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

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Abstract

Default setting has been shown to be a powerful tool to enhance enrollment in pension programs, which often involve multidimensional choices and complex choice menus. We document that the effect is less pronounced when the choice menu is simple and it is easy to opt out. We study low-income employees who face a binary choice: to enroll or not to enroll in the German public pension insurance. Using administrative data, we show that program participation increases by 23 percentage points after the introduction of automatic enrollment. However, the majority of individuals actively opt out. We document that behind this average effect, there is substantial heterogeneity across different groups of individuals. We use linked survey data to further explore this heterogeneity and show that the default is particularly powerful for individuals with low financial literacy.

Keywords: Default-Setting, Auto-Enrollment, Pensions, Financial Literacy
JEL Codes: D14, H55, J26

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1 Introduction

How to improve individual choices is a key question in both economic research and policy making. One way that policy makers can direct choices is by actively designing the choice setting. One design option that has attracted a lot of attention in this context is default setting for pension enrollment. Interest has been growing over the past two decades since retirement plans increasingly build on defined contributions that typically feature default settings both in terms of contributions as well as investment strategy (Benartzi and Thaler 2007). The findings of the literature are very clear: many individuals show passive saving behavior, they are prone to inertia, and, therefore, tend to stick to the default setting if enrolled automatically (e.g., Blumenstock, Callen, and Ghani 2018; Chetty, Friedman, Leth-Petersen, Nielsen, and Olsen 2014; Choi, Laibson, Madrian, and Metrick 2004; Madrian and Shea 2001).

This paper shows that in a setting where the choice menu is simple, the effect of automatic enrollment is smaller than what is typically observed in more complex settings. We use German administrative data to explore the introduction of automatic public pension enrollment for low-income employees in Germany, who face a binary enrollment choice. Employing a regression-discontinuity design (RDD), we document that the change in default has a significant but limited effect on enrollment. The majority of the affected individuals opt out immediately and permanently remain not enrolled under automatic enrollment. We then link the administrative data with survey data to shed light on the heterogeneity of individual enrollment choices that is hidden behind the aggregate numbers.

Enrollment in the German public pension insurance is optional for employees below a certain income threshold – so-called *mini-job* employees. All mini-job employees face the same binary choice: to enroll or not to enroll. There is no choice of the contribution rate level and since the German public pension system is a pay-as-you-go system, there are no funds to choose for investment. This binary choice menu is much simpler than most other settings for automatic enrollment that often include multi-dimensional and continuous options, e.g. 401(k) savings plans.

We examine the power of defaults by studying a natural experiment. A reform in 2013 introduces a change in the default enrollment status for new mini-jobs, going from an opt-in to an opt-out regime with automatic enrollment. Building on administrative panel data from the German Pension Insurance, we employ a regression-discontinuity design to identify the causal effect of automatic enrollment in the binary choice setting. We find that after the reform, enrollment for new mini-jobs increases by 23 percentage points in the first month. Given that the pre-reform enrollment share is about 5 percent, this effect is sizable. However, for the majority

of individuals, the change in default does not affect their enrollment with about 70 percent opting out immediately. The effect decreases with length of employment but remains economically and statistically significant in the medium run: after 12 months in a mini-job, the enrollment share is still about 14 percentage points higher under automatic enrollment.

We find heterogeneous effects of the default across different demographic groups. Automatic enrollment has a stronger effect on younger individuals, non-German citizens, women, and those who live in the Eastern part of Germany. In addition, the effect is less strong for those who have a longer mini-job employment history. From a policy perspective, it is important to understand both heterogeneity and limits of the power of defaults when designing policies aiming to enhance old-age savings, especially as those most likely to adhere to defaults are not necessarily those for whom enrollment is most beneficial.

