

Part-time Work Policies and Older Workers’ Employment: Evidence from German Mini-jobs

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Abstract

This paper studies how part-time work policies shape bridge employment and retirement transitions among older workers, using the 2003 German Mini-job reform as a natural experiment. The reform raised the Mini-job earnings threshold from 325 to 400 Euros per month and abolished the weekly hours cap. Exploiting variation in the age at which different birth cohorts were exposed to the reform, I implement a cohort difference-in-differences design using administrative data from the German Pension Fund. Rather than reducing unemployment or extending working lives as intended, the reform induced older workers to switch from regular to marginal employment without social security contributions, increasing marginal employment by 1.2 percentage points, and accelerated full pension claiming. The shift to marginal employment was driven primarily by women and low-attachment workers, while reductions in unemployment were concentrated among low-income older workers. The findings suggest that part-time work policies may accelerate labor market exit among older workers rather than prolong their participation.

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1 Introduction

Aging populations have been growing rapidly across developed countries. The average old-age dependency ratio in OECD countries reached 33.9 and is expected to double over the next 50 years (OECD, 2024). To address the long-term financial risks to social security and increase labor force participation, the pensionable age has been raised in many countries. In 2022, the average normal retirement age was 64.4 years for men and 63.6 years for women in OECD countries (OECD, 2023). However, it remains challenging for older workers to stay in the labor market until pensionable age, given constraints such as health issues and domestic caring responsibilities. It has therefore become increasingly attractive for older workers to use part-time jobs and unemployment insurance (UI) benefits as a bridge into retirement (Ye, 2022). It is well documented that older workers tend to claim UI benefits for the maximum eligible period right before pension claiming, and that UI benefit generosity shapes their retirement behavior (Inderbitzin et al., 2016; Ye, 2022; Gudgeon et al., 2023). It remains an open question, however, whether older workers would change their bridging strategies and stay in the labor market longer if offered more attractive part-time job options that allow them to reduce working hours and benefit from greater job flexibility.

Motivated by this gap in the literature, this paper studies the effects of part-time work policies on bridge employment and pension claiming. It also offers new insights into the mechanisms through which such policies shape retirement patterns and interact with older workers' working preferences. In Germany, a type of marginal employment known as the *Mini-job* is exempt from income tax and social insurance contributions. In 2003, Germany introduced a large reform that raised the Mini-job earnings threshold from 325 to 400 Euros per month and abolished the maximum working hour requirement. The reform applied to everyone in the German economy and was designed to reduce unemployment and create labor market flexibility. Using this reform as a natural experiment and administrative data from the German Pension Fund, I examine how the reform affected older workers' take-up of marginal employment, their transitions from regular to marginal employment, and the timing and pathway of their pension claiming. Since the reform applied universally, I leverage a cohort difference-in-differences design that compares labor market outcomes of older workers born in different birth cohorts.

A major challenge in studying part-time work policies is finding an empirical setting with a clear institutional definition of marginal employment and large enough reforms to study their effects. Germany provides both. A further advantage is the availability of high-quality administrative data from the German Pension Fund (FDZ-RV), which

provides large samples and fine-grained records of individuals' employment activities from age 14 to 67, including all parallel employment activities observed simultaneously. This enables me to study outcomes that are rarely examined in the literature, including marginal employment as a bridge between regular employment and pension claiming, secondary marginal employment held alongside a main job, and marginal employment among pensioners.

Using the cohort difference-in-differences method, I find several unintended consequences of this reform. First, the 2003 reform did not reduce unemployment as a bridge to retirement. Instead, older workers switched from regular employment to marginal employment without social security contributions as a stepping stone into pension claiming, likely driven by financial incentives from tax exemptions together with older workers' preferences for flexibility. Second, the reform increased the likelihood of pension claiming among older workers, possibly because the switch to marginal employment in their post-career weakened their attachment to the labor market. Third, the reform made it more likely for older workers to claim pensions through the unemployment and part-time job pension pathway. By contrast, the reform had no effect on partial pension claiming. Since the reform made Mini-jobs more attractive, one might expect older workers to claim a partial pension early and combine it with a Mini-job to supplement their income. Instead, they claimed full pensions. Fourth, the reform did not increase marginal employment take-up among pensioners, nor did it increase secondary marginal employment among non-pensioners holding a main job. This suggests that financial incentives alone cannot fully explain older workers' labor market and pension-claiming behavior. Fifth, the heterogeneity analysis shows that the increase in transitions from regular to marginal employment was driven primarily by women, while the reform only reduced unemployment among low-income older workers.

This paper speaks to the literature on bridge employment and retirement. [Ruhm \(1990\)](#) pioneered the concept of the "bridge job", defined as transitional employment between career work and full retirement. Despite a different setting — the US in earlier decades when the standard pensionable age was lower — fewer than 40% of workers in [Ruhm \(1990\)](#) retired directly from their career jobs, highlighting how common bridge employment is. Subsequent work confirms that part-time and self-employed jobs are popular bridge arrangements ([Quinn & Kozy, 1996](#); [Maestas, 2010](#)), and that some retirees "unretire" by returning to work after an initial retirement spell ([Maestas, 2010](#); [Lorenz & Zwick, 2021](#)). However, existing studies are largely descriptive and leave open the question of what drives older workers' bridge job and pension-claiming decisions, particularly in the European context where institutional settings differ substantially from the

US. There is also little causal evidence on pensioners' employment, partly due to data limitations. To the best of my knowledge, this is the first paper to provide causal evidence on how older workers adjust their transition into retirement in response to part-time work policies in a European setting, where bridge employment through unemployment insurance and marginal work plays a central role (Inderbitzin et al., 2016; Ye, 2022; Gudgeon et al., 2023). I also contribute to this literature by showing that pensioners do not strongly respond to the financial incentives created by such policies.

This paper also relates to the literature on older workers' job preferences. Ameriks et al. (2018) documents that workers above 60 are more likely to remain employed in jobs with flexible arrangements, and that 53.5% of bridge jobs are flexible. Ameriks et al. (2020) shows that older Americans have a strong willingness to work, particularly when jobs offer flexible schedules. I contribute to this literature by showing that older workers are willing to switch from regular career jobs to marginal employment, forgoing the opportunity to hold a secondary tax-free Mini-job, which points to flexibility preferences as an important driver of their labor market decisions beyond purely financial considerations.

Finally, this paper contributes to the growing literature on Mini-job reforms (Tazhitdinova, 2020, 2022; Gudgeon & Trenkle, 2024). Tazhitdinova (2020) documents sharp bunching at the Mini-job earnings threshold, reflecting strong behavioral responses to tax incentives. Gudgeon & Trenkle (2024) complements this by showing that adjustment to the new thresholds following the 2003 reform was slow. Tazhitdinova (2022) finds that the 2003 reform increased second job holding. I build on this literature using a different dataset that includes pension claims and parallel employment activity records. Focusing on older workers, I uncover an unintended margin of response that has been overlooked: rather than taking on additional Mini-jobs, older workers used marginal employment as a substitute for regular employment as they transitioned toward retirement.

The rest of the paper proceeds as follows. Section 2 provides an overview of the institutional background of the 2003 Mini-job reform and the German retirement system. Section 3 describes the data and presents summary statistics. Section 4 outlines the cohort difference-in-differences empirical strategy. Section 5 presents the main results. Section 6 discusses the mechanisms behind the reduced-form results, while Section 7 examines heterogeneous effects across subgroups. Section 8 concludes with a discussion of policy implications.

2 Institutions

Mini-job in Germany: The *Mini-job* is a type of marginal employment that has been a key feature of the German labor market. There are 7.6 million Mini-jobbers, of whom 4.9 million hold a Mini-job as their main job (Commission et al., 2018). A Mini-job is defined by a monthly earnings threshold below which income is exempt from social security contributions and income taxation. However, if monthly earnings exceed the threshold, the *total* earnings become subject to both social security contributions and income taxation, creating a notch in the tax schedule that generates strong financial incentives to work below the threshold (Kleven & Waseem, 2013). Prior to April 2003, this threshold was set at 325 Euros per month. Mini-jobbers do not contribute to nursing or unemployment insurance, but may opt into the pension scheme, which provides small pension entitlements in the future. It is worth noting that workers receiving unemployment benefits may also take a Mini-job, subject to an earnings test.¹

The 2003 Mini-job reform, part of the broader Hartz reform package, aimed to reduce unemployment and increase labor market flexibility (Commission et al., 2018). The reform made two main changes. First, the monthly earnings threshold was raised from 325 to 400 Euros. Second, the previous restriction limiting marginal employment to 15 hours per week was abolished. The reform was well publicized by the government and widely understood by labor market participants (Gudgeon & Trenkle, 2024).²

Retirement in Germany: The German public pension scheme operates as a pay-as-you-go system covering 84% of the labor force, with most self-employed workers and civil servants exempted. Pension benefits are calculated based on workers' lifetime earnings, with an average replacement rate of around 50%. The system defines three distinct retirement ages. The Early Retirement Age (ERA) is the earliest age at which workers can claim a pension, though at a permanently reduced rate. The Full Retirement Age (FRA) is the age at which workers can claim their full pension benefits. The Normal Retirement Age (NRA) is the age from which workers can receive benefits above the full rate, rewarding delayed claiming. Specifically, retiring one month before the FRA reduces benefits by 0.3% for each missing month, while delaying retirement beyond the NRA increases benefits by 0.5% per month. The relevant retirement ages for the birth cohorts 1938–1948, which are the focus of this paper, are listed in Table 1. For the birth cohorts 1938–1945, the NRA gradually increased from 62 to 65 while the ERA remained at 60, whereas for cohorts 1946–1948 the ERA progressively rose from 61 to 63 while the NRA remained fixed at 65.

¹For instance, recipients of unemployment benefit type 1 can earn up to an additional 165 Euros per month without a reduction in benefits.

²On the demand side, the employers' social contribution rate was raised from 22% to 25%.

