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Opt-in or Opt-out? The Power of Defaults in Pension Enrollment Choices

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Opt-in or Opt-out? The Power of Defaults in Pension Enrollment Choices*

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Abstract

Default settings strongly increase pension enrollment, especially when savings incentives are high and choices are complex. We show that the effect is weaker when incentives are low, options are simple, and opting out is easy. We study the nationwide introduction of auto-enrollment for low-income employees in Germany's public pay-as-you-go pension system. We find that automatic enrollment raises participation by 23 percentage points, though most individuals actively opt out. Linking administrative and survey data shows that the default effect is stronger when enrollment incentives are higher and among individuals who lack knowledge of their enrollment status.

Keywords: Default-Setting, Auto-Enrollment, Pensions, Financial Literacy

JEL Codes: D14, H55, J26

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1 Introduction

Automatic enrollment in pensions has become a central policy tool for raising retirement savings. Many OECD countries–including Lithuania, New Zealand, Poland, the United Kingdom, and Turkey–have adopted automatic enrollment (OECD 2024). In the United States, 17 states automatically enroll workers in individual retirement accounts (Georgetown Center for Retirement Initiatives 2026) and new 401(k) plans must feature automatic enrollment (Internal Revenue Service 2025). These policies reflect a large literature documenting the power of defaults in this context: When enrollment is the default option, participation in pension accounts rises sharply (e.g., Madrian and Shea 2001; Choi, Laibson, Madrian, and Metrick 2004; Choi, Laibson, Cammarota, Lombardo, and Beshears 2024; Choukhmane 2025). The literature commonly studies the power of automatic enrollment based on firm-level variation in contexts where financial incentives for enrollment are high (i.e., because of employer matching) and where individuals face complex, multi-dimensional choices. However, not all pension settings share these characteristics. Little is known about the impact of enrollment by default when enrollment is a simple yes–no decision, and when enrollment has heterogeneous and mostly small financial benefits, so that individuals may not necessarily benefit from being enrolled.

This paper examines the nationwide introduction of automatic enrollment for all low-income workers in the German public pension insurance. In this context, the financial incentives for enrollment in the pay-as-you-go system are often low and workers face a binary choice–to enroll or not to enroll. Using monthly administrative data for a large representative sample and a regression discontinuity design (RDD) over time, we find that automatic enrollment has a significant and sizable effect on some workers, while the majority remain unenrolled as they opt out immediately. We then link administrative with survey data and show that financial incentives and financial knowledge are key mechanisms behind the observed enrollment behavior. The effect of automatic enrollment is smaller for workers with lower financial incentives for enrollment and larger for those with higher incentives. At the same time, automatic enrollment has a strong effect for individuals who fail to understand their own enrollment situation. This questions the suitability of automatic enrollment as a policy tool in this context, because those who are unknowingly enrolled may be better off without enrollment, for instance because they are liquidity constrained.

Enrollment in the German public pension insurance is optional for workers up to a certain income threshold–so-called *mini-job* employees. These low-income workers can choose whether or not to contribute to the federal pay-as-you-go pension system. Enrollment increases pension savings and can also increase eligibility

for public pensions for some workers. However, for most workers the savings incentives in the public system are low, because the pay-as-you-go system yields relatively low returns. Moreover, many of these low-income workers are liquidity constrained today, and means-tested social security might top up low future pension entitlements, which further reduces savings incentives. In 2013, the German government changed the default for mini-job employees and introduced automatic enrollment for new employments. Pre-reform, workers have to actively opt in if they want to be enrolled, post-reform they have to actively opt out if they do not want to be enrolled. This introduces quasi exogenous variation over time and allows us to estimate the causal effect of the default on enrollment. The contribution rate is set by national legislation and is the same for all of these employees (around 4 percent during our sample period). The employer contribution rate is independent of own contributions and did not change during the sample period. Employees only choose whether or not they want to enroll, there is no choice of the contribution amount, no funds to choose for investment, and costs for opting in or out are low.[1]

The setting we study differs from the previous literature in several key aspects. First, savings incentives in our setting are low compared to settings like 401(k), where enrollment is the beneficial choice for many. Second, our setting with fixed contribution rates, no employer matching, and no investment choice allows us to estimate the pure default effect, ruling out common confounders such as choice overload (e.g., Iyengar and Lepper 2000; Iyengar, Huberman, and Jiang 2004), switching costs (e.g., Heiss, McFadden, Winter, Wuppermann, and Zhou 2021; Choukhmane 2025), or perceiving the default as employer's investment advice (Madrian and Shea 2001), leaving inattention as the main remaining explanation for inertia. Third, the combination of a nationwide policy and high-frequency administrative data allows us to precisely estimate the overall causal effect of auto-enrollment across firms and industries on the monthly level. Importantly, we can observe individuals over time and across employers. This makes identification particularly strong compared to earlier settings and allows us to classify active and passive behavior based on the enrollment behavior of the same individual starting jobs under different regimes. Fourth, we can identify mechanisms behind the observed enrollment behavior by linking administrative data with household survey data. Lastly, we analyze the impact on automatic enrollment for a specific group of low-income workers that is often perceived as vulnerable by policymakers.

Our first main result is that, while most individuals opt out immediately from automatic enrollment, the policy still has a sizable and significant effect. Our RDD estimates show that the reform increases enrollment by 23 percentage points in the first month of employment. Compared to a baseline enrollment share of

[1]There is only a 1-page standard form that has to be filled in. Figure A.2 shows an example.

about 6 percent before the reform this is a substantial effect. In the medium run, the default effect attenuates, but it remains economically and statistically significant: After 12 months in a job, the enrollment share is about 16 percentage points higher under automatic enrollment. The attenuation over time is mostly driven by selection–those with longer tenure are more likely to enroll already pre-reform–rather than by delayed opt-outs. We further distinguish active and passive behavior with respect to the default switch: 17 percent of workers observed under both the opt-in and the opt-out regime are passive, i.e., always stick to the default independent of the default regime. 63 percent show persistent active behavior and never enroll, irrespective of the default.

We then investigate mechanisms behind the observed individual behavior. First, we study the role of financial incentives. Intensive-margin incentives to contribute to the pay-as-you-go system are weak and depend on the worker's longevity after retirement, as well as their discounting of future utility. Moreover, a minimum absolute contribution amount is binding for very low income earners, and increases their enrollment costs without increasing benefits. We explore this variation in incentives and show that both the effect of automatic enrollment and baseline enrollment pre-reform are substantially lower for this group with very low incentives. While incentives at the intensive margin are low, extensive-margin incentives can be high, especially near eligibility thresholds. Pension eligibility depends on accumulated contribution periods (for example, a minimum of 35 years is required for early retirement) and periods count more if workers are enrolled. We find that individuals who are close to these thresholds, and thus have, on average, stronger incentives for enrollment, are more likely to enroll at baseline. In addition, automatic enrollment has a stronger effect for them. Workers who have a mini-job as a secondary job, on top of a main employment, do not face any eligibility incentives from enrollment, as they are already enrolled in their main employment. We find that the effect of automatic enrollment for this group is smaller, compared to those for whom the mini-job is the main employment. Taken together, these results indicate that incentives matter for the effect of automatic enrollment: The effect is lower for those who have lower incentives for enrollment. This mechanism is in line with the high opt-out rates observed in our context with overall low incentives to enroll.

As a second mechanism, we analyze the role of individuals' knowledge of their own enrollment situation. We first provide suggestive evidence from administrative data. Heterogeneous treatment effects show that the default effect is less strong for those who have more experience with mini-jobs and are thus expected to have more institutional knowledge. We then provide direct evidence from linked survey data. In the survey, workers are asked whether they are enrolled. We compare individuals' self-assessed enrollment status with the true enrollment that we ob-

serve in the linked administrative data. Overall, most workers know their own enrollment status, but about one fourth does not–they are enrolled but think they are not, or vice versa. For those individuals with a lack of enrollment knowledge, the effect of the default is almost twice as large compared to those who know their own enrollment. Overall, the binary choice environment arguably implies lower complexity than in multidimensional choice settings like 401(k). But some still do not understand their enrollment situation, and for them the default is more powerful.[2]

Finally, we use the linked survey data to characterize those who stick to the default under automatic enrollment. Mini-job workers in our sample live in low-income households, with an average monthly net household income of 679 EUR. Liquidity constraints are common for these workers. 51 percent are liquidity constrained, suggesting that saving in an illiquid pension with low returns may not be optimal for them. We find that these liquidity constrained workers are not significantly more likely to opt out from automatic enrollment. As a result, 49 percent of those who are enrolled post-reform are liquidity constrained.

This paper contributes to the literature on the impact of default settings on retirement saving that started with Madrian and Shea (2001). A common finding is that under automatic enrollment, most employees contribute and if they contribute, they typically stick to the default contribution rate and invest in the default funds. For an overview of this literature, see for example Benartzi and Thaler (2007) and Beshears, Choi, Laibson, and Madrian (2009, 2024). A large part of that literature studies 401(k) pension schemes in the US, but strong effects of automatic contributions are also found in other countries, e.g., Afghanistan (Blumenstock, Callen, and Ghani 2018), Australia (Butt, Donald, Foster, Thorp, and Warren 2018), Denmark (Chetty, Friedman, Leth-Petersen, Nielsen, and Olsen 2014), and the UK (Cribb and Emmerson 2021; Beshears et al. 2024).

A more recent strand of the literature confirms sharp increases in enrollment but questions the effect on total lifetime savings because of opt-outs in the medium run for the treated (Chalmers, Mitchell, Reuter, and Zhong 2025), as well as opt-ins for the control group after some time (Choukhmane 2025), or because workers withdraw savings from their employer sponsored accounts when they leave firms (Choi, Laibson, Cammarota, Lombardo, and Beshears 2024; Derby, Mackie, and Mortenson 2023). A related strand of the literature studies the overall impact on net savings, finding somewhat mixed results, depending on the context (Chetty, Friedman, Leth-Petersen, Nielsen, and Olsen 2014; Beshears, Choi, Laibson, Madrian, and Skimmyhorn 2022; Beshears et al. 2024; Choukhmane and Palmer 2025).

[2]Our results are consistent with the broader finding that auto-enrollment has stronger effects among individuals with lower financial literacy (Goda, Levy, Manchester, Sojourner, and Tasoff 2020).

Our paper contributes to this literature in several ways. First, we show that how many individuals enroll under automatic enrollment strongly depends on the setting. In our setting with low baseline enrollment of around 6 percent, an effect of 23 percentage points results in enrollment rates around 30 percent. In other settings with higher baseline enrollment, the same absolute effect size can lead to very high enrollment rates as commonly observed in settings like 401(k). Our second contribution is the identification of mechanisms behind the observed behavior. The default effect is stronger for those who do not know their enrollment status and weaker for those with lower financial incentives. This is in line with the overall high opt out rates in our setting as well as with high opt-out rates in US state-sponsored programs without employer matching, and thus lower incentives, such as *OregonSaves* (Chalmers, Mitchell, Reuter, and Zhong 2022, 2025) and *CalSavers* (Center for Retirement Research 2025). Third, we show that automatic enrollment has a significant effect in a setting where enrollment is often not beneficial and can nudge "the wrong" individuals into enrollment. Specifically, we find that those who contribute under automatic enrollment are often liquidity constrained, and may thus often be better off without enrollment. Here, our findings relate to Bourquin, Cribb, and Emmerson (2020), who show that automatic enrollment increases enrollment most among workers who are least financially stable. These are also tangible implications for policymakers who increasingly rely on automatic enrollment in different pension settings.

The remaining part of the paper is structured as follows. We explain the institutional background in section 2 and introduce the datasets and samples we use in section 3. Section 4 provides results for the effect of the default on enrollment. In section 5 we analyze mechanisms behind the observed individual behavior. We conclude with section 6.

2 Institutional Setting

2.1 Default-Changing Reform

In 2013, a reform introduced automatic enrollment in the public pension insurance for mini-job employees. Before the reform, employees had to actively opt in to enroll. Ever since the reform, they have to actively opt out to not enroll. The reform left the enrollment choices and incentives unchanged, both for employees and for employers (we provide details in the sections below). This allows us to identify the causal effect of the default on enrollment. The reform came into effect on January 1, 2013, after the respective law passed the German parliament on December 5, 2012. The new default applied to all mini-jobs that started in 2013 or later. Employees whose mini-job started pre-reform remain under the old opt-in

regime post reform, unless their monthly income surpasses the threshold of 400 €. Throughout our analysis, we focus on new mini-jobs. The reform also increased the monthly income threshold for mini-jobs from 400 € to 450 €.

2.2 German Public Pension Insurance

The German public pension insurance is an earnings related pay-as-you-go system (PAYG) with compulsory enrollment for most employees.[3] Both employees and employers make contributions with contribution rates being defined by law on the national level. Contributions are translated into pension points that individuals accumulate over their working life. In any year, individuals will obtain 1 pension point when earning exactly the average annual national income, 0.5 (1.5) pension points when earning 50 percent (150 percent), and so on. Contributions are capped above a certain income threshold. To a smaller extent, pension points can also be acquired during other periods such as parental leave or unemployment.

Pension payments are determined by multiplying the accumulated pension points with the current pension point value (PPV), which depends on the average labor market income in a given year, the contribution rate for the working population, and a sustainability factor accounting for societal developments, e.g. demographic changes. It is adjusted on a yearly basis and has been steadily increasing over time (see Table A.3 for an overview of the relevant operands). Pensioners with low (pension) income are often also eligible for social security, which is means tested on the household level.[4] In order to become eligible for a regular pension at the statutory retirement age, a minimum qualifying period of five years is required. Eligibility for special pensions is tied to other minimum qualifying periods. For instance, to become eligible for early retirement, 35 years of contributions are required. Table A.4 provides an overview of the different thresholds.