To further assess the heterogeneity of individual enrollment behavior, we link the administrative data with household survey data from the German Socioeconomic Panel (GSOEP). With this linked data set, we construct a measure of financial literacy by comparing the true enrollment status with individual beliefs about their own enrollment, as reported in the survey. We find that about one fourth of mini-job employees have low financial literacy, which we define as not always being aware of their own enrollment status. The default is significantly more powerful for those with low financial literacy, meaning that among individuals who are not informed about their actual enrollment status almost twice as many are sticking to the default compared to those who are informed about their enrollment status.

To better understand why opt outs are so prevalent, we analyze the enrollment behavior of individuals observed under both default regimes. We show that a large share of mini-job employees are so-called "never takers": two thirds never enroll, irrespective of the default. Truly passive behavior, defined as always sticking to the default, is observed for less than 15 percent of the mini-job employees. Furthermore, we show that a significant share of mini-job employees seems to understand and react to enrollment incentives. We show this by analyzing discontinuities in incentives at waiting period thresholds that are decisive for pension eligibility. At the same time, and surprisingly so, the own financial situation does not seem to impact the enrollment choice. We do not find that those facing liquidity constraints are more (or less) likely to opt out from automatic enrollment. This is a relevant finding given that liquidity constraints are common in the population of mini-job employees: For new mini-jobs post reform, 48 percent of employees who contribute to the public pension insurance face liquidity constraints.

This paper contributes to the literature on the impact of default options on indi-

vidual behavior and in particular to the literature on auto-enrollment in retirement saving and pension schemes. A significant share of this literature studies choices in employer sponsored defined contribution plans in the US (401(k) plans). Starting with Madrian and Shea (2001), several studies have investigated the effect of auto-enrollment in 401(k) plans, e.g., Choi, Laibson, Madrian, and Metrick (2004), Bernheim, Fradkin, and Popov (2015) or Beshears, Choi, Laibson, Madrian, and Wang (2016).[1] This literature often documents that the setting of the default has a strong impact on individual 401(k) savings behavior. Under automatic enrollment, employees are more likely to contribute to a 401(k) plan and if they contribute, they typically stick to the default contribution rate and invest in the default funds. Strong effects are also found in other settings, e.g., the United Kingdom (Cribb and Emmerson 2020), Australia (Butt, Donald, Foster, Thorp, and Warren 2018) or Afghanistan (Blumenstock, Callen, and Ghani 2018).

The power of defaults has been documented in settings with complex choice menus. In this paper, we investigate the impact of default setting for a comparably simple choice menu. This allows to disentangle the pure impact of default setting from other factors that are likely at play in situations where individuals face more complex choices. For instance under 401(k) plans, employees do not only choose whether or not to enroll, but also how much to contribute and where to invest their contributions. If the choice problem is multi-dimensional or the number of options is large, choice overload may contribute to the default stickiness (Iyengar and Lepper 2000, Goda, Levy, Manchester, Sojourner, and Tasoff 2020). Madrian and Shea (2001) argue that part of the observed default stickiness also stems from employees' interpretation of the default as their employer's investment advice. Additionally, switching costs can account for inertia as well (Gabaix 2019, Heiss, McFadden, Winter, Wuppermann, and Zhou 2021).

In the setting we study, none of these complications arise. The employees' choice menu is simple: to enroll or not. Choice overload is thus unlikely to explain default stickiness. Since the default is set by the federal government and not the employers, the default cannot be misinterpreted as their advice either. In addition, the immediate cost for opting out from default enrollment for mini-jobs in the pension insurance is comparatively low – there is only a standard form that has to be filled in (see Figure A.6 for an example) – making switching costs an unlikely explanation preventing individuals from opting out.

