.1	−30.0
	(55.4)	(55.4)	(48.8)	(47.4)
60th	−15.0	−190.5	−134.6	−37.9
	(71.5)	(71.6)	(63.3)	(61.7)
70th	31.6	−205.2	−102.2	−92.3
	(99.3)	(99.3)	(88.6)	(86.3)
80th	−30.7	−433.0	−190.8	−2.8
	(180.9)	(180.6)	(162.1)	(157.4)
90th	−135.9	−899.6	−326.4	11.7
	(375.3)	(374.3)	(339.3)	(333.7)

Note: Three-stage generalized least squares estimates of a regression of per capita income on the share of the workforce bounded by the minimum wage, lags (in quarters), and metropolitan area and month indicators.
Source: Author's calculations from household-based PME data.

of households at the very bottom of the distribution, where non-working family units are more likely to be found.

The negative, though imprecisely-estimated coefficients associated with the consumption floor variable, provide some statistical support to the hypothesis whereby a higher wage floor benefits households near the poverty threshold, leaving lower-income ones behind. Moreover, the fact that coefficients are most precisely estimated in the third and fourth quarters after a wage floor hike, when unemployment effects are more likely to be fully manifested, suggests that employment losses could be playing a role in the decline in the expected consumption floor among households that do not cross the poverty line.

Results in Table 3 also establish first-quarter effects on inequality that range from reductions of 1.7% to 3.1%, depending on the sensitivity of the measures to changes at the lower end of the distribution. As in the case of poverty, effects recede over time, so that by the fourth quarter after a wage floor hike, influence on inequality drops by 15%, 23%, and 45% relative to measurement by the A(.25), A(.50), and A(.75) indices, respectively. The precision with which impact is estimated is also associated with a measure's aversion to inequality. That is, whereas all three inequality indices reflect precisely-estimated minimum wage effects two quarters after a wage floor hike, third- and fourth-quarter effects are statistically significant at the 90% confidence level with respect to the A(.25) and A(.50) measures, but are not significant at any commonly-used level of confidence relative to the more bottom-sensitive A(.75). Inflation and resulting real wage erosion that is more pronounced at the lower end of the distribution could provide an explanation for the drop-off of wage floor impact on inequality and on poverty. However, results are also consistent with a process whereby the benefits of minimum wage increases are countered by rising unemployment that falls more heavily at the lower end of the distribution. The fact that the following annual hike in the minimum wage leads to a renewed decline in poverty and inequality, suggests that unemployment costs are then again overwhelmed by benefits in the form of higher real wages among working individuals.

Visual difference-in-difference estimates in Fig. 4 suggest that the evidence regarding minimum wage effects is causal. That is, changes associated with the minimum wage appear to be circumscribed to the lower end of the distribution, as should be the case if the identification strategy adequately controls for confounding factors such as the influence of the business cycle. An evaluation of statistical significance of income changes at points beyond the potential reach of the minimum wage can constitute a falsification test, and to that end, Table 4 presents estimates of wage floor effects by decile in the distribution of equivalent income. These confirm the conclusions of the visual analysis. Robust evidence of impact of the policy tool is evident only at and under the 40th percentile of the distribution of income, where statistical tests reject the null hypothesis that first to third (and fourth) quarter treatment effects are jointly statistically indistinguishable from zero at the 95% level of confidence.

These results stand in contrast to those of Neumark et al. (2006) who did not establish wage floor impact at the 20th or 30th percentiles of the Brazilian distribution of per capita income. To attempt to reconcile disparate findings, per capita household income is regressed on a minimum wage variable indicating the share of the workforce earning no more than the wage floor. Minimum wage lags and binaries representing metropolitan areas and months are also included, and effects estimated allowing for heteroskesdastic errors, correlation across panels, and panel-specific AR(1) processes. Also as in NCS, monthly data span 1996 to 2001 and all metropolitan areas surveyed by the PME.