Pension eligibility depends on workers' contribution history, and there are several distinct pathways into retirement, each requiring different minimum contribution periods and associated with different statutory retirement ages, as summarized in [Table 2](#).

In Germany, many workers do not transition directly from regular employment into retirement. Instead, unemployment insurance, marginal employment, and other social insurance programs commonly serve as stepping stones into pension claiming ([Inderbitzin et al., 2016](#); [Ye, 2022](#); [Gudgeon et al., 2023](#)). [Gudgeon et al. \(2023\)](#) documents sharp bunching in UI inflows precisely at the age at which workers become eligible to claim a pension immediately after UI exhaustion. Marginal employment is another popular bridge arrangement. Workers who wish to avoid the permanent benefit reductions associated with early retirement, or who need to fulfill minimum contribution requirements for pension eligibility, have strong incentives to take a Mini-job as a bridge. Pensioners may also take marginal employment to supplement their pension income.³

3 Data

This paper uses the Insurance Account Sample (VSKT), a unique administrative dataset provided by the German Pension Fund (FDZ-RV). The VSKT contains an approximately 2% stratified random sample of individuals with a pension insurance account in Germany, with employment activities recorded as spells with precise start and end dates from age 14 to 67. All periods of social insurance contributions, as well as non-contribution periods relevant to pension benefit calculations such as child-raising periods, are accurately recorded ([Himmelreicher & Stegmann, 2008](#)), and marginal employment spells without social insurance contributions are also included. A key advantage of using the raw administrative records accessed on site at the FDZ-RV, rather than the scientific use file, is the availability of parallel employment activities. This allows me to observe, for instance, individuals holding a marginal job alongside regular employment, or combining marginal employment with pension claiming before age 67, outcomes that are, to the best of my knowledge, unobservable in any other administrative dataset in Germany. This feature is particularly valuable for studying the effects of the Mini-job reform on bridge employment and pensioners' labor supply in the first few years after retirement. Additional variables relevant to pension benefit calculations, such as earnings, pension contribution points, gender, number of children, nationality, and region, are also recorded. Finally, the accurate lifetime employment records allow for heterogeneity analysis based

³Since a 400 Euros/month Mini-job contract falls below the earnings test threshold of 450 Euros/month for pensioners on the early retirement pathway, Mini-jobbers on pensions do not face any benefit reduction.

on individuals' full employment history prior to age 50.

I use waves 2002 and 2004–2019 to construct the sample.⁴ Unique individual identifiers are available from 2011 onwards, which allow me to directly link individuals across waves. For waves prior to 2011, I match individuals across waves by treating two observations as the same individual if they have identical time-invariant characteristics including birth date, birth dates of all children, gender, nationality, and pension start date, as well as the same employment status during the same period. The monthly employment status indicators, recorded as binary variables, are collapsed to yearly averages, so that each variable represents the probability of being in a given employment status in a given year. The main analysis focuses on birth cohorts 1938–1948, who experienced the 2003 reform between ages 55 and 65, the typical age range over which older workers make decisions about bridge employment and retirement. Since marginal employment was not recorded by the FDZ-RV or any German data center prior to 1999,⁵ the sample is restricted to the period from 1999 onwards. This avoids a mechanical increase in marginal employment driven by the expansion of data coverage between 1998 and 1999, and ensures that the pre-reform period used to assess parallel trends is not contaminated by data availability issues. Finally, the main analysis is restricted to individuals above age 50⁶, as the focus of the paper is on older workers who might start to consider bridge employment into retirement, but employment history prior to age 50 is extracted as predetermined characteristics.

Table 3 presents summary statistics for the sample, shown separately for groups excluding and including pensioners. The sample covers individuals born between 1938 and 1948, who were between 55 and 65 years old when the 2003 reform took place, observed from 2000 to 2019. For the group excluding pensioners, 56% of the sample is female. The average employment duration is 271.16 months (22.6 years), the average sick leave duration is 5.15 months (0.4 years), and the average unemployment duration is 10.03 months (0.8 years). As described in Table 2, the German pension system comprises several pathways into retirement, each with different contribution requirements. The share of individuals receiving a disability pension is 2%, while 30% are on the regular old-age pension pathway, 5% on the long-term insured pension pathway, and 8% on the unemployment or part-time pension pathway. The average age at pension claiming is 62.56 years, and

⁴The 2003 wave is unavailable. However, since each wave contains the full lifetime employment history of individuals included in that wave, each wave can be organized as a panel. Using multiple waves helps maximize the number of individuals with a pension account in the sample.

⁵The German government did not formally classify Mini-jobs until 1999.

⁶The main focus is on individuals aged 55 to 65 at the time of the 2003 reform. The sample is extended to include individuals above age 50 to provide sufficient pre-reform observations for assessing parallel trends in the difference-in-differences design.

the average age at last regular employment is 55.93 years. For the group including pensioners, 57% of the sample is female. The average employment duration is 272.79 months (22.7 years), the sick leave duration is 6.32 months (0.5 years), and the unemployment duration is 9.37 months (0.8 years). The share of individuals receiving a disability pension rises to 7%, while 41% are on the regular old-age pension pathway, 5% on the long-term insured pension pathway, and 10% on the unemployment or part-time pension pathway. The average age at pension claiming is 61.73 years, and the average age at last regular employment is 54.46 years. Notably, the gap of around 7 years between the age at last regular employment and the pension claiming age is substantial in both groups, suggesting that many workers do not transition directly from regular employment into retirement, and that bridge employment plays an important role in the transition process. This pattern motivates the focus of this paper on the role of part-time work policies in shaping these intermediate transitions.

4 Empirical methodology

The main identification challenge is that the 2003 Mini-job reform had no eligibility requirements, making it difficult to find a clean control group. To overcome this, I propose a cohort difference-in-differences design that exploits variation in the age at which different birth cohorts were exposed to the reform. [Figure 1](#) illustrates the share of individuals taking a marginal job by birth cohort at different ages. It shows sharp increases at each age when the 2003 reform took place, for all birth cohorts. For instance, the reform occurred when cohort 1948 was 55 years old, and the share taking marginal employment exceeded 14% at that age. Cohort 1947, by contrast, had not yet been exposed to the reform at age 55, since the reform did not take effect for them until age 56. Older cohorts can therefore serve as a counterfactual for younger cohorts at ages where the younger cohort had already been exposed to the reform but the older cohort had not yet been. Since the reform hit different birth cohorts at different ages, this setting naturally generates staggered treatment in the age dimension.

Since the reform applied to all workers simultaneously in 2003, there is no never-treated group. My design instead exploits the fact that different birth cohorts were exposed to the reform at different ages, so that not-yet-treated older cohorts serve as the control for already-treated younger cohorts at each age. This is analogous to a staggered difference-in-differences design where treatment timing varies across cohorts, but the variation is in the age dimension rather than calendar time. I implement this as a dynamic difference-in-differences estimator with staggered treatment, which allows me

to trace out the evolution of treatment effects relative to the reform exposure age for each cohort, while accounting for cohort-specific trends and calendar time fixed effects.

The identifying assumption is that, in the absence of the 2003 reform, the outcome variables of treated cohorts would have evolved in parallel to those of the control cohorts in the age dimension. One potential concern is that comparing different cohorts at the same age also means comparing different calendar years, so the results could potentially be driven by differential time trends across cohorts rather than the reform itself. To address this, I include both calendar year fixed effects and cohort-specific time trends in the regression:

$$Y_{igat} = \sum_{k=A_0}^{-2} \beta_k \times treat_{igk} + \sum_{k=0}^{T_1} \beta_k \times treat_{igk} + X_{igat}\Gamma + \phi_g + \gamma_a + \phi_g \times t + \mu_t + \epsilon_{iat} \quad (1)$$

Y_{igat} is the outcome for individual i in cohort g at age a and calendar year t . The term $\sum_{k=A_0}^{-2} \beta_k \times treat_{igk}$ captures the pre-reform periods and $\sum_{k=0}^{T_1} \beta_k \times treat_{igk}$ captures the post-reform periods, where k denotes the number of periods relative to the reform exposure age of cohort g . Specifically, $treat_{igk}$ equals 1 if cohort g is exactly k periods away from the onset of the reform. For instance, $treat_{ig0}$ is defined as $\Sigma(d_{55} \times age_{55} + d_{56} \times age_{56} + \dots + d_{65} \times age_{65})$, where $d_{55} \times age_{55}$ denotes the interaction between an indicator for the cohort first exposed to the reform at age 55 and an age-55 dummy, and so on. Similarly, $treat_{ig1}$ is defined as $\Sigma(d_{55} \times age_{56} + d_{56} \times age_{57} + \dots + d_{65} \times age_{66})$. Since the reform took place in 2003 and each cohort was at a different age at that point, the treatment onset age varies from 55 for cohort 1948 to 65 for cohort 1938, generating the staggered exposure that identifies the reform effects. The period $k = -1$ is omitted as the baseline, so all effects are measured relative to the year immediately before reform exposure. X_{igat} includes predetermined control variables, namely gender and labor market history prior to age 50. The specification also includes cohort fixed effects ϕ_g , age fixed effects γ_a , cohort-specific time trends $\phi_g \times t$, and calendar time fixed effects μ_t , with standard errors clustered at the cohort level. The cohort-specific time trends $\phi_g \times t$ are particularly important in this setting, as they absorb any pre-existing differences in outcome trajectories across cohorts that could otherwise be confounded with the reform effect. For example, if cohort 1948 had a generally stronger upward trend in marginal employment than cohort 1945 for reasons unrelated to the reform, failing to account for this would lead to an overestimate of the reform effect. The parallel trends assumption is satisfied if the coefficients β_k for $k < -1$ are jointly insignificantly different from zero. The coefficients β_k for $k \geq 0$ then trace out the dynamic treatment effects of the reform, capturing how the behavioral responses

of older workers evolve in the years following their cohort’s exposure to the reform.

