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ABSTRACT

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

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. In a frictionless world, the workers that made up the excess mass at the old notch should all increase their earnings. While some of these workers indeed adjust their earnings rapidly, over 38% do not, and instead take several years to adjust. 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 predict that adjustment will be quickest at growing firms. We test and find support for these demand-side effects in our linked employer-employee data.

JEL Classification: H24, H31, J22, J23

Keywords: labor supply responses to taxation, earnings adjustment frictions, labor demand

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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.[1] 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, well-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 reform and only gradually do so subsequently. Consequently, intensive margin elasticity estimates over a 1-2 year horizon are significantly attenuated relative to their long-term counterparts.[2]

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.[3] 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. We argue that firm-level labor demand conditions regulate the speed with which workers respond to tax changes. We present a model in which firms with insufficient demand for additional labor will find it optimal to delay their workers' adjustment. This model predicts that workers at growing firms will respond most rapidly to the mini job reforms. We use three reduced form exercises to test for expected associations between the speed of adjustment and different measures of idiosyncratic, establishment-level labor demand and 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), after which they become regular jobs, with gross earnings (not just the marginal euro) taxed at relatively high, standard rates. This creates a discontinuous jump — or notch — in the average tax rate at the threshold 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,

[1] Saez et al. (2012) note: "Estimates of the elasticity of taxable income in the long run (i.e., exceeding a few years) are plagued by extremely difficult issues of identification, so difficult that we believe that there are no convincing estimates of the long-run elasticity of reported taxable income to changes in the marginal tax rate."

[2] Our results complement Gelber et al. (2020)'s recent findings in a new setting.

[3] Chetty et al. (2009, 2011, 2013), which we discuss in more detail below, offer some candidate mechanisms: salience, information, and search costs combined with hours constraints.

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 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 ample 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 their 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 host of potentially distinct processes could generate the observed delays in earnings adjustment. We propose a conceptual framework in which heterogeneity in labor demand across firms leads to heterogeneity in the speed of worker adjustment. We start from the premise that workers are unlikely to leave their firm in the short-term just to obtain the utility bump allowed by the reform, giving firms flexibility in how they set workers' hours after each reform.[4] We study how a firm optimally reacts to the mini job reform using a dynamic labor demand model with linear hiring costs and exogenous labor attrition based on Nickell (1986). We model the reform as a discrete choice to increase all workers' hours and show that it can be optimal for the firm to delay doing so. A firm that has already optimized its workers' total hours would incur costly deviations from its optimum by giving its workers more hours post-reform. Instead, the firm's optimal adjustment strategy involves ceasing hiring upon the announcement of the reform and waiting for labor attrition to reduce the workforce, before increasing remaining workers' hours. In this set up, growing firms will be able to adjust hours quicker, and sometimes immediately. Put another way,

[4]This is a strong premise and stands in contrast to the main mechanism in Chetty et al. (2011) where adjustment operates explicitly through a search channel. Yet, it appears applicable to the first few post-reforms years in our setting. The vast majority of the post-reform earnings increases we observe occur within, and not between, establishments (over 94% of 1-year adjustment and over 85% for non-adjusters in the first year who adjust in the second post-reform year). This is likely in part a consequence of the limited utility gains from adjustment relative to household income (at most a €75 per month gain, less the utility cost of additional work hours).

firm and worker incentives are best aligned when the firm has excess labor demand following the reform and can take advantage of existing workers' desires for additional hours while saving on hiring costs.

As prima facie evidence that a firm's labor demand affects its workers' adjustment speeds, we show that adjustment patterns are correlated within firms. Using a sample of establishments employing the aforementioned married women, and taking all workers at these establishments, we document a robust within-establishment correlation in adjustment rates among tenured workers close to the pre-reform notch. Specifically, an establishment random effect is able to account for 28% of the variance in 1 year, intensive margin, adjustment. Consistent with our relatively homogeneous sample, this within-establishment correlation changes very little after accounting for worker sorting on observable characteristics. Moreover, it is common to observe either zero adjustment or high rates of adjustment within an establishment, even at establishments with many mini jobbers at the pre-reform notch. These descriptive statistics are consistent with establishment-level mechanisms playing a role in shaping earnings responses. We next turn to examining the specific role of establishment-level labor demand.

We propose three reduced form tests for equilibrium associations between workers' adjustment speeds and their establishments' 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. From the establishment's perspective, then, it should only hire a new worker after allowing existing workers who wish to do so to increase their hours. 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 mini-job labor demand.[5] 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 indeed 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 size, or at establishments with high predicted hires. All of our specifications include occupation, industry, state, and establishment size dummies so as to compare 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

[5]Galassi (2018) finds evidence of scale effects following the 2003 mini job reform.

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). Additionally, we leverage the fact that we have two reforms to show that results generally hold both within individuals and within establishments. Consistent with our conceptual framework, we confirm that the association between labor demand and earnings adjustment extends to adjustment on the hours margin following the second reform (for which we have hours data).

We end with an assessment of the quantitative relevance of establishment labor demand for individuals' adjustment. We revisit 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 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 Chetty et al. (2011), Kleven and Waseem (2013), and Gelber et al. (2020), we find compelling evidence of earnings adjustment frictions. Our work confirms Gelber et al. (2020)'s findings of delayed adjustment following a change in a tax discontinuity in a new and different context. 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).

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 et al., 2012).[6] Like Chetty (2012) and Gelber et al. (2020), we show that frictions may be obscuring an underlying, positive earnings elasticity. More pointedly, our proposed mechanism pertains directly to adjustment on hours, potentially explaining why evidence of hours adjustment is harder to come by than taxpayers responding to the tax system more broadly.

Second, we delve deeper into one 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

[6]For example Saez (2010) finds bunching only among the self-employed.

(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.[7] 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.[8] 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., forthcoming). Our conceptual framework is closest to Chetty et al. (2011), who trace out the implications of search costs and hours constraints for labor supply responses to taxes. Their model pertains to the distribution of earnings in equilibrium. Instead, we seek to explain heterogeneity in the speed of adjustment and why some firms will more rapidly offer increased hours to their employees post-reform than others. More specifically, we shut down the job search channel and focus instead on the determinants of within-firm adjustment. We view this shift in focus as particularly relevant to understanding short-term adjustment behavior following moderate tax changes, for which workers are unlikely to leave their jobs solely for the small potential utility gain. In highlighting the role of labor demand, we return to an older literature that focused on 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 the cross-section and why bunching is so large relative to other contexts, arguing that equilibrium labor supply responses depend on whether the statutory incidence of a tax falls on firms or workers. Our paper emphasizes the time dimension by focusing on the un-bunching process after each of the two reforms. Our work speaks explicitly to the understudied question of how short-term labor adjustment occurs, focusing on the dynamic, intensive-margin responses of *existing* mini jobbers' earnings and the role of firm labor demand.

[7] 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). To the extent that the factors that determine whether a firm is in need of new labor also determine the elasticity of labor demand, then a large short-term labor supply elasticity may simply reflect more elastic labor demand, which has very different implications.

[8] For an extensive-margin example, Kroft et al. (2015) derive an optimal tax formula that is allowed to depend on macro employment responses, relaxing the typical assumption that all job seekers find work.

Roadmap. Section 2 provides the necessary context to understand the mini job tax notch and the two reforms we analyze. Section 3 provides a simple framework to conceptualize one way in which lackluster firm labor demand can lead to delayed worker adjustment post-reform. Section 4 describes the data. Section 5 analyzes aggregate, worker-level earnings responses to each reform and establishes some initial facts about the role of establishments in the adjustment process. In Section 6, we establish the link between workers' earnings responses and their establishments' labor demand and quantify the relative importance of this channel. Section 7 concludes.

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.[9] Jobs with monthly earnings above the threshold are subject to the standard, higher social security and income tax rates 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% reduction in taxes. In Germany, both employers and employees pay SSC — approximately 20% each — on workers' gross earnings (see column (3) of Appendix Table C.2). The mini job exemption applies to the individual's SSC. 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. Firms paid a special mini job SSC rate that was only slightly above the SSC rate a firm would pay on a non-mini jobber (see column (2) of Appendix Table C.2). Thus, the *average* SSC rate increased discontinuously at the threshold.[10]

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

[9] 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).

[10] 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 unlikely to be relevant for our sample. We study married women whose husbands have a sufficiently high salary to ensure spousal health insurance coverage. Furthermore, much of our analysis centers on *existing* mini jobbers' responses to the two reforms, neither of which changed rules pertaining to 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).

incomes below €7,000 a year, well above the €3,900 one might earn in a mini job (see Appendix Table C.3 for the annual tax schedules). For married persons, though, this tax exemption is consequential. In Germany, filing jointly always dominates filing individually, and nearly all married couples choose to do so.[11] 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 (this rate hovers at around 26% for the women in our estimation sample; see Appendix Table C.4). Hence, 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 tax and SSC 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 €41,000 a year, the average level in our data (Appendix C.3 provides further details pertaining to budget set construction).

1 April 2003 reform. The first reform we analyze increased the mini job threshold from €325 to €400.[12] 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.[13] In addition to changing the mini job threshold, the 2003 reform greatly reduced the discontinuity stemming from SSC.[14] Importantly, married workers continued to experience a large notch as income taxes remained fully exempt below the threshold and fully applied above (see Figure 1 panel (b)).

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 (Steiner and Wrohlich, 2005; Eichhorst et al., 2012).[15] The reform also allowed workers in regular employment to hold one tax free mini job at a different establishment, incentivizing shifts towards mini job earnings among regular employees. We focus on the adjustment behavior of *exclusive* mini jobbers.

[11] The "splitting advantage" from filing jointly ranges from 0, if both spouses earn the same amount, to above €10,000, if one spouse earns over €100,000 and the other does not work (Steiner and Wrohlich, 2004; Bach et al., 2009).

[12] The €325 (DM 630) cutoff came into effect in 1999. Prior to this, it was indexed to average worker earnings (Arntz et al., 2003).

[13] The other parts of the Hartz I and II packages 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, 2018).

[14] In particular, total SSC (employer+employee) would now increase linearly for jobs with earnings between €400—800 ('midi' jobs) until they reached the standard employer plus employee total at €800. From the perspective of an individual worker, a small notch remained at the threshold, as the employee had to make up the difference between the standard employer rate and the higher mini job employer rate.

[15] A 15-hour a week job earning €325 a month would pay an hourly wage of €5.40. Jobs with more weekly hours would have to pay an even lower hourly wage. Thus, only jobs at a very low wage would have been affected by the hours constraint. According to Eichhorst et al. (2012) in 2010 (7 years after the hours threshold has been abolished and when the earnings threshold was €400) still only 12.7% of exclusive mini jobbers worked more than 15 hours a week.

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 (see 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)).

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.[16] 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, with mini jobs often called '400 (and later 450) euro jobs'. Our own survey (see Appendix D) supports the idea that individuals were well aware of the 2013 reform.[17]

Other relevant reforms. In addition to the preceding reforms, employers' social security contributions due on mini jobs increased over the period, from 22% in 1999-2003, to 25% in 2003-2006, to 30% after 2006 (see column (2) of Appendix Table C.2).[18]

One final point worth noting is that Germany enacted a national minimum wage for the first time in 2015, the final year in our data. This affects mini jobbers, many of whom earned below the minimum wage of €8.50. The bill was passed on 3 July 2014, after a decade-long debate on the subject. The passing of this bill could have impacted the tail end of our observed responses to the 2013 reform.

2.2 Mini Job Descriptives

As of 2010, about 5 million people work exclusively in a mini job.[19] A 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. 68% of exclusive mini jobbers are women. Mini jobs are also disproportionately held by students and older workers. The top occupations (in decreasing order) are cleaners, cashiers/sales clerks, secretaries, and transportation workers.

Employing a mini jobber is common: in 2010, roughly 70% of establishments had at least one

[16] Source: authors' email exchange with the Mini Job Zentrale.
[17] Only 7% of respondents chose to respond that they heard about the reform in 2014 or later.
[18] Collischon et al. (2020) examine these increases in detail.
[19] Summary statistics provided in this section stem from own calculations based on a 2% random sample of individuals (the SIAB) in mini jobs on June 30th of each year, a 50% random sample of establishments (the BHP), and the German Socio-Economic Panel (SOEP).

mini jobber. Establishments with multiple mini jobbers are commonplace.[20] Indeed, 86% of mini jobbers are employed in establishments with at least 2 mini jobbers and 49% in establishments between 3 and 25 mini jobbers. In establishments with mini jobbers, the share of the workforce composed of mini jobbers can vary substantially: while some firms are made up of almost all mini jobbers, it is most common to have a smaller percentage of the workforce made up of mini jobbers. Appendix Figure A.1 provides additional descriptive statistics.

3 Conceptual Framework: Firm Labor Demand and Adjustment

In this section, we present a simple, dynamic model of firm labor demand that can explain why a firm might delay increasing its workers' hours in response to the mini job reforms and why this delay would be shortest in firms with growing labor demand. The model motivates our eventual empirical tests for equilibrium associations between establishment-level labor demand and worker adjustment, but is intended primarily to convey one set of assumptions under which firms and their labor demand conditions could impact the speed with which workers adjust.

Before taking the firm's perspective, we briefly summarize what a standard labor supply model says about how labor supply is affected by the mini job tax notch and its two reform-induced rightward shifts. In a standard labor supply model with heterogeneity in worker ability (e.g. Kleven and Waseem, 2013), 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. This results in bunching to the left of the notch and missing mass to the right.[21] Since the excess density at the notch comes from workers with ideal earnings above the cutoff, all workers that compose this excess mass should strictly prefer increased earnings following a rightward shift in the notch. Some of these workers will have ideal earnings close to, but above, the original notch, while others will be constrained by the new notch. Note that this model has little to say about the speed with which this adjustment occurs — in a completely frictionless world, this ought to happen instantly. We will show this adjustment process is far from instantaneous in practice.

While a host of individual-level frictions can and do account for delayed adjustment, we propose that firms play an important role in mediating the speed with which workers adjust. We turn now to the firm perspective and our labor demand model.

[20] About 0.3 million of 2010's 2.89 million establishments had at least 5 mini jobbers, about 0.11 million at least 10 mini jobbers and about 3,000 establishments had a workforce of at least 100 mini jobbers.

[21] This mass of workers includes all workers whose ideal earnings absent the notch fall in the dominated region above the notch — the region in which a reduction in pre-tax earnings strictly increases post-tax earnings — but also workers with ideal earnings above the dominated region who now find the trade-off between greatly reduced work effort and a small loss in post-tax income worthwhile.

3.1 A Simple Framework

Building off Nickell (1986), we propose a dynamic labor demand model for a single firm that explains why a firm may find it optimal to delay increasing their mini jobbers' hours post-reform.[22] The underlying intuition is straightforward: immediately increasing all existing workers' hours to the desired, post-reform level can produce too little marginal revenue for the firm to justify the additional wage costs. The firm will thus prefer to reduce hiring and wait for worker attrition before increasing the remaining workers' hours. Expanding firms will find it optimal to increase their workers' hours sooner than firms with stable or declining labor demand.[23]

Setup. We assume that the firm employs solely homogeneous, at-the-threshold mini jobbers. The firm pays a fixed wage w and chooses employment N_t (via its hiring rate a_t) and hours h_t in each period. Firms are subject to hiring and firing costs. Firms face linear hiring (or training) costs (α) per new worker. This differentiates existing workers from new hires, and means that increasing existing workers' hours is cheaper than achieving a similar increase in person-hours through new hires. Firms also face firing costs, which we assume are infinite.[24]

We impose constraints on the hours packages the firm can offer, requiring it to treat all mini jobbers equally. Additionally, workers' hours are constrained by the mini job threshold. Workers face large tax penalties if they work more than $\underline{h} = Z/w$ hours, where Z is the mini job threshold. For simplicity, we treat $\underline{h}$ as a binding constraint below the firm's optimal, unconstrained, hours choice. The firm will thus set hours equal to $\underline{h}$ in all pre-reform periods.

The mini job reform relaxes this hours threshold. We model the firm's decision to increase hours post-reform as a discrete choice, effectively prohibiting firms from increasing everyone's hours by small amounts following the reform.[25]

For simplicity, we assume all mini jobbers would prefer to work additional hours post-reform. Importantly, we will also assume that workers do not quit their job solely to increase their hours post-reform, and instead leave only via exogenous attrition at a constant rate σ.[26] This assumption abstracts away from worker behavior and shuts down adjustment via a job search channel — a departure from the primary adjustment mechanism in Chetty et al. (2011). We think this shift in focus towards within-firm adjustment is both instructive and a sensible approximation of the short-run realities in our setting. Adjustment affords relatively small utility gains relative to household in-

[22] A formal treatment of our model is presented in Appendix B.

[23] To fix ideas, consider a firm employing multiple at-the-threshold mini job cleaners who clean offices. Without additional offices to clean, the marginal revenue from increasing labor hours could be low relative to wage costs. As cleaners leave the firm, it becomes easier to accommodate the remaining workers' hours requests and indeed doing so is preferable to meeting labor demand through the more costly option of hiring new workers.

[24] This rules out adjustment strategies in which the firm chooses to fire existing workers and adjust others. Firing costs and fairness considerations make this unlikely to be a commonly used strategy.

[25] The model could be adapted to allow gradual adjustment, but this is of limited interest in our setting where at an average wage of €8, the 2003 reform creates room for only around 2 extra work hours a week.

[26] Given exogenous turnover, hiring costs imply that the firm prefers to have fewer workers working more hours.

come.[27] Accordingly, in Section 5.4 we find that the vast majority of intensive-margin adjustment in the first several post-reform years occurs *within* the worker's pre-reform establishment.[28]

The firm's pre-reform problem. Given these assumptions, the firm's dynamic optimization problem is to maximize profits by choosing the number of mini jobbers N_t in each period through its choice of hiring rate a_t:

$$max_{N_t,a_t|_{t=1}^{\infty}} \sum_{t=0}^{\infty} \delta^t (pF(N_t, \underline{h}) - wN_t\underline{h} - \alpha a_t N_t) \text{ s.t. } N_{t+1} = (1 - \sigma + a_t)N_t \text{ and } N_o \text{ given}$$

The firm sells each unit of output produced through its production function $F(\cdot)$ at a constant output price p. It faces hiring costs of α per worker and pays for hires that enter next period in the current period. Workers leave endogenously at attrition rate σ. The firm discounts the future at a rate of $\delta = \frac{1}{1+r}$ per period, where r is the interest rate.[29]

The model thus far (with fixed $\underline{h}$) is analogous to Nickell (1986). The first order condition with respect to N_t sets the marginal discounted net revenue stream of an additional hire today equal to the hiring cost: $pF_N(N_t, \underline{h}) = w\underline{h} + \alpha(r + \sigma)$. We denote N^* as the solution to this first order condition: $pF_N(N^*, \underline{h}) = w\underline{h} + \alpha(r + \sigma)$. This optimal flow condition sets the marginal revenue from an additional worker *above* the worker's marginal cost ($w\underline{h}$), with hiring costs driving the wedge between the two. Naturally, the wedge is larger for higher hiring costs and attrition rates. If $N_0 < N^*$ the firm hires immediately to N^* and thereafter hires to replace exits at $a_t = \sigma$ for all t. If $N_0 > N^*$ the firm lets attrition take its employment down until next period's employment would first fall below N^* at which point it hires to get to N^*. Thereafter, it hires to replace exits.

Modeling the reform. We model the mini job reform in this context as a permanent shock occurring in period τ, anticipated by 1 period.[30] Starting in period τ, the firm can now opt to increase all of its workers' hours from $\underline{h}$ to $\bar{h} > \underline{h}$ hours. We are interested in when the firm makes this discrete choice to increase hours.

Denote N^{**} as the optimal steady state employment level under the higher hours level $\bar{h}$. N^{**} will be lower than N^* and this steady state is strictly preferred to the steady state at N^* under $\underline{h}$. This is a natural consequence of relaxing the firm's hours constraints.