2.3 Mini-Jobs

We study employees in so called "mini-jobs", employments that are characterized by a very low monthly gross income. During our sample period, the income threshold is 400 € per month for the years before 2013 and 450 € per month for the remaining years. Mini-jobs are exempt from income taxes and from most social security contributions, including contributions to the public health insurance. Mini-jobs can serve as either a primary source of employment or as a supplementary side-job alongside regular employment. This paper focuses on the former

[3] Civil servants and most self-employed are excluded.

[4] Since 2022 pension entitlements are topped up for individuals with long employment histories (33 to 35 years of contribution) and low lifetime income, but this was not yet implemented for our study period.

group, which includes around 5 million workers every month (Bundesagentur für Arbeit 2025).

2.4 Pension Enrollment for Mini-Jobs

Binary Enrollment Choice For mini-job employees, enrollment in the pension insurance is optional. They face a purely binary choice menu: enrollment at contribution rate τ_t^{ee}, or no enrollment. Irrespective of the employee's choice, employers always contribute for them at contribution rate τ_t^{er}. The contribution rates are determined on the national level and change slightly over time (Table A.3 provides an overview of the contribution rates during our sample period). In 2013, $\tau_t^{ee} = 0.039$ and $\tau_t^{er} = 0.15$. If a worker enrolls, the total contribution rate is $\tau_t^{er} + \tau_t^{ee} = 0.189$ as compared to $\tau_t^{er} = 0.15$ if not enrolled.[5] Since employers always contribute the mandated τ_t^{er}, they face no incentives to encourage or discourage their employees' enrollment. Since they pay contributions to the pension insurance anyways, we argue that additional administrative costs are negligible for them.

Costs of Enrollment Enrollment for individual i in period t comes at the monetary cost of $\tau_t^{ee} \times y_{it}$. For instance, a worker with monthly income $y_{it} = 450$€ faces monthly costs of 450€×0.039 =17.50€ when contributing and 0€ else (2013 values). There is a minimum assessment base y_t^{min} that ensures a minimum absolute contribution if enrolled. No matter how low the monthly income, the absolute contribution for enrolled individuals can never fall below $(\tau_t^{er} + \tau_t^{ee}) \times y_t^{min}$. For monthly income $y_{it} < y_t^{min}$, enrolled employees thus face higher costs because they have to top up their contribution.

Low Switching Costs In addition to monetary costs, there could be non-monetary costs for deviating from the default, i.e. time and hassle costs for filling in the required form. However, in our setting, switching costs are arguably low: To deviate from the default, individuals have to fill in a one-page form that requires personal information like name and address, as well as their signature. The complexity of this form is low and remained unchanged pre and post reform (see Figure A.1 and A.2). Given these comparably low requirements for deviating from the default, we argue that switching costs are unlikely to be a key driver for inertia in this setting. Switching costs for employers are also negligible. If individuals deviate from the default, employers only have to sign the respective form, which is unlikely to impose relevant costs on them.

[5] For mini-jobs, τ_t^{er} is higher than for regular employment, where employer and employee each contribute at the same contribution rate, e.g. 0.0945 in 2013 (see Table A.3). If employees are enrolled, the total contribution rate (employer + employee) is the same as under regular employment.

Saving Incentives Under enrollment, individuals save more for retirement since they acquire more pension points:

$$PP_{it} = \begin{cases} \frac{y_{it}}{Y_t} & \text{if enrolled} \\ \frac{y_{it}}{Y_t} \frac{\tau_t^{er}}{\tau_t^{ee}+\tau_t^{er}} & \text{if not enrolled,} \end{cases}$$

where y_{it} denotes the individual gross income for the employment period and Y_t denotes the average annual income for the respective year as defined by the pension insurance (see Table A.3 for details). Enrollment increases pension points in period t by $\Delta_{it} = \frac{y_{it}}{Y_t} \frac{\tau_t^{ee}}{\tau_t^{er}+\tau_t^{ee}}$, which is equivalent to an increase of 26 percent in 2013. In monetary terms, these are relatively low amounts. With the pension point value $PPV_{2013} = 28.14$€, an individual working for 12 months in 2013 with a monthly income of 450€ will increase their monthly pension entitlements by 0.93€ if enrolled (using 2013 values for pension points). Considering only these savings incentives from pension points, the net present value (NPV) of benefits from enrollment is defined as

$$NPV_{it} = -\tau_t^{ee} y_{it} + \sum_{a=a^r}^{a^{max}} \pi_{ia} \frac{1}{(1+\delta)^{a-a_{it}}} \frac{y_{it}}{Y_t} \frac{\tau_t^{ee}}{\tau_t^{er}+\tau_t^{ee}} PPV_t, \tag{1}$$

where π_{ia} denotes the probability that i is alive, and thus receives pension payments at age a. We denote by a^{max} the maximum life expectancy. Whether NPV_{it} is positive depends on the discount factor, the individual's age when making the enrollment decision, and the expected length of their retirement period.

Figure A.3 plots NPV_{it} as a function of a^{max} and δ for $a^r = 65$ and $\pi_{ia} = 1$–for different values of a_{it}. For a^{max} below 84, NPV_{it} is always negative because the German pension insurance is actuarially fair when claiming pensions for around 19 years and we set $a^r = 65$ here. For higher life expectancy, NPV_{it} can be positive or negative, depending on a_{it} and δ. Intuitively, younger individuals require a smaller discount factor and/or higher life expectancy to have a positive NPV of enrollment. The average individual in our sample is female and 40 years old when they start a mini-job (corresponds to Figure A.3b). Together with life expectancy values for our sample period, this implies that the NPV for the average individual is positive only if δ is close to zero.[6] In any case, the average monetary incentives in our setting are small. Importantly, they are smaller than in other pension settings with funded pensions, like 401(k), where individuals typically leave money on the table when not enrolling because of employer subsidies and expected capital market returns.

[6]The period life table from the Federal statistical office shows that the the life expectancy at age 65 during our sample period is 86 for women and 83 for men: `https://www-genesis.destatis.de/datenbank/online/url/fb901b3c` (last accessed: 2025-12-17).

Eligibility Incentives While pension points determine the pension level, the pension eligibility depends on the individual insurance record, i.e., how many pension qualifying periods individuals have accumulated. The number of months credited for a mini-job period depends on the enrollment status. When enrolled, the months are fully credited, i.e. one month of mini-job employment is equivalent to one additional month for the insurance record. Without enrollment, the credited time depends on the income and is determined by $\frac{PP_{it}}{0.0313}$. For example, for $y_{it} = 450$€ in 2013, one month without enrollment will increase the pension record by only 0.34 months. This increases the incentives for enrollment at the extensive margin, especially for individuals (closely) below a relevant qualifying period threshold. An extreme example would be an employee who was enrolled for 4 years and 11 months at some point in their life. Since their insurance record is less than 5 years, they are not eligible for any pension payments. However, by enrolling in a mini-job for only one more month, they will become eligible for monthly pension payment for their entire retirement period.

3 Datasets and Samples

We use administrative data from the German pension insurance (VSKT). To study mechanisms behind the observed individual behavior, we additionally use survey data with record linkage to the administrative data (SOEP-RV). We describe the two datasets below.

3.1 Administrative Data

Dataset Our main analysis is based on the VSKT data from the German pension insurance (Deutsche Rentenversicherung Bund 2017). The VSKT is a monthly panel dataset covering the entire employment biography of a 2% random subsample of the universe of insurees born between 1949 and 2001. The data is based on employer spells and contains information on the individual labor market status (e.g., regular employment, mini-job employment, unemployment, sickness leave) in a given month and wage income for employment, as well as a set of basic demographic characteristics (birth, gender, citizenship, and state of residence) and statistical weights for the day of sampling, December 31, 2016. The spells report the start and end date of a spell within a given year. If the spell started before a given calendar year, the reported starting date is January 1—similarly, the reported end date is December 31 if the spell continued in the following year.[7]

[7]The data is based on annual employer spells, but do not include an employer identifier, which makes it impossible to track employment spells over more than one calendar year.

We observe mini-job workers and their enrollment status if the mini-job is their main employment.

Sample We restrict the sample to individuals for whom a mini-job is the main employment for at least one month between February 2011 and November 2016. We then impose a set of sample restrictions to focus on the core working-age population. We restrict the sample to individuals aged 25 to 65, which largely excludes students. We drop all observations for an individual from (and including) the first month they receive any pension payment onward, so individuals are only observed up to the point they become pensioners. We also exclude workers who are ever employed in workplaces or employment arrangements reserved for people with disabilities.

We impose two further restrictions to address potential measurement error in the data. First, we drop workers who ever have a mini-job with average monthly income exceeding the income threshold for mini-jobs.[8] Second, we restrict the sample to observations for which we can unambiguously observe the starting date of the mini-job employment, which is decisive for determining the applied default regime. Since the pension data stems from annual employer spells, the recorded starting date for a mini-job never dates before January 1 of that year. Consequently, when observing a non-stop mini-job employment period that comprises the turn of a year, it is impossible to tell whether the individual remained in the same employment or whether they started a new mini-job on January 1. For our main analysis, we focus on the first month of mini-job employments, for which we can unambiguously identify the starting date, and thus the default.[9] We use this sample for our main analysis in section 4.

Column A of Table 1 provides sample characteristics for the final sample in the first month of employment, denoted by $m = 1$. Our final sample comprises 92,806 individuals for whom we observe a total of 184,696 mini-jobs ($m = 1$), representing 6.3 million workers and 12.7 million jobs when applying statistical weights. We report the sample characteristics for $m = 3$, 6, and 12 in Table A.1. The number of observations decreases significantly with increasing m, because of the short average duration of mini-job employments.

[8] The data does not contain information on the exact monthly income but the total income for a reported employment spell. Monthly mini-job income is allowed to exceed the income threshold up to 3 times per year if the annual mini-job income does not exceed 12 $\times \bar{y}$. Since the data does not allow for disentangling these cases from reporting errors, we exclude those workers.

[9] A mini-job employment is considered to have its start in month t if the recorded starting date lies within that month but is not January 1. Mini-jobs with a recorded starting date of January 1 are only considered to have started in January if it is the first recorded mini-job employment for the individual or if their last mini-job employment ended before December 31 of the previous year.

Table 1: Sample Characteristics VSKT and SOEP-RV Samples

	A. VSKT	*B. SOEP-RV*	
		B1. Full sample	*B2. 2011–2016*
Sample size			
N individuals	92,806	5,218	602
N new mini-jobs ($m = 1$)	184,696	11,998	3,411
Job characteristics administrative data			
Labor market status before mini-job			
regular employment	0.11	0.11	0.10
mini-job employment	0.33	0.00	0.00
unemployment	0.19	0.30	0.30
other	0.05	0.13	0.11
empty spell	0.32	0.48	0.51
High benefit	0.04		
High cost	0.36		
Demographics administrative data			
Age	40	34	34
Female	0.66	0.66	0.64
German citizen	0.75	0.94	0.93
West German state	0.85	0.79	0.81
Demographics survey data			
Married	-	0.45	0.45
Immigrant background	-	0.21	0.25
Less than high school	-	0.16	0.17
High school	-	0.61	0.57
More than high school	-	0.23	0.26
Net household income (2020 EUR)	-	679	621

Notes: Data from VSKT (column A) and SOEP-RV (columns B1 and B2). B1 is the full sample from the SOEP-RV data, B2 restricts the sample to the years 2011 to 2016 – the years covered by the VSKT data in column A. Reported values are mean values for $m = 1$. Our unit of observation is individuals i in the first month of a mini-job employment ($m = 1$). Labor market status is measured the month before i starts their mini-job. *Other* contains any other status that can be recorded in the administrative data, including education, care, or military service. Spells are empty when no status is recorded, i.e., if the individual is not part of the labor force in that month. Note that the share for mini-job employment in panel B is zero because the SOEP-RV data does not contain exact start and end dates for employment spells, which is why we can only define new mini-jobs for i who did not have a mini-job in $t - 1$. *High benefit* refers to being close (up to 12 months) to an eligibility threshold. *High cost* refers to individuals for whom the contribution rate is higher because their income is below the minimum assessment base y_t^{min}. *Net household income* refers to last month before answering the survey. Note that Marital status, immigrant background, education, and household income are missing in column A as they are only observed in the survey data.

3.2 Linked Survey Data

Dataset The SOEP-RV data links the largest German household survey (SOEP) with administrative data from the VSKT dataset (Goebel et al. 2022). Record linkage between the two datasets is available for SOEP respondents who agreed to the linkage in 2018 or 2020.[10] The linked sample has all information available from the SOEP data, including a broad range of variables, both on the individual and on the household level.

The administrative data that can be linked to the SOEP has a somewhat different structure than the regular VSKT data described in subsection 3.1, but includes all necessary information required for our analysis (see Research Data Centre of the German Pension Insurance (FDZ-RV) (2022) for details). Most importantly, it covers the enrollment status for mini-job employees. Furthermore, it allows observing several labor market status simultaneously, but we focus on individuals whose main employment is a mini-job – similar to the definition for our main dataset. While the VSKT is a monthly panel, the SOEP is an annual panel. When using the SOEP-RV data we thus aggregate the administrative data and plot annual data.

Sample The total sample size of the linked SOEP-RV dataset amounts to about 12,000 individuals. Out of this full sample, we draw a subsample of 5,218 individuals who have at least one mini-job. We report the characteristics for this sample in column B1 of Table 1. For better comparison with the main VSKT sample, column B2 restricts the sample to the same periods we have for the VSKT data in column A – 2011 to 2016.[11] When studying mechanisms in section 5, we rely on subsamples of the SOEP-RV data, depending on the specific variables we use. Many questions in the SOEP survey are only asked in specific years, limiting the respective sample sizes. We report the descriptive statistics for the different SOEP-RV subsamples in Table A.2.