5 Main results

This section discusses the effects of the 2003 reform on bridge employment and pension-claiming behavior among older workers aged 55–65 at the time of the reform.

5.1 Results on marginal employment

Figure 2 shows the dynamic cohort difference-in-differences estimates for marginal employment take-up. The reform had an immediate positive effect of 1.2 percentage points in the reform year, which persisted and grew to 1.6 percentage points one year later, before gradually declining in magnitude by year three. There is some evidence of a pre-trend in Figure 2, which raises the concern that the overall increase in marginal employment may partly reflect a pre-existing trend rather than the reform itself. To address this, Figure 3 shows the effects on taking marginal employment as the only job, where the pre-reform coefficients are closer to zero, providing cleaner evidence that the reform drove the increase in marginal employment take-up. Overall, these results suggest that older workers responded relatively quickly to the reform, and that the full adjustment in marginal employment take-up occurred within two years. The relatively fast response is consistent with the reform being well-publicized and widely understood by labor market participants (Gudgeon & Trenkle, 2024), and contrasts with the slower adjustment documented for the broader working-age population, where earnings responses to the new threshold took considerably longer to materialize.

The 2003 reform made marginal employment more attractive both by raising the earnings threshold for tax and social security contribution exemptions and by removing the restriction on holding a Mini-job alongside regular employment. To better understand which margin drives the overall increase, I examine several specific types of marginal employment separately. Figure 3 shows that the positive effects are driven entirely by workers taking marginal employment as their only job, with effects similar in size to those in Figure 2. This already suggests that the increase in marginal employment is not coming from workers adding a Mini-job on top of existing employment. Figure 4 confirms this directly, showing no effect on taking a secondary Mini-job in addition to main employment, indicating that the increase in marginal employment is not driven by moonlighting. This is in contrast to Tazhitdinova (2022), who finds that the 2003 reform increased second job holding among the broader working-age population. The absence of a moonlighting

response among older workers suggests that this group is less responsive to the financial incentives of holding a secondary Mini-job, possibly because they are already in the process of winding down their labor market participation rather than seeking to supplement their income. Similarly, [Figure 5](#) shows no effect on the probability of combining marginal employment with unemployment benefits, despite the fact that this is permitted subject to an earnings test. Taken together, the evidence points to a single dominant margin of response: older workers switching from regular into marginal employment as their primary activity.

[Figure 6](#) shows that the reform increased the take-up of marginal employment as bridge employment, defined as marginal employment spells occurring after regular employment and before pension claiming, with a positive effect of 1.1 percentage points one year after the reform. This is an important result, as it suggests that the new Mini-job options were used not merely as supplementary income but as a deliberate transitional arrangement in the lead-up to retirement. Importantly, while the reform allowed workers to opt out of social insurance contributions, workers could still choose to contribute voluntarily. [Figures 7](#) and [8](#) shed light on this choice: the reform only increased take-up of Mini-jobs without social insurance contributions, with no effect on Mini-jobs with contributions. This is consistent with older workers prioritizing the immediate financial benefit of the contribution exemption over the marginal increase in future pension entitlements that voluntary contributions would provide. It also suggests that older workers near retirement have little incentive to accumulate additional pension contribution periods, as they are likely already close to meeting the eligibility requirements for their preferred retirement pathway. Together, these results suggest that the reform induced older workers to use marginal employment without social security contributions as a stepping stone into retirement.

5.2 Where Do the New Mini-jobbers Come From?

Since the increase in marginal employment is not driven by moonlighting, it must come from workers transitioning out of other employment activities. This section examines the effects of the 2003 reform on regular employment, unemployment, sickness, and inactivity to identify the source of the new Mini-jobbers.

[Figure 9](#) shows that the reform led to a decline in regular employment of about 2.5 percentage points, an effect that persisted for three years after the reform. Notably, the magnitude of this decline is broadly comparable to the increase in marginal employment documented in [Section 5.1](#), suggesting that the two are closely related and that the reform

essentially facilitated a reallocation of older workers from regular to marginal employment rather than drawing new workers into the labor market. Also, there is no effect on unemployment among older workers. This finding is somewhat surprising: given the sharp bunching at age 58 visible in [Figure 1](#), unemployment insurance is commonly used as a stepping stone into retirement among this age group, and one might have expected the availability of more attractive part-time work options to draw some workers away from unemployment and into marginal employment. However, this substitution does not appear to occur at the population level. One possible explanation is that for most older workers, unemployment benefits are sufficiently generous relative to a 400 Euro Mini-job contract that the financial incentive to switch from unemployment to marginal employment is limited. As I will show in [Section 7](#), the reform did reduce unemployment among low-income older workers, who by definition cannot earn more from a Mini-job contract than from their unemployment benefits, making marginal employment a relatively more attractive bridge option for this group. This heterogeneity result is consistent with the idea that the decision to use marginal employment versus unemployment as a bridge into retirement is primarily driven by the relative financial returns of the two options for each worker. [Figures 10](#) and [11](#) show no significant effects on sick leave or inactivity, indicating that the reform did not induce workers to transition out of these states either. In the case of inactivity, where workers have no income at all, the absence of a response is particularly striking, as one might expect the financial incentive of a tax-exempt Mini-job to be especially attractive for this group. The fact that inactive workers did not respond suggests that inactivity among older workers reflects deeper constraints, such as health limitations or family caring responsibilities, that cannot be overcome by financial incentives alone.

Taken together, the results above suggest that the main source of the new Mini-jobbers is workers transitioning directly out of regular employment. To test this more directly, I construct an indicator variable for switching from regular employment to marginal employment, conditional on the post-switch status being marginal employment. [Figure 12](#) confirms that the 2003 reform significantly increased the probability of this transition, with effects that are persistent and grow slightly over the post-reform period. The magnitude of this switching effect is consistent with the decline in regular employment shown in [Figure 9](#), providing further evidence that the reform primarily reshuffled older workers between employment types rather than affecting the extensive margin of labor force participation. The effects on switches between all other pairs of employment activities are examined in the Appendix, none of which yield significant results, further supporting the conclusion that the regular-to-marginal employment transition is the dominant

behavioral response among older workers.

5.3 Results on pension claiming

As discussed in [Section 2](#), workers in Germany can claim a full pension at the FRA or opt for an early pension subject to actuarial penalties. In principle, the introduction of more attractive part-time work options could induce workers to claim a partial pension early while supplementing their income with a Mini-job. This would represent a form of gradual retirement, where workers smoothly reduce their labor supply while maintaining a stable total income by combining a reduced pension with earnings from marginal employment. [Figure 13](#) shows that this margin of response did not materialize: even before the reform, partial pension take-up was virtually zero, and the reform had no effect on this margin.⁷ This suggests that the partial pension option is not an attractive arrangement for older workers in Germany, possibly because the combination of a reduced pension and a low-earning Mini-job does not provide sufficient income replacement relative to remaining in regular employment or claiming a full pension. It is also possible that the complexity of the partial pension rules or the stigma associated with partial retirement discourages take-up.

In contrast, [Figure 14](#) shows that the reform had a significant and persistent positive effect on full pension claiming, with coefficients ranging from 5.6 to 7.3 percentage points over the three years following the reform. This is a quantitatively large effect and suggests that the shift from regular to marginal employment documented in the previous section weakened older workers' attachment to the labor market, ultimately bringing forward their pension claiming decision. The mechanism is intuitive: once a worker has already reduced their labor supply by switching from regular to marginal employment, the additional utility cost of exiting the labor market entirely and claiming a pension is lower. In other words, marginal employment serves not only as a financial bridge but also as a psychological stepping stone toward full retirement. The persistence of this effect beyond three years further suggests that the behavioral response represents a permanent shift in retirement timing rather than a temporary adjustment.

To better understand which pension pathways are driving this result, [Figures 15–18](#) examine the effects on each pathway separately. [Figure 15](#) shows no effect on disability pensions, consistent with the earlier finding that the reform did not induce transitions out of sickness or inactivity, and suggesting that health-related retirement decisions are not

⁷The coefficients in [Figure 13](#) are very small in magnitude and should be interpreted as economically negligible rather than exactly zero.

sensitive to the availability of more attractive part-time work options. Figure 16 shows no effect on long-term insured pension claiming, which is expected because workers who chose marginal employment without social security contributions did not accumulate additional contribution periods and thus did not become eligible for this pathway any earlier. This result also provides an additional piece of evidence that financial incentives alone cannot fully explain older workers' behavior: even though contributing voluntarily to the pension scheme while on a Mini-job would have extended their contribution history and potentially opened up the long-term insured pathway, workers did not choose to do so. Instead, the positive effects on pension claiming are concentrated in two pathways. Figure 17 shows positive effects on the regular old-age pension pathway, with magnitudes ranging from 3.4 to 3.7 percentage points, consistent with workers who switched from regular to marginal employment eventually claiming through the standard retirement route once they meet the minimum contribution requirements. Figure 18 shows positive effects of 1.1 to 1.7 percentage points on the part-time job and unemployment pension pathway,⁸ suggesting that some workers used marginal employment as a strategy to qualify for this pathway, which requires a period of part-time or unemployment activity before retirement. Together, these results paint a consistent picture: the reform accelerated the transition into retirement through the regular and part-time or unemployment pension pathways, without affecting disability or long-term insured pensions, and did so primarily by facilitating the use of marginal employment as a bridge into full pension claiming.

5.4 Results on pensioners' marginal employment

Mini-jobs are popular among pensioners as they provide both flexibility and a source of supplementary income. Figure 19 shows that the 2003 reform had no significant effect on marginal employment take-up among pensioners, whether full or partial, up to age 67. Before interpreting this result, two important data limitations should be acknowledged. First, the data only track individuals until age 67, so it is not possible to observe whether pensioners increase their marginal employment take-up later in retirement. Second, while marginal employment without social insurance contributions is well documented prior to pension claiming, the pension fund may not systematically record such spells after retirement, as they are no longer relevant for pension benefit calculations. This means the data may undercount marginal employment among pensioners.

