The Speed of Earnings Responses to Taxation and the Role of Firm Labor Demand*

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Abstract

This paper studies the speed at which workers' pre-tax earnings respond to tax changes along the intensive margin. We do so in the context of Germany, where a large discontinuity — or notch — in the tax schedule induces sharp bunching in the earnings distribution. We analyze earnings responses to two policy reforms that shift this notch outward and find clear evidence that frictions delay the earnings responses of over 38% of workers. We propose that heterogeneity in firm labor demand plays a key role in generating the observed differences in the speed of workers' earnings responses and provide supporting evidence.

JEL Classifications: H24, H31, J22, J23

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1 Introduction

Labor supply elasticities are crucial inputs to the optimal design of tax systems. Yet, estimated elasticities may be confounded by various frictions, resulting in estimates that differ from the true underlying 'structural' preference parameter (Chetty, 2012). This confounding can be particularly severe in the short-run following a tax reform — precisely the time period over which commonly used, reduced-form estimation techniques are best suited to identify labor supply responses. Understanding how large such frictions are in practice, how long they take to dissipate, and their underlying sources would help inform policy practitioners using these estimates.

The first contribution of this paper is to document and quantify earnings adjustment delays in response to two tax changes — each a rightward shift of a large tax notch — in a German setting among a relatively homogeneous, informed population of part-time workers. Over 38% of the workers who should adjust based on standard labor supply models fail to do so in the first year following the tax change and only manage to adjust in later years. Consequently, intensive margin elasticity estimates over a 1-2 year horizon are significantly attenuated relative to their long-term counterparts.[1]

Our second contribution is to propose and test for one specific mechanism that delays short-term labor supply responses. Various frictions could delay adjustment in practice (e.g. Chetty et al., 2009, 2011, 2013). Understanding these underlying mechanisms and how they might vary across contexts can inform policy evaluation and design, and improve predictions of how labor supply evolves following a tax change. Here, we argue that firm-level labor demand conditions regulate the speed with which workers respond to tax changes and use three reduced form exercises to test for expected associations between the speed of adjustment and different measures of idiosyncratic, establishment-level labor demand. We find that labor demand plays a quantitatively meaningful role in the adjustment process.

We examine short-term earnings responses in the context of German mini jobs. Mini jobs provide an ideal setting for studying earnings responses. Mini jobs are tax- and social security contribution-free up to a predetermined monthly income threshold (initially €325). Jobs with monthly earnings above this threshold are subject to social security and income tax on the *entirety* of their earnings. This creates a discontinuous jump in the average tax rate at the threshold — a notch — and strongly incentivizes workers to keep earnings at or below the notch (Kleven and Waseem, 2013). Mini jobs are very popular, employing over 7 million part-time workers in 2010, and the tax incentives result in extensive bunching at the mini job notch (Tazhitdinova, 2020). Crucially for our study of earnings responses, the earnings threshold was increased twice, to €400 in 2003 and to €450 in 2013. Following each reform, workers no longer have incentives to bunch

[1]Prior, seminal papers have used bunching techniques to document and study earnings adjustment frictions (Saez, 2010; Chetty et al., 2011; Kleven and Waseem, 2013). Gelber et al. (2020)'s recent work provides a bunching-based framework for explicitly quantifying these frictions as well as evidence on their existence and size from the U.S. Social Security Earnings Test. Our work, along with a few other recent papers, complements Gelber et al.'s findings in a new setting (Zaresani, 2020; Mavrokonstantis and Seibold, 2022).

at the old earnings threshold. Indeed, standard, frictionless labor supply models unambiguously predict that the entire excess density of workers at the old threshold should dissipate immediately after the reform as these workers increase their earnings.

Using rich administrative data, we study the earnings responses of a large sample of prime-aged, married women whose husbands have annual incomes around the national average. This sample offers several advantages. First, women in this sample all face a similarly sized, stable, and large notch across years (the precise size of the mini job tax notch depends on spousal earnings). Second, the relatively homogeneous nature of this population helps us isolate differences across firms. Finally, these women work about 10 hours a week on average and presumably have scope to adjust their labor supply.

In contrast to what would be expected in a frictionless world, we observe a sizable, excess mass of married women at the old threshold after each reform. While many workers increase their earnings and work hours in the year following each reform, 38% of the workers we expect to increase their earnings fail to do so after the 2003 reform. This falls to 29% in the second year and 25% in the third year. Non-adjustment is even more severe for the 2013 reform, which allowed for a smaller earnings increase (€50 instead of €75). While the excess mass at the old notch does eventually fully dissipate, the mass of non-adjusters is sizable enough to generate significantly attenuated, intensive-margin earnings elasticities for the first several post-reform years, a finding we confirm quantitatively using bunching techniques.

A host of potentially distinct processes could generate the observed delays in earnings adjustment. We propose that firm-level conditions play an important role in mediating worker adjustment. Specifically, we argue that heterogeneity in labor demand across firms leads to heterogeneity in the speed of worker adjustment, with workers employed at firms with more labor demand adjusting faster. Intuitively, a firm with fixed hours needs that has already optimized its workers' total hours might find it infeasible to give its workers more hours post-reform, only enabling adjustment later on as workers attrit or as hours needs expand. In contrast, firms with high labor demand, such as rapidly growing firms, should be able to adjust hours quickly and potentially immediately, taking advantage of existing workers' desires for additional hours while saving on hiring costs. Hence, we expect hours adjustments to notch shifts to be more rapid in growing firms.

We propose three reduced form tests for expected associations between workers' adjustment speeds and their establishment's labor demand conditions. We first test for a positive association between post-reform adjustment of pre-existing workers at the notch and an indicator for whether the establishment hires any new mini jobbers post-reform. This test is motivated by the assumption that giving existing workers additional work hours should be less costly than hiring and training new workers. Thus, establishments should only hire a new worker after allowing existing workers who wish to increase their hours to do so. Second, we test for an association between the growth in total non-mini job earnings — i.e workers far from the notch — and the

adjustment rates of pre-existing workers at the notch. If establishments typically scale by increasing both mini and non-mini jobbers, then changes in the total hours (or earnings) of non-mini jobbers should be a viable proxy for a firm's mini-job labor demand. Third, we predict mini job labor demand (hires as a fraction of last year's workforce) in a pre-reform, pre-announcement year using lags of establishment level hiring and growth behavior. We use our model to generate predicted hires in the first post-reform year and test for an association between predicted hires and individual adjustment propensity.

We find robust support for our hypotheses. Pre-existing, long-term mini jobbers with earnings close to the pre-reform notch are more likely to increase their earnings if they are at establishments that hire at least one new mini jobber post-reform, at establishments growing in terms of their non-mini job wagebill, or at establishments with high predicted hires. All of our specifications include occupation, industry, state, and establishment size dummies so as to compare idiosyncratic differences in labor demand across similar establishments. These results are not driven by firms that generally hire versus ones that do not: having a new hire in the reform period is differentially and more strongly associated with adjustment than having a new hire in any prior periods. Results are robust to a battery of additional individual and establishment controls suggesting that individual sorting on observables is not responsible for the observed associations. Moreover, sorting on unobservables would have to be very extreme to overturn results (Oster, 2017). We also conduct several additional robustness checks and specifications which bolster our findings. For example when pooling reforms, our results hold both within individuals and within establishments. Our results extend to the extensive margin of any adjustment within the establishment and to adjustment on hours. Adjustment is also correlated with additional labor demand proxies such as total and non-mini employment growth and industry growth.

We conclude with an assessment of the quantitative relevance of establishment labor demand for individuals' adjustment. We compare how the excess mass at the old notch dissipates post-reform across individuals at establishments with idiosyncratically different labor demand. We find the excess mass at the old notch would be up to 10-15 percentage points lower in the first post-reform years were labor demand not a constraining factor. These differences are quantitatively meaningful — comparable to the largest differences across occupations or industries — and can account for a large portion of the overall role establishments play in mediating individual adjustment.

Related Literature. Our paper makes several contributions. First, like prior work, we find evidence of earnings adjustment frictions (Chetty et al., 2011; Kleven and Waseem, 2013; Gelber et al., 2020). Our work joins other recent papers in confirming Gelber et al. (2020)'s findings of delayed adjustment following a tax change in a new and different context (Zaresani, 2020; Mavrokonstantis and Seibold, 2022). In our context, the notch affects a large number of individuals with ample room to increase hours and the reforms are well-known, easy to understand, and highly salient —

with mini jobs popularly being called "400 euro jobs" (now "450 euro jobs"). This enables us to study channels that affect the speed of adjustment beyond information, optimization errors, and salience (Chetty et al., 2009, 2013; Chetty and Saez, 2013; Kostol and Myhre, 2021).

Among those who do adjust, we document intensive-margin earnings and hours responses to a rightward shift in a tax notch among wage earners. Historically, these compensated, intensive-margin earnings and hours elasticities have been considered to be close to zero (Saez, 2010; Saez et al., 2012). Like Chetty (2012) and Gelber et al. (2020), we show that frictions may be obscuring an underlying, positive earnings elasticity. Moreover, our proposed mechanism pertains directly to adjustment on hours, potentially explaining why evidence of hours adjustment is harder to observe than adjustments to tax changes on other margins.

Second, we delve deeper into one underexplored mechanism that regulates the speed of earnings responses following a tax change. By doing so, we peer behind the curtain of an all-encompassing, fixed adjustment cost model like that in Gelber et al. (2020). Our central thesis — that firm labor demand conditions can affect the speed with which workers respond to tax and transfer changes — can help explain why labor supply elasticities vary with local labor market conditions. Indeed, our intensive-margin implications are consistent with what others have found on the extensive margin. For example, Herbst (2008) argues that extensive margin labor supply responses are cyclical, while Mogstad and Pronzato (2012) find that labor supply responses to a welfare-to-work program are attenuated during high unemployment periods.

Our findings also suggest that typical short-term elasticity estimates may not be capturing a purely supply-side response. This confounding of supply and demand side factors in short-term elasticity estimates could muddle the conclusions we draw from them.[2] Identification strategies that recover estimates of longer-term elasticities, like the approach in Chetty et al. (2013), would help, as would approaches that explicitly account for a demand-side role or local labor market conditions (e.g Kroft et al., 2020). Like Gelber et al. (2020), our work also suggests that this confounding of labor supply preferences with other factors is most severe in the first few years following a reform, implying that estimates of earnings responses over a longer time frame than 1-2 years would get us closer to recovering the underlying 'structural' labor supply elasticity.

Third, we join recent literature that emphasizes the role that firms play in mediating worker earnings responses to tax changes (Chetty et al., 2011; Best, 2014; Tazhitdinova, 2020; Saez et al., 2019). Our work is closest to Chetty et al. (2011), who trace out the equilibrium implications of search costs and hours constraints for labor supply responses to taxes. We focus on why some firms more rapidly offer increased hours to their employees post-reform than others. Studying establishment behavior is particularly relevant for understanding short-term labor supply adjustment following moderate tax changes, as workers are unlikely to leave their jobs solely for small utility gains (indeed, the vast majority of adjustment in our setting occurs within-establishment).

[2]For example, the welfare implications of important policy changes like expanding the earned income tax credit depend jointly and with opposite signs on labor supply and demand elasticities (Rothstein, 2010).

Finally, in highlighting the role of labor demand, we relate to an older literature regarding how firms choose labor in frictional environments (Nickell, 1978, 1986; Hamermesh, 1989, 1995; Bentolila and Bertola, 1990; Anderson, 1993; Pfann and Palm, 1993; Caballero et al., 1997).

Tazhitdinova (2020) is the first to apply a bunching approach to our context — mini jobs in Germany. Her complementary paper focuses on estimating earnings elasticities from bunching in yearly cross sections and argues that observed bunching responses are shaped by adjustment costs and highly dependent on the availability of jobs. Our paper emphasizes the time dimension by focusing on the un-bunching responses of existing mini jobbers following each reform and speaks to the understudied question of how and in which firms short-term labor adjustment occurs.

2 Context: German Mini Jobs

2.1 The Mini Job Tax Notch and its Two Reforms

Mini jobs are the most popular type of German marginal employment, employing more than 7 million workers in 2010 — over 17% of the labor force. These employment relationships are characterized by a maximum allowable monthly earnings limit. Workers with monthly earnings below the predetermined threshold — €325 in 1999 and subsequently increased to €400 in 2003 and €450 in 2013 — are exempt from both social security contributions (SSC) and income taxation.[3] Jobs with monthly earnings above the threshold are subject to social security and income tax on the *entirety* of their earnings. Thus, mini jobs create a discontinuity — or notch — in the tax schedule (Kleven and Waseem, 2013). We now describe the notch and the two reforms that underlie our analysis.

Mini Jobs between January 1999 – March 2003. In 1999, the mini job social security contribution exemption amounted to a 20 percentage point reduction in taxes. In Germany, both employers and employees pay SSC — approximately 20% each — on workers' gross earnings (see column (3) of Table B.2). The mini job exemption applies to the individual's SSC. Consequently, in 1999, a worker with monthly gross pay below €325 could keep all of his/her income, whereas a worker at €326 kept approximately 80% of his/her income, or €261.[4] Firms paid a special mini job SSC rate that was slightly (less than 2 percentage points) higher than the SSC rate a firm would pay on a non-mini jobber (see Table B.2).

[3] Multiple mini jobs are allowed, with the threshold applied to the sum of earnings across jobs. The threshold is binding at the monthly level; exceeding the threshold for 3 consecutive months results, when detected, in an ex-post conversion of mini jobs into regular employment. In such cases, all social security benefits are paid by the employer (source: authors' exchange with the Mini Job Zentrale).

[4] As a consequence of not paying SSC, mini jobbers do not qualify for health benefits, unemployment, or pension, though voluntary pension contributions are allowed. However, this is of limited consequence for our analysis. We study married women whose husbands have a sufficiently high income to ensure spousal health insurance coverage. Furthermore, most of our analysis centers on existing mini jobbers' responses to the two reforms and neither reform changed rules pertaining to these mini jobbers' benefits. The 2013 reform switched the voluntary pension contribution from opt-in to opt-out but this did not affect pre-existing jobs (Bundeszentrale für Politische Bildung, 2014).

In addition to being social security exempt, mini jobs are income tax exempt. For single workers who only hold a mini job, this exemption has no bite as income taxes are not due on taxable incomes below €7,000 a year, well above the €3,900 one might earn in a mini job (see Table B.3 for the annual tax schedules). For married persons, though, this tax exemption is consequential. In Germany, filing jointly always (weakly) dominates filing individually, and nearly all married couples choose to do so (Steiner and Wrohlich, 2004; Bach et al., 2009). As a consequence of pooling taxable income, any additional taxable earnings from the lower income earner get taxed at the household's marginal income tax rate (see Table B.4). Thus, exceeding the mini job threshold subjects the entirety of previously nontaxable earnings to income taxation at the household marginal tax rate, once again creating a (or exacerbating the) notch.

The budget set arising from the mini job SSC and tax exemptions between January 1999 and March 2003 is shown in Figure 1 panel (a) for both a single person and for a married woman whose husband earns approximately €40,000 (see Appendix B.3 for budget set construction details).

1 April 2003 reform. The first reform we analyze increased the mini job threshold from €325 to €400. The 2003 reform was part of the Hartz II reform package which was designed to combat unemployment and increase labor market fluidity (Deutscher Bundestag, 2002). The bill was drafted on 5 November 2002 and passed on 23 December of the same year, with a start date of 1 April 2003. In addition to increasing the mini job threshold, the 2003 reform introduced *midi jobs*, which greatly reduced the discontinuity stemming from SSC. In particular, total SSC (employer plus employee) would now increase linearly with earnings between €400 – 800, starting with the employer mini job SSC rate at €400 and only reaching the standard employer plus employee total at €800. This effectively smoothed out the SSC portion of the tax notch, though a small notch remained at the threshold from the perspective of an individual worker as the employee had to make up the difference between the standard employer rate and the higher mini job employer rate. Importantly, married workers continued to experience a large notch as income taxes remained fully exempt below the threshold and fully applied above.[5] Figure 1 panel (b) shows how the 2003 reform affected individuals' budget sets.

1 January 2013 reform. The second reform differed in that relatively less changed. It was motivated by trying to help constrained mini jobbers' wages keep up with wage growth (Deutscher Bundestag, 2012). The mini job threshold increased from €400 to €450 and the point of full phase in of social security contributions increased from €800 to €850 (see Figure 1 panel (c)).

[5]The 2003 reform also abolished a 15 hour limit on the number of hours allowed per week in a mini job. However, this hour limit was not a significant constraint pre-2003 and would only have affected rare, extremely low wage (below €5.40) jobs (Steiner and Wrohlich, 2005; Eichhorst et al., 2012). The 2003 reform also allowed workers in regular employment to hold *one* tax free mini job at a different establishment, incentivizing regular employees to start a mini job (Tazhitdinova, 2022). We restrict our attention to the adjustment behavior of workers who only hold mini jobs.

Information Dissemination. The mini job reforms were well communicated to firms. A month before each reform, an information sheet with details about the reform was sent to all firms that employed at least one mini jobber (authors' exchange with the Mini Job Zentrale). Additionally, the 2003 reform established the *mini job zentrale* — a new central point of contact for employers of mini jobbers and employees in mini jobs. All mini jobs were now to be reported directly and solely to this center for tax purposes, simplifying reporting procedures. Because this change involved a non-automatic change in reporting systems, it required compliance and awareness on the firm side. Thus, firms employing mini jobbers likely knew about each reform. Individuals were also likely more aware of these reforms than the typical tax reform. Mini jobs are popular and the threshold is salient — individuals in a regular job above the threshold have SSC automatically deducted from their paychecks, whereas no such deductions occur for at-the-threshold mini jobbers. Moreover, mini jobs are often called '400 (and later 450) euro jobs'. Our own survey in Appendix C supports the idea that individuals were well aware of the 2013 reform with only 7% of respondents choosing to respond that they heard about the reform in 2014 or later.

Other reforms and context. The other parts of the Hartz I and II packages, which covered increases in the number of local employment agencies and additional support for vocational education and are not of direct relevance. The Hartz IV labor market reforms came into effect January 2005 and lowered the generosity of the UI system, but these neither coincide with our reforms nor do they appear to have any direct impact on mini jobs (Price, 2019). Nevertheless, it is worth noting that the 2003 reform was introduced in the context of high and persistent unemployment. In contrast, the 2013 reform was introduced when unemployment was significantly lower and declining. Additionally, Germany enacted a national minimum wage for the first time in 2015, the final year in our analysis. The bill was passed on 3 July 2014, after a decade-long debate on the subject. This is relevant to mini jobbers, many of whom earned below the minimum wage of €8.50, and could have impacted the tail end of our observed responses to the 2013 reform. To the extent that earnings responses and frictions differ across reforms, one source could be differences in the macroeconomic context.

In addition to the preceding mini job reforms, employers' social security contributions due on mini jobs also increased over the period, from 22% in 1999-2003, to 25% in 2003-2006, to 30% after 2006 (see column (2) of Table B.2) (Collischon et al., 2020). Following Tazhitdinova (2020), our budget sets take these changes into account (see Appendix B.3).

3 Data

We use linked employer-employee data based on administrative records from the German Social Security system, assembled by the Institute for Employment Research (IAB) into the Integrated Employment Biographies data file (IEB) (see also Schmieder et al., 2012; Card et al., 2013; Jäger

and Heining, 2022). The data contain earnings and employment duration for all jobs covered by the social security system. Around 80% of all jobs fall within the social security system (the main exceptions being the self-employed, and civil servants) (Jäger and Heining, 2022). Earnings are reported by the worker's establishment for social security purposes, minimizing measurement error. Mini jobs are included beginning 1 January 1999. Worker and establishment identifiers allow us to construct establishment-level information from the worker-level data for mini and regular workers separately. Throughout, our "firm level" analysis is based on establishments, which are delineated by municipality (*Gemeinde*). Firms can and do operate multiple establishments across municipalities, but firm identifiers are not included in the data.

3.1 Primary Analysis Samples

We construct two primary analysis samples (see Appendix D for sample construction details).

Married Women Sample. When documenting bunching and de-bunching responses and quantifying earnings elasticities in Section 4, it is helpful to have a sample that is always subject to a large notch and for which we can construct an accurate budget set. As such, we construct a sample of prime aged, married women with husbands who earn between €33,000-53,000. We focus on women whose spouses earn between €33,000-53,000 per year since these women face similar household marginal income tax rates. The upper limit is set just below the lowest cap in the data (€54,000 in West Germany in 2002) so as to avoid situations where husband income is above the cap, complicating the calculation of marginal tax rates. The lower limit ensures that the household marginal income tax rate is relatively stable (see Table B.4). We identify married women in 2008 using the method outlined in Goldschmidt et al. (2017).[6] We then determine whether or not these identified couples in 2008 were married in prior and future years using last names in each year. This yields a sample of married women in each year with attached information on husband annual earnings. While this is not a representative sample, the earnings patterns we document are very similar to those in a random sample of all women 26-55 in West Germany (Figures A.2–A.3).

Altogether, our *married women sample* takes earnings records from 1999-2015 for all married women we can identify with spousal earnings between €33,000-53,000, aged 26-55, residing in West Germany, who do not receive any form of unemployment assistance.[7] For this sample, we

[6]This process uses geocoded addresses in 2008, last names, gender, and ages to identify members of the opposite sex, who share the same last name, live at the same location, and have an age difference of less than 15 years. Goldschmidt et al. (2017) show that this procedure identifies about 35% of married couples with both members in registered employment or unemployment (or about 17% of all married couples) and is unlikely to make false matches: 89%–94% of identified pairs are indeed married to each other.

[7]The age restrictions are set to rule out students and individuals in partial retirement, as both can be subject to additional policies that incentivize limiting monthly earnings below €400. We exclude individuals who receive unemployment insurance or means tested social assistance, individuals registered as job-searchers, and individuals participating in programs offered by the federal employment agency because unemployment benefits are reduced at earnings levels below 400, generating distortions in the earnings distribution.

can be confident in the size and stability of the notch. Moreover, the relative homogeneity of this sample makes the heterogeneous adjustment patterns we document all the more striking.

Establishment Sample. When we examine how earnings responses vary with establishment labor demand in Section 5, we require information on *all* workers at the establishment, not just married women. As such, we construct a second sample — our *establishment sample*. For consistency, we focus on establishments that tend to employ married women in mini jobs. Specifically, for the 2003 reform we take all the establishments that employed at least one low-earnings (below €1,400 a month) woman in our married women sample in January 2002 - March 2003. For the 2013 reform we study all establishments that employed at least one low-earnings married woman in 2012. For these establishments, we construct establishment-level variables using *all* workers at the establishment and we study the adjustment of *all* mini jobbers – not just women – at the pre-reform notch. For added precision and ease of interpretation, we further concentrate on the outcomes of workers presumably constrained by the notch and likely to remain at the same firm: mini jobbers with monthly earnings near the notch (between $Z - 37.5$ and $Z + 12.5$, where Z is the mini threshold) in the year prior to each reform, who did not hold another job, and worked in the given mini job multiple years (2000-2002 for the first reform and 2010-2012 for the second).

3.2 Summary Statistics

Before turning to our empirical analysis, we describe some basic characteristics of both the general mini jobber population and our specific samples. Summary statistics not specific to our sample derive from a 2% random sample of the IEB and from the Socio Economic Panel (SOEP), which contains marital status. Table A.1 contains the main results described here.

In 2012, more than 7 million workers held a mini job and about 5 million worked exclusively in a mini job. The typical mini jobber works about 10 hours per week at a wage of €6-9 per hour, with hours above 15 hours a week being uncommon. Mini jobs are also disproportionately held by students and older workers, with about 40% of exclusive mini jobbers aged 26-55. Among those aged 26-55, the majority are female (75%), married (67%) and living in West Germany (85%).

Married, prime age, West German women in exclusive mini jobs are most commonly employed as cleaners, cashiers, secretaries, and nurses, average 44 years of age, and have around 11 years of education. The characteristics of mini jobbers we are able to identify as married in the administrative data using the Goldschmidt et al. (2017) procedure (approximately 20% of all West German females aged 26-55) are similar to those who report being married in the SOEP (Table A.1), alleviating potential concerns that the imputation method only identifies a very non-representative group of married women. In both the SOEP and the administrative data about 40% of married women in exclusive mini jobs have a husband with earnings between €33,000-53,000. Their characteristics are broadly similar to those of all married women in mini jobs. In the next Section, we plot earnings histograms for everyone in our *married women sample*, regardless of mini job status,

with average monthly earnings below €1,000. Table D.1 provides summary statistics for this sample, which, given the preponderance of mini jobbers, are also highly comparable to those of all prime age married women, and indeed all prime age women, in mini jobs.

The establishments in our *establishment sample*, with at least one worker at the pre-reform notch meeting the requirements described above, employ on average 20-30 mini jobbers and 75 non-mini jobbers (although there is considerable variation) and are predominantly concentrated in the manufacturing, trade, and healthcare sectors. On average, 70-75% of mini jobbers work in their establishment's modal mini job occupation (see Table D.2).

4 Earnings Responses to the Mini Job Reforms

We now examine how the overall earnings distribution of married women changes in response to each of the two mini job reforms. We document earnings increases following each reform for those workers originally constrained by the notch. At the same time, we show that a large fraction of these workers fail to adjust in the first several years following the reform.

4.1 Earnings Responses to each Reform

Figures 2—3 show histograms of average monthly earnings by year for the women in our *married women sample* (see Appendix D for details on how we construct earnings). We plot these distributions year-by-year, as the data generally do not allow us to detect within-year wage changes at the same job. One exception to this is for the 1 April 2003 reform, which required changes in the way mini jobs were reported, and consequently enables us to observe pre-reform and post-reform earnings separately.

Figure 2 panel (a) shows clear and extensive bunching at the monthly earnings threshold of €325 in 2002. There is no comparable bunching anywhere else in the earnings distribution (see Figure A.1 for the full distribution). This is precisely what we would expect to see under a standard labor supply model with heterogeneity in worker ability or preferences (Kleven and Waseem, 2013). In the presence of a tax notch, a mass of workers with earnings that would have exceeded the notch in a no-notch counter-factual will reduce their earnings and locate at the notch, resulting in bunching to the left of the notch and missing mass to the right.

Following the 1 April 2003 reform that moves the notch upward to €400, we would expect the excess density at €325 to dissipate as workers adjust their earnings upwards closer to their ideal earnings in a no-notch counter-factual. Some will have ideal earnings close to, but above, the original notch, while others will be constrained by the new notch. What happens in practice?

Three notable changes take place after the 1 April 2003 reform (see Figure 2 panel (c)). First, a sizable fraction of workers now have earnings around the new notch, €400, indicative of immediate adjustment. Second, a number of workers now earn between €325 and €400 on average. This would include, among others, workers who adjusted to €400 at some point after April but

before December 2003 as well as workers who adjusted instantly to an intermediate earnings level. Third, a clear, excess mass of workers continue to earn €325. This excess mass at the old notch point dissipates over time and is virtually gone by 2010. These three patterns are repeated following the 2013 reform and the notch shift to €450. Indeed, in Figure 3 panel (d), we see even more excess mass at the old notch in 2013, consistent with the smaller potential utility gain from adjusting (Chetty, 2012). Overall, Figures 2—3 show that cross-sectional earnings respond to the tax schedule and that there are meaningful lags in adjustment.

Moving beyond the cross-section, we also examine individual earnings responses over time. Theory predicts that the workers who make up the excess bunching mass at the original notch should be the ones who strictly prefer working more post-reform (Kleven and Waseem, 2013). Figure 4 confirms that individuals are indeed adjusting earnings upwards on the intensive margin following each reform and that this is most common among those with pre-reform earnings closest to the original notch. Panel (a) plots adjustment rates for women based on their pre-reform earnings levels, revealing that 48% of women with 2002 earnings around €325 adjust upwards after the reform (April-December 2003).[8] Adjustment rates follow the expected pattern: they are low for workers with earnings relatively far to the left of the threshold. A similarly clear pattern emerges for the 2013 reform, where 39% of women at €400 adjust following the reform.

Table 1 further confirms that most workers at the initial notch either remain stuck at the old notch or adjust to the new notch. Here, we restrict to all married women in a single mini job with earnings close to the pre-reform notch ($Z - 37.5 <$ earnings $< Z + 12.5$, where Z is the notch threshold) and follow their earnings over time. In the year following the 2003 (2013) reform, 43% (37%) of these women increase their earnings to the new threshold, while 34% (44%) stay near the old notch. 21% (16%) are still at the old notch in 2005 (2015). Relatively few women lower their earnings below the old notch, transition out of a mini job, or leave the data. Panel B restricts to the subset of married women who remain stuck at the old notch at the end of the first post-reform year. Adjustment rates for these women in the second post-reform year, 2004 (2014), decline to 24% (26%), but remain non-negligible, indicating that adjustment can take several years.

Notably, the vast majority of adjustment in Table 1 occurs within establishment. Specifically, over 94% (40.4/42.7) of those who increase their earnings to the new notch in the first post-reform year do so within their initial establishment. Even among the women in Panel B who did not adjust in the first post-reform year, over 85% (20.5/24.1) of those who adjust in the subsequent year do so within their initial establishment. Thus, the vast majority of earnings adjustment for our population occurs within establishment, either through increased hours, wages, or both.

While the administrative data typically do not include information on hours worked, hour information that had been collected for the purposes of occupational accident insurance was integrated in the data between 2011 and 2014 (see Dustmann et al. (2021)). This provides a rare

[8] We define adjustment as having average monthly earnings in the first post-reform year between $Z + 12.5$ and $\tilde{Z} + 12.5$, where Z is the old notch and $\tilde{Z}$ the new notch.

opportunity to examine whether earnings changes following the 2013 reform arise from hours or wage increases. We note up front that under-reporting of hours changes would bias against seeing hours adjustment. Figure 5 panel (a) compares changes in monthly hours worked for women originally at the notch in 2012 who either stayed at the old notch — stayers — or adjusted upwards to the new notch — adjusters (see figure notes for precise definitions). A large share of adjustment on earnings comes at least partially through increased hours – 40% of the adjusters experience positive hours increases with a modal increase of 4–5 hours a month. Figure 5 Panel (b) focuses on larger establishments, which among other differences may be more likely to report changes. Here, 52% of adjusters experience positive hours increases. Overall, at least 40—52% of the 1-year adjustment that occurs following the 2013 reform is accompanied by hours increases. While we do not have hours data around the 2003 reform, we expect, given the larger rightward shift in the notch (€75 as compared to €50), that adjustment on hours might have been even more prevalent.

4.2 Quantifying Delayed Adjustment

We have seen that earnings, and in many cases hours, increase following each tax schedule change. Yet, a considerable fraction of the workers we expect to adjust do not do so in the first few years. We now quantify the excess mass at the old notch in each year following each reform and discuss implications for elasticities.

Using the standard approach in the bunching literature, we fit a counter-factual earnings distribution to the earnings data in Figures 2—3 and measure the corresponding bunching mass (Chetty et al., 2011; Kleven and Waseem, 2013; Kleven, 2016).[9] Figure 6 plots the percentage of the initial bunching mass remaining at the old notch after each reform. 38% of the initial mass remains at the old notch in the first year following the reform (in April—December 2003). This falls to 29% by the end of the second year and decreases by about 4 percentage points a year thereafter. We see a similar pattern for the 2013 reform, with 52% of the initial mass stuck at the old notch in the first year after the reform, declining to 34% in the second year, and 18% in the third year post-reform.

These patterns are a general feature of the economy and are not particular to a handful of regions, industries, occupations, or demographic groups. Figures A.2—A.3 plot the earnings distributions for all women aged 26-55 in West Germany, as opposed to our sample of married women. Adjustment patterns are highly comparable. In general, the shape and evolution of earnings distributions over time is qualitatively similar if we look separately at women of different ages and education levels, as well as for women in each of the largest mini occupations, industries, and states. Figure A.4 demonstrates this by constructing analogues to Figure 6 for each of these different subsamples. While these reveal some heterogeneity in adjustment speeds, the presence of non-negligible delays in adjustment is a robust feature across settings.

[9]Specifically we fit a fifth degree polynomial to the data in Figures 2—3 excluding earnings bins to the left of the notch and iterate over the number of excluded bins to the right of the cutoff until the bunching mass is closest to the missing mass to the right of the cutoff. Counter-factual fits and their corresponding bunching masses can be seen in Figures A.5—A.6.

The observed adjustment delays have implications for the intensive margin elasticity of earnings with respect to the net of tax rate. Consider a scenario in which we compared the change in earnings of mini jobbers at the notch to some plausible control group of non-mini jobbers in a standard difference-in-difference design. Sluggish earnings responses suggest that a 1-year elasticity estimate would be quite a bit smaller than, say, a 5-year elasticity. Self-contained Appendix B uses bunching techniques to explicitly quantify these differences in our context, finding a long-term, intensive margin earnings elasticity of 0.33. A 1-year difference-in-difference inspired estimate would have yielded an elasticity half this size. Additionally, we use Gelber et al. (2020)'s method to simultaneously estimate an elasticity and an (omnibus) adjustment cost. Depending on specification, we estimate long-term elasticities ranging from 0.23-0.33 and adjustment costs of €415-483 in the first post-reform year that dissipate over time.

5 The Role of Firm Labor Demand in Adjustment

While a host of individual-level frictions could account for some adjustment delays, we propose that firms play an important role in mediating the speed with which workers adjust. Specifically, we argue that heterogeneity in labor demand across firms will lead to heterogeneity in workers' earnings responses.[10] To fix ideas, consider a firm employing multiple at-the-threshold mini job cleaners who clean offices that has already optimized its hours. Without exits from the firm or additional offices to clean, immediately increasing the hours of all pre-existing workers is infeasible. As cleaners leave the firm, it becomes easier to accommodate remaining workers' hours requests (and indeed doing so may be preferable to meeting labor demand through the more costly option of hiring new workers). If, however, the number of offices being serviced is rapidly increasing, it is easier to offer increased hours to all who want them and reduce hiring. Hence, we expect adjustment to rightward notch shifts to be more rapid in growing firms.[11]

In order to test this hypothesis, one would want to know how many additional hours each establishment would have desired absent the reform and examine the association between this and adjustment rates. Of course, this is infeasible. In the rest of this section, we use three reduced form strategies to link workers' post-reform adjustment propensity to their establishments' labor demand. Specifically, we show that adjustment is most likely in i) establishments that hire new mini jobbers post reform, ii) establishments expanding their non-mini job workforce around the reform, and iii) establishments with high predicted mini job hires using pre-reform characteristics. We conclude with a discussion of the quantitative importance of labor demand for adjustment.