[27] Gains from adjusting to the reforms are at most €75 per month less the utility costs of additional work hours, a modest sum relative to household income for the married women we study. This is especially the case if the worker expects she might be able to adjust at her existing job in the near future.

[28] Over 94% of 1-year adjustment and over 85% for non-adjusters in the first year who adjust in the second post-reform year occurs within a worker's initial establishment.

[29] The production function satisfies standard conditions: $F_N(N,h) > 0, F_{NN}(N,h) < 0, F_h(N,h) > 0, F_{hh}(N,h) \leq 0$. Moreover, we require that any complementarity between N and h is not too strong ($F_{NH}(N,h) < \frac{w}{p}$). This ensures that the firm prefers lower employment at higher hours levels. In our simulation we use the simple function $F(N,h) = \phi ln(N \cdot h)$.

[30] In practice the 2003 reform was passed 3 months before its implementation and drafted only 1 month earlier, allowing limited scope for anticipatory responses.

The fact that the firm prefers the new steady state does not imply that the transition to this new steady state is costless (or even always worth it).[31] Indeed, the optimal transition path starting at N^* in period τ typically involves reducing hiring and waiting for attrition to bring employment closer to N^{**}. Hours will generally be increased at some intermediate time period, which minimizes costly deviations from each of the two steady states.

A simulation exercise. We simulate the firm's optimal behavior using backward induction over a finite grid space for a set of plausible parameters.[32] Appendix Figure A.2 panel (a) shows the optimal employment size in each year if starting at $N_0 = N^* = 5.1$. Unsurprisingly, the firm remains at $N_t = 5.1$ for all t by replacing hires at the rate $a_t = \sigma = 0.05$. Panel (b) shows the optimal adjustment path in response to a period τ reform (indexed by 0) that allows the firm to increase hours to $\bar{h}$. We assume the firm learns of the reform in year $\tau - 1$. The firm now chooses N_t in each period and when (and if) to optimally increase hours from $\underline{h}$ to $\bar{h}$. We see that the optimal strategy involves slowing down hiring in period $\tau - 1$ in anticipation of the reform. The firm then ceases hiring in periods τ to $\tau + 2$, letting attrition draw down its work force. In period $\tau + 2$ the firm increases its workers hours to $\bar{h}$, but employment remains at $N_{\tau+2} > N^{**}$ and the firm continues to not hire. In the following period, the firm resumes hiring. It reaches the new steady state employment level (N^{**}) in period $\tau + 4$, and returns to hiring at the rate $a_t = \sigma$ to replace exits. This firm's workers thus face a 2 year earnings adjustment delay (as shown by the shaded region). The model captures the idea that under a fixed output price the firm does not always benefit from immediately increasing hours.

We next consider non-stationary environments in which the firm is growing or declining in order to show how heterogeneity in growth trends can generate heterogeneity in the speed of adjustment. In Figure A.2 panel (c), we use the same parameters as before but have the firm facing an exogenous, declining output price over the reform period. Specifically the price starts steady at €80, as before, and then declines by €5 per year until it reaches €55, remaining steady thereafter. Panel (c) shows the optimal choice of N_t in each period absent a reform. As would be expected, the firm reduces hiring in response to the negative output demand shock and allows its employment size to decline. Panel (d) shows the optimal response to the reform. The firm still finds it beneficial to adjust eventually (in period $\tau + 6$), but only after prices have stabilized and employment has declined sufficiently.

Panel (e) performs the same analysis for an increasing output price. The firm actively hires

[31] Generally, making the transition is preferred to not, but there are parameter values (such as sufficiently low σ) that make the transition not worthwhile.

[32] Specifically, we let the time period be a year, the interest rate $r = 0.04$ ($\delta = 0.961$), $w =$ €10 per hour, attrition $\sigma = 0.05$, $\underline{h} = 32.5 \cdot 12$ so that mini jobbers would be at the threshold in 2002, and $\bar{h} = 40 \cdot 12$, the new threshold following the 2003 reform. We choose $\alpha = 200$, or 4.2% of annual earnings, within the range that Nickell (1986) uses for blue-collar workers. We set the production function to $F(N_t, h_t) = 250 \ln(N_t h_t)$ and the output price $p = 80$. We choose a logarithmic specification as we have no strong reason to think the cross partial with respect to N and h should be positive or negative.

in response to the output demand shock. Panel (f) shows how this firm responds to the reform. As expected, this growth makes adapting to the reform easier. In period $\tau - 1$, when the firm first becomes aware of the reform, the firm reduces its hiring, saving on hiring costs, knowing that it will increase $\bar{h}$ in the next period. As soon as the reform passes, the firm increases hours immediately and returns to hiring.

Discussion. These simulations show that when workers are unlikely to leave their establishments it can be optimal for establishments to delay adjustment. In this model, firms adjust by reducing hiring and eventually increasing hours at the now lower number of mini jobbers. Growing firms will generally have speedier adjustment, while workers at declining firms will experience the longest delays before seeing their hours increased. Thus, heterogeneity in labor demand across firms can lead to heterogeneity in worker's earnings responses to policy reforms.

While the model makes strong assumptions, the link between firm level labor demand and adjustment will hold under a variety of alternative assumptions. For example, it need not be the case that all workers increase their hours post-reform. It could be that some face individual level costs, such as rescheduling costs, and choose not to adjust when offered the chance. This would increase hiring in adjusting establishments, and allow others to adjust quicker, but we would continue to see establishments with insufficient labor demand delaying adjustment. Another reasonable extension would allow establishments to implement continuous hours changes. This would always generate immediate partial adjustment in our stationary environment, but the contrast between growing and declining firms would continue to hold.

4 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, 2019). The data contain earnings and employment duration for all jobs covered by the social security system.[33] 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 detailed establishment-level information from the worker-level data, such as employment size, total wage bill, hires, and exits, 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. We also have a standard list of controls covering industry, occupation, and employee demographics.

[33] Around 80% of all jobs fall within the social security system (the main exceptions being the self-employed, and civil servants). See Jäger and Heining (2019) for a comparison to the census.

In order to focus on a group of individuals that face a common budget set, our baseline sample consists of married women whose husbands have similar income levels. We obtain a sample of married women in 2008 using the method outlined in Goldschmidt et al. (2017).[34] 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.[35]

Married women sample. Our primary sample consists of the earnings records from 1999-2015 for the married women we identify, aged 26-55, residing in West Germany, that do not receive any form of unemployment assistance.[36] 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. In addition, we focus on married women whose spouses earn between €33,000-53,000 per year. 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 and hence marginal income tax rates cannot be accurately calculated. The lower limit ensures that the household marginal income tax rate is relatively stable (see Appendix Table C.4). For this sample of married women, we can be confident in the size and stability of the notch. The average annual sample size is 567,056 women, of which 208,779 have monthly incomes below €1000. Appendix E contains additional details and summary statistics.

Establishment sample. After presenting earnings responses to the reforms for the preceding sample, we turn our focus to demand-side mechanisms at the establishment level. We analyze the establishments that employ the aforementioned married women. Specifically, for the first reform we study all the establishments that employed low-earnings married women (below €1400 a month) in 2002 or January-March 2003. For the second reform we study all establishments that employed low-earnings married women in 2012.[37] For these establishments, we construct establishment-level variables using *all* workers at the establishment, not just married women. We study the adjustment of *all* workers at these establishments presumably constrained by the notch, focusing

[34] 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.

[35] As such, this is not a representative sample of all married women — the women in our sample and their husbands need to appear in the data (as employed or unemployed) in 2008. Nevertheless, earnings histograms look similar for a random sample of all women 26-55 in West Germany and our reduced form results apply to all workers at a given establishment, not just married women.

[36] Unemployment benefits are reduced at earnings levels below 400, generating distortions in the earnings distribution. We therefore 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.

[37] Our key reduced-form specifications look very similar for a 25% random sample of establishments employing mini jobbers, available on request.

on exclusive mini jobbers with earnings close to the notch in the pre-reform year and multiple years of experience in this mini job (see Appendix E.2).

Additionally, we draw on several supplementary data sources including our own small survey of mini jobbers (see Appendix D), which we introduce as appropriate. Appendix E.3 contains an exhaustive list of data sources.

5 Earnings Responses to the Mini Job Reforms

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

Having shown individual level variation in adjustment speeds, we establish some preliminary facts about the role establishments play in this process. The data supports the idea that the adjustment process is not a purely individual phenomenon — establishment level factors, broadly speaking, appear to affect the speed of adjustment. We turn to the role of firm labor demand *per se* in Section 6.

5.1 Earnings Responses

Earnings Responses in the Cross-Section. Figures 2—3 show histograms of average monthly earnings (from all employment sources) by year for married women in our baseline sample.[38] 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.[39]

Figure 2 panel (a) shows that in 2002 there is clear and extensive bunching at the monthly earnings threshold of €325. There is no comparable bunching anywhere else in the earnings distribution (see Appendix Figure A.3 for the full distribution). Three notable changes take place after the 1 April 2003 reform (see Figure 2 panel (c)). First, a sizable fraction of mini jobbers adjust immediately to the new earnings level of €400. Second, a number of mini jobbers now earn between €325 and €400 on average. These could either be workers adjusting to €400 at some

[38] An individual's average monthly wage is calculated by taking total earnings from all employment sources over the period divided by the number of distinct days worked multiplied by 365/12 or 366/12 in leap years. We sum earnings across all jobs, as the tax notch applies to that sum.

[39] The vast majority of mini jobs that spanned this period are indeed split into two entries in our data. Regular jobs, and the few mini jobs that are not split, are split manually, assigning the average wage over all of 2003 to each of 2000 (Jan-Mar) and 2003 (Apr-Dec). This means raises before the reform cannot be distinguished from raises afterwards for these entries, but for regular jobs this is relatively inconsequential and does not appear to affect the distribution of jobs right of the threshold.

point after April but before December 2003 or workers who adjust instantly to an intermediate earnings level.[40] Third, a clear, excess mass of workers continue to earn €325 for the duration of the year. 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 (see Figure 3), where we see even more excess mass at the old notch, 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.

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 most of the individuals who adjust on the intensive margin had earnings close 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 close to €325 notch adjust upwards after the reform (April-December 2003).[41] Adjustment rates follow the expected pattern: they are low for workers with earnings relatively far from the threshold.[42] A similarly clear pattern emerges for the 2013 reform, where 39% of women at €400 adjust following the reform.

Hours increases. Figures 2, 3, and 4 document real earnings responses along the intensive margin. While these increased earnings could come from changing jobs or adding new jobs, the vast majority of these changes occur within establishment.[43] Within establishment, earnings increases could arise from hours increases, wage increases, or a mix thereof. For example, a worker earning €10 per hour could increase her monthly earnings from €400 to the 2013 threshold of €450 either by increasing her monthly work hours by 5 or by negotiating a wage increase from €10 to €11.25.

Using establishment-reported data on hours available for 2011-2014, we show that at least half of the adjustment comes from increased hours.[44] 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.[45] 40% of the adjusters experience

[40] The first type of behavior is most likely as the majority of these observations appear at the threshold in following years.

[41] 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, to accommodate small deviations from Z arising from leap years and differing period lengths.

[42] We also note that workers from higher up in the distribution are not moving downwards at high rates, with the possible exception of (the comparatively few workers) in a region around 420-480. Movement rates to earnings of around 400 fall permanently below 3% for high earning workers (above €700).

[43] Table 1 Panel A tracks married women in a single mini job with earnings close to the pre-reform notch. 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.

[44] This hour information has historically been collected for the purposes of occupational accident insurance. For the years 2011-2014 — and only these years — it was integrated in the reporting process of our administrative data.

[45] Specifically, we focus on married women earning between €362.5–412.5 in 2012. Women are classified as stayers if they remain in the range of €362.5–412.5 and have an absolute earnings change below €20. Women are classified as adjusters if they move up between €412.5–462.5 and have an earnings change larger than €30.

positive hours increases (compared to 14% for stayers) with a modal increase of 4–5 hours a month. Thus a large share of the adjustment on earnings comes at least partially through increased hours.

This figure also suggests that a meaningful share of adjusters (49%) increase earnings through a pure wage increase, without reported changes in hours. However, this should be interpreted cautiously and as an upper bound, as establishments may not report all hours changes. Figure 5 Panel (b) focuses on larger establishments, which among other differences may be more likely to report changes accurately. Here, 52% of adjusters experience positive hours increases and only 35% increase earnings without any changes in hours. Overall, we conclude that *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 instead of €50), that adjustment on hours would have been even more prevalent.

5.2 Quantifying Delayed Adjustment

We have seen that earnings, and in many cases hours, increase following the tax change. In other words, the intensive margin elasticity of earnings with respect to the tax rate is positive. And yet a considerable fraction of the workers we expect to adjust do not do so in the first few years. Based on a standard labor supply model, we would expect the entire excess mass at the old notch to dissipate after the reform. This happens eventually (see, for example, the earnings distribution around €325 in 2014 in Figure 3 panel (e)), but it takes time. To get a better sense of these quantities, we fit a counter-factual earnings distribution to the earnings data in Figures 2—3 using the standard approach in the bunching literature (Chetty et al., 2011; Kleven and Waseem, 2013; Kleven, 2016).[46]

Figure 6 plots the percentage of the initial bunching mass remaining at the old notch after each reform.[47] 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.

Table 1 provides a complementary way to explore these patterns. Here we take all married women in a single mini job with earnings close to the pre-reform notch 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 close to the old notch. 21% (16%) of these

[46] Specifically we fit a fifth degree polynomial to the data 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 Appendix Figures A.7—A.8.

[47] Some excess mass remains even in 2012 due to imprecision in bunching at the new notch (see Figure A.7 panel (k)). In order to concentrate on those who remain at the old notch for other reasons, we subtract this amount from all bunching masses.

women are still at the old notch in 2005 (2015). Panel B further restricts to the subset of married women who remained stuck at the old notch in the first post-reform year. Adjustment rates for these women in 2004 (2014) decline to 24% (26%), but remain non-negligible. Repeating this exercise for the non-adjusters in both 2003 and 2004, yields a 2005 adjustment rate of 19%.

The observed delays in earnings responses following each reform are a general feature of the economy and are not particular to a handful of regions, industries, occupations, or demographic groups. Appendix Figures A.4—A.5 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. Appendix Figure A.6 demonstrates this by constructing Figure 6 for each of these different subsamples. While these reveal some interesting heterogeneity in adjustment speeds, the presence of non-negligible delays in adjustment is a robust feature across settings.

Overall, a sizable fraction of workers are subject to delays in adjusting earnings. While adjustment occurs at meaningful rates in the first several years after each reform, what happens in the first year is crucial: those that do not adjust in the first year can take considerably longer to increase their earnings.

5.3 Implications for Earnings Elasticities

The observed delays in adjustment have implications for the 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 C uses bunching techniques to explicitly quantify these differences in our context, finding a long-term, intensive margin elasticity of earnings with respect to the net of tax rate of 0.33. A 1-year difference-in-difference inspired estimate yields an elasticity half this size. Additionally, in Appendix C.2 we use Gelber et al. (2020)'s method to simultaneously estimate elasticities and adjustment costs.[48]

5.4 Some Initial Facts Regarding the Role of Establishments

Before turning to the specific role that establishment labor demand plays, we set the stage by presenting three empirical facts relating to the role of establishments in the adjustment process more generally: i) Much of the adjustment behavior within the first few years occurs within establishments, as opposed to adjusting by moving to a new establishment. ii) Within-establishment

[48] Depending on the specification, we estimate 'long-term' intensive margin elasticities ranging from 0.23-0.33 and adjustment costs of €415-483 in the first post-reform year that dissipate over time.

factors explain up to 28% of the variance in adjustment. iii) Many establishments, including establishments with a large number of mini jobbers, have no mini jobbers that adjust in the first year.

i) Most adjustment occurs within establishment. Table 1 Panel A shows that an extremely large share of the 1-3 year adjustment occurs within establishment, as opposed to adjusting by getting a new job. Panel A tracks married women in a single mini job with earnings close to the pre-reform notch. 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. Panel B looks at those married women who did not adjust in the first perform year. Even among these women, 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 as opposed to through a standard search channel.

ii) An establishment effect explains around 28% of the variance in adjustment. To further examine within-establishment correlation in adjustment, we turn to our *establishment sample*, which is comprised of all workers (including men and non-married women) at the establishments employing the married women in our baseline sample. Using all workers at these establishments, we construct several establishment level covariates. To study adjustment, we focus on workers with a single mini job with earnings close to the notch in the pre-reform year. In order to more precisely capture workers who are likely constrained by the notch, we further restrict to workers who have been at the establishment in a mini job for three consecutive years. Our dependent variable takes the form of an indicator for increasing earnings above the old notch (and below the new notch) in the first post-reform year.

Establishments explain a meaningful share of the variation in this measure of individuals' adjustment rates. If establishments are the sole determinant of adjustment and treat all mini jobbers at the notch equally, then adjustment rates would be either 0 or 100% for all mini jobbers in any given establishment, yielding a within-establishment correlation of 1. At the other extreme, if individuals are randomly sorted to establishments and have their own heterogeneous propensity to adjust, say because of preferences or adjustment costs, then within-establishment correlation in adjustment should be 0. In practice, an establishment random effect explains about 28% of the variation in adjustment in the first year. This can be seen in Table 2 where we estimate a random effects model and report the intraclass coefficient ρ, which measures the proportion of the total variance in 1-year adjustment rates that falls between-establishments.[49,50] Table 2 columns (1) and

[49]Specifically for tenured individuals i at the notch in 2002/2012 establishment e, we obtain an establishment random effect ν_e by estimating Adjustment 03(Apr-Dec)/13$_{i,e} = \alpha + \nu_e + \epsilon_{i,e}$. We assume $E[\epsilon_{i,e}|\nu_e] = 0$, $V[\nu_e] = \sigma^2_\nu$, and $V[\epsilon_{i,e}] = \sigma^2_e$. We estimate the model via feasible GLS and report the intraclass coefficient $\rho = \frac{\sigma^2_\nu}{\sigma^2_\nu + \sigma^2_\epsilon}$.

[50]In principle, one could also use a fixed effect and calculate the within-R^2. Doing so points to an even greater role for within-firm correlation (establishment fixed effects explain over 45% of the variation in adjustment rates), but suffers from over-fitting due to the many firms with just one mini jobber. The more parsimonious random effects model avoids overparametrization.

(3) show that ρ is 0.275 for the first reform and 0.283 for the second. We benchmark the estimates by comparing them to the share of the variance due to between variation of other groups — 2-digit occupations, municipalities, industries, and their cross products — each estimated separately.

Of course, part of our estimate of ρ could be driven by workers sorting to particular establishments in a manner correlated with their adjustment propensity. There is little indication that this is a severe issue based on observable variables. The inclusion of worker level controls does very little to move the estimate of ρ.[51] We address sorting on unobservables more explicitly in subsequent sections. Overall, the within-establishment correlation in adjustment rates suggests scope for the importance of establishment-level factors.

iii) Establishments in which no one adjusts are common. We can visualize the within-establishment correlation in adjustment by grouping establishments by the number of mini jobbers in the proximity of the original threshold in the year prior to the reform ($\tilde{N}$) and plotting the share of establishments of size $\tilde{N}$ with exactly 0 workers adjusting, exactly 1 out of $\tilde{N}$ adjusting, exactly 2 out of $\tilde{N}$ adjusting, and so forth. Figure 7 plots this distribution for selected values of $\tilde{N}$ in dark, shaded grey.[52] We contrast this distribution to the expected distribution of adjustment across establishments if workers were independently and identically distributed across establishments with adjustment propensity $p(\tilde{N})$, where $p(\tilde{N})$ is the average adjustment rate across establishments in this size category.

The most striking pattern is the over-prevalence of establishments with no adjustment (above 18% for all establishment sizes shown, even for establishments with 10 mini jobbers). We also note an over-prevalence of establishments where most workers adjust. These figures are consistent with establishments playing an important role in the adjustment process, perhaps by refusing to accommodate any adjustment or facilitating adjustment for all. Taken together, the facts in this section motivate the question: which establishments are conducive to adjustment?