This paper also adds to the literature on passive savings behavior. We find that only a minority of individuals show truly passive behavior, which we define as always

[1] See Clark and Pelletier (2019) for a more detailed overview of the findings of the default literature in the 401(k) setting.

sticking to the default irrespective of what the default is. The majority of mini-job employees are best described as never takers, who never enroll, irrespective of the default. This is in contrast to findings from Chetty, Friedman, Leth-Petersen, Nielsen, and Olsen (2014), who find that the vast majority of individuals in the Danish context can be described as passive savers, who do not respond to subsidies nor adjust their savings outside their pension accounts when facing changes in contribution rates. One explanation is that passive behavior is not necessarily an individual trait but depends on the decision context (Goda, Levy, Manchester, Sojourner, and Tasoff 2020).

The remaining part of the paper is structured as follows. We give an overview of the institutional background and the reform that changed the enrollment default for mini-job employees in section 2 and introduce the data sets we use in section 3. We analyze the effect of the default on the public pension enrollment in section 4. In section 5 we shed some light on the drivers of the observed individual behavior. We conclude with section 6.

2 Institutional Setting

2.1 German Statutory Pension Insurance

The statutory pension system in Germany is an earnings related pay-as-you-go (PAYG) system with compulsory enrollment for most employees.[2] Both employees and employers make contributions with contribution rates being defined by law on the national level.

Pension Points Contribution payments are translated into pension points that an insuree accumulates over their working life. An enrolled employee earning exactly the average annual income obtains 1 pension point, an employee who earns 50 percent (150 percent) of the average annual income obtains 0.5 (1.5) pension points and so on, with contributions being capped for incomes above a certain threshold. To a smaller extent, pension points can also be acquired during other periods such as parental leave or unemployment.

Pension points are monetized upon retirement and determine the level of the monthly pension payment. Their value mainly depends on the average labor market

[2]Civil servants and most of the self-employed are excluded, but self-employed may enroll deliberately. For certain self-employed, such as physicians or lawyers, occupation-specific plans are available.

income in a given year, and the contribution rate for the working population as well as on 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.1 for an overview of the relevant operands).

Waiting Periods In order to become eligible for a regular pension at the statutory retirement age, a minimum waiting period of five years is required. Eligibility for special pensions is tied to other minimum waiting periods. For instance, to become eligible for early retirement, 35 years of contributions are required. See Table A.2 for details.

2.2 Mini-Jobs

Mini-jobs are characterized by a very low monthly gross income. During our sample period, the upper income threshold for mini-jobs is 400 € for the years before 2013 and 450 € for the remaining years. Mini-jobs are exempt from income taxes and also 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 group.

Employers' Contribution Contributing to the public pension insurance is compulsory for mini-job employers, irrespective of the employee's enrollment choice. Employers contribute at a fixed contribution rate τ_{er} of 0.15 for our sample period.[3] Note that employers always contribute the mandated τ_{er}, so there are no financial incentives for them to encourage or discourage their employees' enrollment. Since they pay contributions to the pension insurance anyways, we argue that administrative costs are also negligible for them. Furthermore, opting in or opting out only requires the employer to sign the respective one-page form (see Figure A.5 and A.6), imposing negligible compliance cost on them.

Individual Enrollment Choice For mini-job employees, enrollment in the statutory pension insurance is optional. They face a purely binary choice menu regarding their enrollment. They can either contribute a given τ_{ee} or not contribute at all.

[3]This is a higher contribution rate than for regular employment, where employer and employee each contribute at the same contribution rate, e.g. 0.0945 in 2013 (see Table A.1). If employees are enrolled, the total contribution rate (employer + employee) is the same as under regular employment.

The employees' contribution rate is much smaller than the employers', with $\tau_{ee} = 0.039$ and $\tau_{er} = 0.15$ in 2013. If an individual enrolls, their total contribution rate is $\tau_{er} + \tau_{ee} = 0.189$ of their gross income as compared to $\tau_{er} = 0.15$ if not enrolled (values for 2013, see Table A.1 for changes over time).

Default Mini-job employees are not required to make an active enrollment choice. As long as they do not actively choose to do the opposite, they will remain at the default enrollment status which is defined by the German government for all mini-jobs. Prior to 2013, the default was no enrollment and mini-job employees had to actively opt in for enrollment. With the 2013 reform, the default changed to automatic enrollment. Ever since, mini-job employees have to actively opt out of enrollment.