4 The Effect of the Default

4.1 Empirical Strategy

We use a regression-discontinuity (RD) approach to determine the causal effect of the default on the enrollment status, with the monthly starting date t of the

[10] For a detailed description of the SOEP-RV dataset and the linkage process, see Lüthen et al. (2021).

[11] We study a longer period than with the VSKT data, because the structure of the SOEP-RV dataset allows for identifying mini-jobs for a longer period.

employment being the running variable. The enrollment default is a deterministic function of t with a discontinuity at the turn of the year 2012/2013. All mini-jobs that start before 2013 are under the opt-in regime (no automatic enrollment) and all mini-jobs that start in 2013 or later are under the opt-out regime (automatic enrollment). We define D_t as a dummy variable for the default with

$$D_t = \begin{cases} 0 \text{ if } t < 01/2013 \text{ (pre reform)} \\ 1 \text{ if } t \geq 01/2013 \text{ (post reform).} \end{cases}$$

We estimate the effect of the default for individual i starting their mini-job in month t on their enrollment status in the m^{th} month of tenure in the employment that started in t. Thus, $m = 1$ refers to the first month of employment (t), $m = 3$ to the third month ($t + 2$), and so on. We denote the individual enrollment status as E_{it}^m with $E_{it}^m = 1$ if enrolled and 0 else. We estimate the impact of the default on enrollment at four different points in time, months $m = 1$, 3, 6 and 12:

$$E_{it}^m = \alpha + \gamma\, t + \beta D_t + \eta_{it} \tag{2}$$

We refer to the effect of the default on enrollment in the first month of employment ($m = 1$) as *immediate effect* and to the effect on enrollment at later points in time ($m = 3, 6, 12$) as *medium-run effects*. The coefficient of interest, β, measures the estimated effect of automatic enrollment.

We then include a set of individual characteristics J of individual i in month t captured by the vector X_{Jit} and allow for different trends over time pre and post reform, for $t < 01/2013$ and $t \geq 01/2013$, respectively. The set of characteristics is described in detail in subsection 4.3. We use ordinary least squares (OLS) to estimate the full model:

$$E_{it}^m = \alpha + \gamma_{pre}\, t\, \mathbb{1}(t < 01/2013) + \gamma_{post}\, t\, \mathbb{1}(t \geq 01/2013) + \beta D_t + \sum_J \delta_J X_{Jit} + \eta_{it} \tag{3}$$

Identifying Assumptions The main identifying assumption is that assignment in the neighborhood of the cutoff (January 1, 2013) is as good as random, such that any discontinuity in the outcome at the threshold can be attributed to a discontinuity in the treatment variable. Put differently, we require that, absent the change in default, there would be no discontinuity in the enrollment share.

One concern in RD designs is that covariates other than the running variable may be discontinuous at the cutoff (Imbens and Lemieux 2008). To address this concern, we show monthly mean values for a set of covariates in Figure A.5, as well as the mean estimated error term $\hat{\eta}_{it}$ from Equation 3. The figures provide no evidence for discontinuities.

A second concern for RD designs is individuals' ability to manipulate the running variable, leading to non-random assignment around the cut-off (Imbens and Lemieux 2008). In our setting, the running variable is the starting date of the mini-job employment. Clearly, employees as well as employers have leeway over the starting date of an employment contract. If there was manipulation around the reform, we would expect a discontinuity in the density of contracts around the cutoff. Figure A.4 shows that, while there are clear seasonal patterns, the number of new mini-jobs in the months in 2012 and 2013 are comparable to the years before and after. This is in line with there being no incentive for manipulation, neither for employees nor for employers. While the default changes at the cutoff, costs and benefits of being enrolled remain unchanged and there are no changes in the enrollment incentives.

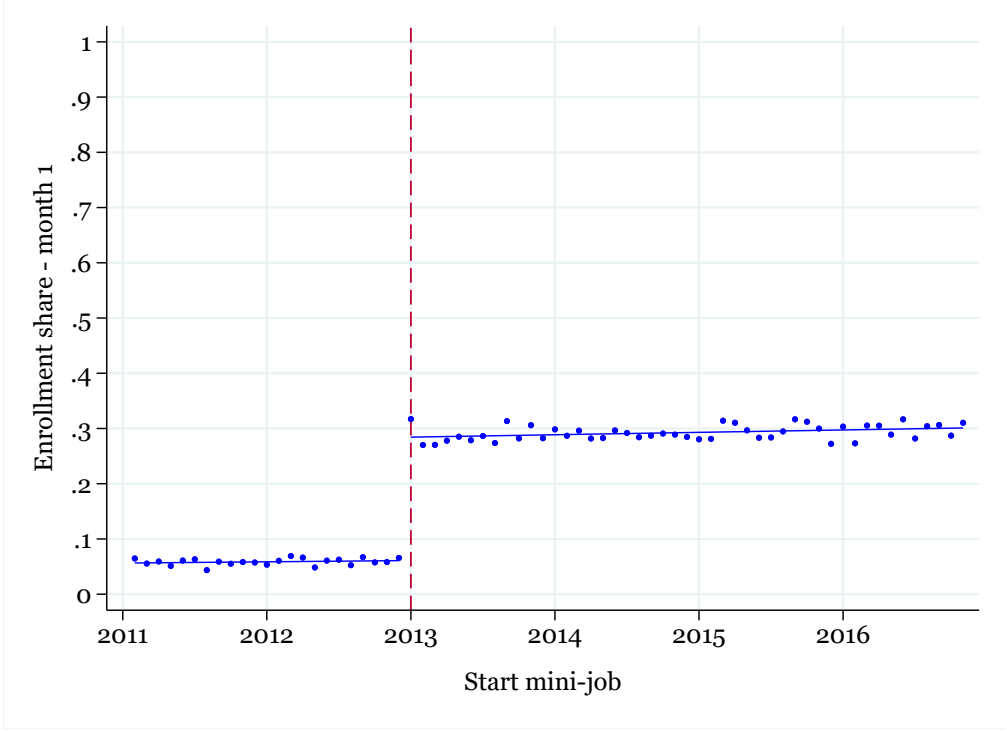
4.2 Overall Effect on Enrollment

Immediate Effect Figure 1 provides graphical evidence that changing the default to automatic enrollment has a positive effect on individual enrollment. The graph plots the unconditional enrollment share in the first month of a new mini-job ($m = 1$). Under the opt-in regime pre reform, the mean enrollment share is 6 percent, averaged over all starting months of employment. After the introduction of automatic enrollment in January 2013, the enrollment share jumps immediately and stabilizes at that higher level for all post-reform months with a slight increase over time and an average of 29 percent.

While Figure 1 shows a clear increase in enrollment post reform, it also shows that the majority of mini-job employees are not affected by the default. Both pre and post reform, most individuals are not enrolled. Put differently, under automatic enrollment, about 70 percent of mini-job employees opt out immediately. Table 2 reports the results from estimating the models specified in Equation 2 and 3 and confirms the graphical evidence from Figure 1. Automatic enrollment significantly increases the enrollment share by 22-23 percentage points in the first month of employment. Our preferred specification is column (4), which includes individual controls and allows for different linear time trends before and after the reform, but both magnitude and significance remain virtually unchanged over different specifications.

Medium-Term Effect To better understand the impact of automatic enrollment on public pension entitlements, we widen the time horizon and analyze the medium-term effect of automatic enrollment. We do so by tracking individuals over three different points during the first year of employment, in the third, sixth and twelfth month ($m = 3, 6, 12$). Intuitively, by increasing m we decrease the

Figure 1: Enrollment Share New Mini-Jobs



Notes: Weighted VSKT data. Each dot represents the average enrollment share in the first month of the mini-job for individuals who started their mini-job in a given month t. The line plots the linear best fit (OLS) from estimating Equation 3 without controls. Corresponding estimation results are shown in column (3) of Table 2. The corresponding sample size is shown in Figure A.4.

sample size, as employment contracts can end before 3, 6 or 12 months. Furthermore, assuming that at least some individuals are partly inert and take some time to deviate from the default and to actively opt in (under the old default) or opt out (under automatic enrollment), we expect β to decrease as m increases.

Widening the time horizon comes at the cost of loosing precise information. As soon as we track individuals for $m > 1$ months, the employment history will include a turn of the year for at least some individuals. For $m = 3$ for instance, every individual who started their job in November or December is now observed in the next year (January and February, respectively). Because of the data structure, we cannot distinguish between individuals who remained in the same job over the turn of the year and those who started a new mini-job at the beginning of the new year. If the turn of a year is not 2012/2013, we know the default but there remains uncertainty about the precise length of their current employment. For individuals whose employment period includes the turn of the year 2012/2013, we

Table 2: Immediate Effect of Automatic Enrollment

	(1)	(2)	(3)	(4)
	Equation 2			Equation 3
D_t	0.2220***	0.2219***	0.2234***	0.2261***
	(0.0047)	(0.0046)	(0.0046)	(0.0045)
t	0.0003**	0.0006***		
	(0.0001)	(0.0001)		
t_{pre}			0.0002	0.0001
			(0.0002)	(0.0002)
t_{post}			0.0004**	0.0006***
			(0.0001)	(0.0001)
Constant	0.0628***	0.1151***	0.0609***	0.1098***
	(0.0021)	(0.0073)	(0.0031)	(0.0078)
X_{Jit}	No	Yes	No	Yes
R^2	0.0721	0.1176	0.0721	0.1176
N $i \times t$	184,696	182,245	184,696	182,245
N i	92,806	91,215	92,806	91,215

Notes: Effect of automatic enrollment on enrollment E_{it} of individual i in the first month of their mini-job in month of observation t. Coefficients from the OLS estimation of Equation 2 and 3, robust standard errors in parentheses. t: month of observation, 1/2011 = 1. t_{pre}: pre-reform months. t_{post}: post-reform months. D_t: Dummy for the default, $D_t = 1$ under auto-enrollment and 0 else. X_{Jit}: vector of individual control variables. Controls include age, gender, citizenship, east/west, being close to an eligibility threshold, facing higher enrollment costs, and experience with mini-job employment in the past. N $i \times t$ is the number of new mini-job observations, while N i is the number of individuals. Weighted VSKT data. Significance level: *** 0.001; ** 0.01; * 0.05; ^{x} 0.1.

can no longer determine the default. We exclude this group when estimating the medium-term effect of automatic enrollment, since we cannot observe their default. We deviate from the standard RD design here, because the discontinuity does no longer occur between adjacent months.

We plot the enrollment shares for $m = 3$, 6, and 12 in Figure A.6 and Table 3 provides the results from the estimation of Equation 3. Automatic enrollment significantly increases enrollment shares in the medium run, but compared to the first month, the effect decreases over time, to about 16 percentage points after 12 months. Again, including individual characteristics has little effect on the coefficient.

The medium-run effect is smaller than the immediate effect, both because the post-reform enrollment share decreases with m and because the pre-reform enrollment share increases with m. To some extent, this is because of inertia: Some individuals take some time to deviate from the default. A second explanation is attrition. Individuals who remain longer in the sample because they have longer

Table 3: Medium-Term Effects of Automatic Enrollment

	m = 3		m = 6		m = 12	
D_t	0.1842***	0.1863***	0.1715***	0.1712***	0.1713***	0.1562***
	(0.0063)	(0.0061)	(0.0098)	(0.0096)	(0.0262)	(0.0260)
t_{pre}	0.0002	0.0000	0.0003	0.0002	-0.0003	0.0003
	(0.0003)	(0.0003)	(0.0005)	(0.0005)	(0.0014)	(0.0014)
t_{post}	0.0006***	0.0007***	0.0008***	0.0010***	0.0009*	0.0012**
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0004)	(0.0004)
Constant	0.0650***	0.1099***	0.0775***	0.1319***	0.0881***	0.1773***
	(0.0047)	(0.0098)	(0.0082)	(0.0139)	(0.0250)	(0.0308)
X_{Jit}	No	Yes	No	Yes	No	Yes
R^2	0.0554	0.1285	0.0484	0.1322	0.0360	0.1339
N $i \times t$	113,555	112,209	68,082	67,427	31,442	31,139
N i	71,095	70,098	50,155	49,607	27,548	27,273

Notes: Effect of automatic enrollment on enrollment E_{it}^m of individual i in the m^{th} month of their mini-job in month t. Only observations with certainty about the default. Coefficients from the OLS regression specified in Equation 3 with (right columns) and without individual characteristics (left columns). See notes in Table 2 for more details. Robust standard errors in parentheses. Weighted VSKT data. Significance level: *** 0.001; ** 0.01; * 0.05; ^{x} 0.1.

employment periods in mini-jobs are more likely to make an active choice and deviate from the default, both pre and post reform. With increasing m, these longer-term employees account for a larger share of the sample.

4.3 Heterogeneous Effects

Understanding the interplay between automatic enrollment and individual characteristics is of great relevance for policymakers, since it allows for understanding the impact of the default for different groups of the population. To account for potential heterogeneous effects of automatic enrollment, we interact the dummy D_t with individual characteristics X_{Jit}–the same characteristics included as control variables in Equation 3. X_{Jit} contains demographic characteristics observable in the administrative data (age, gender,[12] citizenship, region), as well as mini-job related characteristics: the number of months i has worked in a mini-job before, an indicator for whether i faces higher costs because $y_{it} < y_t^{min}$, and an indicator for whether i faces higher benefits because they are close (1 to 12 months) to an

[12]Following the gender records in the administrative data, we can only differentiate gender along the binary distinction of female and male.