[10] A prior version of this paper provided one way to formalize this argument, applying Nickell (1986)'s model of firm labor demand with hiring and firing costs to the mini job reforms (Gudgeon and Trenkle, 2020), though this result may follow from other models with firm level frictions.

[11] This channel should be most relevant if firms do not fire workers to accommodate adjustment of others and if workers are unlikely to incur the costs of finding of a new job simply to increase earnings by a small amount for several years (especially compared to household income).

5.1 Individual Adjustment and Establishment Labor Demand: Three Tests

We present each of our three empirical strategies and corresponding findings in turn, after which we jointly address key robustness concerns like worker sorting.

Throughout this section, we test for expected associations between adjustment and establishment-level labor demand proxies in our *establishment sample* (Section 3). Establishment-level variables are constructed using everyone employed at the establishment in the given year. Within the establishment, we study the adjustment behavior of a specific group of mini jobbers presumably constrained by the notch: mini jobbers with monthly earnings close to the threshold (between $Z - 37.5$ and $Z + 12.5$, where Z is the mini threshold) in the year prior to the reform, whose only job in this year was this mini job, and who also worked in this mini job in 2000 and 2001 for the first reform (2010 and 2011 for the second reform). Throughout, we define adjustment using a binary indicator variable for whether or not the individual increased her monthly mini job earnings within the original establishment to between $Z + 12.5$ and $\tilde{Z} + 12.5$ in the given post-reform period (where $\tilde{Z}$ is the new threshold).[12] Appendix D.2 contains additional details on this sample, including key definitions and summary statistics.

Adjustment Incidence and Any New Mini Hires. First, we test for an expected positive association between the adjustment rates of pre-existing workers at the notch and an indicator for whether the establishment hires any *new* mini jobbers post-reform. This test is based on the assumption that giving existing workers additional work hours should be less costly than hiring new workers. From the establishment's perspective, it should only hire a new worker after allowing existing workers who desire to increase their hours to do so. Thus, only establishments that allowed adjustment and have remaining demand for hours should hire a new mini jobber. In contrast, establishments with little demand for additional mini job hours might not allow (or only allow partial) adjustment and should not hire. There are, of course, reasons that an establishment might nevertheless hire a worker, say to fill a particular occupation. Moreover, some establishments may allow adjustment and not require additional hires. These would bias us towards not finding an association. Overall, though, we expect a positive association between adjustment rates of pre-existing workers at the notch and an indicator for any new mini hires post-reform.

For mini jobbers at the pre-reform notch (i) we regress

$$\text{Adjusted (in T)}_i = \alpha + \sum_{t=-2}^{2} \beta_{T+t} \text{Any Mini Hire (in T+t)}_i + \gamma \mathbf{X}_i + \epsilon_i \tag{1}$$

Year T corresponds to the first post-reform period (April-December 2003 or, separately, 2013).

The key coefficient of interest is β_T. We expect hiring in the first post-reform period to be

[12] We explore sensitivity to dropping the tenure restriction in Table A.11 and to alternative definitions of adjustment in Table A.12.

associated with adjustment in that period: $\beta_T > 0$. In order to ensure that establishments with a post-reform mini job hire are not simply consistently different along some unmodeled dimension, we opt to include leads and lags of Any Mini Hire in our baseline. The inclusion of the full sequence of hiring patterns helps us differentiate establishments that generally hire from ones that hire at the right time by looking for a jump in coefficients between the years before the reform and the year of the reform.[13] We also present results without these leads and lags in Table A.2. Since some establishments might accommodate adjustment in the first post-reform year without hiring and then resume hiring next year, Any Mini Hire in $T+1$ may also contain a meaningful signal about adjustment in year T and hence $\beta_{T+1} > 0$. While the relationship between adjustment and pre-reform hires is more ambiguous, we expect $\beta_T > \beta_{T-1}$, as having a post-reform hire should be more indicative of having allowed adjustment than pre-reform hires.

One advantage of our data and our large number of establishments is that we can control for many differences across establishments, allowing us to emphasize the role of idiosyncratic differences in labor demand for establishments of similar size in the same industry and location. As such, $\mathbf{X}_i$ is a vector of establishment-level controls measured in the year prior to the reform (2002 or 2012). Our baseline controls always include dummies for 22 industries, dummies for 88 occupations as measured by the modal mini job occupation at the establishment, dummies for the 10 West German states, and 34 dummies for establishment mini and, separately, non-mini employment size. Additionally, in some specifications, we also include a host of individual level controls — occupation dummies, demographic information, tenure, earnings histories, and indicators for holding multiple jobs. When mentioned, we further include additional establishment controls — 5 digit industry dummies, information on the establishment's work-force structure, establishment age, establishment-level worker demographics, information on the earnings distribution within establishment, and local municipality (*gemeinde*) unemployment level and its growth rate. For the list of controls see Appendix D.2. Throughout, standard errors are clustered at the establishment level.

Column (1) of Table 2 Panel A estimates Equation 1 for the 2003 reform. The coefficient on any hires in the first post-reform year is indeed positive and significantly larger than coefficients in any pre-reform year. Adjustment rates of pre-existing workers at the notch are 5.5 percentage points higher — or 15% larger relative to the mean of 36% — in establishments that hire a new worker post-reform. Long-term workers at the notch are also significantly more likely to adjust to the new notch in 2003 if the establishment has a hire in 2004, although this coefficient (0.022) is smaller. This is consistent with the presence of establishments that adjust in the first year without hiring and resume hiring in the next year.

Column (4) estimates Equation 1 for the 2013 reform. We see the same qualitative patterns as in the 2003 reform, but smaller effect sizes. Adjustment rates of pre-existing workers at the notch

[13] We restrict to establishments that have at least one mini jobber in each of the periods that we use the hiring variable. This ensures that a 0 is truly no hiring, as opposed to simply the establishment no longer existing.

are 2.8 percentage points higher — or 8.4% larger relative to the mean of 33% — in establishments that hire a new mini jobber post-reform.

Columns (2)–(3) and (5)–(6) add the additional pre-reform individual and establishment level controls. Results move relatively little with the progressive inclusion of controls, but to the extent that they do, they paint an even clearer picture. Panels (A) and (B) in Figure 7 plot the regression coefficients from column (3) and (6) respectively.

Table 2 Panel A explored adjustment in the first year following the reform. We are also interested in the adjustment behavior of those that do not adjust as quickly, as establishment labor demand should continue to be relevant in subsequent years. Table 2 Panel B column (1) replicates Panel A column (1) for presentation purposes. Panel B column (2) restricts to workers in establishments where *none* of the tenured mini jobbers we study adjusted in 2003 and redefines the independent variable to be adjustment in 2004 (as opposed to 2003). Column (3) further restricts to workers in establishments where no one adjusted in 2003 and 2004 and redefines the independent variable to be adjustment in 2005. While noisier and smaller in magnitude, we nevertheless see the largest effects appear in the right period — adjustment in 2004 is most strongly associated with any new hires in 2004, and analogously for 2005. Columns (5) and (6) do the same for the 2013 reform, where adjustment in 2014 is associated with having hires in 2014, and adjustment in 2015 is associated with any hires in 2015. This suggests that establishment labor demand conditions continue to affect adjustment for several years following each reform.

Adjustment Incidence and Non-Mini Job Growth. In our second strategy, we exploit information on workers far away from the notch — non-mini jobbers. While preceding results are consistent with establishment labor demand mediating earnings responses, it is possible that the observed association between hiring behavior and adjustment arises for reasons unrelated to labor demand that are difficult to control for. For example, it could be that seeing a new mini hire at the new notch transmits information to pre-existing workers about the reform. As such, it is helpful to test for additional equilibrium associations that do not exploit information on mini hires. One such test involves non-mini jobbers: if establishments typically scale by increasing the number of both mini and non-mini jobbers, then changes in the total hours (or earnings) of non-mini jobbers should be a useful proxy for latent mini job labor demand.[14] This motivates our second test for a positive association between the change in total non-mini job earnings and adjustment rates of pre-existing workers at the notch.

For each establishment in our *establishment sample* we construct total non-mini job earnings. High earnings are capped in the data and the cap varies across years. For consistency, we cap

[14]Consistent with the presence of positive scale effects, an establishment level fixed effect regression of the logarithm of mini size on the logarithm of non mini size in the same year yields positive and significant elasticities between 0.1 and 0.2, both for the entire period 1999-2015 and for samples that drop post-reform years. Galassi (2021)'s findings are also consistent with the presence of positive scale effects. Moreover, to the extent that these scale effects are weak or absent, our results will simply be biased towards 0. Any bias from establishments substituting mini jobbers for non-mini jobbers post-reform also works against us.

earnings at the minimum across years. We then take the year-on-year difference in the natural logarithm of total non-mini earnings at the establishment. This measure is preferred because it captures both changes in the number of workers as well as increases in earnings (and hence potentially hours), but we also show how adjustment varies with the change in non-mini employment in Table A.13. In order for these measures to be meaningful, it is necessary that establishments not be composed entirely of mini jobbers. For precision, we focus on establishments with at least 10 non-mini jobbers in 2002, as these establishments are likely to have a reasonable number of non-mini jobbers across the years of study, though we also present results using alternate restrictions in Table A.5. We then estimate the analog of Equation 1 replacing Any Mini Hire with the change in the natural logarithm of non-mini earnings relative to the prior year, $\Delta Ln(\text{Total Non-Mini Earnings})$:

$$\text{Adjusted (in T)}_i = \alpha + \sum_{t=-2}^{2} \beta_{T+t} \Delta Ln(\text{Total Non-Mini Earnings (in T+t)})_i + \gamma \mathbf{X}_i + \epsilon_i \qquad (2)$$

Table 3 presents estimates of Equation 2. Column (1) shows that total non-mini job earnings growth around the reform period (changes between 2003 and 2002) is significantly associated with adjustment. A one standard deviation increase (0.22) in non-mini earnings growth is associated with an 8% increase in adjustment relative to the mean. Unlike with the any mini hires results, pre-reform non-mini growth is also associated with adjustment. The inclusion of controls in columns (3) and (6) reduces the association with pre-reform growth, suggesting that non-mini growth may be correlated with some degree of worker sorting and/or other establishment characteristics relevant to adjustment. Nevertheless, and as in Table 2, the association between adjustment and non-mini job earnings growth is larger in the reform year than in prior years and this is robust to the inclusion of our battery of individual and establishment controls. Columns (4)–(6) portray much the same picture for the 2013 reform. Here a one standard deviation increase (0.18) is associated with a 7% increase in adjustment relative to the mean. The coefficients from columns (3) and (6) are plotted in Figure 7 Panels (C) and (D) respectively.

Table 3 Panel B explores adjustment in later years. Column (2) restricts to workers in establishments where no one adjusted in 2003 and redefines the independent variable to be adjustment in 2004. Column (3) does the same for workers in establishments were no one adjusted in 2003 and 2004, exploring adjustment in 2005. Columns (5) and (6) work analogously for the 2013 reform. These results are noisy, but generally supportive of a positive (albeit not always differential from prior years) association between contemporaneous non-mini job growth and adjustment. Overall, Table 3 provides further evidence that establishment labor demand, this time proxied for by increases in the total amount paid to workers away from the notch, mediates the speed of earnings responses to the reform.

Adjustment Incidence and Predicted Mini Hires. Our third and final strategy to capture latent

mini job labor demand takes a different approach. Instead of leveraging contemporaneous indicators of labor demand, we predict mini job hiring using establishments' historical hiring and growth patterns. We train a predictive model on a pre-reform year and then use that model to predict mini hiring in the reform year. This enables us to explore the association between an establishment's predicted mini hiring and individual-level adjustment. We expect adjustment to be low for small values of predicted hiring and to increase with predicted hires.

For the 2003 reform, we train a model to predict the fraction of mini hires in 2002 (expressed relative to the number of mini jobbers in 2001 and capped at 1 to reduce the impact of outliers). Since data on mini jobs are available beginning 1999, we use 3 lags of establishment-level variables and 2 lags of establishment-level growth measures as predictors. We employ two predictive models. First, we use a parsimonious OLS model to predict hiring in 2002. We feed this model a limited set of variables so as to retain an understanding over how our predictions are generated. Specifically, we feed the model establishment size dummies in 1999 and 2 lags of establishment hiring rates (the fraction of new employees) and growth rates (change in the number of employees as a fraction of last years' employees), each for mini and non-mini jobbers separately (Appendix D.2 has additional details).

Second, we use a cross-validated LASSO approach on a larger set of predictors. In addition to the predictors used in the OLS, we also allow the LASSO to select from variables relating to the wage and age structure of the establishment as well as detailed industry and region dummies.[15] We use 5-fold cross validation to select the penalty parameter that minimizes the mean squared prediction error (MSPE). Relative to the OLS approach, this has the potential to improve our predictions but also means that predictions are derived from a larger, less easily characterized set of predictors.

For the 2013 reform we predict the fraction of mini hires in 2012. For consistency, we use up to 3 lags of variables (and 2 lags of growth-related variables), but our conclusions are comparable if we use a longer time series or if we train our model on 2011 mini hires instead of 2012. We perform this exercise for all establishments in our *establishment sample* with at least one mini and one non-mini jobber between 1999-2002 (2009-2012) (so our growth and hiring predictors are defined).

Our model predicts the fraction of mini hires in 2002 (2012) reasonably well. The bottom panel of Table 4 shows that our model has an adjusted R^2 of around 0.25 and improves the root-mean-squared-error (RMSE) by around 15% relative to using only a constant as a predictor. The LASSO approach modestly improves predictions. While it selects several new variables for inclusion, it also selects almost all of the hiring and growth lags used in the OLS approach.

Next, we construct predicted mini hiring in 2003 and 2013. That is, we apply the coeffi-

[15]Specifically, the LASSO selects from the same set of predictors as the OLS plus two-year growth and hiring rates (as opposed to one-year), three lags of the wage-structure of the establishment (the 25th, the 50th and the 75th percentile, the 50th percentile squared and the share of the 75th to 25th percentile), and three lags of the share of workers in 8 different age-groups in the establishment. Furthermore, the model can include any relevant 5-digit industry dummies, county dummies (400 regions), or interactions between 3-digit industry and county dummies.

cients from our predictive model to the relevant, updated predictor lags. This yields our best prediction for the fraction of mini hires the establishment would have had absent the reform. We then test for an association between predicted mini job hires in the first post-reform year — Frac. $\widehat{\text{Mini Hires}}$ (in T)$_i$ — and our usual measure of individual adjustment:

$$\text{Adjusted (in T)}_i = \alpha + \beta \text{ Frac. } \widehat{\text{Mini Hires}} \text{ (in T)}_i + \gamma \mathbf{X}_i + \epsilon_i \tag{3}$$

We cluster bootstrap the entire procedure at the establishment level in order to correct standard errors for the generated regressor.

Table 4 examines the association between predicted mini hires and individual-level adjustment. Panel A uses predicted hires from the OLS approach while Panel B uses predicted hires from the LASSO approach. We include the same sets of controls as in Table 2 and 3. Panel A shows that an increase in predicted hires by 0.1 (around a 1 sd increase) is associated with a 4.1-4.8 percentage point increase in adjustment (11-14% of the mean) in the 2003 reform and a 1.7-2.6 percentage point (5-8% of the mean) increase in the 2013 reform. Panel B yields a similar picture, albeit with slightly smaller coefficients, when we predict hiring using a larger set of potential variables via LASSO. Thus, adjustment propensity is associated not only with contemporaneous indicators of labor demand, but it can also be predicted using historical hiring and growth patterns.

Finally, in Table 4 Panel C we re-run Equation 3 including the *realized* value of new mini hires, as opposed to its prediction. While still positive and significant, coefficients are attenuated relative to predicted hires. This is expected and consistent with establishments adjusting their workers and reducing hiring.

5.2 Robustness and Discussion

Taken together, Tables 2—4 are consistent with establishment labor demand affecting adjustment speeds. Nevertheless, it is helpful to address several competing hypotheses head on. First, we address the concern that individuals with higher adjustment propensity might be sorting to establishments that grow (differentially) in the future. Our battery of individual level controls rules out concerns about individuals sorting to establishments based on observable characteristics (compare columns (2) and (5) to (1) and (4) in each of Table 2—4). Moreover, Oster (2017)'s test for selection on unobservables suggests an implausibly large degree of sorting on unobservables required to overturn results.[16] This is consistent with the relatively large gain in adjusted R^2 from columns (1) to (2) and (4) to (5) combined with the stability of the coefficient of interest after the inclusion of individual controls. Additionally, Table A.3, leverages the fact that we have two reforms to

[16]Specifically, we calculate Oster (2017)'s δ — the degree of selection on unobservables relative to selection on observables required to nullify the coefficient on our labor demand proxy in the first post-reform year. We use her suggested R_{max} of 1.3 times the observed R^2 and treat the baseline controls as well as any hires and non-mini earnings growth in other years as unrelated controls. We obtain large δ's in Table 2—4, suggesting we need an order of magnitude more selection on unobservables than observables to nullify our results.

include individual fixed effects. In columns (2) and (3), we take all individuals in each sample that experience both reforms. We first show that the results in Tables 2—4 broadly hold up in these sub-samples (column (2)). Next, in column (3) we add individual fixed effects. The effect of labor demand on adjustment speed broadly persists after including these fixed effects.

Second, we consider whether establishment level omitted variables other than labor demand could account for the observed results. For example, two establishments that would benefit equally from adjusting their workers immediately could have different collective labor agreements, allowing one to both hire and adjust immediately, while forcing the other into negotiations before adjusting or hiring. We first note that the battery of establishment level controls in columns (3) and (6) of Tables 2—4 include 5 digit industry codes which should capture much of the variation in collective labor agreements. These do not move our coefficients of interest. Oster (2017)'s test continues to suggest that we would need an unlikely degree of selection unobservables to nullify results (compare columns (3) and (6) to (1) and (2), respectively). Additionally, in columns (4) and (5) of Table A.3 we focus our attention on the subset of establishments that we observe in both reforms, enabling us to include establishment fixed effects. Column (5) shows that the effect of each of our labor demand measures on adjustment persists even when we only leverage within establishment variation across the reforms. It remains possible that *time-varying* omitted variables could influence both our establishment labor demand proxies and individual adjustment. However, it is typically not trivial to explain all three of our main findings away with a single alternative explanation.

One potential concern involves differential knowledge about the reform (Chetty et al., 2013). It is possible that establishments whose managers are more aware of the reform make structural changes that they would not otherwise have made following the reform (e.g. hire more mini jobbers) while better transmitting information to their workers. To the extent that this is not captured by establishment controls (including establishment fixed effects), this could confound one or more of our empirical tests. Several points help assuage this concern: establishments with pre-existing mini jobbers were informed of the reform (Section 2), the mini job threshold is well known and salient, our own survey supports the idea that information was not an impediment to adjustment (Appendix C), and we continue to find associations between labor demand and adjustment multiple years post-reform when information has had more time to disseminate. Additionally, Tables A.4 – A.6 show that the association between labor demand and adjustment holds for all three labor demand proxies across a range of establishment sizes (including when restricting to large mini job employers). To the extent that managers of larger establishments are more likely to know about the reforms, the variation in adjustment rates in response to heterogeneous labor demand *within the subset of large establishments* helps speak against our results being driven by asymmetric managerial knowledge of the reform. Finally, to the extent that firms who want their workers to work more hours are more likely to disseminate information about the reform and encourage adjustment, this can be viewed as a consequence of our labor demand channel.

Why do we see differences across the two reforms? The associations between labor demand proxies and adjustment are typically a bit weaker for the 2013 reform. While we cannot definitively explain these differences, two possibilities are worth mentioning. The 2013 reform allowed for a smaller earnings increase and was motivated by keeping up with inflation. To the extent that adjustment occurred more via wage than hours increases, our labor demand channel could be less directly relevant for adjustment than, say, worker bargaining power. Individual-level frictions could also plausibly have mattered more. Second, the 2013 reform occurred at a time of lower and declining unemployment (consistently, the means for our labor demand proxies are higher in 2013 than 2003). As such, lackluster labor demand may have been less of a constraint on adjustment.

Last, we mention several additional results that bolster our claim that establishments with insufficient labor demand delay adjustment. i) To the extent that establishments, for fairness considerations or other reasons, refuse to accommodate any hours increases before they are able to do so for most, we would expect our results to extend to the extensive margin of any adjustment within the establishment. Figure A.7 shows that it is indeed (overly) common to observe no one adjusting within an establishment. For example, panels (b) and (d) show that around 20% of establishments with exactly 10 mini jobbers at the notch in the pre-reform year have zero workers adjusting to the reform. Furthermore, Tables A.7-A.9 estimate establishment-level analogs to the regressions presented in Tables 2-4, replacing the dependent variable with a binary indicator for *any* adjustment at the establishment.[17] Our labor demand proxies are positively associated with this extensive margin measure of adjustment, consistent with establishments playing an important role in the adjustment process and potentially refusing to accommodate any adjustment until they are able to offer this option to all. ii) While the data generally limit us to studying earnings adjustment, for the 2013 reform we are able to examine whether our labor demand proxies are associated with adjustment on hours. In Table A.10 we show that our labor demand proxies are associated with adjustment on hours — defined as an indicator that is now equal to one only if adjustment (as previously defined) is also accompanied by an increase in at least 0, 1, and 2 monthly hours respectively in columns (1)-(3) — and this association is particularly apparent in larger establishments (columns (4)-(6)). iii) Adjustment is faster in firms with larger increases in total employment or non-mini employment (Table A.13). iv) Finally, we ask whether adjustment is faster in growing industries. Here we exploit leave-out county (*kreis*) 5-digit industry-level variation in employment growth rates. While this variation does not allow us to include controls at the same level of granularity as our baseline results (for example, we can no longer include 5-digit industry fixed effects), it lends itself to a natural economic and policy interpretation. Using the same *establishment sample*, in Table A.14 we find a significant positive association between leave-out industry growth and adjustment for the 2003 reform and an insignificant (though not statistically different) positive association for the 2013 reform. Effect sizes are somewhat smaller than in our

[17] This is defined using the sample of tenured mini jobbers at the notch used above, but results are comparable when using all mini jobbers at the notch. We weight regressions by the number of tenured mini jobbers off which the 'any adjustment' variable is based.

establishment-level results, consistent with industry growth being a weaker proxy for firm-level labor demand than our more direct measures.

5.3 How Quantitatively Important is this Labor Demand Channel?

We now tie Section 4 and 5 together by revisiting how the speed with which the excess mass at the old notch dissipates varies across establishments with differential labor demand. The exercise below provides suggestive evidence of the quantitative importance of the labor demand channel for adjustment.[18]

Revisiting Figure 6, we examine the speed with which the excess mass at the old notch dissipates across establishments with idiosyncratically different labor demand. Specifically, we take all married women at establishments in Table 2 in the year prior to the reform with 3 years of tenure and explore how their earnings distribution evolves following the reform based on whether or not that establishment hires a new mini jobber. To mirror Table 2 as closely as possible and to control for observable differences between individuals at establishments with and without a post-reform mini hire, we use one-to-one propensity score matching to match each woman at an establishment with no new post-reform mini hires to a similar woman at an establishment with a post-reform mini hire. We obtain the propensity scores by running probit regressions for having a post-reform mini hire on our baseline set of control variables. We do so separately within 14 different pre-reform earnings brackets in order to match the pre-reform earnings distribution as closely as possible across the two groups. Figure A.8 shows the propensity score matched distributions before and after each reform. Similar pre-reform distributions across individuals in hiring and non-hiring establishments give way to visually dissimilar post-reform distributions.

Figure 8 plots the percentage of the initial bunching mass remaining at the old notch after each reform for women at establishments with and without a post-reform mini hire. As expected based on Table 2, women at establishments with a post-reform mini hire adjust significantly faster. Only 33.2% remain at the old notch after the reform as compared to 48.6% of women at establishments without a new mini hire – a 15 percentage point difference. With regards to the 2013 reform, women at a hiring establishment in 2013 have 10 percentage points lower excess mass at the old notch as compared to women at establishments without a new mini hire. These 10-15 percentage point differences are meaningful, comparable to the differences between the fastest and slowest adjusting occupations or industries in Figure A.4. For the 2003 reform, attributing all differences between establishments with and without mini job hires to the role of firm labor demand in adjustment suggests that up to one third of the excess mass (15/48.6) could be due to differences in

[18] For several reasons, a definitive quantification of how important this labor demand channel is for adjustment is challenging. First there is considerable reason to think the labor demand proxies used in Tables 2—4 will underestimate the effect of labor demand on individual adjustment. For example, the coefficient on any new mini hires will be downward biased because some establishments will have sufficient labor demand to allow their workers to adjust but not enough to hire a new worker. Meanwhile non-mini job growth is an imperfect proxy for mini job labor demand and we predict mini hires imperfectly, introducing measurement error. Second, precise quantification requires additional structural modeling and associated assumptions that fall beyond the scope of this paper.

firm labor demand conditions.

Overall, these 10-15 percentage points reductions in excess mass suggests that labor demand plays a meaningful role in mediating the speed with which workers adjust their earnings. Of course, the overall role establishments play in the adjustment process may be and likely is even larger.[19] Nevertheless, establishment labor demand, both on its own and as a share of the overall establishment contribution, appears to play meaningful role in mediating the speed with which workers adjust their earnings.

6 Conclusion

This paper shows that short-term earnings responses to tax reforms along the intensive margin can be significantly attenuated. While some workers adjust instantly, increasing earnings and hours, others take several years to do so. We document these delayed intensive-margin earnings responses in a setting where the policy change is salient and where the group under study is relatively homogeneous and has room to increase hours. Our proposed, firm-level labor demand mechanism helps explain why adjustment is delayed even in this setting. In particular, we show that heterogeneity in firm-level labor demand can generate heterogeneity in adjustment speeds and provide supporting evidence of the quantitative importance of this channel.

These results emphasize the importance of accounting for confounding between pure labor supply preferences and frictions when estimating elasticities. Moreover, the documented relationship between adjustment speeds and firm labor demand conditions implies that we should be cautious interpreting short-run elasticities as capturing a purely supply-side response. Expanding estimation windows to estimate elasticities over a longer time horizon or using a bounding procedure like that in Chetty (2012) to account for this confounding will help recover estimates closer to the long-term 'structural' elasticity. Indeed, as more studies track the speed of earnings responses following tax changes across contexts, we will continue to gain insight into what typical adjustment processes looks like. In the same way that policy practitioners settle on a reasonable elasticity to use, we could eventually settle on an expected response path over time (conditional on potential utility gains from the tax change and context specific factors like labor demand). This would yield more fine-tuned revenue projections for tax changes and better calibrate our expectations of short-term earnings responses.

We note that elasticities are most attenuated in the first few years and that much of the action in these first periods occurs within workers' initial firms, as opposed to through job-to-job transitions. This points towards the theoretical relevance of thinking about within-firm adjust-

[19] For example Figure A.7 suggests that 20% of even relatively large firms have no adjustment whatsoever. For the sample in Table 2, 28% of individuals are at establishments with no adjustment in 2003 and 35% of individuals are at establishments with no adjustment in 2013. An establishment fixed effect accounts for over 45% of the variation in one-year adjustment rates in the same sample while an establishment random effect accounts for 28% of the variance and is not sensitive to individual controls.

ment mechanics and the sources of heterogeneity across firms. Our evidence suggests that other, establishment-level adjustment channels beyond labor demand matter; these would be worth exploring. Our adjustment mechanism also hints at asymmetric responses, as establishments may have more difficulty preventing workers from reducing hours than increasing them. This suggests that responses to policy changes encouraging labor might have different short-run effects than policies discouraging labor.

While a full assessment of external validity of our mechanism is beyond the scope of this paper, our findings have implications that may extend to other settings. For example, consider a policy that increases the phase-out location for the Earned Income Tax Credit (EITC). Our work would predict that adjustment to such a policy would be delayed, and moreover, such a policy change would produce more rapid responses if firms that typically employ EITC-eligible workers are growing. More generally, our work suggests that policies aimed at increasing labor might produce more rapid responses if enacted during expansions as opposed to recessions.

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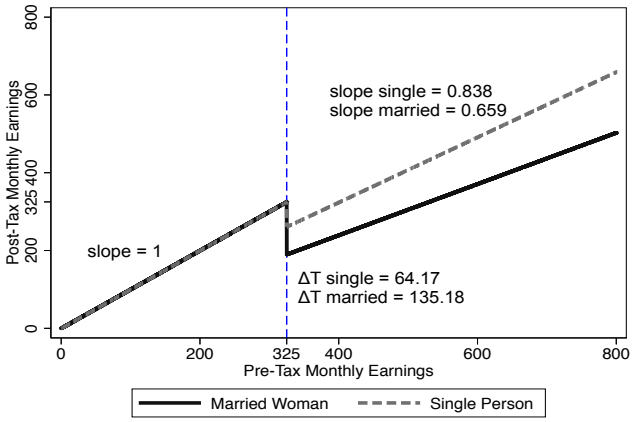
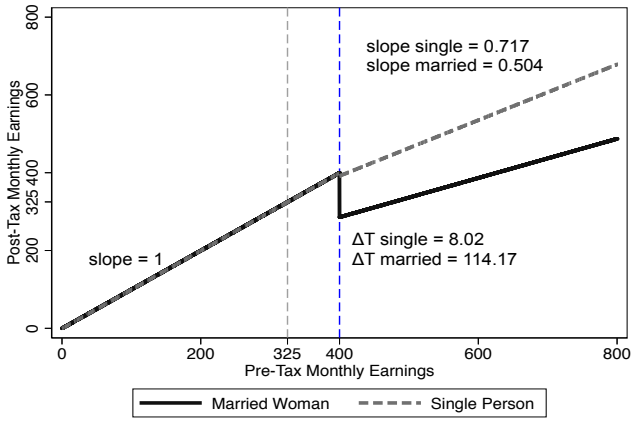
Figures

Figure 1: Tax Schedules

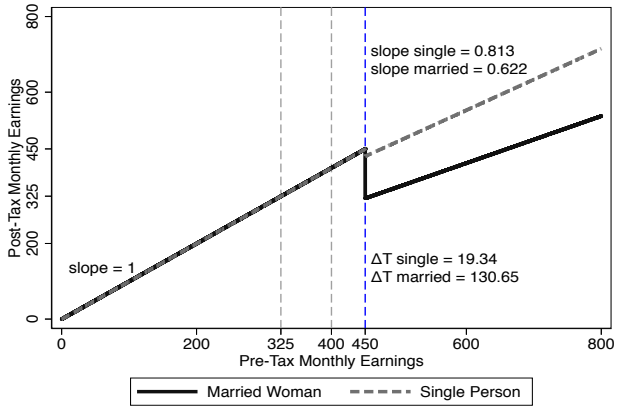
(a) 1999-Mar 2003



(b) Apr 2003 - 2012



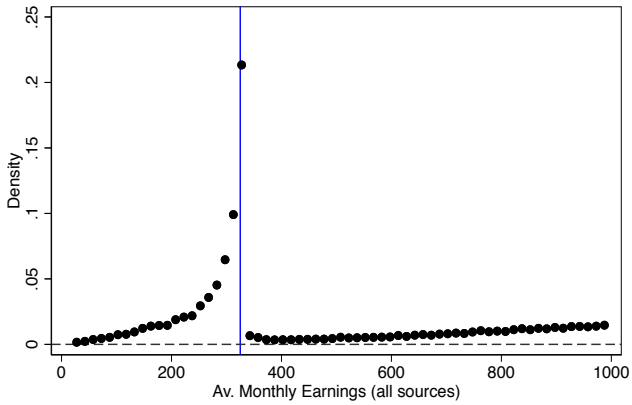
(c) Jan 2013 - Present



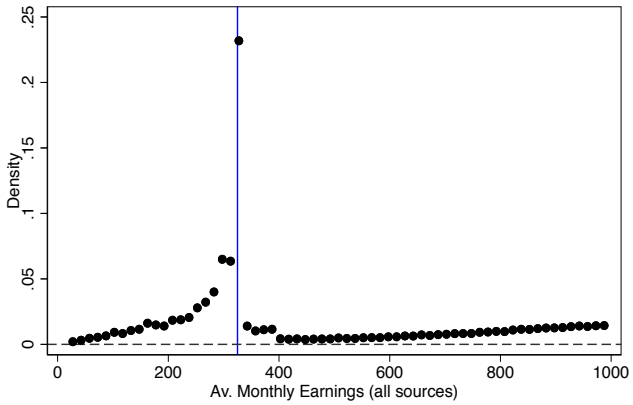
Notes: This figure displays budget sets in 2002, 2003, and 2013 for single persons and for married women, assuming husband earnings of €41,000. The prevailing mini job threshold is drawn as a dashed blue vertical line, with defunct thresholds shown as gray vertical lines. Single persons are assumed to have no other sources of income and hence earn too little to be subject to income taxes, so that all variation in the budget set is driven by changes in social security contributions. Earnings below the mini job threshold are income tax exempt, while earnings above the threshold enter into household income. This is relevant for married women under joint taxation, since earnings above the threshold get taxed at the household's marginal tax rate. For details on budget set construction see Appendix B.3.

