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ABSTRACT

Evidence from studies of the employment effects of minimum wages in developing countries is mixed. One interpretation is that there is simply no clear evidence of disemployment effects in developing countries. Instead, however, we find evidence that the heterogeneity is systematic, with estimated effects more consistently negative in studies with relatively more features for which institutional factors and the competitive model more strongly predict negative effects – studies covering vulnerable workers, in the formal sector, when minimum wage laws are strong, and when minimum wages are binding.

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I. Introduction

Minimum wages have been a controversial subject among policymakers and economists in the United States and many other countries. The evidence on employment effects in developing countries is quite mixed.[1] In the studies we survey in this paper, simple averaging of all of the reported estimates yields a fairly modest negative employment elasticity of −0.062, and averaging the authors' preferred estimates from each study yields an elasticity of −0.103. However, looking across all the studies reveals considerable heterogeneity, with many non-negative estimates.

The goal of our analysis of the evidence from the large set of studies we survey is to try to understand this heterogeneous evidence and what we can learn from it. Is there simply no consistent evidence of negative employment effects of minimum wages in developing countries? That is, do we get heterogeneous effects – with both positive and negative estimates – across similar studies looking at workers most likely to be affected by minimum wages both because of their skills and because of the nature of a country's minimum wage law? Or, instead, is the heterogeneity in estimated minimum wage effects more systematic, with negative effects where we would expect them – e.g., for vulnerable low-skill workers where minimum wage laws are strong and binding – but not for higher-skill workers or where minimum wages laws are less relevant or effective?

We pursue these questions by conducting a version of a meta-analysis of a large set of studies of minimum wage effects in developing countries. In contrast to the focus of some meta-analyses on questions like publication bias, or arriving at a single estimates from a body of studies (e.g., Belman and Wolfson, forthcoming; and Broecke et al., 2016), and also in contrast to general surveys of the evidence (e.g., Belman and Wolfson, 2016; Bhorat et al., 2017), our focus is explicitly on understanding the differences in estimated employment effects across studies.[2] Still, there are clearly complementarities

[1] For a recent review of the U.S. evidence, including discussion of the conflicting evidence and which methods point to disemployment effects, see Neumark (2019).

[2] See Neumark (2016) for discussion of some of these meta-analyses of estimated minimum wage effects in the United States, especially with reference to testing for publication bias. In a nutshell, it is hard to distinguish between publication bias and other sources of patterns in the published evidence consistent with publication bias. For example, meta-analyses like Doucouliagos and Stanley (2009) argue that if published negative estimates of minimum wage

between our evidence and the evidence in these other surveys or meta-analyses.

It is important to consider how to interpret our evidence. There are three important points. First, stronger and more consistent evidence of adverse employment effects under conditions where we would expect adverse effects – e.g., for less-skilled workers, when minimum wages are binding or strongly enforced, or in the formal sector – would not negate the fact that estimated employment effects in developing countries vary. But such evidence would be informative about the institutional settings and contexts in which minimum wages reduce employment – such as when they are imposed in the formal sector and are strongly enforced.

Second, such evidence could indicate that minimum wages have more adverse consequences when they have the greatest potential benefits – i.e., for low-skilled workers for whom they are binding. Evidence that minimum wages reduce employment of lower-skilled workers does not imply that minimum wages are the wrong policy choice. However, it would imply that minimum wages in developing countries reflect more of a tradeoff between higher wages and lower employment than what one might conclude from a simple overview of the heterogeneous evidence. Ultimately, we think the wisdom of higher wages in developing countries should hinge more on whether they help raise incomes of low-income families.[3]

And third, this kind of evidence may speak to the right model of the labor market to use in thinking about labor market policy and other questions in developing countries. If evidence on employment effects of minimum wages for less-skilled workers, when minimum wages are binding and enforced, is inconsistent, then it is possible that the monopsony model may better explain the evidence than the competitive model.[4] In contrast, consistent evidence of disemployment effects of, e.g., strong and binding minimum wages for low-skilled workers – despite less consistent evidence under other conditions – would

effects have larger standard errors, this is evidence of publication bias. However, the same phenomenon can arise if studies using better research designs lead to “truer” (i.e., less biased) estimates, which happen to be negative, and which have larger standard errors because they demand more of the data.

[3] For evidence on this question from about a decade ago, see Neumark et al. (2006) and Cunningham (2007).

[4] Still, the monopsony model makes more direct predictions than simply that employment effects are heterogeneous, and it would be important to test these predictions. For related work for the United States, see Azar et al. (2009) and Munguia Corella (2020).

bolster the competitive characterization of labor markets (although it could still be possible to reconcile such evidence with monopsony).

We conclude that one can draw firmer conclusions about the employment effects of minimum wages in developing countries than first meets the eye when simply looking at all the estimates. We find that the estimated employment effects of minimum wages in developing countries are more likely to be negative, and larger negative, when estimates focus on data and sectors for which the competitive model predicts disemployment effects, and in institutional settings in which we would expect the minimum wage to have more impact. Specifically, there is more consistent evidence of negative employment effects when the minimum wage is binding, where minimum wage enforcement is stronger, for estimates of effects in the formal sector, and when the data focus on more vulnerable (lower-wage) workers.

One dimension we do not explore is whether monopsony power is sometimes relevant. There are some cases of positive estimates (although not many) in studies with features for which the competitive model and institutional factors predict negative employment effects, and these positive estimates are more prevalent in studies with only one feature or no features for which the competitive model and institutional factors predict negative effects. Monopsony is a potential explanation, but not the only one; for example, the standard two-sector competitive model predicts positive employment effects in the informal sector.

II. Studies Surveyed

We reviewed 60 papers on the employment effects of minimum wages in developing countries[5] – all of the papers we identified that met our study criteria. To select these papers, we searched for papers in journals and on Google Scholar, covering all the regions in the developing world. We search using keywords related to minimum wages and developing countries. Our search was conducted from April 2017 to August 2017. We also consulted recent surveys (Belman and Wolfson, 2016; and Broecke et al., 2017) to check for any papers we missed, which resulted in adding two additional papers from Belman and

[5] We define developing countries as those that the World Bank does not classify as a high-income country. Poland became a high-income country in 2009, but the data in the papers on Poland cover predominantly earlier data (1999 to 2011 in all papers except one that extends to 2013).

Wolfson (2016).[6] We focused mainly on recently published papers (published since 2000), because we wanted to analyze the burgeoning wave of minimum wage papers in developing countries; of the 60 in our survey, 93.4% were published after 2000.[7] Most of the papers are in English, but we also include papers in Spanish and Portuguese. We also restricted the analysis to papers that report employment elasticities with respect to the minimum wage, or for which we had enough information to compute these elasticities.[8]

We created a data set of all estimates from these papers. However, because many papers present estimates that the authors do not even take as credible (e.g., showing the estimates for panel data specification without the fixed effects), we also tried to extract the authors' main or preferred estimates from each study. To do this, we read each paper in detail and selected preferred estimates following three rules. First, in some cases the authors specifically say that a subset of estimates are their preferred results. This kind of statement is based, for example, on the authors presenting specifications missing some controls (e.g., year fixed effects), while arguing that the controls are needed to correctly estimate the effects of minimum wages. Second, if there is not a statement this explicit, authors often summarize what they say are their main findings, often referring only to these estimates in the abstract, the introduction, or the conclusion – i.e., underscoring some specific estimates. Third, absent either of these conditions, if estimates are reported for many regions in a country, we select the estimate for the whole country as the preferred result.[9] But to be clear, rule one overrides two and three, and rule two overrides three. Thus, for

[6] Belman and Wolfson (2016) is a broad survey of the effects of minimum wages on many different outcomes, and does not – in contrast to the present paper – focus on reconciling conflicting evidence, but more on issues of empirical methods. Broecke et al. (2017) use a meta-analysis to analyze 14 emerging economies instead of developing countries. They focus mainly on publication bias, but, closer to our paper, they also whether minimum wage effects are more substantial for vulnerable workers and the formal sector, finding some evidence of more adverse effects for lower-skilled workers.

[7] The only exceptions were four earlier, often-cited papers that appear in more than one meta-analysis: Bell (1997) for Mexico; Castillo-Freeman and Freeman (1992) for Puerto Rico; and Feliciano (1998) and Foguel (1998) for Brazil. Appendix Figure A1 shows the distribution of these studies by year (publication date). The figure shows that the plurality of these studies were published in this decade and most in the last two decades. Of course papers studying minimum wages in developing countries continue to be produced and published (e.g., Asmal et al. (2019), but we had to cut off the sample period for analysis for this version of our paper.

[8] Below, we list and discuss the full set of studies we include, and the elasticity calculations.

[9] In principle, one could classify the results for different regions as vulnerable or not vulnerable, if the authors had classified regions this way. However, the papers we cover do not classify regions this way, and, in line with our strategy in this paper, we did not want to try to make our own determinations.

instance, if the authors point out that their preferred result is for region A, we use region A as the main result instead of the estimate of the whole country. However, in the spirit of a meta-analysis, we do not impose (or even offer) our subjective assessments of which studies are more credible, and certainly do not discard some that could plausibly be viewed as less credible or plausible.[10]

Finally, it is important to mention that studies sometimes report estimates for different groups or sectors, like all workers and more vulnerable workers, or the formal and the informal sector. We capture all of these estimates, but also flag – when the authors do – the subset of these estimates preferred by the authors, based on the rules above.

We believe that in analyzing the set of estimates from a research literature, it makes sense to focus on the preferred estimates. For example, suppose there are two papers estimating the effect of policy X, and both authors believe that one needs to instrument for policy X to get the causal effect. If one paper presents only the instrumental variables (IV) estimate, while the other presents both the OLS and the IV estimate, then why give weight to the OLS estimate in summarizing the evidence? Neither author believes the OLS effect is of interest, and the second author chose to include it for some other reason – perhaps to confirm the expected direction of the bias in the OLS estimate, for which the IV corrects.[11] At the same time, we understand that the selection of preferred estimates potentially allows for an element of subjectivity compared to simply capturing all estimates in the surveyed papers; our use of a set of rules for identifying authors' preferred estimates should mitigate any concerns regarding our decisions about which estimates to study.

Table 1 reports descriptive information on the estimated minimum wage effects on employment in the studies we surveyed. Among the 60 studies, there are 1,232 total estimates. There are 14 studies that

[10] We are not arguing that this is necessarily the preferred approach for interpreting a broad literature. Indeed, in the U.S. context, Neumark and Wascher (2007) offer reasons why a narrative review (with some emphasis on what appear to be more credible estimates) may be preferred. On the other hand, they also argue that a narrative review may be more effective at highlighting some of the reasons for differences across studies attributable to the groups studied or other reasons theory might predict different effects. The present paper adopts the latter perspective to some extent – focusing on explaining differences in results across studies, albeit without discarding estimates.

[11] An example of the latter is Mayneris et al. (2014), who report both OLS and IV estimates, but take a clear stance that there may be endogeneity bias in their approach that requires instrumenting for the minimum wage variable.

report the effects of the minimum wage on the probability of being employed (or something closely related), rather than an elasticity, and for which we could recover estimates of elasticities to make all the evidence comparable. We use reported means of employment rates and the minimum wage if the paper reported them. If these were not reported, we use alternative data sources to obtain these averages and estimate the elasticities. The data sources and calculations are described in Appendix Table A1.[12]

Across all the estimates in the surveyed studies, the average estimated elasticity is −0.062, the maximum elasticity is 4.51, and the minimum is −4.73. We identified 223 preferred estimates, using the rules discussed above. The average elasticity for this subset of estimates is −0.103, with a maximum of 2.19 and a minimum of −2.53. The standard deviation is 0.502, very similar to the standard deviation for all estimates (0.451). Note that the authors' preferred estimates exclude some more extreme elasticity estimates.

Figure 1 provides histograms for the two sets of estimates, to provide more evidence on their distributions. We plot only estimates between −1 to 1 to make the figure easier to read.[13] Panel A provides the histogram for the full set of estimates, and Panel B the preferred estimates. The negative means and medians of the estimates are clear for both sets of estimates, as is the fact that there clearly are positive estimates. Note also that the medians are considerably closer to zero.

III. Classifying Studies/Estimates, and Predictions for Employment Effects

The key question we assess is whether there are systematic differences across studies and estimates that explain the variation in estimated employment effects. In particular, we classify the estimates in the studies in our survey by specific features of the estimates. We then ask whether features of estimates more likely to predict negative effects either based on the competitive model of the labor market, or because of institutional factors, in fact do so. As an example, the competitive model of the labor market would predict that less-skilled workers are more adversely affected by a higher minimum wage. Consistent with this, for

[12] For studies for which we had to compute elasticities, we use the statistical significance of the reported employment effect.
[13] We do not do this trimming in any of the figures or estimates that follow, where we use all the elasticities preferred by the authors, even if the elasticity appears to be an extreme value.

example, Neumark and Wascher (2007) argue that studies of minimum wages in the United States (and some other countries) are more likely to find negative employment effects when the focus is on the least-skilled workers. So, one question is how employment effects differ when estimates are computed for less-skilled workers. However, we also consider another theoretical dimension, as well as institutional factors that might predict stronger or weaker effects of minimum wages. We classify estimates based on four features. Appendix Table A2 lists the studies we use, the preferred estimates as discussed earlier, and the classifications of studies and estimates – which we now discuss in detail.

Binding minimum wages

The first feature of estimates we use is whether minimum wages are binding. We classified estimates along this dimension based on evidence reported in the studies on the effects of minimum wages on wages. If the study reported a statistically significant positive effect of the minimum wage on wage measures studied, or evidence of a spike in the wage distribution at the minimum wage (based on visual inspection of figures as described by the authors), we classify the corresponding employment estimate as pertaining to a binding minimum wage. If evidence was reported and does not indicate a positive and significant effect on wages, we classify the study as pertaining to a non-binding minimum wage. Our third category is "no data," meaning that the study did not report evidence on effects on wages. We would expect more evidence of adverse effects of minimum wages on employment when minimum wages are binding, at least under the competitive model.[14]

Sector

The second feature we use to classify estimates is whether the estimate was for the formal sector, the informal sector, or both (total employment). In the formal ("covered") sector, minimum wage laws apply, in principle at least. Minimum wage laws do not cover the informal sector. The informal sector can

[14] It is possible that there is a "file-drawer" problem (e.g., Franco et al., 2014), in that studies that do not detect, in initial analyses, an effect of the minimum wage on wages of low-wage workers are not pursued further, because of the strong expectation that – whatever the effects on employment, etc. – minimum wages should push up wages at the bottom of the wage distribution. This may constrain our ability to garner evidence on how the employment effects of minimum wages estimated in different studies vary with whether or not the minimum wage is binding.

be defined by firms that operate illegally, by self-employed workers and, as in Chun and Khor (2010) and Del Carpio et al. (2015), by small firms that enforcement authorities do not visit. In developing countries, both sectors can be sizable. The distinction between the effects for the formal and informal sector in developing countries is important. A high share of jobs is estimated to be informal: 46.8% of jobs in Latin America (ILO, 2015a), 66% in Sub-Saharan Africa (ILO, 2015b), and 68.2% in Asia-Pacific (ILO, 2018).

Some papers do not report if their estimates cover the formal sector, the informal sector, or both sectors. However, it was possible to classify these papers by analyzing the data the authors used. For example, for Mexico there are two main employment surveys – the Employment and Occupation Survey, and Social Security Administrative Data. The former has data on both sectors; hence, if the author uses total employment from this survey, we know that the estimates cover both sectors. The latter survey only has data for formal-sector workers, and thus we know that estimates using this survey are for the formal sector.