Table 4: Heterogeneous Effects of Automatic Enrollment

	(1)	(2)	(3)
D_t	0.2290***	0.2510***	0.3042***
	(0.0142)	(0.0143)	(0.0141)
t_{pre}	0.0003	0.0002	0.0001
	(0.0002)	(0.0002)	(0.0002)
t_{post}	0.0006***	0.0006***	0.0006***
	(0.0001)	(0.0001)	(0.0001)
Female	0.0475***	0.0366***	0.0366***
	(0.0026)	(0.0026)	(0.0025)
Female $\times D_t$	-0.0064	0.0105*	0.0043
	(0.0044)	(0.0045)	(0.0044)
Non-German	-0.0397***	-0.0351***	-0.0352***
	(0.0020)	(0.0020)	(0.0020)
Non-German $\times D_t$	-0.0003	-0.0085*	-0.0067
	(0.0036)	(0.0037)	(0.0036)
East	-0.0180***	-0.0098**	-0.0017
	(0.0037)	(0.0038)	(0.0038)
East $\times D_t$	-0.0111	-0.0236***	-0.0071
	(0.0063)	(0.0064)	(0.0062)
Experience		0.0006***	0.0006***
		(0.0001)	(0.0001)
Experience $\times D_t$		-0.0010***	-0.0011***
		(0.0001)	(0.0001)
Higher Cost			-0.0548***
			(0.0027)
Higher Cost $\times D_t$			-0.1445***
			(0.0042)
Higher Benefit			0.0508***
			(0.0095)
Higher Benefit $\times D_t$			0.0880***
			(0.0130)
Constant	0.0568***	0.0421***	0.0575***
	(0.0095)	(0.0094)	(0.0094)
Age	Yes	Yes	Yes
R^2	0.0847	0.0861	0.1268
N $i \times t$	182,245	182,245	182,245
N i	91,215	91,215	91,215

Notes: Effect of automatic on the enrollment of individual i in the first month of their mini-job in month of observation t. Coefficients from the regression specified in Equation 4, robust standard errors in parentheses. See notes in Table 2 for more details. Effects over the age distribution are shown graphically in Figure A.7 Weighted VSKT data. Significance level: *** 0.001; ** 0.01; * 0.05; ^{x} 0.1.

eligibility threshold. Adding the interaction terms $D_t \times X_{Jit}$ to Equation 3 yields:

$$E_{it}^m = \alpha + \gamma_{pre}\, t\, \mathbb{1}\,(t < 01/2013) + \gamma_{post}\, t\, \mathbb{1}\,(t \geq 01/2013) + \beta D_t + \sum_J \delta_J X_{Jit} + \sum_J \zeta_J D_t \times X_{Jit} + \eta_{it}, \tag{4}$$

where ζ_J captures heterogeneity in the effect of automatic enrollment for different values of characteristic J.

Table 4 displays the results from estimating Equation 4. For each characteristic J, the table displays the estimated coefficient $\hat{\delta}_J$ as well as the coefficient from the interaction with the default $\hat{\zeta}_J$. Age-specific $\hat{\delta}$ and $\hat{\zeta}$ are plotted in Figure A.7. Our results document heterogeneity in the baseline enrollment behavior across different demographic groups. Women are significantly more likely to enroll at baseline than men, and foreign workers are significantly less likely to enroll than Germans. However, the effect of automatic enrollment does not differ significantly across these groups. Regarding age, Figure A.7 documents that baseline enrollment is lowest for youngest workers, while the default effect is strongest for them, and weakest for those close to retirement age. Table 4 also documents substantial heterogeneity across mini-job employment related characteristics, both for the baseline enrollment and the effect of automatic enrollment. We discuss those in detail in section 5, when investigating mechanisms behind the observed enrollment behavior.

4.4 Within Individual Variation

Given the nationwide policy and the panel structure of the dataset, we can analyze how the same individual behaves under different enrollment regimes, before and after the reform, in different jobs. This unique setting allows us to estimate the effect of automatic enrollment using individual fixed effects, and to classify individuals behavior as active or passive.

Effect of Automatic Enrollment with Individual Fixed Effects To account for unobserved, time-constant individual characteristics, we include individual fixed effects in our regression. Intuitively, this allows us to assess the within-individual impact of automatic enrollment. For this analysis, we restrict the sample to workers that start at least one new mini-job under the old opt-in regime and one under the new opt-out regime.

Table 5 reports the regression results: Automatic enrollment increases the enrollment share in $m = 1$ by 19 percentage points. This is slightly lower, but comparable to the estimates from our main analysis. Table 5 also documents that

Table 5: Immediate Effect of Automatic Enrollment with Individual Fixed Effects

	(1)	(2)	(3)	(4)
D_t	0.1851***	0.1893***	0.1838***	0.1881***
	(0.0058)	(0.0057)	(0.0061)	(0.0060)
t	0.0008***	0.0024***		
	(0.0002)	(0.0002)		
t_{pre}			0.0010**	0.0025***
			(0.0004)	(0.0004)
t_{post}			0.0008***	0.0023***
			(0.0002)	(0.0003)
Constant	0.0666***	0.2044***	0.0686***	0.2063***
	(0.0028)	(0.0099)	(0.0047)	(0.0105)
X_{Jit}	No	Yes	No	Yes
μ_i	Yes	Yes	Yes	Yes
R^2	0.6107	0.6202	0.6107	0.6202
N $i \times t$	68,512	68,512	68,512	68,512
N i	17,949	17,949	17,949	17,949

Notes: This table shows the results from estimating our main model for $m = 1$ with individual fixed effects μ_i. The sample includes all workers that we observe starting a new mini-job at least once before and once after the reform. Note that X_{Jit} contains only time varying characteristics as time-constant characteristics are observed by μ_i. Weighted VSKT data, robust standard errors in parentheses. Significance level: *** 0.001; ** 0.01; * 0.05; ^{x} 0.1.

individual fixed effects can explain much of the variation in enrollment shares, with substantially higher R^2 compared to Table 2.

Classification of Behavioral Patterns Based on the enrollment behavior of the same individual under different enrollment regimes, we define different behavioral patterns. For this analysis, we restrict the sample to individuals that we observe at least once under each default regime. As discussed in section 4, we cannot unambiguously determine the default for individuals who remain in mini-job employment around the reform date. To account for this uncertainty, we further restrict the sample to workers for whom the default is never ambiguous. The final sample size for this analysis is 10,376 individuals.

We define *never takers* as workers who never enroll, neither under the opt-in nor under the opt-out regime. *Always takers* on the other hand always enroll irrespective of the default. Both behaviors can be described as *active*, since individuals make a persistent and active enrollment choice that is not impacted by automatic enrollment. The third group is characterized by *passive* behavior, i.e., always sticking to the default. They do not enroll under the opt-in regime and they do not opt out when automatically enrolled. Their enrollment follows the default-setting and a default-changing reform thus influences their future pension entitlements. The

Table 6: Classification of Behavioral Patterns

	Active		Passive	Other
	Never takers	Always takers		
Enrollment under different default regimes				
Opt-in	No	Yes	No	Yes/No
Opt-out	No	Yes	Yes	Yes/No
Share of individuals	0.63	0.02	0.17	0.18
Individual characteristics				
Birth year	1972	1967	1972	1973
Female	0.62	0.91	0.72	0.67
German	0.76	0.92	0.80	0.74
West German state	0.82	0.91	0.87	0.85

Notes: Table defines different behavioral patterns based on the enrollment under different default regimes, as well as the share of individuals that can be classified in the respective category and basic characteristics. The sample includes individuals that are observed in a mini-job at least once before and once after the reform and for whom the default is always unambiguous (sample size: 10,376 individuals).

remaining behavior is neither purely active behavior, nor passive and summarized as *other* behavior.

Table 6 documents that the majority of individuals are never-takers (63 percent). They show active behavior with no inertia and opt out immediately from auto-enrollment. The default does thus not influence their enrollment behavior. The default setting has a consistent effect solely on passive individuals, who, in our context, constitute only a minority of individuals. There are few always takers and 18 percent of individuals show neither purely active nor purely passive behavior. These findings provide important insights for policymakers. If passive behavior is uncommon, as shown here, a change in default can only have limited effects.

4.5 Robustness Checks

Change in Income Threshold With the 2013 reform, the income threshold for mini-jobs increased from 400€ to 450€. This constitutes a potential confounder for our analysis if the income threshold impacts the selection into mini-jobs and enrollment. We provide several findings to address that concern. First, Figure A.4 show that the number of mini-job workers does not jump at the reform date, indicating that individuals do not at large number select into mini-jobs when the income threshold is increased. Second, Figure A.9 shows that most workers earn

no more than 400 € after reform, with a remaining peak at 400 €.[13] Lastly, we rerun the main analysis including only post-reform mini-jobs with monthly income $\leq$ 400 €. The estimated effect of auto-enrollment is 22 percentage points, virtually the same as in our main specification in Table A.5.

Sample Composition Because of our sample restrictions, not all individuals are observed for all periods from 2011 to 2016. For example, a worker born in 1990 is not observed before 2015 because we only include workers aged 25 or older. One potential concern is that this changes enrollment shares over time because of differential baseline enrollment behavior. While we see no reason to expect a discontinuous change at the reform date, we also provide evidence that the sample composition does not impact the reform effect. First, Figure A.5 shows no discontinuous jump for any of the individual characteristics around the reform, besides seasonal patterns that are also observed in other years. Second, we rerun our analysis on a balanced subsample, i.e., for workers observed in all periods. Table A.6 shows that the estimated effect of auto-enrollment for this subsample is 22 percentage points, again very similar to our main specification.

5 Mechanisms Behind Enrollment Behavior

The key finding of our analysis is that, even though automatic enrollment has a positive and sizable effect, most individuals are never-takers and opt out immediately. This contrasts with findings from the existing literature, which typically document that the majority of automatically enrolled individuals remain enrolled.[14] One likely explanation is that, unlike in most other settings, common drivers of inertia do not apply in our context, including high switching costs, choice overload, and interpreting the default as employer advice. This leaves inattention as the primary remaining explanation for inertia. A growing literature shows that consumers are frequently inattentive and fail to actively re-optimize in household financial decisions. Inattention is an important source of inertia that explains why beneficiaries remain in default Medicare Part D plans for years, even when doing so reduces welfare (Brot-Goldberg, Layton, Vabson, and Wang 2023), and why

[13] One explanation for the persisting income concentration at 400 € post reform are adjustment frictions for jobs that started pre reform. Employees (or employers) may also take the 400 € as reference point, because mini-jobs have long been referred to as "400-Euro-jobs", even though there are no financial reasons to do so. Seibold (2021) documents strong reference point dependence for German employees in the context of retirement age thresholds.

[14] Participation rates in US 401(k) plans commonly reach around 90% after the introduction of automatic enrollment (see, e.g., Madrian and Shea 2001; Choukhmane 2025), and similar values are documented in the UK (Bourquin, Cribb, and Emmerson 2020).

only a small share of consumers switch their Medicare Part D prescription drug insurance plans despite large potential savings (Heiss, McFadden, Winter, Wuppermann, and Zhou 2021). Similarly, inattention is a main driver of why consumers rarely revisit health insurance plan choices, which is associated with welfare losses (Drake, Ryan, and Dowd 2022). In a simpler environment, Heiss, Ornaghi, and Tonin (2023) show that even when switching costs are low, households often fail to switch providers, consistent with pure inattention.

The high opt-out rates imply that many attentive individuals decide against enrollment. One potential explanation is that they expect limited benefits from enrollment. As discussed in section 2, and unlike in many other pension settings such as 401(k) plans, enrollment incentives for the targeted workers are generally low. At the same time, the lower degree of complexity in our setting, namely, a binary choice menu, may make the often low benefits from enrollment more salient. This suggests that financial incentives as well as individuals' ability to understand the setting are key mechanisms behind enrollment behavior.

To test these mechanisms, we use our main VSKT data as well as survey data with record linkage to administrative data (SOEP-RV). To be precise, we use the SOEP-RV data to identify mini-jobs held as side jobs and for information on individual knowledge of the own enrollment status (and later for measuring liquidity constraints). The sample size for this linked dataset is substantially smaller, and the administrative data that can be linked does not provide exactly the same information as the main VSKT data (see subsection 3.2 for more details). To validate the SOEP-RV data, we replicate our main results with that dataset. We first provide graphical evidence in Figure A.10, showing a clear discontinuity in enrollment shares for new mini-jobs at the reform date that is of similar size to the one plotted in Figure 1. Table A.7 reports the results from the corresponding regressions. The point estimate for the effect of automatic enrollment from the SOEP-RV sample is comparable to the estimate from the main VSKT data— 20 versus 23 percentage points—despite the substantially smaller sample (see column 4 in Table 2 and Table A.7).

5.1 Financial Incentives

Variation in Benefits: Eligibility Thresholds As described in section 2, mini-job employment periods are fully credited only if individuals enroll. Different amounts of these qualifying periods are required to meet with eligibility thresholds for different types of pensions. For individuals close to a threshold, e.g., 35 years for early retirement (see Table A.4 for more details), benefits from enrollment are larger and are potentially decisive for eligibility at the extensive margin for the respective pension type. For instance, an individual with an insurance record of 34 years will become eligible for early retirement if they work for one additional

year in a mini-job and enroll in the public pension insurance. Without enrollment, less than 12 months will be considered for the qualifying period and they will not reach the 35 year threshold. The same logic applies for the other thresholds. Consequently, we expect higher enrollment for individuals close to a threshold if the incentives are salient to them.

To assess whether mini-job employees understand and react to these incentives, we define an indicator variable that equals one if individuals are less than 12 months away from reaching one of the eligibility thresholds, and zero else. We include that indicator in our regression and interact it with the treatment dummy. Table 4 shows that workers who are close to an eligibility threshold are more likely to enroll at baseline and the default effect is stronger for them. The observed behavior is in line with individuals responding to the incentives for enrollment that are tied to the qualifying period thresholds.