⁸In the German pension data, it is not possible to separately identify the part-time job pension pathway and the unemployment pension pathway.

With these caveats in mind, the results nonetheless provide some suggestive evidence on the first years of retirement life for this cohort of German pensioners. The absence of a significant reform effect up to age 67 suggests that the behavioral response to the reform was more concentrated in the pre-retirement period, and that the financial incentives provided by the reform were not sufficient to induce a measurable change in pensioners' labor supply within the observation window.⁹ This finding relates to the broader literature on "unretirement" (Maestas, 2010; Lorenz & Zwick, 2021), though the data limitations discussed above preclude strong conclusions about pensioners' longer-run labor supply responses to the reform.

5.5 Placebo test

To validate the identification design, I estimate the main regression using a sample of workers who were already on a Mini-job before the 2003 reform. Since these workers were already in marginal employment, they should not be affected by the reform in the same way as the main sample. Figures 20–22 show no significant effects on marginal employment, regular employment, or unemployment for this group, which is reassuring. If the cohort difference-in-differences design were picking up spurious trends rather than the reform effect, we would expect to see similar patterns in this placebo sample, but we do not. This provides confidence that the main results are driven by the reform rather than by pre-existing differences across cohorts.

6 Mechanisms

The main results show significant positive effects on marginal employment and negative effects on regular employment, but no effects on unemployment or inactivity among older workers. This suggests that the Mini-job reform did not prolong working life or draw unemployed workers into part-time employment as originally intended. Instead, the reform induced workers to switch from regular to marginal employment as a stepping stone into pension claiming, and increased the likelihood of pension take-up. In this section, I discuss several possible mechanisms behind these findings.

⁹In Germany, pensioners do not contribute to nursing or unemployment insurance.

6.1 Strategic planning: early retirement while doing marginal employment

One possible mechanism is that workers strategically plan their retirement by claiming an early pension at the ERA, accepting the actuarial penalty of 0.3% per missing month relative to the FRA, while simultaneously taking a Mini-job to supplement their reduced pension income. In the Appendix, I provide calculations of how early a worker could retire while doing a Mini-job and maintain the same total income, assuming a full pension of 1,200 Euros per month and a 400 Euro Mini-job contract. The calculations show that such a worker could in principle retire up to 111 months (9.3 years) before the FRA while fully offsetting the pension reduction through Mini-job earnings. In practice, the gap between the ERA and FRA is much smaller. Assuming an ERA of 63 and an FRA of 65, a worker with a strong preference for early retirement, such as one wishing to retire jointly with a spouse, would need to work on a Mini-job for only around 4 months at any point during retirement to maintain the same lifetime income. These calculations suggest that early retirement combined with marginal employment could be a financially viable strategy for some workers.

However, the empirical evidence does not support this mechanism. [Figure 19](#) shows no significant effect of the reform on marginal employment among pensioners, and [Figure 13](#) shows no effect on early partial pension take-up. While the data limitations discussed in [Section 5.4](#) mean that these null results in [Figure 19](#) should be interpreted with caution, taken together they provide little support for strategic retirement planning as the primary driver of the main results.

6.2 Financial incentives

Financial incentives undoubtedly played a role in driving the behavioral responses documented in this paper, given that the core of the reform was to exempt Mini-job earnings from taxes and social insurance contributions. However, financial incentives alone cannot fully account for the results. If financial incentives were the primary driver, we would expect to see a strong increase in secondary Mini-job holding among older workers still in regular employment, since the reform made it possible to earn up to 400 Euros per month tax-free on top of a regular wage. [Figure 4](#) shows no such effect. These patterns suggest that while financial incentives are part of the story, they are insufficient on their own to explain the full pattern of results, and that other factors must be at play.

6.3 Flexibility preference

A more compelling explanation for the results is that older workers have a strong preference for flexible working arrangements, and that Mini-jobs, which are known for flexible hours, minimal administrative requirements, and ease of entry, are particularly well suited to meeting this preference. As documented by [Ameriks et al. \(2020\)](#) and [Ameriks et al. \(2018\)](#), older workers place a high value on schedule flexibility, and a majority of bridge jobs held by older Americans are flexible. The switch from regular to marginal employment observed in [Figure 12](#) is consistent with this interpretation: rather than simply responding to financial incentives, older workers appear to be trading a higher-paying but more rigid regular job for a lower-paying but more flexible Mini-job as they approach retirement. It is worth noting that some of the increase in marginal employment from regular jobs could be mechanically driven by the raise in the Mini-job threshold from 325 to 400 Euros, as some workers previously earning just above the old threshold would now qualify as Mini-jobbers without changing their actual hours or earnings. However, this mechanical channel is unlikely to fully account for the results. In particular, I find positive effects on the switch from regular to marginal employment even among workers with a monthly income above 400 Euros prior to the reform, for whom the threshold change is not directly relevant and the financial incentive of the contribution exemption is relatively small. Unfortunately, the VSKT data do not contain information on working hours or job characteristics, so it is not possible to provide direct causal evidence that flexibility preferences drive the results. Nevertheless, the overall pattern of findings is more consistent with a flexibility preference story than with a purely financial one.

7 Heterogeneous effects

This section examines heterogeneous effects across three dimensions: gender, income, and employment history.

[Table 4](#) reports the heterogeneous effects by gender. In the first two years after the reform, the increase in marginal employment and the decline in regular employment were both larger for women than for men, indicating that women were more likely to switch from regular to marginal employment following the reform. The effect on marginal employment is 1.8 percentage points for women compared to 0.7 percentage points for men in the first year, and grows to 2.2 percentage points versus 1.3 percentage points in the second year. This gender difference is consistent with women having stronger preferences for flexible working arrangements, partly driven by greater domestic and caring respon-

sibilities. The flexibility offered by Mini-jobs, including flexible hours and ease of entry and exit, may have been particularly valuable for women managing competing work and family demands, making the switch from regular employment more attractive for them than for men. It is also worth noting that women in this cohort tend to have less continuous employment histories and lower earnings than men, which may make the financial cost of switching to a lower-paying Mini-job smaller for women.

[Table 5](#) reports the heterogeneous effects by income. The income split is motivated by the observation that the decision to use marginal employment versus unemployment benefits as a bridge into retirement depends critically on the level of unemployment benefits a worker is eligible for. In Germany, unemployment benefits amount to 67% of previous earnings for workers with at least one dependent child, and 60% for those without children. Workers whose eligible unemployment benefits fall below the earnings from a full 400 Euro Mini-job contract are therefore defined as the low-income sample, namely workers with children earning below 667 Euros per month and workers without children earning below 597 Euros per month prior to the reform. This definition captures workers for whom marginal employment is financially more attractive than unemployment as a bridge into retirement, providing an economically motivated way to split the sample. The low-income group shows a substantially larger increase in marginal employment take-up, with a coefficient of 5.6 percentage points two years after the reform, which is considerably larger than for any other subgroup. This large response among low-income workers is intuitive: for workers whose eligible unemployment benefits are lower than the earnings from a 400 Euro Mini-job contract, the reform effectively made marginal employment the most financially attractive bridge option available. Importantly, unemployment also declined significantly for the low-income group two years after the reform, by 3.7 percentage points, providing direct evidence of substitution from unemployment to marginal employment within this group. This result also helps explain the absence of an unemployment effect at the population level documented in [Section 5](#): the substitution from unemployment to marginal employment is concentrated among low-income workers, for whom the financial incentive to make this switch is strongest, and is not large enough in aggregate to generate a detectable effect at the population level. For the high-income group, the reform also reduced regular employment but had no significant effect on marginal employment in the first year, suggesting that high-income workers who left regular employment did not substitute into marginal employment to the same degree, possibly because their unemployment benefits are sufficiently generous to make unemployment a more attractive bridge option.

[Table 6](#) reports the heterogeneous effects by employment history length prior to age

50. I split the sample based on whether individuals have an employment history length below or above the median prior to age 50.¹⁰ The positive effects on marginal employment are larger for workers with a below-median employment history, with effects of 2.4 and 2.6 percentage points in the first and second years, respectively, compared to small and insignificant effects for workers with an above-median employment history. Workers with shorter employment histories may have weaker labor market attachment, making them more willing to accept the reduction in earnings and social security contributions associated with switching to a Mini-job. They are also less likely to hold tenured positions in career jobs, reducing the cost of transitioning to marginal employment. For workers with an above-median employment history, the reform instead reduced regular employment and increased unemployment, suggesting that more strongly attached workers responded to the reform by exiting regular employment but turned to unemployment rather than marginal employment as their bridge activity. This is consistent with the idea that workers with longer employment histories tend to have higher previous earnings and are therefore eligible for more generous unemployment benefits, making unemployment a more attractive bridge option relative to a 400 Euro Mini-job contract. Taken together, the heterogeneity results paint a coherent picture: the switch from regular to marginal employment is driven by groups for whom marginal employment is either financially attractive relative to unemployment, such as low-income workers, or for whom flexibility is particularly important, such as women and workers with weaker labor market attachment.

8 Conclusion

Whether older workers would adjust their bridge strategies and extend their labor market participation when offered more attractive part-time work options remains an important open question for policymakers. This paper studies this question using the 2003 German Mini-job reform as a natural experiment. The reform raised the Mini-job earnings threshold from 325 to 400 Euros per month and abolished the 15-hour weekly hours limit, with the stated goal of reducing unemployment and increasing labor market flexibility. Since the reform applied universally, I exploit variation in the age at which different birth cohorts were exposed to the reform, implementing a cohort difference-in-differences design that uses older cohorts as a counterfactual for younger cohorts at ages where the younger cohort had already been exposed to the reform but the older cohort had not yet.

¹⁰I also split the sample based on the 25th, 50th, and 75th percentiles, and the results are displayed in the Appendix.