The prediction from the standard two-sector competitive labor market model is that a higher minimum wage reduces employment in the formal sector, because in the formal sector minimum laws (and other labor regulations) apply and are more likely to be enforced. However, employment in the informal sector may increase, depending on informal sector wages and the expected value of search for formal-sector work while employed vs. not employed in the informal sector (Harris and Todaro, 1970; Mincer, 1976). However, some recent work has highlighted the potential for different effects in the informal sector. For example, Gindling (2018) argues that some evidence points to wage increases in the informal sector from "lighthouse effects." Lighthouse effects may arise because employers have to compete for workers with the formal sector, which could imply that minimum wages constrain the wages employers pay in the informal sector and hence reduce employment there. Or lighthouse effects could reflect a reference price, a signal for bargaining, or the impact of fairness concerns – all influences on wages outside of the usual competitive model.[15] Other studies, in contrast, have found no effect on wages in the informal sector

[15] See the discussion and related references in Boeri et al. (2011).

(Papps, 2012; Carneiro and Corseuil, 2001). Thus, we should expect adverse employment effects of minimum wages in the formal sector – at least under the competitive model – whereas the prediction for the informal sector is perhaps less clear.

Enforcement

Our third feature of estimates is the degree of enforcement of the minimum wage law, which we break into three categories. Countries with no enforcement are those that do not penalize violations of the minimum wage law. Countries with weak enforcement have low-cost fees for a violation. And countries with strong enforcement are those that have severe penalties for not abiding by the law, like time in prison or shutdown of the company. The prediction, of course, is that minimum wages should have more impact generally, including reducing employment (according to the competitive model), when minimum laws are more strongly enforced.

The classification of enforcement is developed and described in Munguia (2019). He systematizes labor codes and minimum wage laws by country, and constructs an indicator for the degree of enforcement, using the ILO's "Database of National Labour, Social Security and Related Human Rights Legislation" (NALEX).[16] NALEX compiles records of labor laws for 196 countries and 160 territories. As an illustrative example, Ghana does not have any penalty specified in its Labor Act of 2003; the Act established a Tripartite Committee that oversees the minimum wage rate, but does not specify what happens when an establishment fails to abide by the law. Hence, Ghana is classified as having "no enforcement." In contrast, Bolivia has strong penalties and a solid mechanism to inspect companies. Fines are costly (up to 1,447 USD per violation), and the authorities might shut down an establishment in case of repeated violations. Hence Bolivia is classified as having "strong enforcement." Given the constraints of the data used to classify enforcement, the degree of enforcement is assigned at the country level and does not change over time. Moreover, a potential limitation of the enforcement measure is that it captures potential penalties. It is possible that in some countries, even if the law is stringent, actual implementation

[16] See http://www.ilo.org/dyn/natlex/natlex4.home?p_lang=en.

is weak. However, in a more standard panel data analysis of the effects of minimum wages in developing countries, Munguia (2019) finds stronger adverse employment effects when the law dictates stronger enforcement, without regard to how well labor laws are enforced (although also finding that enforcement has stronger effects in countries with more effective labor market regulations, based on a World Bank index).

Vulnerability/low-skill

Finally, the fourth feature of estimates we use in our classification is whether the estimate is for low-skilled or "vulnerable" workers, or instead for all workers. We classify studies or estimates for vulnerable workers as those estimated for young adults, for women, or for unskilled workers. The competitive model of labor markets, of course, predicts that we should find stronger evidence of adverse employment effects of minimum wages in data on vulnerable workers because their wage is more likely to directly affected by the minimum wage. However, if the minimum wage is very low, it is possible that it is not binding even for low-wage, vulnerable workers.

IV. Differences in Estimated Employment Effects: Evidence

We now turn to our analysis exploring how estimated employment effects vary with features of the studies or estimates. In particular, we focus on whether the evidence is more consistent with negative employment effects for estimates based on one or multiple features that predict more adverse employment effects of minimum wages, and conversely whether there is less evidence of negative effects when these features are absent.

Differences in estimates: one-way comparisons

We begin, in Table 2, with univariate comparisons across estimates. Table 2 reports the number and percent of estimated employment elasticities with respect to the minimum wage that are negative and significant, positive and significant, or insignificant, for estimates with each of the four features by which we classify them: binding minimum wages, sector, enforcement, and type of workers. This table is based on the authors' preferred estimates of the employment elasticity, summarized in the second row of Table 1 and in Panel B of Figure 1.

To better understand what is reported in Table 2, consider some specific examples. Foguel et al. (2001) analyze the employment effects of minimum wages in the formal and informal sectors in Brazil. Their main results indicate that minimum wages have a negative (and significant, at the 10% level) effect on formal-sector employment (elasticity of −0.011) and a positive (and insignificant) effect on informal-sector employment (elasticity of 0.018). Table 2 classifies these two results as one negative and significant estimate for the formal sector and one positive and insignificant estimate for the informal sector.

Another example is Bhorat et al. (2014), who analyze the effects on formal-sector wages and employment in South Africa. Their main results indicate that the elasticity of wages with respect to the minimum wage is between 0.176 and 0.22 (and statistically significant). Hence, these results are classified as "binding." For employment effects, they estimate have two preferred elasticities (based on different econometric models). These are both assigned to the categories "binding" and "formal sector." Hence, this study results in two negative and significant elasticities reported in the binding row of Table 2, and two negative and significant elasticities reported in the formal-sector row. Moreover, because South Africa has weak penalties, thus, this study is also coded as having two negative elasticities in the weak enforcement row.

Panel A of Table 2 reports results based on whether the minimum wage is binding, non-binding, or there are no data on wages with which to classify the study and its estimates. There is somewhat more evidence of negative employment effects when minimum wages are binding (or are likely to be binding – as discussed below). For the estimates based on binding minimum wages, 43.4% of the elasticities (69 estimates) indicate negative and significant effects on employment.[17] Only 6.9% of the results (11) with a binding minimum wage report positive and significant elasticities. In 49.7% of the cases (79) the estimated employment elasticity is insignificant. Thus, for binding minimum wages, if the estimated elasticity is significant, the evidence points much more strongly to adverse employment effects than to positive employment effects, although the share of negative and significant employment elasticities is slightly lower

[17] In this and subsequent tables and figures, we classify estimates based on statistical significance at the 10% level.

than the share of insignificant elasticities.

There is only a small number of estimated elasticities from studies where the minimum wage is non-binding (11), and nearly two-thirds of them (63.6%) report an insignificant employment elasticity. However, the remainder (36.4%) of the estimated employment elasticities are negative and significant.

There is a much larger number of studies with no information on whether the minimum wage is binding, corresponding to 53 estimated elasticities. Among these, the results are very similar to the estimates based on studies reporting that the minimum wage is binding, with 41.5% of the estimated employment elasticities negative and significant, and 37.7% insignificant. Given the distribution of estimates (and studies) as having binding or non-binding minimum wages in the first two rows, it seems likely that in most of the unclassifiable studies the estimated minimum wage effect is in fact for a binding minimum wage. For instance, as shown in Appendix Table A3, China has four studies classified as "no data," but it has four that are classified as binding, and only one classified as non-binding, so it seems plausible that the minimum wage is binding in the first four. Similarly, Brazil has three studies classified as "no data," 12 classified as binding, and none classified as non-binding. Thus, it seems reasonable to view the results in the "no data" row of Table 2 as largely reinforcing the conclusion that estimates of the effects of binding minimum wages point to disemployment effects, although to be more agnostic we continue to treat these two groups of studies separately, and to study binding minimum wages we focus on the estimates for which we can explicitly classify the data as pointing to a minimum wage that is binding.

Panel B reports results for estimates classified by sector – formal or informal. The results tend to point to evidence of negative employment elasticities in both sectors. However, there is more evidence of positive effects for estimates based on the informal sector. For the formal-sector estimates, 41.9% of the estimated elasticities (57 estimates) point to negative employment effects, while only 8.8% (12) point to positive employment effects; 49.3% of estimates (67) are insignificant. For the informal sector, the percentage of positive and significant employment elasticities is more than double that for the formal sector (17.4% vs. 8.8%), although still, more estimates are negative or insignificant (41.3% in both cases). For estimates covering both sectors, the percentage of estimates that are negative and significant is similar, and the percentage of insignificant estimated elasticities is higher.

Panel C disaggregates the estimated elasticities based on enforcement. In this case, the results appear to be less sharply delineated and in some respects counterintuitive. In particular, the elasticities for minimum wage laws with strong enforcement are negative and significant in 52.4% of cases (33 estimates), compared to 29.6% (29) with weak enforcement; but the percentage is higher (53.3%) with no enforcement. Weak and especially no enforcement is associated with more evidence of positive elasticities than strong enforcement (17.7% of estimates with no enforcement, vs. 9.5% of estimates with strong enforcement). However, there is not a clear pattern of a greater percentage of insignificant elasticities the weaker is enforcement.

Finally, Panel D turns to results disaggregated by type of worker. Estimates for vulnerable workers point more clearly to disemployment effects – with 50.6% of such estimates (41) negative and significant, compared to a lower percentage (38%) among estimates computed instead for all workers.[18] Correspondingly, there is a lower percentage of estimates with positive effects when looking at vulnerable workers compared to all workers (7.4% vs. 11.3%), and the percentage with insignificant results is lower for vulnerable workers (42% vs 50.7%).

Thus, based on the univariate comparisons, for three of the four the classifications of estimates we use – binding minimum wages, sector, and type of worker – we find some evidence consistent with minimum wages doing more to reduce employment where there is a stronger prediction of negative employment effects, and for the formal/informal-sector distinction, more evidence of positive effects in the informal sector. These results are consistent with expectations from the competitive model (while not necessarily contradicting other models), including the two-sector model. We next turn to evidence that more sharply delineates studies and estimates by simultaneously considering multiple features of these estimates.

Differences in estimates: multi-way comparisons

[18] This same pattern of variation is observed within studies. For instance, in Fang and Lin (2015), the elasticity of teenage employment to minimum wages is −0.78, whereas it is −0.55 for total employment.

The one-way comparisons we have presented thus far may not be that informative, for four reasons. First, we may not be isolating the effect of a particular features of an estimate, because estimates can vary along multiple dimensions at once. Second, given that each of the features we study – bindingness, formality, enforcement, and vulnerability – can matter independently for whether minimum wages reduce employment, it follows that estimated employment effects may be more negative if *more* features of an estimate predict negative effects – based on the competitive model or institutional factors. Third, we have taken no account of the estimated magnitudes of the elasticities. And fourth, related to the last point, the signs of insignificant estimates are also of interest.[19] Hence, we now present analyses that incorporate all of this information. For this analysis, we present evidence in sets of figures, rather than tables, because the figures make the evidence much clearer. In the next subsection, we turn to some regression estimates that refine the analysis further.

We begin with two-way comparisons. In Panel A of Figure 2A, we summarize the evidence for the estimates based on two features that more strongly predict negative employment effects based on the competitive model and institutional factors – for example, estimates covering vulnerable workers with strong enforcement, or estimates for the formal sector where minimum wages are binding. Note that this is a two-way comparison, so the third and fourth feature not specified in each pair could be anything (similar to in our one-way comparisons in Table 2); thus, two features predicting stronger negative effects means two or more features. We report (as we do in the remaining panels of Figure 2) the percentage of estimates that are positive but insignificant ("insignificant positive"), negative but insignificant ("insignificant negative"), positive and significant ("positive"), and negative and significant ("negative"). In Panel B, we report these percentages for estimated elasticities for which only one feature of the estimates in each possible pair of features predicts negative employment effects. And Panel C does this for pairs in which

[19] However, we created a version of Table 2 in which we broke out the insignificant negative and the insignificant positive estimates. There was not much systematic difference across the different types of estimates; in other words, the differences associated with whether the estimate is negative *and significant* are more pronounced. (Results available upon request.) However, in the more-refined analyses that follow, we look at estimates distinguished in this way.

neither feature predicts negative employment effects. Corresponding to what we said above, in Panel B one of more features more strongly predict negative employment effects, and in Panel C at most two features more strongly predict negative employment effects (or alternatively two or more features do *not* predict more negative effects). Appendix Table A4 reports the total number of estimates for each pair shown in the figure and reports similar information for the figures that follow.

Figure 2A shows a few things. Looking first at Panel A, when two (or more) features of an estimate more strongly predict negative effects, the estimated elasticity is much more likely to be negative. This is reflected in the black bars (for negative effects) being much longer than the gray bars, indicating higher percentages of estimated elasticities that are negative. In all cases, fewer than 20% of estimates are positive – summing across the solid gray bars for negative and significant elasticities, and the patterned gray bars for negative and insignificant elasticities. This contrasts with Panels B and C – when only one, or neither, feature in the pair considered predicts stronger negative effects. In Panel B, the differences between the black and gray bars – corresponding, respectively, to negative estimates and positive estimates – are less pronounced, and in some cases there are not many fewer positive than negative estimates (whether significant or not).[20] This weaker evidence of negative effects when fewer features more strongly predict negative employment effects is even more apparent in Panel C, for which neither feature in the pair predicts stronger negative employment effects (meaning that at least two of the four features we consider do not more strongly reflect negative employment effects). Indeed, while Panel B still indicates a preponderance of negative elasticities, while in Panel C there are many cases with a larger share of estimates that are positive.

One might also ask, from this figure, if there is evidence about which features of estimates are more strongly associated with finding a negative employment effect. However, because other features of estimates not in each pair considered can vary, this can be misleading. We come back to more explicit evidence on this question below.

[20] There is one case – "Strong/Informal" – where "all" the estimates are positive and significant. But Appendix Table A4 shows that there is only one elasticity in this category.

Figure 2B presents three panels for the same pairs of features, but this time reporting the average magnitude of the elasticity. In Panel A, for pairs in which both features of estimates more strongly predict negative employment effects, the elasticities are negative in every case, with one in the range of −0.05 range, four in the range of about −0.12 to −0.15, and one around −0.20. In Panel B, the average elasticity is negative in all cases but one (strong/informal). But in many cases the elasticities are close to zero, although there are some cases with larger negative elasticities. (However, the most extreme case (for "Vulnerable/Non-binding") is based on only two estimates.) Finally, in Panel C, when neither feature predicts stronger negative employment effects, more of the average elasticities are positive.

Thus, the evidence from Figures 2A and 2B suggests that when more features of estimated elasticities more strongly predict negative employment effects, the estimates are more likely to be negative. However, when we look at only pairs of features of estimates, the information can be quite noisy because the other two features of estimates not included in the pair are not specified.

Hence, we next look at sharper evidence – based on whether three features of estimated elasticities or all four features more strongly predict negative employment effects based on the competitive model and institutional factors. This evidence paints an even clearer picture: when many features of an estimate more strongly predict negative employment effects, the evidence points quite unambiguously in that direction. In contrast, when many features do not more strongly predict negative employment effects, the evidence is much more mixed.

Figure 3A presents the evidence on the sign and significance of the estimates, for estimates for which three or more features more strongly predict negative employment effect. In Panel A, the first set of bars (above the horizontal dashed line) are for all four features. For these estimates, all of the estimates are negative, with 64.3% significant and 35.7% insignificant. The remaining sets of bars are for estimates for each set of three features that more strongly predict negative employment effects.[21] It is clear that for these

[21] Following what we did before, we report results for each combination of three features of estimates that more strongly predict negative employment effects, without specifying the fourth feature – which hence may or may not more strongly predict negative effects.

estimates, nearly all of the estimates are negative, and more are statistically significant than not.

Panel B goes in the opposite direction, summarizing results for sets of features – in threes or all four – that do not more strongly predict negative employment effects. In this case, for which most (or none) of the features more strongly predict negative effects, there is clearly no clear pattern of more negative than positive elasticities, and there are many sets of features for which there are more positive than negative effects. Note that for the bars above the dashed line, for estimates for which none of the four features more strongly predict negative effects, there are very few elasticities (see Appendix Table A4); hence the percentages reported for this set of bars, including the couple of cases of 100% negative elasticities, are not very reliable.