There are two potential mechanisms behind the observed effects. First, individuals who are in a mini-job already, may start enrolling when approaching the threshold. Second, individuals may start a mini-job with enrollment when approaching the threshold.[15] In both cases, a certain knowledge of the institutional setting is required to be able to react to the thresholds. If (at least parts of) the mini-job employees understand incentives in the German public pension system, it seems plausible that they are also aware of their enrollment default. This is one potential explanation for why most mini-job employees opt out of automatic enrollment.

Variation in Costs: Minimum Contribution While financial incentives are likely to play a role for enrollment choices, they are not observed on the individual level in the administrative data. However, we can identify a group that experiences higher costs: individuals with income below the minimum contribution threshold y_t^{min}. As described in section 2, enrolled individuals with income below a certain threshold have to top up regular contribution rates to meet an absolute minimal monthly contribution. All else equal, this decreases incentives for enrollment by imposing higher costs. We add a dummy for this group of individuals in column 3 of Table 4 and show that their enrollment share is much lower compared to individuals who face the regular τ_t^{ee}. The estimated interaction effect is by far the most sizable, its absolute value amounts to about half of the effect size of D_t. This shows that individuals are more likely to deviate from the auto-enrollment default if facing higher costs, and thus lower financial incentives, of enrollment.

[15] Figure A.8 shows that the number of mini-jobs continuously increases before reaching any of the thresholds, and then drops as soon as workers reach that threshold.

Variation in Benefits: Mini-Jobs as Side Jobs Throughout this paper, we focus on workers for whom the mini-job is their main employment. However, mini-jobs can also be side jobs for workers who are in regular employment. In this case, workers are compulsorily enrolled in any case because of their main employment. Additional enrollment in the mini-job impacts only savings, but not eligibility. These workers thus have less benefits from enrollment in their mini-job, compared to those for whom this is the main employment. Since we cannot observe side-job employment in the main VSKT data, we use the SOEP-RV data for this exercise.

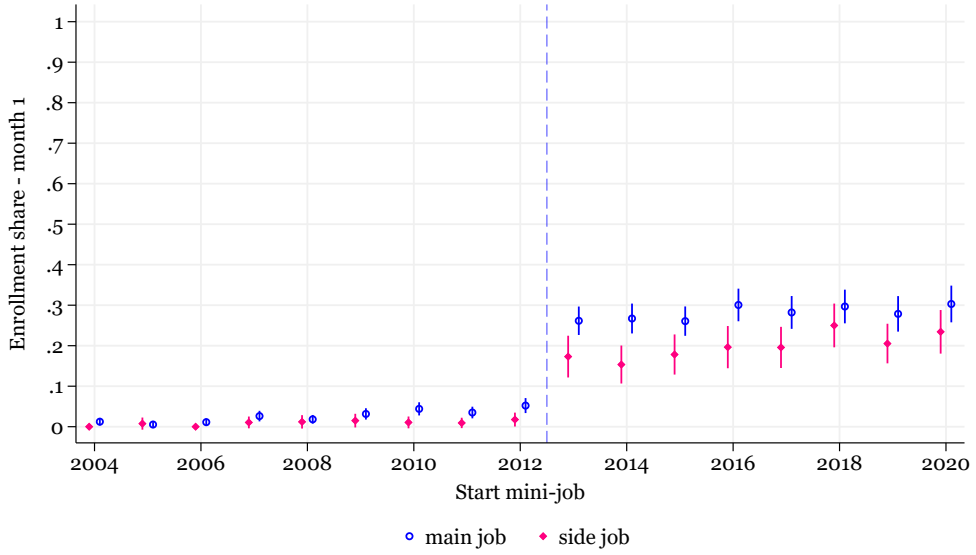


Figure 2: Enrollment Share Side Job vs. Main Job

Notes: SOEP-RV data, means with 95 percent confidence intervals. The figure plots the enrollment share in the first month of a new mini-job (similar to Figure 1), aggregated over years. The sample includes all individuals who start a new mini-job–either as their main employment or as a side job in addition to regular employment. The sample includes years 2004 to 2020, as side jobs became possible only after a reform in 2003

Figure 2 shows an increase in the enrollment share in side jobs after the introduction of automatic enrollment. However, the increase is significantly less strong than for workers for whom the mini-job is their main employment. Table A.8 confirms the graphical evidence: Automatic enrollment increases the enrollment share in side jobs by 12 percentage points, compared to 20 percentage points for main jobs in the SOEP-RV sample (see Table A.7).

5.2 Individual Understanding of Enrollment

Financial literacy is well known to play an important role in economic decisions in general and in retirement savings in particular, while, at the same time, significant parts of the population are not financially literate (e.g., Lusardi and Mitchell 2014, 2023). Given the widespread lack of financial literacy more broadly, one potential explanation for the heterogeneous effects of the default on enrollment is differences in individuals' understanding of their enrollment setting.

Table 7: Direct Measure for Enrollment Knowledge

	Share of individuals
Always knows of own enrollment	0.76
Does not know that they are enrolled	0.13
Does not know that they are not enrolled	0.12
N	821

Individual-level data from SOEP-RV. Individuals that always indicate their own enrollment status correctly are classified as having high enrollment knowledge. If an individual reports an enrollment status that deviates from the observed administrative records at least once, they are classified as having low enrollment knowledge.

The results from our heterogeneity analysis in subsection 4.3 already suggest that the power of the default is stronger for those who understand their enrollment situation better. Column 2 of Table 4 shows that the default effect decreases with past experience with mini-jobs. Being exposed to the mini-job system for a longer period is likely to increase specific institutional knowledge, suggesting that the default is more powerful for those who understand the setting less well.

To directly assess the role of individual understanding of the setting for the power of the default, we use survey data on individual's self-reported enrollment status from the SOEP-RV data. We construct a direct measure of the individual understanding of the enrollment setting, by comparing survey respondents' self-reports about their enrollment status to the true enrollment status observed in the administrative records. If the self-report deviates from the true enrollment at least once, we classify an individual as having low enrollment knowledge. On the contrary, we refer to individuals as having high enrollment knowledge if their self-reports always match their true enrollment status. We treat enrollment knowledge as a time-constant individual trait, since the survey question on the enrollment status is only included since 2018. This allows to study the enrollment behavior of individuals with high and low enrollment knowledge for all periods observed in the administrative data.

Our sample consists of individuals who report a mini-job, who answer the question on their enrollment status at least once in the SOEP, and whose administrative

records show a mini-job employment for at least one month in the same year.[16] To rule out coordination issues related to the exact timing of the survey in a given survey year, we only use observations from years where the individual enrollment status, as reported in the administrative data, does not change within the year. Our final sample consists of 821 individuals.

Table 7 documents that most individuals know whether or not they are enrolled. However, 24 percent do not always correctly assess their current enrollment status. To assess the differential power of default setting for those with high and low levels of enrollment knowledge, we first provide graphical evidence. Figure 3 plots the enrollment shares for new mini-jobs over time for both subsamples. While enrollment shares are similar for both groups before the reform, post-reform enrollment shares are higher for those with low levels of enrollment knowledge. Put differently, individuals are more likely to stick to the auto-enrollment default if they do not understand the enrollment setting.

We then support this graphical evidence by interacting the dummy for automatic enrollment with a dummy for knowledge of the own enrollment status:

$$E_{it} = \alpha + \gamma t + \beta D_t + \delta \text{ low knowledge}_i + \zeta D_t \times \text{low knowledge}_i + \eta_{it} \qquad (5)$$

The notation follows the notation for our main analysis, and *low knowledge*$_i$ is 1 if individual i does not know their own enrollment status at least once, and 0 else. We estimate Equation 5 for the first month of employment ($m = 1$) and report the results in Table A.10.

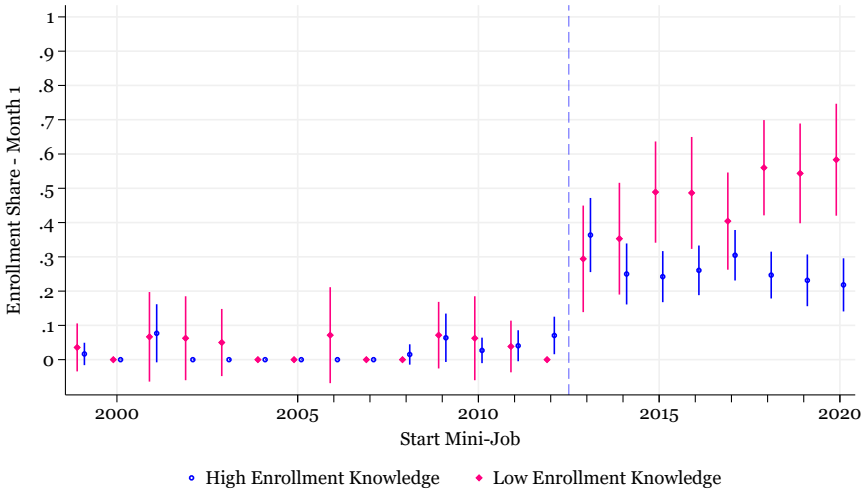
In line with the graphical evidence in Figure 3, Table A.10 shows that the effect of automatic enrollment is significantly stronger for those with low enrollment knowledge. In addition to the baseline default effect of 21 percentage points, the enrollment of those with low enrollment knowledge increases by an additional 20 percentage points when enrolled by default. These results from survey data confirm the suggestive evidence from administrative data: the default is more powerful for those who understand the setting less well.

5.3 Liquidity Constraints

Our finding that the default is particularly powerful for individuals with limited knowledge of their enrollment situation raises the question whether automatic enrollment nudges the "right" individuals into enrollment. While enrollment can be beneficial for some, others may be better off when not contributing. One key factor here is financial insecurity: If individuals face liquidity constraints, saving more

[16] We exclude cases where survey respondents report a mini-job, while this is not observed in the administrative records, and vice versa.

Figure 3: Enrollment Share by Enrollment Knowledge



Notes: SOEP-RV data, means with 95 percent confidence intervals. The figure plots the enrollment share in the first month of a new mini-job (similar to Figure 1), aggregated over years. The sample includes all individuals who at some point had a mini-job and answered the survey question on their enrollment status. The *High Enrollment Knowledge* subgroup includes those whose beliefs about the own enrollment status always map the true enrollment observed in the administrative records, while the *Low Enrollment Knowledge* group includes those who do not know their own enrollment at least once. Note that the knowledge measure is defined on the individual level, i.e., constant over time if an individual is observed more than once, because the question is only included in surveys since 2018.

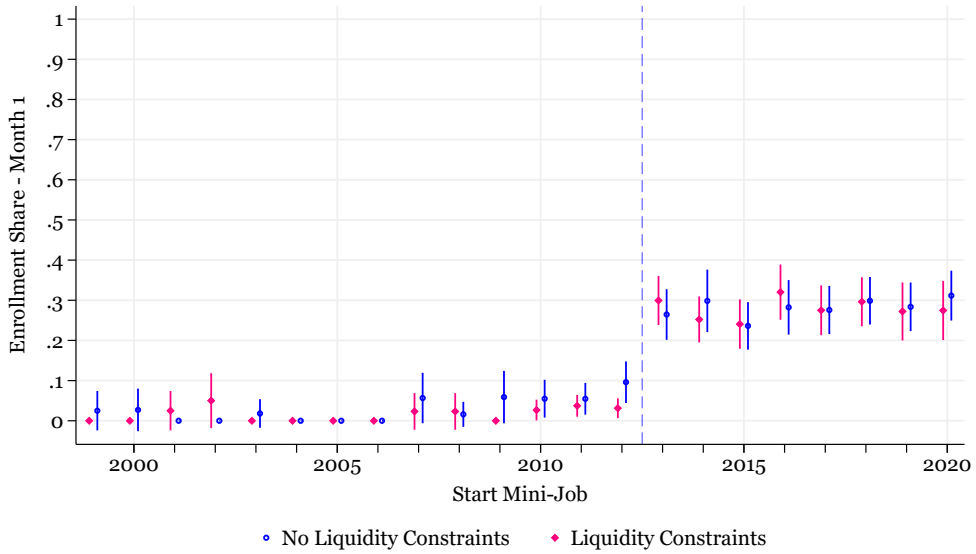
might not be optimal—especially since contributions to the pension system are illiquid. In line with these concerns, Bourquin, Cribb, and Emmerson (2020) find that automatic enrollment yields high participation rates for financially vulnerable workers in the UK system. In our setting, all workers have low income, which raises the concern that liquidity constraints are relevant for this population.

To measure liquidity constraints we use the linked SOEP-RV survey data. We define the measure at the household level and consider a household as liquidity constrained if they report that income is not sufficient for saving anything at the end of the month. Our sample consists of individuals who start a new mini-job, as observed in the administrative records, and answer the question on their savings potential in the SOEP in the same calendar year (we report sample characteristics in column B4 of Table 1).

Liquidity constraints are common among mini-job employees. When starting a new mini-job, 51 percent of mini-job employees are liquidity constrained. However, this group is not more likely to opt out from automatic enrollment. Rather,

Figure 4 shows that their enrollment behavior is similar to those without liquidity constraints. This raises concerns about the extent to which automatic enrollment policies nudge the wrong individuals into saving more. When starting a new mini-job under automatic enrollment, individuals who do not opt out are liquidity constrained in 49 percent of all cases.

Figure 4: Enrollment Share by Liquidity Constraints



Notes: SOEP-RV data, means with 95 percent confidence intervals. The figure plots the enrollment share in the first month of a new mini-job (similar to Figure 1), aggregated over years. The sample includes all individuals who start a new mini-job and answer the survey question on their ability to save.