Table 6
Minimum wage treatment effects $\times 10^3$ by quarter after an increase and by comparison groups (robust s.e. $\times 10^3$ in parentheses)†

Measure	Quarter			
	First	Second	Third	Fourth
		Rio de Janeiro versus Synthetic		
FGT(0)	−9.5	−9.4	−11.3	−7.9
	(4.8)	(5.3)	(4.8)	(4.3)
FGT(1)	−6.3	−5.3	−5.1	−3.6
	(3.2)	(3.4)	(2.9)	(2.8)
FGT(2)	−4.8	−3.2	−2.8	−1.9
	(2.7)	(2.8)	(2.5)	(2.4)
ECF	−134	−400	−646	−569
	(235)	(286)	(298)	(295)
A(.25)	−6.2	−6.8	−7.1	−5.5
	(1.9)	(2.0)	(2.1)	(2.3)
A(.50)	−8.7	−8.4	−7.8	−7.6
	(2.7)	(3.0)	(3.0)	(3.3)
A(.75)	−9.6	−7.2	−6.4	−6.8
	(3.6)	(3.8)	(3.6)	(3.9)
90th	0.6	0.2	2.3	−6.6
	(6.9)	(7.2)	(8.1)	(9.3)
80th	5.0	4.0	6.4	2.5
	(3.4)	(3.8)	(4.6)	(5.2)
70th	4.0	3.8	6.6	5.0
	(2.4)	(2.7)	(3.2)	(3.4)
60th	2.6	2.6	4.3	3.9
	(1.9)	(2.1)	(2.4)	(2.5)
50th	2.2	2.8	3.8	3.0
	(1.5)	(1.6)	(1.7)	(1.9)
40th	2.5	2.8	3.8	3.3
	(1.2)	(1.3)	(1.4)	(1.6)
30th	2.2	2.6	3.7	2.8
	(1.0)	(1.1)	(1.1)	(1.2)
20th	2.0	1.1	1.6	1.8
	(1.0)	(1.0)	(1.0)	(1.1)
		Recife versus Salvador		
FGT(0)	6.9	7.3	14.8	13.6
	(7.0)	(7.4)	(7.8)	(8.3)
FGT(1)	5.2	3.4	7.8	7.5
	(5.0)	(5.6)	(5.5)	(5.2)
FGT(2)	4.1	1.1	4.6	4.3
	(4.3)	(5.1)	(4.9)	(4.3)
ECF	−18	363	339	458
	(336)	(417)	(413)	(402)
A(.25)	2.6	−2.3	−4.2	−1.0
	(3.1)	(3.1)	(3.4)	(3.1)
A(.50)	3.9	−4.5	−6.2	−1.9
	(4.8)	(5.0)	(5.2)	(4.7)
A(.75)	5.0	−5.0	−4.3	−2.0
	(5.9)	(6.8)	(6.6)	(5.8)

Source: Author's calculations from household-based PME data.
† Minimum wage treatment effects and robust s.e. in parentheses for decile-based measures.

Three-stage generalized least squares estimates in the first panel of Table 5 do not establish a contemporaneous relationship between the minimum wage and incomes at the 20th or 30th percentiles and, in fact, suggest negative effects one quarter after a wage floor increase. Information in the second and third panels, where estimates by region are displayed, establishes that these results are driven by wage floor influence in the two lowest-income metropolitan areas of Recife and Salvador, where between 1996 and 2001, the minimum wage averaged the equivalent of 49% and 52% of full-time median earnings, respectively, as compared to 34% in Rio de Janeiro and 27% in São Paulo. Results pertaining to impact in the two higher-income areas establish no statistically significant contemporaneous or lagged relationship. Data pertaining to the 1996–2001 PMEs therefore support a qualified finding where the minimum wage has negative lagged effects on per capita income when it is close to the middle of the distribution of earnings and no effect when it is closer to the 27%–34% range.

These conclusions are nonetheless contingent on an identification strategy that relies on a minimum wage variable that aims to capture differences in treatment intensity across regions. If it were capable of isolating wage floor effects, there would be no reason to expect impact at levels far beyond the reach of the policy instrument. However, this is not the case since results in the fourth panel of Table 5, where influence above the 30th percentile of per capita income is displayed, indicate negative lagged effects that are evident all the way up to the 90th percentile of the distribution.

8. Alternative controls

Besides being Brazil's main metropolitan areas, São Paulo and Rio de Janeiro are similar in terms of relative size of the informal sector, share of the workforce that is female, college educated, and constituted by entrepreneurs, for example. However, a synthetic control can improve balance in other determinants of income distribution such as labor force participation, the self-employment rate, and racial and age composition, with São Paulo's labor force participation rate being somewhat higher than Rio's

Table 7
Determinants of changes in poverty and inequality by distribution measure.