Figure 3B presents similar evidence, but for the magnitudes (average elasticities). Not surprisingly, the estimated magnitudes are all negative in Panel A, for estimates for which all or most features more strongly predict negative employment effects. In contrast, the evidence in Panel B, for estimates for which most features do *not* more strongly predict negative employment effects, is very mixed, with one-third of the sets of estimates on average positive. Note that all of the larger positive magnitudes (and six of the seven positive ones overall) correspond to estimates for the informal sector.

Differences in estimates across estimates: meta-regressions

Finally, we turn to regression analysis of the estimates – or meta-regressions. In particular, we estimate models for whether the estimate is negative, whether it is negative and significant, and the estimated elasticity. For the first two cases, we use a linear probability model. The regressors are mutually exclusive variables for whether no, one, two, three, or four features of the estimates more strongly predict negative employment effects based on the competitive model and institutional factors. That is, for each of our outcomes, we estimate regression models of the form (with no constant, j indexing estimates, and $SF^{\#}$ indicated dummy variables for the number of estimate features predicting stronger negative employment effects):

$$Y_j = \beta_0 SF_j^0 + \beta_1 SF_j^1 + \beta_2 SF_j^2 + \beta_3 SF_j^3 + \beta_4 SF_j^4 + \varepsilon_j \ . \qquad (1)$$

This analysis provides some important advantages relative to the preceding figures in terms of summarizing the evidence, at the cost of losing some of the richness of those figures. In particular, this analysis averages over the sets of features of estimates we considered in the figures – for example, for the pairs of estimate features, we now obtain an average estimate for when two features of estimates more strongly predict negative effects. We are also able to do statistical inference on the results. And finally, this regression analysis avoids the ambiguity of the whether the unspecified features of the estimates in the sets of two or three features of estimates considered in the figures do or do not more strongly predict negative employment effects – because here the variables are defined to be mutually exclusive. Thus, e.g., the variable for "two estimate features" is the estimate corresponding to study estimates with exactly two features that more strongly predict negative employment effects.

The results are reported in Table 3.[22] In general, we see more systematic evidence of the conclusions we drew from the figures: when more features of estimates more strongly predict negative employment effects, the estimates are more consistent with negative employment effects. The estimates in column (1) are for the dichotomous outcome of whether the estimated elasticity is negative. There is a positive monotonic relationship between the number of features of estimates that more strongly predict negative employment effects and the probability that the estimated elasticity is negative. (Indeed for four such features, there is no variation, as we saw in the top set of bars in Figure 3A.)

We see very similar evidence in column (2) – where the outcome is a negative and significant elasticity. There is just one deviation from monotonicity, for the difference between zero and one features of estimates. The estimated coefficients are smaller than in column (1), and the differences with more estimate features predicting negative effects are smaller, implying that there is a stronger relationship between the number of features of estimates that more strongly predict negative employment effects and finding a negative employment effect without regard to significance, than finding a negative and significant one.

[22] One might be concerned that the evidence for Brazil drives the results because we have 15 studies for this country (see Appendix Table A3). However, the estimates are very similar excluding the studies of Brazil (Appendix Table A5).

Finally, in column (3), for the actual estimated elasticities, the evidence is not quite as clean with regard to a monotonic relationship, reflecting the variability in the estimates. Moreover, the average estimated elasticity is significant only for cases where two features of estimates more strongly predict a negative employment effect, although the point estimate is larger when all four features of estimates more strongly predict a negative employment effect (−0.192 vs. −0.119). As reflected in the counts of estimates with different numbers of features more strongly predicting negative employment effects (Appendix Table A4), this difference in statistical significance likely reflects at least in part the small number of estimates for which all four features more strongly predict negative employment effects.

Note that Table 3 also reports the statistical significance of the estimated differences based on the number of features that more strongly predict negative employment effects. Despite the generally quite clear relationships indicating that when there are more such features, estimated employment effects are more likely to be negative, these differences often are not significant. They are, however, in a number of cases in columns (1) and (2), for tests of the difference in coefficients when all four features of estimates more strongly predict negative employment effects, vs. fewer features.

Finally, we can adapt this framework to test more explicitly which features of estimates are more likely to lead to evidence of negative employment effects, or a larger negative elasticity, based on the competitive model and institutional factors We do this by modifying equation (1), so that for the variables corresponding to one, two, or three features of estimates, we alternatively define these to include or to exclude each estimate feature. For example, to ask whether evidence that the minimum wage is binding leads to stronger evidence of negative employment effects, we break each of the variables SF_j^1, SF_j^2, and SF_j^3 into two separate variables, based on whether or not the estimate is for a binding minimum wage. Denoting these, for SF_j^1, for example, SF_j^{1B} and SF_j^{1NB}, equation (1) becomes:

$$Y_j = \beta_0 SF_j^0 + \beta_1^B SF_j^{1B} + \beta_1^{NB} SF_j^{1NB} + \beta_2^B SF_j^{2B} + \beta_2^{NB} SF_j^{2NB} + \beta_3^B SF_j^{3B} + \beta_3^{3NB} SF_j^{1NB} + \beta_4 SF_j^4 + \varepsilon_j \quad . \qquad (2)$$

Note that the variables corresponding to zero features or four features are unaffected by this change. For this specification, evidence of more negative estimates for SF_j^{1B} than for SF_j^{1NB} (or similarly for SF_j^{2B} vs. SF_j^{2NB} or SF_j^{3B} vs. SF_j^{3NB}) would indicate that estimates for binding minimum wages – for the same number of estimate features more strongly predicting negative employment effects – are more likely to find evidence of negative employment effects. Hence, we also report tests of equality for these pairs of coefficients.

We report these results in Table 4, for the same outcomes as in Table 3 – a negative elasticity, a negative and significant elasticity, and the estimated elasticity itself. Each set of three columns considers one of our four features of estimates. The simplest way to interpret this evidence is to compare the estimated coefficient between the "includes feature" row and the "excludes feature" row, for a given number of features of estimated elasticities that more strongly predict negative employment effects.

Consider first the estimates in columns (1)-(3), for binding minimum wages. Column (1) reports results for whether the estimate is negative, comparing estimates that do and do not come from binding minimum wages. For estimates for which two features more strongly predict negative minimum wage effects, the estimated coefficient is larger in the "excludes feature" rows – i.e., when the estimate features that more strongly predict negative employment effects are *not* binding minimum wages. In contrast, for estimates for which three features more strongly predict negative employment effects, the coefficient is larger when one of these features *is* binding minimum wages. In column (2) as well – where the outcome is negative and significant employment effect, the relative magnitudes of these coefficients do not exhibit a consistent pattern. However, in column (3) – for the actual magnitude of the elasticity – the average elasticity is always larger negative for the features of estimates that exclude binding minimum wages. Note that the table also reports the p-values for the tests of equality of these pairs of coefficients. There is never significant evidence of differences in columns (1)-(3); the lowest p-value if 0.24 (for three features of estimates).

Columns (4)-(6) report the same kind of evidence, but this time distinguishing estimates by whether they are for the formal sector or not. In this case, too, the evidence for whether the estimated coefficient is negative or negative and significant is not unambiguously in one direction. However, in column (6) the estimated elasticity is always larger negative when formality feature is excluded. Again, none of these pairwise differences in estimates are statistically significant (except in one case in column (4), for an estimated coefficient that has no variation).

The estimates in columns (7)-(9) consider differences depending on whether the estimate features include or exclude strong enforcement. In this case, there is no clear difference. Finally, the estimates in columns (10)-(12) focus on whether the estimate is for vulnerable workers. In this case, again, there is not clear evidence that the evidence of negative employment effects, or the magnitude of the negative effect, differs systematically based on whether one of the estimate features is a focus on vulnerable workers.[23]

Note that the specification in Table 4 is different from what might be viewed as a standard meta-regression that simply includes, on the right-hand side, dummy variables for the different study features. A regression like this takes no account, however, of whether (for example) studies with binding minimum wages tend to have only one study feature that more strongly predicts negative employment effects, while studies focusing on the formal sector tend to have more features that more strongly predict negative employment effects. If studies are unlikely to detect negative employment effects unless multiple features of the study more strongly predict negative employment effects, then there are important interactions between specific study features and the number of features that more strongly predict negative employment effects, which is why we think the specifications in Table 4 are particularly useful.

Nonetheless, we have estimated versions of the more standard meta-regression, and report the results in Table 5. In the first three columns, we omit the weakest study feature in terms of predicting negative employment effects (non-binding, no enforcement, informal sector, and all workers). In the next three columns we use a more parsimonious model, retaining only the strongest such study feature (binding,

[23] The estimates in column (12) provide a nice illustration of why the evidence from the columns for whether there is a negative estimated effect or a negative and significant estimated effect can be more reliable that the evidence for the estimated elasticity, as the latter can be sensitive to outliers. For the estimates for studies with one feature that more strongly predicts negative employment effects, the coefficients in columns (10) and (11) are larger for the studies that do not focus on vulnerable workers. But the estimated elasticity (column (12)) is larger (negative) for the studies that do focus on vulnerable workers. There are only four studies in this category (focus on vulnerable workers, and no other features that more strongly predict negative employment effects), and one of these has an extreme estimated elasticity (−1.99).

strong enforcement, formal sector, and vulnerable workers).[24] In this table, the clearest evidence is that studies focusing on vulnerable workers are most likely to provide evidence of negative employment effects, and there is also some evidence of this (although a good deal weaker) for studies of countries with strong enforcement. However, Table 4 – which, as explained above, compares results based on study features for studies including the same number of features that more strongly predict negative employment effects – suggests we have to be a bit cautious about this interpretation. In Table 4, for studies of vulnerable workers we find stronger evidence of negative effects for studies with two features that more strongly predict negative employment effects (in all three columns (10)-(12), although the p-values for equal effects are all slightly above 0.1), but not for studies with other numbers of features that more strongly predict negative employment effects.

To summarize, Tables 3, 4, and 5 consider three different but related kinds of evidence. Table 3 focuses simply on the number of features of estimates – of the four we consider – that more strongly predict negative employment effects based on the competitive model and institutional factors. Table 4 tries to disaggregate this evidence, paying attention not only to the counts of estimate features, but also asking whether particular features of estimates among these four features are more consistently associated with evidence of negative employment effects, conditional on the number of features that more strongly predict negative employment effects. And Table 5 presents a more standard type of meta-regression that focuses on study features but without reference to how many more strongly predict negative employment effects. In general, we do not find strong evidence pointing to particular features of estimates that generate stronger evidence of negative employment effects. There is some evidence of this for studies focusing on vulnerable workers, in Table 5, but this is not robust in Table 4. However, the evidence (from Table 3) is quite clear that estimated employment elasticities based on a greater number of features that more strongly predict negative employment effects are, in fact, more likely to be negative, or negative and significant. And such estimates, to a limited but lesser extent, are more likely to take on larger negative values.

[24] To be clear, standard regressions in meta-analyses usually include other controls as well, such as for the data used, the sample size, perhaps the precision, etc.

V. Conclusions

The goal of this paper is to see whether we can make sense of the mixed evidence on the employment effects of minimum wages in developing countries. Although estimated effects tend to be negative, there is considerable heterogeneity, with many non-negative estimates. We try to distinguish between two explanations. One is that there simply is no clear evidence that minimum wages reduce employment in developing countries, in which case we should see heterogeneous estimates even across similar studies or estimates looking at workers most likely to be adversely affected by minimum wages (because they are low skill, or work in the formal sector), and in contexts where negative effects are more likely (e.g., when minimum wages are more binding). Alternatively, the heterogeneity in estimated minimum wage effects may instead reflect heterogeneity in studies along dimensions more likely or less likely to predict negative employment effects – e.g., studies of binding wages for low-skill workers vs. studies of weakly enforced minimum wages in the informal sector. To try to distinguish between these explanations, we conduct a version of a meta-analysis of the estimates from a large set of studies of minimum wage effects in developing countries.

We conclude that the evidence is much more consistent with the second explanation. That is, we find that the estimated employment effects of minimum wages in developing countries are more likely to be negative, and larger negative, when estimates focus on data and sectors for which the competitive model predicts disemployment effects and in institutional settings in which we would expect the minimum wage to have more impact. Specifically, there is more consistent evidence of negative employment effects for studies for which more features of estimates – based on the competitive model and institutional factors – predict negative employment effects, including: when the minimum wage is binding, where minimum wage enforcement is stronger, for estimates of effects in the formal sector, and when the data focus on more vulnerable (lower-wage) workers. To be precise, the evidence is less clear on whether a particular one of these features characterizes a study (although there some evidence that disemployment effects are more likely to emerge from studies of vulnerable – i.e., lower-wage – worker). The difficulty of pinning down exactly which study features matter the most for whether the evidence points to negative employment effects likely arises because studies can vary on many dimensions (corresponding to all of these features). But the evidence is clearer that when all or most features of a study predict negative employment effects, the study is in fact more likely to find negative employment effects.

One implication of this conclusion is that the apparently mixed evidence is a result of many studies focusing on data, sectors, or institutional settings in which negative employment effects are less likely. As such, many of these studies may be uninformative about the effects of minimum wages when the competitive model and institutional factors more strongly predict negative employment effects: studies of binding minimum wages, with strong enforcement, focusing on vulnerable workers in the formal sector. On the other hand, the implication is that in some developing country settings negative employment are in fact less likely – e.g., for informal sector employment. However, one implication is that precisely when minimum wages in developing countries could potentially deliver the most benefits – when minimum wages are binding and enforced, and when they apply to vulnerable workers in the formal sector – the disemployment effects are most apparent, implying that minimum wages in developing countries may present more of a tradeoff between higher wages and lower employment than might be apparent from a simpler look at the evidence across studies of employment effects in developing countries. Hence, in assessing the wisdom of minimum wage increases in developing countries, it is important also to weigh evidence on other outcomes, such as whether higher minimum wages in developing countries raise incomes of low-income families – benefits that might offset the costs of some job losses for vulnerable workers. Gindling (2018) suggests that, overall, minimum wages tend to reduce poverty in developing countries, but only modestly.[25]

Finally, one dimension we do not explore is whether monopsony power is sometimes relevant. There are some cases of positive estimates (although not many) with features for which the competitive

[25] He also suggests effects will vary across countries depending on a variety of factors including the share of informal workers not covered, coverage of secondary workers in families, and whether minimum wage workers tend to be heads of low-income households.

model and institutional factors predict negative employment effects. (These positive estimates are more prevalent in studies with only one feature for which the competitive model and institutional factors predict negative effects; see, e.g., Figures 2A and 3A). Monopsony is a potential explanation, but not the only one; for example, the standard two-sector competitive model predicts positive employment effects in the informal sector. Testing whether monopsony can sometimes explain a positive effect of the minimum wage on employment is hard. Recent work for the United States (Azar et al., 2019; Munguia Corella, 2020) tries to do this using disaggregated, sub-national variation in measures of labor market concentration and worker mobility, and finds some evidence consistent with monopsony power in more-rural, less-dense counties. There is no way to apply this type of analysis to the "study-level" observations we use in the present paper, but exploring whether monopsony power sometimes generates positive effects of the minimum wage on employment in developing countries would be useful.

Still, at this point our view is that there is no clear reason, based on the existing evidence, to conclude that competitive models of the labor market do not do a good job of characterizing low-wage labor markets in developing countries. Evidence of negative employment effects tends to emerge where the competitive model predicts it should, although this conclusion does not apply to every study, and different conclusions more consistent with monopsony could hold for some countries or more likely sub-regions of countries.