In robustness checks, we use a set of different definitions for measuring liquidity constraints, including having debt and reporting required household income that is larger than the reported household income. We also allow for a larger sample by including not only observations from individuals who answer the respective survey question in the exact year they start the mini-job, but using their answer from the closest point in time they answered that question. We provide sample sizes in Table A.9 and plot enrollment shares in Figure A.11. The overall pattern remains the same: automatic enrollment impacts individuals with and without liquidity constraints similarly. This is a worrying side-effect of automatic enrollment: it also nudges those into enrollment who are likely better off not contributing.

6 Conclusion

It is well established that individuals commonly stick to defaults, particularly in the context of automatic pension enrollment. We show that the power of the default is limited in a pension setting in which the choice menu is binary and the financial incentives for enrollment are low for many individuals. In this setting, many of the explanations for inertia in more complex settings can be ruled out, including switching costs, choice overload, and the misperception of a default as investment advice from the employer, leaving inattention as the main explanation.

We analyze a natural experiment created by the introduction of automatic enrollment for low-income employees with a binary choice menu in the German PAYG pension system. We find that automatic enrollment significantly increases take-up. However, compared to the existing literature on auto-enrollment, the effect of changing the regime from opt-in to opt-out is small because the majority of individuals opt out immediately. Only a few individuals exhibit truly passive behavior, defined as always sticking to the default, while the majority is best described as never-takers who never enroll.

Overall, the observed behavior is in line with a significant share of individuals understanding and reacting to enrollment incentives. Using automatic enrollment to nudge workers into higher public pension savings may thus be less effective than policymakers hope when incentives are small. At the same time, some individuals fail to understand their own enrollment situation, and those workers with limited knowledge are substantially more likely to stick to the default. However, saving more in their public pension accounts is not necessarily beneficial for them. For instance, we find that many of the enrolled workers are liquidity-constrained. Automatic enrollment thus risks nudging the "wrong" individuals into enrollment.

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Appendix

A.1 Additional Figures and Tables

Figure A.1: Example Form for Opting in for Enrollment

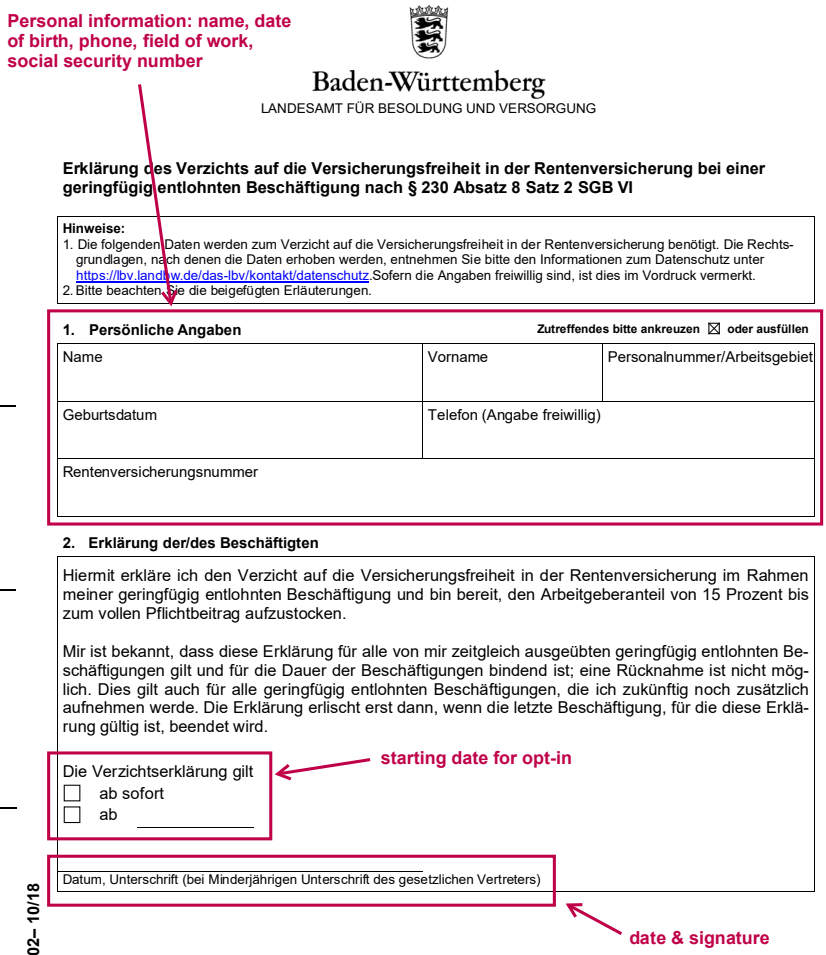


Figure A.2: Example Form for Opting out from Enrollment

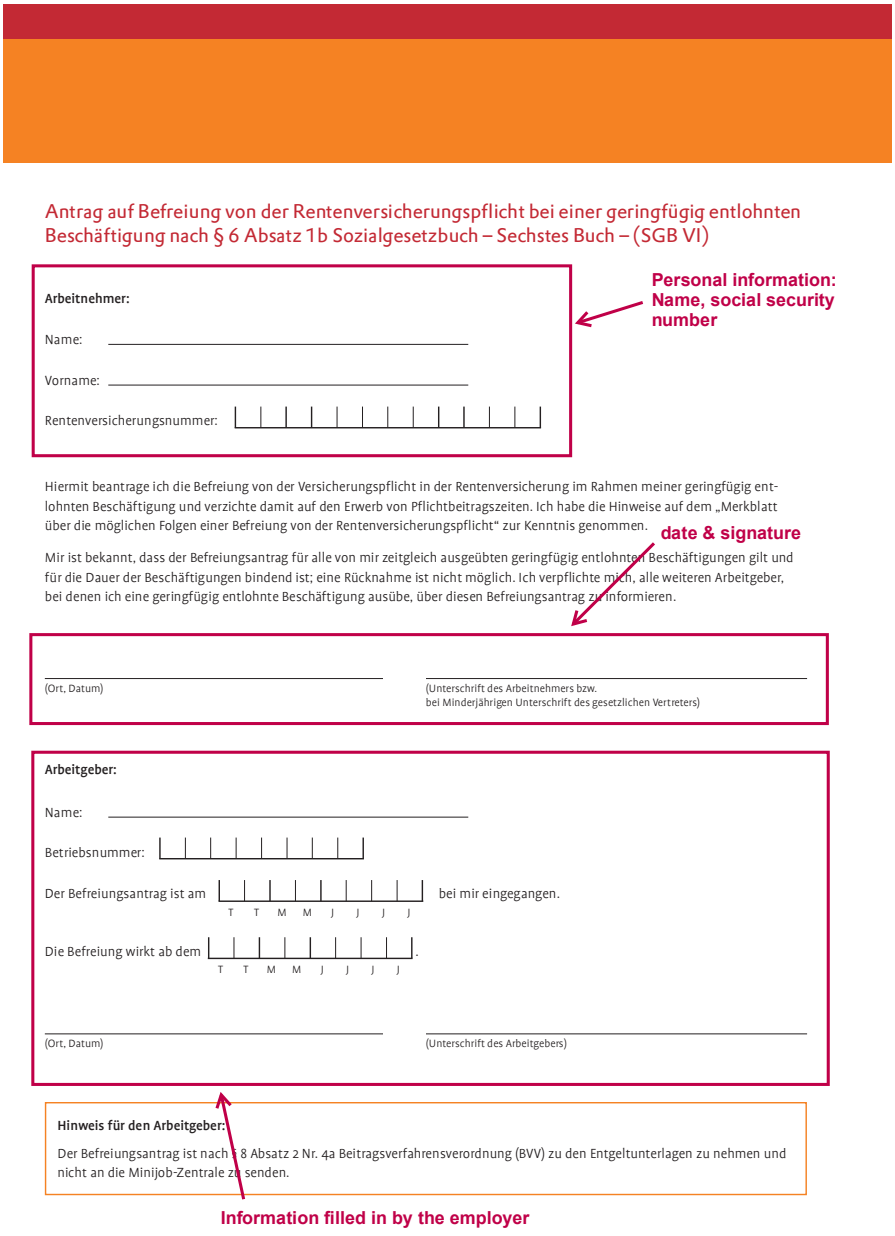
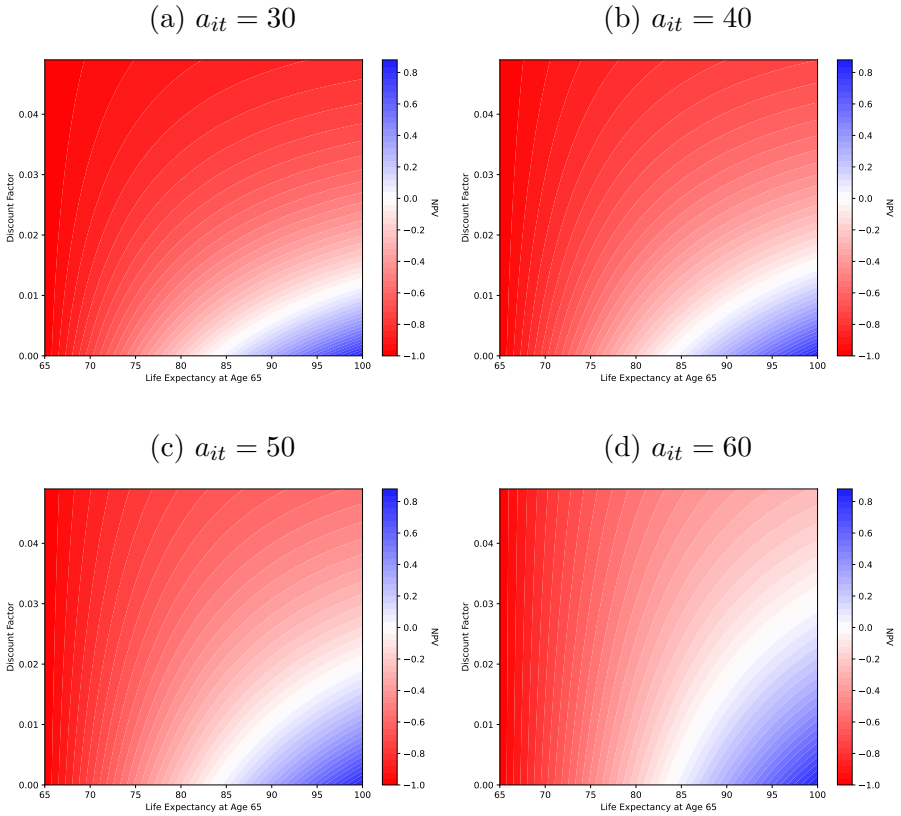
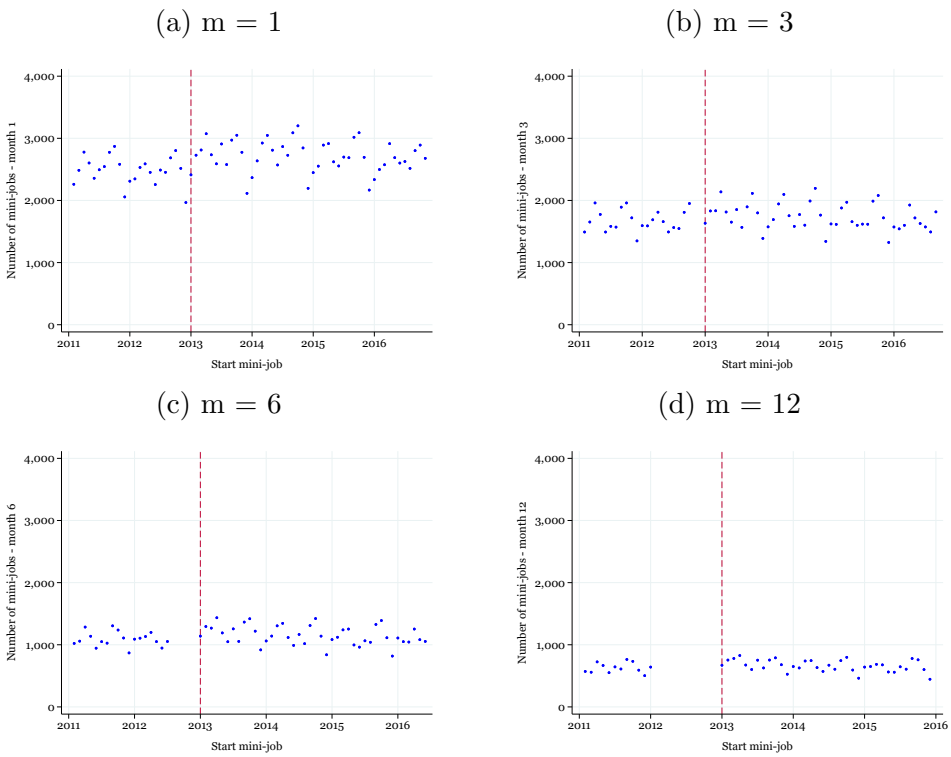