Measure	change in mw		initial value of mw		F
	$\beta \times 10^2$	$s.e. \times 10^2$	$\beta \times 10^2$	$s.e. \times 10^2$	
			Specification 1		
FGT(0)	−52.6	16	88.8	14.8	23.4
FGT(1)	−24.5	12.4	43.9	15.8	4.9
FGT(2)	−17.9	10.5	28.5	15.2	2.4
A(.25)	−2	4.3	24	7.1	5.8
A(.50)	−2.8	6.8	39.1	12.9	4.6
A(.75)	−7.6	10.5	42.5	21.3	2
			Specification 2		
FGT(0)	−18.3	5.4	58.2	27.3	6.3
FGT(1)	−14.2	3.4	35.4	19.7	9
FGT(2)	−11	2.5	23	16.2	9.6
A(.25)	−4.5	2.3	26.8	8.1	5.6
A(.50)	−7.8	4	44.5	14.8	4.7
A(.75)	−10	5	45.5	22	2.8

Note: The first specification measures intensity of minimum wage increases using the fraction affected variable whereas the second specification uses the wage gap.
Source: Author's calculations using household-based PME data for distribution measures and person-based data for minimum wage variables.

and its workforce less non-white, younger, and less likely to be self-employed. Minimum wage effects are therefore re-estimated comparing Rio de Janeiro with its synthetic control, which as described in Section 5, is a weighted average of São Paulo (69%), Recife (13%), Salvador (9%), Porto Alegre (7%), and Belo Horizonte (2%).[7]

Results in Table 6 establish more precisely-estimated treatment effects than in Table 3, where standard errors are on average 9% larger. Moreover, fourth-quarter effects on poverty are more substantial than estimated with the non-synthetic control. The wage floor's impact on the incidence of poverty is statistically significant in the three quarters following a wage hike, as indicated in Table 3, but also in the fourth quarter, albeit at the 93% confidence level. Third- and fourth-quarter effects on inequality are also stronger than estimated with the non-synthetic control. Measured by the A(.25) and A(.50) indices, influence is statistically significant at the 95% confidence level in each of the four quarters after a wage floor increase. Falsification tests do not reject the null of no minimum wage influence above the 40th percentile of equivalent income, and provide evidence of impact at the 30th and 40th percentiles that is statistically significant from the first to the fourth quarter of influence.

An additional assessment of minimum wage effects can involve estimation using other suitably comparable regions such as Recife and Salvador, with results of special interest as they can shed light on the issue of the benefits associated with raising a wage floor that is already high relative to median earnings. Both metropolitan areas are similar in terms of household composition, age distribution, size of the informal sector, share of the workforce that is college educated, or is self-employed, for example. However, the minimum wage bites a little harder in Recife, where it is equivalent to 68% of median earnings among full-time salaried workers and over the period under examination, as compared to a figure of 65% in Salvador. Minimum wage influence is thus estimated using Recife as the treated region and Salvador as the control, with resulting coefficient estimates and associated standard errors shown in the second panel of Table 6. These do not suggest a clear relationship between income distribution and the policy tool, as effects are neither consistently negative, positive, nor statistically distinguishable from zero. However, given the relatively small income gap between these two metropolitan areas, one cannot rule out that absence of evidence is related to insufficient statistical power rather than lack of real benefits (or costs) to the strategy.

9. Elasticities and determinants of treatment effects

Gathered evidence points to minimum wage effects that depend on the policy instrument's position in the wage distribution. To provide further evidence on the potential limits of the policy tool, its degree of influence, and on practices associated with more lasting impact, fourth-quarter treatment effects of each of the 21 minimum wage hikes encompassed by the data are regressed on two variables of interest. The first gauges initial location of the wage floor in the wage distribution through the share of full-time workers bounded by the minimum wage at the time of a wage floor increase. A second variable measures the intensity of the minimum wage hike through the increase in the share of the workforce bounded by the wage floor as a result of the wage hike, sometimes referred to as the "fraction affected" (Card, 1992b), or through a "wage gap" measure that calculates the percentage difference between a new minimum and the wage of impacted workers before the wage floor increase (Currie & Fallick, 1996). The larger the difference, the greater the increase in the bite of the minimum wage.

Table 7 presents regression coefficients and standard errors robust to the presence of heteroskedasticity and autocorrelation. Results establish positive coefficient values for the share of the working population that is bounded by the minimum wage at the time of a wage floor increase. They also establish negative coefficient values for the "fraction affected" and "wage gap" measures. In combination, results suggest that large hikes in the minimum wage are more likely to reduce poverty and inequality than small ones, but that effects become more muted as the wage floor becomes increasingly more binding. However, the sample size is not large and coefficients are not all precisely estimated.