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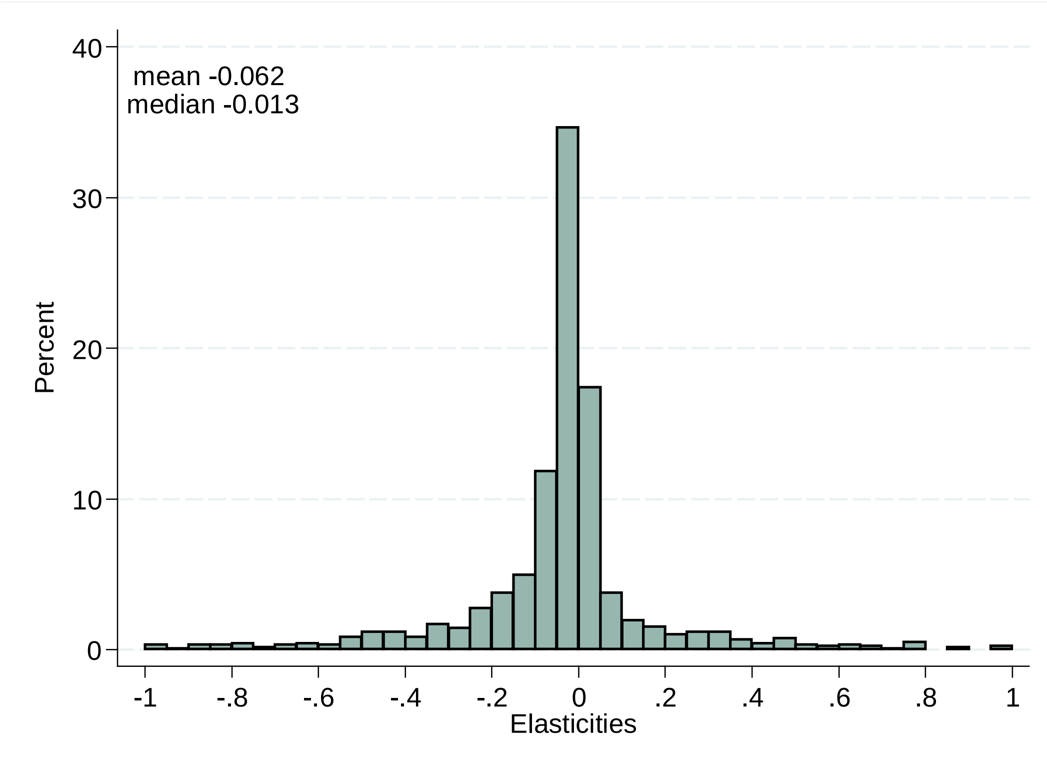
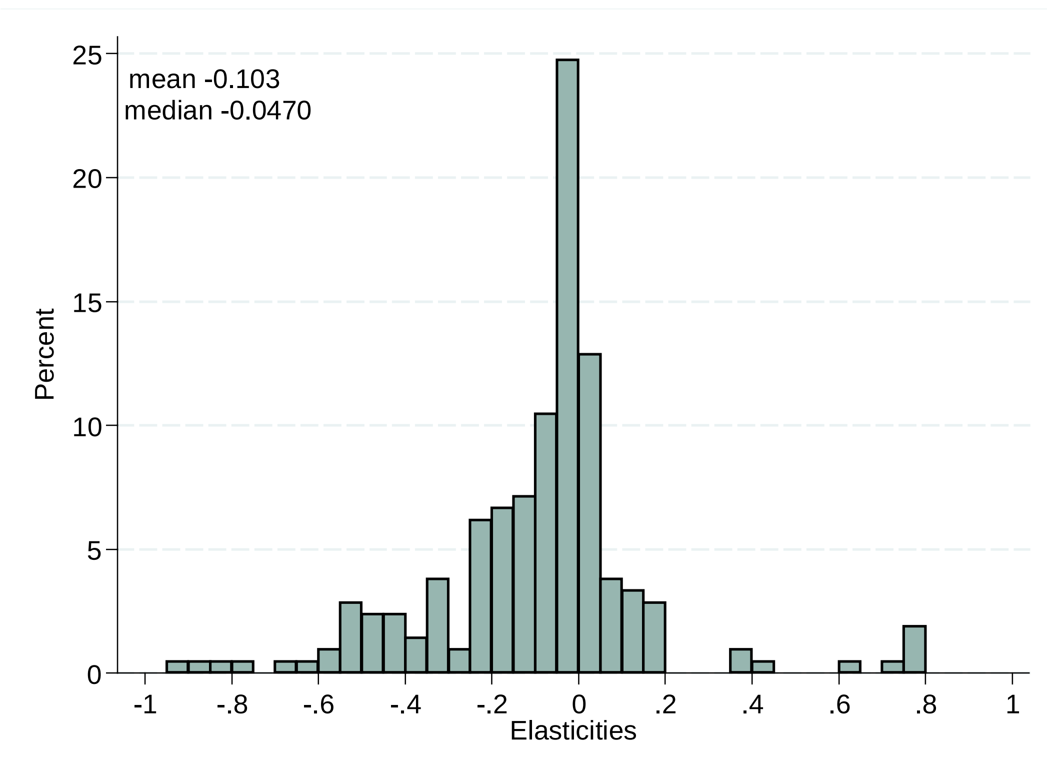
Figure 1: Histogram of Estimated Elasticities in Surveyed Studies and Authors' Preferred Elasticities

A. All estimates



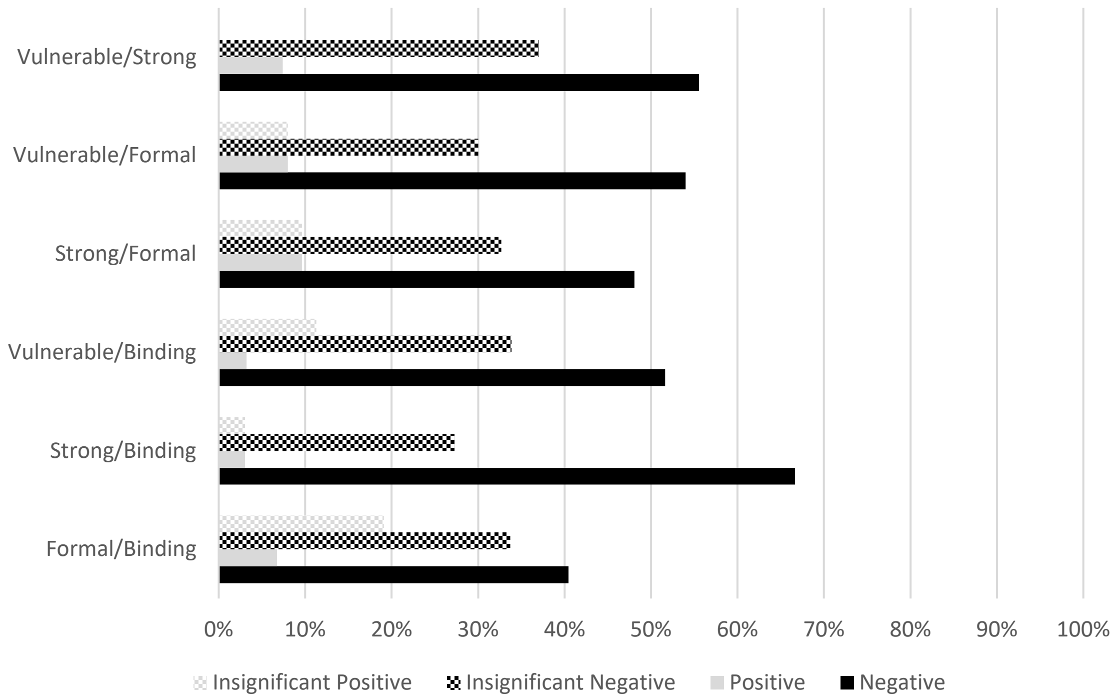
B. Authors' Preferred Estimates



Note: We drop from the histograms (but include in the means and medians) the observations that are larger than 1 in absolute value to eliminate outliers and because most of the observations are between −1 and 1.

Figure 2A: Results by Features of Estimates, Authors' Preferred Estimates, Sign and Significance

A. Both features more strongly predict negative effects



B. One feature more strongly predicts negative effects

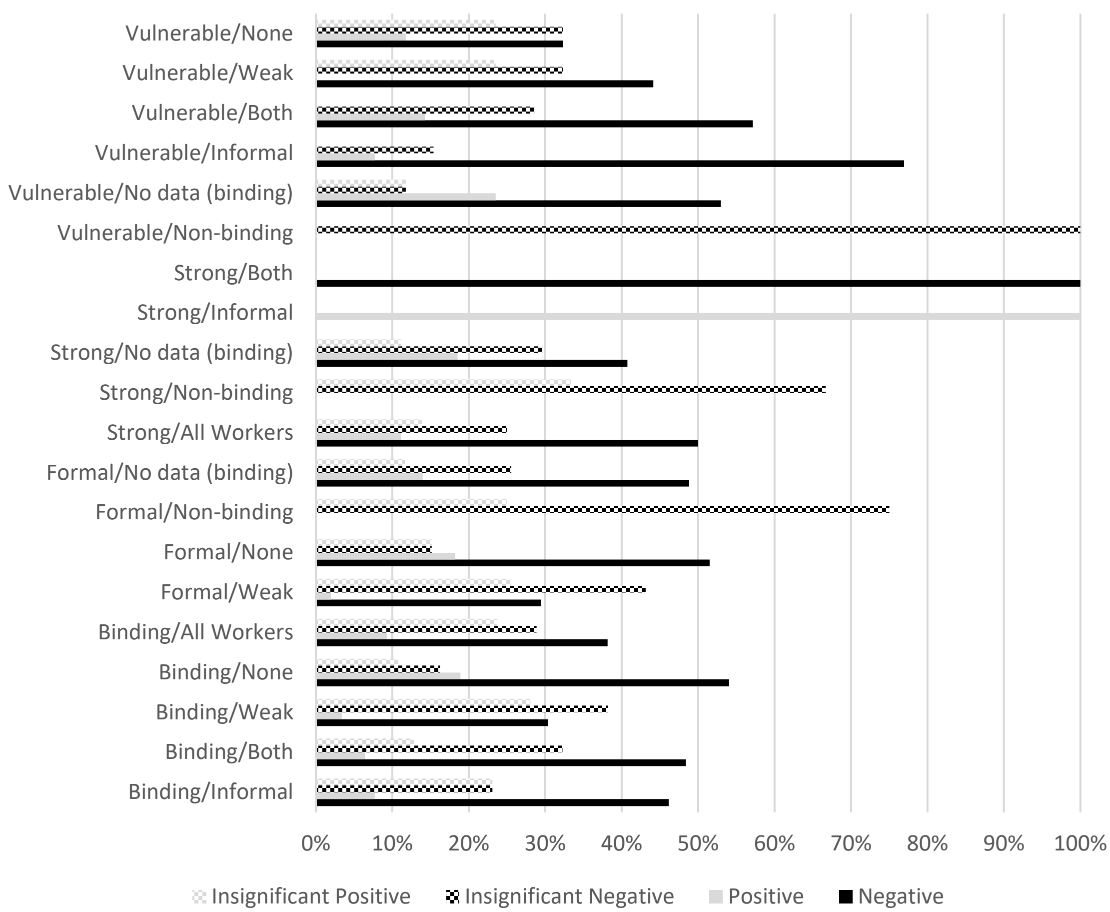
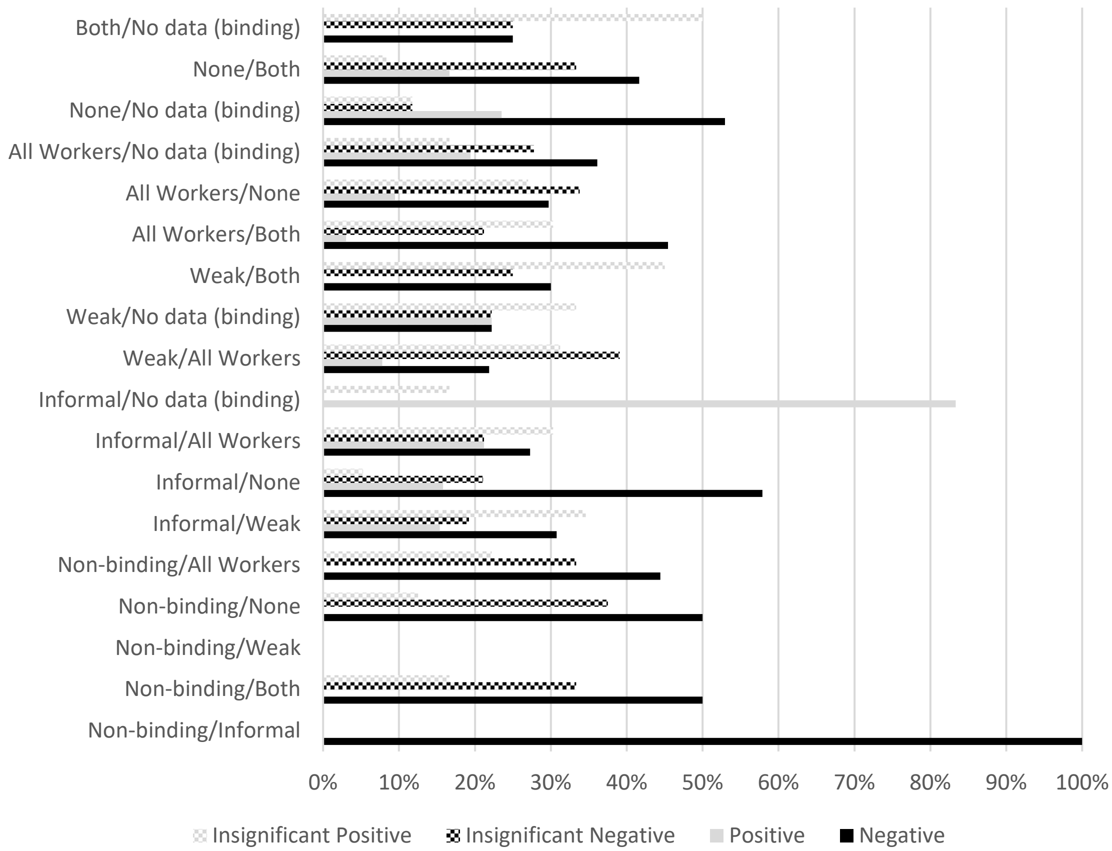


Figure 2A (continued)

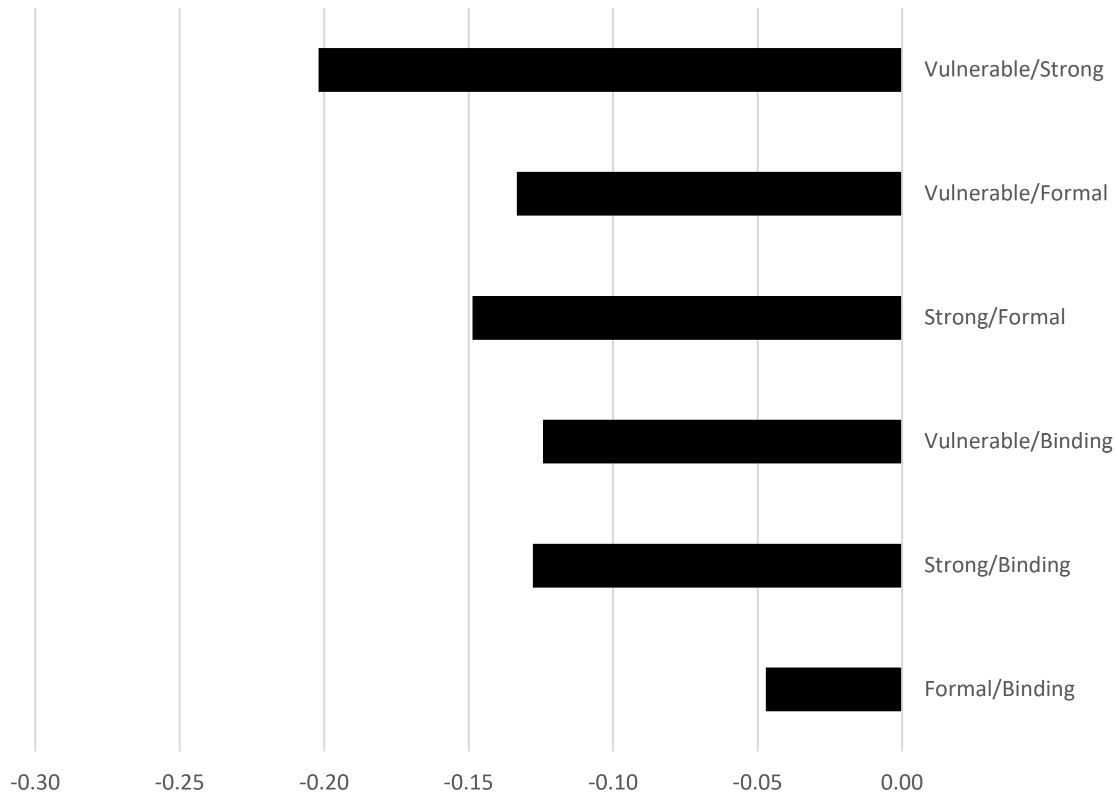
C. Neither feature more strongly predicts negative effects



Note: Results labeled “Positive” or “Negative” have p-values ≤ 0.1.