6 Evidence of the Role Firm Labor Demand Plays in Adjustment

In this section, we turn to the role that firm labor demand plays in mediating individual's earnings responses following each reform. We concentrate on mini jobbers at the pre-reform notch and use three reduced form strategies to link their 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. We conclude with a brief discussion of the quantitative importance of labor demand for adjustment.

[51] The included controls are monthly wages and days worked in the 2 years prior to the reform, tenure, occupation tenure, age, education years, gender, a dummy for German nationality, and a dummy for working in more than one firm in each of the 2 years prior to the reform.

[52] Omitted values of $\tilde{N}$ portray the same picture.

6.1 Individual Adjustment and Establishment Labor Demand: Three Tests

We expect establishments with sufficient labor demand to allow (and perhaps encourage) all their workers to adjust.[53] Workers may or may not oblige, but regardless, the situation at such establishments should differ from that at establishments without demand for additional hours, which may simply not accommodate hours or earnings increases.

In order to test this hypothesis, one would ideally 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. We instead approach the problem using three distinct empirical approaches. We present each strategy and corresponding findings in turn, after which we jointly address key concerns like worker sorting.

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 wish to do so to increase their hours (as in Section 3.1). 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 allow partial) adjustment, and should not hire new workers.[54] In equilibrium, we expect a positive association between between adjustment rates of pre-existing workers at the notch and an indicator for any new mini hires post-reform.

We test for this and subsequent associations using our *Establishment Sample* (Section 4). This sample takes as a starting point *all* mini jobbers at establishments employing the married women in Figures 2—3 in pre-reform years. In order to concentrate on workers presumably constrained by the notch, it further restricts to mini jobbers with monthly earnings close to the threshold in the year prior to each reform, whose only job in this year was a 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).

[53] To fix ideas, consider an establishment with N workers working for €8 an hour, 40 hours a month. Suppose that post-reform all workers now want to earn €400 (instead of 320). If wages are fixed, this requires giving each worker 10 additional hours. In other words, instead of hiring a new worker to work 40 hours, the establishment could allow four workers to adjust. In this example, a firm that typically would have hired $\frac{N}{4}$ or more workers absent the reform could accommodate full adjustment by reducing hiring.

[54] There are multiple reasons that an establishment might nevertheless hire a worker, say to fill a particular occupation. Moreover, we expect some establishments to allow adjustment but not require an additional hire. As such, this test is biased towards zero.

For tenured mini jobbers at the pre-reform notch (i) at establishment (e), we regress

$$\text{Adjustment}_{i,e,T} = \alpha + \beta_T \text{Any Mini Hires}_{i,e,T} + \sum_{t=\{-2,-1,1,2\}} \beta_{T+t} \text{Any Mini Hires}_{i,e,T+t} + \gamma \mathbf{X}_{i,e,T-1} + \epsilon_{i,e,T} \quad (1)$$

Year T corresponds to the first post-reform period (April-December 2003 or, separately, 2013). Adjustment takes the form of an indicator for increasing mini job earnings above the old notch in the first post-reform period at this establishment. $\mathbf{X}_{i,e,T-1}$ is a vector of pre-reform controls measured in the year prior to the reform (2002 or 2012). 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.[55] While these controls potentially absorb valid identifying variation, we include them throughout to emphasize the role of idiosyncratic differences in labor demand for establishments of similar size in the same industry and location.

Lastly, in order to help 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 Hires in our baseline: Any Mini Hires$_{i,e,T+t}$ for $t = \{-2, -1, 1, 2\}$. 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.[56] We also present results without these leads and lags in Appendix Table A.1. Throughout, standard errors are clustered at the establishment level.

Our central prediction is that we expect post-reform hiring to be associated with adjustment: $\beta_T > 0$. Since some establishments might accommodate adjustment in the first post-reform year without hiring and then resume hiring next year, Any Mini Hires 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.[57]

Column (1) of Table 3 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

[55] 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.

[56] 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.

[57] It could be that establishments with a hire are simply on a different growth path than others. To the extent that any hires (a somewhat incomplete measure of overall growth) in the past, conditional on post-reform hires, better captures establishments on differentially positive growth trends, we might expect $\beta_{T-1} > 0$. If however, potential trend differences are already captured by post-reform hires or controls we would expect $\beta_{T-2} = \beta_{T-1} = 0$.

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 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) and (3) add additional pre-reform controls. In column (2), we add a vector of individual level controls.[58] In column (3), we further add establishment level controls.[59] Results move relatively little with the progressive inclusion of controls, but to the extent that they do, they get larger and clearer. Panels (A) and (B) in Figure 8 plot the regression coefficients from column (3) and (6) respectively.

Table 3 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 3 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 for the first time 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

[58] Specifically, controls include 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).

[59] Specifically, we include 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).

association between hiring behavior and adjustment arises for reasons unrelated to labor demand that are difficult to control for.[60] 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.[61] 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 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 captures both changes in the number of workers as well as increases in earnings (and hence potentially hours). 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 Appendix Table A.4. 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, $\Delta Ln(\textit{Total Non-Mini Earnings})$:

$$\text{Adjustment}_{i,e,T} = \alpha + \sum_{t=-2}^{2} \beta_{T+t} \Delta Ln(\textit{Total Non-Mini Earnings})_{i,e,T+t} + \gamma \mathbf{X}_{i,e,T-1} + \epsilon_{i,e,T} \qquad (2)$$

Table 4 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 and differentially so relative to the pre-periods. A one standard deviation increase (0.22) in non-mini earnings growth is associated with 8% increase in adjustment relative to the mean. As with the any new mini hire results, this (differential) association is robust to the inclusion of our battery of individual and establishment controls. Unlike with the any mini hires results, pre-reform non-mini growth is 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. 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 adjust-

[60] For example, it could be that seeing a new mini hire at the new notch transmits information to pre-existing workers about the reform.

[61] 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. 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.

ment relative to the mean. The coefficients from columns (3) and (6) are plotted in Figure 8 Panels (C) and (D) respectively.

Table 4 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 4 provides further evidence that establishment labor demand, this time measured 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 firm 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.

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 firm as well as detailed industry and region dummies.[62] 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 and less easily characterized set of predictors.

[62]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 firm (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 firm. Furthermore, the model can include any relevant 5-digit industry dummies, county dummies (400 regions), or interactions between 3-digit industry and county dummies.

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 when using a longer time series as well as when we train our model in 2011 instead of 2012. We do so 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 5 shows that our 'first stage' fit 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 (the only exception is the second lag of non-mini-jobber growth for the 2002 prediction) used in the OLS approach.

Next, we use our model to predict hiring in 2003 and 2013. That is, we apply the coefficients 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 — Frac. $\widehat{\text{New Mini}}$ Hires$_{i,e,T}$ — and our usual measure of individual adjustment:

$$\text{Adjustment}_{i,e,T} = \alpha + \beta \text{Frac. } \widehat{\text{New Mini}} \text{ Hires}_{i,e,T} + \gamma \mathbf{X}_{i,e,T-1} + \epsilon_{i,e,T} \tag{3}$$

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

Table 5 examines the association between our predicted fraction of 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 controls as prior Tables. 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 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 significantly attenuated relative to predicted hires. This downward bias is expected: it is consistent with high adjustment rates in establishments that are able to adjust pre-existing workers hours by reducing hiring. Overall the results in Table 5 support a link between an individual's post-reform adjustment propensity and their establishments' labor demand.

6.2 Robustness and Additional Results

Taken together, Tables 3—5 are consistent with establishment labor demand affecting adjustment speeds. Nevertheless, it is helpful to address several competing explanations 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 3—5). Moreover, Oster (2017)'s test for selection on unobservables suggests an implausibly large degree of sorting on unobservables required to overturn results.[63] 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. Finally, Appendix Table A.2, leverages the fact that we have two reforms to 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 3—5 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. While the effect of non-mini earnings growth is no longer detectable, the individual fixed effects strengthen the association between adjustment and the labor demand proxies in panels A and C.

Second, we consider whether establishment level omitted variables other than labor demand could account for the observed results. For example, two firms that would benefit equally from adjusting their workers immediately could be differentially covered by collective labor agreements, allowing one to both hire and adjust immediately, while forcing the other into negotiations before adjusting or hiring. The inclusion of a battery of establishment level controls – including 5 digit industry codes which should capture much of the variation in collective labor agreements – in columns (3) and (6) does little to move coefficients of interest. Even after including our full suite of individual and establishment controls, 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) in Appendix Table A.2 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.

Of course, it remains possible that *time-varying* omitted variables could influence both our establishment labor demand proxies and individual adjustment (though it is difficult to explain all three sets of results away with a single alternative explanation). One potential competing expla-

[63]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 3—5, suggesting we need an order of magnitude more selection on unobservables than observables to nullify our results.

nation involves differential knowledge about the reform (Chetty et al., 2013). For example, better informed establishments employing informed individuals may decide to make structural changes to their workforce as a result of the reform (e.g. shift towards more mini jobbers). This could create a contemporaneous correlation between hires or hours changes and adjustment rates that — if not fully captured by our controls or establishment fixed effects — could confound our tests. Several additional results suggest this is likely not a major concern. As described in Section 2, establishments with pre-existing minijobbers were informed about the reform. This is supported by our own survey where only 11% of respondents said information was an impediment to adjustment (Appendix D). Additionally, Appendix Tables A.3 – A.5 show that the association between labor demand and adjustment holds across a range of establishment sizes – both in the sense of more mini jobbers and more total employees — and for all three labor demand proxies. Importantly, they hold for large establishments and establishments with many initial mini jobbers, where we expect awareness of the reform to be greatest.

Overall, our findings are consistent with our conceptual framework and proposed channel: establishments with insufficient labor demand delay adjustment. Two final results further bolster this claim. First, our conceptual framework suggests that establishments with low labor demand might delay *all* adjustment. Figure 7 shows that it is indeed (overly) common to observe no one adjusting within an establishment. To investigate whether labor demand proxies are associated with zero adjustment, Appendix Tables A.6-A.8 estimate establishment-level analogs to the regressions presented in Tables 3 and 5 replacing the dependent variable with a binary indicator for *any* adjustment at the establishment.[64] Consistent with our conceptual framework, our labor demand proxies are generally positively associated with this extensive margin measure of adjustment. Second, our conceptual framework revolves around adjustments on the hours margin but the data generally limit us to studying adjustment in terms of earnings. For the 2013 reform we are able to examine whether our labor demand proxies predict adjustment on the hours margin. Recall that Figure 5 documented that adjustment is often accompanied by recorded changes in hours, particularly in establishments with at least 10 mini jobbers where hours might be recorded more reliably. In Appendix Table A.9 we show that our labor demand proxies reliably predict 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). Effects are particularly evident in establishments with at least 10 mini jobbers (columns (4)-(6)).

[64] 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.

6.3 Magnitudes and Discussion

In this subsection we discuss the quantitative importance of labor demand in mediating adjustment. For several reasons, a definitive quantification of this channel is challenging.[65] Accordingly, we focus on presenting suggestive evidence of this channel's quantitative importance. In a last exercise, we tie Section 5 and 6 together by revisiting how the speed with which the excess mass at the pre-reform notch dissipates varies across establishments with differential labor demand. A simple back of the envelope calculation suggests that establishment labor demand explains a quantitatively meaningful portion of the observed adjustment delays.

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 3 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 3 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. Appendix Figure A.9 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 9 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 3, 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. This 15 percentage point difference is large. By way of comparison, recall Appendix Figure A.6 explored how the excess mass at the old notch varies across individual and job characteristics. The difference we see based on labor demand is in line with the largest differences across occupations or industries. 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.

To place these numbers in their larger context, recall that Section 5.4 showed that Establish-

[65] First there is considerable reason to think the labor demand proxies used in Tables 3—5 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 would require additional structural modeling and associated assumptions that fall beyond the scope of this paper.

ments account for about 28% of the variance in adjustment rates. Furthermore, a simple back-of-the-envelope calculation suggests that establishments can plausibly account for around 25 percentage points of the excess mass at the old notch.[66] This helps contextualize the amount we can reasonably expect any single establishment-level factor could explain. Taken at face value, the 10-15 percentage points of the excess mass explained by establishment labor demand suggests that 40-60% of the total establishment level effect stems from a labor-demand related channel. Both on its own and as a share of the overall role establishments play, labor demand plays a meaningful role in mediating the speed with which workers adjust their earnings.

7 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 has ample 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 evidence supporting 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

[66] To see this, recall that Figure 7 showed that for establishments with 10 tenured mini jobbers at the notch, around 18—20% of establishments have no mini jobbers adjusting — far more than the 1% that would be expected if workers were independently and identically distributed across establishments. For the sample in Table 3, 28% of individuals are at establishments with no adjustment in 2003 and 35% of individuals are at establishments with no adjustment in 2013. If we are willing to make the assumption that establishments either accommodate or do not accommodate adjustment we can quantify how much excess mass at the old notch establishment-level factors might account for. Suppose 28% of individuals are in establishments that do not accommodate any adjustment and hence have 100% excess mass, while the remaining 72% of individuals are in establishments that accommodate adjustment. To reach the average of 38% excess mass in Figure 6, individuals at accommodating firms must account for around 14% of the excess mass (through individual-level adjustment frictions). Hence, if the 28% of individuals at non-accommodating establishments were at accommodating establishments, we would expect the fraction of excess mass to drop by $0.28 \times 86 = 24$ percentage points. The analogous calculation around the 2013 reform yields 26 percentage points.

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 adjustment 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 trouble preventing workers from reducing hours. 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 or industries 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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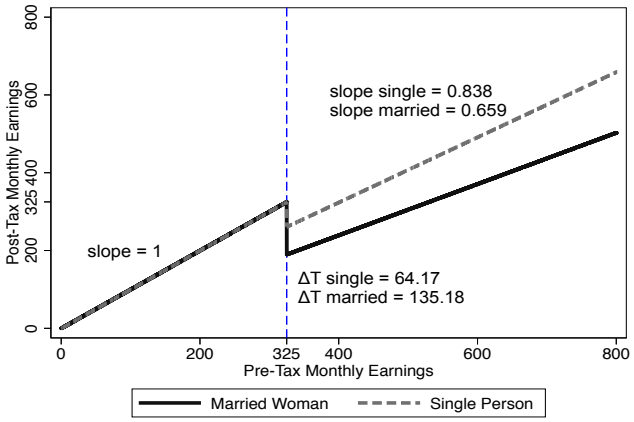
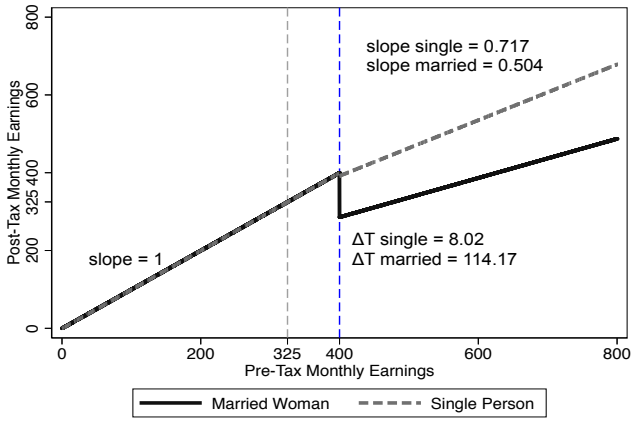
Figures

Figure 1: Tax Schedules

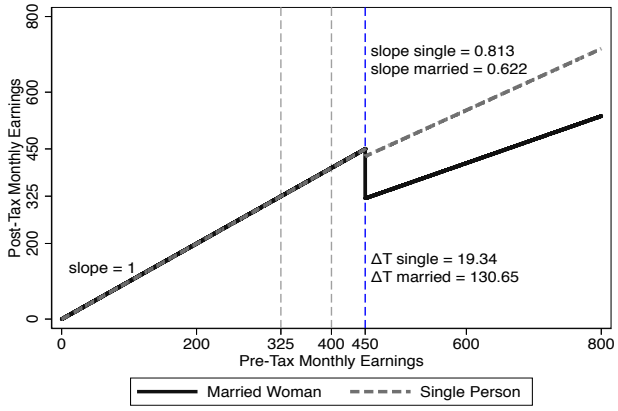
(a) 1999-Mar 2003



(b) Apr 2003 - 2012



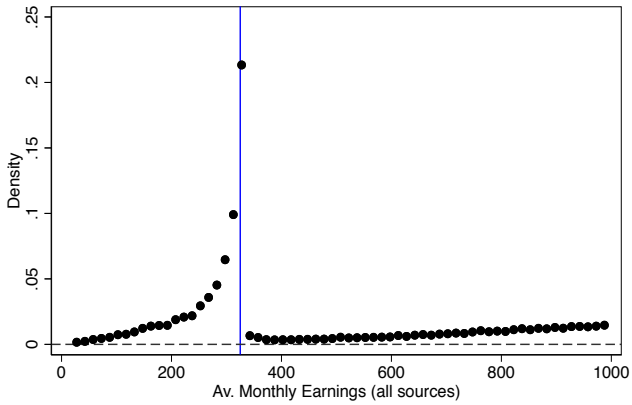
(c) Jan 2013 - Present



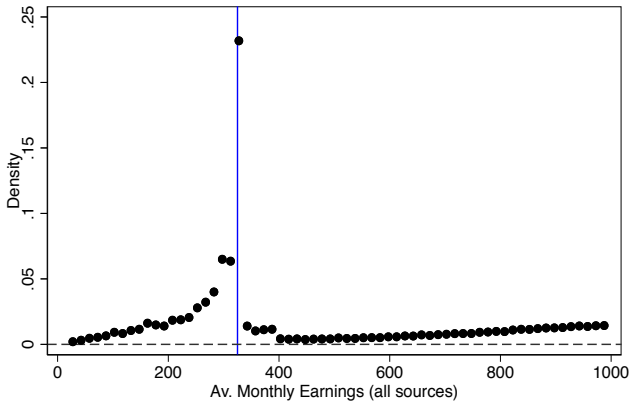
Notes: This figure displays the budget sets in 2002, 2003, and 2013 for single persons and for married women, assuming the husband earns €41000, the mean in our sample. The prevailing mini job threshold is drawn as a dashed blue vertical line, with defunct thresholds in gray. Single persons are assumed 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. All budgets are calculated taking into account differences in the firms' social security contributions between mini and regular workers, assuming 100% pass through to the worker. For further details on the budget set construction see Appendix C.3.