Reform The reform came into effect on January 1, 2013, after the respective law passed the German parliament on December 5, 2012. The new default with automatic enrollment applies to all mini-job employees whose employment starts in 2013 or later. Employees whose mini-job starts pre reform (2012 or earlier) remain under the old opt-in regime post reform. However, the new default applies to them if their income surpasses the former income threshold of 400 € or if they take up a new mini-job. As a second feature of the reform, the allowed income threshold for mini-jobs increased from 400 € to 450 € per month from 2013 onwards. This second feature attracted much public attention, while the change in default setting, key feature for our analysis, was perceived as a minor change.

It is important to note that the actual enrollment options are not affected by the reform and that there was also no change in incentives, neither for employees nor for employers. This makes the reform particularly interesting and suitable for investigating the impact of default setting on individual retirement savings behavior for very low income earners who face a simple choice menu.

Costs of Enrollment Enrollment comes at the monetary cost of contribution payments that are deducted from wage earnings and depend on the mini-job employee's income y_t and the respective τ_{ee} in period t. For instance, an employee with a monthly income of 450 € in 2013 faces monthly costs of 450 €$\times 0.039$ =17.50 € when contributing and 0 € else. There is a minimum assessment base y_{min} that ensures a minimum absolute contribution if enrolled. No matter how low their monthly income, the absolute contribution for enrolled individuals can never fall below $(\tau er + \tau ee) \times y_{min}$. For monthly income $y < y_{min}$, enrolled employees have to top up their regular contribution until this minimum absolute contribution is reached. In subsection 4.3,

we investigate heterogeneity in enrollment behavior related to increased enrollment costs for this group.

In addition to monetary costs, there could be non-monetary costs for enrolling under the opt-in regime, i.e. time costs for filling in the required form. Since opting in requires filling in a one-page form with easily accessible information only, we argue that compliance costs are negligible (see Figure A.5 for an example). opting out under automatic enrollment is equally simple (Figure A.6 shows an opt-out form).

Incentives for Enrollment There are two incentives for enrolling in the statutory public pension insurance. First, individuals acquire more earning-points and thus increase their future pension entitlements on the intensive margin when enrolling in the public pension insurance. Second, mini-job employees increase their insurance record when enrolling, which counts towards their waiting periods that may be decisive for public pension eligibility and thus pension entitlements on the extensive margin.

Pension Points The number of acquired earning-points for a given employment period is defined as

$$EP = \begin{cases} \frac{y}{Y} \text{ if enrolled} \\ \frac{y}{Y} \times \frac{\tau_{er}}{\tau_{ee}+\tau_{er}} \text{ if not enrolled,} \end{cases}$$

where y denotes the individual gross income for the employment period and Y denotes the average annual income for the respective year as defined by the pension insurance (see Table A.1 for details). Enrollment increases EP for the employment period by $\frac{\tau_{ee}}{\tau_{er}+\tau_{ee}}$, which is equivalent to an increase of 26 percent in 2013. This first incentive increases future pension entitlements on the intensive margin and is thus relevant for individuals who expect to be eligible for a pension in the future. By enrolling, they increase their pension entitlements at the intensive margin.

Waiting Periods While the earnings points determine the pension level, the pension eligibility depends on the individual waiting period. When enrolled, months employed in a mini-job are fully credited, i.e. one month of mini-job employment is equivalent to one additional month for the waiting period. Without enrollment, the credited waiting period depends on the income and is determined by $\frac{EP}{0.0313}$. This second incentive can increase future pension entitlements at the extensive margin for individuals below a relevant waiting 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 waiting period 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.[4]

We illustrate the two different enrollment incentives with an exemplary mini-job employee in subsection A.2. In subsection 5.2, we analyze the individual enrollment behavior with respect to these (dis-)incentives and find that individuals (at least partly) react to those thresholds when deciding about their contributions.