Figure 2: Earnings Distributions: Married Women 2002-2006

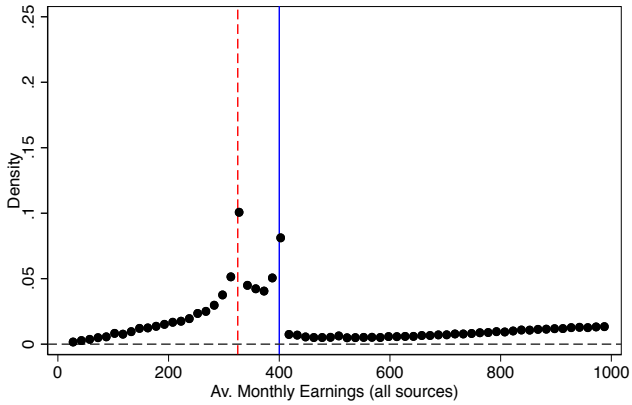
(a) Earnings Distribution 2002



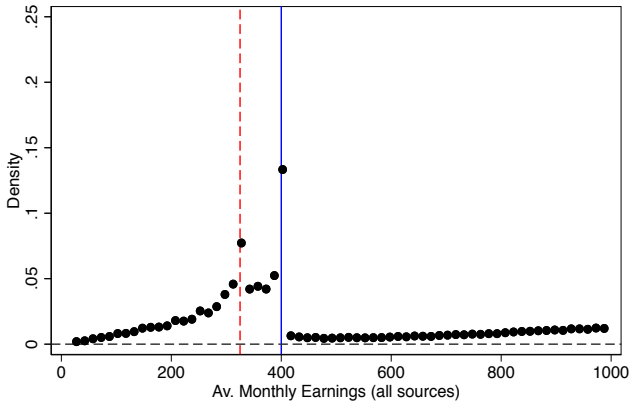
(b) Earnings Distribution 2003 (Jan-Mar)



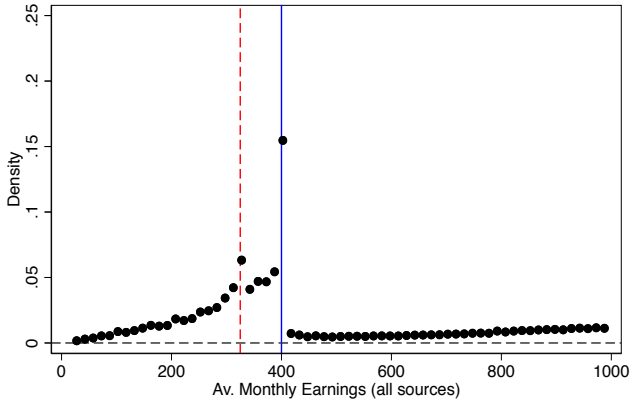
(c) Earnings Distribution 2003 (Apr-Dec)



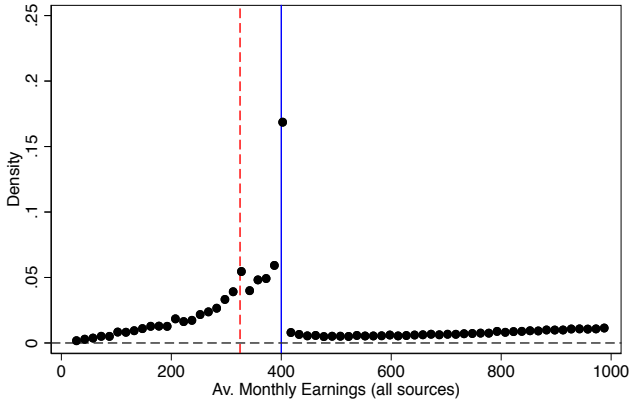
(d) Earnings Distribution 2004



(e) Earnings Distribution 2005

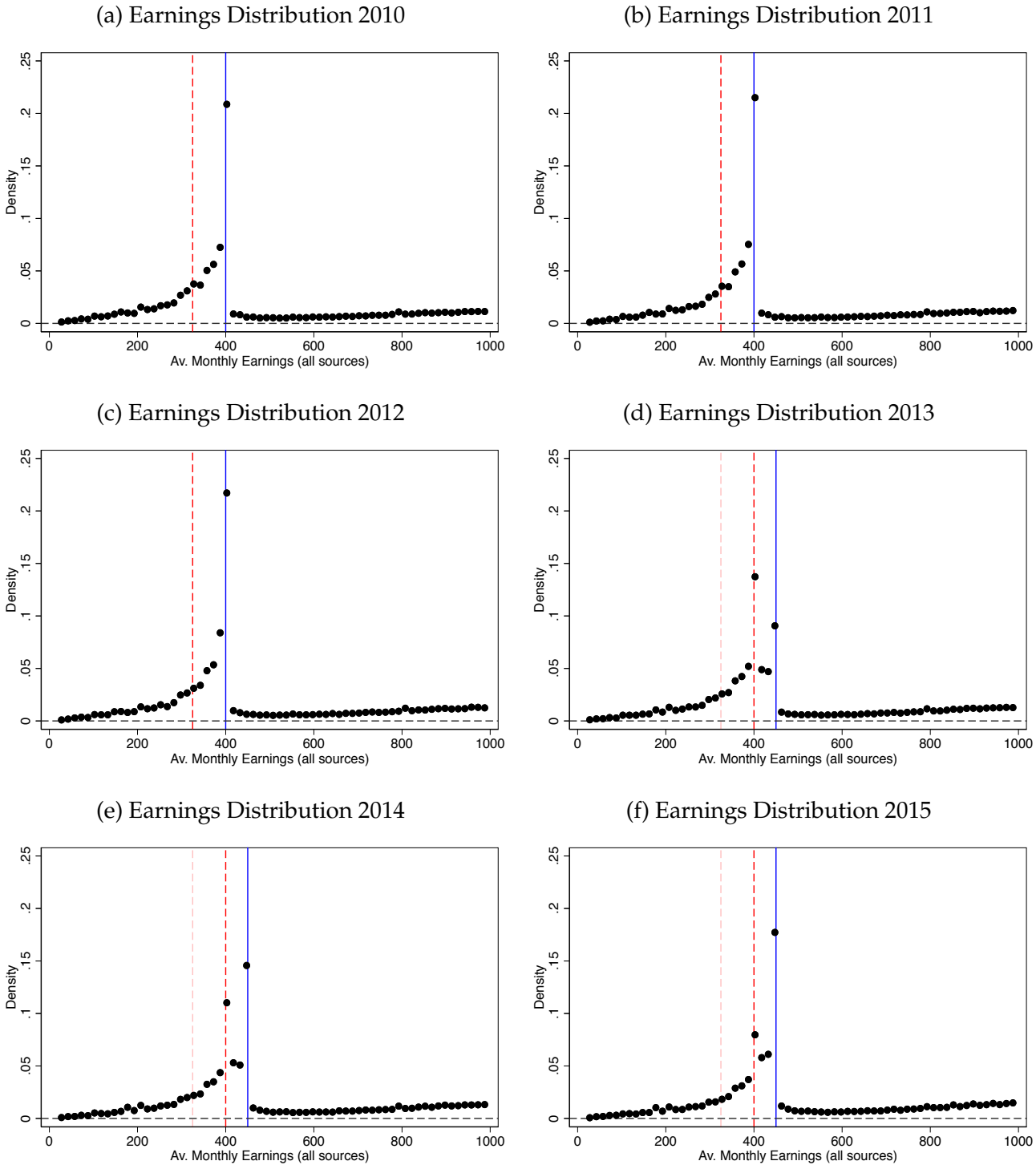


(f) Earnings Distribution 2006



Notes: This figure plots the distribution of duration-weighted, average monthly earnings (all employment sources) for our *married women sample* (Section 3) between 2002 and 2006. Observations are weighted by the fraction of days an individual is in dependent employment, where individuals working year-round get a weight of one. Bins are €15 wide beginning with a bin centered at €27.5. The prevailing mini job threshold is indicated by the blue solid line, while old thresholds are indicated by the red, dashed line. The threshold was at €325 prior to April 1st 2003, at €400 prior to January 1st 2013, and at €450 thereafter.

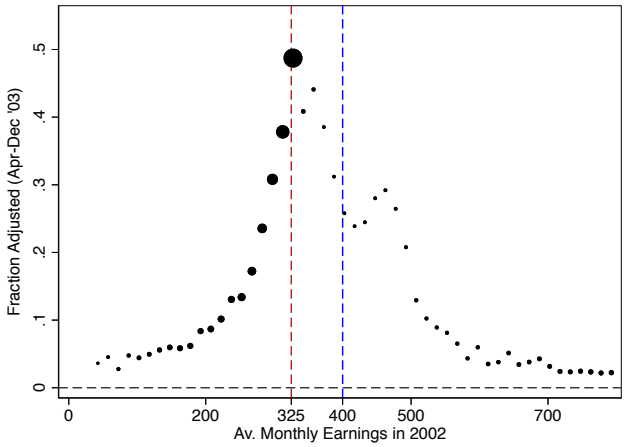
Figure 3: Earnings Distributions: Married Women 2010-2015



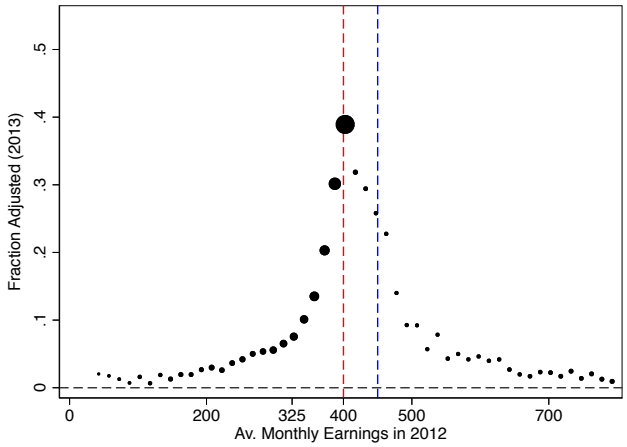
Notes: This figure plots the distribution of duration-weighted, average monthly earnings (all employment sources) for our *married women sample* (Section 3) between 2010 and 2015. Observations are weighted by the fraction of days an individual is in dependent employment, where individuals working year-round get a weight of one. Bins are €15 wide beginning with a bin centered at €27.5. The prevailing mini job threshold is indicated by the blue solid line, while old thresholds are indicated by the red, dashed line. The threshold was at €325 prior to April 1st 2003, at €400 prior to January 1st 2013, and at €450 thereafter.

Figure 4: Adjustment Rates as a Function of Initial Earnings

(a) Frac. Adj. in '03 by 2002 earnings



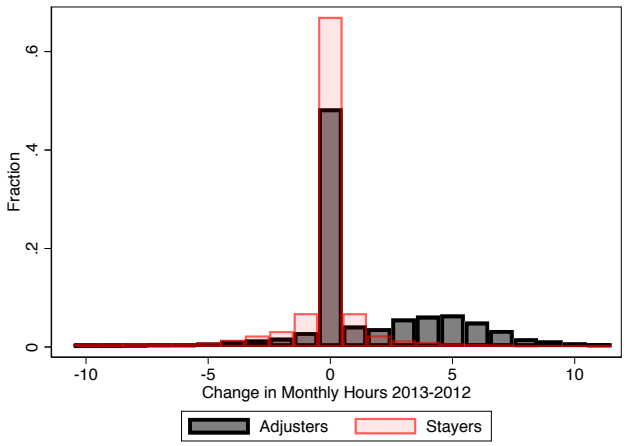
(b) Frac. Adj. in '13 by 2012 earnings



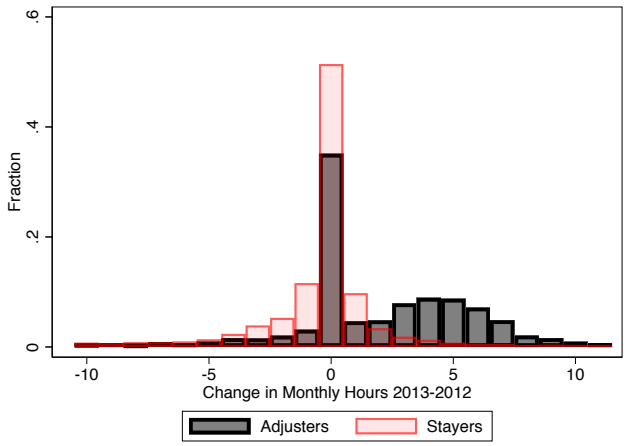
Notes: This figure plots average adjustment rates in the first post-reform year as a function of pre-reform earnings for our *married women sample*. Panel (a) plots average adjustment (in Apr-Dec 2003) as a function of 2002 earnings. Panel (b) plots average adjustment (in 2013) as a function of 2012 earnings. Adjustment is defined as having average monthly earnings between $Z + 12.5$ and $\tilde{Z} + 12.5$, where Z is the old notch and $\tilde{Z}$ the new notch. The dashed red line corresponds to the pre-reform notch, while the dashed blue line corresponds to the post-reform notch. The size of each point is proportional to the number of mini jobbers at that pre-reform earnings level. Bin sizes are €15 wide starting at €20.

Figure 5: Hours Changes Following the 2013 Reform

(a) Δ Hours for Adjusters

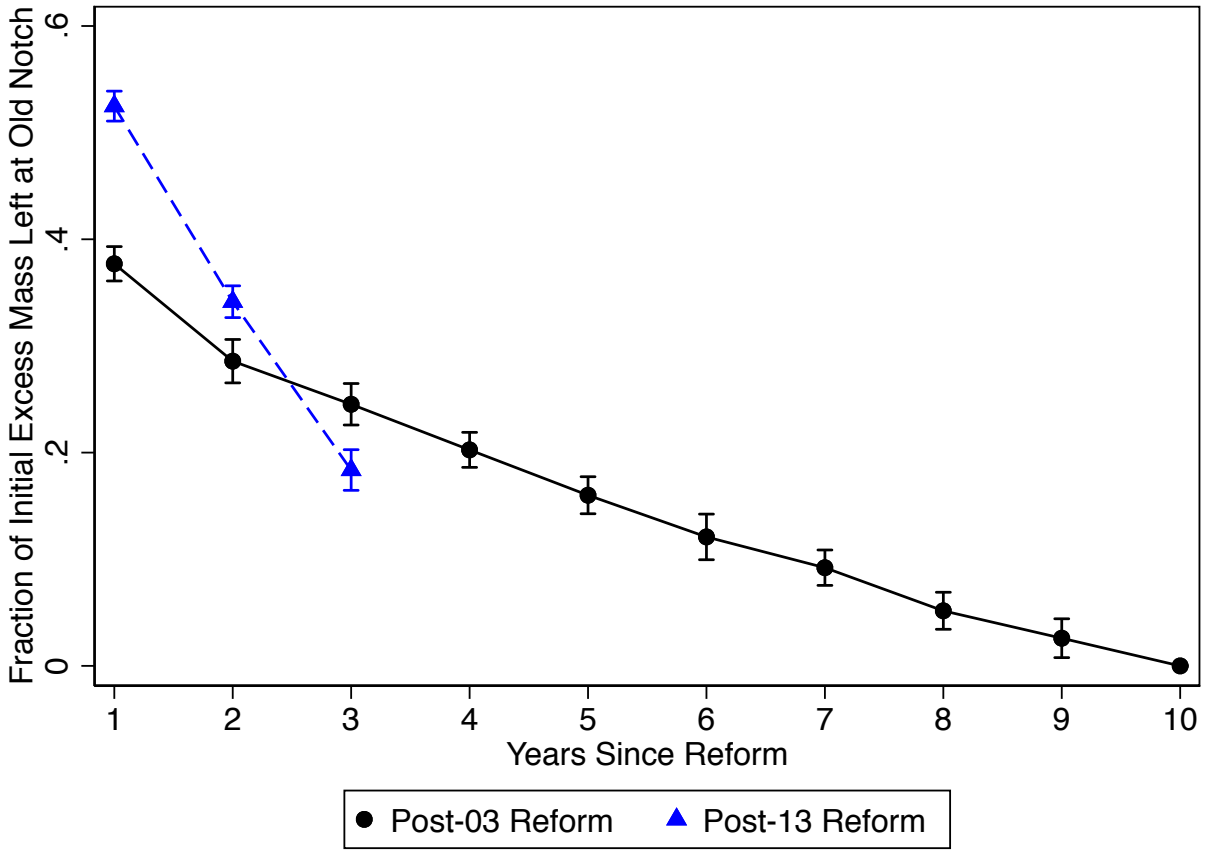


(b) Δ Hours, Estab. w/ ≥ 10 Mini Jobbers



Notes: This figure shows how hours change between 2013 and 2012 for adjusters — mini jobbers close to the original notch in 2012 that increased their mini job earnings post-reform — and for stayers — mini jobbers close to the original notch in 2012 that remained at the old notch in 2013 — in our *married women sample*. Stayers are defined as persons with average monthly earnings between €362.5 and 412.5 in 2012 who remain in that range in 2013 and whose average monthly earnings change by less than €20. Adjusters are defined as persons with average monthly earnings between €362.5 and 412.5 in 2012 whose earnings increase by at least €30 and fall between €412.5 and 462.5 euro in 2013. Panel (a) plots the change in average monthly hours worked between 2012 and 2013 for stayers and adjusters separately. Panel (b) does the same but restricts to women at establishments with at least 10 mini jobbers earning between €362.5 and 412.5 euro in 2012.

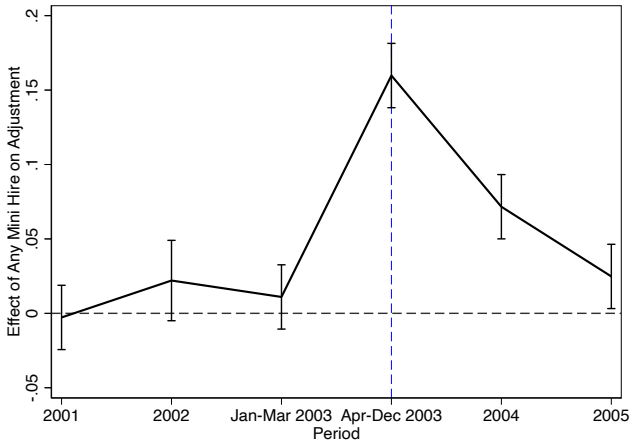
Figure 6: Bunching at the Old Notch Following Each Reform



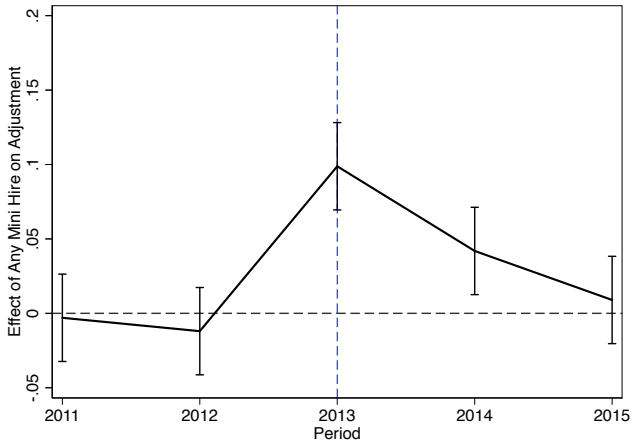
Notes: This figure plots the share of the initial bunching mass at the original notch that remains stuck at the old notch in each post-reform year. The black solid line corresponds to the 2003 reform, where year 1 is Apr-Dec 2003, year 2 is 2004, and so on. The blue dashed line corresponds to the 2013 reform, with year 1 being 2013, year 2 being 2014, and so on. The bunching mass is calculated by fitting a 5th degree counter-factual polynomial to the data, excluding earnings bins between z_l and z_u. z_l is selected visually to be centered at €147.5. z_u is chosen to be the bin that most closely sets the total bunching mass left of the notch equal to the missing mass to the right of the threshold. Average monthly earnings are divided into €15 bins starting at €20 and the estimation window extends to €2007.5. Figures A.5–A.6 show the counter-factual densities and shade in what counts towards bunching at the old threshold. We note that the estimated excess mass in 2012 is likely due to imprecision in targeting the new threshold and term this level the 'natural' level of excess mass. We subtract this 'natural' level of excess mass from all bunching masses. We plot the bunching mass in each year as a fraction of the bunching mass at the notch in the last pre-reform year.

Figure 7: Effects of Establishment Level Labor Demand on Adjustment

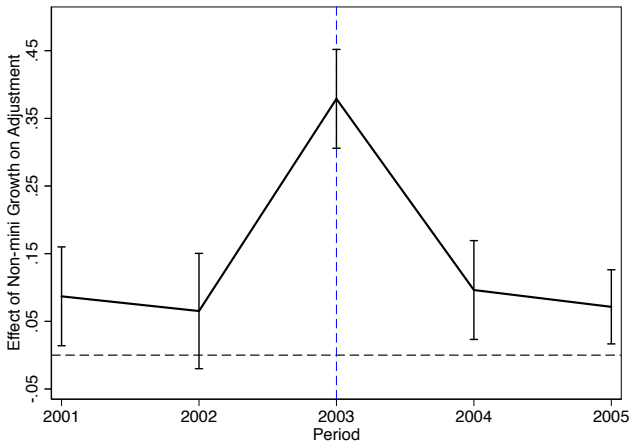
(a) 2003 Reform: Any New Mini Hire



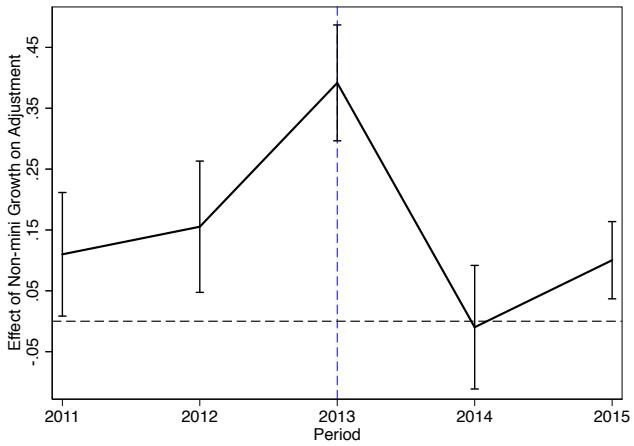
(b) 2013 Reform: Any New Mini Hire



(c) 2003 Reform: Non-Mini Growth

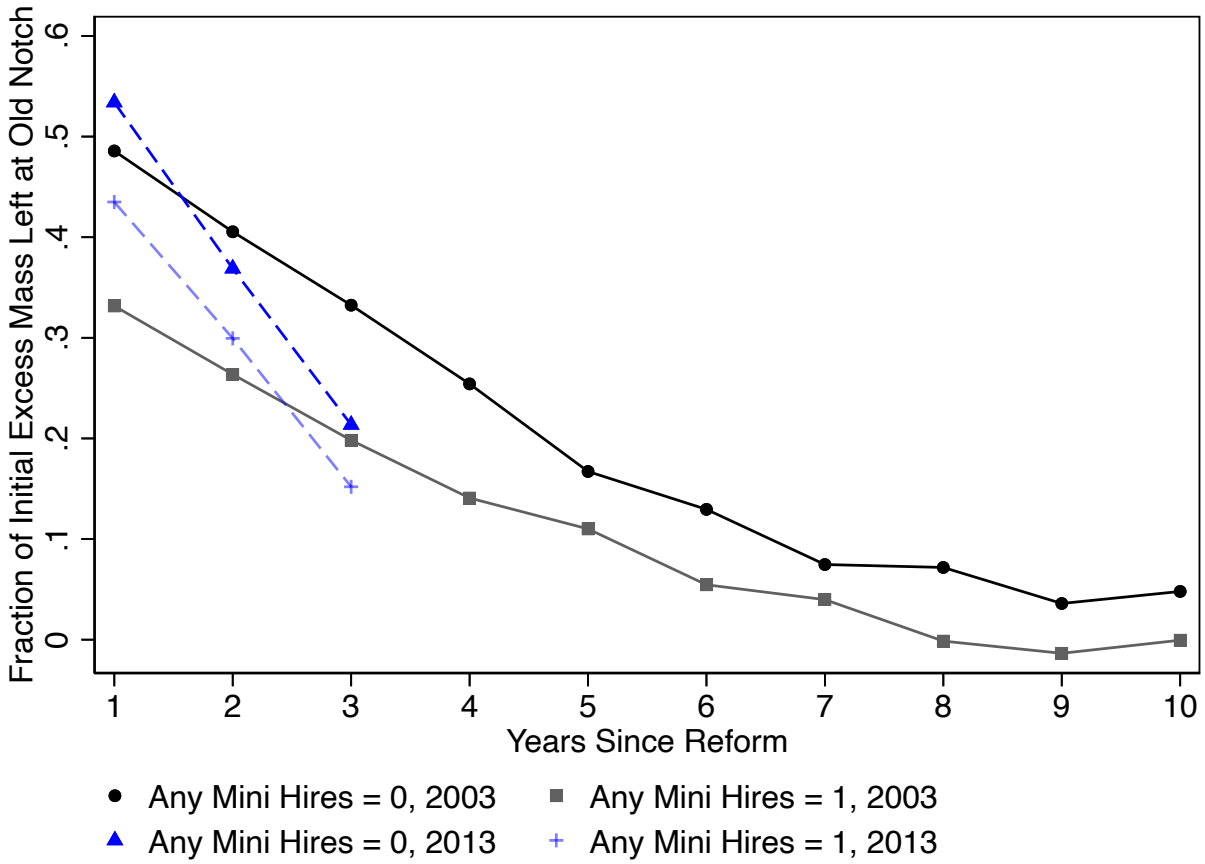


(d) 2013 Reform: Non-Mini Growth



Notes: Panel (a) plots the regression coefficients from Table 2, Panel A, Column (3). Panel (b) plots the regression coefficients from Table 2, Panel A, Column (6). Panel (c) plots the regression coefficients from Table 3, Panel A, Column (3). Panel (d) plots the regression coefficients from Table 3, Panel A, Column (6). All coefficients are plotted as a fraction of the dependent variable mean. The dashed blue line corresponds to the first post-reform period.

Figure 8: Bunching at the Old Notch Following Each Reform Varies with Labor Demand



Notes: This figure plots the share of the initial bunching mass at the original notch that remains stuck at the old notch in each post-reform year for a propensity score matched sample of married women in establishments with and without a post-reform mini hire (see Section 5 for details). The black solid line with circles corresponds to the excess mass following the 2003 reform for women at an establishment with no mini hires in 2003, while the gray solid lines with squares corresponds to the excess mass following the 2003 reform for women at an establishment with at least one mini hire in 2003. The blue dashed line with triangles corresponds to the excess mass following the 2013 reform for women at an establishment without a mini hire in 2013, while the light blue dashed line with plusses shows the excess mass following the 2013 reform for women at an establishment with at least one mini hire in 2013. The bunching mass is calculated as in Figure 6.

Tables

Table 1: Future Earnings Distributions for Mini Jobbers at the Notch

	Panel A: Mini jobbers at the notch pre-reform							
	2002	2003*	2004	2005	2012	2013	2014	2015
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Earnings below original notch	**0**	**7.0**	**8.7**	**9.6**	**0**	**5.6**	**5.9**	**5.3**
— At Same Establishment	*0*	*6.2*	*6.8*	*6.7*	*0*	*5.2*	*4.8*	*3.9*
Earnings stay at original notch	**100**	**33.7**	**26.2**	**21.3**	**100**	**43.6**	**28.6**	**15.9**
— At Same Establishment	*100*	*32.6*	*24.2*	*18.5*	*100*	*42.7*	*27.1*	*14.3*
Earnings increase to new notch	**0**	**42.7**	**46.4**	**47.0**	**0**	**36.5**	**40.3**	**38.5**
— At Same Establishment	*0*	*40.4*	*41.2*	*39.3*	*0*	*35.1*	*37.6*	*34.7*
Earnings exceed mini threshold (Midi or Regular job)	**0**	**9.1**	**12.3**	**16.0**	**0**	**13.0**	**23.4**	**33.3**
Not in Data	**0**	**7.6**	**6.5**	**6.2**	**0**	**1.2**	**1.8**	**6.9**
Column Total (for rows in bold)	100%	100%	100%	100%	100%	100%	100%	100%
N	64160	64160	64160	64160	51283	51283	51283	51283

	Panel B: Mini jobbers stuck at the old notch in 1st year post-reform							
	2002	2003*	2004	2005	2012	2013	2014	2015
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Earnings below original notch	**0**	**0**	**7.9**	**9.5**	**0**	**0**	**5.1**	**5.6**
— At Same Establishment	*0*	*0*	*6.8*	*6.7*	*0*	*0*	*4.3*	*4.3*
Earnings stay at original notch	**100**	**100**	**59.6**	**44.5**	**100**	**100**	**56.5**	**28.5**
— At Same Establishment	*100*	*100*	*57.7*	*41.3*	*100*	*100*	*54.9*	*26.6*
Earnings increase to new notch	**0**	**0**	**24.1**	**32.2**	**0**	**0**	**25.8**	**36.3**
— At Same Establishment	*0*	*0*	*20.5*	*26.0*	*0*	*0*	*23.4*	*32.5*
Earnings exceed mini threshold (Midi or Regular job)	**0**	**0**	**5.2**	**9.2**	**0**	**0**	**11.4**	**22.6**
Not in data in given year	**0**	**0**	**3.3**	**4.6**	**0**	**0**	**1.2**	**7.0**
Column Total (for rows in bold)	100%	100%	100%	100%	100%	100%	100%	100%
N	20897	20897	20897	20897	21914	21914	21914	21914

Notes: This table examines earnings adjustment patterns for those in our *married women sample* who hold a single mini job with earnings close to the notch Z ($Z - 37.5 <$ monthly earnings $< Z + 12.5$) in the year prior to each reform. The bold rows categorize the proportion of these workers that fall into each of the following average monthly earnings ranges in subsequent years: $< Z - 37.5$, in the same range, above $Z + 12.5$ up to $\tilde{Z} + 12.5$ where $\tilde{Z}$ is the new threshold, or beyond the new mini job threshold ($> \tilde{Z} + 12.5$). Italicized rows categorize workers using the same earnings ranges, but based on mini job earnings at the workers' initial establishment (as opposed to earnings anywhere). Panel B does as in Panel A but further restricts to mini jobbers who remained stuck at the old notch at their initial establishment for the entire first post-reform year. 2003* refers to April-December 2003.

Table 2: Adjustment on Labor Demand Indicators: Any Mini Hires

	2003 Reform: Adjusted to 400 in 2003			2013 Reform: Adjusted to 450 in 2013		
	Panel A: Adjustment on Any New Mini Hire					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
any mini hires 01/11	-0.010*	-0.004	-0.002	-0.009	-0.002	0.000
	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
any mini hires 02/12	-0.007	0.004	0.007	-0.017***	-0.005	-0.002
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
any mini hires 03(Jan-Mar)	0.000	0.004	0.004			
	(0.004)	(0.004)	(0.004)			
any mini hires 03(Apr-Dec)/13	0.055***	0.057***	0.058***	0.028***	0.032***	0.033***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
any mini hires 04/14	0.022***	0.025***	0.026***	0.009	0.014**	0.015**
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
any mini hires 05/15	0.007	0.009*	0.009*	-0.001	0.003	0.005
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Num. of Individuals	220307	220307	220307	194877	194877	194877
Num. of Establishments	74086	74086	74086	68409	68409	68409
Adj. R^2	0.043	0.109	0.115	0.024	0.080	0.091
Dep. Var. Mean	0.363	0.363	0.363	0.334	0.334	0.334
Mean Any Hires 03(Apr-Dec)/13	0.766	0.766	0.766	0.809	0.809	0.809
Oster's δ		-57.737	-69.100		-16.515	-16.839
Baseline Controls	X	X	X	X	X	X
Individual Controls		X	X		X	X
Establishment Controls			X			X

	Panel B: Adjustment in Later Periods					
	IN '03	IN '04	IN '05	IN '13	IN '14	IN '15
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
any mini hires 01/11	-0.010*	-0.000	0.000	-0.009	-0.009	-0.019**
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.006)
any mini hires 02/12	-0.007	0.000	-0.007	-0.017***	-0.005	-0.024***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
any mini hires 03(Jan-Mar)	0.000	-0.005	-0.005			
	(0.004)	(0.004)	(0.003)			
any mini hires 03(Apr-Dec)/13	0.055***	0.011**	-0.011***	0.028***	-0.007	-0.023***
	(0.004)	(0.004)	(0.003)	(0.005)	(0.005)	(0.006)
any mini hires 04/14	0.022***	0.019***	0.004	0.009	0.021***	-0.006
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
any mini hires 05/15	0.007	0.007	0.015***	-0.001	0.011*	0.024***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Num. of Individuals	220307	65697	48272	194877	71149	49072
Num. of Establishments	74086	34919	28018	68409	36367	28113
Adj. R^2	0.043	0.010	0.005	0.024	0.010	0.017
Dep. Var. Mean	0.363	0.136	0.072	0.334	0.162	0.173
Mean Any Hires 03(Apr-Dec)/13	0.766	0.630	0.603	0.809	0.727	0.697
Baseline Controls	X	X	X	X	X	X

Notes: Columns (1)–(3) examine adjustment among mini jobbers in our *establishment sample* following the 2003 reform and columns (4)–(6) examine the 2013 reform. Panel A regresses a dummy for whether the mini jobber adjusted to the new notch in the first post-reform year at their establishment on indicators for any new mini hire at the establishment. Columns (1) and (4) include our baseline controls, columns (2) and (5) add individual controls, and columns (3) and (6) further add additional establishment-level controls (see Section 5.1 and Appendix D for details on the estimation sample and controls). Panel B explores adjustment in later periods. Column (2) [5] explores adjustment in 2004 [2014], conditional on those establishments where no one adjusted in 2003 [2004]. Column (3) [(6)] explores adjustment in 2005, conditional on those establishments without adjustment in 2003 and 2004 [2013 and 2014]. Standard errors, clustered at the establishment level, are in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 3: Adjustment on Labor Demand Indicators: Nonmini Earnings Growth