The findings reveal several unintended consequences of the reform among older workers. Rather than reducing unemployment or extending working lives, the reform primarily induced transitions from regular to marginal employment without social security contributions, serving as a stepping stone into retirement. Full pension claiming increased by 5.6 to 7.3 percentage points, concentrated in the regular old-age pension pathway and the part-time job and unemployment pension pathway. The reform had no effect on disability pensions or long-term insured pensions, consistent with the finding that workers opted out of social insurance contributions when taking a Mini-job, leaving their contribution histories unchanged. Notably, the reform did not increase secondary Mini-job holding among non-pensioners, nor did it increase marginal employment among pensioners, suggesting that financial incentives alone cannot account for the behavioral responses observed. Instead, the results point to older workers' preferences for flexible working arrangements as an important driver of the switch from regular to marginal employment. Heterogeneity analysis reveals that the transition to marginal employment was driven primarily by women and workers with weaker labor market attachment, while the reduction in unemployment was concentrated among low-income workers for whom a Mini-job contract offers better financial returns than unemployment benefits.

These findings carry several policy implications. First, part-time work policies aimed at extending older workers' labor market participation may have the unintended effect of accelerating retirement transitions if they make marginal employment an attractive stepping stone out of regular work. The 2003 Mini-job reform is a case in point: rather than keeping older workers in the labor market longer, it facilitated an earlier and smoother exit. Second, the heterogeneous responses documented here highlight the importance of accounting for workers' diverse financial circumstances and preferences when designing labor market policies. A policy that reduces unemployment among low-income workers while inducing higher-income workers to reduce their working hours may have ambiguous aggregate effects on labor supply and social security finances. Together, these results suggest that the design of part-time work policies requires careful consideration of how older workers' preferences and economic circumstances interact with the incentives created by the reform, and that policies intended to increase labor market flexibility can have unintended consequences for retirement transitions that are difficult to anticipate *ex ante*.

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Table 1: Statutory Retirement Ages for Birth Cohorts 1938–1948 in Germany

Birth Year	Early Retirement Age	Normal Retirement Age
1938	60	rising to 62 (2000)
1939	60	rising to 63 (2002)
1940	60	rising to 64 (2004)
1941	60	rising to 65 (2006)
1942	60	65
1943	60	65
1944	60	65
1945	60	65
1946	rising to 61 (2007)	65
1947	rising to 62 (2009)	65
1948	rising to 63 (2011)	65

Note: Table 1 reports the statutory Early Retirement Age (ERA) and Normal Retirement Age (NRA) for birth cohorts 1938–1948 in Germany. The ERA is the earliest age at which individuals can claim a pension, subject to actuarial deductions of 0.3% per month below the Full Retirement Age. The NRA is the age from which individuals receive benefits above the full rate, with a bonus of 0.5% per month of delay. For cohorts 1938–1945, the ERA was fixed at 60 while the NRA gradually increased from 62 to 65. For cohorts 1946–1948, both ages rose, with the ERA reaching 63 and the NRA fixed at 65. Source: Riphahn & Schrader (2021).

Table 2: Pension Claiming Pathways in Germany

Pathway	Required Contribution Years	Other Requirements
Regular	5	Unemployed or in part-time employment before retirement
Long-term Insured	35	
Unemployed/Part-time	15	
Invalidity	35	Disability status
Disability	5	Stricter disability status

Note: Table 2 summarizes the main pension claiming pathways in the German statutory pension system, listing the minimum required contribution years and any additional eligibility conditions. The Regular pathway requires only 5 contribution years and is available to most workers. The Long-term Insured pathway requires 35 years and allows for earlier retirement. The Unemployed/Part-time pathway requires 15 years of contributions and a period of unemployment or part-time work immediately before retirement. The Invalidity and Disability pathways require medical certification of reduced earning capacity. Source: Seibold (2021).

Table 3: Summary of Statistics

Variable	Excluding Pensioners	Including Pensioners
Female	0.56 (0.50)	0.57 (0.50)
Employment Duration	271.16 (118.98)	272.79 (117.57)
Sick Duration	5.15 (8.74)	6.32 (10.31)
Unemployment Duration	10.03 (20.79)	9.37 (19.88)
Disability Pension	0.02 (0.15)	0.07 (0.25)
Old Age Pension	0.30 (0.46)	0.41 (0.49)
Long Insured Pension	0.05 (0.22)	0.05 (0.22)
Unemploy/part-time Pension	0.08 (0.27)	0.10 (0.29)
Pension Claim Age	62.56 (2.73)	61.73 (4.00)
Last Employment Age	55.93 (12.42)	54.46 (12.19)
Observations	575,253	994,826

Note: Table 3 reports means and standard deviations (in parentheses) for the main analysis sample, shown separately for groups excluding and including pensioners. The sample covers birth cohorts 1938–1948, observed from 2000 to 2019, collapsed to the yearly level. Employment, sick leave, and unemployment durations are measured in months prior to age 50 and used as predetermined controls. Pension pathway variables are indicator variables equal to one if the individual ever claims through the respective pathway. Pension claim age and last regular employment age are measured in years. The gap between these two ages reflects the prevalence of bridge employment arrangements in the sample.

Table 4: Heterogeneous Effects by Gender

	Female	Male
1 Year Effect		
Marginal	0.018* (0.010)	0.007** (0.003)
Regular_main	-0.028** (0.011)	-0.025*** (0.007)
Unemploy	0.007 (0.007)	0.022*** (0.007)
Inactive	0.004 (0.004)	0.002 (0.004)
Sick	-0.001 (0.001)	-0.005** (0.002)
2 Year Effect		
Marginal	0.022** (0.010)	0.013*** (0.003)
regular_main	-0.030** (0.012)	-0.025*** (0.007)
Unemploy	-0.003 (0.008)	0.019*** (0.007)
Inactive	0.001 (0.004)	-0.001 (0.003)
Sick	0.002** (0.001)	0.001 (0.001)
3 Year Effect		
Marginal	0.010 (0.008)	0.003 (0.003)
Regular_main	-0.034*** (0.011)	-0.006 (0.007)
Unemploy	0.015* (0.008)	0.006 (0.009)
Inactive	-0.001 (0.003)	-0.005 (0.004)
Sick	-0.001 (0.001)	-0.002 (0.002)

Note: Table 4 reports heterogeneous treatment effects by gender, estimated from Equation 1. The dependent variables are yearly average employment activity indicators for marginal employment, regular employment, unemployment, inactivity, and sickness. Coefficients represent the effect of the 2003 Mini-job reform at 1, 2, and 3 years after each cohort's reform exposure age. Standard errors clustered at the cohort level are shown in parentheses. The sample excludes pensioners and covers birth cohorts 1938–1948. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Heterogeneous Effects by Income

	Low Income	High Income
1 Year Effect		
Marginal	0.019* (0.011)	0.004 (0.003)
Regular_main	-0.022*** (0.005)	-0.016** (0.007)
Unemploy	0.013 (0.015)	0.014*** (0.005)
Inactive	-0.011 0.008	0.000 0.003
Sick	-0.002** 0.001	-0.002 0.002
2 Year Effect		
Marginal	0.056*** (0.015)	0.005* (0.003)
Regular_main	-0.013* (0.007)	-0.039*** (0.009)
Unemploy	-0.037*** (0.013)	0.027*** (0.007)
Inactive	-0.003 (0.007)	0.000 (0.003)
Sick	-0.001 (0.001)	0.003*** (0.001)
3 Year Effect		
Marginal	0.010 (0.011)	0.006** (0.003)
Regular_main	0.007* (0.004)	-0.021*** (0.008)
Unemploy	-0.005 (0.012)	0.009 (0.006)
Inactive	-0.020*** (0.007)	0.001 (0.003)
Sick	-0.001 (0.001)	-0.001 (0.002)

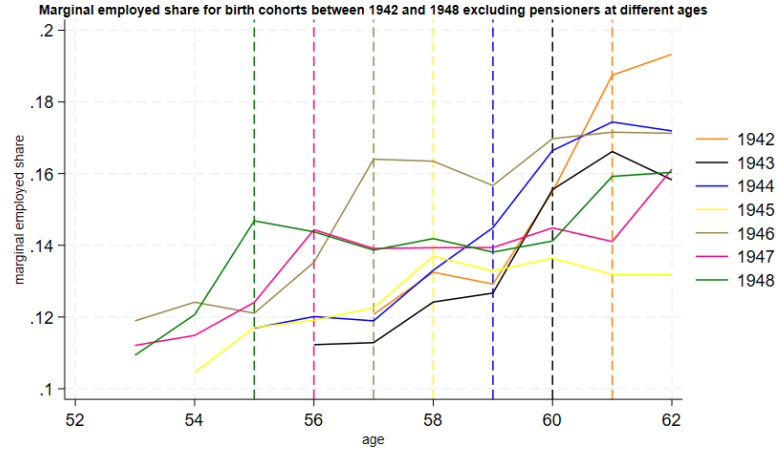
Note: Table 5 reports heterogeneous treatment effects by income group, estimated from [Equation 1](#). The low-income group is defined as workers whose eligible unemployment benefits are lower than the earnings from a full 400/month Mini-job contract: specifically, workers with dependent children earning below 667/month and workers without children earning below 597/month prior to the reform. The high-income group comprises all remaining workers. The dependent variables are yearly average employment activity indicators. Standard errors clustered at the cohort level are shown in parentheses. The sample excludes pensioners and covers birth cohorts 1938–1948. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 26