3 Data

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 data sets below.

3.1 VSKT

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 data set with information for the entire employment biography of a subsample of the universe of insurees born between 1949 and 2001. Our main variable of interest records the insuree's social insurance status for every month of their working life, including not only enrollment through mini-job employment but also other periods, for instance periods of regular employment, unemployment or parental leave. We analyze the enrollment behavior for mini-job employees for whom their mini-job is their main employment. A set of demographic characteristics, such as date of birth, gender, citizenship or region of living is available for the day of sampling, December 31, 2016. The same applies for the statistical weights that allow for drawing conclusions from the sample for the entire population.

Sample We restrict the sample to individuals for whom a mini-job is the main employment for at least one month between January 2011 and November 2016. We exclude mini-job observations if the reported average monthly income exceeds the

[4] In the PAYG system, the sum of the monthly payment depends on the income. If they earned the average income during the 4 years and 11 months, they acquired 4.9167 EP which is equivalent to a monthly payment of 138.36 € in 2013.

Table 1: Sample Characteristics

	$m = 1$	$m = 3$	$m = 6$	$m = 12$
Female	0.61	0.63	0.66	0.69
Age	32	33	34	37
West Germany	0.85	0.86	0.86	0.87
German citizenship	0.79	0.81	0.82	0.83
Observations	337,109	201,808	119,554	57,353
Weighted	25,621,597	15,575,701	9,369,103	4,581,213

Notes: Number of new mini-jobs in the period 01/2011– 11/2016. 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. *Female*, *West Germany* and *German citizenship*: share of mini-jobs with this attribute. *Age*: mean age for a mini-job observation.

applying upper bound threshold $\bar{y}$ for mini-job income.[5] Until 2012, $\bar{y} = 400$€, before it increases to 450€ from 2013 onwards.

Furthermore, 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: all mini-jobs that started in or after 2013 are affected by the reform and therefore subject to the new default of automatic enrollment. Since the pension data stems from annual employer spells, the recorded starting date for a mini-job in year t never dates before January 1 of year t. 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.[6] We use this sample for our main analysis in section 4.

Table 1 provides sample characteristics for the final sample in the first, third,

[5]The data does not contain information on the exact monthly income but the total income for an observed employment period of x months. For the average monthly income, we divide the total income by x. Mini-job income is allowed to exceed $\bar{y}$ up to 3 times per year if the annual mini-job income does not exceed $12 \times \bar{y}$. The data does not allow for disentangling these cases from reporting errors which is why we exclude those observations.

[6]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.

sixth, and twelfth month of employment, denoted by $m = 1, 3, 6, 12$. Our final sample comprises 337,109 monthly observations for $m = 1$, representing more than 25 million mini-jobs over the sample period when applying statistical weights. The number of observations decreases significantly with increasing m, because of the short average duration of mini-job employments.

3.2 SOEP-RV

Dataset The SOEP-RV data links the largest German household survey (GSOEP) with administrative data from the VSKT dataset (Goebel et al. 2022). Record linkage between the two datasets is available for GSOEP respondents who agreed to the linkage in 2018 or 2020.[7] The linked sample has all information available from the GSOEP 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 GSOEP 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 the code plan). Most relevant, it covers the enrollment status for mini-job employees. While the VSKT is a monthly panel, the GSOEP is an annual panel. When using the SOEP-RV data we thus often 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 4,555 individuals who start at least one mini-job between 2005 and 2020.[8] The final sample sizes we use for the analyses with the SOEP-RV data depend on the specific variables we use. Many questions in the GSOEP are only asked in some years, limiting the respective sample size. We report the sample sizes and descriptive statistics for the different SOEP-RV subsamples in Table 2.