	2003 Reform: Adjusted to 400 in 2003			2013 Reform: Adjusted to 450 in 2013		
Panel A: Adjustment on Non-Mini Earnings Growth						
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Δ Ln total non-mini earnings 01-00/11-10	0.055***	0.041***	0.025*	0.046**	0.035*	0.031*
	(0.012)	(0.012)	(0.012)	(0.016)	(0.016)	(0.016)
Δ Ln total non-mini earnings 02-01/12-11	0.050***	0.026	0.023	0.069***	0.054**	0.049**
	(0.015)	(0.014)	(0.014)	(0.017)	(0.017)	(0.016)
Δ Ln total non-mini earnings 03-02/13-12	0.133***	0.119***	0.122***	0.125***	0.122***	0.117***
	(0.012)	(0.012)	(0.012)	(0.017)	(0.016)	(0.015)
Δ Ln total non-mini earnings 04-03/14-13	0.040***	0.037**	0.031**	0.009	0.003	-0.003
	(0.012)	(0.012)	(0.012)	(0.017)	(0.017)	(0.016)
Δ Ln total non-mini earnings 05-04/15-14	0.033***	0.028**	0.023*	0.037***	0.036***	0.026**
	(0.010)	(0.009)	(0.010)	(0.011)	(0.011)	(0.010)
Num. of Individuals	153979	153979	153979	137984	137984	137984
Num. of Establishments	39913	39913	39913	37802	37802	37802
Adj. R^2	0.043	0.106	0.114	0.028	0.086	0.099
Dep. Var. Mean	0.322	0.322	0.322	0.309	0.309	0.309
Δ Ln total non-mini earnings 03-02/13-12	-0.042	-0.042	-0.042	0.019	0.019	0.019
Oster's δ		15.175	14.174		64.215	18.091
Baseline Controls	X	X	X	X	X	X
Individual Controls		X	X		X	X
Establishment Controls			X			X
Panel B: Adjustment in Later Periods						
	in '03	in '04	in '05	in '13	in '14	in '15
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Δ Ln total non-mini earnings 01-00/11-10	0.055***	0.037**	0.012	0.046**	0.023	-0.048*
	(0.012)	(0.013)	(0.011)	(0.016)	(0.016)	(0.019)
Δ Ln total non-mini earnings 02-01/12-11	0.050***	-0.023	0.011	0.069***	0.035	-0.010
	(0.015)	(0.014)	(0.012)	(0.017)	(0.019)	(0.020)
Δ Ln total non-mini earnings 03-02/13-12	0.133***	0.055***	0.023**	0.125***	0.048**	0.049**
	(0.012)	(0.010)	(0.008)	(0.017)	(0.016)	(0.018)
Δ Ln total non-mini earnings 04-03/14-13	0.040***	0.057***	0.017*	0.009	0.083***	0.050**
	(0.012)	(0.010)	(0.008)	(0.017)	(0.016)	(0.016)
Δ Ln total non-mini earnings 05-04/15-14	0.033***	0.011	0.047***	0.037***	-0.006	0.047***
	(0.010)	(0.009)	(0.007)	(0.011)	(0.013)	(0.012)
Num. of Individuals	153979	41745	30164	137984	45614	29966
Num. of Establishments	39913	19267	15598	37802	19730	14910
Adj. R^2	0.043	0.014	0.009	0.028	0.015	0.016
Dep. Var. Mean	0.322	0.119	0.062	0.309	0.156	0.153
Mean Δ Ln total non-mini earnings 03-02/13-12	-0.042	-0.040	-0.045	0.019	0.011	0.006
Baseline Controls	X	X	X	X	X	X

Notes: Columns (1)–(3) examine adjustment among mini jobbers in our *establishment sample* following the 2003 reform and columns (4)–(6) examine the 2013 reform. We also require establishments have at least 10 non-mini jobbers in 2002 (2012). Panel A regresses a dummy for whether the mini jobber adjusted to the new notch in the first post-reform year at their establishment on the change in the natural logarithm of the total earnings accruing to non-mini jobbers at the establishment. Columns (1) and (4) include our baseline controls, columns (2) and (5) add individual controls, and columns (3) and (6) further add additional establishment-level controls (see Section 5.1 and Appendix D for details on the estimation sample and controls). Panel B explores adjustment in later periods. Column (2) [5] explores adjustment in 2004 [2014], conditional on those establishments where no one adjusted in 2003 [2004]. Column (3) [(6)] explores adjustment in 2005, conditional on those establishments without adjustment in 2003 and 2004 [2013 and 2014]. Standard errors, clustered at the establishment level, are in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 4: Adjustment on Labor Demand Indicators: Predicted Mini Hires

	2003 Reform:			2013 Reform:		
	Panel A: Prediction Frac. Hires – Parsimonious Spec.					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Predicted Hires (OLS)	0.406***	0.433***	0.482***	0.169***	0.229***	0.259***
	(0.014)	(0.013)	(0.015)	(0.021)	(0.021)	(0.020)
	Panel B: Prediction Frac. Hires – LASSO					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Predicted Hires (LASSO)	0.351***	0.358***	0.400***	0.100***	0.176***	0.257***
	(0.024)	(0.025)	(0.027)	(0.020)	(0.019)	(0.021)
	Panel C: Actual Frac. Hires					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Actual Hires	0.096***	0.107***	0.114***	0.053***	0.075***	0.084***
	(0.006)	(0.006)	(0.006)	(0.010)	(0.009)	(0.009)
Num. of Individuals	207106	207106	207106	177202	177202	177202
Num. of Establishments	66730	66730	66730	58770	58770	58770
Dep. Var. Mean	0.359	0.359	0.359	0.332	0.332	0.332
Adj. R^2 (Pred. Hires OLS)	0.049	0.116	0.121	0.025	0.083	0.094
Adj. R^2 (Pred. Hires LASSO)	0.048	0.114	0.120	0.025	0.082	0.094
Oster's δ (Pred. Hires OLS)		-30.797	128.380		-8.495	-10.962
Oster's δ (Pred. Hires LASSO)		-173.158	19.614		-5.265	-5.331
Baseline Controls	X	X	X	X	X	X
Individual Controls		X	X		X	X
Establishment Controls			X			X
	First Stage Fit					
Adj. R^2 (OLS)	0.244	0.244	0.244	0.230	0.230	0.230
Adj. R^2 (LASSO)	0.262	0.262	0.262	0.271	0.271	0.271
RMSE (No X Vars.)	0.216	0.216	0.216	0.195	0.195	0.195
RMSE (OLS)	0.188	0.188	0.188	0.171	0.171	0.171
RMSE (LASSO)	0.185	0.185	0.185	0.167	0.167	0.167

Notes: This table shows the association between predicted hires and individual adjustment for both the 2003 as well as the 2013 reform for those mini jobbers in our *establishment sample*. We also require establishments have at least 1 mini and non-mini jobber in 1999-2002 (2009-2012). Predictions are formed by regressing the fraction of new mini-job hires (capped at 1) in the pre-reform period on a set of pre-determined predictors using OLS (panel A) and LASSO (panel B), and then creating a prediction for the fraction of mini hires in the reform year based on the obtained coefficients. The set of predictors for each specification is detailed in Section 5 and Appendix D. Panel C shows the association between adjustment and realized (as opposed to predicted) mini hires. Standard errors, clustered at the establishment level, are in parentheses. In panel A and B these are bootstrapped based on 500 replications to address the generated regressor.

Online Appendix for "The Speed of Earnings Responses to Taxation and the Role of Firm Labor Demand"

MATTHEW GUDGEON AND SIMON TRENKLE

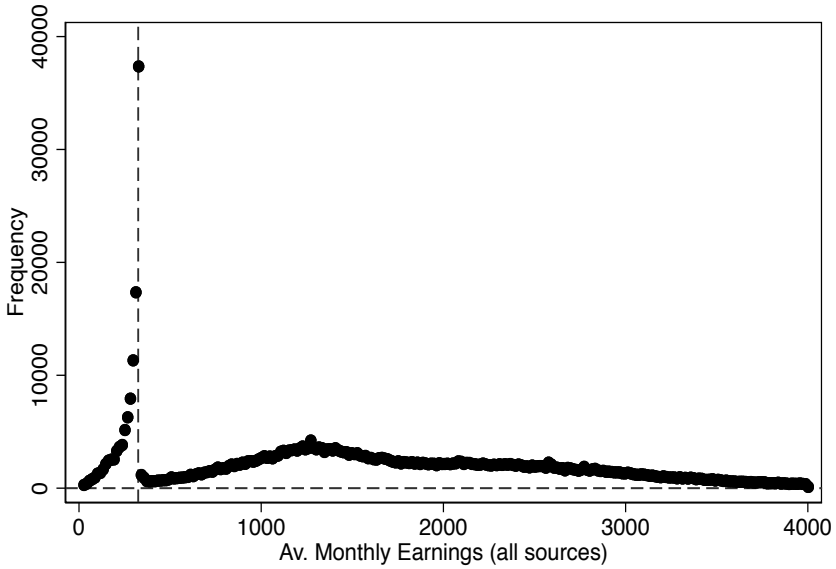
Appendix Table of Contents

A Appendix Figures and Tables

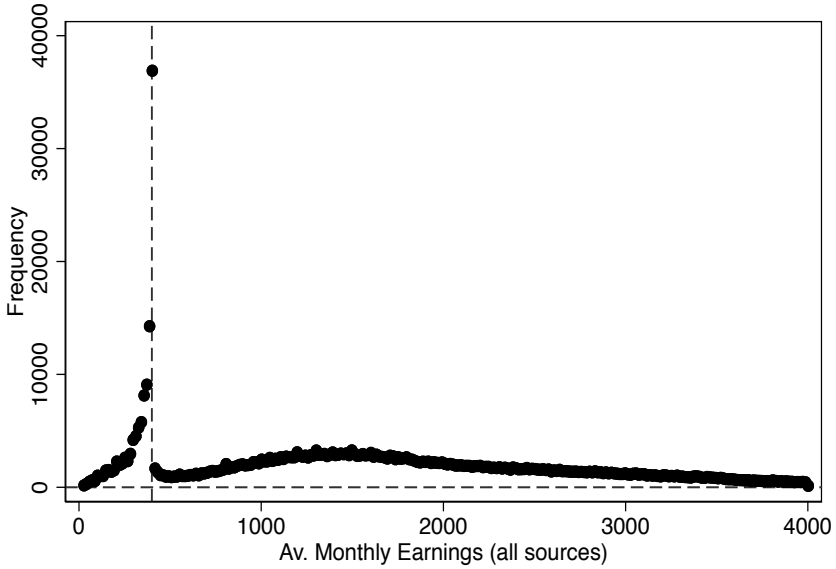
A.1 Appendix Figures

Figure A.1: Full Earnings Distribution: Married Women 2002 and 2012

(a) Earnings Distribution for Married Women 2002



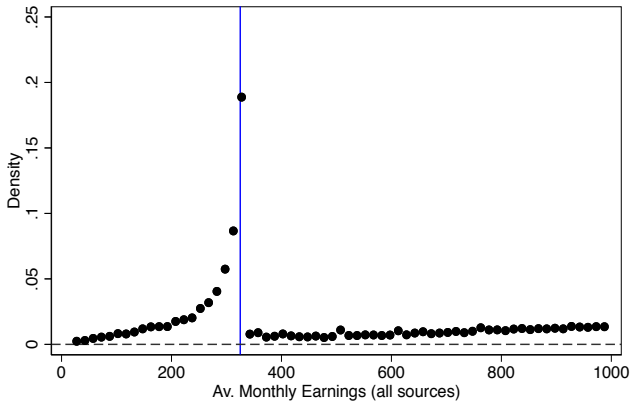
(b) Earnings Distribution for Married Women 2012



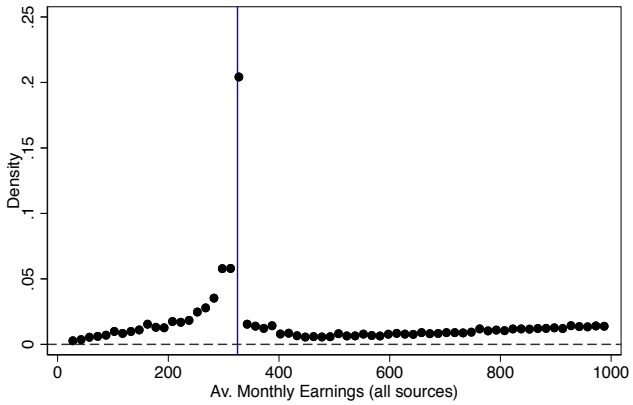
Notes: This figure plots the same 2002 and 2012 earning distributions as Figures 2- 3, but shows the entire distribution.

Figure A.2: Earnings Distributions: All Women 2002-2006

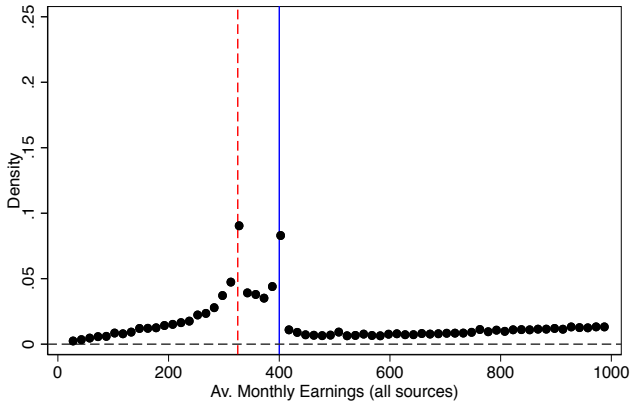
(a) Earnings Distribution 2002



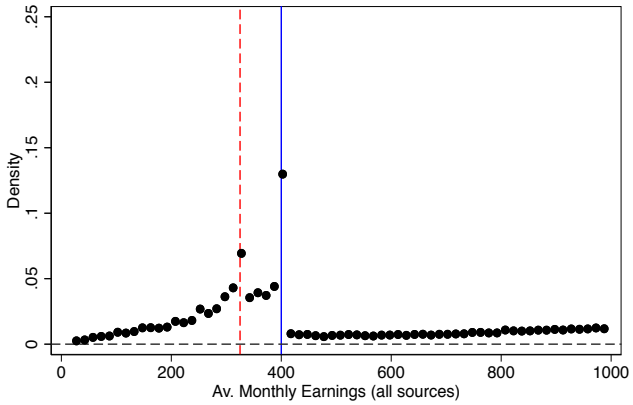
(b) Earnings Distribution 2003 (Jan-Mar)



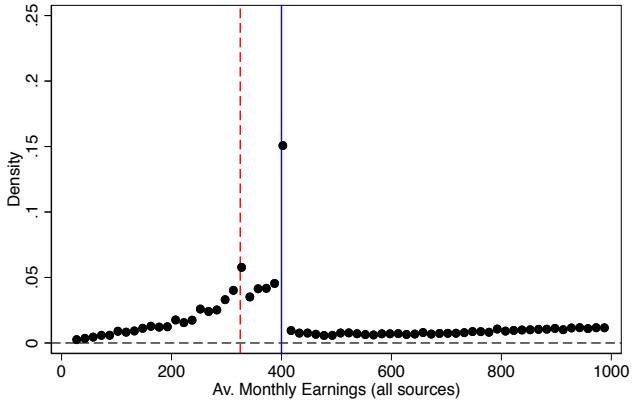
(c) Earnings Distribution 2003 (Apr-Dec)



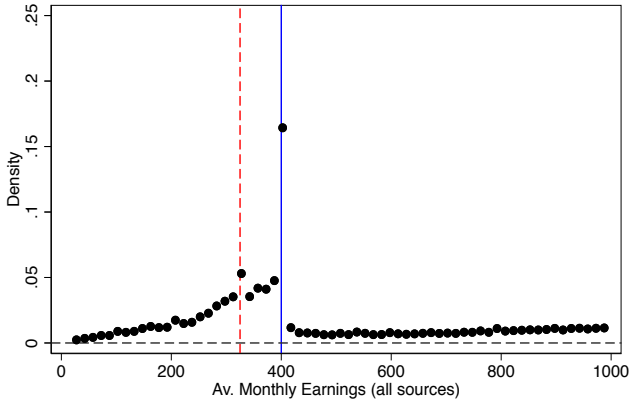
(d) Earnings Distribution 2004



(e) Earnings Distribution 2005



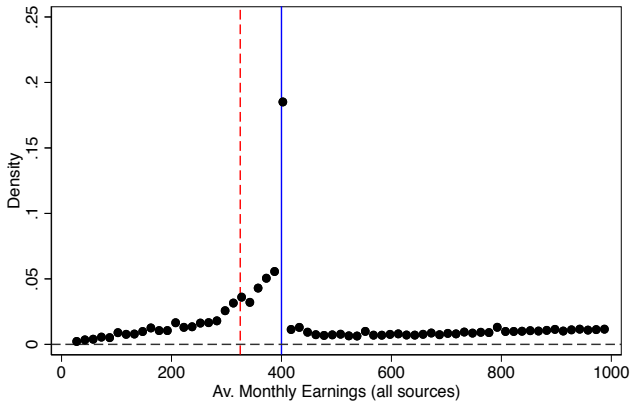
(f) Earnings Distribution 2006



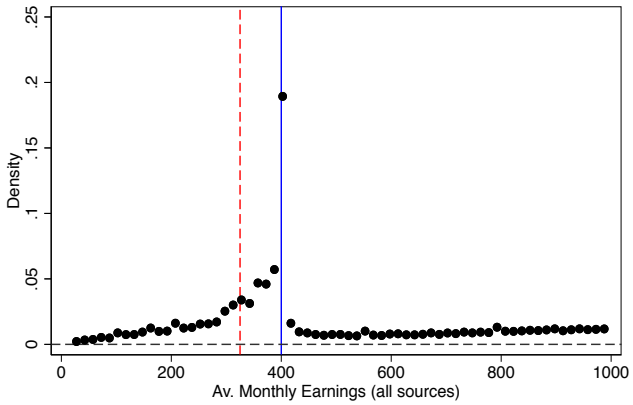
Notes: This figure replicates Figures 2–3 for all women, instead of just the identified married women with husband annual earnings between 33000 and 53000. It still restricts to women working in West Germany, aged 26 to 55 (inclusive). It plots the distribution of average monthly earnings (all employment sources). The mini job threshold was at 325 euro prior to April 1st 2003, at 400 euro prior to January 1st 2013, and at 450 euro thereafter.

Figure A.3: Earnings Distributions: All Women 2010-2015

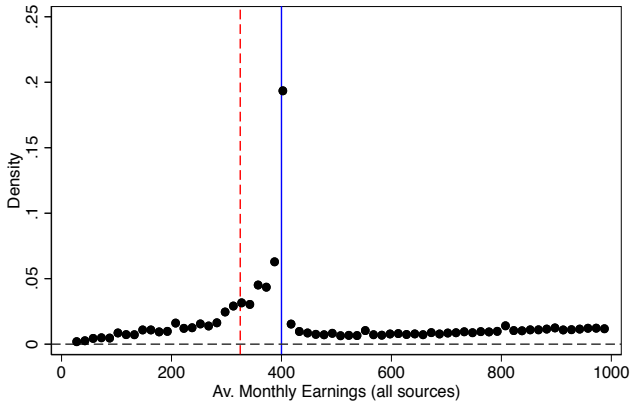
(a) Earnings Distribution 2010



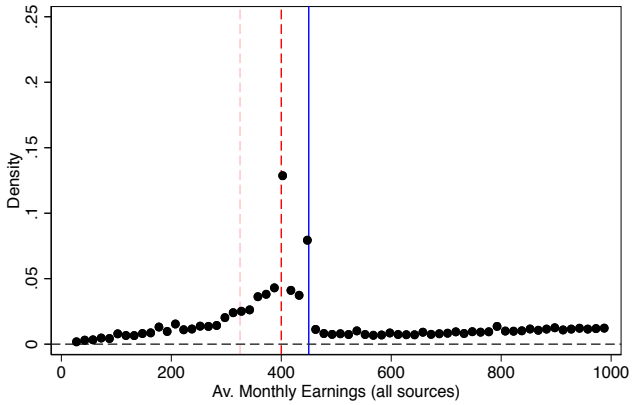
(b) Earnings Distribution 2011



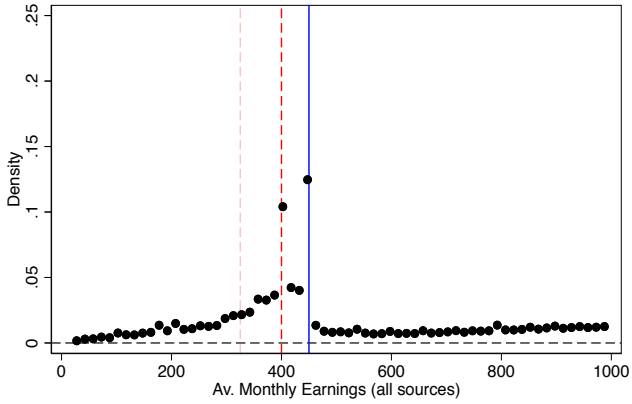
(c) Earnings Distribution 2012



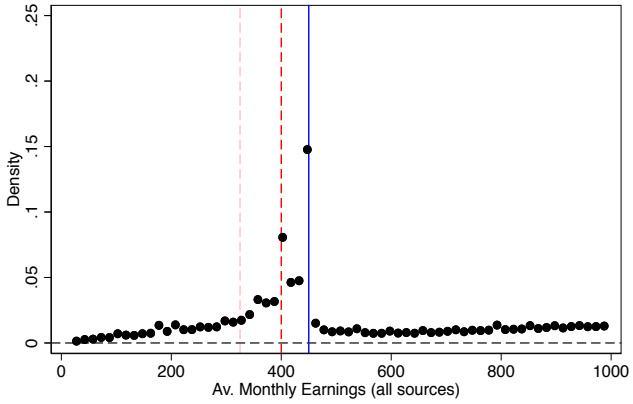
(d) Earnings Distribution 2013



(e) Earnings Distribution 2014

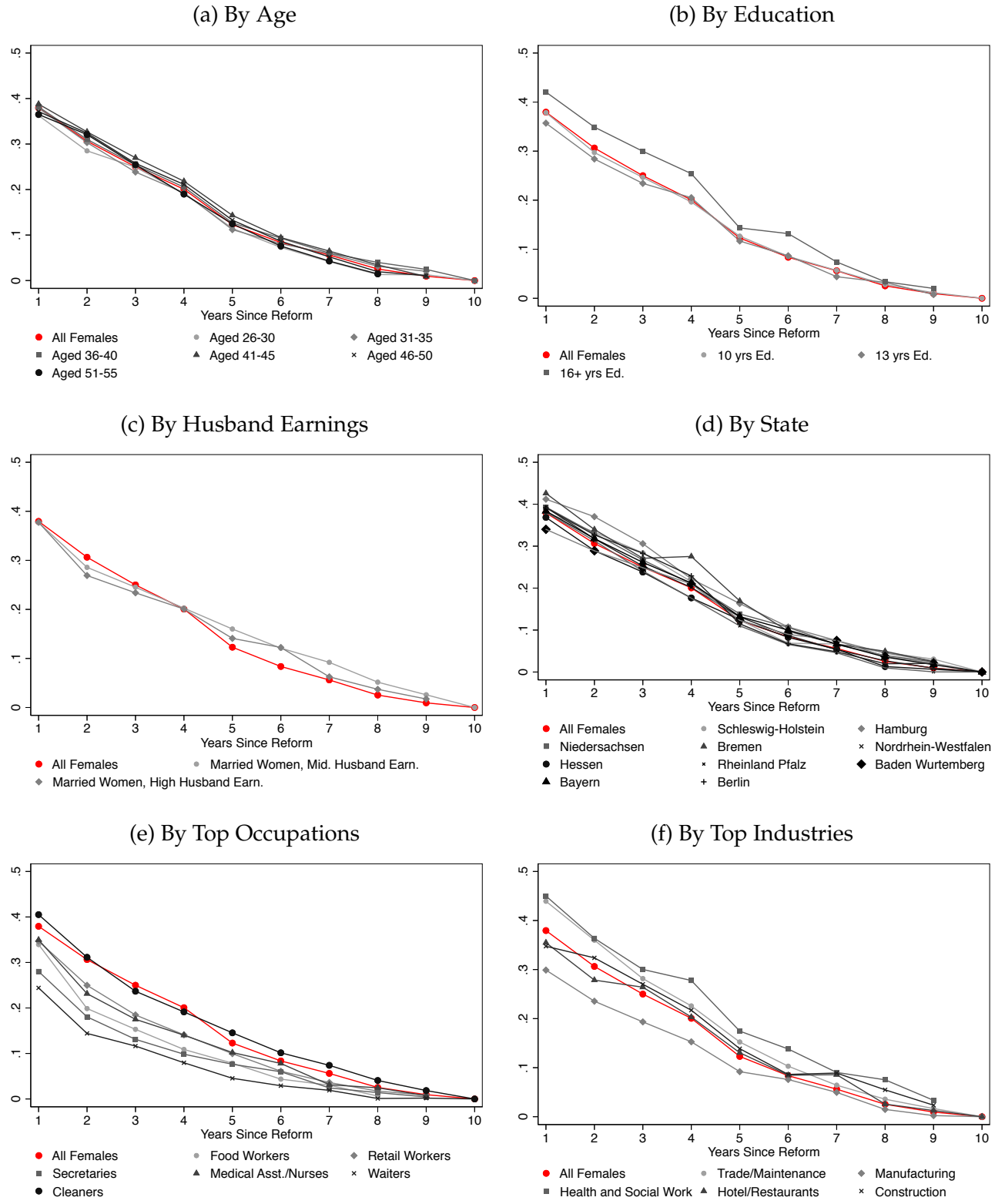


(f) Earnings Distribution 2015



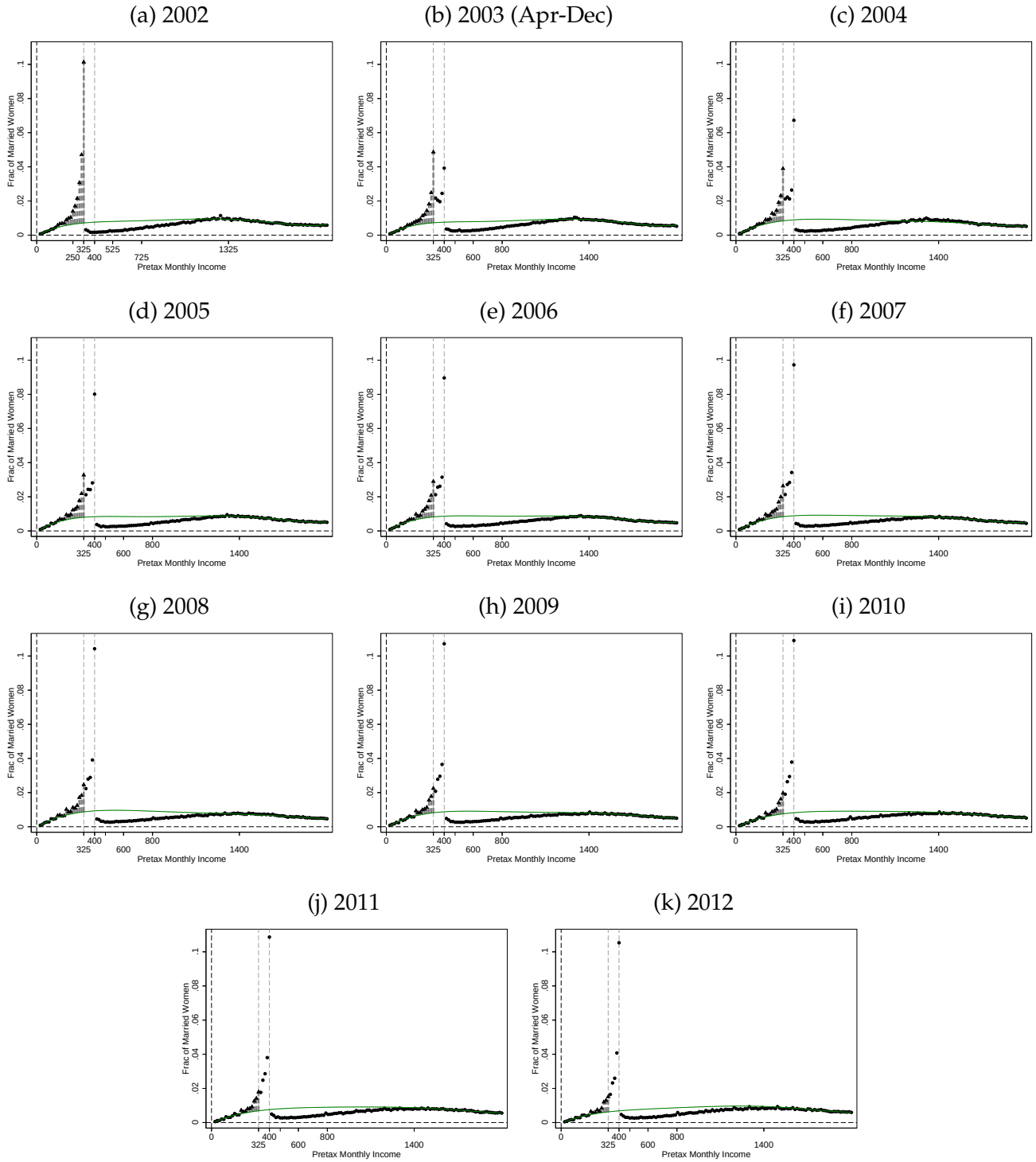
Notes: This figure replicates Figures 2–3 for all women, instead of just the identified married women with husband annual earnings between 33000 and 53000. It still restricts to women working in West Germany, aged 26 to 55 (inclusive). It plots the distribution of average monthly earnings (all employment sources). The mini job threshold was at 325 euro prior to April 1st 2003, at 400 euro prior to January 1st 2013, and at 450 euro thereafter.

Figure A.4: Heterogeneity in the Fraction of Initial Bunching Mass Left at Old Notch



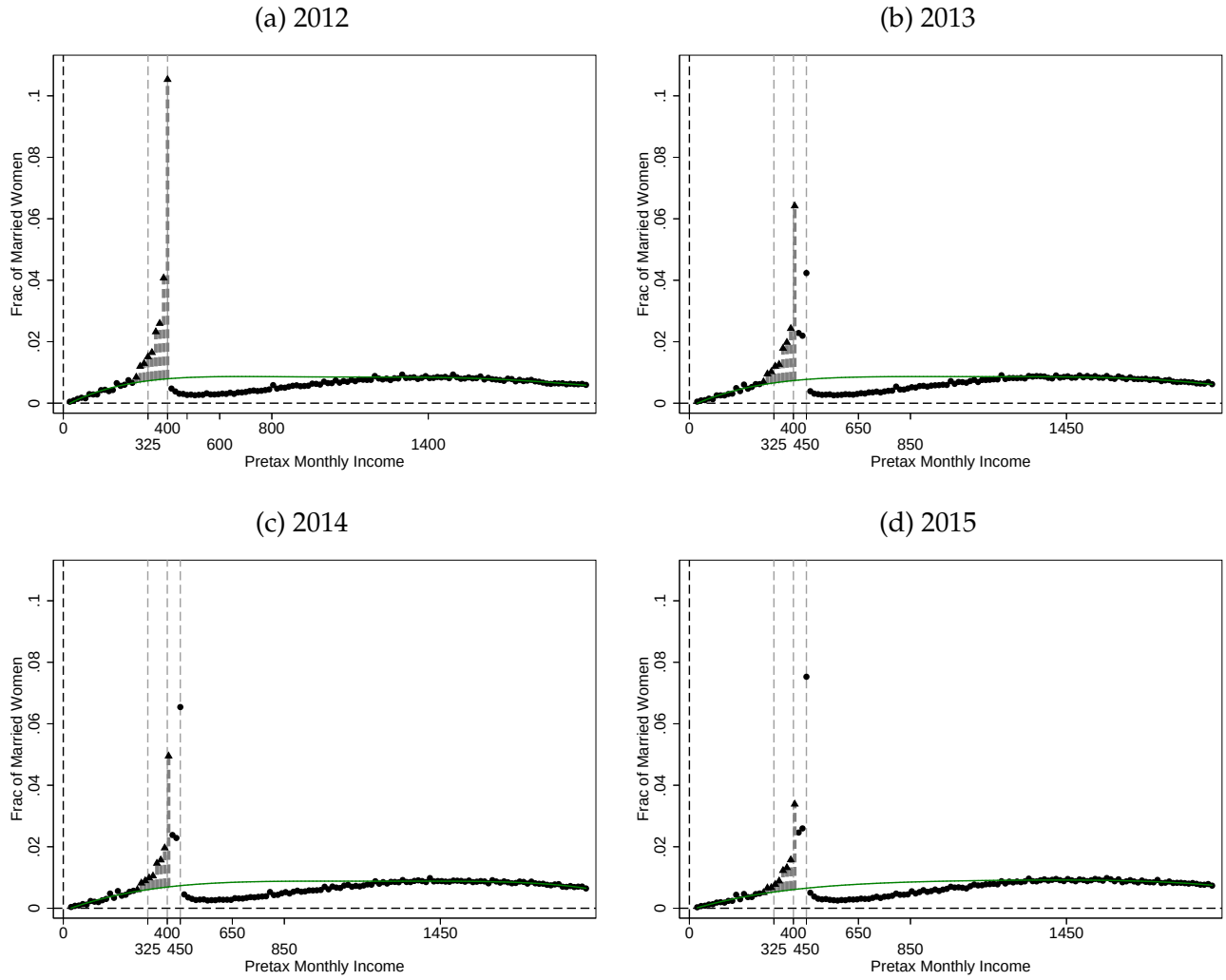
Notes: This figure is constructed in the same manner as Figure 6 and plots the share of the initial bunching mass at the pre-reform notch that remains at the old notch in each year following the 2003 reform. The red line does this for a sample of all women (not just the married women in Figure 6) while each other line does so for a sub-sample of the all women sample. For additional details see the notes to Figure 6.

Figure A.5: Bunching at Old Notch: Married Women 2002-2012



Notes: These figures show the excess bunching mass at the old notch (325 euro) after the 2003 reform for the period-by-period sample of married women in Figure 2. The estimated period-by-period counter-factual density is depicted as a light green line, while the bunching mass at the old notch is shaded in dashed-gray. Average monthly earnings (from all employment sources) are divided into 15 euro bins, starting at 20 euro. When estimating the counter-factual we choose the lowermost omitted bin (z_l) to be the bin centered at 225 euro. We select (z_u) as the bin that most closely sets the total bunching mass (not just at the old threshold) equal to the missing mass to the right of the threshold. We use a quintic polynomial. The estimation window extends up to 2007.5 euro. We note that the estimated excess mass in 2012 is likely due to imprecision in targeting the new threshold and term this level the 'natural' level of excess mass.

Figure A.6: Bunching at Old Notch: Married Women 2012-2015

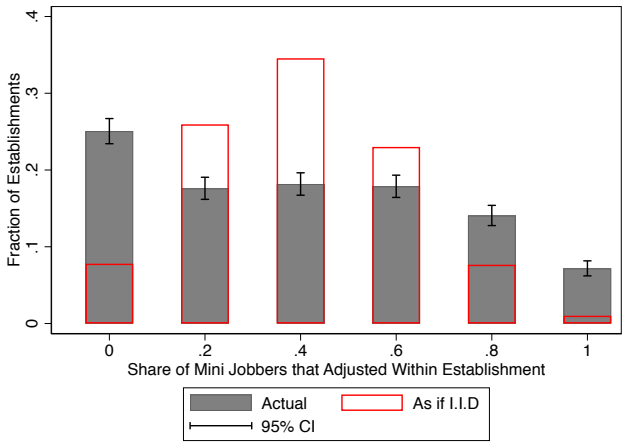


Notes: These figures show the excess bunching mass at the old notch (400 euro) after the 2013 reform for the period-by-period sample of married women in Figure 3. The estimated period-by-period counter-factual density is depicted as a light green line, while the bunching mass at the old notch is shaded in dashed-gray. Average monthly earnings (from all employment sources) are divided into 15 euro bins, starting at 20 euro. When estimating the counter-factual we choose the lowermost omitted bin (z_l) to be the bin centered at 297.5 euro. We select (z_u) as the bin that most closely sets the total bunching mass (not just at the old threshold) equal to the missing mass to the right of the threshold. We use a quintic polynomial. The estimation window extends up to 2007.5 euro.