Figure A.3: Net Present Value of Enrollment



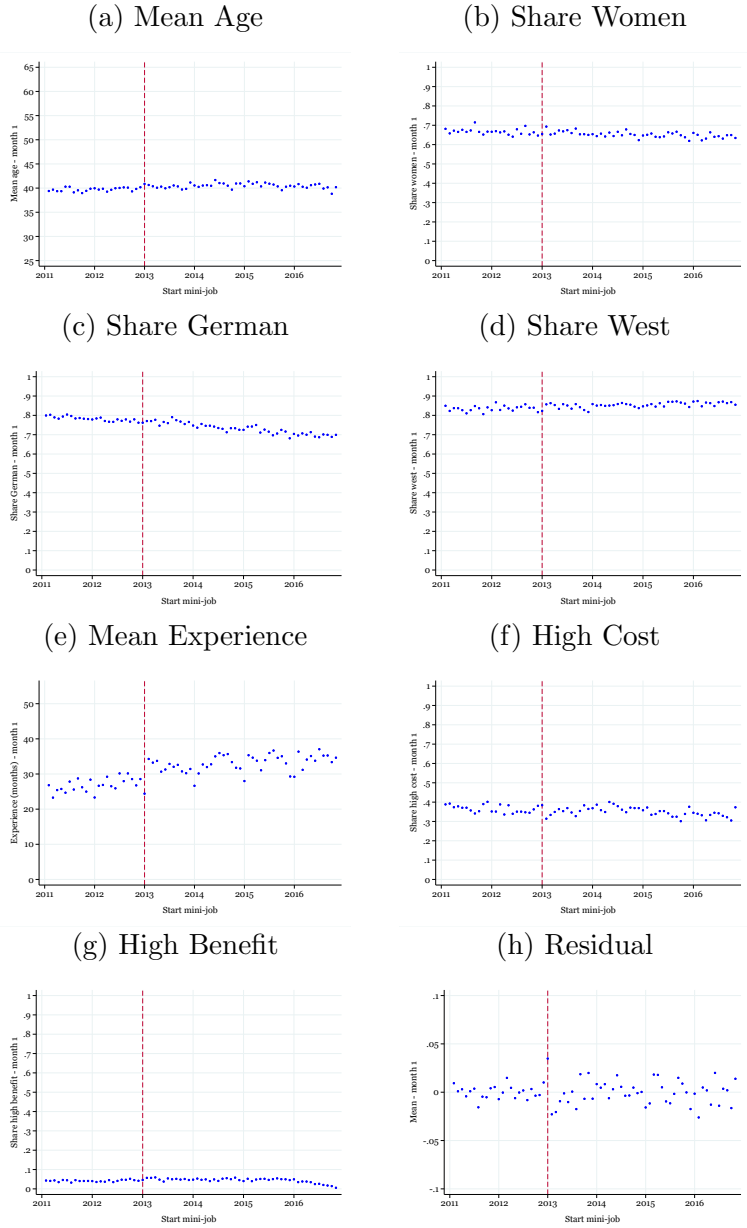
Notes: This figure plots the net present value (NPV) of contributing 1 Euro for 30, 40, 50, and 60 year old individuals, as a function of the discount factor (δ) and expected life expectancy at retirement age (a^{max}) for $\pi_{ia} = 1$, $a^r = 65$, and $y_{it} = 450$ in 2013. The white area marks combinations of a^{max} and δ, for which NPV_{it} is (close to) zero. For all combinations to the left of (red areas), NPV $<$ costs, while NPV $>$ costs for all combinations to the right (blue areas).

Figure A.4: Number of Mini-Jobs



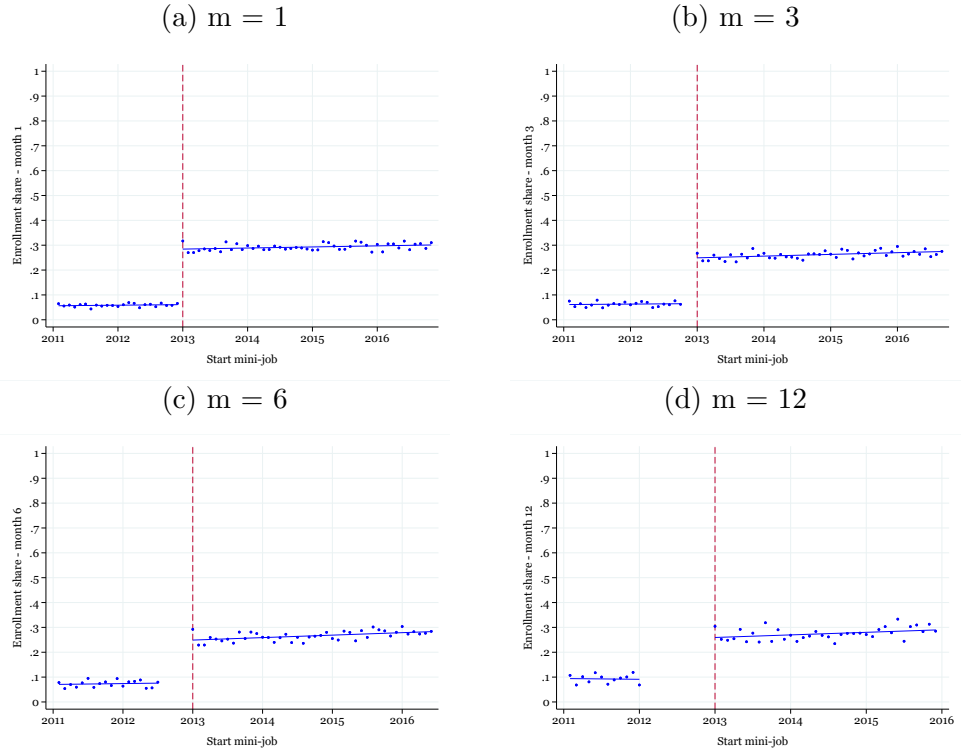
Notes: Raw data. Graphs display the number of mini-jobs for which we identify a start in month t in the m^{th} month of that mini-job over t. In each panel, the line at given t contains the same group of individuals, exclusive those who dropped out of their mini-job employment before reaching the m^{th} month of this employment. For example, an individual who is employed for four months, is only considered for Panel a and b. For $m > 1$, we only consider months that we use for our regression, i.e., months in which we can observe the default.

Figure A.5: Individual Characteristics and Residual Over Time



Notes: Weighted data. Scatter plots in panel a to e display the average value for each control variable in the first month of the mini-job for individuals who started their mini-job in month t over that t. Panel h displays the average residuals (η_{it}) from Equation 3, again for the first month of employment.

Figure A.6: Enrollment Share over Tenure



Notes: Weighted VSKT data. Each dot represents the average enrollment share E_t^m in the m^{th} month of the mini-job for individuals who started their mini-job in month t over that t. The line plots the linear best fit (OLS) from estimating Equation 3 without controls. Each panel includes only observations for which the default is known, which excludes continuous mini-job employment spells that start pre-reform and are observed post reform for $m > 1$. The corresponding sample size is shown in Figure A.4. Panel a is the same as Figure 1 and is included here to facilitate comparison with the other months.

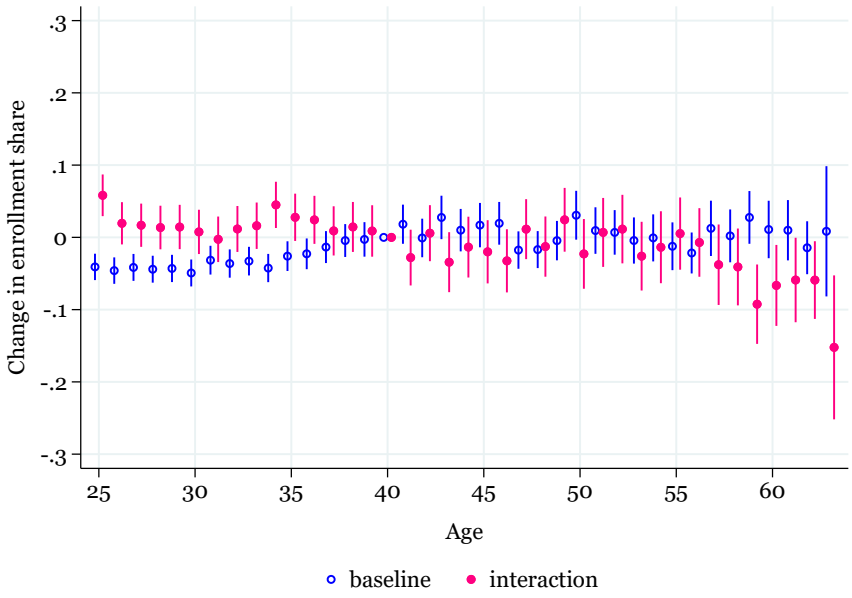


Figure A.7: Age-specific Enrollment

Notes: This figure plots age dummies for $\hat{\delta}$ (baseline) and $\hat{\zeta}$ (interaction with effect of automatic enrollment). The estimates are obtained from estimating Equation 4, and correspond to the results in column (3) of Table 4.

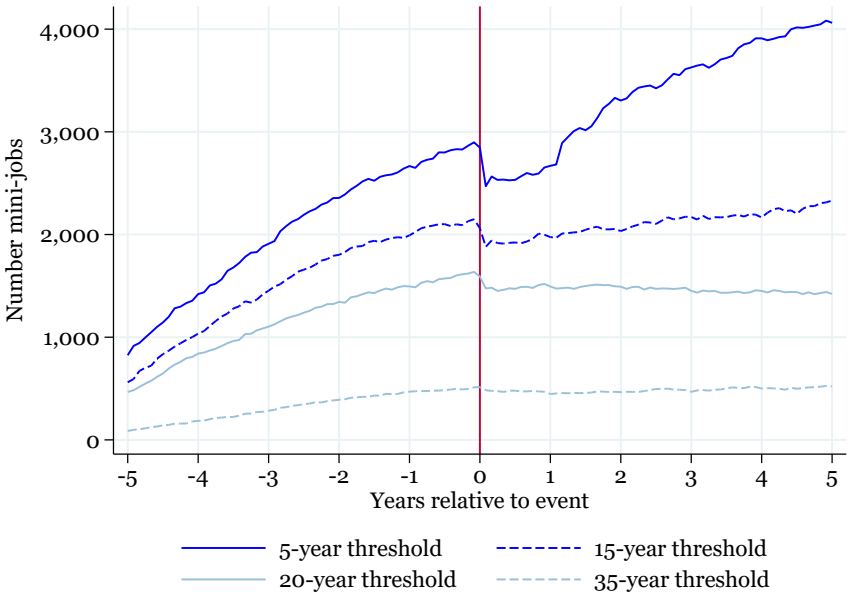
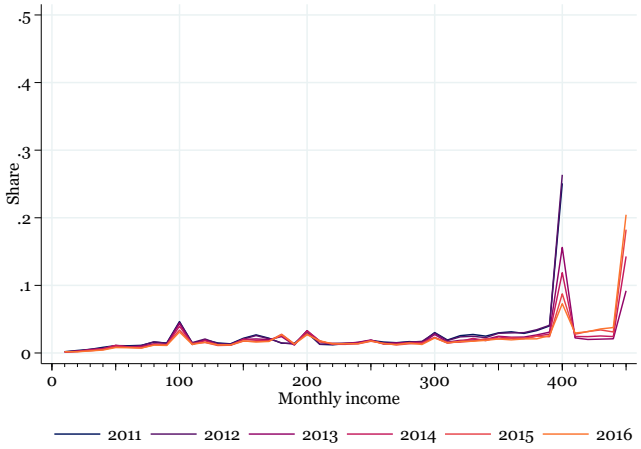


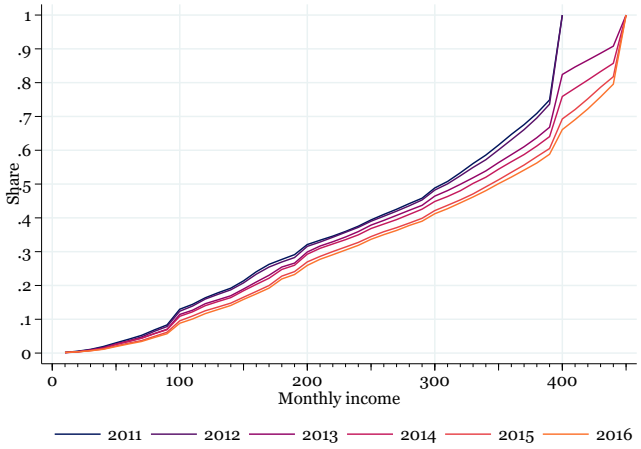
Figure A.8: Number of mini-jobs and eligibility thresholds

Notes: Unweighted VSKT data. This figure plots the number of mini-jobs over time relative to reaching different eligibility thresholds.

Figure A.9: Income Distribution Mini-Jobs



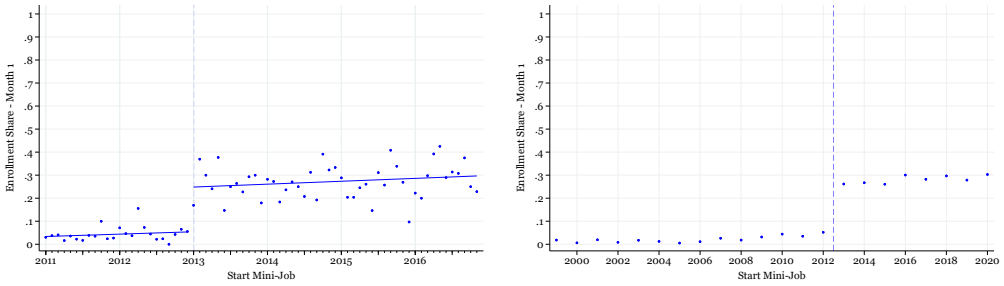
(a) Probability density function



(b) Cumulative distribution function

Notes: Annual income distribution for mini-jobs. Income binned in 10-EUR bins, weighted VSKT data. The income threshold for mini-jobs is 400 € for 2011 and 2012 and 450 € from 2013 on.