Figure 2: Earnings Distributions: Married Women 2002-2006

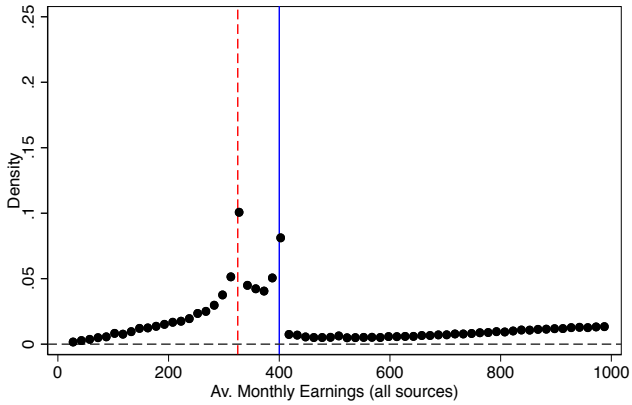
(a) Earnings Distribution 2002



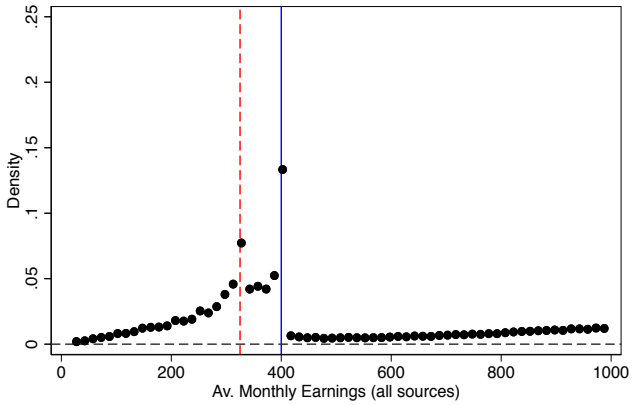
(b) Earnings Distribution 2003 (Jan-Mar)



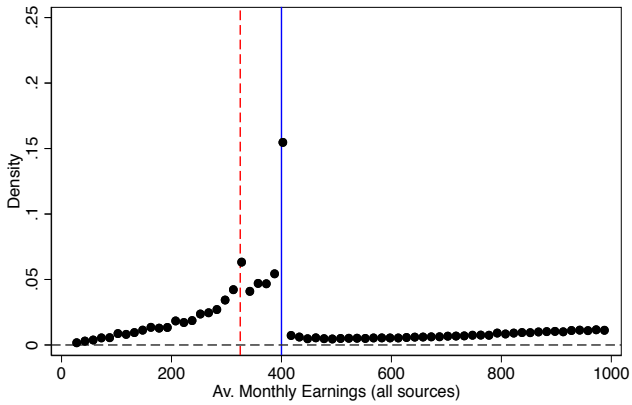
(c) Earnings Distribution 2003 (Apr-Dec)



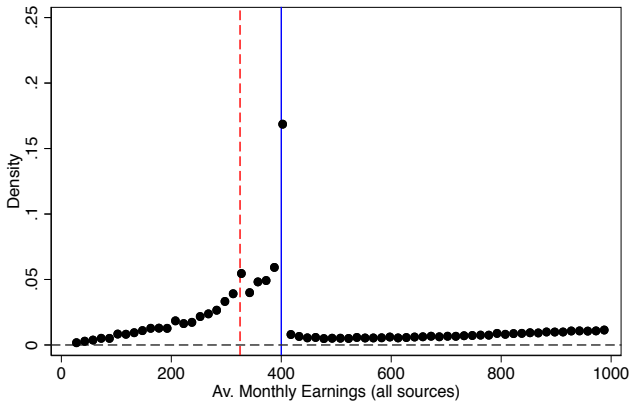
(d) Earnings Distribution 2004



(e) Earnings Distribution 2005

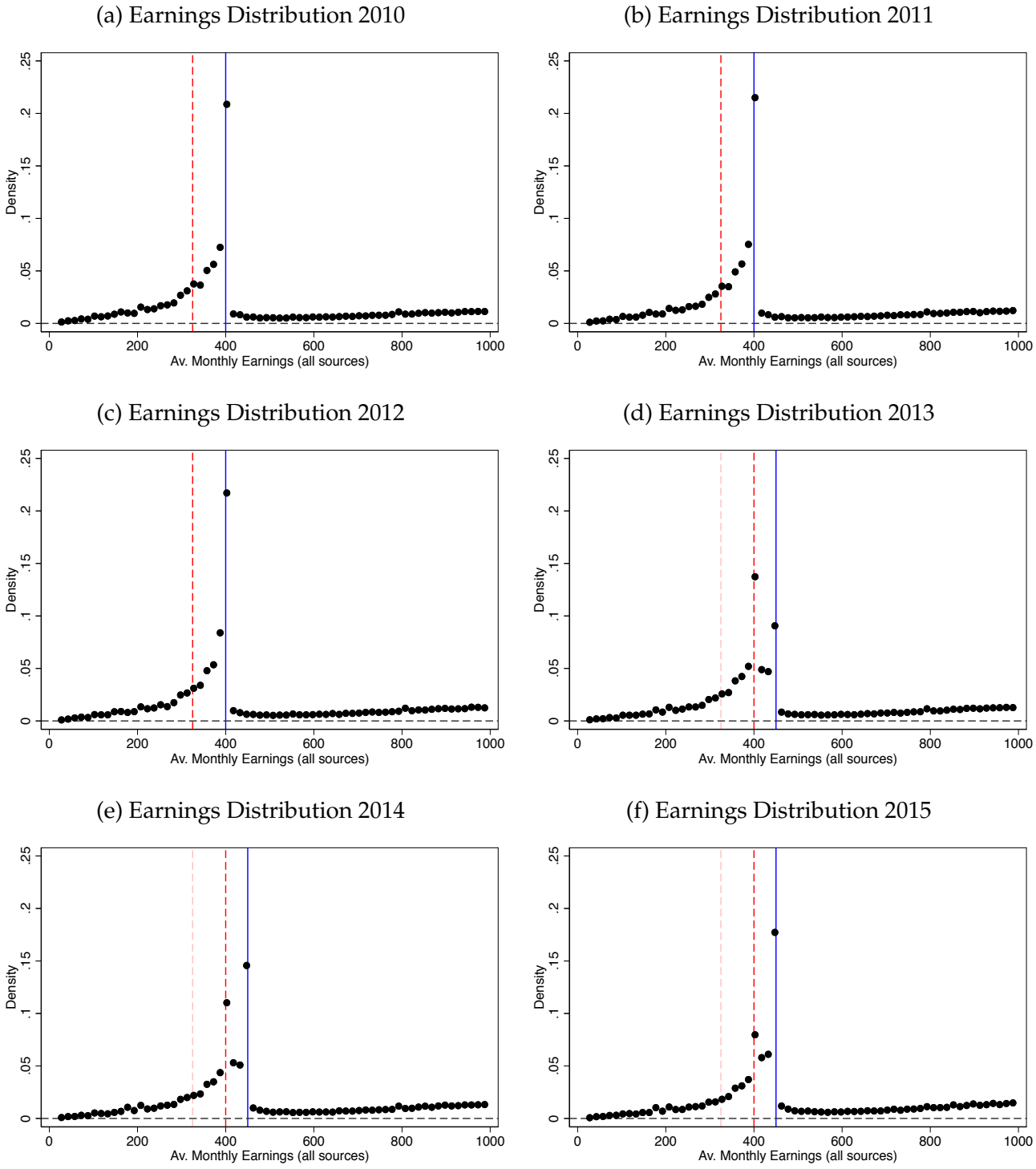


(f) Earnings Distribution 2006



Notes: This figure plots the distribution of duration-weighted, average monthly earnings (all employment sources) for the married women identified by our couple identification process between 2002 and 2006. We restrict to women working in West Germany, aged 26 to 55 (inclusive) whose husband's annual earnings falls between 33,000 and 53,000 euro and who do not receive any form of unemployment assistance. 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 euro 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 euro prior to April 1st 2003, at 400 euro prior to January 1st 2013, and at 450 euro thereafter.

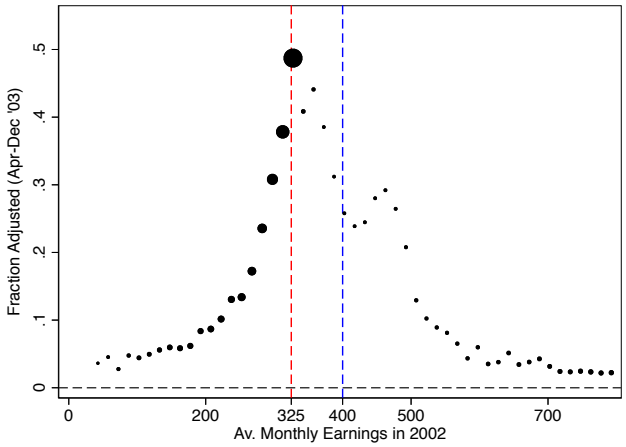
Figure 3: Earnings Distributions: Married Women 2010-2015



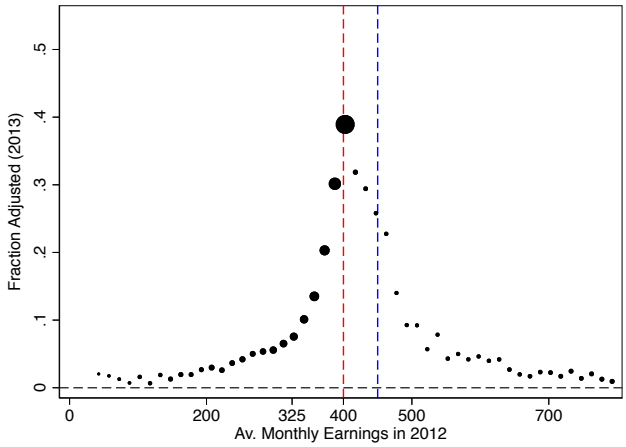
Notes: This figure plots the distribution of duration-weighted, average monthly earnings (all employment sources) for the married women identified by our couple identification process between 2010 and 2015. We restrict to women working in West Germany, aged 26 to 55 (inclusive) whose husband's annual earnings falls between 33000 and 53000 euro and who do not receive any form unemployment assistance. 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 euro 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 euro prior to April 1st 2003, at 400 euro prior to January 1st 2013, and at 450 euro thereafter.

Figure 4: Adjustment Rates as a Function of Initial Earnings

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



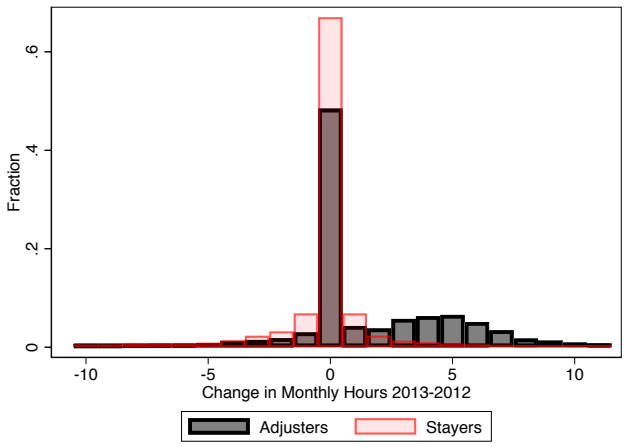
(b) Frac. Adj. in '13 by 2012 earnings



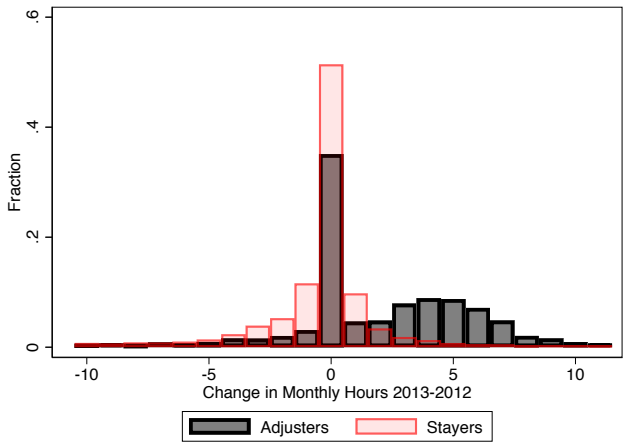
Notes: This figure plots average adjustment rates in the first post-reform year as a function of pre-reform earnings for the same sample as in Figures 2—3. Adjustment is defined as having average monthly earnings in the first post-reform year between $Z + 12.5$ and $\bar{Z} + 12.5$, where Z is the old notch and $\bar{Z}$ 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. Panel (a) shows average adjustment rates (based on earnings in Apr-Dec 2003) as a function of 2002 earnings. Panel (b) does the same for married women in 2012 adjusting to the new threshold in 2013. In each case most of the adjustment comes from workers close to the original notch.

Figure 5: Hours Changes Following the 2013 Reform

(a) Δ Hours for Adjusters

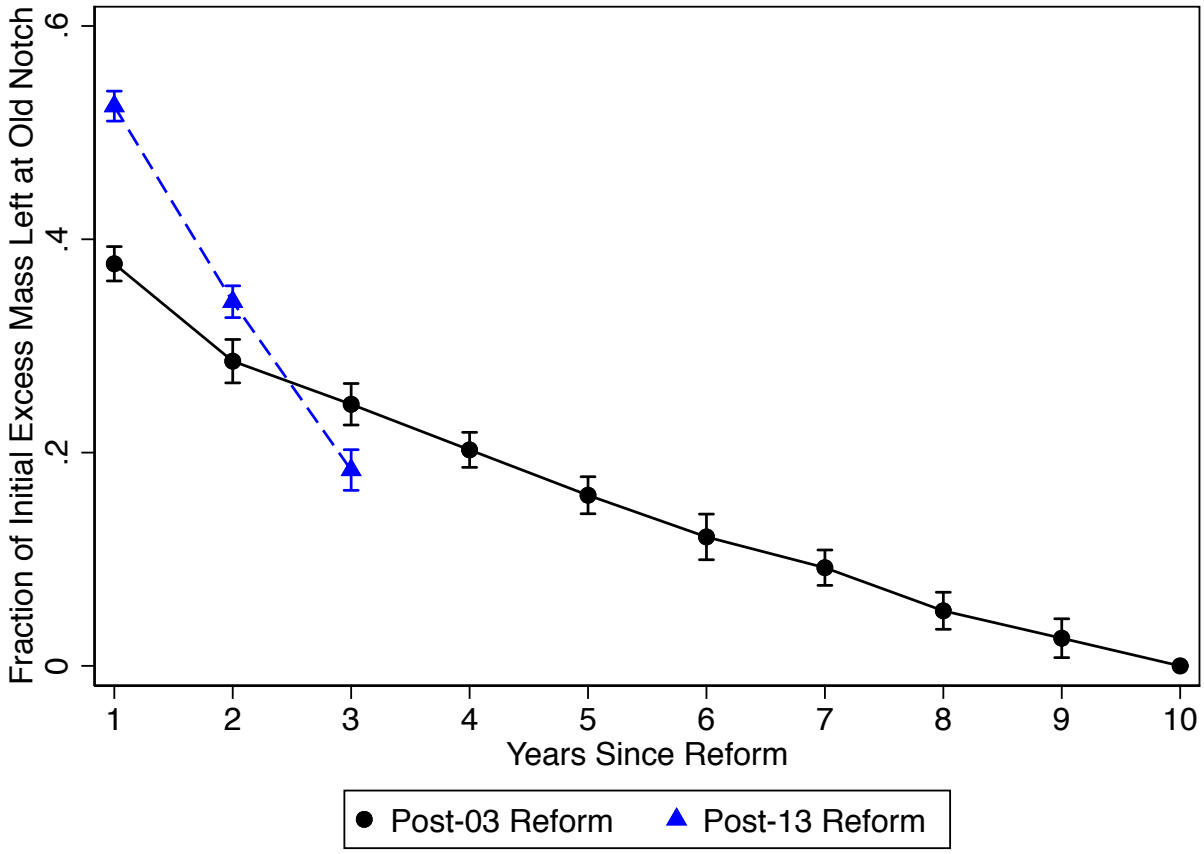


(b) Δ Hours, Estab. w/ ≥ 10 Minijobbers



Notes: This figure shows how hours change between 2013 and 2012 for adjusters — minijobbers close to the original notch in 2012 that increased their mini job earnings post-reform — and for stayers — minijobbers close to the original notch in 2012 that remained at the old notch in 2013. We use the same sample as in Figure 3. Stayers are defined as persons with average monthly earnings between 362.5 and 412.5 euro (both total and mini) in 2012 who remain in that range in 2013 and whose average monthly earnings change by less than 20 euro. Adjusters are defined as persons with average monthly earnings between 362.5 and 412.5 euro (both total and mini) in 2012 whose earnings increase by at least 30 euro and fall between 412.5 and 462.5 euro in 2013. Panel (c) plots the change in average **monthly** hours worked between 2012 and 2013 for stayers and adjusters separately. Panel (d) is the same as (c) except it 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

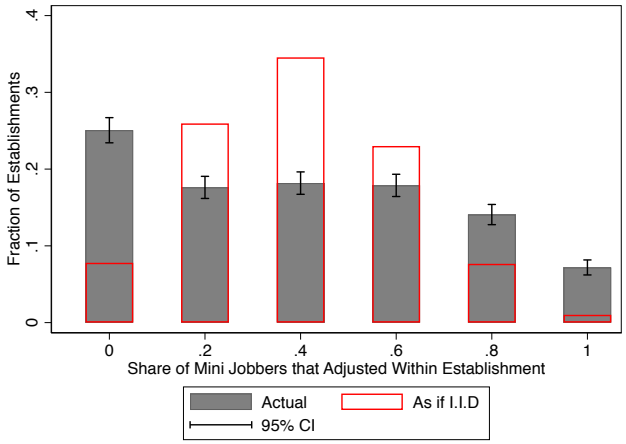


Notes: This figure plots the share of the initial bunching mass at the original notch that remains stuck at the old notch in each post-reform year. The black solid line corresponds to the 2003 reform, where year 1 is Apr-Dec 2003, year 2 is 2004, and so forth. The blue dashed line corresponds to the 2013 reform, with year 1 being 2013, year 2 being 2014, and so forth. 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. 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 and the estimation window extends to €2007.5. Figures A.7–A.8 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.

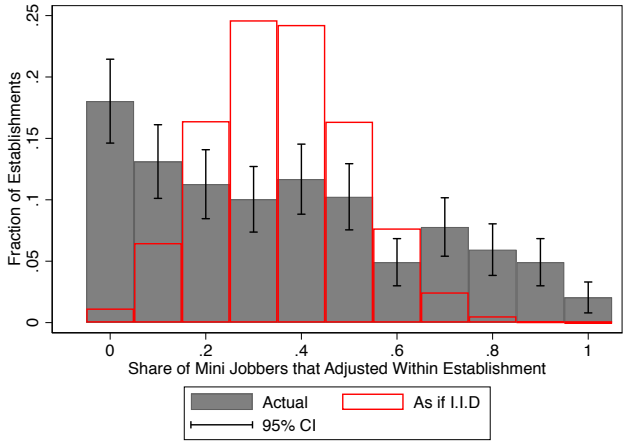
Figure 7: Share of Mini Jobbers who Adjusted within Establishment

2003 Reform

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

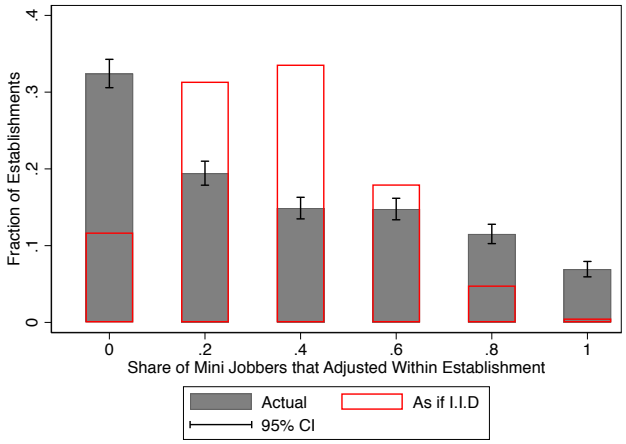


(b) Establishments with $\tilde{N} = 10$ in 2002

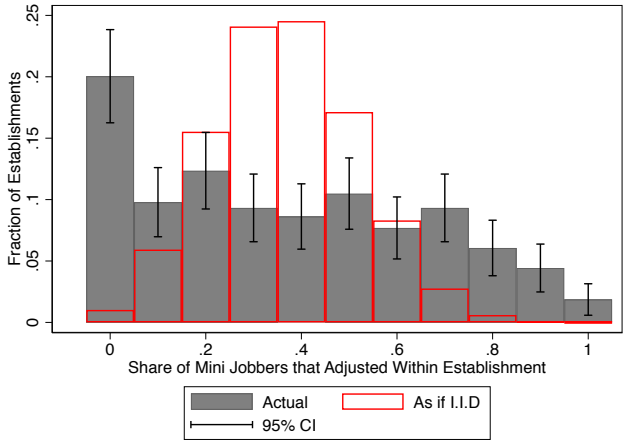


2013 Reform

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



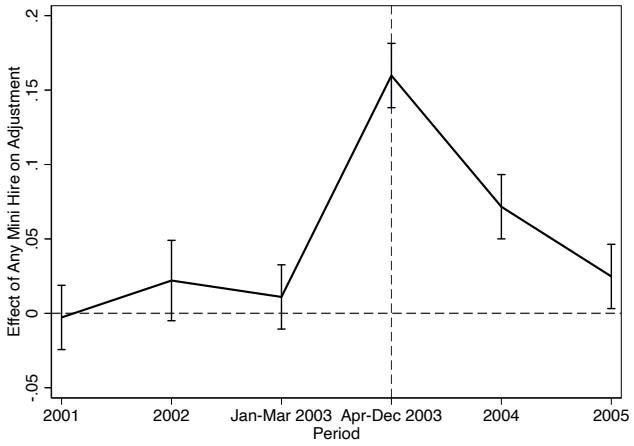
(d) Establishments with $\tilde{N} = 10$ in 2012