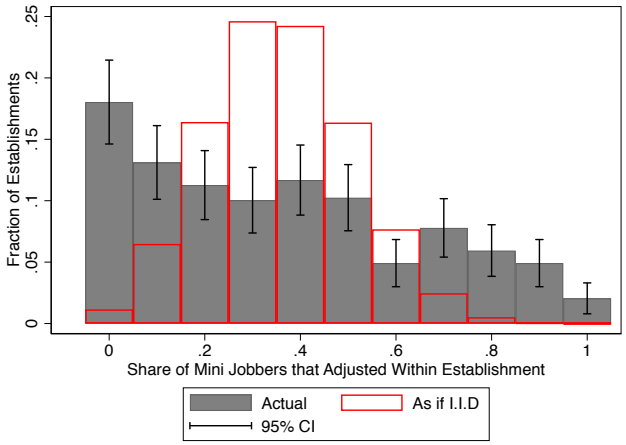
Figure A.7: Share of Mini Jobbers who Adjusted within Establishment

2003 Reform

(a) Establishments with $\tilde{N} = 5$ in 2002

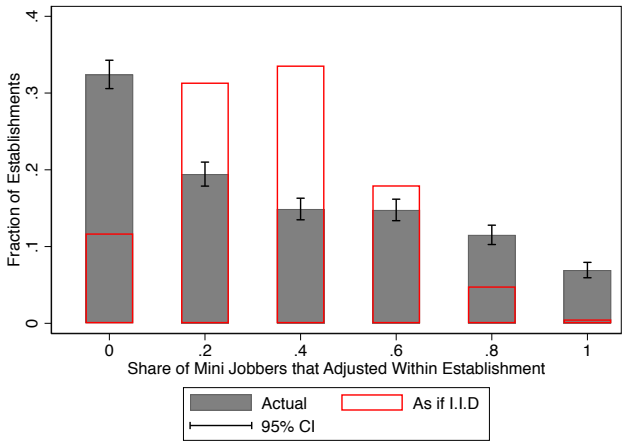


(b) Establishments with $\tilde{N} = 10$ in 2002

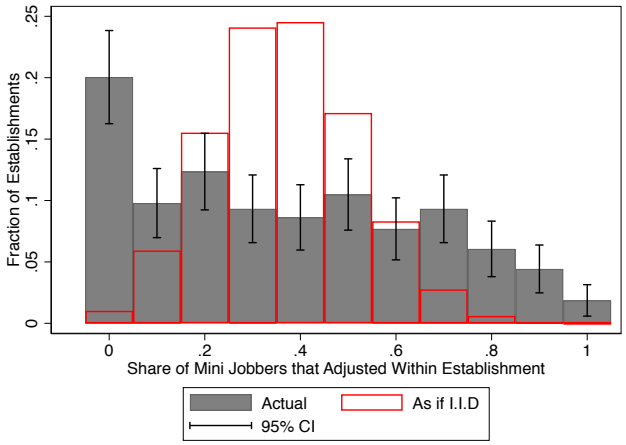


2013 Reform

(c) Establishments with $\tilde{N} = 5$ in 2012



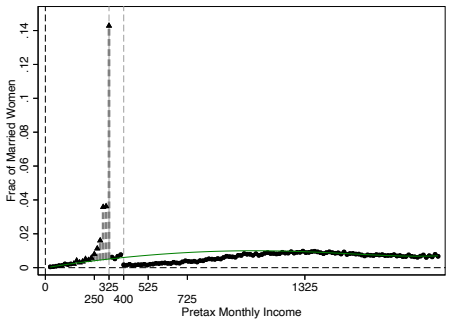
(d) Establishments with $\tilde{N} = 10$ in 2012



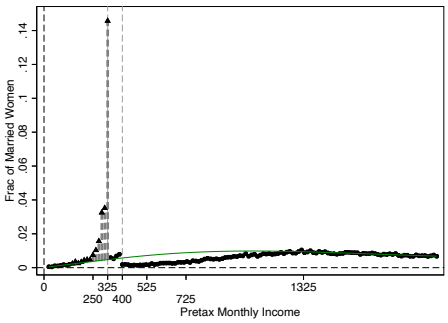
Notes: These figures focus on establishments with a fixed number of mini jobbers ($\tilde{N}$) at the old threshold and calculate the fraction of these mini jobbers that increase their earnings post-reform. The dark grey, shaded bars plot the share of establishments that fall into each of the possible adjustment categories (exactly 0 out of $\tilde{N}$ workers adjust, exactly 1 out of $\tilde{N}$ adjust, and so forth). The empty bars (labeled as if i.i.d, or independently and identically distributed) plot the distribution that would arise if workers all had the average adjustment propensity (among firms in the figure) and were randomly distributed across the firms in the figure. 95% confidence intervals are calculated separately for each possible fraction adjusting and all distributions are highly statistically different from their i.i.d counterpart in chi-squared tests. The estimation sample consists of all long-term mini jobbers in our establishment sample in a single job with average monthly earnings between $Z - 37.5$ and $Z + 12.5$ in the pre-reform year, where Z is the notch threshold. Long-term mini jobbers are defined as people who held a mini job at this establishment for the years 2000-2002 (2010-2012). Adjustment equals 1 if the workers' average monthly mini earnings at the establishment in the first post-reform year (Apr-Dec 2003 for the first reform and 2013 for the second) increase to between $Z + 12.5$ and $\tilde{Z} + 12.5$, where $\tilde{Z}$ is the new notch after the reform. Movers or exiters are thus classified as non-adjusters. We only show these figures for selected values of $\tilde{N}$ to keep figure sizes reasonable, but the figures portray a similar qualitative picture for other values of $\tilde{N}$.

Figure A.8: Bunching at Old Notch: Split By Any Mini Hires

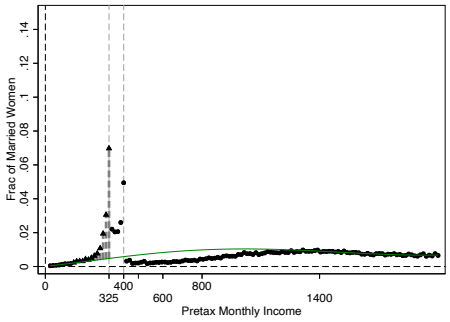
(a) 2002, Any Mini Hires = 0



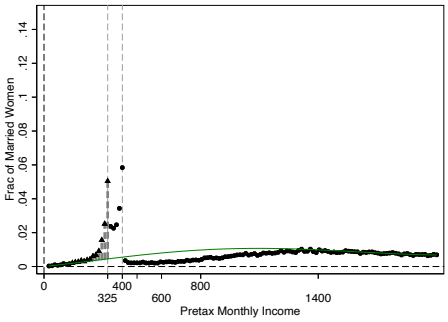
(b) 2002, Any Mini Hires = 1



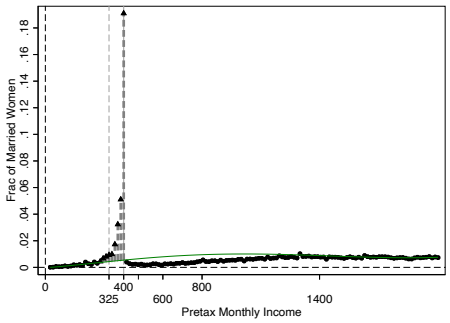
(c) 2003, Any Mini Hires = 0



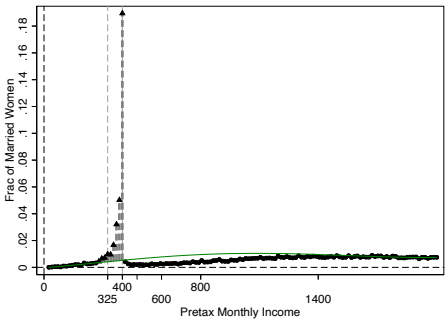
(d) 2003, Any Mini Hires = 1



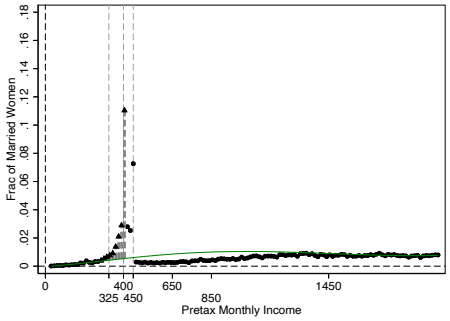
(e) 2012, Any Mini Hires = 0



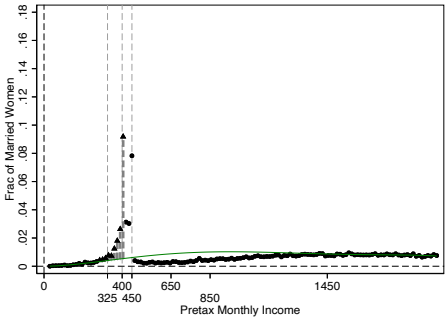
(f) 2012, Any Mini Hires = 1



(g) 2013, Any Mini Hires = 0



(h) 2013, Any Mini Hires = 1



Notes: These figures show the excess bunching mass at the old notch before and after both reforms for married women in establishments that have no mini hires post-reform (leftmost panels) propensity score matched to married women in establishments that have a mini hire post-reform (rightmost panels). We describe this process in Section 5. We estimate the period-by-period counter-factual density using the same procedure as in Appendix Figures A.5-A.6. This is depicted as a light green line, while the bunching mass at the old notch is shaded in dashed-gray.

A.2 Appendix Tables

Table A.1: Summary Statistics in 2012, based on Different Sample Restrictions

	Exclusive Mini jobbers	+ Age 26-55	+ Female & West German	+ Married*	+ Husband Anual Inc. €33,000 − 53,000
	(1)	(2)	(3)	(4)	(5)
			Panel A: Complementary Survey Data (SOEP)		
Average Monthly Wage	323.5	341.0	349.1	356.5	369.4
	[160.2]	[184.8]	[139.1]	[138.9]	[150.4]
Age in Years	44.10	41.27	42.88	44.36	42.60
	[17.44]	[9.272]	[8.426]	[7.200]	[6.819]
Education in Years	10.25	10.74	10.87	10.94	10.69
	[4.280]	[3.679]	[3.198]	[2.485]	[2.486]
Occ: Cleaner	0.129	0.149	0.181	0.178	0.196
Occ: Cashier/ Sales Clerk	0.116	0.109	0.127	0.132	0.130
Occ: Secretary	0.117	0.112	0.135	0.142	0.128
Occ: Nurse/Health Worker	0.0872	0.0915	0.112	0.114	0.144
Female	0.604	0.762	1	1	1
West German	0.836	0.832	1	1	1
Female & West German	0.536	0.689	1	1	1
Married	0.525	0.677	0.838	1	1
N Population (Estimate)	4227093	1589487	1095543	918061	374170
N Sample	2022	623	453	375	150
			Panel B: Administrative Data		
Average Monthly Wage	285.3	299.0	316.2	333.8	338.9
	[122.4]	[117.1]	[107.3]	[94.69]	[87.94]
Age in Years	45.02	41.90	42.47	43.65	43.46
	[17.80]	[8.592]	[8.089]	[6.552]	[6.390]
Education in Years	10.66	10.72	10.73	10.72	10.23
	[1.944]	[2.009]	[2.012]	[1.992]	[1.053]
Occ: Cleaner	0.156	0.191	0.234	0.243	0.252
Occ: Cashier/ Sales Clerk	0.144	0.148	0.165	0.189	0.201
Occ: Secretary	0.131	0.132	0.148	0.131	0.108
Occ: Nurse/Health Worker	0.0585	0.0796	0.102	0.116	0.114
Female	0.651	0.763	1	1	1
West German	0.851	0.857	1	1	1
Female & West German	0.565	0.673	1	1	1
Married (Imputed)	0.0887	0.143	0.194	1	1
Establishment Size	270.2	205.5	182.4	148.4	150.0
	[1058.5]	[794.4]	[696.7]	[521.2]	[477.2]
Establishment Mini Share	0.564	0.568	0.560	0.538	0.529
	[0.308]	[0.303]	[0.297]	[0.294]	[0.292]
Regional Unemp. Rate	4.724	4.688	4.649	4.646	4.514
	[2.178]	[2.187]	[2.181]	[2.200]	[2.155]
N Population (Estimate)	4914900	2266300	1524850	296350	125250
N Sample	98298	45326	30497	5927	2505

Notes: This table reports means (sd in brackets, omitted for binary variables) for demographic and labor market characteristics for the year 2012 in the SOEP (Goebel et al., 2019) and admin-data after consecutively applying the different sample restrictions used in our analysis. SOEP-data applies survey weights. Calculations for the admin data are based on a 2% random sample of the individual employment history (BeH, see Antoni et al. (2016) for a description). *Individuals in the SOEP are classified as married based on their response to a question on marital status, whereas in the administrative data only those identified as married by the method of Goldschmidt et al. (2017) are classified as such. Column (1) restricts to all workers in an exclusive mini job, Column (2) restricts to individuals aged 26-55, Column (3) additionally restricts to females living in West Germany, Column (4) further restricts to married individuals, and Column (5) to women whose husbands' gross annual earnings fall between €33,000 and 53,000. In the SOEP, husbands' gross earnings are calculated using household net income, subtracting the individuals' mini job income and applying the 2012 tax and social security schedule.

Table A.2: Adjustment on Labor Demand Indicators (no lags/leads)

	2003 Reform: Adjusted to 400 in 2003	2013 Reform: Adjusted to 450 in 2013
	Panel A: Adjustment on Any New Mini Hire	
	(1)	**(2)**
any mini hires 03(Apr-Dec)/13	0.066***	0.037***
	(0.004)	(0.005)
Num. of Individuals	220307	194877
Num. of Firms	74086	68409
Adj. R^2	0.114	0.091
Dep. Var. Mean	0.363	0.334
Mean Any Hires 03(Apr-Dec)/13	0.766	0.809
Oster's δ	-54.061	-9.422
Baseline Controls	X	X
Individual Controls	X	X
Firm Controls	X	X
	2003 Reform: Adjusted to 400 in 2003	2013 Reform: Adjusted to 450 in 2013
	Panel A: Adjustment on Non-mini Earnings Growth	
	(1)	**(2)**
Δ Ln total non-mini earnings 03-02/13-12	0.136***	0.126***
	(0.011)	(0.015)
Num. of Individuals	153979	137984
Num. of Firms	39913	37802
Adj. R^2	0.114	0.098
Dep. Var. Mean	0.322	0.309
Δ Ln total non-mini earnings 03-02/13-12	-0.042	0.019
Oster's δ	9.152	11.278
Baseline Controls	X	X
Individual Controls	X	X
Firm Controls	X	X

Notes: This table drops the lags and leads of our labor demand proxies from the regressions in Panel A columns (3) and (6) in Tables 2—3. The resulting coefficients are similar, but slightly larger. See notes to Tables 2—3 for additional details. Standard errors, clustered at the establishment level, are in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.3: Pooling Reforms

	Panel A: Any New Mini Hire				
	Pooled	Individ. FE Sample		Estab. FE Sample	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
any mini hires 01/11	-0.008*	0.011	0.024	-0.001	0.007
	(0.003)	(0.011)	(0.021)	(0.008)	(0.011)
any mini hires 02/12	-0.013***	0.005	0.008	-0.006	0.005
	(0.003)	(0.011)	(0.019)	(0.008)	(0.011)
any mini hires 03/13	0.041***	0.057***	0.077***	0.046***	0.069***
	(0.003)	(0.010)	(0.019)	(0.008)	(0.011)
any mini hires 04/14	0.019***	0.021*	0.026	0.022**	0.027*
	(0.003)	(0.010)	(0.019)	(0.008)	(0.011)
any mini hires 05/15	0.005	0.020*	0.041*	-0.005	0.014
	(0.003)	(0.010)	(0.018)	(0.007)	(0.010)
Num. of Obs.	415184	33212	33212	174831	174831
Num. of Individuals	398578	16606	16606	162177	162177
Num. of Establishments	124776	13346	13346	17719	17719
Adj. R^2	0.031	0.044	0.125	0.044	0.187
Dep. Var. Mean	0.349	0.448	0.448	0.333	0.333
Baseline Controls	X	X	X	X	X
Individual-FE			X		
Establishment-FE					X
	Panel B: Non-Mini Earnings Growth				
	Pooled	Individ. FE Sample		Estab. FE Sample	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Δ Ln non-mini wagebill 01-00/11-10	0.053***	0.122***	0.098*	0.087***	0.068***
	(0.009)	(0.030)	(0.045)	(0.016)	(0.020)
Δ Ln non-mini wagebill 02-01/12-11	0.055***	0.062	0.044	0.041*	-0.016
	(0.011)	(0.034)	(0.062)	(0.018)	(0.024)
Δ Ln non-mini wagebill 03-02/13-12	0.100***	0.135***	0.062	0.102***	0.057**
	(0.007)	(0.020)	(0.041)	(0.012)	(0.018)
Δ Ln non-mini wagebill 04-03/14-13	0.008	-0.002	-0.026	0.015	-0.011
	(0.006)	(0.016)	(0.027)	(0.012)	(0.016)
Δ Ln non-mini wagebill 05-04/15-14	0.015***	0.030**	0.020	0.035***	0.028*
	(0.004)	(0.010)	(0.015)	(0.009)	(0.011)
Num. of Obs.	292089	21570	21570	145298	145298
Num. of Individuals	281304	10785	10785	136653	136653
Num. of Establishments	65212	7617	7617	12574	12574
Adj. R^2	0.032	0.052	0.138	0.042	0.170
Dep. Var. Mean	0.316	0.416	0.416	0.310	0.310
Baseline Controls	X	X	X	X	X
Individual-FE			X		
Establishment-FE					X
	Panel C: Predicted Mini Hires				
	Pooled	Indiv. FE Sample		Estab. FE Sample	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Predicted Hires (OLS)	0.322***	0.437***	0.578***	0.311***	0.443***
	(0.012)	(0.038)	(0.069)	(0.023)	(0.033)
Num. of Obs.	384308	29942	29942	169346	169346
Num. of Individuals	369337	14971	14971	157503	157503
Num. of Establishments	108794	11495	11495	16706	16706
Adj. R^2	0.035	0.051	0.140	0.046	0.187
Dep. Var. Mean	0.346	0.445	0.445	0.330	0.330
Baseline Controls	X	X	X	X	X
Individual-FE			X		
Establishment-FE					X

Notes: This table presents effects of labor demand proxies on adjustment when pooling both reforms. It explores how adjustment varies with any new mini hire (panel A), with changes in the total non-mini wagebill (panel B), and with changes in the predicted fraction of new mini hires (panel C). Column (1) pools the two reform samples. Columns (2) and (3) restrict to individuals that are part of the baseline regression in both reforms, adding individual FE in column (3). Columns (4) and (5) restrict to establishments that are part of the baseline regression in both reforms and adds establishment FE in column (5). For comparability to 2013, any hires in 2003 is defined over the full year. All baseline control variables follow the same definitions for both reforms and can vary between the reform-years. Standard errors are clustered on the establishment level. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.4: Adjustment on Any New Mini Hire: Different Establishment Sizes

	Panel A: Heterogeneity by Number of Mini Jobbers					
	Adjusted to 400 in 2003			Adjusted to 450 in 2013		
	Mini Size:			Mini Size:		
	$1-5$	$6-25$	$26+$	$1-5$	$6-25$	$26+$
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
any mini hires 01/11	0.012*	-0.016*	-0.029	0.001	0.002	0.064
	(0.005)	(0.007)	(0.076)	(0.007)	(0.008)	(0.058)
any mini hires 02/12	0.008	0.012	-0.054	0.001	-0.005	0.082
	(0.006)	(0.007)	(0.050)	(0.008)	(0.008)	(0.044)
any mini hires 03(Jan-Mar)	-0.012	0.004	0.020			
	(0.007)	(0.005)	(0.011)			
any mini hires 03(Apr-Dec)/13	0.053***	0.054***	0.103***	0.024***	0.036***	0.063*
	(0.005)	(0.006)	(0.022)	(0.007)	(0.006)	(0.030)
any mini hires 04/14	0.021***	0.030***	0.038	0.003	0.016**	0.058**
	(0.005)	(0.006)	(0.023)	(0.007)	(0.006)	(0.023)
any mini hires 05/15	0.010	0.007	0.004	0.012	0.009	-0.019
	(0.005)	(0.006)	(0.017)	(0.007)	(0.006)	(0.017)
Num. of Individuals	48404	81727	90176	29665	70937	94275
Num. of Establishments	33758	30367	9961	22047	31447	14915
Adj. R^2	0.087	0.112	0.126	0.067	0.083	0.121
Dep. Var. Mean	0.415	0.401	0.301	0.368	0.356	0.306
Mean Any Hires 03(Apr-Dec)/13	0.389	0.756	0.979	0.352	0.763	0.988
Oster's δ	19.647	-67.293	13.084	-40.018	-14.783	3.366
Baseline Controls	X	X	X	X	X	X
Individual Controls	X	X	X	X	X	X
Firm Controls	X	X	X	X	X	X
	Panel B: Heterogeneity by Total Firm Size					
	Adjusted to 400 in 2003			Adjusted to 450 in 2013		
	Estab. Size:			Estab. Size:		
	$1-9$	$10-49$	$50+$	$1-9$	$10-49$	$50+$
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
any mini hires 01/11	0.006	-0.004	-0.007	-0.001	0.005	-0.003
	(0.008)	(0.006)	(0.010)	(0.009)	(0.007)	(0.013)
any mini hires 02/12	0.008	0.014*	-0.001	0.012	-0.006	0.003
	(0.009)	(0.007)	(0.010)	(0.011)	(0.008)	(0.012)
any mini hires 03(Jan-Mar)	-0.002	0.003	0.006			
	(0.010)	(0.006)	(0.006)			
any mini hires 03(Apr-Dec)/13	0.055***	0.058***	0.054***	0.048***	0.026***	0.039***
	(0.007)	(0.006)	(0.009)	(0.009)	(0.006)	(0.011)
any mini hires 04/14	0.025***	0.023***	0.034***	0.002	0.017**	0.027**
	(0.007)	(0.006)	(0.009)	(0.009)	(0.006)	(0.010)
any mini hires 05/15	-0.001	0.012*	0.014	0.007	0.010	-0.002
	(0.007)	(0.006)	(0.008)	(0.009)	(0.006)	(0.010)
Num. of Individuals	30005	69952	120350	20512	60345	114020
Num. of Establishments	20002	30897	23187	14426	29086	24897
Adj. R^2	0.089	0.102	0.113	0.072	0.082	0.106
Dep. Var. Mean	0.440	0.431	0.304	0.388	0.374	0.302
Mean Any Hires 03(Apr-Dec)/13	0.345	0.672	0.926	0.343	0.706	0.948
Oster's δ	17.479	-70.137	240.424	-31.905	-15.387	11.926
Baseline Controls	X	X	X	X	X	X
Individual Controls	X	X	X	X	X	X
Firm Controls	X	X	X	X	X	X

Notes: This table explores heterogeneity of the results in Table 2 by establishment size. Panel A groups establishments into 3 categories based the number of mini jobbers at the establishment in 2002 or 2012 (columns (1) and (4): 1 to 5 mini jobbers, columns (2) and (5): 6 to 25 mini jobbers, and columns (3) and (6): 26 or more mini jobbers). Panel B groups establishments into three categories based on the total number of employees at the establishment (columns (1) and (4): 1 to 9 employees, columns (2) and (5): 10 to 49 employees, and columns (3) and (6): 50 or more employees). Standard errors, clustered at the establishment level, are in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.5: Adjustment on Total Non-Mini Earnings Growth: Different Size Restrictions

	Panel A: 2003 Reform Adjusted to 400 in 2003				
	2002 Non-mini size restriction:				
	≥ 1	≥ 3	≥ 5	≥ 10	weighted
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Δ Ln total non-mini earnings 01-00	0.024**	0.028***	0.033***	0.025*	0.026*
	(0.007)	(0.008)	(0.010)	(0.012)	(0.011)
Δ Ln total non-mini earnings 02-01	0.024**	0.019*	0.021	0.023	0.013
	(0.008)	(0.010)	(0.011)	(0.014)	(0.014)
Δ Ln total non-mini earnings 03-02	0.056***	0.080***	0.097***	0.122***	0.095***
	(0.008)	(0.009)	(0.010)	(0.012)	(0.012)
Δ Ln total non-mini earnings 04-03	0.055***	0.049***	0.040***	0.031**	0.044***
	(0.008)	(0.009)	(0.010)	(0.012)	(0.013)
Δ Ln total non-mini earnings 05-04	0.028***	0.020**	0.021*	0.023*	0.012
	(0.007)	(0.008)	(0.008)	(0.010)	(0.011)
Num. of Individuals	213597	196937	180658	153979	213597
Num. of Establishments	71363	62197	53189	39913	71363
Adj. R^2	0.113	0.114	0.115	0.114	0.120
Dep. Var. Mean	0.353	0.345	0.337	0.322	0.353
Δ Ln total non-mini earnings 03-02/13-12	-0.038	-0.041	-0.042	-0.042	-0.038
Oster's δ	8.445	12.216	12.632	14.174	9.289
Baseline Controls	X	X	X	X	X
Individual Controls	X	X	X	X	X
Firm Controls	X	X	X	X	X
	Panel B: 2013 Reform Adjusted to 450 in 2013				
	2012 Non-mini size restriction:				
	≥ 1	≥ 3	≥ 5	≥ 10	weighted
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Δ Ln total non-mini earnings 11-10	0.024**	0.027*	0.038**	0.031*	0.019
	(0.009)	(0.011)	(0.013)	(0.016)	(0.014)
Δ Ln total non-mini earnings 12-11	0.048***	0.055***	0.046***	0.049**	0.052***
	(0.010)	(0.012)	(0.014)	(0.016)	(0.015)
Δ Ln total non-mini earnings 13-12	0.068***	0.087***	0.105***	0.117***	0.101***
	(0.010)	(0.011)	(0.013)	(0.015)	(0.017)
Δ Ln total non-mini earnings 14-13	0.039***	0.021***	0.007	-0.003	-0.017
	(0.010)	(0.012)	(0.014)	(0.016)	(0.017)
Δ Ln total non-mini earnings 15-14	0.033***	0.034**	0.031***	0.026**	0.037***
	(0.007)	(0.008)	(0.009)	(0.010)	(0.010)
Num. of Individuals	188627	173521	160762	137984	188627
Num. of Establishments	65151	56400	49266	37802	65151
Adj. R^2	0.092	0.094	0.095	0.099	0.118
Dep. Var. Mean	0.329	0.324	0.319	0.309	0.330
Δ Ln total non-mini earnings 03-02/13-12	0.024	0.020	0.020	0.019	0.025
Oster's δ	20.126	20.585	24.477	18.091	83.387
Baseline Controls	X	X	X	X	X
Individual Controls	X	X	X	X	X
Firm Controls	X	X	X	X	X

Notes: This table explores the robustness of results in Table 3 to alternative restrictions on initial (2002/2012) non-mini size. Columns (1)–(4) restrict to establishments with at least x non-mini workers in the pre-reform year. The sample in Column (4) ($\geq$ 10 non-mini workers) matches the one used in Table 3. Meanwhile, Column (5) keeps all establishments with at least one non-mini worker in the pre-reform year but weights the regression by the square root of the number of non-mini employees in that year. Panel A corresponds to the 2003 reform, while Panel B corresponds to the 2013 reform. Standard errors, clustered at the establishment level, are in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.6: Adjustment on Predicted Mini Hires: Different Establishment Sizes

	Panel A: Heterogeneity by Number of Mini Jobbers					
	Adjusted to 400 in 2003			Adjusted to 450 in 2013		
	Mini Size:			Mini Size:		
	$1-5$	$6-25$	$26+$	$1-5$	$6-25$	$26+$
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Predicted Frac. Hires (OLS)	0.413***	0.491***	0.585***	0.213***	0.283***	0.286***
	(0.019)	(0.022)	(0.034)	(0.028)	(0.029)	(0.051)
Prediction Frac. Hires (LASSO)	0.341***	0.393***	0.519***	0.222***	0.280***	0.271***
	(0.018)	(0.021)	(0.033)	(0.029)	(0.029)	(0.049)
Num. of Individuals	47098	79236	80772	27653	69180	80369
Num. of Establishments	32159	27011	7560	20111	27948	10711
Adj. R^2	0.099	0.118	0.135	0.073	0.087	0.129
Dep. Var. Mean	0.409	0.395	0.294	0.367	0.350	0.304
Baseline Controls	X	X	X	X	X	X
Individual Controls	X	X	X	X	X	X
Firm Controls	X	X	X	X	X	X
	Panel B: Heterogeneity by Total Estab. Size					
	Adjusted to 400 in 2003			Adjusted to 450 in 2013		
	Estab. Size:			Estab. Size:		
	$1-9$	$10-49$	$50+$	$1-9$	$10-49$	$50+$
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Predicted Frac. Hires (OLS)	0.438***	0.472***	0.510***	0.234***	0.277***	0.257***
	(0.027)	(0.022)	(0.023)	(0.040)	(0.029)	(0.035)
Predicted Frac. Hires (LASS0)	0.355***	0.381***	0.437***	0.235***	0.283***	0.243***
	(0.025)	(0.020)	(0.023)	(0.042)	(0.029)	(0.035)
Num. of Individuals	27796	70052	109258	17165	60099	99938
Num. of Establishments	18631	28655	19444	12103	26697	19970
Adj. R^2	0.096	0.110	0.117	0.073	0.085	0.113
Dep. Var. Mean	0.439	0.426	0.296	0.391	0.366	0.301
Baseline Controls	X	X	X	X	X	X
Individual Controls	X	X	X	X	X	X
Firm Controls	X	X	X	X	X	X

Notes: This table explores heterogeneity of the results in Table 4 by establishment size. Panel A groups establishments into 3 categories based the number of mini jobbers at the establishment in 2002 or 2012 (columns (1) and (4): 1 to 5 mini jobbers, columns (2) and (5): 6 to 25 mini jobbers, and columns (3) and (6): 26 or more mini jobbers). Panel B groups establishments into three categories based on the total number of employees at the establishment (columns (1) and (4): 1 to 9 employees, columns (2) and (5): 10 to 49 employees, and columns (3) and (6): 50 or more employees). The predicted frac. hires (OLS) and predicted frac. hires (LASSO) present results from separate regressions. Standard errors, clustered at the establishment level, are in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.7: 'Any Adjustment' at Establishment on Any Mini Hires

	2003 Reform: Adjusted to 400 in 2003			2013 Reform: Adjusted to 450 in 2013		
Panel A: Any Adjustment on Any New Mini Hire						
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
any mini hires 01/11	-0.041***	-0.023***	-0.020***	-0.035***	-0.004	-0.005
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
any mini hires 02/12	-0.033***	0.012*	0.006	-0.031***	0.014*	0.009
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
any mini hires 03(Jan-Mar)	-0.003	0.015**	0.013*			
	(0.006)	(0.006)	(0.006)			
any mini hires 03(Apr-Dec)/13	0.076***	0.081***	0.080***	0.033***	0.052***	0.050***
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
any mini hires 04/14	0.028***	0.036***	0.036***	0.002	0.015*	0.015*
	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)
any mini hires 05/15	0.015**	0.020***	0.019***	-0.003	0.011	0.010
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)
Num. of Observations	74086	74086	74086	68409	68409	68409
Num. of Establishments	74086	74086	74086	68409	68409	68409
Adj. R^2	0.145	0.180	0.204	0.119	0.155	0.174
Dep. Var. Mean	0.529	0.529	0.529	0.468	0.468	0.468
Mean Any Hires 03(Apr-Dec)/13	0.618	0.618	0.618	0.683	0.683	0.683
Oster's δ		-12.896	-26.902		-2.452	-3.520
Baseline Controls	X	X	X	X	X	X
Individual Controls			X			X
Establishment Controls		X	X		X	X
Panel B: Any Adjustment in Later Periods						
	in '03	in '04	in '05	in '13	in '14	in '15
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
any mini hires 01/11	-0.041***	-0.023**	-0.010	-0.035***	-0.029***	-0.037***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.008)	(0.009)
any mini hires 02/12	-0.033***	-0.019**	-0.016*	-0.031***	-0.017*	-0.047***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.008)	(0.009)
any mini hires 03(Jan-Mar)	-0.003	-0.017	-0.013			
	(0.006)	(0.009)	(0.008)			
any mini hires 03(Apr-Dec)/13	0.076***	0.019*	-0.030***	0.033***	-0.003	-0.028**
	(0.006)	(0.007)	(0.007)	(0.007)	(0.008)	(0.009)
any mini hires 04/14	0.028***	0.021**	0.006	0.002	0.031***	-0.007
	(0.006)	(0.007)	(0.007)	(0.007)	(0.008)	(0.009)
any mini hires 05/15	0.015**	0.013	0.031***	-0.003	0.019*	0.040***
	(0.005)	(0.007)	(0.007)	(0.006)	(0.008)	(0.009)
Num. of Observations	74086	34919	28018	68409	36367	28113
Num. of Establishments	74086	34919	28018	68409	36367	28113
Adj. R^2	0.145	0.057	0.038	0.119	0.061	0.061
Dep. Var. Mean	0.529	0.198	0.104	0.468	0.227	0.222
Mean Any Hires 03(Apr-Dec)/13	0.618	0.561	0.551	0.683	0.650	0.637
Baseline Controls	X	X	X	X	X	X

Notes: This table presents the establishment-level analog of Table 2 where the dependent variable is replaced with a binary indicator for "Any Adjustment" at the establishment (among the long-term mini jobbers at the notch in Table 2). In columns 3 and 6, the individual level controls are now replaced by their establishment level means. Regressions are weighted by the number of long-term mini jobbers at the establishment. See notes to Table 2 for additional details. Standard errors, clustered at the establishment level, are in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.8: 'Any Adjustment' at Establishment on Nonmini Earnings Growth