Table 6: Heterogeneous Effects by Employment History

	Below Median	Above Median
1 Year Effect		
Marginal	0.024*** (0.009)	-0.002 (0.002)
Regular	-0.003 (0.007)	-0.026*** (0.009)
Unemploy	-0.013* (0.007)	0.023** (0.009)
Inactive	-0.002 0.004	0.004 0.003
Sick	-0.006*** 0.001	0.001 0.002
2 Year Effect		
Marginal	0.026*** (0.009)	0.004 (0.003)
Regular_main	0.002 (0.007)	-0.031*** (0.010)
Unemploy	-0.031*** (0.009)	0.023** (0.010)
Inactive	-0.002 (0.004)	0.001 (0.003)
Sick	-0.001 (0.001)	0.004*** (0.001)
3 Year Effect		
Marginal	0.003 (0.007)	0.000 (0.003)
Regular_main	-0.001 (0.006)	-0.015 (0.010)
Unemploy	-0.010 (0.008)	0.014 (0.011)
Inactive	-0.004** (0.002)	-0.004 (0.003)
Sick	-0.002** (0.001)	0.000 (0.001)

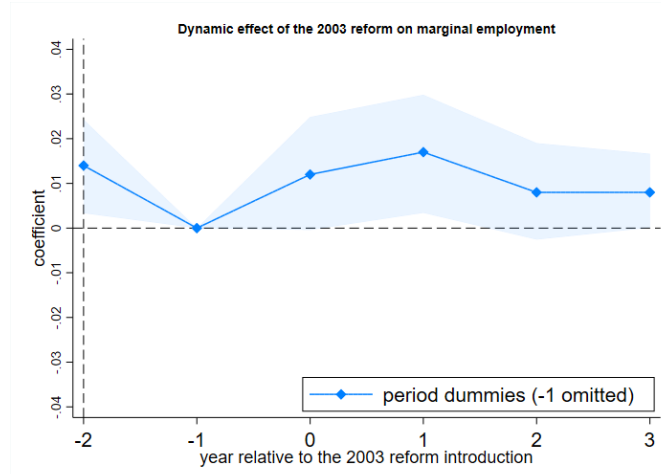
Note: Table 6 reports heterogeneous treatment effects by employment history prior to age 50, estimated from [Equation 1](#). The below-median group comprises individuals whose total months of employment before age 50 fall below the sample median, and the above-median group comprises the remainder. Employment history prior to age 50 is used as a proxy for labor market attachment. The dependent variables are yearly average employment activity indicators. Standard errors clustered at the cohort level are shown in parentheses. The sample excludes pensioners and covers birth cohorts 1938–1948. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: Marginal Employment Take-up by Birth Cohort and Age



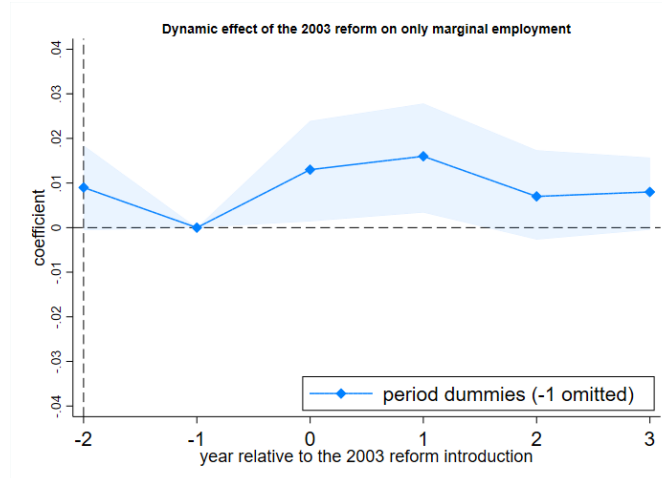
Note: Figure 1 plots the share of individuals in marginal employment at each age for birth cohorts 1942–1948, using annual data. Each line corresponds to one cohort, and the vertical dashed line of the same color marks the age at which the 2003 Mini-job reform took effect for that cohort. The sharp increases in marginal employment at the reform age for each cohort provide the visual motivation for the cohort difference-in-differences design. The sample excludes pensioners.

Figure 2: Effect of the 2003 Reform on Marginal Employment Take-up



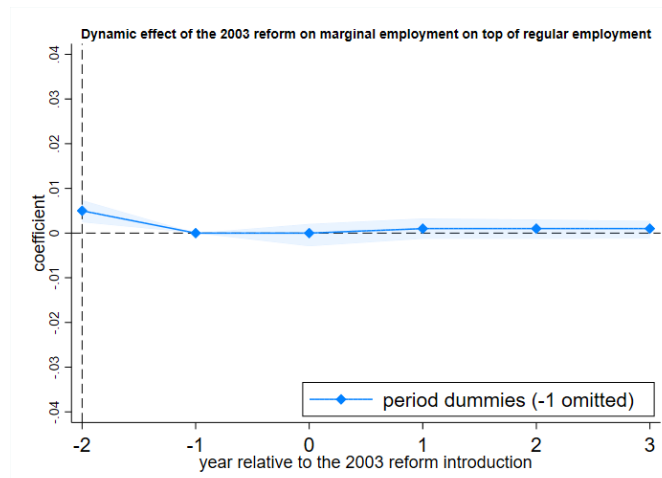
Note: Figure 2 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for overall marginal employment take-up. The outcome variable is the yearly average probability of being in marginal employment. Period $k = -1$ is omitted as the baseline. $k = 0$ denotes the reform year for each cohort, $k = 1$ the one-year lag, and so on. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level. The sample excludes pensioners and covers birth cohorts 1938–1948.

Figure 3: Effect of the 2003 Reform on Marginal Employment as the Only Job



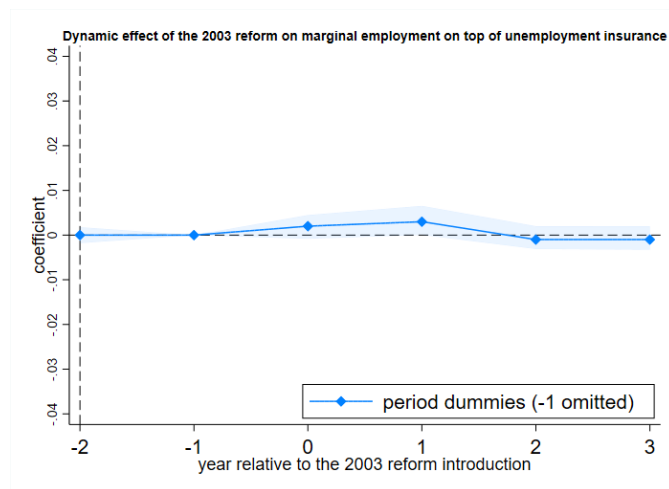
Note: Figure 3 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for marginal employment as the worker's only job, excluding cases where marginal employment is held alongside regular employment or unemployment benefits. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level. The sample excludes pensioners.

Figure 4: Effect of the 2003 Reform on Secondary Marginal Employment Alongside Main Employment



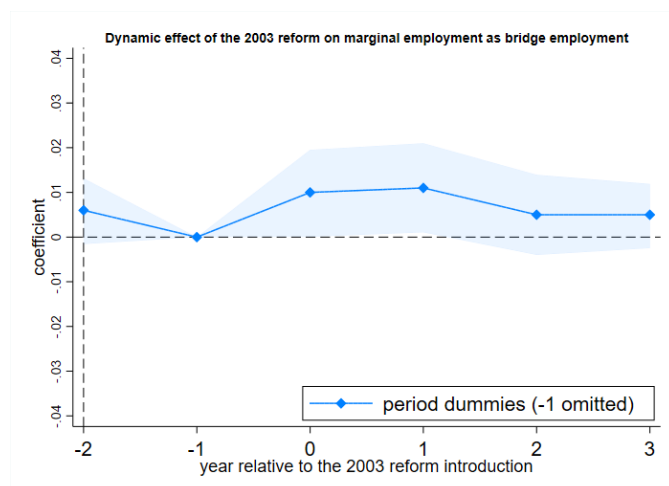
Note: Figure 4 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for holding a Mini-job as a secondary job alongside a main regular employment spell. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level. The sample excludes pensioners.

Figure 5: Effect of the 2003 Reform on Marginal Employment Alongside Unemployment Benefits



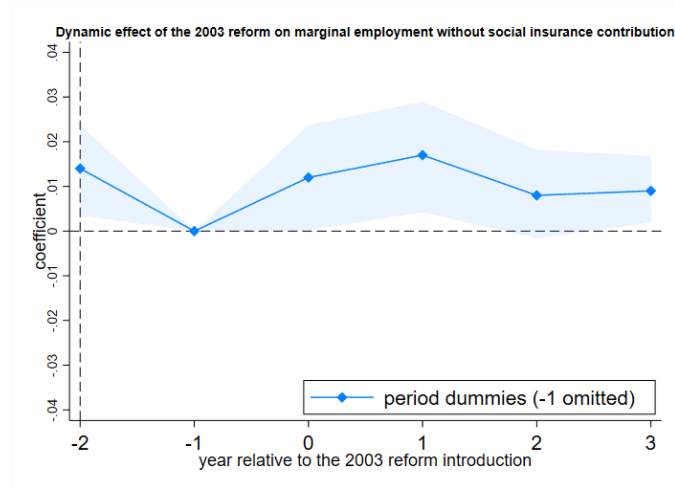
Note: Figure 5 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for holding a Mini-job alongside unemployment benefit receipt. Although this combination is permitted subject to an earnings test, the reform had no significant effect on this margin. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level. The sample excludes pensioners.

Figure 6: Effect of the 2003 Reform on Marginal Employment as Bridge Employment



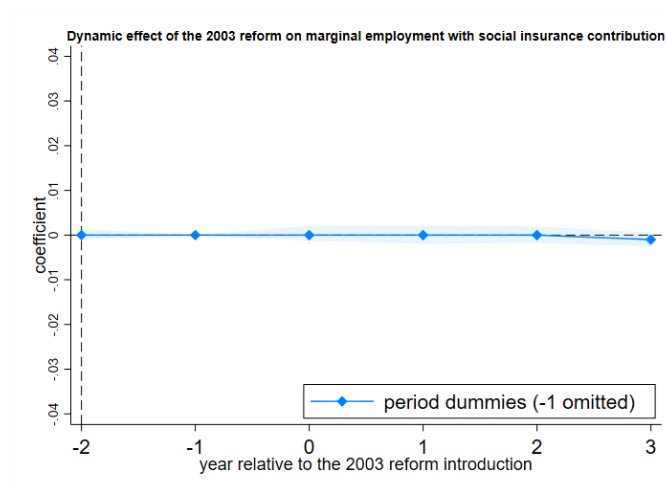
Note: Figure 6 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for marginal employment as bridge employment, defined as a marginal employment spell occurring after the end of regular employment and before the start of pension claiming. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level. The sample excludes pensioners.