Figure 2B: Results by Features of Estimates, Authors' Preferred Estimates, Average Elasticities

A. Both features more strongly predict negative effects



B. One feature more strongly predicts negative effects

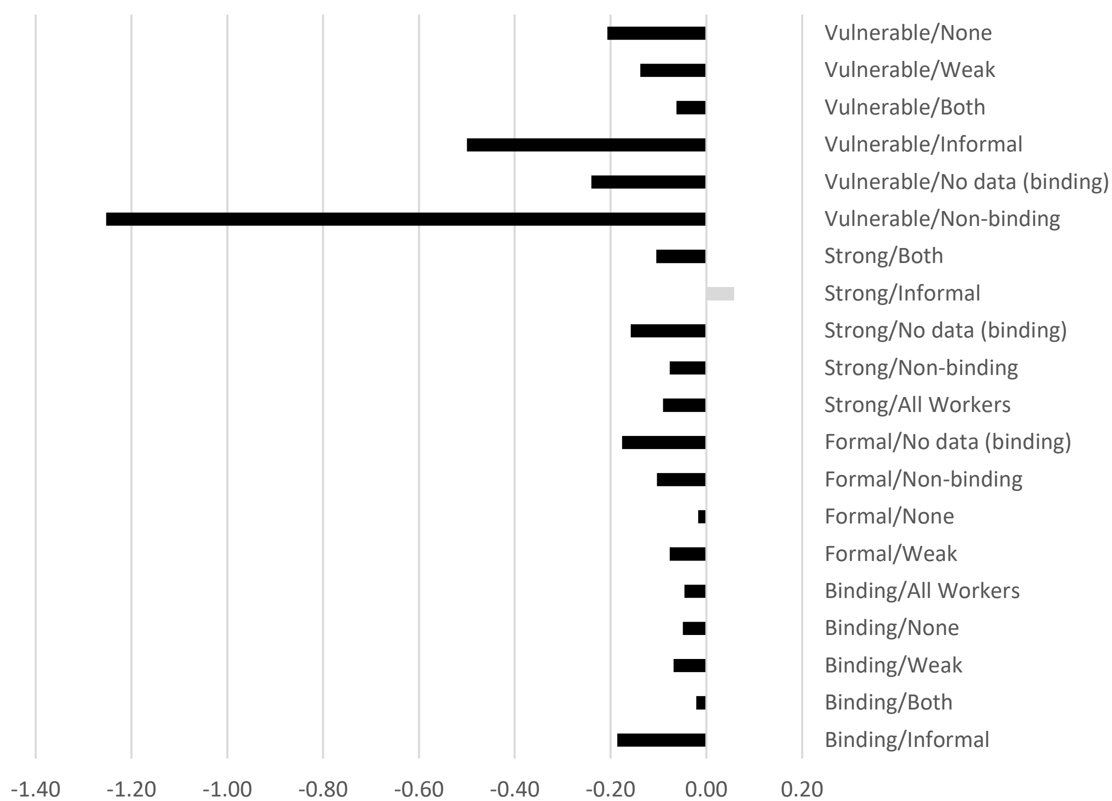


Figure 2B (continued)

C. Neither feature more strongly predicts negative effects

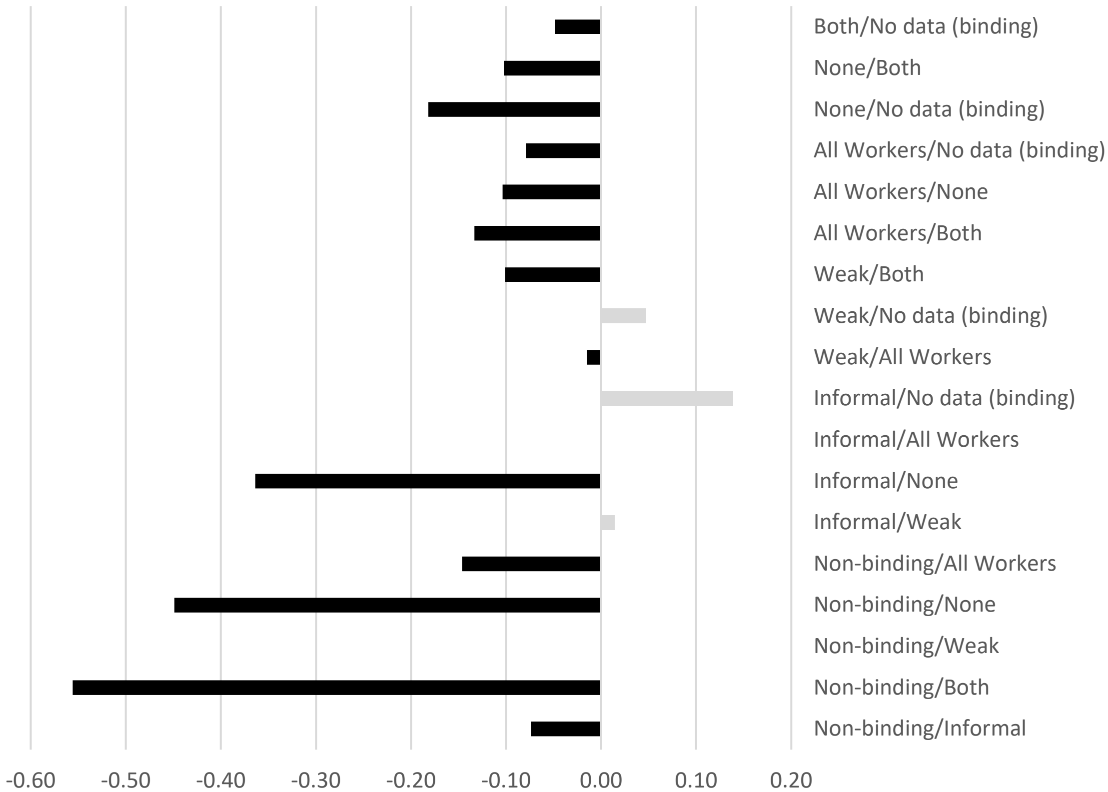
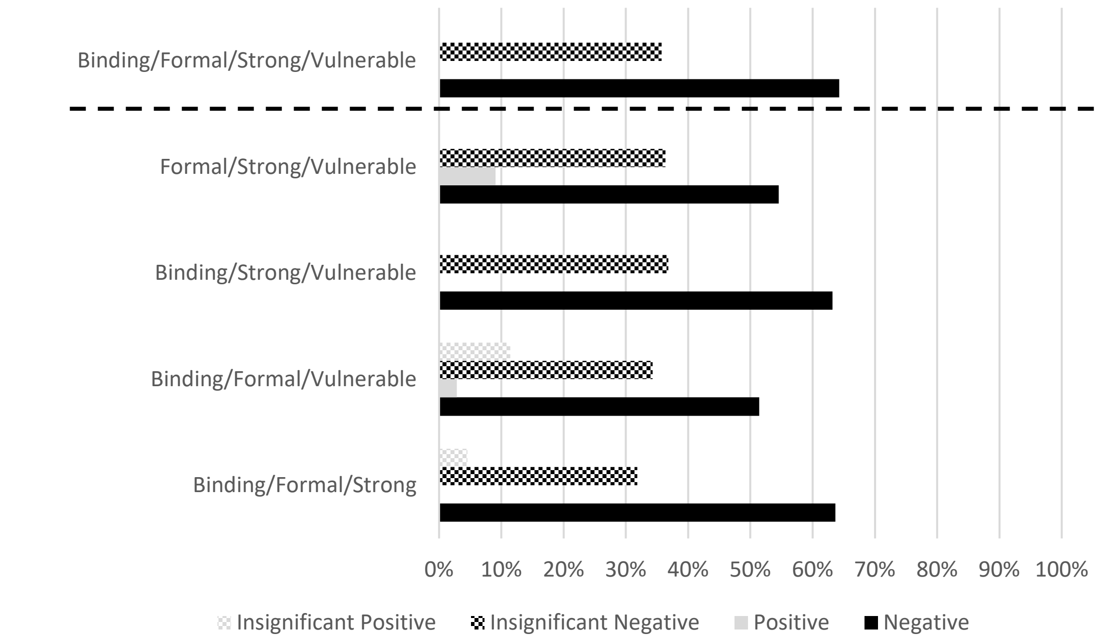
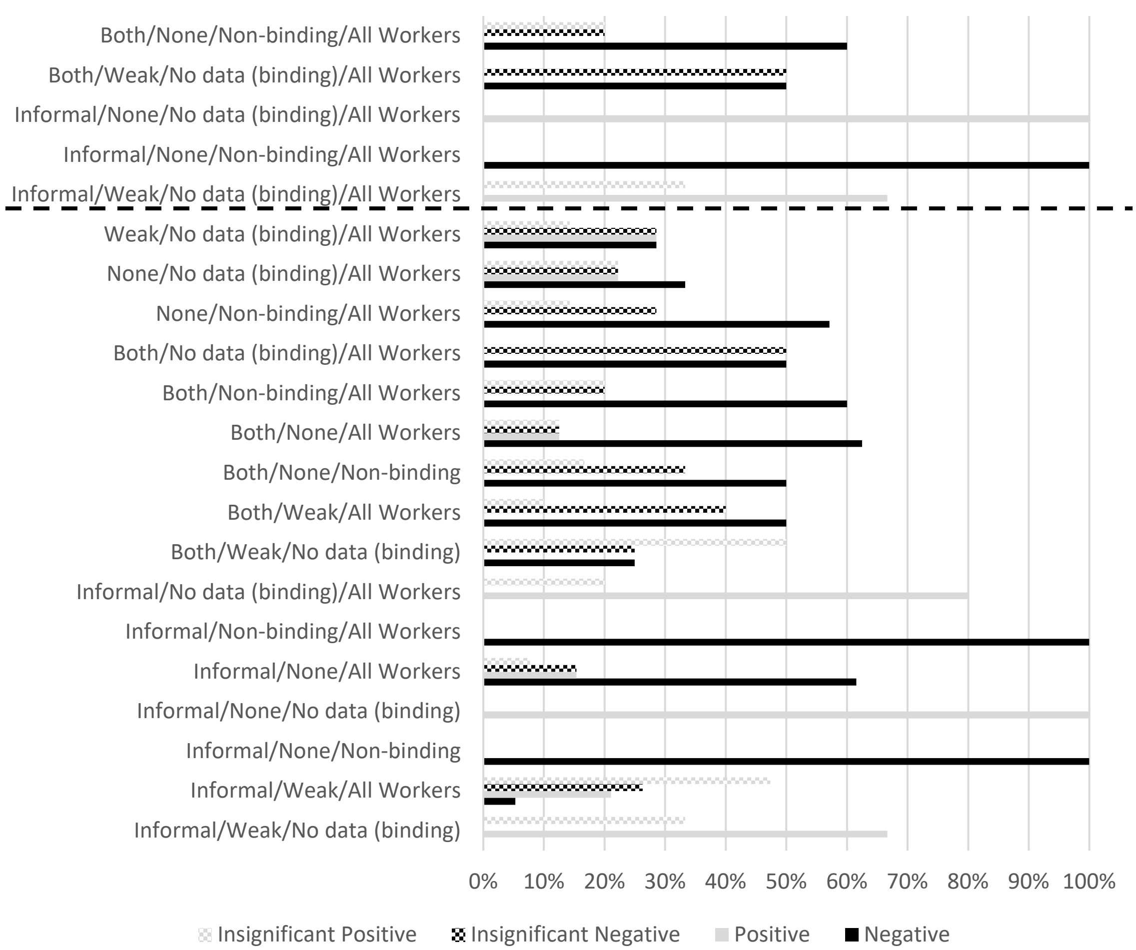


Figure 3A: Results by Features of Estimates, Authors' Preferred Estimates, Sign and Significance

A. Three or four features more strongly predict negative effects



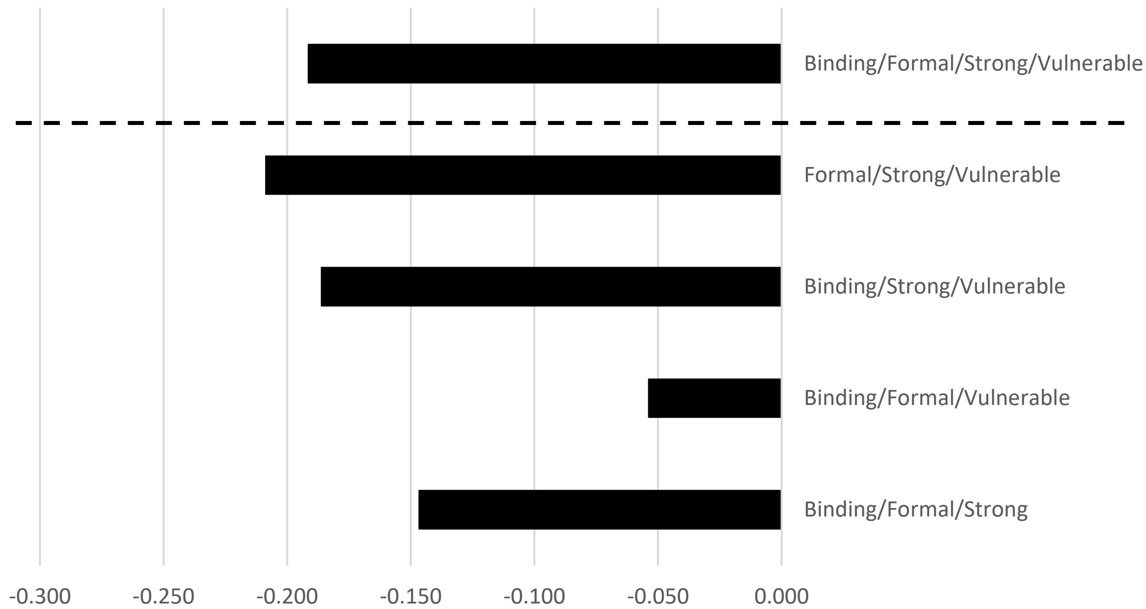
B. Three or four features do not more strongly predict negative effects