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

[8] We study a longer period than with the VSKT data, because the different structure of the SOEP-RV dataset allow for identifying mini-jobs for a longer period. For the period we study in our main analysis, January 2011 to December 2016, the SOEP-RV sample size is 763.

Table 2: Sample Characteristics SOEP-RV subsamples

	Sample size		Characteristics $m = 1$			
	i	$m = 1$	age	female	West	German
A. SOEP-RV samples						
full mini-job sample	4,555	10,245	34	0.63	0.81	0.94
01/2011-12/2016	763	4,097	33	0.63	0.82	0.93
financial literacy	841	2,285	36	0.75	0.86	0.94
liquidity constraints	1,210	5,346	35	0.66	0.81	0.95
B. Mini-jobs VSKT[a]	–	337,109	32	0.61	0.85	0.79

Notes: Data from SOEP-RV. i refers to individuals while $m = 1$ refers to the first month of a new mini-jobs. Since individuals can have more than one job over the sample period, the sample for $m = 1$ is larger than the sample for i. Characteristics are mean values for $m = 1$.
[a] Values for $m = 1$ from Table 1.

4 Estimating the Impact 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 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 an individual i that starts 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 individual i's enrollment choice at four different points in time, months $m = 1$, 3, 6

and 12:

$$E_{it}^{m} = \alpha + \gamma\, t + \beta D_t + \eta_{it} \tag{1}$$

We refer to the effect of the default on enrollment in the first month of employment ($m = 1$) as *instantaneous 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 4.2. We use ordinary least squares (OLS) to estimate the full model, including X_{Jit} and allowing for different time trends:

$$E_{it}^{m} = \alpha + \gamma_{pre}\, t\, \mathbb{1}\left(t < 01/2013\right) + \gamma_{post}\, t\, \mathbb{1}\left(t \geq 01/2013\right) + \beta D_t + \sum_{J} \delta_J X_{Jit} + \eta_{it}\,. \tag{2}$$

We replicate the analysis with non-linear Logit regressions, see subsection 4.4. The results are very similar, with the marginal effects from the Logit regressions being close to the OLS estimates.

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.2, including age, gender, nationality, state of residence and past experience with mini-jobs. There is no evidence for discontinuities for these characteristics. In addition, panel f of Figure A.2 shows that there is also no discontinuity in the estimated error term $\hat{\eta}_{it}$ from Equation 2. We have no reason to suspect a discontinuity for any unobserved characteristics.

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; Lee 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.1 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

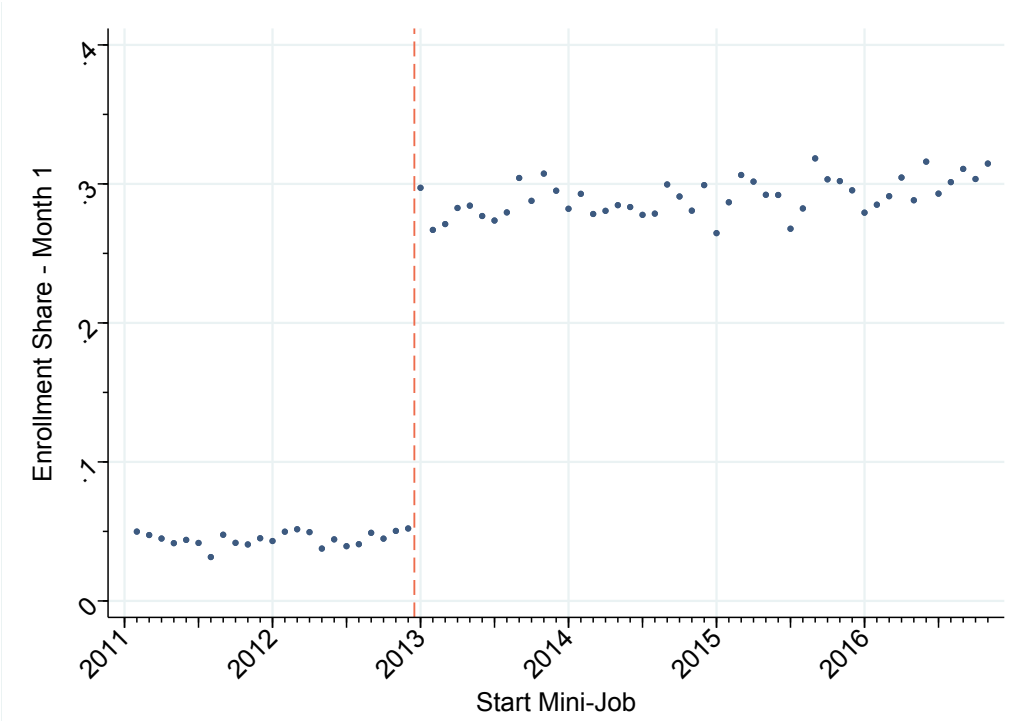
Instantaneous Effect Figure 1 provides graphical evidence that introducing an opt-out regime has a positive effect on enrollment. The graph plots the unconditional enrollment share in the first month of a new mini-job ($m = 1$). Under the opt-in regime pre reform, the enrollment share is about 5 percent for all starting months of employment. Starting with automatic enrollment in January 2013, the enrollment share jumps to a share of about 30 percent and stabilizes at that level for all post-reform months with a slight increase over time.