Figure 7: Effect of the 2003 Reform on Marginal Employment without Social Insurance Contributions



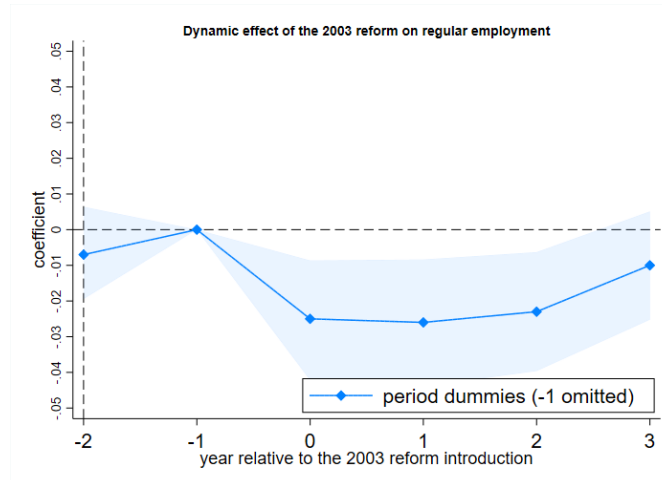
Note: Figure 7 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for marginal employment spells where the worker opted out of social insurance contributions. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level. The sample excludes pensioners.

Figure 8: Effect of the 2003 Reform on Marginal Employment with Social Insurance Contributions



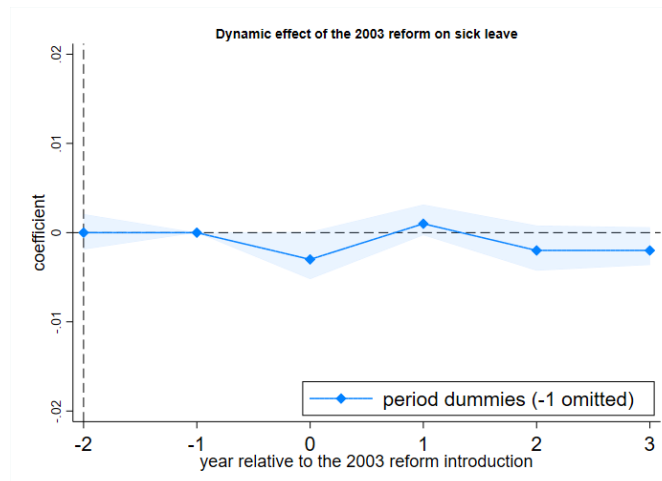
Note: Figure 8 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for marginal employment spells where the worker voluntarily opted into social insurance contributions. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level. The sample excludes pensioners.

Figure 9: Effect of the 2003 Reform on Regular Employment



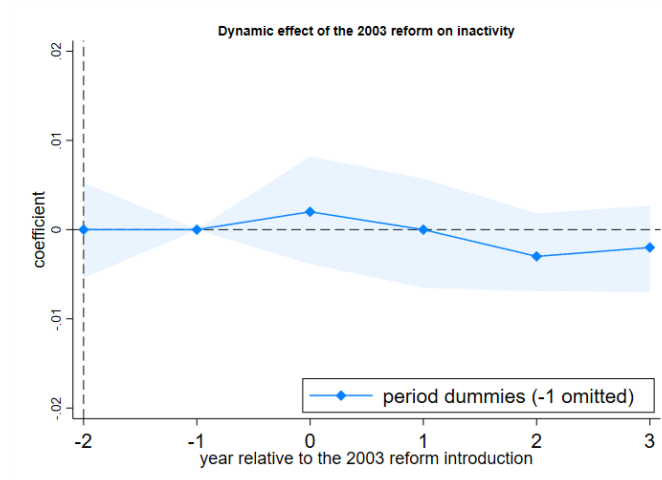
Note: Figure 9 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for regular employment. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level. The sample excludes pensioners.

Figure 10: Effect of the 2003 Reform on Sick Leave



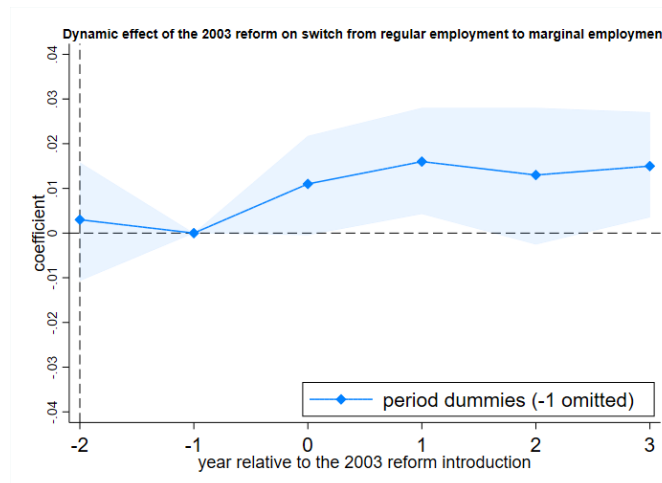
Note: Figure 10 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for sick leave. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level. The sample excludes pensioners.

Figure 11: Effect of the 2003 Reform on Inactivity



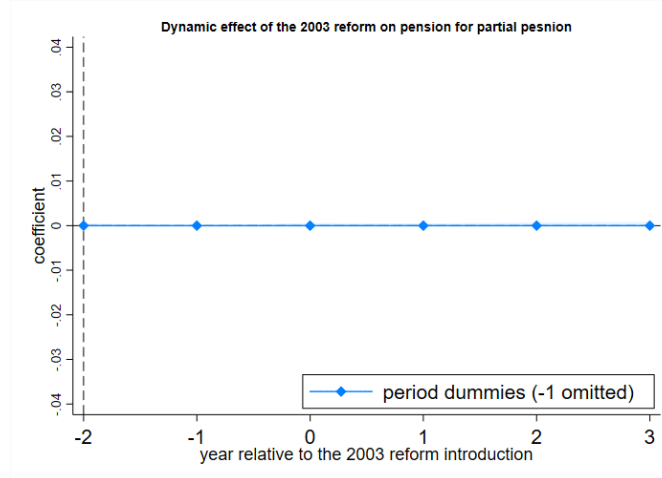
Note: Figure 11 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for inactivity, defined as periods with no recorded employment activity and no receipt of social insurance benefits. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level. The sample excludes pensioners.

Figure 12: Effect of the 2003 Reform on Switching from Regular to Marginal Employment



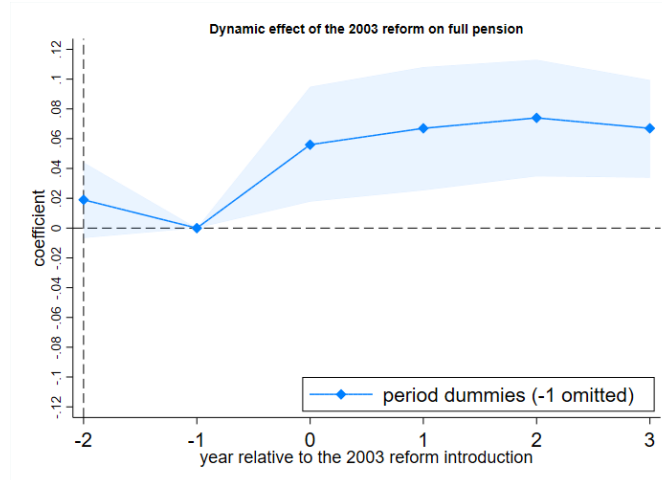
Note: Figure 12 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for the probability of switching from regular to marginal employment, conditional on the post-switch status being marginal employment. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level. The sample excludes pensioners.

Figure 13: Effect of the 2003 Reform on Partial Pension Claiming



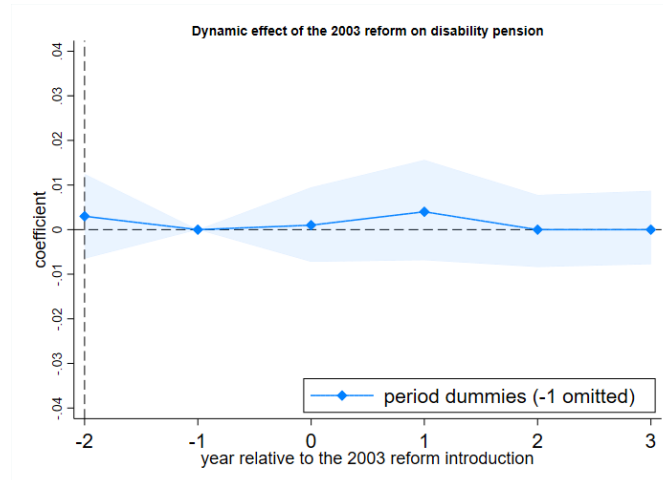
Note: Figure 13 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for partial pension claiming. Partial pension take-up was virtually zero both before and after the reform, indicating that older workers did not respond by claiming a reduced pension early while supplementing income with a Mini-job. Note that the coefficients are very small in magnitude and should be interpreted as economically negligible. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level.