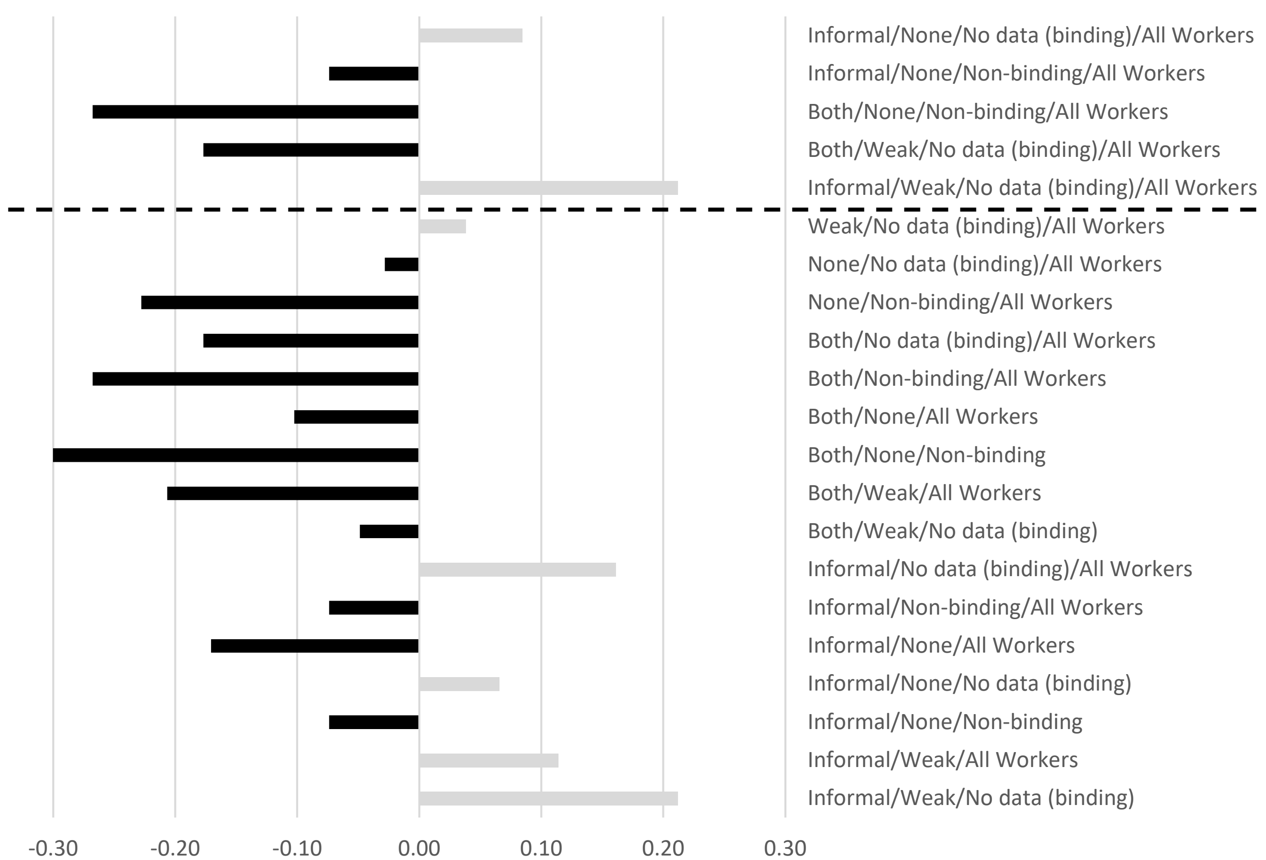
Note: Entries with no estimates are not shown. Entries above the dashed line are for four-way classifications of features of estimates. Results labeled "Positive" or "Negative" have p-values ≤ 0.1.

Figure 3B: Results by Features of Estimates, Authors' Preferred Estimates, Average Elasticities

A. Three or four features more strongly predict negative effects



B. Three or four features do not predict stronger negative effects



Note: Entries with no estimates are not shown. Entries above the dashed line are for four-way classifications of features of estimates.

Table 1. Summary of Estimated Elasticities from Surveyed Studies and Authors' Preferred Estimates

	Mean	Median	Minimum	Maximum	Standard dev.	Obs.
All estimates	-0.062	-0.013	-4.73	4.51	0.458	1,232
Authors' preferred estimates	-0.103	-0.047	-2.53	2.19	0.502	223

Table 2. One-Way Classification of Estimation Results by Features of Estimates, Authors' Preferred Estimates

	Negative and significant	Positive and significant	Insignificant	Total
A. Binding				
Binding	69 (43.4%)	11 (6.9%)	79 (49.7%)	159 (100.0%)
Not binding	4 (36.4%)	0 (0.0%)	7 (63.6%)	11 (100.0%)
No data	22 (41.5%)	11 (20.8%)	20 (37.7%)	53 (100.0%)
B. Sector				
Formal	57 (41.9%)	12 (8.8%)	67 (49.3%)	136 (100.0%)
Informal	19 (41.3%)	8 (17.4%)	19 (41.3%)	46 (100.0%)
Both	19 (46.3%)	2 (4.9%)	20 (48.8%)	41 (100.0%)
C. Enforcement				
Strong	33 (52.4%)	6 (9.5%)	24 (38.1%)	63 (100.0%)
Weak	29 (29.6%)	5 (5.1%)	64 (65.3%)	98 (100.0%)
No enforcement	33 (53.2%)	11 (17.7%)	18 (29.0%)	62 (100.0%)
D. Workers				
Vulnerable	41 (50.6%)	6 (7.4%)	34 (42.0%)	81 (100.0%)
All workers	54 (38.0%)	16 (11.3%)	72 (50.7%)	142 (100.0%)

Notes: Each cell reports the number of results and the row percent (in parentheses). Each category adds to the total of 221 preferred estimates. We classify results as significant if the p-value ≤ 0.1.

Table 3. Meta-Analysis Regressions, Based on Counts of Features of Estimates More Strongly Predicting Negative Employment Effects

	(1)	(2)	(3)
Variables: number of features of estimates that more strongly predict negative employment effects	Negative estimate (LPM)	Negative and significant estimate (LPM)	Estimated elasticity
No estimate features	0.538^{***}	0.385^{**}	-0.074
	(0.190)	(0.152)	(0.112)
One estimate feature	0.647^{***}	0.353^{***}	-0.086
	(0.087)	(0.084)	(0.057)
One = No (p-value)	0.614	0.853	0.925
Two estimate features	0.709^{***}	0.417^{***}	-0.119^{***}
	(0.052)	(0.079)	(0.042)
Two = One (p-value)	0.561	0.557	0.595
Two = No (p-value)	0.393	0.849	0.711
Three study features	0.810^{***}	0.476^{***}	-0.060
	(0.091)	(0.105)	(0.118)
Three = Two (p-value)	0.267	0.593	0.662
Three = One (p-value)	0.154	0.338	0.867
Three = No (p-value)	0.185	0.614	0.927
Four estimate features	1	0.643^{***}	-0.192
	(0)	(0.058)	(0.127)
Four = Three (p-value)	0.040	0.183	0.433
Four = Two (p-value)	0.000	0.023	0.590
Four = One (p-value)	0.000	0.006	0.452
Four = No (p-value)	0.018	0.117	0.490
Joint test: Four = Three = Two = One (p-value)	0.000	0.033	0.880

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. There are 233 observations.

Note: LPM = linear probability model. The variables are defined to be mutually exclusive. For the LPMs, standard errors are clustered by study. Note that for the estimates in column (1), there is no variation in the dependent variable for the “Four estimate features” variables, which is why there is no variation in the estimated coefficient.

Table 4. Meta-Analysis Regressions, Testing Specific Features of Estimates More Strongly Predicting Negative Employment Effect, Conditional on Number of Such Features

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Negative estimate (LPM)	Neg. and sign. est. (LPM)	Estimated elasticity	Negative estimate (LPM)	Neg. and sign. est. (LPM)	Estimated elasticity	Negative estimate (LPM)	Neg. and sign. est. (LPM)	Estimated elasticity	Negative estimate (LPM)	Neg. and sign. est. (LPM)	Estimated elasticity
Feature:	Binding			Formal sector			Strong enforcement			Vulnerable workers		
Variables: number of estimate features that more strongly predict negative employment effects												
No estimate features	0.538***	0.385**	-0.074	0.538***	0.385**	-0.074	0.538***	0.385**	-0.074	0.538***	0.385**	-0.074
	(0.191)	(0.153)	(0.113)	(0.191)	(0.153)	(0.113)	(0.191)	(0.153)	(0.113)	(0.191)	(0.153)	(0.113)
One estimate feature (includes feature)	0.649***	0.378***	-0.053	0.800***	0.400**	-0.062**	N.A.	N.A.	N.A.	0.250	0.000	-0.452
	(0.113)	(0.107)	(0.063)	(0.156)	(0.174)	(0.025)				(0.240)	(0.000)	(0.496)
One estimate feature (excludes feature)	0.643***	0.286**	-0.174	0.610***	0.341***	-0.092	0.647***	0.353***	-0.086	0.681***	0.383***	-0.055
	(0.133)	(0.124)	(0.147)	(0.105)	(0.098)	(0.071)	(0.088)	(0.085)	(0.057)	(0.095)	(0.092)	(0.050)
Equal coefficients for one estimate feature (p-value)	0.974	0.572	0.469	0.324	0.771	0.690	0.000	0.000	0.139	0.126	0.000	0.432
Two estimate features (includes feature)	0.703***	0.392***	-0.092**	0.667***	0.360***	-0.096*	0.714***	0.464***	-0.097**	0.828***	0.586***	-0.269**
	(0.065)	(0.092)	(0.036)	(0.062)	(0.083)	(0.055)	(0.077)	(0.129)	(0.042)	(0.088)	(0.139)	(0.118)
Two estimate features (excludes feature)	0.724***	0.483***	-0.188*	0.821***	0.571***	-0.181**	0.707***	0.400***	-0.128**	0.662***	0.351***	-0.060*
	(0.085)	(0.148)	(0.107)	(0.088)	(0.131)	(0.085)	(0.066)	(0.097)	(0.056)	(0.060)	(0.075)	(0.032)
Equal coefficients for two estimate features (p-value)	0.842	0.605	0.401	0.162	0.135	0.437	0.941	0.693	0.660	0.133	0.109	0.105
Three estimate features (includes feature)	0.824***	0.500***	-0.018	0.784***	0.459***	-0.045	0.857***	0.524***	-0.158**	0.794***	0.441***	-0.058
	(0.100)	(0.122)	(0.142)	(0.102)	(0.114)	(0.134)	(0.099)	(0.135)	(0.065)	(0.109)	(0.114)	(0.146)
Three estimate features (excludes feature)	0.750***	0.375**	-0.239*	1.000***	0.600***	-0.172**	0.762***	0.429**	0.038	0.875***	0.625**	-0.069
	(0.222)	(0.181)	(0.122)	(0.000)	(0.209)	(0.077)	(0.150)	(0.162)	(0.226)	(0.111)	(0.240)	(0.071)
Equal coefficients for three estimate features (p-value)	0.764	0.570	0.243	0.038	0.546	0.418	0.598	0.653	0.408	0.603	0.485	0.949
Four estimate features	1	0.643***	-0.192	1	0.643***	-0.192	1	0.643***	-0.192	1	0.643***	-0.192
	(0)	(0.059)	(0.128)	(0)	(0.059)	(0.128)	(0)	(0.059)	(0.128)	(0)	(0.059)	(0.128)

*** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered by study. There are 233 observations.

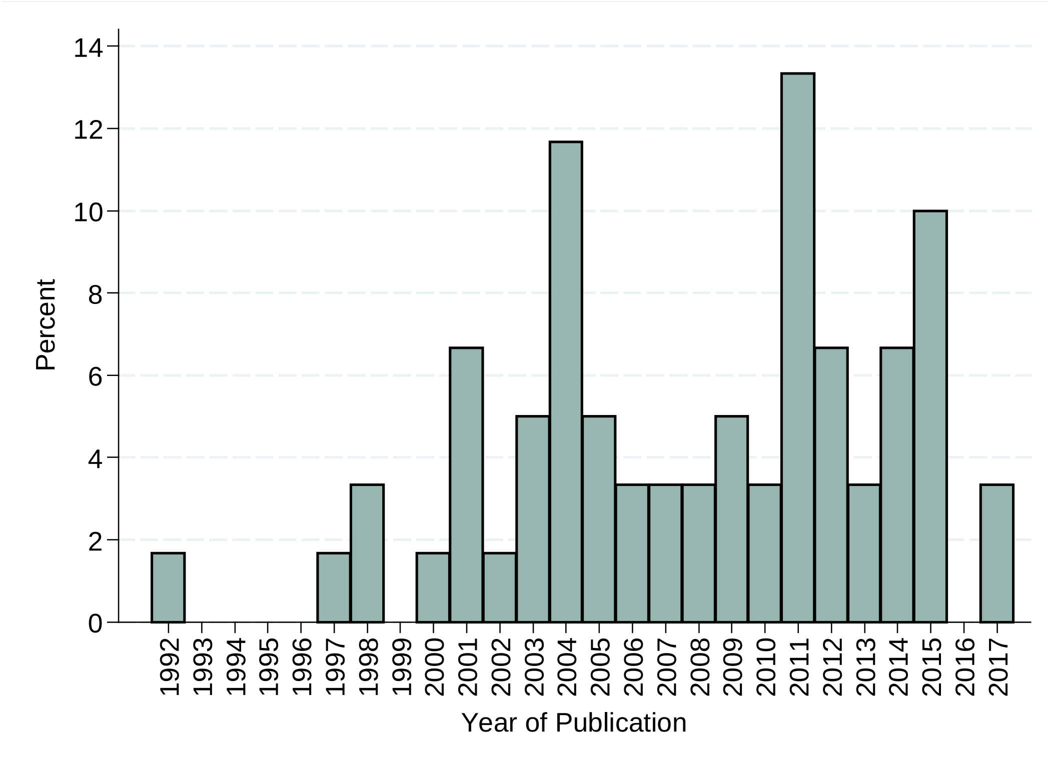
Note: LPM = linear probability model. The variables are defined to be mutually exclusive. For columns (7)-(9), "N.A." indicates that there are no estimates in the corresponding cell. Note that for the estimates in columns (1), (4), (7), and (10), there is no variation in the dependent variable for the "Four estimate features" variables, which is why there is no variation in the estimated coefficient.

Table 5. Standard Meta-Analysis Regressions, Testing Specific Features of Estimates More Strongly Predicting Negative Employment Effect

	(1)	(2)	(3)	(4)	(5)	(6)
	Negative estimate (LPM)	Neg. and sign. est. (LPM)	Estimated elasticity	Negative estimate (LPM)	Neg. and sign. est. (LPM)	Estimated elasticity
Binding	-0.028	0.235	0.299	0.103	0.049	0.106
	(0.130)	(0.195)	(0.228)	(0.074)	(0.098)	(0.087)
No data on binding	-0.194	0.126	0.245			
	(0.136)	(0.204)	(0.201)			
Strong enforcement	0.101	-0.012	-0.030	0.151^{**}	0.155	-0.028
	(0.106)	(0.124)	(0.130)	(0.069)	(0.097)	(0.067)
Weak enforcement	-0.081	-0.292^{**}	0.030			
	(0.119)	(0.144)	(0.104)			
Formal sector	0.102	-0.032	0.092	0.028	-0.051	0.058
	(0.127)	(0.093)	(0.179)	(0.089)	(0.075)	(0.102)
All sectors	0.117	0.047	0.104			
	(0.152)	(0.149)	(0.197)			
Vulnerable workers	0.121^{*}	0.102	-0.134^{**}	0.125^{*}	0.110	-0.123^{**}
	(0.070)	(0.107)	(0.060)	(0.069)	(0.101)	(0.058)
Minimum wage (baseline)	0.667^{***}	0.334	-0.405	0.543^{***}	0.339^{***}	-0.161
	(0.152)	(0.208)	(0.260)	(0.088)	(0.095)	(0.102)

*** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered by study. There are 233 observations.
Note: LPM = linear probability model.