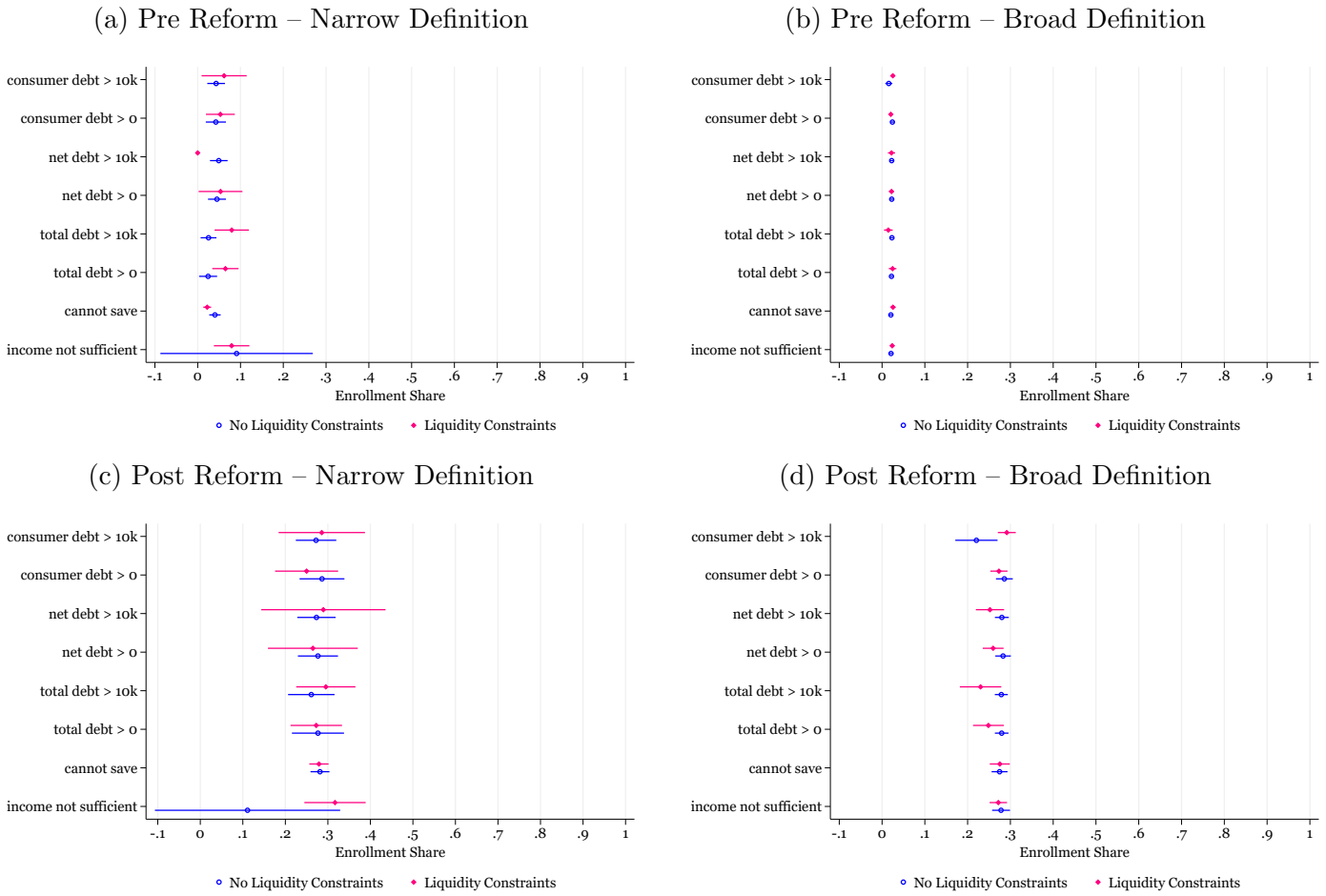
Figure A.10: Replication Main Result with Survey Data



(a) Monthly data (b) Annual data

Notes: This figure replicates out main result from Figure 1 using the linked survey data from SOEP-RV. Each dot represents the average enrollment share in the first month of the mini-job for individuals who started their mini-job in a given period. Panel a is the equivalent to Figure 1, using monthly data for the same period. Panel b plots the annual averages for a longer time period.

Figure A.11: Enrollment by Liquidity Constraints



Notes: SOEP-RV data, means with 95 percent confidence intervals. The figure plots the enrollment share in the first month of a new mini-job aggregated over periods pre and post reform. *Narrow Definition* includes only cases where individuals start a new mini-job in the year they answer the respective survey question. *Broad Definition* includes all cases where individuals start a new mini-job and have answered the respective survey question at least once, in any year. For a given period, we use the survey answer that is closest in time.

Table A.1: Sample Characteristics

	$m = 1$	$m = 3$	$m = 6$	$m = 12$
Female	0.66	0.69	0.72	0.76
Age	40	41	42	43
West German state	0.85	0.85	0.86	0.87
German citizenship	0.75	0.77	0.78	0.80
N individuals	92,806	74,919	56,582	37,073
N mini-jobs	184,696	123,047	81,099	45,657

Notes: Up to four points of observation for each employment: in the first ($m = 1$), third ($m = 3$), sixth ($m = 6$) and twelfth ($m = 12$) month of employment. Basic characteristics for the weighted sample. Note that for $m > 1$ this includes also months where the default is ambiguous, which is why the sample sizes differ from the ones reported in Table 3.

Table A.2: Sample Characteristics SOEP-RV Samples

	(1) Full sample	(2) Knowledge	(3) Liquidity
Sample size			
N i	5,218	821	720
N $i \times t$ $(m = 1)$	11,998	2,372	4,916
Demographics			
Age	34	36	36
Female	0.66	0.79	0.67
German	0.94	0.94	0.94
Migrant	0.21	0.26	0.21
West	0.79	0.85	0.80
Married	0.45	0.52	0.45
$<$ High school	0.16	0.19	0.16
High school	0.61	0.67	0.61
$>$ High school	0.23	0.14	0.23

Notes: Data from SOEP-RV for different subsamples. (1) is the full SOEP-RV sample for reference (same as column B1 in Table 1). (2) and (3) are subsamples that allow for analyzing individual knowledge of the own enrollment status and liquidity, respectively. These subsamples are smaller because not all questions are asked every year in the survey. See Table 1 for more details on the variables.

Table A.3: Operands of the German Statutory Pension Insurance

year	Y	$\overline{Y}$		PPV		τ		
		east	west	east	west	τ^{full}	τ^{er}	τ^{ee}
2011	32,100	57,600	66,000	24.37	27.47	19.9	15	4.9
2012	33,002	57,600	67,200	24.92	28.07	19.6	15	4.6
2013	33,659	58,800	69,600	25.74	28.14	18.9	15	3.9
2014	34,514	60,000	71,400	26.39	28.61	18.9	15	3.9
2015	35,363	62,400	72,600	27.05	29.21	18.7	15	3.7
2016	36,187	64,800	74,400	28.66	30.45	18.7	15	3.7

Notes: Y = average annual income in €, valid from July in the given year until June in the following year; $\overline{Y}$ income threshold in € (no contributions for income above $\overline{Y}$); PPV = pension point value in € for pensions payed in the given year; τ = contribution rate; τ^{full} = contribution rate under full contribution; τ^{er} = contribution rate for mini-job employers; τ^{ee} = contribution rate for mini-job employees (all in percent).

Table A.4: Qualifying Period Thresholds

Threshold	Pension Type	Eligible Group
5 years	Standard old-age pension	No further restrictions
15 years	Old-age pension for women	Women born before 1952
15 years	Old-age pension on account of unemployment	Individuals born before 1952 and above a certain age threshold
20 years	Reduced earning capacity pension	Individuals with reduced earnings capacity who have not reached the 5-year threshold.
35 years	Long service pension	Individuals above a certain age threshold
35 years	Old-age pension for people with severe disabilities	Individuals with severe disabilities and above a certain age threshold
45 years	Exceptionally long service pension	Individuals above a certain age threshold

Notes: Simplified overview of the different eligibility thresholds in the German public pension insurance. Qualifying periods include periods of (regular) employment as well as a variety of other situations, including parental leave or unemployment. Which situations are considered as qualifying period differs slightly across the different thresholds.

Table A.5: Robustness Check: Income ≤ 400 € ($m = 1$)

	(1) Equation 2	(2)	(3)	(4) Equation 3
D_t	0.2058***	0.2179***	0.2067***	0.2197***
	(0.0054)	(0.0053)	(0.0052)	(0.0051)
t	0.0003	0.0004***		
	(0.0001)	(0.0001)		
t_{pre}			0.0002	0.0003
			(0.0002)	(0.0002)
t_{post}			0.0003	0.0005**
			(0.0001)	(0.0001)
Constant	0.0546***	0.1058***	0.0533***	0.1034***
	(0.0023)	(0.0081)	(0.0033)	(0.0085)
X_{Jit}	No	Yes	No	Yes
R^2	0.0713	0.1201	0.0714	0.1201
N $i \times t$	132,050	130,268	132,050	130,268
N i	69,429	68,198	69,429	68,198

Notes: This table replicates the findings from Table 2, but restricting the post-reform sample to mini-jobs with income up to the pre-reform income threshold of 400 €, as described in subsection 4.5. See notes in Table 2 for explanation of variables etc.

Table A.6: Robustness Check: Balanced Sample

	(1) Equation 2	(2)	(3)	(4) Equation 3
D_t	0.2052***	0.2147***	0.2042***	0.2156***
	(0.0061)	(0.0060)	(0.0058)	(0.0057)
t	0.0003	0.0004**		
	(0.0002)	(0.0002)		
t_{pre}			0.0004	0.0003
			(0.0002)	(0.0003)
t_{post}			0.0003	0.0004*
			(0.0002)	(0.0002)
Constant	0.0545***	0.1018***	0.0559***	0.1006***
	(0.0026)	(0.0083)	(0.0036)	(0.0087)
X_{Jit}	No	Yes	No	Yes
R^2	0.0769	0.1237	0.0769	0.1237
N $i \times t$	109,931	108,318	109,931	108,318
N i	56,603	55,490	56,603	55,490

Notes: This table replicates the findings from Table 2, but restricting the sample to workers that are observed for the entire sample period, as described in subsection 4.5. See notes in Table 2 for explanation of variables etc.

Table A.7: Replication Main Result with Survey Data

	(1) Equation 2	(2)	(3)	(4) Equation 3
D_t	0.1924***	0.1957***	0.1894***	0.1983***
	(0.0247)	(0.0257)	(0.0360)	(0.0375)
t	0.0010	0.0010		
	(0.0006)	(0.0007)		
t_{pre}			0.0009	0.0012
			(0.0008)	(0.0009)
t_{post}			0.0010	0.0010
			(0.0007)	(0.0007)
cons	0.0309**	-0.0561*	0.0329**	-0.0578*
	(0.0094)	(0.0221)	(0.0104)	(0.0232)
X_{Jit}	No	Yes	No	Yes
R2	0.0778	0.0870	0.0778	0.0870
N	3,374	3,132	3,374	3,132

Notes: This table provides the regression results from estimating Equation 2 and 3 with the SOEP-RV sample. Each column is the same specification as the respective column in Table 2.

Table A.8: Effect of Automatic Enrollment for Side Jobs

	(1) Equation 2	(2)	(3)	(4) Equation 3
D_t	0.1401***	0.1408***	0.1202***	0.1216***
	(0.0184)	(0.0184)	(0.0258)	(0.0259)
t	0.0005**	0.0005**		
	(0.0002)	(0.0002)		
t_{pre}			0.0001*	0.0001
			(0.0001)	(0.0001)
t_{post}			0.0009**	0.0009*
			(0.0003)	(0.0003)
$cons$	0.0223***	0.0067	0.0135***	-0.0032
	(0.0050)	(0.0208)	(0.0037)	(0.0205)
X_{Jit}	No	Yes	No	Yes
R2	0.0927	0.0964	0.0938	0.0975
N	3,506	3,489	3,506	3,489

Notes: This table provides the regression results from estimating Equation 2 and 3 for side jobs based on SOEP-RV data. The sample includes years 2004 to 2020, as side jobs became possible only after a reform in 2003. Each column is the same specification as the respective column in Table 2.

Table A.9: Share of New Mini-Jobs Started Under Liquidity Constraints

	Narrow Definition		Broad Definition	
	Share	N	Share	N
Expenses > income	0.94	345	0.85	7,791
Cannot save	0.51	4,916	0.49	11,992
Gross debt	0.53	868	0.51	11,089
Gross debt > 10k	0.39	868	0.38	11,089
Net debt	0.16	868	0.16	11,089
Net debt > 10k	0.07	868	0.08	11,089
Consumer debt	0.35	868	0.34	11,089
Consumer debt > 10k	0.18	868	0.17	11,089
Any of the above	0.57	4,919	0.86	11,998

Notes: This table shows how common liquidity constraints are for mini-jobs. Each row represents a different definition of liquidity constraints from the SOEP-RV data. Many questions are not asked every year in the survey. The narrow definition includes only mini-jobs that start in a year where the worker answers the respective question. The broad definition includes all mini-jobs for workers who have answered the respective question at least once and uses the closest survey year if they answered the question more than once.

Table A.10: Interaction Enrollment Knowledge and Default

	Outcome: enrollment in month 1
D_t	0.2142***
	(0.0253)
Low Enrollment Knowledge	0.0112
	(0.0122)
Interaction	0.1986***
	(0.0332)
t	0.0002
	(0.0001)
cons	0.0332**
	(0.0102)
R^2	0.1672
N	2,372

Notes: Effect on the enrollment of individual i in the first month of their mini-job in month of observation t. Coefficients from the regression specified in Equation 5, robust standard errors in parentheses. D_t: Dummy for the default, $D_t = 1$ under auto-enrollment and 0 else. *cons*: constant. N: number of new mini-jobs. Individuals can have more than one mini-job over the sample period, which is why N > the number of individuals as reported in Table 7. Significance level: *** 0.001; ** 0.01; * 0.05; ^{x} 0.1.

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