Impact of both variables on the headcount ratio is quite precisely estimated in both specifications and can be used to obtain a measure of the elasticity of the incidence of poverty with respect to minimum wage changes. These establish that a 1% increase in the fraction of the workforce affected by the minimum wage or a 1% increase in the wage gap measure reduces the headcount ratio by 0.11% and 0.08%, respectively. These elasticities are consistent with results established in Sections 7,8 through the difference-in-

[7] Averaged over the time period under examination, the share of the workforce that is college-educated, engaged in the informal sector, and constituted by employers stands at 23%, 21%, and 4.8%, respectively, in São Paulo, as compared to 22%, 21%, and 4% in Rio de Janeiro. The share of individuals engaged in work for a positive wage, classified as non-white, or self-employed stands at 52%, 36%, and 18%, respectively, in São Paulo, as compared to 48%, 48%, and 24% in Rio de Janeiro. Finally, the average age of working individuals is 37 in São Paulo and 39 in Rio.

difference approach. That is, on average, minimum wage revisions raise the value of the wage floor from 45% to 51% of median full-time earnings in Rio de Janeiro. Relative to established treatment effects, a 1% increase in the wage floor relative to the median wage translates to a 0.13% fourth-quarter reduction in the prevalence of poverty. Few studies have found a statistically significant relationship between poverty and the minimum wage in the United States, but elasticities based on imprecisely-estimated coefficients stand at an average of −0.13 (Dube, 2019). One exception is Dube (2019), who establishes a statistically significant relationship translating to elasticities of the non-elderly poverty rate that range from −0.22 to −0.459. Neumark, Schweitzer, and Wascher (2005) also find a statistically significant relationship, but in the opposite direction, where increases in the minimum wage raise the headcount ratio with an elasticity of 0.41.

10. Conclusion

Especially during the decade of the 2000s, many developing economies turned to new or higher minimum wages as redistribution instruments, despite limited and conflicting evidence regarding impact on family income, poverty, and income inequality. Brazil in particular raised its minimum wage with gusto, and the paper set out to estimate influence on distribution, taking advantage of data encompassing 21 increases in the wage floor. Sizable regional income disparities provided a setting for identifying treatment effects through a difference-in-difference approach.

Visual evidence suggested minimum wage influence on earnings that translated to impact on the household. Statistical results indicated that within three months of a minimum wage hike, poverty and income inequality decline, on average, by 2.8% and 2.4%, respectively. Effects fade over time, particularly with respect to bottom-sensitive distribution measures, a process that is consistent with resulting job loses that fall more heavily among poorer households. The fact that the following annual hike in the minimum wage leads to a renewed decline in poverty and inequality suggests that potential unemployment costs are then again overwhelmed by benefits in the form of higher wages among working individuals. Use of a synthetic control generates more precisely-estimated effects and impact on income that is statistically significant four quarters after a wage floor hike.

The causal nature of the evidence is supported by falsification tests that do not detect minimum wage effects where they should not be found. Influence is centered around the 30th–40th percentile range of the distribution of equivalent income and is based on minimum wage levels that in the main treatment region averaged 49% of median earnings. This ratio is within the range where the wage floor is found in many countries (ILO, 2016), and is hence an element that contributes to the external validity of the evidence. Impact associated with a higher minimum wage was also examined through use of alternative treatment and control areas where the wage floor bites much harder, and results failed to indicate any precisely-estimated minimum wage effect on income, poverty or inequality.

The answer to the question of whether the minimum wage can reduce poverty and inequality in the developing world is thus a yes, qualified by the finding that the relationship between the incidence of poverty and the wage floor is very inelastic, that the policy instrument requires continuous reinforcement, that it is subject to diminishing returns, and that it is not an effective strategy for addressing extreme poverty. Policy choices should also be informed by potential long-term effects associated with exposure to the minimum wage at young ages and consideration of influence through other channels such as prices (MaCurdy, 2015; Neumark & Nizalova, 2007). An assessment of impact on distribution in developing settings remains a subject of future research.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

CRediT authorship contribution statement

Orlando J. Sotomayor: Conceptualization, Methodology, Software, Validation, Formal analysis, Visualization.

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