Appendix Figure A1: Histogram of Surveyed Studies by Year



Appendix Table A1. Calculated Elasticities for Studies Not Estimating Elasticities

Study	Country	Equation	Period	Avg. empl. rate	Avg. MW	Comments
Alaniz et al. (2011)	Nicaragua	E = ln(MW)	1998-2006	Varies by group	--	The paper provides the total number of workers, the proportion of each group in the total, and the sample size including the non-employed. We use this information to calculate the employment rate by group.
Arango and Pachón (2004)	Colombia	E = MW	1984-2001	0.74	202,778.4	The minimum wage variable is the ratio (minimum wage)/(median income), so the elasticity calculation requires the mean of this variable. We do not have that, but we have median income from the paper, and obtain the average minimum wage from ILO, for the period 1991-2001. The paper estimates the effects on paid and self-employed workers. We calculate the employment rate from Table 2, which reports the number of paid and self-employed workers and the total sample including non-workers.
Baranowska-Rataj and Magda (2015)	Poland	E = ln(MW)	2003-2011	0.78 for total, varies for the rest of the groups	--	We estimate the average employment rate by group to retrieve the elasticity. The paper reports total employment, the shares in each category (gender, type of worker, etc.), and the sample size.
Bhorat et al. (2014)	South Africa	E = ln(MW)	2000-2007	0.40	--	This paper studies the share of workers by industry. We calculate the average number of workers in the treatment (Table 1) and in the control (Table 2) per year, and calculate the average employment rate (Treatment/Control+Treatment).
Carneiro and Corseuil (2001)	Brazil	E = ln(MW)	1995-1999	Varies by year	--	We use ILOSTAT data to calculate average the formal employment rate by year. We do not have data on informal employment in the same range of years, but we have the ratio of formal to informal employment, and use this ratio to estimate employment by sector. The formal to informal ratio is estimated with 2009 data (the first year reported in ILO for Brazil), so we are assuming that this ratio was the same in the sample period.
Del Carpio et al. (2014)	Thailand	E = ln(MW)	1998-2010	Varies by group. For the total is 0.71.	--	We use information from ILOSTAT to calculate the employment rate by group. The average employment rate in this period for all workers is 0.71, and the rate varies across groups. We estimate employment rates by gender and age. However, we could not determine the rates by education level; thus we applied the total employment rate (0.71) to retrieve the elasticity for education groups.
Dinkelman and Ranchhod (2012)	South Africa	E = ln(MW)	2001-2004	0.13	--	The paper reports the sample size and the number employed (Table 1). We the information to calculate the average employment rate.
Gindling and Terrell (2007)	Costa Rica	E = ln(MW)	1988–2000	0.625	--	We use data from Table 2 in the paper to estimate the average employment rate for total workers.
Grau and Landerretche (2011)	Chile	E = ln(MW)	1996-2005	0.91	--	We do not have enough information from the paper, so we use data from ILOSTAT for the corresponding period. We estimate the employment rate by dividing the number of employed workers by the working-age population.
Hohberg and Lay (2015)	Indonesia	E = ln(MW)	1997-2007	0.664	--	The paper reports the employment rates in Table 1.
Maloney and Nuñez Mendez (2004)	Colombia	E = ln(MW)	1997-1999	--	--	The authors use dummies for brackets of the initial individual wage relative to the minimum wage, to estimate the impact of a change in the minimum wage throughout the wage distribution. Hence, the non-employed are not included, and they estimate the effect of the minimum wage on the share in each bracket. We use the shares in the brackets to retrieve the elasticity (Table 2). Also, the authors estimate and report an average employment elasticity of −0.15. (This is not stated in any table; it is a calculation reported by the authors in the results section.) We use the average elasticity calculated by the authors and our estimations of the elasticities by brackets.
Menon and Meulen Rodgers (2017)	India	E = ln(MW)	1983-2008	Varies by group	--	We use data from ILOSTAT to estimate the employment rate of female and male workers in India with information by region (urban and rural). We only have data from the period 1994-2010.
Montenegro and Pagés (2004)	Chile	E = ln(MW)	1960-1998	Varies by group	--	The paper gives the number of workers, but does not provide information on workers by age, skill level, and gender. We estimate the employment rate by group using information from ILOSTAT. The data are from 1998 only (we could not find data before this year).
Strobl and Walsh (2003)	Trinidad and Tobago	E = MW	1996-1998	294.3 males 167 females	7	The authors study the effect of the implementation of the minimum wage on bound vs. not bound workers, based on wages prior to the minimum wage, by sex, for small and large firms. For each category, they report the percent change in the wage bill if all workers are topped up to the minimum wage, which we use to compute the percent change in the wage for bound workers. And they report the raw baseline rate of job loss for low-wage (bound) workers, by sex. We use these for both small and large firms. Thus, the elasticity is calculated as the marginal effect on job loss, multiplied by the ratio of the proportional change in the wage bill divided by the rate of job loss.

Note: We are estimating the employment rate elasticities. For example, in Alaniz et al. (2011), the estimated effect of the log minimum wage on the probability of being employed is −0.31 for all workers. The paper reports an employment rate in the sample of 0.58, so the elasticity of −0.53 results from dividing −0.31 by 0.58.

Appendix Table A2. Surveyed Studies, Estimated and Calculated Elasticities, and Classifications of Estimates (Authors' Preferred Estimates)

Study	Country	Elasticity	Binding	Sector	Enforcement	Vulnerable	Comments
Alaniz et al. (2011)	Nicaragua	-0.898***	Yes	Formal	Strong	Vulnerable	Coefficients are unique for the categories.
		-0.834	Yes	Formal	Strong	Vulnerable	
		-0.533***	Yes	Formal	Strong	All Workers	
Alatas and Cameron (2008)	Indonesia	-0.20	Yes	Informal	None	All Workers	Different time periods.
		-0.459***	Yes	Informal	None	All Workers	
		-0.016	Yes	Informal	None	All Workers	
		-0.16*	Yes	Informal	None	All Workers	
		0.037	Yes	Formal	None	All Workers	
		0.032	Yes	Formal	None	All Workers	
		-0.41*	Yes	Informal	None	All Workers	
Arango and Pachón (2004)	Colombia	-0.407**	Yes	Both	Weak	All Workers	Heads and non-heads of households.
		-1.205***	Yes	Both	Weak	All Workers	
Baranowska-Rataj and Magda (2015)	Poland	-0.186***	N.d.	Formal	Strong	All Workers	Coefficients are unique for the categories.
		-0.365***	N.d.	Formal	Strong	Vulnerable	
Bell (1997)	Mexico	-0.027	Yes	Formal	Weak	Vulnerable	Different econometric models: with and without time fixed effects.
	Colombia	-0.182	No	Formal	None	All Workers	
		-0.337***	Yes	Formal	Weak	All Workers	
		-0.033*	Yes	Formal	Weak	Vulnerable	
Bhorat et al. (2014)	South Africa	-0.130***	Yes	Formal	Weak	All Workers	Different econometric models: with and without covariates.
		-0.082	Yes	Formal	Weak	All Workers	
Broecke and Vandeweyer (2015)	Brazil	-0.022***	Yes	Both	Weak	All Workers	Different units: regions and individuals. Different econometric models: with and without lags; different fixed effects.
		-0.014	Yes	Both	Weak	Vulnerable	
		-0.047	Yes	Both	Weak	Vulnerable	
		-0.026	Yes	Both	Weak	Vulnerable	
Carneiro (2004)	Brazil	0.018**	N.d.	Informal	Weak	All Workers	Coefficients are unique for the categories
		-0.005	N.d.	Formal	Weak	All Workers	
Carneiro and Corseuil (2001)	Brazil	2.097	Yes	Formal	Weak	All Workers	Different time periods.. .
		-0.551	Yes	Informal	Weak	All Workers	
		0	Yes	Informal	Weak	All Workers	
		-2.530	Yes	Formal	Weak	All Workers	
		1.185	Yes	Formal	Weak	All Workers	
		0.718	Yes	Informal	Weak	All Workers	
		0	Yes	Informal	Weak	All Workers	
		-0.055	Yes	Formal	Weak	All Workers	
		-0.178	Yes	Formal	Weak	All Workers	
		0.754	Yes	Informal	Weak	All Workers	
Castillo-Freeman and Freeman (1992)	Puerto Rico	-0.54***	Yes	Formal	None	All Workers	Different time periods. .
		-0.91***	Yes	Formal	None	All Workers	
Chun and Khor (2010)	Indonesia	-0.112**	Yes	Formal	None	All Workers	Coefficients are unique for the categories.
		-0.027	Yes	Formal	None	Vulnerable	
Comola and Mello (2011)	Indonesia	0.087***	N.d.	Informal	None	All Workers	Different econometric methods of estimation: OLS and SUR.
		-0.053	N.d.	Formal	None	All Workers	
		0.082***	N.d.	Informal	None	All Workers	
		-0.052***	N.d.	Formal	None	All Workers	
		-0.028***	N.d.	Formal	None	Vulnerable	
		0.027***	N.d.	Informal	None	Vulnerable	
Del Carpio et al. (2015)	Indonesia	-0.069***	Yes	Informal	None	Vulnerable	Different vulnerable groups: low-education and female workers.
		-0.196***	Yes	Informal	None	Vulnerable	
		-0.034**	Yes	Formal	None	All Workers	
		-0.026*	Yes	Informal	None	All Workers	
		-0.043	Yes	Informal	None	Vulnerable	
Del Carpio et al. (2014)	Thailand	-0.171***	Yes	Formal	Strong	Vulnerable	Coefficients are unique for the categories.
		-0.078**	Yes	Both	Strong	All Workers	
		-0.041	Yes	Both	Strong	Vulnerable	
		-0.011	Yes	Formal	Strong	All Workers	
Dinkelman and Ranchhod (2012)	South Africa	-0.138	Yes	Formal	Weak	Vulnerable	Different econometric models: with and without covariates.
		-0.192	Yes	Formal	Weak	Vulnerable	
Dung (2017)	Vietnam	-0.527**	No	Both	None	All Workers	Different sectors. Type of workers: part-time and full-time.
		-0.157	No	Both	None	All Workers	
		-0.614***	No	Both	None	All Workers	
		-0.216*	No	Both	None	All Workers	
Fajnzylber (2001)	Brazil	-0.05***	Yes	Informal	Weak	Vulnerable	Different econometric models: with and without lags (formal): long-run and short-run (informal).
		-0.08***	Yes	Formal	Weak	Vulnerable	
		-0.05***	Yes	Formal	Weak	Vulnerable	
		-0.15***	Yes	Informal	Weak	Vulnerable	
		-0.10***	Yes	Formal	Weak	Vulnerable	
		-0.25***	Yes	Informal	Weak	Vulnerable	
		-0.35***	Yes	Informal	Weak	Vulnerable	

Study	Country	Elasticity	Binding	Sector	Enforcement	Vulnerable	Comments
Fang and Lin (2015)	China	-0.148***	Yes	Formal	Strong	Vulnerable	Different vulnerable groups: females, young adults, and low-wage workers.
		-0.213*	Yes	Formal	Strong	Vulnerable	
		-0.088**	Yes	Formal	Strong	Vulnerable	
		-0.055***	Yes	Formal	Strong	All Workers	
		-0.062	Yes	Formal	Strong	Vulnerable	
Feliciano (1998)	Mexico	-0.406**	N.d.	Formal	None	Vulnerable	Different econometric models: with and without covariates, and OLS or IV.
		-0.522***	N.d.	Formal	None	Vulnerable	
		-1.107***	N.d.	Formal	None	Vulnerable	
		-0.074	N.d.	Formal	None	All Workers	
		0.005	N.d.	Formal	None	All Workers	
		0.014	N.d.	Formal	None	All Workers	
		-0.426***	N.d.	Formal	None	Vulnerable	
		-1.13**	N.d.	Formal	None	Vulnerable	
Foguel (1998)	Brazil	-0.135***	N.d.	Both	Weak	All Workers	Coefficients are unique for the categories.
		0.60***	N.d.	Informal	Weak	All Workers	
Foguel et al. (2001)	Brazil	0.018	N.d.	Informal	Weak	All Workers	Coefficients are unique for the categories.
		-0.011*	N.d.	Formal	Weak	All Workers	
Garza Cantú and Bazaldúa (2002)	Mexico	0.754***	N.d.	Formal	None	Vulnerable	Coefficients are unique for the categories.
		-0.204**	N.d.	Formal	None	All Workers	
Gindling and Terrell (2007)	Costa Rica	-0.109*	Yes	Formal	Weak	All Workers	Coefficients are unique for the categories.
Gindling and Terrell (2008)	Honduras	-0.458***	Yes	Formal	Weak	All Workers	Large and small firms.
		0.392*	Yes	Formal	Weak	All Workers	
Grau and Landerretche (2011)	Chile	-0.312***	Yes	Both	Strong	Vulnerable	Different interactions.
		-0.339***	Yes	Both	Strong	Vulnerable	
Harrison and Scorse (2010)	Indonesia	-0.125***	Yes	Both	None	All Workers	Different sectors: one excludes textiles.
		-0.116***	Yes	Formal	None	All Workers	
		-0.123***	Yes	Both	None	All Workers	
Hernandez Diaz and Pinzon Garcia (2006)	Colombia	-0.245	Yes	Formal	Weak	Vulnerable	Coefficients are unique for the categories.
		-0.207	Yes	Formal	Weak	All Workers	
Hernandez and Lasso (2003)	Colombia	0.154	N.d.	Both	Weak	Vulnerable	Different vulnerable groups: young and low-skilled workers.
		-0.219	N.d.	Both	Weak	All Workers	
		0.005	N.d.	Both	Weak	Vulnerable	
Hertz (2005)	South Africa	-0.33	Yes	Formal	Weak	All Workers	Coefficients are unique for the categories.
		-0.46	Yes	Formal	Weak	Vulnerable	
Hohberg and Lay (2015)	Indonesia	-0.074***	No	Informal	None	All Workers	Coefficients are unique for the categories.
		0.090***	Yes	Formal	None	All Workers	
Huang et al. (2014)	China	-0.033***	Yes	Formal	Strong	All Workers	Different regions.
		-0.017***	Yes	Formal	Strong	All Workers	
		0.058***	Yes	Informal	Strong	All Workers	
		-0.017***	Yes	Formal	Strong	All Workers	
Islam and Nazara (2000)	Indonesia	-0.059***	N.d.	Formal	None	All Workers	Coefficients are unique for the categories.
Kamińska and Lewandowski (2015)	Poland	-0.027	Yes	Formal	Strong	Vulnerable	Different vulnerable groups: young and low-wage workers divided in: full-time and part-time, and temporary and permanent workers.
		-0.005	Yes	Formal	Strong	Vulnerable	
		-0.016***	Yes	Formal	Strong	Vulnerable	
		-0.010	Yes	Formal	Strong	Vulnerable	
		-0.06***	Yes	Formal	Strong	Vulnerable	
		-0.101***	Yes	Formal	Strong	Vulnerable	
		-0.049***	Yes	Formal	Strong	Vulnerable	
Lemos (2004a)	Brazil	0.004	Yes	Formal	Weak	All Workers	Different econometric models: dynamic and with covariates.
		0.003	Yes	Formal	Weak	All Workers	
		-0.038	Yes	Formal	Weak	All Workers	
Lemos (2004b)	Brazil	-0.001	Yes	Both	Weak	All Workers	Coefficients are unique for the categories.
Lemos (2004c)	Brazil	-0.001	Yes	Formal	Weak	All Workers	Different econometric models: with and without lags of employment.
		0.010	Yes	Informal	Weak	All Workers	
		-0.017***	Yes	Informal	Weak	All Workers	
		-0.004**	Yes	Formal	Weak	All Workers	
Lemos (2005a)	Brazil	0.012	Yes	Both	Weak	Vulnerable	Different econometric models and different estimation methods: with and without lags; OLS and IV.
		-0.009	Yes	Informal	Weak	All Workers	
		0.002	Yes	Formal	Weak	All Workers	
		-0.003	Yes	Formal	Weak	All Workers	
		-0.005	Yes	Both	Weak	Vulnerable	
		-0.003	Yes	Formal	Weak	All Workers	
		-0.029	Yes	Formal	Weak	All Workers	
		-0.004	Yes	Formal	Weak	All Workers	
		-0.002	Yes	Formal	Weak	All Workers	
		-0.021	Yes	Informal	Weak	All Workers	
		-0.003	Yes	Formal	Weak	All Workers	
Lemos (2005b)	Brazil	-0.005*	Yes	Both	Weak	All Workers	Coefficients are unique for the