	2003 Reform: Adjusted to 400 in 2003			2013 Reform: Adjusted to 450 in 2013		
Panel A: Adjustment on Non-Mini Wagebill Growth						
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Δ Ln total non-mini earnings 01-00/11-10	0.006	-0.026	-0.030	0.026	0.029	0.022
	(0.015)	(0.016)	(0.016)	(0.025)	(0.026)	(0.025)
Δ Ln total non-mini earnings 02-01/12-11	0.003	-0.020	-0.031	0.052	0.032	0.025
	(0.018)	(0.018)	(0.018)	(0.027)	(0.025)	(0.025)
Δ Ln total non-mini earnings 03-02/13-12	0.140***	0.133***	0.132***	0.157***	0.150***	0.142***
	(0.023)	(0.024)	(0.024)	(0.032)	(0.031)	(0.032)
Δ Ln total non-mini earnings 04-03/14-13	0.030	0.032	0.032	-0.028	-0.035	-0.037
	(0.022)	(0.023)	(0.023)	(0.026)	(0.026)	(0.027)
Δ Ln total non-mini earnings 05-04/15-14	0.040*	0.031	0.029	0.060***	0.050***	0.048**
	(0.017)	(0.017)	(0.017)	(0.018)	(0.015)	(0.015)
Num. of Observations	39913	39913	39913	37802	37802	37802
Num. of Establishments	39913	39913	39913	37802	37802	37802
Adj. R^2	0.174	0.211	0.233	0.138	0.187	0.209
Dep. Var. Mean	0.517	0.517	0.517	0.478	0.478	0.478
Δ Ln total non-mini earnings 03-02/13-12	-0.024	-0.024	-0.024	0.020	0.020	0.020
Oster's δ		9.712	11.240		15.387	9.547
Baseline Controls	X	X	X	X	X	X
Individual Controls			X			X
Establishments Controls		X	X		X	X
Panel B: Adjustment in Later Periods						
	in '03	in '04	in '05	in '13	in '14	in '15
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Δ Ln total non-mini earnings 01-00/11-10	0.006	0.036	-0.029	0.026	-0.038	-0.153***
	(0.015)	(0.028)	(0.025)	(0.025)	(0.037)	(0.038)
Δ Ln total non-mini earnings 02-01/12-11	0.003	-0.059	-0.004	0.052	0.057	-0.050
	(0.018)	(0.031)	(0.028)	(0.027)	(0.039)	(0.042)
Δ Ln total non-mini earnings 03-02/13-12	0.140***	0.150***	0.054*	0.157***	0.123**	0.139**
	(0.023)	(0.030)	(0.026)	(0.032)	(0.042)	(0.043)
Δ Ln total non-mini earnings 04-03/14-13	0.030	0.164***	0.045	-0.028	0.172***	0.118***
	(0.022)	(0.033)	(0.023)	(0.026)	(0.037)	(0.034)
Δ Ln total non-mini earnings 05-04/15-14	0.040*	-0.019	0.091***	0.060***	-0.011	0.088***
	(0.017)	(0.026)	(0.022)	(0.018)	(0.031)	(0.026)
Num. of Individuals	39913	19267	15598	37802	19730	14910
Num. of Establishments	39913	19267	15598	37802	19730	14910
Adj. R^2	0.174	0.080	0.050	0.138	0.071	0.078
Dep. Var. Mean	0.517	0.190	0.100	0.478	0.244	0.216
Mean Δ Ln total non-mini earnings 03-02/13-12	-0.024	-0.028	-0.030	0.020	0.014	0.012
Establishment Controls	X	X	X	X	X	X

Notes: This table presents the establishment-level analog of Table 3 where the dependent variable is replaced with a binary indicator for "Any Adjustment" at the establishment (among the long-term mini jobbers at the notch in Table 3). In columns 3 and 6, the individual level controls are now replaced by their establishment level means. Regressions are weighted by the number of long-term mini jobbers at the establishment. See notes to Table 3 for additional details. Standard errors, clustered at the establishment level, are in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.9: 'Any Adjustment' at Establishment on Predicted Mini Hires

	2003 Reform: Adjusted to 400 in 2003			2013 Reform: Adjusted to 450 in 2013		
Panel A: Adjustment on Non-Mini Wagebill Growth						
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Predicted Hires (OLS)	0.335***	0.364***	0.465***	-0.032	0.050	0.167***
	(0.024)	(0.024)	(0.026)	(0.029)	(0.028)	(0.029)
Num. of Observations	66730	66730	66730	58770	58770	58770
Num. of Establishments	66730	66730	66730	58770	58770	58770
Adj. R-Squared	0.147	0.188	0.208	0.119	0.153	0.174
Dep. Var. Mean	0.710	0.710	0.710	0.648	0.648	0.648
Baseline Controls	X	X	X	X	X	X
Individual Controls		X	X		X	X
Establishment Controls			X			X

Notes: This table presents the establishment-level analog of Table 4 where the dependent variable is replaced with a binary indicator for "Any Adjustment" at the establishment (among the long-term mini jobbers at the notch in Table 4). In columns 3 and 6, the individual level controls are now replaced by their establishment level means. Regressions are weighted by the number of long-term mini jobbers at the establishment. See notes to Table 4 for additional details. Standard errors, clustered at the establishment level, are in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.10: Adjustment with Hours Changes (2013 Reform)

	2013 Reform: Adjusted to 450 in 2013					
	Δhrs > 0 all	Δhrs > 1 all	Δhrs > 2 all	Δhrs > 0 ≥ 10mini	Δhrs > 1 ≥ 10mini	Δhrs > 2 ≥ 10mini
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	Panel A: Any Mini Hires					
any mini hires 11	0.003	0.004	0.004	0.017	0.019	0.016
	(0.005)	(0.004)	(0.004)	(0.016)	(0.014)	(0.014)
any mini hires 12	-0.007	-0.006	-0.006	-0.019	-0.018	-0.012
	(0.005)	(0.004)	(0.004)	(0.015)	(0.013)	(0.013)
any mini hires 13	0.023***	0.016***	0.015***	0.046***	0.043***	0.040***
	(0.004)	(0.004)	(0.003)	(0.010)	(0.010)	(0.009)
any mini hires 14	0.011**	0.010**	0.009*	0.012	0.009	0.004
	(0.004)	(0.004)	(0.004)	(0.010)	(0.009)	(0.009)
any mini hires 15	-0.003	-0.002	-0.000	-0.004	-0.005	-0.002
	(0.004)	(0.004)	(0.004)	(0.009)	(0.008)	(0.008)
Num. of Individuals	158678	158678	158678	101501	101501	101501
Num. of Establishments	58521	58521	58521	21825	21825	21825
Adj. R^2	0.040	0.041	0.040	0.058	0.057	0.055
Dep. Var. Mean	0.181	0.147	0.131	0.192	0.162	0.145
	Panel B: Non Mini Earnings Growth					
Δ Ln total non-mini earnings 11-10	0.016	0.014	0.009	0.028	0.021	0.019
	(0.014)	(0.013)	(0.013)	(0.017)	(0.016)	(0.015)
Δ Ln total non-mini earnings 12-11	0.012	0.013	0.019	0.011	0.018	0.021
	(0.016)	(0.015)	(0.014)	(0.019)	(0.018)	(0.017)
Δ Ln total non-mini earnings 13-12	0.066***	0.049***	0.038**	0.069***	0.053***	0.042**
	(0.014)	(0.014)	(0.014)	(0.016)	(0.016)	(0.015)
Δ Ln total non-mini earnings 14-13	-0.008	-0.009	-0.010	-0.009	-0.012	-0.012
	(0.013)	(0.013)	(0.013)	(0.015)	(0.015)	(0.014)
Δ Ln total non-mini earnings 15-14	0.028**	0.025*	0.023*	0.029*	0.028*	0.025*
	(0.010)	(0.010)	(0.010)	(0.012)	(0.012)	(0.011)
Num. of Individuals	111189	111189	111189	88022	88022	88022
Num. of Establishments	32076	32076	32076	17622	17622	17622
Adj. R^2	0.049	0.049	0.048	0.061	0.059	0.057
Dep. Var. Mean	0.179	0.149	0.132	0.186	0.158	0.141
	Panel C: Predicted Mini Hires					
Predicted Hires (OLS)	0.081***	0.077***	0.060***	0.143***	0.129***	0.095**
	(0.019)	(0.017)	(0.017)	(0.034)	(0.032)	(0.031)
Num. of Individuals	144193	144193	144193	96499	96499	96499
Num. of Establishments	50417	50417	50417	20172	20172	20172
Adj. R^2	0.041	0.044	0.042	0.059	0.058	0.056
Dep. Var. Mean	0.182	0.148	0.132	0.192	0.163	0.145
Baseline Controls	X	X	X	X	X	X
Individual Controls	X	X	X	X	X	X
Establishment Controls	X	X	X	X	X	X

Notes: This table reexamines the effects of our labor demand proxies on adjustment in the 2013 reform (see Tables 2–4) redefining the outcome variable. Here adjustment is a binary variable that equals one if and only if we observe both an earnings adjustment (as before) *and* a recorded change in monthly hours (see Figure 5). In column (1) we require a positive hours change, in column (2) the observed change in hours needs to be $>$ 1 hour per month, and in column (3) it needs to be $>$ than 2 hours per month. Columns (4)-(6) repeat the exercise in columns (1)-(3) for establishments with at least 10 mini jobbers. Panel A examines the effects of any mini hires on these new definitions of adjustment, Panel B examines the effects of non-mini earnings growth, and Panel C examines the effects of predicted mini hires. Standard errors, clustered at the establishment level, are in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.11: Robustness to Dropping Tenure Restriction

	2003 Reform: Adjusted to 400 in 2003			2013 Reform: Adjusted to 450 in 2013		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Any New Mini Hires					
any mini hires 01/11	-0.029***	-0.016***	-0.015***	-0.026***	-0.008	-0.005
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
any mini hires 02/12	-0.012**	0.003	0.006	-0.019***	-0.004	-0.001
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
any mini hires 03(Jan-Mar)	-0.002	0.004	0.005			
	(0.004)	(0.004)	(0.004)			
any mini hires 03(Apr-Dec)/13	0.051***	0.054***	0.055***	0.026***	0.032***	0.033***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)
any mini hires 04/14	0.020***	0.024***	0.025***	0.008	0.013**	0.015***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)
any mini hires 05/15	0.006	0.009*	0.009*	-0.003	0.002	0.004
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)
Num. of Individuals	274596	274596	274596	231530	231530	231530
Num. of Establishments	82110	82110	82110	74128	74128	74128
Adj. R^2	0.043	0.109	0.114	0.025	0.083	0.093
Dep. Var. Mean	0.347	0.347	0.347	0.318	0.318	0.318
Mean Any Hires 03(Apr-Dec)/13	0.781	0.781	0.781	0.822	0.822	0.822
Oster's δ		-31.101	-34.392		-11.632	-12.212
	Panel B: Non-Mini Wagebill Growth					
Δ Ln total non-mini earnings 01-00/11-10	0.028**	0.027**	0.016	0.013	0.025	0.021
	(0.010)	(0.010)	(0.010)	(0.013)	(0.013)	(0.013)
Δ Ln total non-mini earnings 02-01/12-11	0.047***	0.028*	0.025*	0.061***	0.042**	0.035*
	(0.013)	(0.012)	(0.012)	(0.016)	(0.015)	(0.015)
Δ Ln total non-mini earnings 03-02/13-12	0.130***	0.116***	0.117***	0.121***	0.118***	0.113***
	(0.011)	(0.011)	(0.011)	(0.015)	(0.015)	(0.014)
Δ Ln total non-mini earnings 04-03/14-13	0.038***	0.034**	0.026*	0.007	0.001	-0.004
	(0.011)	(0.011)	(0.011)	(0.015)	(0.014)	(0.014)
Δ Ln total non-mini earnings 05-04/15-14	0.031***	0.026**	0.021*	0.038***	0.035***	0.026**
	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.009)
Num. of Individuals	195615	195615	195615	167963	167963	167963
Num. of Establishments	44365	44365	44365	41397	41397	41397
Adj. R^2	0.042	0.105	0.113	0.028	0.087	0.100
Dep. Var. Mean	0.307	0.307	0.307	0.294	0.294	0.294
Δ Ln total non-mini earnings 03-02/13-12	-0.042	-0.042	-0.042	0.019	0.019	0.019
Oster's δ		15.162	12.606		80.079	19.083
	Panel C: Predicted Mini Hires					
Predicted Hires (OLS)	0.367***	0.417***	0.466***	0.124***	0.224***	0.254***
	(0.013)	(0.012)	(0.013)	(0.019)	(0.019)	(0.019)
Num. of Individuals	249788	249788	249788	204786	204786	204786
Num. of Establishments	66730	66730	66730	58770	58770	58770
Adj. R^2	0.049	0.116	0.122	0.027	0.085	0.097
Dep. Var. Mean	0.344	0.344	0.344	0.319	0.319	0.319
Oster's δ		-14.956	-35.938		-4.868	-5.679
Baseline Controls	X	X	X	X	X	X
Individual Controls		X	X		X	X
Establishment Controls			X			X

Notes: This table re-estimates the specifications in Table 2 Panel A, Table 3 Panel A, and Table 4 Panel A without restricting to mini jobbers with tenure at the firm in 2000-2002 (2010-2012). Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.12: Robustness to Alternative Def. of Adjustment

	2003 Reform: Adjusted to 400 in 2003			2013 Reform: Adjusted to 450 in 2013		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Any New Mini Hires					
any mini hires 01/11	-0.008	-0.003	-0.003	-0.007	-0.001	0.001
	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
any mini hires 02/12	-0.005	0.005	0.007	-0.015**	-0.004	-0.002
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
any mini hires 03(Jan-Mar)	0.001	0.004	0.004			
	(0.004)	(0.004)	(0.004)			
any mini hires 03(Apr-Dec)/13	0.057***	0.058***	0.058***	0.027***	0.032***	0.033***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
any mini hires 04/14	0.022***	0.024***	0.025***	0.009	0.013**	0.015**
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
any mini hires 05/15	0.003	0.005	0.005	-0.002	0.001	0.004
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Num. of Individuals	220307	220307	220307	194877	194877	194877
Num. of Establishments	74086	74086	74086	68409	68409	68409
Adj. R^2	0.040	0.103	0.109	0.023	0.077	0.087
Dep. Var. Mean	0.378	0.378	0.378	0.342	0.342	0.342
Mean Any Hires 03(Apr-Dec)/13	0.766	0.766	0.766	0.809	0.809	0.809
Oster's δ		-74.164	-198.905		-17.495	-17.904
	Panel B: Non-Mini Wagebill Growth					
Δ Ln total non-mini earnings 01-00/11-10	0.059***	0.043***	0.025*	0.055***	0.043**	0.036*
	(0.012)	(0.012)	(0.012)	(0.016)	(0.016)	(0.016)
Δ Ln total non-mini earnings 02-01/12-11	0.056***	0.031*	0.026	0.069***	0.053**	0.046**
	(0.015)	(0.014)	(0.014)	(0.018)	(0.017)	(0.017)
Δ Ln total non-mini earnings 03-02/13-12	0.087***	0.074***	0.076***	0.054**	0.051**	0.046**
	(0.012)	(0.012)	(0.012)	(0.018)	(0.016)	(0.016)
Δ Ln total non-mini earnings 04-03/14-13	0.035**	0.030*	0.025*	0.016	0.010	0.006
	(0.012)	(0.012)	(0.012)	(0.018)	(0.017)	(0.017)
Δ Ln total non-mini earnings 05-04/15-14	0.033***	0.027**	0.022*	0.037***	0.036***	0.027**
	(0.010)	(0.009)	(0.009)	(0.011)	(0.011)	(0.010)
Num. of Individuals	153979	153979	153979	137984	137984	137984
Num. of Establishments	39913	39913	39913	37802	37802	37802
Adj. R^2	0.038	0.099	0.107	0.025	0.082	0.094
Dep. Var. Mean	0.340	0.340	0.340	0.317	0.317	0.317
Δ Ln Ln total non-mini earnings 03-02/13-12	-0.042	-0.042	-0.042	0.019	0.019	0.019
Oster's δ		10.722	10.590		30.957	10.474
	Panel C: Predicted Mini Hires					
Predicted Hires (OLS)	0.361***	0.309***	0.396***	0.115***	0.171***	0.192***
	(0.014)	(0.013)	(0.014)	(0.020)	(0.020)	(0.020)
Num. of Individuals	207106	207106	207106	177202	177202	177202
Num. of Establishments	66730	66730	66730	58770	58770	58770
Adj. R^2	0.044	0.096	0.113	0.024	0.079	0.090
Dep. Var. Mean	0.374	0.374	0.374	0.340	0.340	0.340
Oster's δ		-3.751	-3.868		-7.050	-8.796
Baseline Controls	X	X	X	X	X	X
Individual Controls		X	X		X	X
Establishment Controls			X			X

Notes: This table re-estimates the specifications in Table 2 Panel A, Table 3 Panel A, and Table 4 Panel A where the binary dependent variable is redefined such that it equals 1 if the individual increases total mini job earnings to between Z+12.5 and $\tilde{Z}$+12.5, where Z and $\tilde{Z}$ are the new and old mini job thresholds, *regardless* of where the worker is employed. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.13: Alternative Measures of Firm Labor Demand

	2003 Reform: Adjusted to 400 in 2003			2013 Reform: Adjusted to 450 in 2013		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Any New Mini Hires					
Emp. growth non-mini 01-00/11-10	0.048***	0.036***	0.020	0.042**	0.032*	0.028*
	(0.012)	(0.011)	(0.011)	(0.014)	(0.014)	(0.014)
Emp. growth non-mini 02-01/12-11	0.044**	0.023	0.016	0.058***	0.046**	0.047**
	(0.014)	(0.014)	(0.014)	(0.017)	(0.017)	(0.016)
Emp. growth non-mini 03-02/13-12	0.130***	0.106***	0.113***	0.088***	0.083***	0.084***
	(0.012)	(0.012)	(0.012)	(0.017)	(0.016)	(0.015)
Emp. growth non-mini 04-03/14-13	0.046**	0.047***	0.038**	0.021	0.013	0.006
	(0.014)	(0.014)	(0.014)	(0.017)	(0.017)	(0.016)
Emp. growth non-mini 05-04/15-14	0.044***	0.036***	0.030**	0.041**	0.038**	0.029*
	(0.011)	(0.011)	(0.011)	(0.013)	(0.013)	(0.012)
Num. of Individuals	153979	153979	153979	137984	137984	137984
Num. of Establishments	39913	39913	39913	37802	37802	37802
Adj. R^2	0.042	0.106	0.114	0.027	0.085	0.098
Dep. Var. Mean	0.322	0.322	0.322	0.309	0.309	0.309
Mean Emp. growth non-mini 03-02/13-12	-0.042	-0.084	-0.084	0.019	0.019	0.007
Oster's δ		8.269	6.004		34.320	18.905
Baseline Controls	X	X	X	X	X	X
Individual Controls		X	X		X	X
Firm Controls			X			X
	Panel B: Non-Mini Wagebill Growth					
Emp. growth total 01-00/11-10	0.041***	0.022	0.011	0.011	-0.004	-0.004
	(0.012)	(0.011)	(0.012)	(0.017)	(0.016)	(0.016)
Emp. growth total 02-01/12-11	0.059***	0.013	0.016	0.075***	0.043*	0.039*
	(0.016)	(0.015)	(0.015)	(0.020)	(0.020)	(0.019)
Emp. growth total 03-02/13-12	0.276***	0.263***	0.257***	0.185***	0.177***	0.176***
	(0.013)	(0.013)	(0.013)	(0.017)	(0.017)	(0.017)
Emp. growth total 04-03/14-13	-0.042***	-0.028**	-0.032**	0.009	0.005	0.003
	(0.011)	(0.010)	(0.010)	(0.019)	(0.018)	(0.018)
Emp. growth total 05-04/15-14	0.048***	0.038***	0.028*	0.035*	0.038**	0.032*
	(0.012)	(0.011)	(0.011)	(0.014)	(0.014)	(0.013)
Num. of Individuals	153979	153979	153979	137984	137984	137984
Num. of Establishments	39913	39913	39913	37802	37802	37802
Adj. R^2	0.048	0.111	0.119	0.028	0.086	0.099
Dep. Var. Mean	0.322	0.322	0.322	0.309	0.309	0.309
Mean Emp. growth non-mini 03-02/13-12	-0.013	-0.013	-0.013	-0.019	-0.019	-0.019
Oster's δ		31.482	17.840		43.276	21.704
Baseline Controls	X	X	X	X	X	X
Individual Controls		X	X		X	X
Firm Controls			X			X

Notes: This table re-estimates the specifications in Table 3 Panel B, changing the independent variable to be total non-mini employment growth in Panel A and total employment growth in Panel B. These year-on-year growth measures are defined as the duration weighted change in the number of employees between year t and $t-1$, relative to the stock of employment in $t-1$, and winsorized at -1 and 1. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.14: Industry Growth

	All	Mini	Non-Mini
	(1)	(2)	(3)
	Panel A: 2003 Reform		
Industry Growth	0.098*	0.045**	0.097**
	(0.040)	(0.016)	(0.033)
Num. of Individuals	220260	220260	220260
Num. of Establishments	74062	74062	74062
Num. of 5-Digit Industries	952	952	952
Adj. R^2	0.107	0.107	0.107
Dep. Var. Mean	0.363	0.363	0.363
Indep. Var. SD	0.070	0.190	0.094
Baseline Controls	X	X	X
Individual Controls	X	X	X
	Panel B: 2013 Reform		
Industry Growth	0.057	0.056	0.044
	(0.063)	(0.052)	(0.055)
Num. of Individuals	194830	194830	194830
Num. of Establishments	68393	68393	68393
Num. of 5-Digit Industries	757	757	757
Adj. R^2	0.079	0.079	0.079
Dep. Var. Mean	0.334	0.334	0.334
Indep. Var. SD	0.059	0.063	0.068
Baseline Controls	X	X	X
Individual Controls	X	X	X

Notes: This table reports the association between 5-digit, leave-out county (*kreis*) industry growth and individual adjustment. Industry growth rates are calculated between year 2002 and 2004 in Panel A and between 2012 and 2014 in Panel B. Column (1) reports the association between adjustment and industry growth defined as total employment growth in the given 5 digit industry. Column (2) does so for total mini jobber growth in the given industry, and column (3) does so for total non-mini jobber growth. Growth rates are winsorized at the 1st and 99th percentiles. Standard errors are clustered at the 5-digit industry level. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.15: Robustness to Adding County (*kreis*) FE

	2003 Reform: Adjusted to 400 in 2003			2013 Reform: Adjusted to 450 in 2013		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Any New Mini Hires					
any mini hires 01/11	-0.009	-0.003	-0.002	-0.007	-0.001	0.001
	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
any mini hires 02/12	-0.007	0.004	0.007	-0.017***	-0.005	-0.004
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
any mini hires 03(Jan-Mar)	0.001	0.004	0.004			
	(0.004)	(0.004)	(0.004)			
any mini hires 03(Apr-Dec)/13	0.056***	0.057***	0.058***	0.029***	0.033***	0.033***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
any mini hires 04/14	0.022***	0.024***	0.025***	0.010*	0.013**	0.014**
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
any mini hires 05/15	0.008	0.009*	0.010*	-0.000	0.003	0.006
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Num. of Individuals	220307	220307	220307	194877	194877	194877
Num. of Establishments	74086	74086	74086	68409	68409	68409
Adj. R^2	0.046	0.112	0.117	0.029	0.085	0.094
Dep. Var. Mean	0.363	0.363	0.363	0.334	0.334	0.334
Mean Any Hires 03(Apr-Dec)/13	0.766	0.766	0.766	0.809	0.809	0.809
Oster's δ		-60.469	-84.753		-17.053	-19.833
	Panel B: Non-Mini Wagebill Growth					
Δ Ln total non-mini earnings 01-00/11-10	0.053***	0.039***	0.026*	0.047**	0.036*	0.034*
	(0.012)	(0.011)	(0.011)	(0.016)	(0.015)	(0.015)
Δ Ln total non-mini earnings 02-01/12-11	0.047**	0.025	0.024	0.064***	0.049**	0.044**
	(0.014)	(0.014)	(0.013)	(0.017)	(0.017)	(0.016)
Δ Ln total non-mini earnings 03-02/13-12	0.132***	0.119***	0.123***	0.125***	0.122***	0.119***
	(0.012)	(0.012)	(0.012)	(0.015)	(0.014)	(0.014)
Δ Ln total non-mini earnings 04-03/14-13	0.037**	0.034**	0.029*	0.006	0.001	-0.003
	(0.012)	(0.012)	(0.012)	(0.016)	(0.016)	(0.015)
Δ Ln total non-mini earnings 05-04/15-14	0.033***	0.028**	0.024*	0.035**	0.033**	0.026**
	(0.010)	(0.009)	(0.009)	(0.011)	(0.010)	(0.010)
Num. of Individuals	153979	153979	153979	137984	137984	137984
Num. of Establishments	39913	39913	39913	37802	37802	37802
Adj. R^2	0.048	0.110	0.118	0.036	0.093	0.104
Dep. Var. Mean	0.322	0.322	0.322	0.309	0.309	0.309
Δ Ln total non-mini earnings 03-02/13-12	-0.042	-0.042	-0.042	0.019	0.019	0.019
Oster's δ		15.079	15.599		72.602	20.665
	Panel C: Predicted Mini Hires					
Predicted Hires (OLS)	0.414***	0.440***	0.482***	0.185***	0.239***	0.257***
	(0.013)	(0.013)	(0.014)	(0.020)	(0.019)	(0.020)
Num. of Individuals	207106	207106	207106	177202	177202	177202
Num. of Establishments	66730	66730	66730	58770	58770	58770
Adj. R^2	0.052	0.118	0.124	0.031	0.088	0.098
Dep. Var. Mean	0.359	0.359	0.359	0.332	0.332	0.332
Oster's δ		-39.486	59.512		-8.156	-12.016
Baseline Controls + County FE	X	X	X	X	X	X
Individual Controls		X	X		X	X
Establishment Controls			X			X

Notes: This table re-estimates the specifications in Table 2 Panel A, Table 3 Panel A, and Table 4 Panel A including county (*kreis*) fixed effects in all columns. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

B Elasticity Estimation Appendix

This self-contained appendix uses bunching techniques to tease out the implications of the delayed adjustment observed in Figures 2–3 on the elasticity of earnings with respect to the net of tax rate. Section B.1 compares the estimated elasticity in the pre-reform years to the elasticity obtained using only the mass that responds to each reform. Section B.2 uses the method developed by Gelber et al. (2020) to quantify the fixed adjustment cost that rationalizes the aggregate bunching responses in Figures 2–3. This exercise provides estimates of how large adjustment costs for married women are in aggregate, without specifying particular sources of the adjustment cost. We provide these estimates for ease of comparison to the few others in the literature. Finally Section B.3 contains all the details relevant to how we construct the budget sets for the preceding calculations.

B.1 Elasticity Estimation

In section 4.2 we fit counter-factual earnings distributions to the data in Figures 2–3 in order to calculate the share of initial excess mass at the old notch that remains in the years after each reform. These results were plotted in Figure 6. Counter-factual fits are plotted in Appendix Figures A.5–A.6. Here, we quantify the implications of these measures of excess mass for elasticities using bunching techniques (see Kleven and Waseem, 2013). These techniques require making several additional assumptions, like specifying a quasi-linear utility function.[1]

Quantifying Bunching at the Old Notch. Following, among others, Gelber et al. (2020); Kleven and Waseem (2013); Chetty et al. (2011), we assume workers have utility given by the following iso-elastic, quasi linear function

$$u = z - T(z) - \frac{n}{1+1/e}\left(\frac{z}{n}\right)^{1+1/e}$$

where z denotes pre-tax earnings, $T(z)$ is tax liability, and n is an ability parameter that is smoothly distributed according to cumulative density function $F(n)$. This utility function, commonly used in the literature, rules out income effects and ensures that the utility of moving to the notch is decreasing with ability n.[2] e is the elasticity of earnings with respect to the net of tax rate.

In the absence of taxes workers optimize by setting earnings equal to their ability level ($z = n$) and hence the smooth ability distribution translates into a smooth earnings distribution. We recover this 'counter-factual' earnings distribution from the data using the standard approach of fitting a polynomial to the earnings distributions. Following Kleven (2016), we fit a 5th degree polynomial, excluding bins to the left of the notch (up to z_l), chosen visually, and bins to the right of the notch (up to z_u). z_u is chosen using an iterative process that sets the bunching mass equal to the missing mass.

We note that our budget set has both a pure tax notch and a relatively large change in slope after the notch (in contrast with the seminal example in Kleven (2016) which has a small slope change). The intensive margin responses arising purely from this change in slope would shift the density down relative to the counter-factual. This shift is ignored in our estimation, but, in practice, our counter-factual turns out to be relatively flat, making shifts less relevant.

Appendix Figures A.5–A.6 show the estimated counter-factuals for each year after the reform. We estimate counter-factual densities period by period, keeping the lower omission threshold z_l fixed over time. We shade the excess mass at the *old notch* after each of the two reforms in gray.[3]

[1] They are also subject to the Blomquist and Newey (2017) critique regarding the potential arbitrariness of the counter-factual fit. It helps that we observe the observed earnings distribution in 2012, well after workers have adjusted to the first reform. This allows us to see what the actual earnings distribution around €300 might have looked like absent the notch in earlier periods.

[2] Income effects are potentially secondary in our context given that earnings are a relatively small part of household earnings.

[3] One challenge arises from the fact that the old threshold is relatively close to the new threshold, such that imprecision in

Figure B.1 panel (a) plots this excess mass at the old notch following each reform over time. We normalize the excess mass by the average counter-factual density from z_l to the old notch. Consistent with Figures 2–3, we note that the excess mass dissipates over time. The excess mass at the old notch following the 2013 reform is higher than that in 2003, consistent with lower potential gains from adjusting upwards by €50 instead of €75.

Implications for Elasticities. The amount of bunching at the notch prior to each reform can be used to recover the long-term, structural elasticity parameter in our utility function. Assuming, by this point, that all short-term frictions have dissipated, the Kleven and Waseem (2013) bunching approach recovers this elasticity. Since the utility of relocating to the notch point is strictly decreasing in ability (n), there is a marginal buncher with ability $\hat{n}$ who is indifferent between relocating to the notch and staying at her optimal earnings above the notch. Her indifference conditions are given by setting the utility she gets at the notch point equal to the utility she would get at her optimal earnings level above the notch:

$$\hat{n}(1-\Delta t)^e - \Delta T - \Delta t(\hat{n}(1-\Delta t)^e - z^*) - \frac{\hat{n}}{1+1/e}(1-\Delta t)^{(1+e)} - z^* + \frac{\hat{n}}{1+1/e}(\frac{z^*}{\hat{n}})^{(1+1/e)} = 0 \quad \text{(B.1)}$$

Since the bunching mass estimated in the data gives us $\hat{n}$, the best way to read this equation is as one that relates the unknown elasticity parameter e to known (ΔT and Δt, i.e. the change in level and slope of the budget set) and estimated ($\hat{n}$) quantities, allowing us to solve for e numerically.

The budget set is constructed as in Tazhitdinova (2020) and described in detail in Section B.3. In order to take into account firm SSC rates (that also change discontinuously at the threshold) we calculate the size of the notch in terms of gross earnings (earnings + employer SCC + employee SSC), assuming full pass through to the worker. Crossing the notch point (in terms of gross earnings) subjects workers to taxation which depends on the amount of SSC due (calculated using the midi job formula) and their household marginal tax rate. To get the relevant household marginal tax rate we use the average annual husband income in the sample (€41,000) and assume 80% of this is taxable.[4] We then apply the year-by-year tax formulas to this amount to determine the household marginal tax rate.[5] In this manner, we calculate the size of the notch and the difference in slope above the notch (ΔT Married and Δt Married). In our implementation, we revert to modeling the budget set from the perspective of the individual (no tax below the threshold), but use the ΔT and Δt calculated above. Appendix Table B.5 shows the relevant model inputs used and Appendix B.3 contains a full explanation of how the budget sets are constructed.

Applying this estimation in 2002 and 2012 recovers a 'long-term' elasticity of 0.328 in 2002 and 0.228 in 2012, comparable to those in Tazhitdinova (2020).

To understand the implications of adjustment frictions for elasticity estimation, we propose a simple exercise. Call the bunching mass at the notch in 2002 B_{2002}. In a friction-less economy, we expect all of this excess mass to dissipate immediately following the reform. Now suppose that B_{2002} was unobservable to us and that all we observed was the mass of persons moving after the reform (akin to what one

targeting the new notch could be misinterpreted as excess mass at the old notch. When looking at the years 2008-2012, it indeed appears that what is left over and counted as excess mass is more likely imprecision in targeting the new notch. To remedy this we subtract the excess mass left at the old threshold in 2012 from all estimates to obtain what we term 'normalized bunching less the natural level'. For the 2013 reform we do not see far enough into the future to make this correction appropriately, so we subtract the amount we used for the 2003 reform.

[4]The €41,000 that we see includes the employee SSC contribution, which hovers at around 20% and much of which is not part of taxable income. In practice, employees can also benefit from other deductions which we cannot estimate, so we think it reasonable to assume 80% of observed earnings are taxable. We do not and cannot account for non-labor income.

[5]We take the marginal household tax rate as approximately constant so as to have a linear budget set. As such, we ignore the modest increases in the rate that arise from including wife's earnings above the threshold in household earnings and any increases from increasing earnings within the €400-1000 range. In practice the marginal household rate increases only slightly when including this extra income (see Appendix Table B.4).

might recover from a difference-in-difference design in an environment without a notch/kink). If we observed the equivalent of B_{2002} persons moving immediately, we would conclude that the elasticity had to be e. Suppose we observed, instead, that only $\tilde{B} < B_{2002}$ mass moved right after the reform. Then, we would have concluded that the elasticity was less than e. That is, we would actually have recovered an attenuated, short-run elasticity.