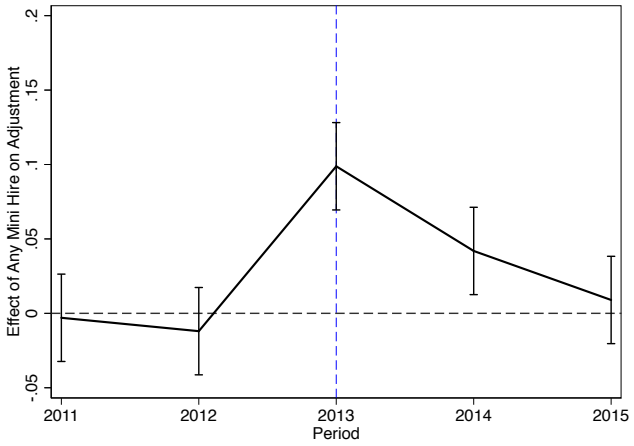
Notes: These figures focus on establishments with a fixed number of minijobbers ($\tilde{N}$) at the old threshold and calculate the fraction of these minijobbers that increase their earnings post-reform. The dark grey, shaded bars plot the share of establishments that fall into each of the possible adjustment categories (exactly 0 out of $\tilde{N}$ workers adjust, exactly 1 out of $\tilde{N}$ adjust, and so forth). The empty bars plot the distribution that would arise if workers all had the figure-level, average adjustment propensity and were randomly distributed across firms (see Section 5.2). 95% confidence intervals are calculated separately for each possible fraction adjusting and all distributions are highly statistically different from their i.i.d counter-part 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 8: Effects of Establishment Level Labor Demand on Adjustment

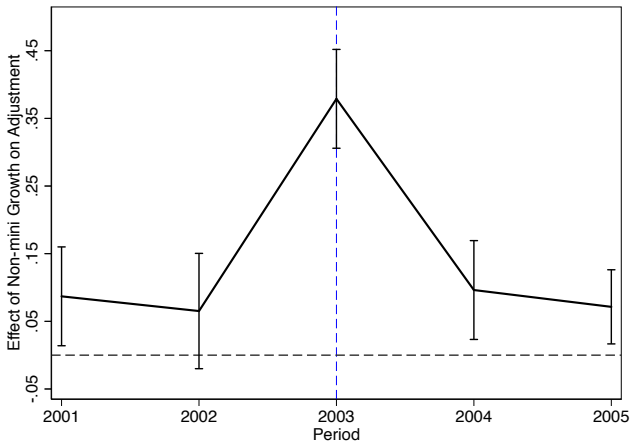
(a) 2003 Reform: Any New Mini Hire



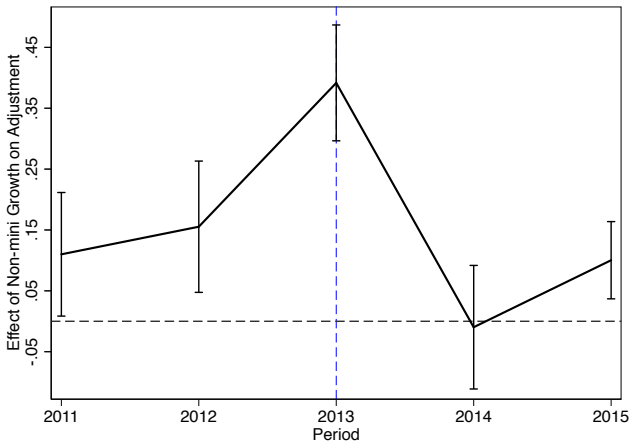
(b) 2013 Reform: Any New Mini Hire



(c) 2003 Reform: Non-Mini Growth

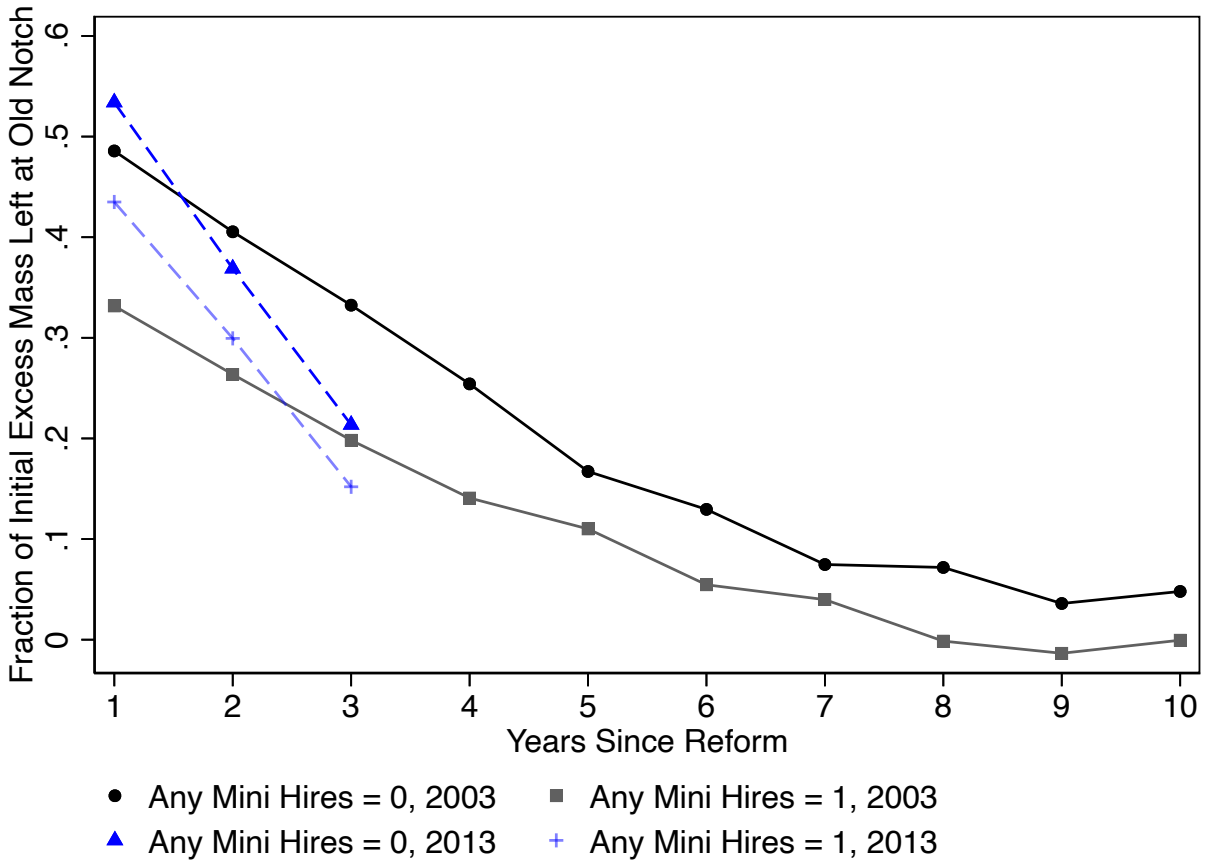


(d) 2013 Reform: Non-Mini Growth



Notes: Panel (a) plots the regression coefficients from Table 3, Panel A, Column (3), as a fraction of the dependent variable mean (see notes to Table 3). Panel (b) plots the regression coefficients from Table 3, Panel A, Column (6), as a fraction of the dependent variable mean and shows the effect of any new mini job hire on adjustment for the 2013 reform. Panel (c) plots the regression coefficients from Table 4, Panel A, Column (3), as a fraction of the dependent variable mean (see notes to Table 4). While Panel (d) plots the regression coefficients from Table 4, Panel A, Column (6), as a fraction of the dependent variable mean. The dashed blue line corresponds to the first post-reform period.

Figure 9: 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 6 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 Minijobbers at the Notch

	Panel A: Minijobbers 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: Minijobbers 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 all the married women from Figures 2—3 who hold a single mini job with average monthly 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 these workers by their future average monthly earnings, which either fall below this initial amount, stay in the same range, increase, or increase beyond the mini job threshold. Italicized rows use the same earnings categories, but are based on mini job earnings at the workers' initial establishment, as opposed to average monthly earnings from all sources. All numbers (in both italicized and bold rows) are expressed as a fraction of the total number of workers in 2002 or 2012. 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: Variance Decomposition of Adjustment Rates Between Establishments

	2003 Reform: Adjusted to 400 in 2003		2013 Reform: Adjusted to 450 in 2013	
	RE	RE w/ controls	RE	RE w/ controls
	(1)	**(2)**	**(3)**	**(4)**
Var. Explained by Establishment (ρ)	0.275	0.271	0.279	0.283
Var. Explained by 1-Digit Industry (ρ)	0.003	0.000	0.000	0.000
Var. Explained by 2-Digit Occupation (ρ)	0.028	0.010	0.004	0.000
Var. Explained by State Dummies (ρ)	0.003	0.000	0.004	0.000
Var. Explained by Municipality Dummies (ρ)	0.075	0.081	0.077	0.077
Var. Explained by Municipality $\times$ Ind. (ρ)	0.128	0.127	0.134	0.135
Var. Explained by Municipality $\times$ Occ. (ρ)	0.130	0.125	0.131	0.133
Var. Explained by Municipality $\times$ Occ. $\times$ Ind. (ρ)	0.179	0.170	0.179	0.181
Dep. Var. Mean	0.372	0.372	0.325	0.325
Num. of Individuals	220302	220302	194866	194866
Num. of Establishments	74085	74085	68402	68402
Num. of Industries	21	21	22	22
Num. of Occupations	88	88	81	81
Num. of States	10	10	10	10
Num. of Municipalities	5468	5468	5477	5477
Num. of Municipalities $\times$ Ind.	25107	25107	24228	24228
Num. of Municipalities $\times$ Occ.	36903	36903	36070	36070
Num. of Municipalities $\times$ Occ. $\times$ Ind	61862	61862	57614	57614

Notes: Each entry in the table corresponds to a different regression designed to estimate the fraction of the variance in adjustment rates that can be attributed to between-group variation. We estimate $Adj_{if} = Const + \nu_f + \epsilon_{if}$, where ν_f is an establishment (or industry, or occupation, etc) random effect, and we report estimates of $\rho = \frac{\sigma^2_\nu}{\sigma^2_\nu + \sigma^2_\epsilon}$. Columns (1)-(3) examine the 2003 reform and columns (4)-(6) examine the 2013 reform. 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 is defined as being equal to 1 if average monthly earnings (from all sources) in Apr-Dec 2003 (2013) fall between $Z + 12.5$ and $\tilde{Z} + 12.5$, where $\tilde{Z}$ is the new notch after the reform. This definition allows adjustment outside the establishment. Not adjusting encompasses all other earning levels as well as leaving the data. Columns 2 and 4 include a matrix of controls X in the regression $Adj_{if} = Const + X_i'\beta + \nu_f + \epsilon_{if}$ to check for sensitivity to first order sorting on observables. The included controls are monthly wages and days worked in the 2 years prior to the reform, tenure, occupation tenure, age, education years, gender, a dummy for German nationality, and a dummy for working in more than one establishment and a dummy for working both in a mini and regular job in the same year in each of the 2 years prior to the reform.

Table 3: 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 the 2003 reform and columns (4)–(6) examine the 2013 reform. 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 people who held a mini job at this establishment for the years 2000-2002 (2010-2012). Panel A regresses a dummy for whether these mini jobbers increased their mini earnings above $Z + 12.5$ (and below the new notch: $\tilde{Z} + 12.5$) at this establishment post-reform (in Apr-Dec 2003 for the first reform and in 2013 for the second) on indicators for any new mini hire at the establishment. All columns include dummies for mini and non-mini employment size categories, modal 2-digit occupation, 2-digit industry, and state. Columns (2) and (5) add individual controls while columns (3) and (6) further add a series of establishment-level controls (see Section 6.1 for the list of 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]. Oster (2017)'s δ, which is the degree of selection on unobservables (in terms of selection on observables) needed to nullify the coefficient on any hires in the first post-reform year, is calculated using her psacalc code, using the suggested R_{max} of 1.3 times the observed R^2 from the regression with controls. We treat the baseline controls and the other any hire variables as unrelated controls. Standard errors, clustered at the establishment level, are in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 4: 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 the 2003 reform and columns (4)–(6) examine the 2013 reform. 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. In addition, we restrict to establishments with at least 10 non-mini jobbers in 2002 (2012) and at least one non-mini jobber in 2000-2005 (2010-2015). Panel A regresses a dummy for whether these mini jobbers increased their mini earnings above $Z+12.5$ (and below the new notch: $\tilde{Z}+12.5$) at this establishment post-reform (in Apr-Dec 2003 for the first reform and in 2013 for the second) on the change in the natural logarithm of the total earnings accruing to non-mini jobbers at the establishment. All columns include dummies for mini and non-mini employment size categories, modal 2-digit occupation, 2-digit industry, and state. Columns (2) and (5) add a number of individual controls while columns (3) and (6) further add a series of establishment-level controls (see Section 6.1 for the list of 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 [2013]. Column (3) [(6)] explores adjustment in 2005, conditional on those establishments without adjustment in 2003 and 2004 [2013 and 2014]. Oster (2017)'s δ, which is the degree of selection on unobservables (in terms of selection on observables) needed to nullify the coefficient on any hires in the first post-reform year, is calculated using her psacalc code, using the suggested R_{max} of 1.3 times the observed R^2 from the regression with controls. We treat the baseline controls and the other any hire variables as unrelated controls. Standard errors, clustered at the establishment level, are in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 5: 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

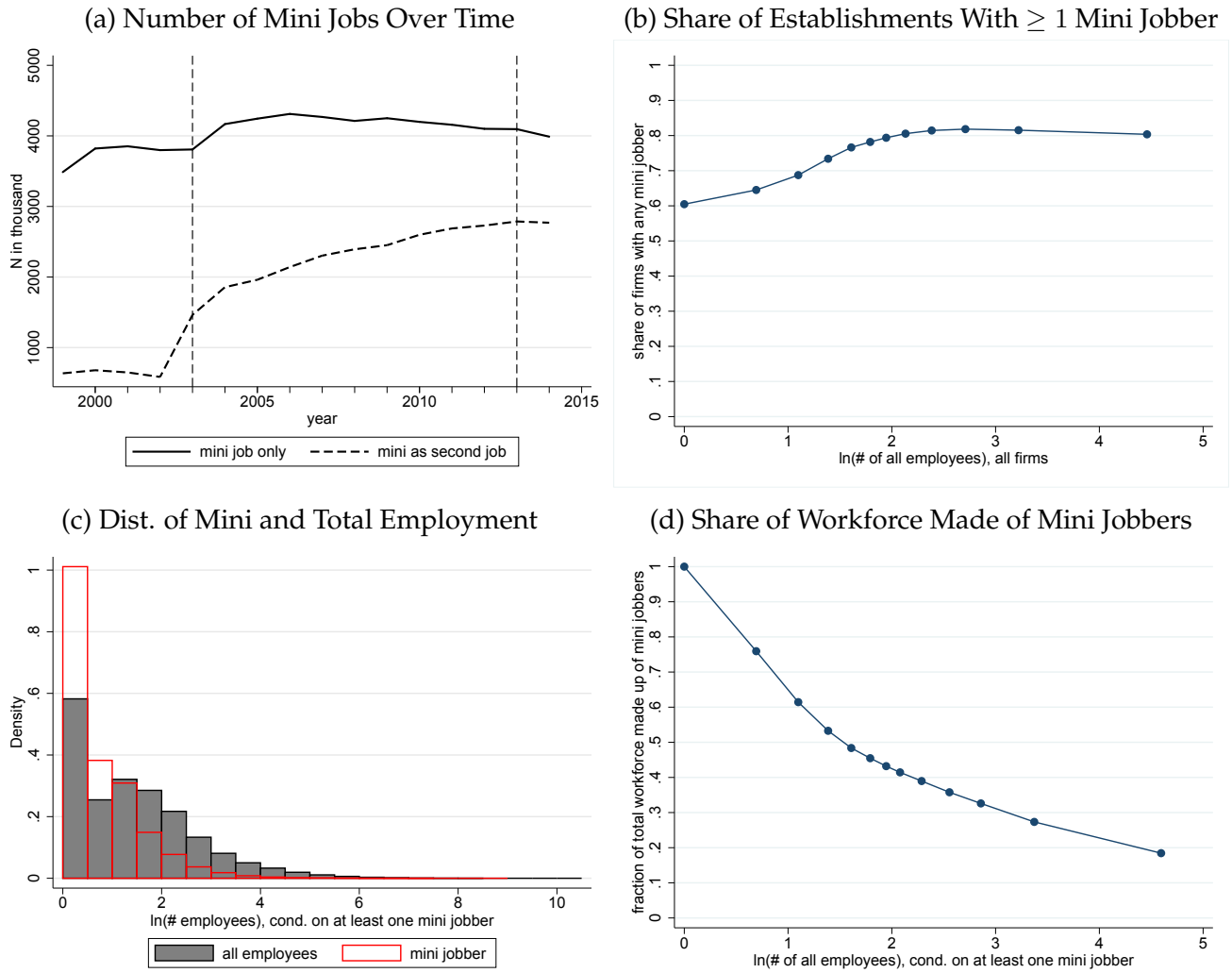
Notes: This table shows the association between predicted hires and individual adjustment for both the 2003 as well as the 2013 reform. The estimation sample consists of all long-term mini jobbers in our establishment sample in a single job with average monthly earnings between $Z - 37.5$ and $Z + 12.5$ in the pre-reform year, where Z is the notch threshold. In addition, we restrict to establishments with at least 1 mini and non-mini jobber in 1999-2002 (2009-2012). The dependent variable is a dummy for whether these mini jobbers increased their mini earnings above $Z + 12.5$ (and below the new notch: $\tilde{Z} + 12.5$) at this establishment post-reform. Predictions are formed by regressing the fraction of mini-job hires (capped at 1) in the pre-reform period on a set of pre-determined predictors using OLS, and then creating a prediction for frac. hires in the reform year based on the obtained coefficients. The set of predictors in the parsimonious specification (panel A) consists of establishment-size dummies in $t - 3$ for mini and non-mini work-force separately (with t being the year), the growth rate for mini and non-mini jobbers between $t - 3$ and $t - 2$ and $t - 2$ and $t - 1$ and the hiring rate for mini and non-mini jobbers between $t - 3$ and $t - 2$ as well as $t - 2$ and $t - 1$, where growth rates are capped at 1. The set of *potential* predictors used for the LASSO procedure in panel B consists of these same variables plus the two-year growth and hiring rate between $t - 3$ and $t - 1$, 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, separately for $t - 1$, $t - 2$ and $t - 3$), the share of workers in 8 different age-groups in the establishment, separately for $t - 1$, $t - 2$ and $t - 3$. Furthermore it contains time-consistent dummies for the 5-digit industry-classification, 400 regions (counties) and dummies for the interaction between 3-digit industry and county. The resulting prediction is based on those coefficients that are selected by LASSO using a tuning parameter that minimizes the MSPE in 5-fold cross validation with 50 iterations. 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

A Appendix Figures and Tables

A.1 Appendix Figures

Figure A.1: Mini Job Descriptives

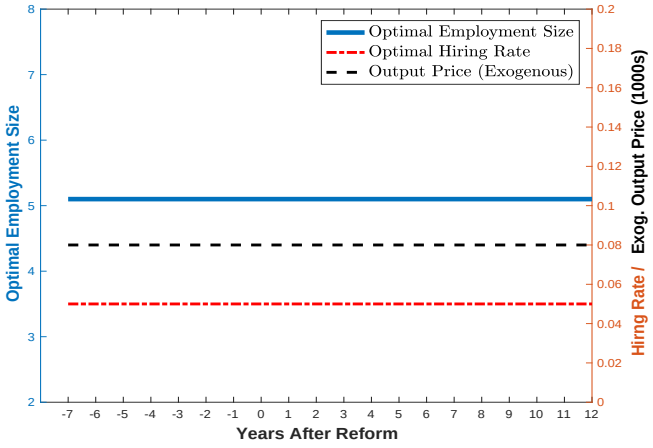


Notes: This figure summarizes the development of mini jobs over time and the distribution of mini jobbers across firms. Panel (a) shows the number of mini jobbers over time for exclusive mini jobbers as well as workers with a mini job as a second job. It is constructed from the SIAB data by taking the number of distinct individuals holding a mini job (erwstat = 109) in June 30th in each year in the SIAB data. Individuals having both a mini job and any other non-mini employment on June 30th are classified as having a mini as a second job. Vertical dashed lines indicate the two reforms. Panels (b)-(d) are based on the BHP for the year 2010, a 50% random sample of establishments as of June 30th. Panel (b) is a binned scatterplot that shows the share of establishments that employ at least one mini jobber and how this varies with the natural logarithm of (all) employment. Panel (c) compares the distribution of log employment size for mini jobbers and all workers separately, at establishments with at least one mini jobber. Panel (d) shows the fraction of the total workforce in the establishment that is working in mini jobs. Like (B), it shows how this fraction varies with the natural logarithm of employment size using a bin scatter. Like (c), it restricts to establishments with at least one mini jobber.