Figure 14: Effect of the 2003 Reform on Full Pension Claiming



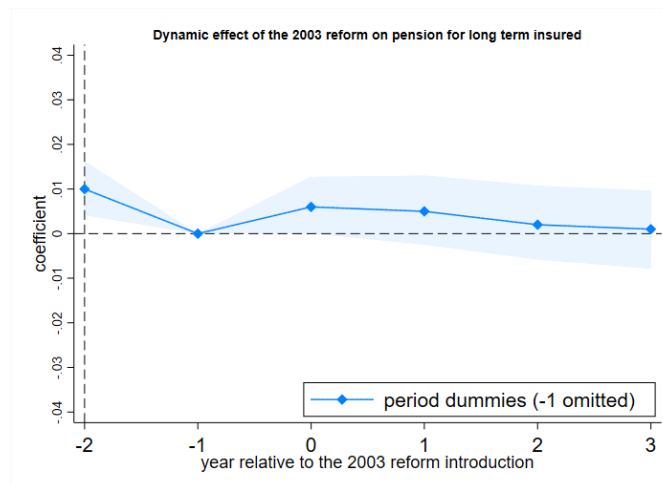
Note: Figure 14 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for full pension claiming. The reform increased full pension take-up by 5.6 to 7.3 percentage points over the three years following reform exposure, with the effect persisting beyond the observation window. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level.

Figure 15: Effect of the 2003 Reform on Disability Pension Claiming



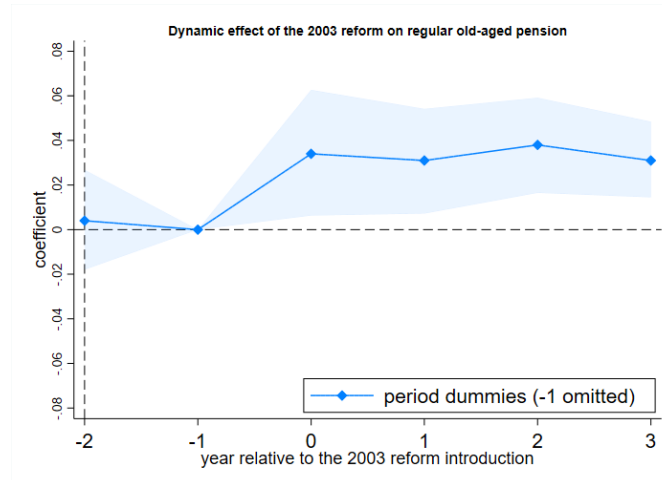
Note: Figure 15 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for disability pension claiming. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level.

Figure 16: Effect of the 2003 Reform on Long-term Insured Pension Claiming



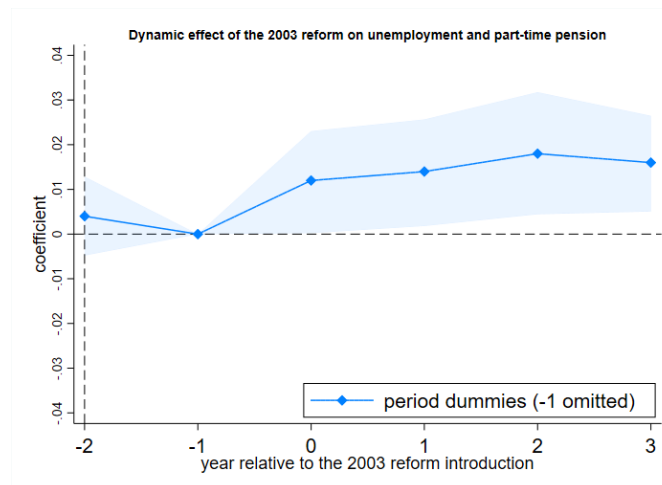
Note: Figure 16 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for long-term insured pension claiming, which requires 35 years of contributions. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level.

Figure 17: Effect of the 2003 Reform on Regular Old-age Pension Claiming



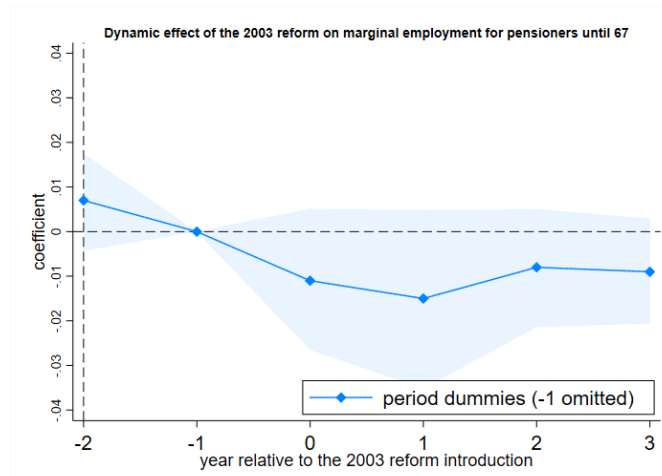
Note: Figure 17 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for the regular old-age pension pathway, which requires only 5 years of contributions and is the most widely used pathway. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level.

Figure 18: Effect of the 2003 Reform on Part-time and Unemployment Pension Claiming



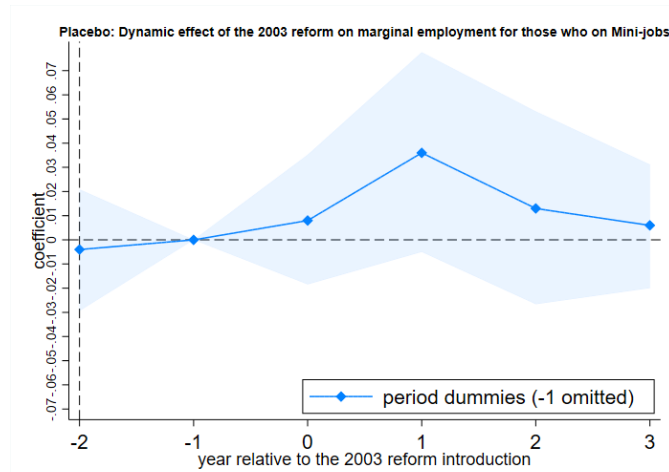
Note: Figure 18 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for the combined part-time job and unemployment pension pathway, which requires 15 years of contributions and a period of part-time or unemployment activity before retirement. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level.

Figure 19: Effect of the 2003 Reform on Marginal Employment among Pensioners



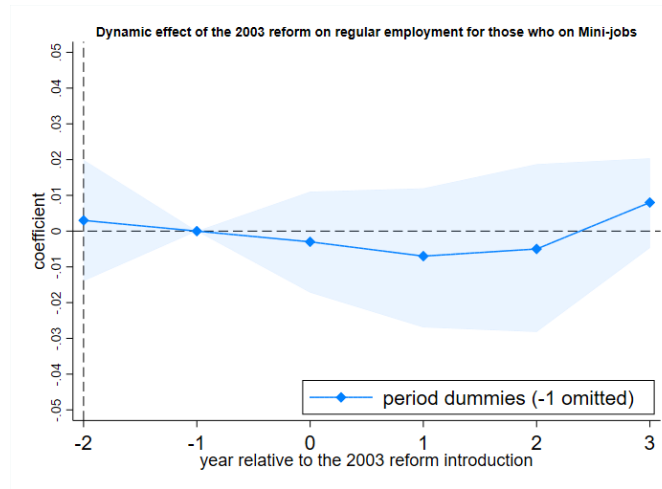
Note: Figure 19 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for marginal employment take-up among pensioners, observed up to age 67. The absence of a significant effect suggests that the reform did not meaningfully change pensioners' labor supply behavior in the first years after retirement. Two caveats apply: the data are truncated at age 67, and marginal employment without social contributions may be underreported after pension claiming. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level.

Figure 20: Placebo Test: Effect on Marginal Employment among Workers Already on Mini-jobs before the Reform



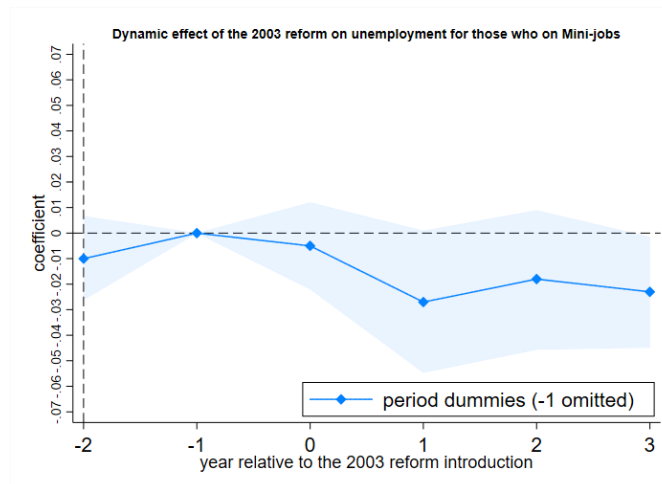
Note: Figure 20 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for marginal employment take-up among workers who were already in a Mini-job before the 2003 reform. Since these workers were already in marginal employment, they should not be meaningfully affected by the reform. The absence of significant effects validates the identification design and provides reassurance that the main results are not driven by spurious cohort trends. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level. The sample excludes pensioners.

Figure 21: Placebo Test: Effect on Regular Employment among Workers Already on Mini-jobs before the Reform



Note: Figure 21 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for regular employment among workers who were already in a Mini-job before the 2003 reform. The absence of significant effects corroborates the placebo test in Figure 20 and further supports the validity of the identification design. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level. The sample excludes pensioners.

Figure 22: Placebo Test: Effect on Unemployment among Workers Already on Mini-jobs before the Reform



Note: Figure 22 plots the dynamic cohort difference-in-differences coefficients from Equation 1 for unemployment among workers who were already in a Mini-job before the 2003 reform. Together with Figures 20 and 21, the absence of significant effects across all three placebo outcomes confirms that the cohort difference-in-differences design is not picking up spurious pre-existing trends. Period $k = -1$ is the omitted baseline. The shaded area shows 90% confidence intervals. Standard errors are clustered at the cohort level. The sample excludes pensioners.