Study	Country	Elasticity	Binding	Sector	Enforcement	Vulnerable	Comments
							categories
Lemos (2007)	Brazil	0.002	Yes	Both	Weak	Vulnerable	Different econometric models all workers: lags and no lags. Different vulnerable groups: young adults and female workers.
		-0.001	Yes	Both	Weak	All Workers	
		0.002	Yes	Both	Weak	Vulnerable	
		0.003	Yes	Both	Weak	All Workers	
Lemos (2009a)	Brazil	-0.062	Yes	Formal	Weak	All Workers	Different models: lags and no lags; with covariates and without covariates.
		0.026	Yes	Informal	Weak	All Workers	
		0.177*	Yes	Informal	Weak	All Workers	
		-0.126*	Yes	Formal	Weak	All Workers	
		0.147	Yes	Informal	Weak	All Workers	
Lemos (2009b)	Brazil	-0.045***	Yes	Both	Weak	Vulnerable	Different vulnerable groups: young adults and the affected fraction of workers (based on low wages).
		-0.096	Yes	Both	Weak	Vulnerable	
		-0.073	Yes	Both	Weak	All Workers	
Luo et al. (2011)	China	0.109***	N.d.	Formal	Strong	All Workers	Different sectors: manufacturing, construction, and wholesale.
		-0.236***	N.d.	Formal	Strong	All Workers	
		0.134***	N.d.	Formal	Strong	All Workers	
Magruder (2013)	Indonesia	-0.218***	Yes	Informal	None	All Workers	Different type of workers: full-time and self-employed. Different distance in difference-in-differences estimates: 15 and 30 miles.
		-0.090***	Yes	Informal	None	All Workers	
		0.104**	Yes	Formal	None	All Workers	
		0.127***	Yes	Formal	None	All Workers	
Majchrowska and Zółkiewski (2012)	Polonia	-0.08***	N.d.	Formal	Strong	All Workers	Different econometric models: Arellano-Bond and Blundell-Bond. Different time periods.
		-0.10***	N.d.	Formal	Strong	All Workers	
		-0.27*	N.d.	Formal	Strong	Vulnerable	
		-0.50***	N.d.	Formal	Strong	Vulnerable	
		-0.47	N.d.	Formal	Strong	Vulnerable	
Maloney and Nuñez Mendez (2004)	Colombia	-0.524***	Yes	Formal	Weak	Vulnerable	Workers with different levels of income: Workers earning between 0 and 0.5 MW, 0 and 0.7 MW and 0.7, and 0.9 MW.
		-0.345***	Yes	Formal	Weak	Vulnerable	
		-0.432***	Yes	Informal	Weak	Vulnerable	
		-0.15***	Yes	Formal	Weak	All Workers	
		-0.367***	Yes	Informal	Weak	Vulnerable	
		-0.205***	Yes	Informal	Weak	Vulnerable	
		-0.683***	Yes	Formal	Weak	Vulnerable	
Martinez et al. (2001)	Chile	-0.01	N.d.	Formal	Strong	All Workers	Different econometric methods: OLS and Stock-Watson. Different periods.
		0.04	N.d.	Formal	Strong	All Workers	
Mayneris et al. (2014)	China	-0.045	Yes	Formal	Strong	All Workers	Different regions: with and without the periphery.
		0.162	Yes	Formal	Strong	All Workers	
Menon and Meulen Rodgers (2017)	India	-1.996	No	Both	None	Vulnerable	Different regions: rural and urban. Different sectors: all industries and other industries.
		0.792***	Yes	Formal	None	All Workers	
		0.767***	Yes	Both	None	All Workers	
		0.175	No	Both	None	All Workers	
		-2.231***	Yes	Informal	None	Vulnerable	
		0.051	Yes	Informal	None	All Workers	
		1.793***	Yes	Both	None	Vulnerable	
		2.073***	Yes	Formal	None	Vulnerable	
		-0.067	Yes	Formal	None	All Workers	
		-0.787***	Yes	Informal	None	All Workers	
		2.194	Yes	Formal	None	Vulnerable	
		-2.183	Yes	Informal	None	Vulnerable	
Miranda (2013)		-0.36***	N.d.	Formal	Strong	All Workers	Different sectors: all goods and only “tradable” goods.
		-0.28***	N.d.	Formal	Strong	All Workers	
Montenegro and Pagés (2004)	Chile	0.140***	N.d.	Formal	Strong	Vulnerable	Different vulnerable groups: female and young workers.
		0.095***	N.d.	Formal	Strong	Vulnerable	
Neumark et al. (2006)	Brazil	0.068	Yes	Formal	Weak	All Workers	Coefficients are unique for the categories.
		-0.012	Yes	Formal	Weak	Vulnerable	
Ni et al (2011)	China	-0.032	N.d.	Formal	Strong	Vulnerable	Coefficients are unique for the categories.
		0.098	N.d.	Formal	Strong	All Workers	
Papps (2012)	Turkey	0.004	Yes	Formal	Weak	All Workers	Coefficients are unique for the categories.
		0.001	Yes	Formal	Weak	All Workers	
		-0.002	Yes	Informal	Weak	All Workers	
Pelek (2011)	Turkey	0.182	Yes	Informal	Weak	All Workers	Different measurements of the minimum wage: Kaitz index, real, and fraction between 0.95 and 1.05 times the minimum wage.
		0.008	Yes	Formal	Weak	Vulnerable	
		0.149***	Yes	Informal	Weak	All Workers	
		0.022	Yes	Formal	Weak	Vulnerable	
		0.024	Yes	Formal	Weak	Vulnerable	
		-0.029	Yes	Informal	Weak	All Workers	
		0.008	Yes	Formal	Weak	All Workers	
		0.024	Yes	Formal	Weak	All Workers	
Shi (2011)	China	-0.587***	N.d.	Formal	Strong	All Workers	Different sectors: construction and manufacturing.
		-0.128	N.d.	Formal	Strong	All Workers	
Strobl and Walsh (2003)	Trinidad	-0.048	Yes	Both	Strong	All Workers	Different firm sizes.

Study	Country	Elasticity	Binding	Sector	Enforcement	Vulnerable	Comments
		-0.151	Yes	Both	Strong	Vulnerable	
		-0.016	Yes	Both	Strong	Vulnerable	
		-0.036*	Yes	Both	Strong	All Workers	
Suryahadi et al. (2003)	Indonesia	-0.112***	Yes	Formal	None	All Workers	Different vulnerable groups: female and young workers.
		-0.307***	Yes	Formal	None	Vulnerable	
		-0.307***	Yes	Formal	None	Vulnerable	
Wang and Gunderson (2011)	China	-0.51	No	Formal	Strong	Vulnerable	Different regions. Different types of firms: state-owned and private.
		-0.15	No	Formal	Strong	All Workers	
		0.43	No	Formal	Strong	All Workers	
		-0.178	N.d.	Formal	Strong	All Workers	
		-0.156	N.d.	Formal	Strong	All Workers	
		-1.042**	N.d.	Formal	Strong	All Workers	
		0.166	N.d.	Formal	Strong	All Workers	
Wang and Gunderson (2012)	China	-0.225	N.d.	Formal	Strong	All Workers	Effects for different sectors of the economy like construction, retail, etc.
		0.356*	N.d.	Formal	Strong	All Workers	
		-0.202	N.d.	Formal	Strong	All Workers	
Xiao and Xiang (2009)	China	-0.022**	Yes	Both	Strong	All Workers	Different estimation methods: difference-in-differences and levels.
		-0.001***	Yes	Both	Strong	All Workers	

*** p<0.01, ** p<0.05, * p<0.1.

Notes: Vulnerable workers are young adults, less-skilled workers, female workers, or workers earning very close to the minimum wage. Informal sector includes small firms for the case of Indonesia (as suggested in some papers). Binding is defined based on evidence of positive wage effects. Most analyses are for the formal section, while some papers report results for the informal sector or the two sectors combined. Enforcement is defined by penalties in the law, following Munguia (2019). For studies for which we had to compute elasticities, we use the statistical significance of the reported employment effect. For Neumark et al. (2006), the estimate for household heads is classified as for all workers, and the estimate excluding the household head is classified as for vulnerable workers. For Strobl and Walsh (2003), the estimated elasticity for small firms, for men, is statistically significant. They also report a significant coefficient estimate for the interaction of the minimum wage variable with an indicator for large firms, for women. However, this estimate is not statistically significant; and we have no way of assessing the significance of the overall effect of the minimum wage for women working at large firms (which is this interaction plus the estimated minimum wage effect), so we do not code this estimate as statistically significant.

Appendix Table A3. Classification of Studies by Country and Bindingness

Country	Number of studies	Binding	Not binding	No data
Brazil	15	12	0	3
Chile	4	1	0	3
China	9	4	1	4
Colombia	5	4	0	1
Costa Rica	1	1	0	0
Honduras	1	1	0	0
India	1	0.8	0.2	0
Indonesia	9	6.5	0.5	2
Mexico	3	0	1	2
Nicaragua	1	1	0	0
Poland	3	1	0	2
Puerto Rico	1	1	0	0
South Africa	3	3	0	0
Thailand	1	1	0	0
Trinidad	1	1	0	0
Turkey	2	2	0	0
Vietnam	1	0	1	0

Notes: In the second through fourth columns, we average the number of results by study, and then we sum by country. The non-integers result when there is variation in bindingness across estimates in a study. For India (Menon and Meulen Rodgers, 2017), the minimum wage is non-binding in the urban areas, but it is binding in the rural areas. For Indonesia (Hohberg and Lay, 2015), the minimum wage is non-binding for the informal sector and binding for the formal sector.

Appendix Table A4. Numbers of Estimates for Sets of Estimate Covered in Figures 2A-3B

Two estimate features	Number of estimates	Three estimate features	Number of estimates	Four estimate features	Number of estimates
Both predict stronger negative effects		***All predict stronger negative effects***		***All predict stronger negative effects***	
Formal/Binding	89	Binding/Formal/Strong	22	Binding/Formal/Strong/Vulnerable	14
Strong/Binding	33	Binding/Formal/Vulnerable	35	***None predict stronger negative effects***	
Vulnerable/Binding	62	Binding/Strong/Vulnerable	19	Informal/Weak/Non-binding/All Workers	0
Strong/Formal	52	Formal/Strong/Vulnerable	22	Informal/Weak/No data (binding)/All Workers	3
Vulnerable/Formal	50	***None predict stronger negative effects***		Informal/None/Non-binding/All Workers	1
Vulnerable/Strong	27	Informal/Weak/Non-binding	0	Informal/None/No data (binding)/All Workers	2
One predicts stronger negative effects		Informal/Weak/No data (binding)	3	Both/Weak/Non-binding/All Workers	0
Binding/Informal	39	Informal/Weak/All Workers	19	Both/Weak/No data (binding)/All Workers	2
Binding/Both	31	Informal/None/Non-binding	1	Both/None/Non-binding/All Workers	5
Binding/Weak	89	Informal/None/No data (binding)	3	Both/None/No data (binding)/All Workers	0
Binding/None	37	Informal/None/All Workers	13		
Binding/All Workers	97	Informal/Non-binding/All Workers	1		
Formal/Weak	51	Informal/No data (binding)/All Workers	5		
Formal/None	37	Both/Weak/Non-binding	0		
Formal/Non-binding	4	Both/Weak/No data (binding)	4		
Formal/No data (binding)	43	Both/Weak/All Workers	10		
Strong/All Workers	36	Both/None/Non-binding	6		
Strong/Non-binding	3	Both/None/No data (binding)	0		
Strong/No data (binding)	27	Both/None/All Workers	8		
Strong/Informal	1	Both/Non-binding/All Workers	5		
Strong/Both	8	Both/No data (binding)/All Workers	2		
Vulnerable/Non-binding	2	None/Non-binding/All Workers	7		
Vulnerable/No data (binding)	17	None/No data (binding)/All Workers	9		
Vulnerable/Informal	13	Weak/Non-binding/All Workers	0		
Vulnerable/Both	7	Weak/No data (binding)/All Workers	7		
Vulnerable/Weak	34	Informal/Weak/Non-binding	0		
Vulnerable/None	34	Informal/Weak/No data (binding)	3		
Neither predicts stronger negative effects		Informal/Weak/All Workers	19		
Non-binding/Informal	1	Informal/None/Non-binding	1		
Non-binding/Both	6	Informal/None/No data (binding)	3		
Non-binding/Weak	0	Informal/None/All Workers	13		
Non-binding/None	8	Informal/Non-binding/All Workers	1		
Non-binding/All Workers	9	Informal/No data (binding)/All Workers	5		
Informal/Weak	26	Both/Weak/Non-binding	0		
Informal/None	19	Both/Weak/No data (binding)	4		
Informal/All Workers	6	Both/Weak/All Workers	10		
Informal/No data (binding)	33	Both/None/Non-binding	6		
Weak/All Workers	64				
Weak/No data (binding)	9				
Weak/Both	12				
All Workers/Both	20				
All Workers/None	74				
All Workers/No data (binding)	0				
None/No data (binding)	17				
None/Both	4				
Both/No data (binding)	33				

Note: As explained in the text, the classifications here pertain to the listed features of estimates. Thus, for example, under "two estimate features, both predict stronger negative effects," the two listed features more strongly predict negative effects and the other features are unspecified, so in actual fact in some cases three or four features of estimates may more strongly predict negative effects.

Appendix Table A5. Meta-Analysis Regressions, Based on Counts of Features of Estimates More Strongly Predicting Negative Employment Effects (Excluding Brazil)

	(1)	(2)	(3)
Variables: number of features of estimates that more strongly predict negative employment effects	Negative estimate (LPM)	Negative and significant estimate (LPM)	Estimated elasticity
No estimate features	0.667^{***}	0.444^{**}	-0.163
	(0.234)	(0.197)	(0.120)
One estimate feature	0.710^{***}	0.452^{***}	-0.178^{**}
	(0.098)	(0.108)	(0.072)
One = No (p-value)	0.864	0.972	0.915
Two estimate features	0.700^{***}	0.514^{***}	-0.166^{***}
	(0.066)	(0.086)	(0.054)
Two = One (p-value)	0.940	0.666	0.888
Two = No (p-value)	0.892	0.747	0.981
Three study features	0.789^{***}	0.447^{***}	-0.060
	(0.099)	(0.108)	(0.131)
Three = Two (p-value)	0.373	0.566	0.496
Three = One (p-value)	0.473	0.976	0.513
Three = No (p-value)	0.613	0.989	0.500
Four estimate features	1	0.643^{***}	-0.192
	(0)	(0.059)	(0.127)
Four = Three (p-value)	0.040	0.131	0.459
Four = Two (p-value)	0.000	0.216	0.851
Four = One (p-value)	0.005	0.127	0.927
Four = No (p-value)	0.161	0.339	0.869
Joint test: Four = Three = Two = One (p-value)	0.000	0.429	0.947

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Note: LPM = linear probability model. The variables are defined to be mutually exclusive. For the LPMs, standard errors are clustered by study. Note that for the estimates in column (1), there is no variation in the dependent variable for the “Four estimate features” variables, which is why there is no variation in the estimated coefficient. The only difference relative to Table 3 is the exclusion of studies for Brazil.