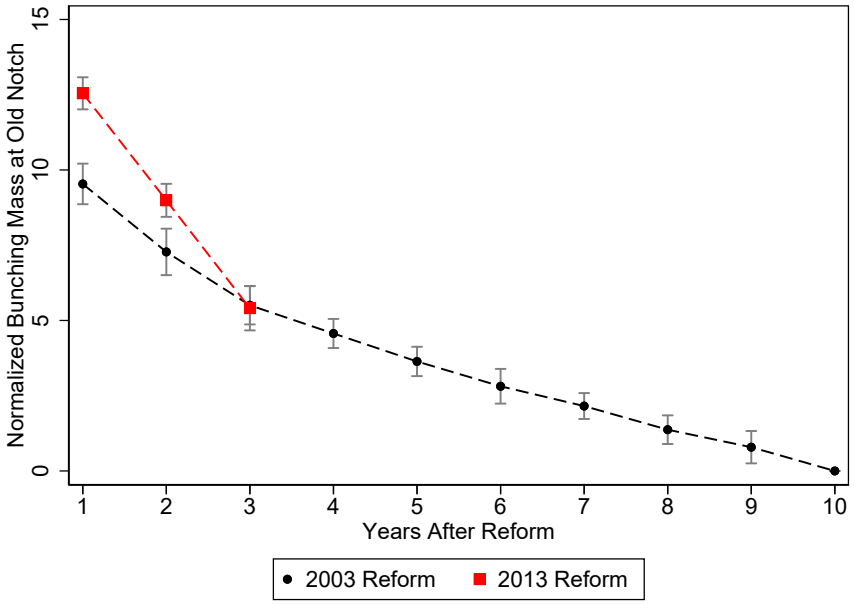
We calculate these 'short-run' elasticities using what we term 'the observed adjustment mass' in each post-reform period. We define the observed adjustment mass as the original 2002/2012 bunching mass less the mass stuck at the old notch in each period. This is meant to capture the real amount of un-bunching post-reform in the most precise and conservative manner.[6] Then, we calculate the 2002 elasticity using the standard bunching approach on the 2002 budget set, assuming that the bunching mass in 2002 was this observed adjustment mass.

We plot these implied 'short-run' elasticities over time in Figure B.1 panel (b). We see that elasticities are significantly and meaningfully attenuated in the short-run. The elasticity immediately following the reform is 0.154 or 47% of the long-run, structural elasticity. The 3 year elasticity gets closer, but at 0.229 is still only 70% of the long-run structural elasticity. Attenuation is even more severe for the 2013 reform. The 1-year elasticity is 0.02, as barely enough mass to cover the dominated region adjusts. The 3-year elasticity here is 0.140 or 61% of the long-term elasticity. The more severe attenuation makes sense given that the utility gain from adjustment post-2013 is lower: as Chetty (2012) suggests, the short-run elasticity is closer to the long-run structural elasticity when the utility gains from adjustment are larger.

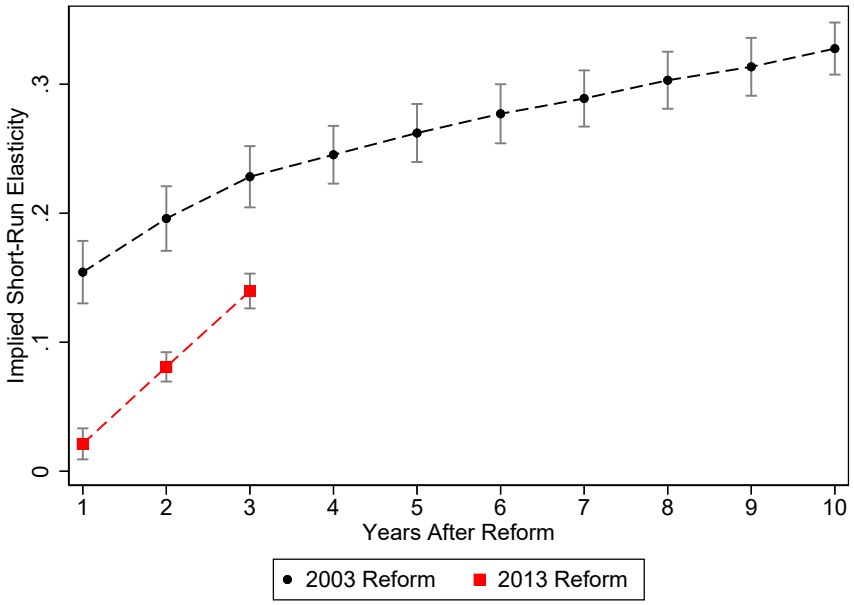
[6] We prefer this method to using the mass at the new notch for two reasons. First, some workers adjust to the reform by moving into the midi job region, so we cannot simply recover the adjusted mass from the mass between the old and new notch. Second, this effectively counts exits from the data or other unrelated dissipation of mass at the old notch as adjustment. This biases elasticities upwards, and hence is the most conservative choice.

Figure B.1: Bunching and Elasticity Estimates

(a) Bunching at the Old Notch following each Reform



(b) Implied Elasticities



Notes: Panel (a) plots normalized excess mass at the old notch (€325 post-2003 and €400 post-2013) in each year following the reforms. Year 1 is Apr-Dec 2003 for the 2003 reform and 2013 for the 2013 reform. The bunching mass is normalized by the average counter-factual density over the bunching region. Counter-factual densities are estimated period by period keeping z_l fixed, chosen to be the bin centered at €225 for the first reform and €297.5 for the second. We select (z_u) as the bin that most closely sets the total bunching mass (not just at the old threshold) equal to the missing mass to the right of the threshold. Average monthly earnings were divided into €15 bins starting at €20. The counter-factual polynomial was quintic, and the estimation window extended up to €2007.5 euro. Appendix Figures A.5–A.6 show the counter-factual densities and shade in what counts towards bunching at the old threshold. We note that the estimated excess mass in 2012 is likely due to imprecision in targeting the new threshold and term this level the 'natural' level of excess mass. This figure plots excess mass at the old threshold less this 'natural' level. Panel (b) plots the implied elasticities from the exercise described in Section B.1.We plot 95% confidence intervals around each estimate. We bootstrap standard errors by drawing 2500 trials with replacement from the earnings distributions and running all of the counter-factual estimation and model fit programs. This means the standard errors take into account both the natural variation in the data arising from the sample size and variation in the polynomial fit.

B.2 Fixed Adjustment Cost Estimation

Gelber et al. (2020) propose a method for estimating an average, fixed adjustment cost in settings such as ours. They assume workers face a fixed cost of changing their earnings level and that there is a chance in each period that this fixed cost no longer has to be paid. The fixed cost thus encompasses everything from a cost paid to learn the tax system to search costs to renegotiation costs. The dynamic nature of the process could reflect everything from the natural spread of information to the fact that some costs like search or scheduling costs may naturally be reset after some other shock like moving jobs for other reasons or a firm labor demand shock. In the interest of producing comparable estimates from a new context, we present the results obtained from their methodology in our setting here.

Following Gelber et al. (2020), assume that after a reform workers pay a fixed cost ϕ to change their earnings. This is incorporated directly into their money metric utility function.

$$u_t = z_t - T(z_t) - \frac{n}{1+1/e}\left(\frac{z_t}{n}\right)^{1+1/e} - \hat{\phi}\mathbf{1}(z_{t+1} \neq z_t)$$

Additionally, we assume agents are myopic and that both ability and the elasticity are time-invariant. In each period after a reform period T, there is a random chance $(1-\pi_t)$ that individuals no longer have to pay the adjustment cost and draw a free adjustment. Thus, $\hat{\phi} = \phi$ with probability π_t and $\phi = 0$ with probability $(1-\pi_t)$.

Figures 2–3 clearly suggest that all observable excess mass at the old threshold has dissipated by 2002 (2012) after the 1996 (2003) reforms. In the lens of the above model, we take this to suggest that by this time $\prod_{t \geq T} \pi_t$ has reached 0. As a consequence, bunching in the year right before the reform (2002 or 2012) pins down the true structural elasticity. Bunching at the old notch in the following periods (the reform period and subsequent years) helps us estimate ϕ and the cumulative products of π.

Specifically, the bunching mass in 2002/2012 alone identifies e. In order to be just identified we cannot estimate π_t for each t right after the reform; so we impose $\pi = 1$ immediately after the reform (2003b/2013) and estimate all later πs. When we pool the reforms we relax this and allow for some to draw a 0 cost immediately. The excess mass at the old notch in the period of the reform (2003b and 2013) identifies ϕ as the cost that justifies the left over bunching. Every person with utility gain from adjusting greater than ϕ will adjust immediately and the rest will stay. In the following period (2004/2014) every buncher still left at the old notch who gets a good draw will adjust freely and the rest will stay. Hence the excess mass at the old notch in 2004/2014 identifies π_{2004}, and so on. We compare the mass at the old threshold in each period generated by our model under a uniform counter-factual to the observed bunching and estimate parameters using a minimum distance estimator with an identity weighting matrix. The model will fit the data perfectly as long as excess mass at the old notch dissipates over time.

We implement this estimation on our married women sample for which we can feel confident we are correctly specifying the tax notch. The budget set is constructed as described in Appendix B.3. Appendix Table B.5 shows the relevant inputs.

We make three observations related to implementation. i) We note that the budget set changes in some of the years post-reform. This could potentially shift around the identity of the marginal unbuncher. In practice, it turns out that the estimated marginal unbuncher is sufficiently close to the new notch that she wants to relocate to the new notch point and not to a point above the new notch under all budget sets post-reform. This simplifies the analysis. ii) Another point of note is that in practice people exit the data, move downwards, or move far upwards in the distribution. In the model we treat any changes in the excess mass at the old notch as adjustment, but some of these changes may be due to unmodeled shocks. In practice this means we will under-estimate frictions. An alternative is to restrict the sample to non-exiters, but this is too restrictive over the long time horizon. iii) We bootstrap standard errors by drawing 2500 trials with replacement from the earnings distributions and running all of the counter-factual estimation and model fit programs. This means the standard errors take into account

both the natural variation in the data arising from the sample size and variation in the polynomial fit. The minimum distance estimator takes the best fit over 20 trials for different starting values of e and π_t between 0 and 1 and ϕ between 0 and 100.

Results. Appendix Figures A.5–A.6 show the estimated counter-factuals and the excess mass at the old notch for the 2003 and 2013 reform respectively. We estimate the counter-factual densities period by period, keeping the lower omission threshold z_l fixed. Every mass point that contributed to bunching at the notch in 2002 continues to be omitted post-reform and counted as bunching at the old notch; this is shaded in gray. One challenge arises from the fact that the old threshold is relatively close to the new threshold, such that imprecision in targeting the new notch could be misinterpreted as excess mass at the old notch. To remedy this we subtract the excess mass left at the old threshold in 2012 from all estimates to obtain what we term 'normalized bunching less the natural level'. For the 2013 reform we do not see far enough into the future to make this correction appropriately, so we subtract the amount we used for the 2003 reform. We match these modified moments with our model. This naturally yields a lower adjustment cost than the alternative. The estimated normalized bunching mass less the natural level can be read from Appendix Table B.1.

We estimate elasticities of 0.328 and 0.228 for 2002 and 2012 respectively (see Appendix Table B.1). These are comparable to those in Tazhitdinova (2020). We estimate an adjustment cost (ϕ) of around €40 in both periods, indicating that immediately following a reform people will only adjust if the utility gain from adjusting is larger than a €40 per month raise (the quasi linear utility function is a money metric). However, this is not true forever. In the year following the reform year only 72-76% of people will still have to pay this cost, the rest can adjust freely. In the year following this (2005/2015) only 43-58% of people have to pay this cost, and so forth.

We pool the two reforms and re-estimate the model in the last panel of Table B.1. Pooling the reforms (but continuing to fix $\pi_{03/13} = 1$) yields a slightly smaller, but comparable, adjustment cost (35) and an elasticity of 0.275. When pooling the reforms we can also now estimate and identify $\pi_{03/13}$. That is, we can allow for a fraction of workers to immediately have a chance of not paying the adjustment cost. This model estimates a comparable elasticity (0.272) and suggests a higher adjustment cost (67) but gives individuals a 43% chance of drawing a 0 adjustment cost in the first post-reform period.

Table B.1: Dynamic Adjustment Cost Estimation: Married Women

	2003 Reform										
Year	2002	2003b	2004	2005	2006	2007	2008	2009	2010	2011	2012
Normalized Bunching Less 'Natural' Level (Old Notch)	31.320	9.536	7.277	5.504	4.568	3.638	2.814	2.157	1.371	0.789	0
	(0.577)	(0.344)	(0.394)	(0.324)	(0.246)	(0.249)	(0.294)	(0.219)	(0.243)	(0.274)	
Average Adjustment Cost (ϕ)	**40.231**										
	(1.433)										
Elasticity (e)	**0.328**										
	(0.010)										
Cumulative Prob. of Having to Pay Adj. Cost ($\prod_{t=2004}^{Year} \pi_t$)	–	1	**0.763**	**0.577**	**0.479**	**0.382**	**0.295**	**0.226**	**0.144**	**0.083**	**0.000**
			(0.044)	(0.034)	(0.025)	(0.025)	(0.029)	(0.021)	(0.024)	(0.028)	(0.000)
SSE	0.000 (0.005)										

	2013 Reform			
Year	2012	2013	2014	2015
Normalized Bunching Less 'Natural Level' (Old Notch)	26.097	12.546	8.990	5.407
	(0.303)	(0.272)	(0.280)	(0.376)
Average Adjustment Cost (ϕ)	**37.886**			
	(0.450)			
Elasticity (e)	**0.228**			
	(0.005)			
Cumulative Prob. of Having to Pay Adj. Cost ($\prod_{t=2014}^{Year} \pi_t$)	–	1	**0.717**	**0.431**
			(0.018)	(0.024)
SSE	0.000 (0.000)			

	Pooled Reforms ($\pi_{03/13} = 1$)				**Pooled Reforms ($\pi_{03/13}$ estimated)**			
Year	02/12	03b/13	04/14	05/15	02/12	03b/13	04/14	05/15
Average Adjustment Cost (ϕ)	**34.603**				**67.167**			
	(0.426)				(0.552)			
Elasticity (e)	**0.275**				**0.272**			
	(0.005)				(0.005)			
Cumulative Prob. of Having to Pay Adj. Cost ($\prod_{t=03/13}^{Year} \pi_t$)	–	1	**0.789**	**0.513**	–	**0.425**	**0.312**	**0.207**
			(0.022)	(0.021)		(0.008)	(0.008)	(0.008)
SSE	31.074 (4.918)				18.038 (3.875)			

Notes: This table shows the results of the fixed adjustment estimation described in Appendix B.2. The estimation is for the married women, whose earnings distributions are shown in Figures 2–3. Counter-factual densities are shown in Appendix Figures A.5–A.6. We subtract the bunching mass from 2012 – or the 'natural level' – to not overstate bunching at the old notch coming from imprecision in targeting. Standard errors are bootstrapped 2500 times and account for both the counter-factual estimation as well as the minimum distance estimation.

B.3 Budget Sets

We construct the mini job budget sets shown in Figure 1 and used in this appendix for bunching calculations following Tazhitdinova (2020). While the construction of the budget sets from the individual perspective, ignoring employer social security contributions, is relatively simple, we find it unappealing to ignore the changes in employer social security contributions on mini jobs (from 22% to 30%). To this end, we explicitly account for the changes, while retaining the standard individual perspective in the literature.

To account for the employer contributions, we assume 100% pass-through of taxes to the employee. We translate what we observe in the data (posted earnings) into gross earnings, defined as earnings inclusive of the employer social security contribution. Gross earnings thus correspond to the total cost of a worker. A worker in a mini job in 2002 earning 325 euro a month in posted earnings actually earns $325 * 1.22 = 396.5$ in gross earnings (0 individual SSC and income tax; 22% employer SSC).[7] We calculate tax rates on gross earnings as in Saez et al. (2012). Let z be gross earnings inclusive of employer and employee SSC. Let w be gross earnings net of employer SSC and c be what the worker receives after individual and employer SSC. τ_r corresponds to the employer's SSC rate and τ_e corresponds to the employee's SSC rate. $c = (1 - \tau_e)w$ and $z = (1 + \tau_r)w$ so $c = \frac{(1-\tau_e)}{(1+\tau_r)}z = (1 - \frac{\tau_r+\tau_e}{1+\tau_r})z$. That is, the sum of employer and employee SSC is equivalent to a combined tax rate of $\tau = \frac{\tau_r+\tau_e}{1+\tau_r}$.

Calculating taxes as above creates a $t1 > 0$ below the notch, and a $t2 > t1$ with $t2 - t1 = \Delta t$. The vast majority of the public finance literature ignores employer social security contributions when calculating elasticities (as it does not often change at a notch/kink). Including $t1$ as the baseline tax rate dampens elasticities and makes them less comparable to other work. To keep with the individual-focused literature, we subtract $t1$ and model the final budget set as facing a tax of 0 below the notch and Δt above. This keeps the budget set from the individual perspective, but accounts for employer-level tax changes over time.

Pre-April 1 2003 Budget Set. Below the mini job threshold the employer pays τ_{mini} in SSC and the employee pays 0 in SSC and income taxes. Above the threshold the employer pays $\tau_{all}/2$, and the employee pays $\tau_{all}/2$ plus income taxes. Let N be the mini job threshold and then $N_g = N(1 + \tau_{mini})$ be the mini threshold in terms of gross earnings. Let X_g be gross earnings, equal to $(1 + \tau_{mini}) * X_p$ for mini jobs and $(1 + \frac{\tau_{all}}{2}) * X_p$ for regular jobs, where X_p is posted earnings, or what we observe in the data.

Taxes in terms of gross earnings for persons who do not owe income taxes are:

$$T(X_g) = \frac{\tau_{mini}}{1 + \tau_{mini}} X_g \qquad \text{if } X_g \leq N_g$$

$$T(X_g) = \frac{\tau_{all}}{1 + \tau_{all}/2} X_g \qquad \text{if } X_g > N_g$$

In practice, our married women also owe income taxes, but only if they cross into the regular job region. Once in a regular job, all earnings are pooled with household income and hence extra earnings from crossing the notch are taxed at the household marginal tax rate. We assume households use income splitting. We further assume that the household marginal tax rate is constant, as opposed to increasing with wife earnings. This linearizes the budget set and ignores what are in practice for our sample modest changes of 1-2 percentage points.[8] We use $\hat{\tau}_{income}$ from Appendix Table B.4, which is the marginal household rate for a husband earning 41,000 euro a year (the mean in our sample). Once the wife enters

[7] One minor complication that arises from looking at gross earnings is that workers with posted earnings at the threshold (one in a regular job and one in a mini job) are no longer directly comparable. Across the threshold, the employer taxes drop to 20% and thus a worker with gross earnings just above 396.5 actually earns 330.41 after the employer contribution, slightly above 325. Thus a mini jobber with posted monthly earnings at 325 is comparable in total cost to a regular worker at 330.41.

[8] See Appendix Table B.4 which shows the effect of adding approximately 10,000 euro a year in taxable income, well above the 4800 a year from a 401 euro regular job.

a regular job her earnings are taxable at this household rate, after deductions d_w. In the pre-2003 period we assume a $d_w = 20\%$ deduction, roughly corresponding to social security deductions. From 2003-2005 we use a lower $d_w = 5\%$, as close-to-the-threshold midi jobs pay approximately 5% SSC as opposed to usual 20%, lowering the deductions rate. Analogously, for 2006-2015 we use $d_w = 10\%$ deduction rate. We call the effective income tax rate for the wife $\tau_{income} = d_w \hat{\tau}_{income}$.

The pre-2003 budget set inclusive of income taxes becomes:

$$T(X_g) = \frac{\tau_{mini}}{1+\tau_{mini}} X_g \qquad \text{if } X_g \leq N_g$$
$$T(X_g) = \frac{\tau_{all}+\tau_{income}}{1+\tau_{all}/2} X_g \qquad \text{if } X_g > N_g$$

It will be convenient to split this up between what is due at the notch and what is due above. Call $\Delta T = \Delta T_{income} + \Delta T_{ssc}$ the lump sum amount due to the pure notch, which comes from the additional SSC due right after the notch plus the income tax due. Crossing the notch means increasing gross earnings above $N_g = N(1+\tau_{mini})$ (we compare you to a comparable worker in terms of *gross* earnings). The amount of income tax due right at this point is $\Delta T_{income} = \frac{\tau_{income}}{(1+\tau_{all}/2)} * N_g$. The extra SSC due is $\Delta T_{ssc} = \frac{\tau_{all}}{1+\tau_{all}/2} * N_g - \frac{\tau_{mini}}{1+\tau_{mini}} * N_g$. The tax rate below the threshold is given by $t_1 = \frac{\tau_{mini}}{1+\tau_{mini}}$ and the tax rate above the threshold by $t_2 = \frac{\tau_{all}+\tau_{income}}{1+\tau_{all}/2}$. We call Δt the difference between these two. Together ΔT and Δt describe the budget set drawn in Figure 1. Specifically, the individual faces a tax of 0 below the notch, the size of the discontinuity at the notch is given by ΔT, and the slope of the budget set above the notch is given by (1-Δt). These numbers can be found in Appendix Table B.5 and are the numbers used in our estimations.

2003(Apr-Dec)-2012. Once midi jobs are introduced the budget set changes. Let N_p be the mini job threshold in posted earnings (€400 a month) and $N2_p$ be the midi job threshold in posted earnings (€800 a month). Below the threshold nothing changes. Above the threshold, the official midi job formula in terms of posted earnings is given by

$$T(X_p) = [\tau_{mini} N_p + (\tau_{all} \frac{N2_p}{N2_p - N_p} - \frac{\tau_{mini} N_p}{N2_p - N_p})(X_p - N_p)] \text{ if } N_p < X_p \leq N2_p$$

Or

$$T(X_p) = [\tau_{mini} N_p + (2\tau_{all} - \tau_{mini})(X_p - N_p)] \text{ if } N_p < X_p \leq N2_p$$

This means that a person with posted earnings just above 400 pays $\tau_{mini} * 400$ (the same as a mini jobber at 400), while a person with posted earnings at 800 pays $\tau_{all} * 800$, the amount due on regular jobs, with the tax schedule being linear in between the two amounts.

The budget set from 2003(Apr-Dec)-2013 in terms of gross earnings is thus

$$T(X_g) = \frac{\tau_{mini}}{1+\tau_{mini}} X_g \qquad \text{if } X_g \leq N_g$$
$$T(X_g) = \frac{2\tau_{all} - \tau_{mini} + \tau_{income}}{1+\tau_{all}/2} X_g - \frac{2N_g(\tau_{all} - \tau_{mini})}{1+\tau_{mini}} \qquad \text{if } N_g < X_g \leq N2_g$$

Again, we find it convenient to split out ΔT explicitly, now given by $\Delta T_{income} = \tau_{income} * N_g/(1 + \tau_{all}/2)$. The extra SSC due is $\Delta T_{ssc} = \frac{2\tau_{all}-\tau_{mini}}{1+\tau_{all}/2} N_g - \frac{2(\tau_{all}-\tau_{mini})}{1+\tau_{mini}} N_g - \frac{\tau_{mini}}{1+\tau_{mini}} * N_g$ The tax rate below the threshold is $t_1 = \frac{\tau_{mini}}{1+\tau_{mini}}$ and the tax rate above the threshold is $t_2 = \frac{2\tau_{all}-\tau_{mini}+\tau_{income}}{1+\tau_{all}/2}$. Once again, these numbers are shown in Appendix Table B.5.

2013-2015. This is analogous to the budget set from 2003-2013, except N_p and $N2_p$ change from 400 and

800 to 450 and 850. The budget sets for this period are:

$$T(X_g) = \frac{\tau_{mini}}{1+\tau_{mini}} X_g \qquad \text{if } X_g \leq N_g$$

$$T(X_g) = \frac{2.125\tau_{all} - 1.125\tau_{mini} + \tau_{income}}{1+\tau_{all}/2} X_g - \frac{2.125 N_g(\tau_{all} - \tau_{mini})}{1+\tau_{mini}} \qquad \text{if } N_g < X_g < 2N_g$$

Again we find it convenient to split out ΔT explicitly, now given by $\Delta T_{income} = \tau_{income} * N_g/(1+\tau_{all}/2)$. The extra ssc tax due is $\frac{2.125\tau_{all}-1.125\tau_{mini}}{1+\tau_{all}/2} N_g - \frac{2.125 N_g(\tau_{all}-\tau_{mini})}{1+\tau_{mini}} - \frac{\tau_{mini}}{1+\tau_{mini}} * N_g$ The tax rate below the threshold is $t_1 = \frac{\tau_{mini}}{1+\tau_{mini}}$ and the tax rate above the threshold is $t_2 = \frac{2.125\tau_{all}-1.125\tau_{mini}+\tau_{income}}{1+\tau_{all}/2}$. Once again, these numbers are shown in Appendix Table B.5.

We do not model the budget set above $N2_g$ and its convex kink points. We do not observe any holes in the data at this point so for simplicity we extend the budget set past 800/850. In practice the marginal buncher/unbuncher falls below this, validating this simplification.

Table B.2: Social Security Contribution Rates

	Mini Job Region		**Regular Job Region**		**Midi Job Region**	
Year	Employee	Employer (τ_{mini})	Employee or Employer	τ_{all}	Employee (at $Z+\epsilon$)	Employer
	(1)	(2)	(3)	(4)	(5)	(6)
2000	0	22.00	20.54	41.07	–	–
2001	0	22.00	20.44	40.88	–	–
2002	0	22.00	20.64	41.28	–	–
2003 (Apr-Dec)	0	25.00	20.85	41.70	4.15	20.85
2004	0	25.00	21.00	42.00	4.00	21.00
2005	0	25.00	20.95	41.90	4.05	20.95
2006	0	25.00	20.95	41.90	3.50	20.95
2007	0	30.00	19.55	39.10	10.45	19.55
2008	0	30.00	19.40	38.80	10.06	19.40
2009	0	30.00	20.08	40.15	9.92	20.08
2010	0	30.00	19.78	39.55	10.22	19.78
2011	0	30.00	20.18	40.35	10.82	20.18
2012	0	30.00	20.03	40.05	9.97	20.03
2013	0	30.00	19.73	39.45	10.27	19.73
2014	0	30.00	19.73	39.45	10.27	19.73
2015	0	30.00	19.78	39.55	10.22	19.78

Notes: Table shows approximate SSC rates for mini-, midi- and regular employment over time that are used in the calculation of the budget sets. For regular employment, the SSC by the Federal Ministry of Labor and Social Affairs (BMAS) are reported (τ_{all}). These rates avoid any notch between the transition from mini- to midi-job region. For employer and employees equal split of SSC rates is assumed for simplicity (τ_{all}). Column (3) and (6) highlight identical SSC employer contributions for regular- and midi-jobs. Due to the smooth change in total SSC-contributions, employee contributions at the lower midi-jobs threshold are calculated as τ_{mini} - $\tau_{all}/2$. Sources: http://tinyurl.com/table-minijobzentrale, https://tinyurl.com/BMAS-F-factor, http://www.sozialpolitik-aktuell.de/.

Table B.3: German Tax Schedule for Taxable Income

Taxable Income Cutoffs (€per year)	(1)	(2)	(3)	(4)	(5)
2000	0	6902	8946	58643	–
Marginal Rates on Taxable Income	**0**	**22.9-25.0**	**25.0-51.0**	**51.0**	–
2001	0	7206	9249	55009	–
Marginal Rates on Taxable Income	**0**	**19.9-23.0**	**23.0-48.5**	**48.5**	–
2002-3	0	7235	9252	55008	–
Marginal Rates on Taxable Income	**0**	**19.9-23.0**	**23.0-48.5**	**48.5**	–
2004	0	7665	12740	52152	–
Marginal Rates on Taxable Income	**0**	**16.0-24.05**	**24.05-45.0**	**45.0**	–
2005-6	0	7665	12740	52152	–
Marginal Rates on Taxable Income	**0**	**15.0-23.97**	**23.97-42.0**	**42.0**	–
2007-8	0	7665	12740	52152	250000
Marginal Rates on Taxable Income	**0**	**15.0-23.97**	**23.97-42.0**	**42.0-45.0**	**45.0**
2009	0	7835	13140	52552	250400
Marginal Rates on Taxable Income	**0**	**14.0-23.97**	**23.97-42.0**	**42.0-45.0**	**45.0**
2010-12	0	8004	13470	52881	250730
Marginal Rates on Taxable Income	**0**	**14.0-23.97**	**23.97-42.0**	**42.0-45.0**	**45.0**
2013	0	8130	13470	52881	250730
Marginal Rates on Taxable Income	**0**	**14.0-23.97**	**23.97-42.0**	**42.0-45.0**	**45.0**
2014	0	8355	13470	52881	250730
Marginal Rates on Taxable Income	**0**	**14.0-23.97**	**23.97-42.0**	**42.0-45.0**	**45.0**
2015	0	8473	13470	52881	250730
Marginal Rates on Taxable Income	**0**	**14.0-23.97**	**23.97-42.0**	**42.0-45.0**	**45.0**

Notes: Taxes apply to singles. Taxation of married couples follows the same schedule except that tax liability is determined by pooling income and dividing it in half. The tax brackets are applied to this sum to calculate tax liability, which is then doubled to come up with the final amount owed. In each range, marginal tax rates increase linearly with taxable income. Cutoffs for 2000 and 2001 are expressed in Euros (From DM) using the conversion 1DM = 1.95583 euro. Source: https://www.bmf-steuerrechner.de/

Table B.4: Marginal Household Income Tax Rates

Wife Gross Income	0	0	0	0	0
Husband Gross Income	13000	23000	33000	**41000**	53000
Household Net Income	10400	18400	26400	**32800**	42400
– After Splitting Rule –	5200	9200	13200	**16400**	21200
	Marginal Tax Rate (€1 extra to **Taxable** income)				
2000	0	0.25	0.27	**0.29**	0.31
2001	0	0.23	0.25	**0.27**	0.30
2002-3	0	0.23	0.25	**0.27**	0.30
2004	0	0.18	0.24	**0.26**	0.30
2005-6	0	0.18	0.24	**0.26**	0.28
2007-8	0	0.18	0.24	**0.26**	0.28
2009	0	0.18	0.24	**0.25**	0.28
2010-12	0	0.16	0.23	**0.25**	0.28
2013	0	0.16	0.23	**0.25**	0.28
2014	0	0.16	0.23	**0.25**	0.28
2015	0	0.15	0.23	**0.25**	0.28

Notes: This table shows the marginal tax on an extra euro in taxable income. Net income is calculated from Gross by taking out a 20% social security contribution in all years. To find the effective marginal income tax rate for a wife moving from a mini to a regular job, we multiply these rates by 0.8, as only 80% of the additional income is subject to income taxation. For example, if a wife of a person earnings €41000 a year in 2005 goes from a mini job (which does not count towards taxable income) to a regular job earning, say €1000 a month, that would add about €12000· 0.8 =€9600 to taxable income. This extra income only modestly pushes up the marginal tax rate (to 0.27 to be exact) and will effectively be taxed at a rate of 0.26. This means that €2500 will be due in income taxes on the €12000; resulting in a marginal income tax rate of 0.21 ($= 0.8 * 0.26$), when 0 was due on the mini job earnings. Source: Own Calculations from Table B.3.

Table B.5: Budget Set Inputs

	τ_{mini}	τ_{all}	τ_{income}	ΔT Single	ΔT Married	t_1 Sing & Married	t_2 Married	t_2 Single	Δt Married
2002	0.22	0.4128	0.2161	64.17	135.18	0.180	0.521	0.342	0.341
2003(Apr-Dec)	0.25	0.4170	0.2566	8.02	114.17	0.200	0.696	0.483	0.496
2004	0.25	0.4200	0.2470	7.80	109.86	0.200	0.692	0.488	0.492
2005	0.25	0.4190	0.2436	7.88	108.59	0.200	0.688	0.486	0.488
2006	0.3	0.4190	0.2308	16.1	115.33	0.231	0.636	0.445	0.405
2007	0.3	0.3910	0.2308	16.85	117.24	0.231	0.596	0.403	0.365
2008	0.3	0.3880	0.2308	16.90	117.42	0.231	0.592	0.399	0.361
2009	0.3	0.4015	0.2292	16.63	115.87	0.231	0.610	0.419	0.379
2010	0.3	0.3955	0.2278	16.77	115.67	0.231	0.600	0.410	0.369
2011	0.3	0.4035	0.2278	16.58	115.16	0.231	0.611	0.422	0.381
2012	0.3	0.4005	0.2278	16.65	115.35	0.231	0.607	0.417	0.376
2013	0.3	0.3945	0.2278	19.34	130.65	0.231	0.609	0.418	0.378
2014	0.3	0.3945	0.2278	19.34	130.65	0.231	0.609	0.418	0.378
2015	0.3	0.3955	0.2278	19.32	130.59	0.231	0.610	0.418	0.379

Notes: This table shows the budget set parameters used in our elasticity calculations above, as calculated from the information in the preceding tax and SSC tables. Our final budget sets for married women, shown for 2002, 2003, and 2013 in Figure 1 have a slope of 1 before the notch, then dip by (ΔT Married), and have a slope of ($1 - \Delta t$ Married) thereafter.

C Own Survey

We performed our own survey of mini jobbers that experienced the 2013 reform.[1] The aim of the survey was to receive more information on the actual adjustment process from the perspective of individuals that were employed in a mini job at the time of the reform.

C.1 Implementation and Survey Structure

We implemented our survey using an online platform similar to Amazon Turk – www.clickworker.de – using a two-step survey design. In the first step we asked women on the platform aged 25 to 55 (30-59 at time of survey) whether they held a mini job for at least 10 months in any of the years 2011-2015. If they held a job in 2012, we asked what their monthly earnings were. The blurb indicated that the survey was geared towards persons with mini job experience. We reached 1042 women in this first stage.

We then selected all individuals that responded yes to having worked in a mini job for at least 10 months in 2012 and reported monthly earnings in the range of 350 to 500 Euro for this year for a follow-up survey. This selection criteria applied to 185 individuals, all of whom we contacted for a detailed follow-up survey. This main survey asked each participant 22 questions, which were mainly related to the circumstances of the reform and the reason for (not) adjusting to the new earnings threshold. 103 individuals participated in the main survey and we payed each of them 5 Euro. Some of the questions we asked to all individuals, but we also implemented forks in the survey that allowed us to target some adjustment related question in greater detail. The list of questions can be found here: https://tinyurl.com/yz8unb57. We present a summary of our core findings below.

C.2 Summary of Main Findings

In this subsection we present the main findings of the additional survey. All results are shown for 3 groups: All 103 people who took the follow-up survey (henceforth: 'all'), the sample of individuals in the follow-up survey who continue to report that they held a mini-job in 2012 (henceforth: 'In Mini'), and the sample of individuals in the follow-up survey that report wages in 2012 at the threshold, which is defined as reporting monthly wages between 365.5 Euro and 412.5 Euro (henceforth: 'In Mini at threshold'). The latter is our preferred sample given its consistency with our empirical results, but sample sizes are small. Table C.1 shows the number of observations for these three groups, split by survey specific forks. Adjustment was specifically defined to include both asking for a raise or more hours.