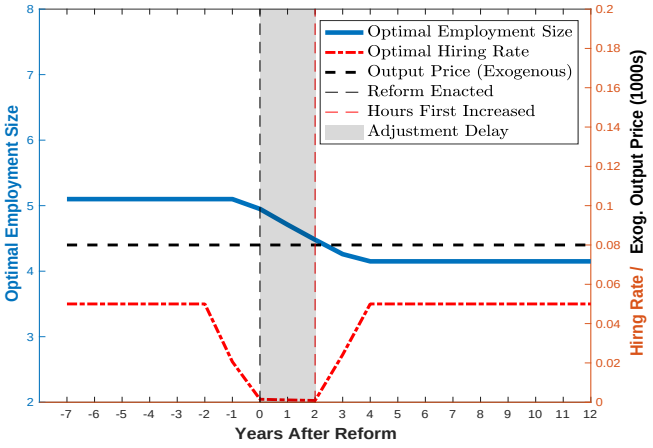
Figure A.2: Model Simulations of a Firm's Optimal Hours Adjustment Process

Optimal Emp. & Hiring | Optimal Emp., Hiring, & Adj. Timing

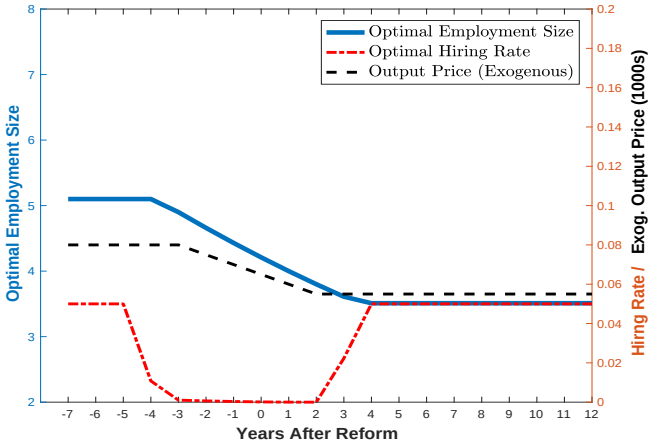
(a) Stationary Environment (no reform)



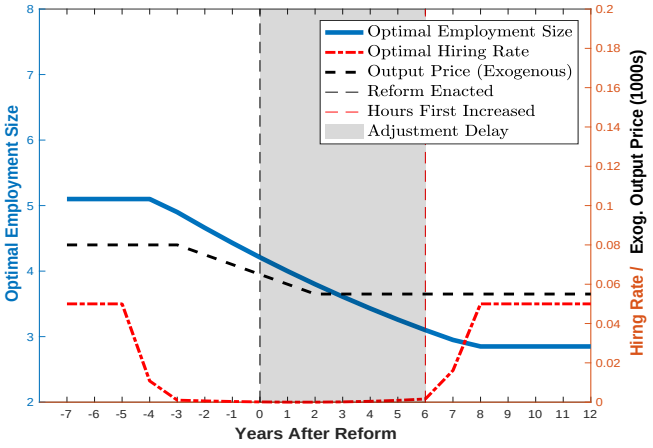
(b) Stationary Environment (reform)



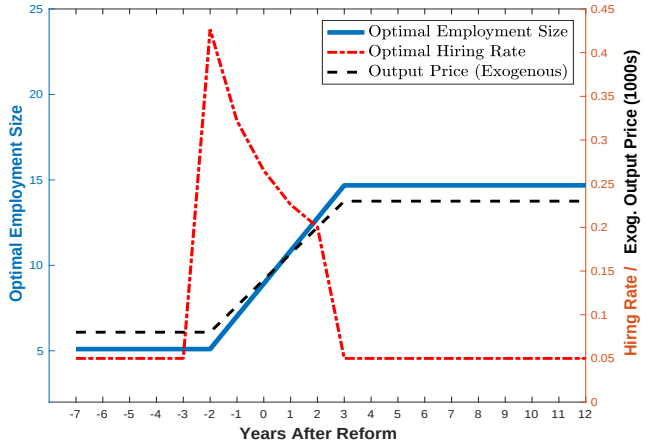
(c) Declining Output Price (no reform)



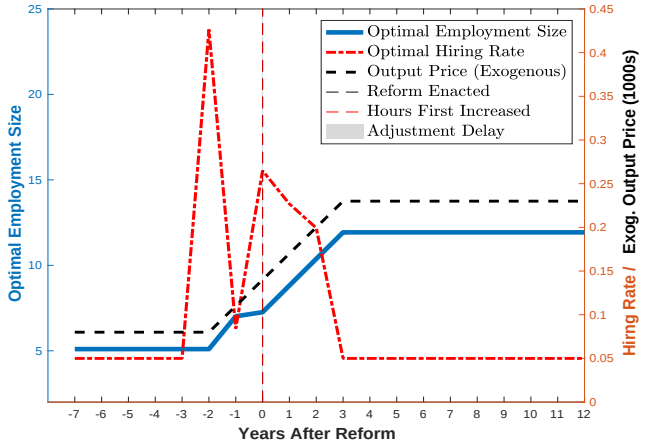
(d) Declining Output Price (reform)



(e) Increasing Output Price (no reform)



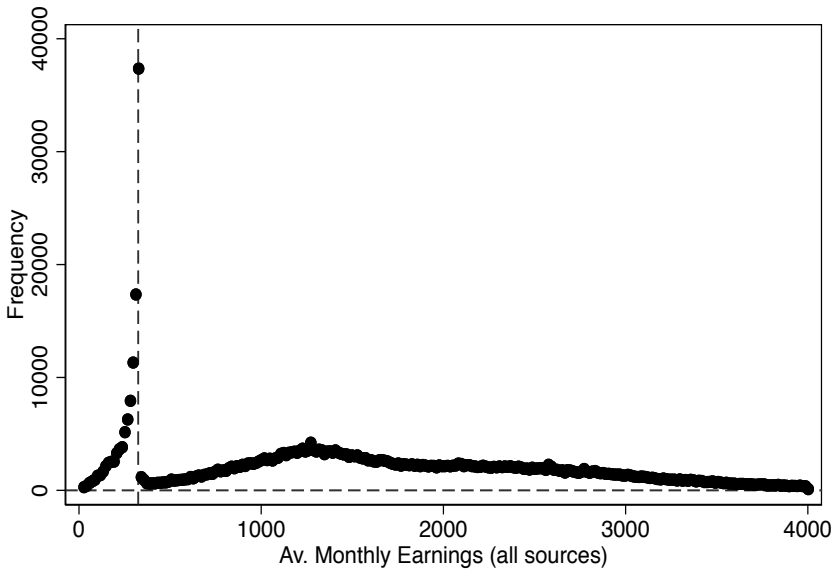
(f) Increasing Output Price (reform)



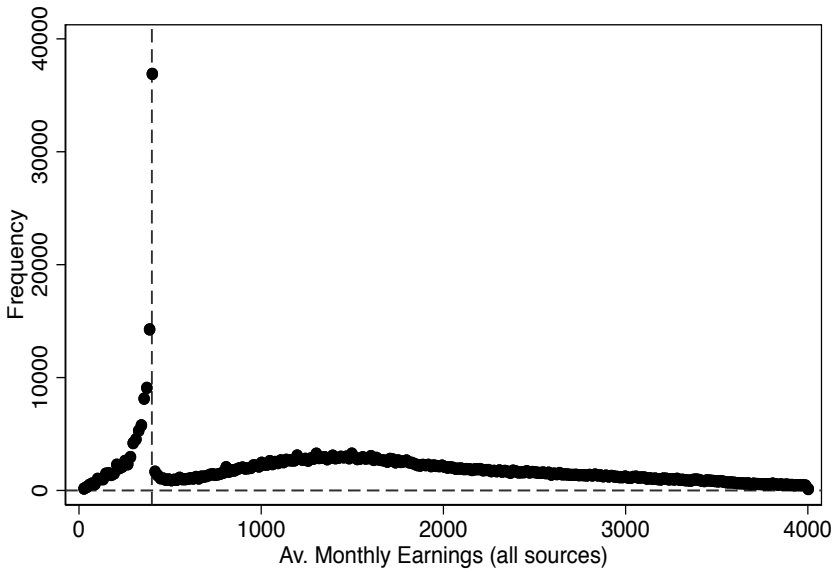
Notes: This figure simulates the model described in Section 3 under 3 different environments. Panels (a) and (b) do so for a stationary environment, (c) and (d) do so for a contracting firm (declining output prices), and (e) and (f) do so for an expanding firm. Parameters are $w = 10$, $h = 32.5 \cdot 12$, $\bar{h} = 40 \cdot 12$, $p = 80$, $\alpha = 200$, $\sigma = 0.05$, $\delta = 0.961$. The production function used is $\phi \ln(N \cdot h)$ with $\phi = 250$. The left 3 panels show the optimal employment path (N_t) and corresponding hiring rate (a_t) for the given price path when h is exogenously fixed. The right three panels show the optimal employment path (N_t) and corresponding hiring rate (a_t) with a period 0 reform (known in period -1) that allows the firm to increase to $\bar{h}$. For these panels, we also show the (optimally chosen) adjustment period, given by the dashed red line. The shaded region indicates the years in which the firm has delayed increasing hours. In Panel (f) the firm increases hours as soon as the reform is enacted.

Figure A.3: Full Earnings Distribution: Married Women 2002 and 2012

(a) Earnings Distribution for Married Women 2002



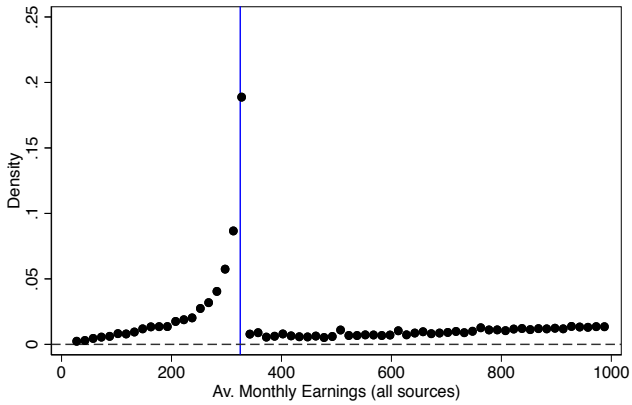
(b) Earnings Distribution for Married Women 2012



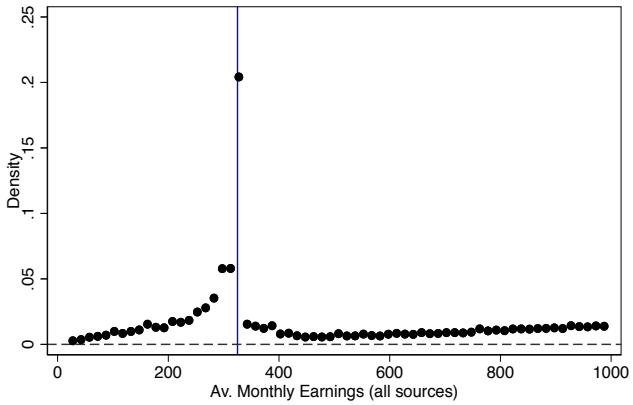
Notes: This figure plots the same 2002 and 2012 earning distributions as Figures 2- 3, but shows the entire distribution.

Figure A.4: Earnings Distributions: All Women 2002-2006

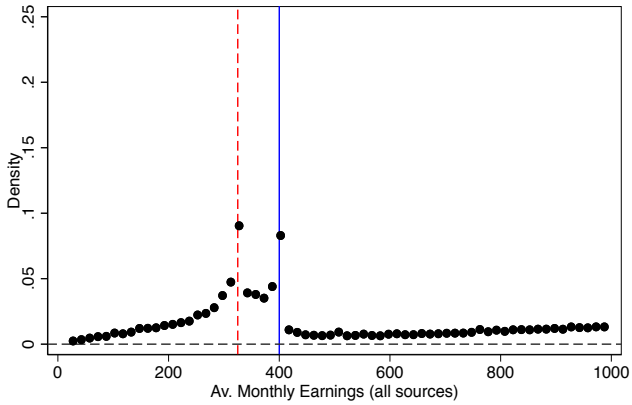
(a) Earnings Distribution 2002



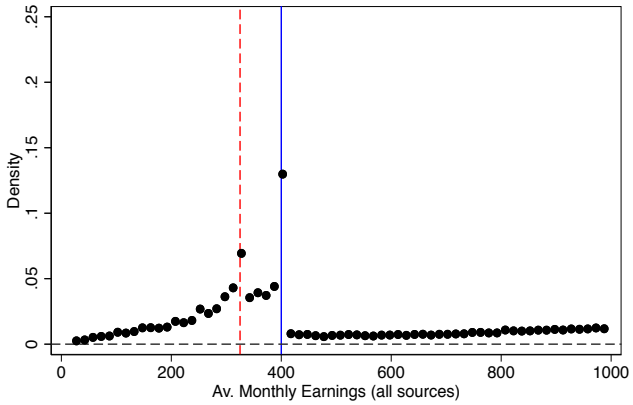
(b) Earnings Distribution 2003 (Jan-Mar)



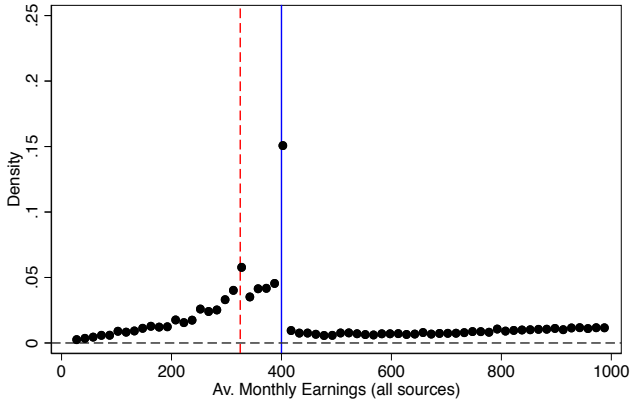
(c) Earnings Distribution 2003 (Apr-Dec)



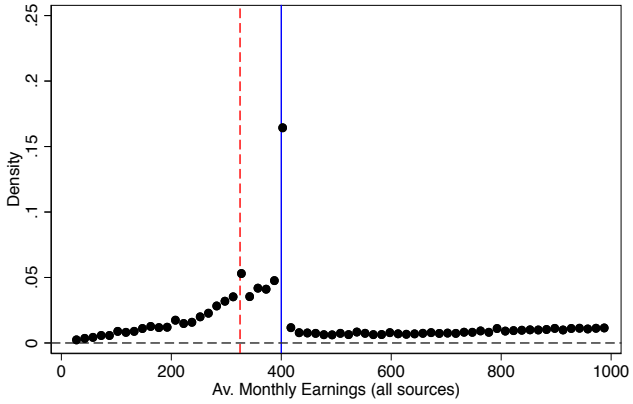
(d) Earnings Distribution 2004



(e) Earnings Distribution 2005



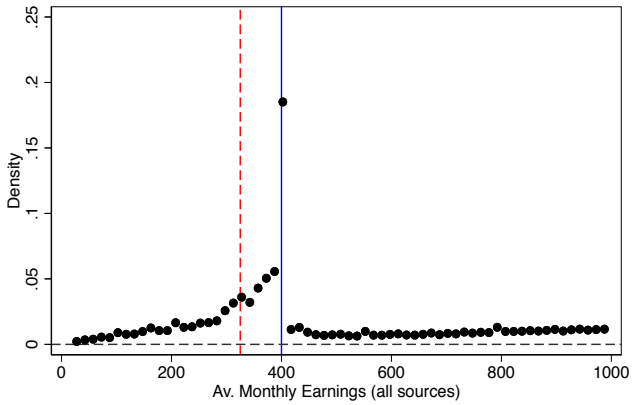
(f) Earnings Distribution 2006



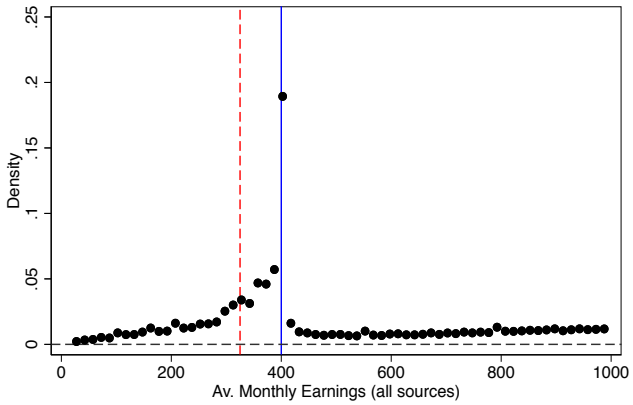
Notes: This figure replicates Figures 2–3 for all women, instead of just the identified married women with husband annual earnings between 33000 and 53000. It still restricts to women working in West Germany, aged 26 to 55 (inclusive). It plots the distribution of average monthly earnings (all employment 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.5: Earnings Distributions: All Women 2010-2015

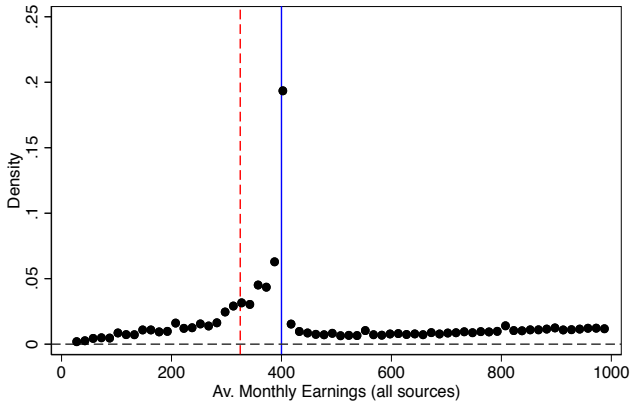
(a) Earnings Distribution 2010



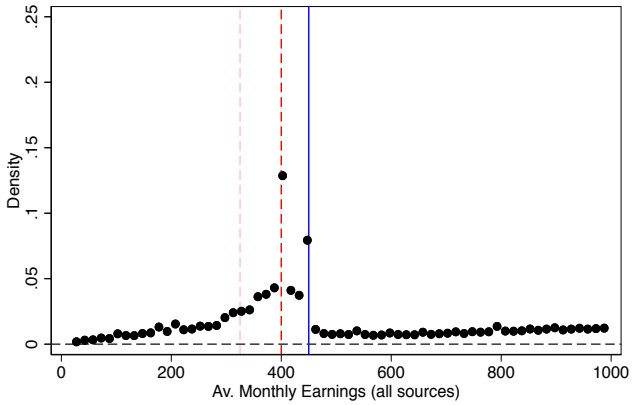
(b) Earnings Distribution 2011



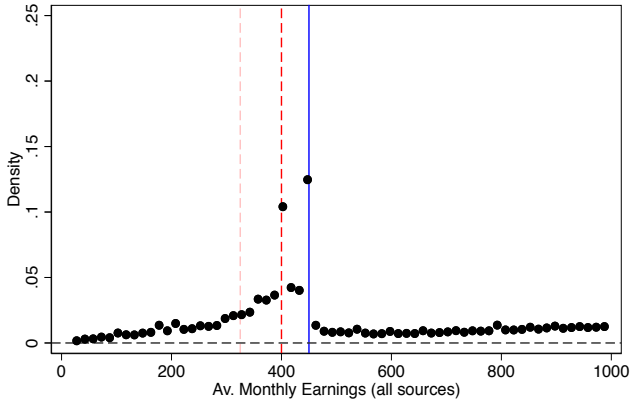
(c) Earnings Distribution 2012



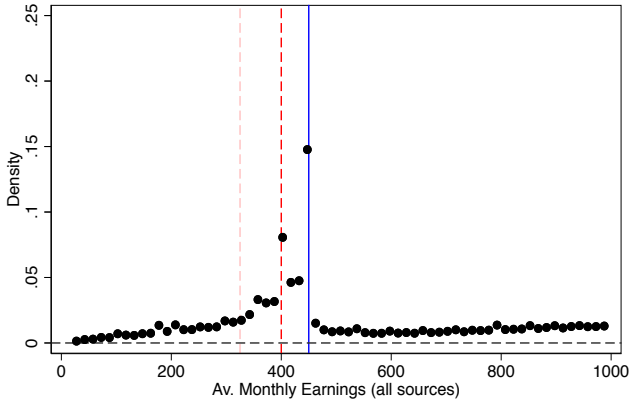
(d) Earnings Distribution 2013



(e) Earnings Distribution 2014



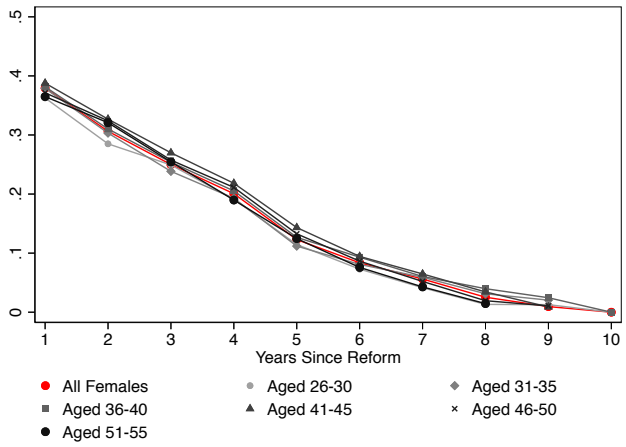
(f) Earnings Distribution 2015



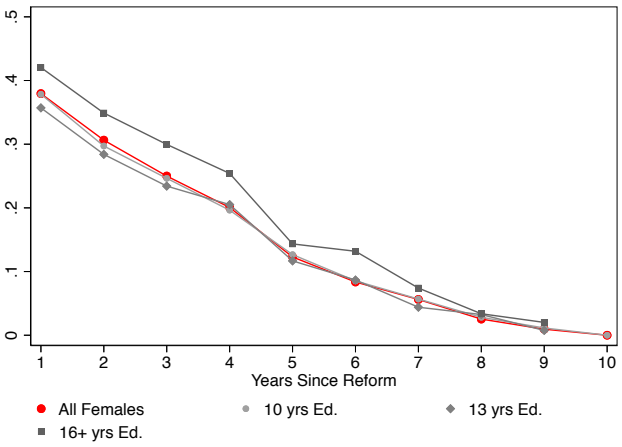
Notes: This figure replicates Figures 2–3 for all women, instead of just the identified married women with husband annual earnings between 33000 and 53000. It still restricts to women working in West Germany, aged 26 to 55 (inclusive). It plots the distribution of average monthly earnings (all employment 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.6: Heterogeneity in the Fraction of Initial Bunching Mass Left at Old Notch

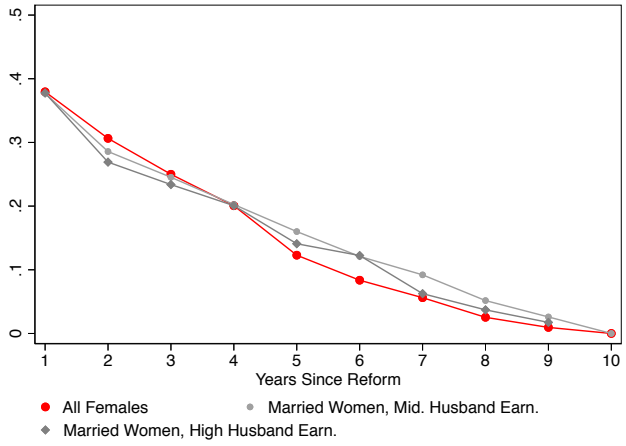
(a) By Age



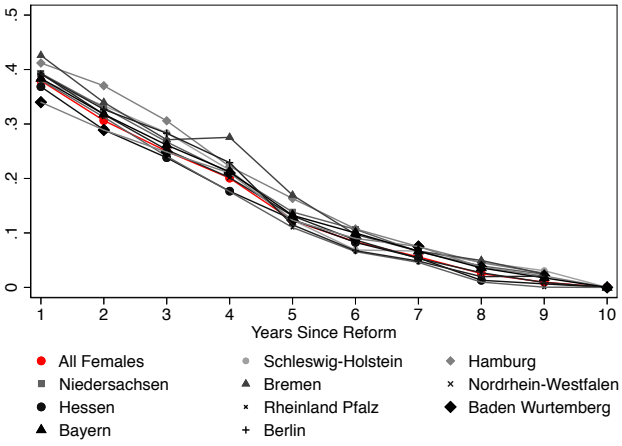
(b) By Education



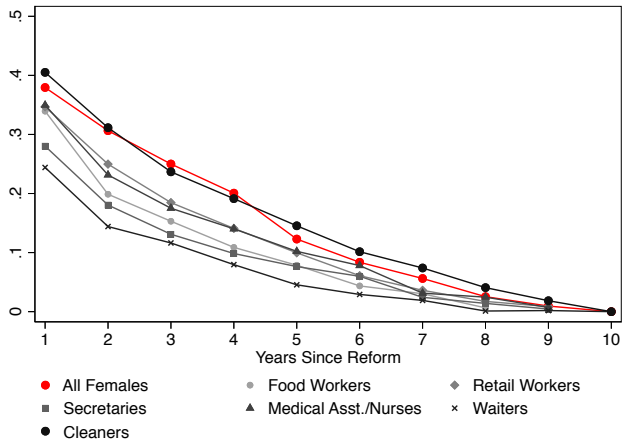
(c) By Husband Earnings



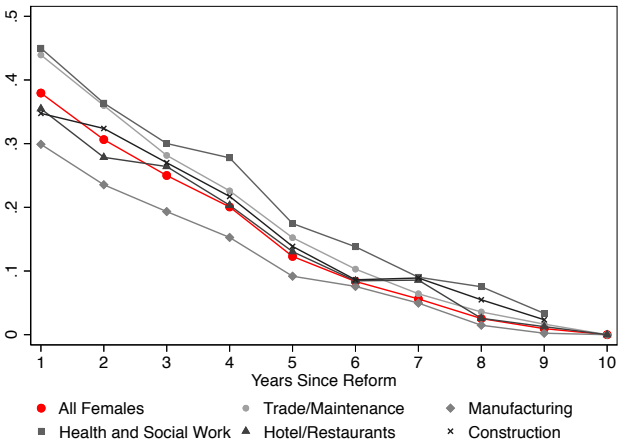
(d) By State



(e) By Top Occupations

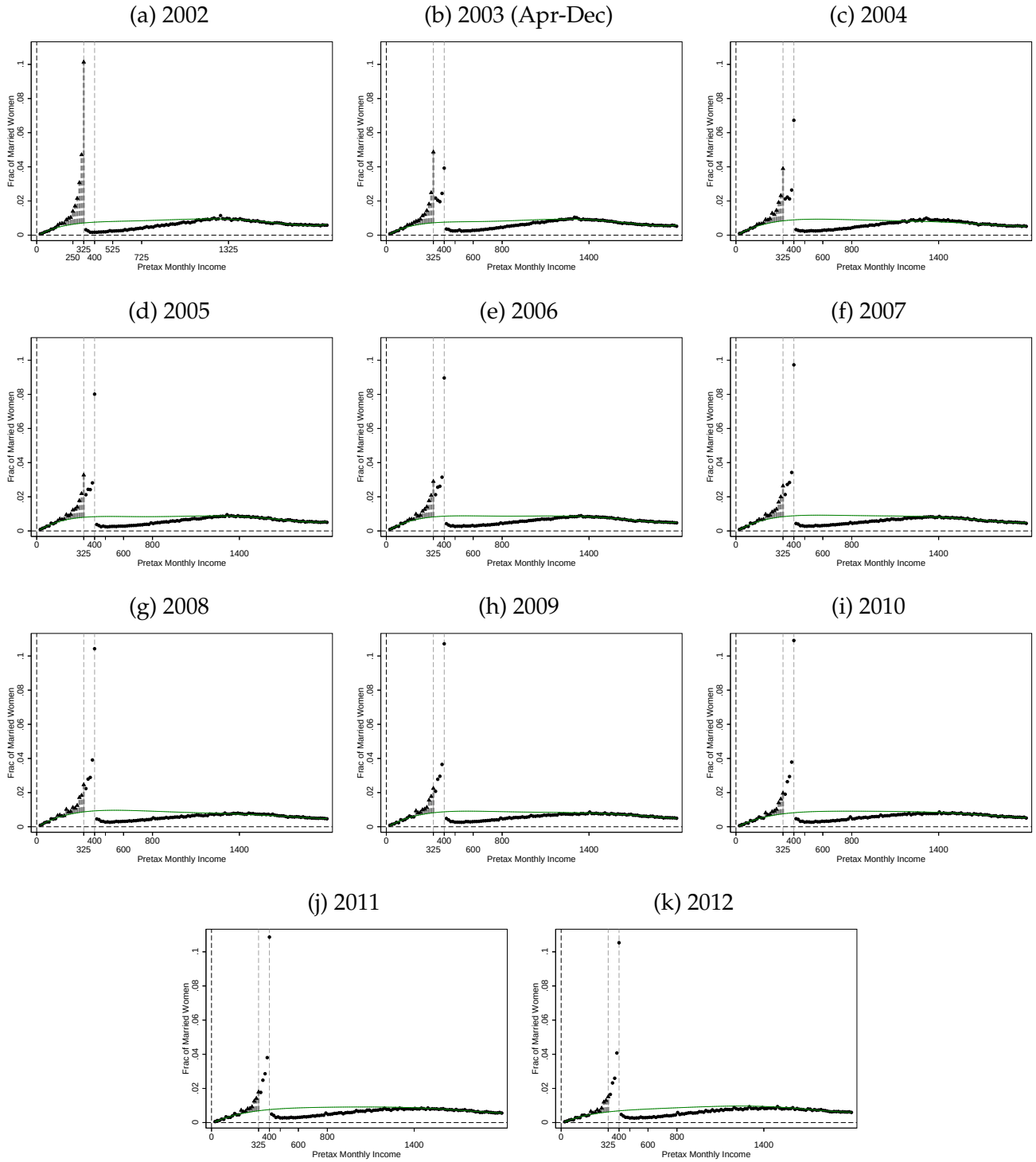


(f) By Top Industries



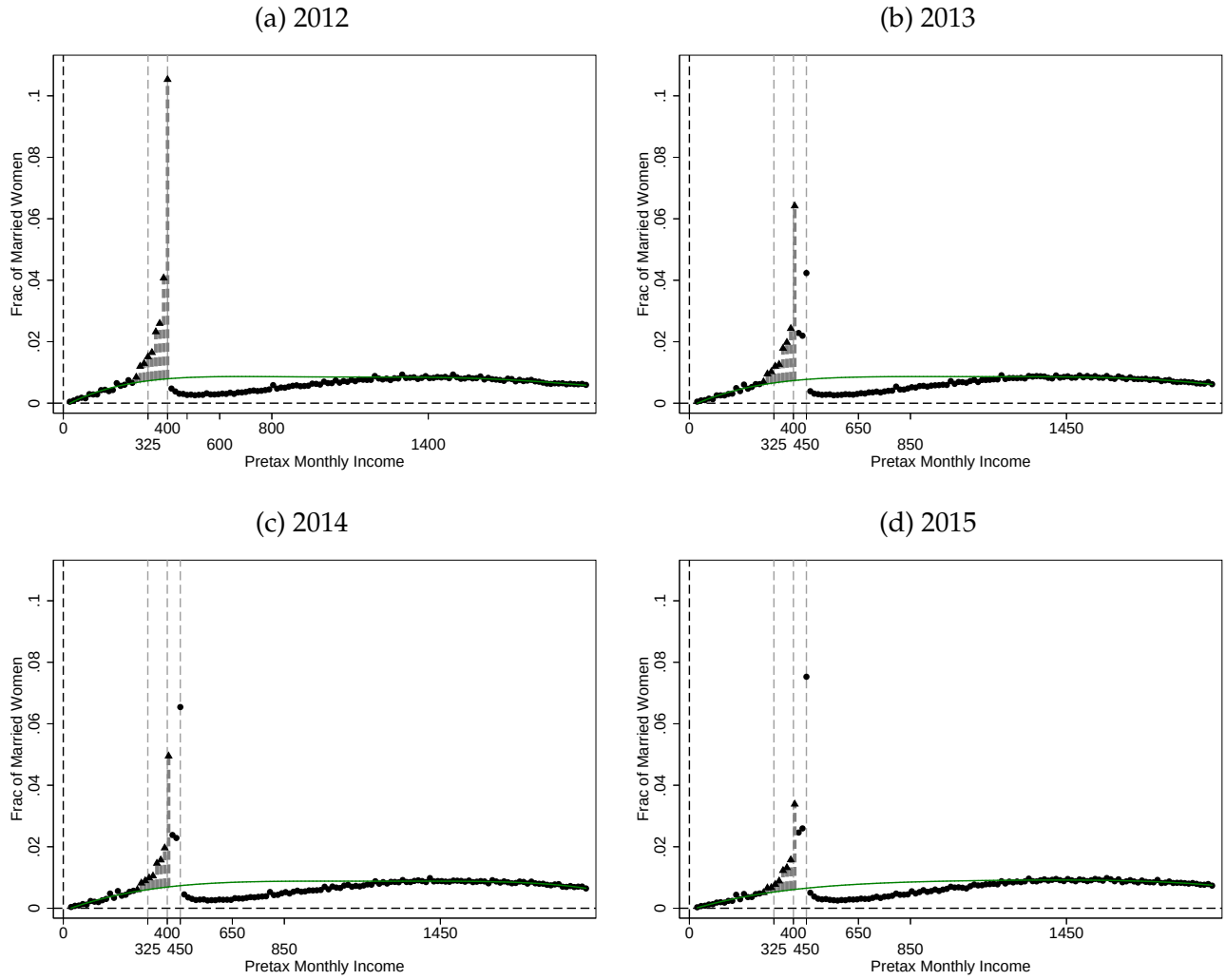
Notes: This figure is constructed in the same manner as Figure 6 and plots the share of the initial bunching mass at the pre-reform notch that remains at the old notch in each year following the 2003 reform. The red line does this for a sample of all women (not just the married women in Figure 6) while 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.7: 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.

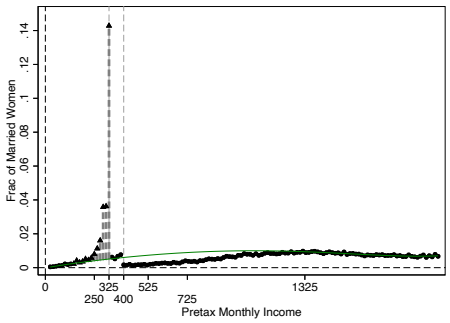
Figure A.8: Bunching at Old Notch: Married Women 2012-2015



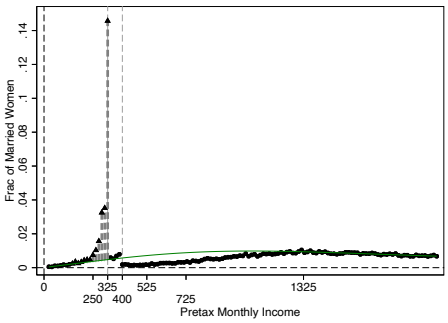
Notes: These figures show the excess bunching mass at the old notch (400 euro) after the 2013 reform for the period-by-period sample of married women in Figure 3. The estimated period-by-period counter-factual density is depicted as a light green line, while the bunching mass at the old notch is shaded 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.9: Bunching at Old Notch: Split By Any Mini Hires

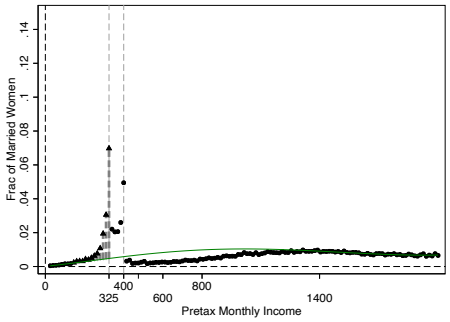
(a) 2002, Any Mini Hires = 0



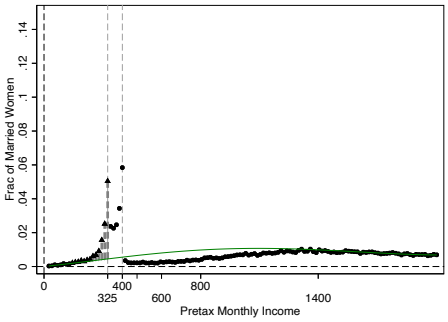
(b) 2002, Any Mini Hires = 1



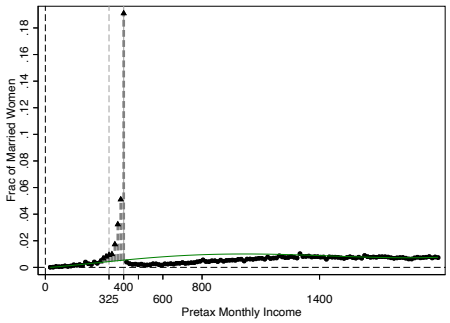
(c) 2003, Any Mini Hires = 0



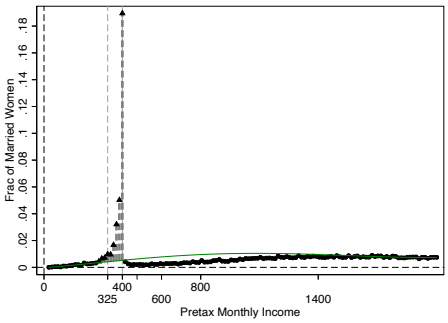
(d) 2003, Any Mini Hires = 1



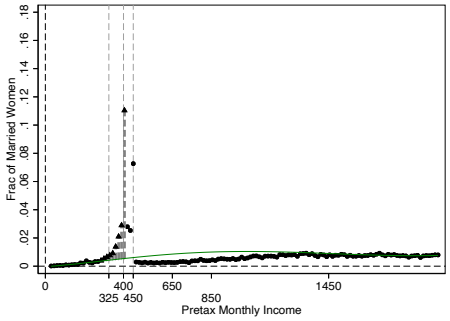
(e) 2012, Any Mini Hires = 0



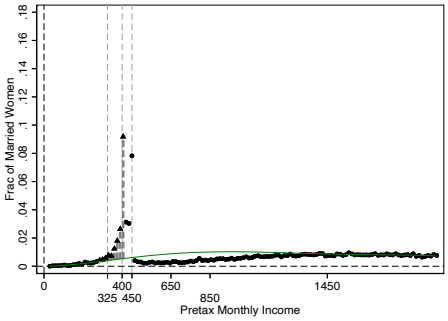
(f) 2012, Any Mini Hires = 1



(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 6. We estimate the period-by-period counter-factual density using the same procedure as in Appendix Figures A.7-A.8. 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: 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 3—4. The resulting coefficients are similar, but slightly larger. See notes to Tables 3—4 for additional details. Standard errors, clustered at the establishment level, are in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.2: Pooling Reforms

	Pooled	Individ. FE Sample		Estab. FE Sample	
	Panel A: Any New Mini Hire				
	(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.3: 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 Firms	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 Firms	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 3 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.4: 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 Firms	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 Firms	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 4 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 4. 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.5: 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 Firms	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 Firms	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 5 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.6: '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 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.7: '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	0.022	0.029
	(0.015)	(0.016)	(0.016)	(0.025)	(0.025)	(0.026)
Δ Ln total non-mini earnings 02-01/12-11	0.003	-0.020	-0.031	0.052	0.025	0.032
	(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***	0.142***	0.150***
	(0.023)	(0.024)	(0.024)	(0.032)	(0.032)	(0.031)
Δ Ln total non-mini earnings 04-03/14-13	0.030	0.032	0.032	-0.028	-0.037	-0.035
	(0.022)	(0.023)	(0.023)	(0.026)	(0.027)	(0.026)
Δ Ln total non-mini earnings 05-04/15-14	0.040*	0.031	0.029	0.060***	0.048**	0.050***
	(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	0.209	0.187
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		9.547	15.387
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 Firms	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 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.8: '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 5 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 5). 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 5 for additional details. Standard errors, clustered at the establishment level, are in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.9: Adjustment with Hours Changes (2013 Reform)

	2013 Reform: Adjusted to 450 in 2013					
	Δhrs >0 all	Δhrs >1 all	Δhrs >2 all	Δhrs >0 $\geq$ 10mini	Δhrs >1 $\geq$ 10mini	Δhrs >2 $\geq$ 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
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: 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
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 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
Firm 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 3–5) 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%.