Table C.1: Number of Observations by Forks and Sample Restrictions

Responses: N	All	In Mini	In Mini at threshold
Not in Mini in both years (2012 and 2013)	25	9	6
Stayed in Mini in 2013 but switched employer	21	16	10
Stayed in Mini in 2013 at same employer, of which:	57	48	30
I asked my employer for adjustment	13	13	9
My employer asked me for adjustment	18	15	11
Neither asked for adjustment	26	20	10
Column Total	103	73	46

[1] The survey was conducted with approval from Boston University's Institutional Review Board's approval. The survey was submitted under the title "Survey for Frictions in Adjusting Earnings: Evidence from Notches in German Mini Jobs" and was classified 'Exempt' by the IRB and given protocol number 4375X.

Awareness of the Reform. To investigate the awareness of individuals about the reform, we asked three different questions. In Question 2 of the survey we asked "when did you first hear about the reform that came into effect in January 2013?" The results are displayed in Table C.2: Of all respondents more than 74% say they heard about it at or before the time of reform. Less than 11% say they heard about the reform in 2014 or later. When restricting to individuals that were in a mini-job in 2012 (in a mini-job at the threshold), the respective numbers are 80.12% and 7% (82.6% and 6.8%). In Question 32 we asked individuals directly, whether they think a lack of information was in any way related to problems in adjusting to the new threshold. Results are presented in Table C.3. For all specifications, most individuals (80.6% -87%) responded that a lack of information was not an issue, while between 10.9%-12.6% reported it was. In a third question (Question 33) we asked how people heard about the reform (Table C.4). About three quarter report they heard it through media, followed by their boss and by friends. Importantly, only one responded that a new colleague hired after the reform told her about the reform, suggesting that hires of new mini jobbers are not or at most very weakly associated with the transmission of information.

Table C.2: In 2013 the mini-job threshold was increased from 400 to 450. When did you first hear about this reform? (Question 2)

Responses: N (%)	All	In Mini	In Mini at threshold
1- Before January 2013	53 (51.1 %)	43 (60.6 %)	27 (58.7 %)
2- In January 2013	24 (23.3 %)	15 (20.6 %)	11 (23.9 %)
3- Later in 2013	15 (14.6 %)	10 (14.1 %)	5 (10.9 %)
4- In 2014 or later	4 (3.9 %)	3 (4.2 %)	2 (4.4 %)
5- Not before today	7 (6.8 %)	2 (2.8 %)	1 (2.2 %)
Column Total	103 (100 %)	73 (100 %)	46 (100 %)

Table C.3: Do you think a lack of information was in anyway related to potential problems in adjusting?

Responses: N (%)	All	In Mini In 2012	In Mini In 2012 and 2013
0- Prefer not to answer/Other	7 (6.8 %)	4 (5.5 %)	1 (2.2 %)
1- Yes	13 (12.6 %)	7 (9.6 %)	5 (10.9 %)
2- No	83 (80.6 %)	62 (85.9 %)	40 (87.0 %)
Column Total	103 (100 %)	73 (100 %)	46 (100 %)

Table C.4: How did you hear about the reform?

Responses: N (%)	All	In Mini In 2012	In Mini In 2012 and 2013
0- Prefer not to answer	4 (3.9 %)	-	-
1- Friends or acquaintance	9 (8.7 %)	5 (6.9 %)	2 (4.4 %)
2- I was hired after reform	1 (1,0 %)	-	-
3- Media (newspaper, television...)	75 (72.8 %)	56 (76.7 %)	36 (78.3 %)
4- My boss	6 (5.8 %)	6 (8.2 %)	5 (10.9 %)
5- By an old colleague (in company before reform)	4 (3.9 %)	3 (4.1 %)	2 (4.4 %)
6- Through internet	1 (1,0 %)	1 (1.4 %)	1 (1.7 %)
7- By a new colleague (hired at the time of reform)	1 (1,0 %)	1 (1.4 %)	-
8- By an employee representative	2 (1,9 %)	1 (1.4 %)	1 (2.2 %)
Column Total	103 (100 %)	73 (100 %)	46 (100 %)

Adjustment-Specific Questions. Table C.5 shows adjustment rates for all 3 sample groups by different categories such as perceived firm demand for mini jobbers and contract-type. We define adjustment as individuals reporting a monthly wage for 2013 of at least 412.5 but below 500 Euro per month. For all specifications, there is a clear positive association with individual adjustment rates and the perceived demand for mini jobbers at the firm. The adjustment rates for cases that think information was an issue in adjusting are a bit lower than those who think it was not.

Table C.5: Group-Specific Adjustment Rates

Adjustment-Rates in % (Group-specific N)	All	In Mini	In Mini at threshold
	ADJUSTMENT RATES		
by demand for mini jobbers at firm			
Had more mini-jobbers than needed	25 % (4)	25 % (4)	25 % (4)
Was satisfied with number of mini jobbers	41.5 % (53)	37.5 % (40)	50 % (22)
Wanted to hire a few more mini jobbers	53.3 % (15)	40 % (10)	66.7 % (6)
Wanted to hire many new mini jobbers	85.7 % (7)	100 % (6)	100 % (5)
by type of contract			
Fixed Number of Hours	42.9 % (42)	44.4 % (36)	55.6 % (27)
Flexible Number of Hours	46.7 % (45)	39.4 % (13)	58.8 % (17)
Do you think lack of Information was an issue?			
Yes	46.2 % (13)	28.6 % (7)	40 % (5)
No	41.0 % (83)	43.55 % (62)	57.5 % (40)
All People in Column	42.7% (103)	42.5 % (73)	56.5 % (46)

Notes: This table displays adjustment rates (the percentages) for different groups of people. Adjustment is defined as reporting a monthly wage of at least €412.5 and below €500 in 2013. The number of observations is in parentheses. The Panel "by demand for mini jobbers at firm" shows the responses to the question "How would you have described your employer's demand for mini job labor at the time of your 2012 mini job?" and the Panel "by type of contract" shows the responses to a question that asks: "What type of contract did you have for your 2012 mini job?". Categories like "'I prefer not to answer"' are omitted. Category specific numbers do therefore not necessarily add up to the total number of observations.

Path-Specific Questions. Table C.6 - C.8 show the responses to questions that are path-specific. Table C.6 shows responses to questions that are targeted to individuals that report that they asked for adjustment. While most of the 13 respond that there were no problems in adjustment, one notes that a bad business situation was a problem and two note that their employer had to first reach out to other parties. Table C.7 shows results for individuals who were asked by their employer whether or not they wanted to adjust. Many of these report being offered additional work hours, though some report wage increases. Table C.8 shows responses for individuals who were neither asked by their employer nor asked to adjust. Adjustment was explicitly defined to include both increases in hours or in wages. Of these the majority report not wanting to work more, though some report that they knew their employer did not need them for additional hours and many think their employer would not have accommodated them had they asked.

Table C.6: Asked Employer for adjustment

Responses: N (%)	All/ In Mini	In Mini at threshold
Were there any problems in accommodating your request (to adjust)?		
Employer had first to talk to other parties (i.e. workers council)	2 (15.38%)	2 (22.22%)
Bad Business Situation	1 (7.69%)	1 (11.11%)
No	7 (53.85%)	4 (44.44%)
Prefer not to answer/I don't know/ Other	3 (23.08%)	2 (22.22%)
Did other workers at your firm have similar requests?		
Yes	3 (23.08%)	2 (22.22%)
No	4 (30.77%)	2 (22.22%)
There are no other mini jobbers	1 (7.69%)	-
I don't know	5 (38.46%)	5 (55.55%)
Column Total	13 (100%)	9 (100%)

Notes: This table summarizes question 20 and 21 of the survey, that were asked only to those who asked their employer for adjustment.

Table C.7: Employer asked for adjustment

Responses: N (%)	All	In Mini	In Mini at threshold
What describes Your Situation best?			
Employer offered me more work hours	10 (55.56%)	9 (60%)	7 (63.64%)
Employer offered me a wage increase	8 (44.44%)	6 (40%)	4 (36.36%)
What do you think was the reason your employer offered you a wage/hours increase?			
Employer wanted to increase wages of all worker.	2 (11.11%)	2 (13.33%)	1 (9.09%)
Fairness.	2 (11.11%)	-	-
There was more to do and I could work more.	6 (33.33%)	5 (33.33%)	3 (27.27%)
Increased Workload	1 (5.56%)	1 (6.67%)	1 (9.09%)
I only worked more hours	1 (5.56%)	1 (6.67%)	1 (9.09%)
My employer is very social	1 (5.56%)	1 (6.67%)	1 (9.09%)
Because he was obliged by law	1 (5.56%)	1 (6.67%)	-
Wanted to keep me in the company	1 (5.56%)	1 (6.67%)	1 (9.09%)
No idea..., definitely not out of kindness	1 (5.56%)	1 (6.67%)	1 (9.09%)
Because he liked me	2 (11.11%)	2 (13.33%)	2 (18.08%)
Did your employer make similar offers to other mini jobbers?			
Yes	8 (44.44%)	6 (40%)	4 (36.36%)
No	4 (22.22%)	5 (33.33%)	4 (36.36%)
I don't know	6 (33.33%)	4 (26.67%)	3 (27.27%)
Column Total	18 (100%)	15 (100%)	11 (100%)

Notes: This table summarizes question 23-25 that were asked only to individuals whose employers asked for adjustment.

Table C.8: Nobody asks for adjustment

Responses: N (%)	All	In Mini	In Mini at threshold
Why didn't you ask for a raise or more hours?			
My employer had no financial resources	1 (3.85%)	1 (5%)	-
I was not at the old threshold	2 (7.69%)	2 (10%)	-
I thought/knew my employer didn't needed me	3 (11.45%)	3 (15%)	1 (10%)
I didn't want to work more/was satisfied as was	12 (46.15%)	8 (40%)	5 (50%)
I was too shy to ask	2 (7.69%)	2 (10%)	1 (10%)
Other	6 (23.08%)	4 (20%)	3 (30%)
Do you think your employer would have adjusted you if you had asked?			
I don't know	10 (38.46%)	8 (40%)	2 (20%)
Yes	2 (7.69%)	1 (5%)	1 (10%)
No	13 (50%)	10 (50%)	6 (60%)
Prefer not to answer	1 (3.85%)	1 (5%)	1 (10%)
Column Total	26 (100%)	20 (100%)	10 (100%)

Notes: This table summarizes question 27 and 29 that were asked only to individuals that did not ask and were not asked to adjust. Number of observations in parentheses.

D Data Appendix

We summarize here the key samples, variables, and data sources used in the paper. We also present summary statistics for the samples in Figures 2—3 and Tables 2—3.

D.1 Married Women Sample

Our primary sample for examining earnings responses to each reform consists of earnings record from the Integrated Employment Biographies data file (IEB) from 1999-2015. We keep all employment spells (*quelle* $= 1$), drop all spells with missing wages, and drop any full duplicates. We restrict to women aged 26 to 55 at the start of their employment spell and to women whose establishment is located in West Germany. We exclude individuals who receive unemployment insurance or means tested social assistance, individuals registered as job-searchers, and individuals that participate in programs offered by the federal employment agency.

We further restrict to married women with husband's annual earnings (from all employment sources in the data) between €33000 and 53000. The data do not contain indicators on marital status.[1] As mentioned in the main text, we construct a sample of married couples using the methodology outlined in Goldschmidt et al. (2017). Simon Trenkle, who works at IAB, conducted the identification procedure. All subsequent analysis used only an indicator for a "married woman" with attached husband earnings, rounded to the nearest 10 euro for anonymity. No couple identifiers were used subsequently. Matthew Gudgeon only accessed anonymized social security data on-site in the secure environment of the research data center in Nuremberg, Germany. Couples are matched in 2008. Last names are used to verify these persons are still married in prior and future years (they need to still share the same last name). Thus, all women in our sample need to show up in the data (employment or unemployment) with their husband in 2008.

One additional feature of the data is that an ongoing employment spell will not necessarily be interrupted for a wage increase. We only observe total earnings and duration, so a wage increase within-year will be undetectable. For this reason, most of the analysis is done at the year level. This does not restrict us except for the fact that the 2003 reform started April 1st. Fortunately, the 2003 reform also required a change in the way establishments report mini job social security contributions. For the majority of firms, this required a manual change in the software used for reporting that resulted in the interruption of all ongoing mini job employment relationships on March 30th. Ongoing mini jobs then resumed as a new employment period at the same establishment on April 1st. The vast majority of mini job relationships are split on April 1st, allowing us to see exact average monthly earnings in January-March 2003 separately from April-December 2003. For non-split spells (like regular jobs and the few mini jobs that did not abide by this), we manually 'split' spells that go through 1 April 2003.

Average Monthly Earnings. An individual's average monthly earnings (from all sources) is calculated by taking total earnings from all employment sources over the year divided by the number of distinct days worked and multiplied by 365/12 or 366/12 in leap years. We sum earnings across all jobs (regular or mini jobs), as the tax notch applies to that sum. Figures 2 and 3 plot duration-weighted average monthly earnings; that is each individual is weighted by the fraction of days in the year he or she is employed.

Adjustment. In Figure 4, adjustment is defined as having average monthly earnings in Apr-Dec 2003, or 2013 for the second reform, between $Z + 12.5$ and $\tilde{Z} + 12.5$, where Z is the old mini job threshold and $\tilde{Z}$ is the new threshold.

[1] The only exception is for persons who register as job seekers, at which point this is collected.

D.2 Establishment Sample

When looking at establishment level variables, we focus on the establishments employing the married women in our baseline estimation sample. Specifically we use all establishments that employ our 26-55 year old identified married women in West Germany with husband earnings between €33,000-53,000 and average monthly earnings below €1400 in either 2002/January-March of 2003 for the first reform or 2012 for the second. We use *all* workers, not just married women, at the establishment to construct establishment level variables for a window of years around each reform.

In order to focus on workers constrained by the notch, we take workers with monthly earnings at the threshold in the year prior to each reform (2002 and, separately, 2012). We further restrict to workers whose only job in this year was a mini job, and who worked in this mini job in 2000 and 2001 for the first reform (2010 and 2011 for the second reform). The variables used in Section 5 are defined below.

At the threshold. At the threshold (Z) in 2002/2012 is defined as having average monthly earnings between $Z - 37.5$ and $Z + 12.5$. The room on the left is motivated by Figure 4. The room on the right allows for small rounding and reporting errors. We require that average monthly earnings from all sources, average monthly earnings at the firm, and average monthly earnings in a mini job all fall within this range. This ensures we are focusing on exclusive mini jobbers.

Adjustment (Immediate). We focus first on 1-year adjustment rates, defining a worker as adjusted if she increases her average *mini* earnings *within the original establishment* above the old threshold level Z, to between $Z+12.5$ to $\tilde{Z}+12.5$, in the period immediately following the reform, where $\tilde{Z}$ is the new threshold.

Any Mini Hires. "Any new mini hire" is defined as a binary variable equal to $= 1$ if the firm has at least one entirely new mini jobber in that period and 0 otherwise. Specifically, a new mini jobber is defined as a person appearing in a mini job at the establishment that has not been in any job at that establishment in the prior years up to 2000/2010. Persons with both a mini and regular job at the same establishment in the same period (which is uncommon) are counted as regular workers. We restrict to establishments that have at least one mini jobber in all periods we use the hiring variable.

Total Non-Mini Earnings Growth. We take the change in the natural logarithm of total non-mini earnings at the firm. Total non-mini earnings are calculated by adding up all employee earnings. We first cap all earnings at the lowest cap across years. We also top and bottom code the change in the natural logarithm to be between -1 and 1 to address outliers.

Predicted Hires. We construct predicted mini hires in the reform year T using two steps. In the first step we regress the fraction of new mini hires in the pre-reform year $T-1$ on a set of predetermined predictor variables. The fraction of new mini hires is defined as: Frac. New Mini Hires$_{i,e,T-1} = min(\frac{\#\text{new mini-jobbers}_{e,T-1}}{\#\text{ mini-jobbers}_{e,T-1}}, 1)$ with $\#$new mini-jobbers$_{T-1}$ being the number of new mini-jobbers at establishment e and $\#$mini-jobbers$_{T-1}$ being the total number of mini jobbers in that establishment in year $T-1$. We use the following set of predictors in the parsimonious OLS specification:

- Firm size in T-4: 33 firm-size dummies for mini-job employment size and 35 dummies for non-mini-job employment size 4 years prior to the reform
- Growth in mini-jobbers in establishment e between year $T-t-1$ and $T-t$ defined, – $\forall\, t = \{2, 3\}$, as: growth mini$_{T-t} = min(\frac{\#\text{mini jobbers}_{e,T-t} - \#\text{mini jobbers}_{e,T-t-1}}{\#\text{mini jobbers}_{e,T-t-1}}, 1)$

- Growth in non-mini-jobbers in establishment e between year $T-t-1$ and $T-t$ defined $\forall\, t = \{2, 3\}$ as: growth non-mini$_{T-t} = min(\frac{\#\text{non-mini jobbers}_{e,T-t} - \#\text{non-mini jobbers}_{e,T-t-1}}{\#\text{non-mini jobbers}_{e,T-t-1}}, 1)$
- Fraction of new mini job hires. Defined as above, but for the years $T-3$ and $T-2$
- Fraction of new non-mini job hires. Defined as for mini-job hires, but using the number of new non-mini job hires relative to the non-mini job work-force

In the LASSO specification, we allow the LASSO to select any of the preceding variables in addition to any of the potential predictors below:

- Establishment Wage Structure: 25th, 50th, 50th squared, 75th and the share of the 75th to 25th percentile of wages at the establishment in year $T-t\ \forall\, t = \{1, 2, 3\}$
- Establishment Age Structure: 8 different age categories as share of all employees in the year $T-t$ $\forall t = 1, 2, 3$. The age groups in years are: age ≤ 19, $20 \leq$ age ≤ 24, $25 \leq$ age ≤ 49, $50 \leq$ age ≤ 54, $55 \leq$ age ≤ 59, $60 \leq$ age ≤ 64, age ≥ 65. In addition the square of $25 \leq$ age ≤ 49 is included
- Industry. Dummies for approximately 750 time-consistent industries, based on 2008's 5-digit industry classification
- Region (County Level). Dummies for approximately 400 time-consistent Counties (German: Kreise)

For the 2002 regression, it the optimal LASSO selects 3 variables for past growth, 4 variables for past hires, 21 firm-size dummies, 11 age-variables, 56 industry dummies and 42 industry dummies. The number of variables selected for the 2012 regression are relatively similar.

In the second step, we calculate predicted mini hires for the reform-year using the regression coefficients from the pre-reform year now applied to the updated predictors in the reform year. Formally, let Frac. New$\hat{}$Mini Hires$_{i,e,\mathbf{T-1}} = \hat{\eta} Z_{i,e,\mathbf{T\text{-}1}}$ be the predicted hires pre-reform where $Z_{i,e,\mathbf{T\text{-}1}}$ are the variables used in the pre-reform regression and $\hat{\eta}$ the corresponding coefficients from that regression. We obtain our reform prediction as Frac. New$\hat{}$Mini Hires$_{i,e,\mathbf{T}} = \hat{\eta} Z_{i,e,\mathbf{T}}$ using the same coefficients but updated variable values.

Baseline Controls. We always include dummies for 22 industries, dummies for 88 occupations as measured by the modal mini job occupation at the establishment, dummies for the 10 West German states, and 34 dummies for firm mini and, separately, non-mini employment size. Specifically, we dummy out the floor of duration-weighted employment for 1 to 25 workers. Beyond that we group establishments with 26-50, 51-75, 76-100, 101-150, 151-200, 201-300, 301-400, 401-500, and 500+ employees into their own bins.

Individual Controls. Individual level controls cover demographic information (age, years of education, gender, and a dummy for German Nationality), tenure (job tenure, occupation tenure), earnings histories in 2001 and 2002 (days worked, mini job earnings at the establishment, and total earnings from all sources), indicators for holding multiple jobs in 2001 (a dummy for working in 2 or more establishments and a dummy for working in both a mini and a regular job), and occupation dummies (as opposed to just the modal occupation).

Establishment Controls. Establishment level controls cover information on the structure of the firm (the fraction of mini jobbers at the notch, the fraction of the work force that is mini jobbers, fraction of mini and non-mini jobbers in the modal occupation, the fraction of mini and non-mini jobbers

working year round, and whether the modal mini and modal non-mini occupations differ), firm age, establishment level worker demographics (average age, share of commuters, share of non-Germans, average education, share of females, average tenure and occupation tenure, each for non-mini and mini jobbers separately), earnings concentration (the standard deviation of earnings for mini jobbers and non-mini jobbers), local municipality (*gemeinde*) unemployment level and its growth rate, and 5 digit industry dummies (the lowest level available).

Table D.1: Married Women Sample Summary Statistics (for Figures 2–3)

	PANEL A: 2002				
	mean	sd	min	med	max
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Average Monthly Wage	448.000	262.489	20.075	323.633	1009.992
Average Monthly Wage if in Mini Job^{+}	275.532	70.493	0.304	303.863	359.235$^{!}$
Average Monthly Wage if Nonmini Job^{+}	801.429	247.962	0.304	822.163	1549.121$^{!}$
Average Days Worked*	335.700	70.862	1	365	365
Average Days Worked if in Mini Job*$^{+}$	321.122	85.632	1	365	365
Average Days Worked if Nonmini Job*$^{+}$	315.326	97.180	1	365	365
Age	38.473	6.892	26	37	55
Female	1	0	1	1	1
Husband Annual Earnings (2002)	40717	5566	33005	39625	52995
Non-German	0.059	0.235	0	0	1
Education (Years)	10.996	2.365	8	10	18
Occ: Cleaner	0.180	0.385	0	0	1
Occ: Cashier/Sales Clerk	0.181	0.385	0	0	1
Occ: Secretary	0.171	0.376	0	0	1
Occ: Nurse/Health Worker	0.123	0.328	0	0	1
Occ: Transportation	0.026	0.160	0	0	1
Held a Mini Job in 2002	0.688	0.463	0	1	1
Held a Midi Job in 2002	0	0	0	0	0
Held a Regular Job in 2002	0.360	0.480	0	0	1
Num. of Individuals	193307				

	PANEL B: 2012				
	mean	sd	min	med	max
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Average Monthly Wage	478.365	234.503	20.128	399.855	1009.951
Average Monthly Wage if in Mini Job^{+}	333.344	91.351	0.915	371.935	440.697$^{!}$
Average Monthly Wage if Nonmini Job^{+}	729.615	318.747	0.915	774.395	1650.354$^{!}$
Average Days Worked*	353.671	48.255	1	366	366
Average Days Worked if in Mini Job*$^{+}$	326.254	84.530	1	366	366
Average Days Worked if Nonmini Job*$^{+}$	303.312	107.524	1	366	366
Age	43.012	6.513	26	43	55
Female	1	0	1	1	1
Husband Annual Earnings (2012)	41608	5515	33005	40950	52995
Non-German	0.109	0.311	0	0	1
Education (Years)	11.327	2.806	8	10	18
Occ: Cleaner	0.225	0.417	0	0	1
Occ: Cashier/Sales Clerk	0.187	0.390	0	0	1
Occ: Secretary	0.116	0.320	0	0	1
Occ: Nurse/Health Worker	0.095	0.293	0	0	1
Occ: Transportation	0.037	0.189	0	0	1
Held a Mini Job in 2012	0.754	0.431	0	1	1
Held a Midi Job in 2012	0.119	0.323	0	0	1
Held a Regular Job in 2012	0.346	0.476	0	0	1
Num. of Individuals	182644				

Notes: This Table presents selected summary statistics for the married women in Figures 2–3 with average monthly earnings from all sources less than or equal to €1000. Statistics are weighted by the fraction of the year that the person is employed. Panel A corresponds to 2002 (panel (a) in Figure 2) and Panel B corresponds to 2012 (panel (c) in Figure 3). See D.1 for sample construction details. *non-weighted. $^{!}$99th percentile instead of max. $^{+}$When conditioning on a mini jobbers in 2002 (2012) the sample size is 134356 (138095). When conditioning on non-mini jobbers in 2002 (2012) the sample size is 69285 (70940).

Table D.2: Establishment Sample Summary Statistics (for Table 2)

	mean	sd	min	med	max
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
PANEL A: 2003 REFORM					
2002 Mini Job Earnings at Establishment	314.382	11.301	288	318.767	337
Age	45.486	11.307	19	45	65
Female	0.895	0.306	0	1	1
Non-German	0.099	0.298	0	0	1
Education (Years)	11.167	2.942	8	10	18
Occ: Cleaner	0.270	0.444	0	0	1
Occ: Cashier/Sales Clerk	0.195	0.396	0	0	1
Occ: Secretary	0.113	0.317	0	0	1
Occ: Nurse/Health Worker	0.057	0.232	0	0	1
Occ: Transportation	0.042	0.201	0	0	1
Adjusted to 400 in 2003	0.363	0.481	0	0	1
Any Mini Hires 2001	0.813	0.390	0	1	1
Any Mini Hires 2002	0.780	0.414	0	1	1
Any Mini Hires 2003(Apr-Dec)	0.766	0.423	0	1	1
Any Mini Hires 2004	0.777	0.417	0	1	1
Any Mini Hires 2005	0.750	0.433	0	1	1
Any Mini Hires 2001(est. level)	0.670	0.470	0	1	1
Any Mini Hires 2002 (est. level)	0.628	0.483	0	1	1
Any Mini Hires 2003(Apr-Dec) (est. level)	0.618	0.486	0	1	1
Any Mini Hires 2004 (est. level)	0.630	0.483	0	1	1
Any Mini Hires 2005 (est. level)	0.599	0.490	0	1	1
Firm Age (Years since 1975) (est. level)	17.835	9.324	0	19	28
Total Nonmini Employment (est. level)	73.548	417.037	0	13	54916
Total Mini Employment (est. level)	18.858	78.751	1	6	7816
Fraction Mini (est. level)	0.387	0.271	0	0.333	1
Fraction of mini jobbers in modal occupation (est. level)	0.752	0.249	0	0.799	1
Fraction of mini jobbers working year-round (est. level)	0.730	0.230	0	0.753	1
Num. of Individuals	220307				
Num. of Establishments	74086				
PANEL B: 2013 REFORM					
	mean	sd	min	med	max
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
2012 Mini Job Earnings at Establishment	390.711	11.379	363	396.195	412
Age	46.874	10.229	19	47	65
Female	0.883	0.321	0	1	1
Non-German	0.104	0.306	0	0	1
Education (Years)	11.653	3.132	8	10	18
Occ: Cleaner	0.217	0.412	0	0	1
Occ: Cashier/Sales Clerk	0.215	0.411	0	0	1
Occ: Secretary	0.097	0.296	0	0	1
Occ: Nurse/Health Worker	0.062	0.241	0	0	1
Occ: Transportation	0.069	0.254	0	0	1
Adjusted to 450 in 2013	0.334	0.471	0	0	1
Any Mini Hires 2011	0.844	0.363	0	1	1
Any Mini Hires 2012	0.831	0.375	0	1	1
Any Mini Hires 2013	0.809	0.393	0	1	1
Any Mini Hires 2014	0.797	0.402	0	1	1
Any Mini Hires 2015	0.770	0.421	0	1	1
Any Mini Hires 2011(est. level)	0.727	0.446	0	1	1
Any Mini Hires 2012 (est. level)	0.709	0.454	0	1	1
Any Mini Hires 2013 (est. level)	0.683	0.465	0	1	1
Any Mini Hires 2014 (est. level)	0.666	0.472	0	1	1
Any Mini Hires 2015 (est. level)	0.637	0.481	0	1	1
Firm Age (Years since 1975) (est. level)	21.719	12.161	2	20	38
Total Nonmini Employment (est. level)	74.769	353.204	0	15	36954
Total Mini Employment (est. level)	30.332	129.022	1	9	8269
Fraction Mini (est. level)	0.444	0.276	0	0.403	1
Fraction of Mini in Modal Occupation (est. level)	0.708	0.258	0	0.726	1
Fraction of Mini working Year-Round (est. level)	0.722	0.210	0	0.735	1
Num. of Individuals	194877				
Num. of Establishments	68409				

Notes: This Table presents selected summary statistics for the establishment sample in Table 2. Panel A corresponds to the 2003 reform and Panel B to the 2013 reform. (est. level) means summary statistics are generated at the establishment level (as opposed to at the individual level) and is used for variables that vary only at that level. See D.2 for sample construction details.

Table D.3: Establishment Sample Summary Statistics (for Table 3)

	PANEL A: 2003 REFORM				
	mean	sd	min	med	max
	(1)	(2)	(3)	(4)	(5)
2002 Mini Job Earnings at Establishment	313.143	11.407	288	316.637	337
Age	46.061	11.755	19	46	65
Female	0.871	0.335	0	0	1
Non-German	0.121	0.327	0	0	1
Education (Years)	11.286	3.078	8	10	18
Occ: Cleaner	0.329	0.470	0	0	1
Occ: Cashier/Sales Clerk	0.155	0.362	0	0	1
Occ: Secretary	0.097	0.296	0	0	1
Occ: Nurse/Health Worker	0.036	0.186	0	0	1
Occ: Transportation	0.054	0.226	0	0	1
Adjusted to 400 in 2003	0.322	0.467	0	0	1
Δ Ln total non-mini earnings 2001-2000	0.064	0.162	-1	0.047	1
Δ Ln total non-mini earnings 2002-2001	0.029	0.144	-1	0.026	1
Δ Ln total non-mini earnings 2003-2002	-0.042	0.176	-1	-0.016	1
Δ Ln total non-mini earnings 2004-2003	-0.059	0.186	-1	-0.025	1
Δ Ln total non-mini earnings 2005-2004	-0.045	0.205	-1	-0.011	1
Δ Ln total non-mini earnings 2001-2000 (est. level)	0.063	0.168	-1	0.041	1
Δ Ln total non-mini earnings 2002-2001 (est. level)	0.027	0.150	-1	0.023	1
Δ Ln total non-mini earnings 2003-2002 (est. level)	-0.024	0.165	-1	-0.004	1
Δ Ln total non-mini earnings 2004-2003 (est. level)	-0.032	0.169	-1	-0.008	1
Δ Ln total non-mini earnings 2005-2004 (est. level)	-0.037	0.201	-1	-0.005	1
Firm Age (Years since 1975) (est. level)	20.741	8.666	2	26	28
Total Nonmini Employment (est. level)	139.162	570.152	10	48	54916
Total Mini Employment (est. level)	29.890	105.641	1	10	7816
Fraction Mini (est. level)	0.225	0.176	0	0.182	1
Fraction of mini jobbers in modal occupation (est. level)	0.705	0.251	0	0.720	1
Fraction of mini jobbers working year-round (est. level)	0.674	0.223	0	0.693	1
Num. of Individuals	153979				
Num. of Establishments	39913				
	PANEL B: 2013 REFORM				
	mean	sd	min	med	max
	(1)	(2)	(3)	(4)	(5)
2012 Mini Job Earnings at Establishment	389.485	11.588	363	393.755	412
Age	46.976	10.649	19	48	65
Female	0.864	0.342	0	0	1
Non-German	0.119	0.323	0	0	1
Education (Years)	11.775	3.230	8	10	18
Occ: Cleaner	0.254	0.435	0	0	1
Occ: Cashier/Sales Clerk	0.193	0.394	0	0	1
Occ: Secretary	0.082	0.275	0	0	1
Occ: Nurse/Health Worker	0.041	0.197	0	0	1
Occ: Transportation	0.084	0.277	0	0	1
Adjusted to 450 in 2013	0.309	0.462	0	0	1
Δ Ln total non-mini earnings 2011-2010	0.060	0.134	-1	0.044	1
Δ Ln total non-mini earnings 2012-2011	0.053	0.126	-1	0.046	1
Δ Ln total non-mini earnings 2013-2012	0.019	0.146	-1	0.025	1
Δ Ln total non-mini earnings 2014-2013	0.033	0.149	-1	0.039	1
Δ Ln total non-mini earnings 2015-2014	-0.010	0.222	-1	0.026	1
Δ Ln total non-mini earnings 2011-2010 (est. level)	0.061	0.143	-1	0.041	1
Δ Ln total non-mini earnings 2012-2011 (est. level)	0.051	0.129	-1	0.042	1
Δ Ln total non-mini earnings 2013-2012 (est. level)	0.020	0.132	-1	0.023	1
Δ Ln total non-mini earnings 2014-2013 (est. level)	0.032	0.138	-1	0.036	1
Δ Ln total non-mini earnings 2015-2014 (est. level)	-0.017	0.211	-1	0.020	1
Firm Age (Years since 1975) (est. level)	24.475	12.364	2	25	38
Total Nonmini Employment (est. level)	134.224	470.582	10	47	36954
Total Mini Employment (est. level)	46.971	169.774	1	16	8269
Fraction Mini (est. level)	0.291	0.204	0	0.250	1
Fraction of mini jobbers in modal occupation (est. level)	0.665	0.256	0	0.667	1
Fraction of mini jobbers working year-round (est. level)	0.671	0.196	0	0.683	1
Num. of Individuals	137984				
Num. of Establishments	37802				

Notes: This Table presents selected summary statistics for the establishment sample in Table 3, the sub-sample of Table 2 with establishments with at least 10 non-mini jobbers at the notch in 2002 (or 2012 for the second reform). Panel A corresponds to the 2003 reform and Panel B to the 2013 reform. (est. level) means summary statistics are generated at the establishment level (as opposed to at the individual level) and is used for variables that vary only at that level. See D.2 for sample construction details.

D.3 Other Data Sources

For descriptive purposes we use the Sample of Integrated Employment Biographies (SIAB), the Establishment History Panel (BHP), and the German Socio-Economic Panel (SOEP). The SIAB is a 2% random sample of individuals in the IEB (see Antoni et al. (2016)) from which we select all individuals employed on June 30th of each year between 2000-2014. The BHP, a 50% random sample of establishments drawn from the IEB data, contains employee information as of 30th of June of each year aggregated to the establishment-level. The SOEP is an annual, representative panel survey of German households. While the SOEP has the disadvantage of relatively low numbers of observations (it contains approximately 1,500 mini jobbers in 2010) making it unusable for analyzing adjustment, it contains a variety of useful descriptive variables like hours worked. Finally, we use our own survey data that asked mini jobbers who experienced the 2013 reform about their adjustment experiences (see Appendix C for details and results).

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