B Dynamic Labor Demand Model: Proofs and Derivations

B.1 Optimization Problem

We assume a firm employs only at-the-threshold mini jobbers who all prefer to work $\Delta h = \bar{h} - \underline{h}$ more hours after the reform. The firm pays a competitive wage w and would normally choose h_t optimally. Here, workers are constrained at $\underline{h} = Z/w$, where Z is the mini job threshold. The firm would have to pay a much higher wage to induce hours above this threshold. For simplicity, we assume that $\underline{h}$ is at or below the optimal hours level, so the firm sets hours $= \underline{h}$ in all periods. Relaxing the threshold, as will happen with the mini job reform, leads firms to want to increase hours, but the process of re-optimizing is costly and can take time.

The firms dynamic optimization problem consists of choosing N_t and the hiring rate a_t to maximize:

$$max_{N_t, a_t|_{t=1}^{\inf}} \sum_{t=0}^{\inf} \delta^t (pF(N_t, \underline{h}) - wN_t\underline{h} - \alpha a_t N_t) \text{ s.t. } N_{t+1} = (1 - \sigma + a_t)N_t \text{ and } N_o \text{ given}$$

$$max_{N_t|_{t=1}^{\inf}} \sum_{t=0}^{\inf} \delta^t (pF(N_t, \underline{h}) - wN_t\underline{h} - \alpha(N_{t+1} - (1-\sigma)N_t) \text{ s.t. } N_o \text{ given} \tag{B.1}$$

The production function in terms of hours satisfies standard conditions:

$$F_N(N,h) > 0, F_{NN}(N,h) < 0, F_h(N,h) > 0, F_{hh}(N,h) \leq 0$$

In addition we require that any potential complementarities between the number of workers and hours worked is not too strong ($F_{NH}(N,h) < \frac{w}{p}$). This ensures that the firm prefers lower employment at higher hours levels.

It is simplest to specify the production function in terms of person hours $N_t h_t$, such as $F = \psi \ln(N_t h_t)$ or $F = \psi(N_t h_t)^{\frac{1}{2}}$. In both cases the presence of hiring costs implies that the firm would prefer one worker working the optimal number of hours. To limit this, hours need to either become prohibitively expensive or unproductive. For our purposes, we think it reasonable to believe that the firm would prefer the existing workers work more hours — it is unlikely that the difference between 10 and 12.5 hours per week seriously diminishes productivity. Put another way, we are stating that a mini jobber working 10 hours per week has the same effect on output as would having 4 mini jobbers increase their weekly hours by 2.5. For the types of jobs we are considering like cashiers and cleaners working shifts, this seems a tolerable approximation.

B.2 Steady State

The first order condition with respect to N_t sets the marginal discounted net revenue stream of an additional hire today equal to the hiring cost. Denoting $\delta = 1/(1+r)$, the FOC is:

$$pF_N(N_t, \underline{h}) = w\underline{h} + \alpha(r + \sigma) \tag{B.2}$$

Denote N^* as the solution to $pF_N(N^*, \underline{h}) = w\underline{h} + \alpha(r+\sigma)$.

If $N_0 < N^*$ the firm hires immediately to N^* and thereafter hires to replace exits at $a_t = \sigma$ for all t. If $N_0 > N^*$ the firm lets attrition take its employment down until next period's employment would first fall below N^* at which point it hires to get to N^*. Thereafter, it hires to replace exits at $a_t = \sigma$ for all t.

Optimal Employment Size is Decreasing in Hours Worked. Let $\bar{h} > \underline{h}$ and denote N^{**} as the solution to $pF_N(N^{**}, \bar{h}) = w\bar{h} + \alpha(r+\sigma)$. For the stated conditions on the production function, $N^{**} < N^*$.

Proof. Letting $\tilde{N}(h)$ be the optimal steady state employment level for any given hours level h; the implicit function theorem tells us that $\frac{\partial \tilde{N}(h)}{\partial h} = \frac{-(pF_{NH}(\cdot)-w)}{pF_{NN}(\cdot)} < 0$.

Steady States with Higher Hours Are Preferred. Furthermore, the firm prefers the N^{**} steady state to that at N^*.

Proof. Denoting $\tilde{N}(h)$ as the optimal steady state employment level for a given h, we let

$$V(h) = \Pi(\tilde{N}(h), h) = \frac{1}{1-\delta}(pF(\tilde{N}(h), h) - (wh + \alpha\sigma)\tilde{N}(h))$$

The envelope theorem gives us that

$$\frac{\partial V(h)}{\partial h} = \frac{\partial \Pi(\tilde{N}(h), h)}{\partial h}$$

Thus

$$\frac{\partial V(h)}{\partial h} = \frac{1}{1-\delta}\left[\underbrace{p\frac{\partial F(\tilde{N}(h), h)}{\partial h} - w\tilde{N}(h)}_{>0}\right]$$

This term is positive because hours are constrained. Without the constraint, hours would be set (if possible) so that $p\frac{\partial F(\tilde{N}(h),h)}{\partial h} = w\tilde{N}(h)$. With hours constrained, $p\frac{\partial F(\tilde{N}(h),h)}{\partial h} > w\tilde{N}(h)$. So, as long as hours are constrained over the range of h being considered, $\frac{\partial V(h)}{\partial h} > 0$ for all h.

B.3 Transition Dynamics Following the Reform

We model the Mini Job reform as a shock in period τ — which could be anticipated or unanticipated — assuming the firm has already reached its steady state by then. The reform allows the firm to increase all workers from $\underline{h}$ to $\bar{h}$ hours, should they choose to do so. We model this as a discrete choice and hence disallow partial hours increases. We also prohibit firms from increasing some workers' hours but not others.

As we saw above, the firm will prefer the new steady state with higher hours and lower employment. However, what the optimal transition path looks like is more complex. Indeed, it is possible for some parameters (like sufficiently low attrition σ) that the firm prefers to never adjust to the new steady state. Instead of proving general statements about the optimal transition path, we illustrate by example that delaying adjustment can be optimal and then use a simulation approach to address comparative statics.

Delaying adjustment can be preferred to instant adjustment. Consider an unanticipated shock that is perceived to be permanent arriving in period τ. Further assume that attrition is not too high. In particular, assume that $(1-\sigma)N^* > N^{**}$ and $(1-\sigma)^2 N^* < N^{**}$. This implies that a firm in steady state that stops hiring in period τ will still have more than N^{**} workers in period $\tau+1$, but if it continues to not hire in $\tau+1$, it will have less than N^{**} workers in period $\tau+2$.

We can directly compare profits for a firm that chooses to cease hiring and to increase hours in period τ, to a firm that ceases hiring and chooses to wait to increase hours until period $\tau+1$, to one that ceases hiring and chooses increase hours in $\tau+2$. Note that since attrition drives N below N^{**} in $\tau+2$, any firm that ceased hiring in periods τ to $\tau+2$ will prefer adjusting its workers to the new hours level in period $\tau+2$ to doing so in any later period. While this is obviously not an exhaustive list of all possible strategies available to the firm, it serves to illustrate the point that delaying adjustment can be preferred.

The firm prefers adjustment in $\tau+1$ to immediate adjustment if and only if the difference in profit streams between these two choices is positive. Note that they both follow the exact same hiring path, choosing not to hire until $\tau+2$ and at that point they both hire up to N^{**}. This boils the problem down to whether the firm prefers the original steady state level of hours ($\underline{h}$) and wage costs ($wN^*\underline{h}$) to higher hours ($\bar{h}$) and higher wage costs ($wN^*\bar{h}$) in τ. Precisely, adjustment in $\tau+1$ is preferred if and only if

$$p(F(N^*,\bar{h}))-F(N^*,\underline{h}))-wN^*(\bar{h}-\underline{h})<0$$

If $\Delta h=\bar{h}-\underline{h}$ is large enough, the concavity in the production function ensures that the firm will prefer waiting until $\tau+1$ to increase h.[1]

Moreover, the firm may prefer adjustment in $\tau+2$ to $\tau+1$. As before, each strategy has the firm pursuing an identical hiring path. Adjustment is preferred in $\tau+2$ if and only if

$$p(F((1-\sigma)N^*,\bar{h})-F((1-\sigma)N^*,\underline{h}))-w(1-\sigma)N^*(\bar{h}-\underline{h})<0$$

The intuition is similar to before, but now attrition has moved the firm to a lower point on the production function, making an increase in hours more desirable and this equation less likely to hold. Nevertheless, as long as F_{Nh} is not too large (recall that we limited this complementarity between hours and employment size by assumption), there exist values of Δh and σ that satisfy this equation while keeping σ in the relevant range. When presented with a discrete choice of increasing hours, the firm is effectively trading off which deviation from steady state is most costly: too few person-hours relative to the old steady state versus too high person-hours relative to the new steady state. For certain parameter values delayed adjustment in $\tau+2$ will be preferred to increasing hours in either prior period.

B.4 Simulating the Model

To make these ideas more concrete, we fix parameter values and solve the model by backwards induction from the new steady state over a discretized state space for N. We compute the optimal hiring path corresponding to every possible adjustment decision (i.e. increasing hours in each of periods τ till $\tau+T$). We then pick the path that yields the highest profit stream, verifying that this is preferred to simply remaining at the initial steady state N^*. We allow for the reform to be anticipated by one period and we set $T=20$ and discretize N in steps of 0.01.

We set our parameters as follows. We let the time period be a year and the interest rate $r=0.04$ ($\delta=0.961$). $w=$€10 per hour. $\underline{h}=32.5\cdot 12$ so that mini jobbers would be at the threshold in 2002. Let $\bar{h}=40\cdot 12$, the new threshold following the 2003 reform. Let $\alpha=200$, or 4.2% of annual earnings, within the range that Nickell (1986) uses for blue-collar workers. Let attrition $\sigma=0.05$ and the output price $p=80$. We set the production function to $F(N_t,h_t)=250\ln(N_th_t)$. We choose a logarithmic specification as we have no strong reason to think the cross partial with respect to N and h should be positive or negative.

Appendix A.2 panel (a) shows the optimal employment size in each year if starting at $N_0=N^*=5.1$. Unsurprisingly, the firm remains at $N_t=5.1$ for all t by replacing hires at the rate $a_t=\sigma=0.05$. Panel (b) shows the optimal adjustment path in response to a period τ reform (indexed by 0) that allows the firm to increase hours to $\bar{h}$. We assume the firm learns of the reform in year $\tau-1$. The firm now chooses N_t in each period and when (and if) to optimally increase hours from $\underline{h}$ to $\bar{h}$. We see that the optimal strategy involves slowing down hiring in period $\tau-1$ when the firm learns about the upcoming reform. The firm then ceases hiring in periods τ to $\tau+2$, letting attrition draw down its work force. In period $\tau+2$ the firm increases its workers hours to $\bar{h}$, but employment remains at $N_{\tau+2}>N^{**}$ and the firm continues to not hire. In the following period, the firm resumes hiring. It reaches the new steady state employment

[1] For sufficiently small Δh adjusting immediately can be preferred. This is because hours are constrained and $pF_h(N^*,\underline{h})>wN^*$, so there is some room to increase hours and come out ahead over the wagebill costs.

level (N^{**}) in period $\tau + 4$, and returns to hiring at the rate $a_t = \sigma$ to replace exits. This firm's workers thus face a 2 year earnings adjustment delay (as shown by the shaded region). The model captures the idea that under a fixed output price the firm does not always benefit from immediately increasing hours.

As discussed in the main text, we can then use our simulation to study comparative statics. We show that adjustment is more delayed in declining firms (as proxied for by having a declining output price p) and more repaid in growing firms.

C 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 C.1 compares the estimated elasticity in the pre-reform years to the elasticity obtained using only the mass that responds to each reform. Section C.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 C.3 contains all the details relevant to how we construct the budget sets for the preceding calculations.

C.1 Elasticity Estimation

In section 5.2 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.7–A.8. 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.7–A.8 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 C.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:

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

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 C.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 C.5 shows the relevant model inputs used and Appendix C.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 C.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.

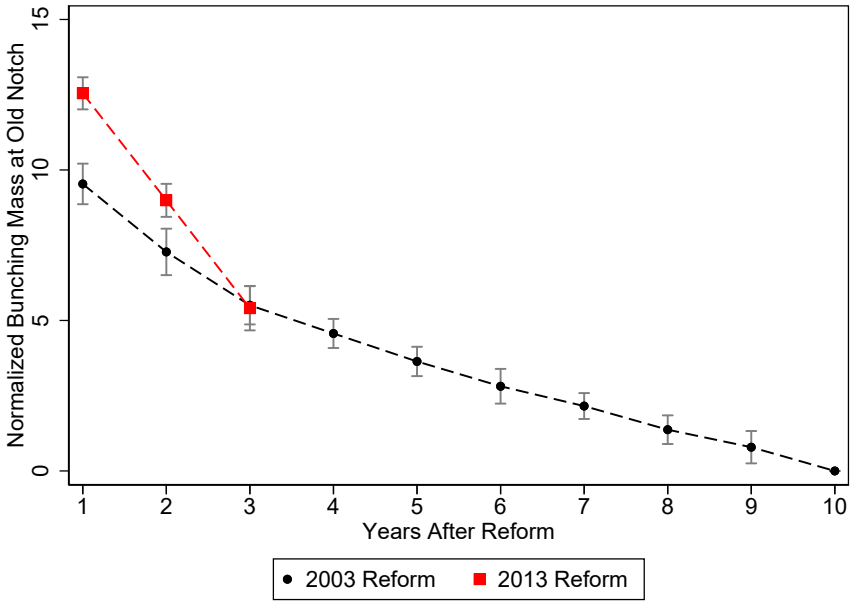
We calculate these 'short-run' elasticities using what we term 'the observed adjustment mass' in each post-reform period. We define the observed adjustment mass as the original 2002/2012 bunching mass less the mass stuck at the old notch in each period. This is meant to capture the real amount of un-bunching 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 C.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.

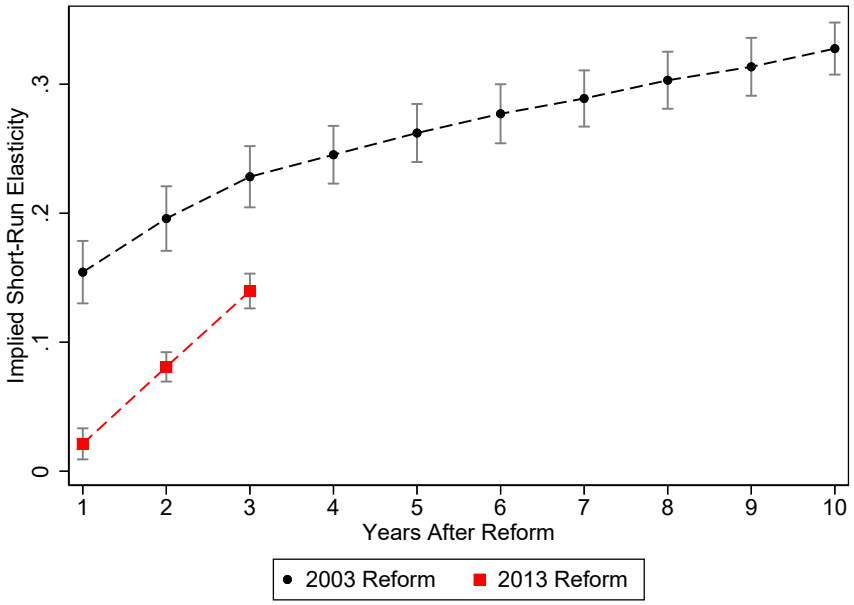
[6] We prefer this method to using the mass at the new notch for two reasons. First, some workers adjust to the reform by moving into the midi job region, so we cannot simply recover the adjusted mass from the mass between the old and new notch. Second, this effectively counts exits from the data or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

Figure C.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.7–A.8 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 C.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.

C.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 C.3. Appendix Table C.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.7–A.8 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 C.1.

We estimate elasticities of 0.328 and 0.228 for 2002 and 2012 respectively (see Appendix Table C.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 C.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 C.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 C.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.7–A.8. 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.

C.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 large 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 C.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 C.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 C.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 C.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 C.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 C.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 C.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 C.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 C.3.

Table C.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.

D 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.

D.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 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 and results are available upon request. We present a summary of our core findings below.

D.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 D.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 D.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 D.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 D.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 D.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 D.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 D.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 D.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 D.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 D.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)

Table 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. 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 D.6 - D.8 show the responses to questions that are path-specific. Table D.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 D.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 D.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 D.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%)

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

Table D.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%)

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

Table D.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%)

Note: 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.

E 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 3—4.

E.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.

E.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

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

first reform (2010 and 2011 for the second reform). The variables used in Section 6 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 E.1: Married Women Sample Summary Statistics (for Figures 2–3)

	mean	sd	min	med	max
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
PANEL A: 2002					
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
Tenure (Years)	4.485	4.257	0	3.321	27.923
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					
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
Tenure (Years)	4.912	5.133	0	3.414	35.901
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 2002	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 2002	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 E.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 E.2: 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	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
Tenure	4.269	3.202	0.164	3.740	28
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
Tenure	5.512	4.588	0.000	4.153	36
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 3. 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 E.2 for sample construction details.

Table E.3: Establishment Sample Summary Statistics (for Table 4)

	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
Tenure	4.347	3.476	0.164	3.740	28
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
Tenure	5.318	4.588	0.000	3.978	36
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 4, the sub-sample of Table 3 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 E.2 for sample construction details.

E.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 D for details and results).

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