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 3 reports the results from estimating the models specified in equation Equation 1 and 2 and confirms the graphical evidence from Figure 1. Automatic enrollment significantly increases the enrollment share by about 23 percentage points in the first month of employment. Both magnitude and significance remain unchanged over different specifications reported in columns (1) to (4).

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 3 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 our the 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

Figure 1: Enrollment Share – Unconditional Means



Notes: Weighted data. Scatter plot displays 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 corresponding absolute numbers are shown in Figure A.1.

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). While the starting date is still observable for those who started their employment in the same calendar year (group 1), we lose this information for everyone else, because we cannot distinguish individuals who remained in the same job over the turn of the year from those who started a new mini-job at the beginning of the new year. For employment periods over the turn of a year pre or post reform, we know the default but there remains uncertainty about the precise length of their current employment (group 2). For individuals whose employment period includes the turn of the year 2012/2013, we can no longer determine the default (group 3).

Figure 2 shows a clear positive medium-term effect of auto-enrollment: over 20

Table 3: Instantaneous Effect of Automatic Enrollment

	(1)	(1a)	(1b)	(2)
D_t	0.2301***	0.2328***	0.2244***	0.2272***
	(0.0031)	(0.0031)	(0.0047)	(0.0047)
t	0.0005***	0.0004***		
	(0.0001)	(0.0001)		
t_{pre}			0.0001	0.0001
			(0.0001)	(0.0001)
t_{post}			0.0005***	0.0004***
			(0.0001)	(0.0001)
cons	0.0385***	0.0477***	0.0426***	0.0517***
	(0.0014)	(0.0027)	(0.0020)	(0.0031)
X_{Jit}	No	Yes	No	Yes
R2	0.0794	0.1093	0.0794	0.1094
N	337109	333707	337109	333707

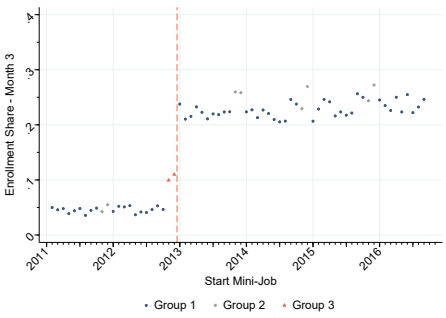
Notes: Effect on the enrollment E_{it} of individual i in the first month of their mini-job in month t. Coefficients from the OLS estimation of Equation 1 to 2, robust standard errors in parentheses. Weighted data. Significance level: *** 0.001; ** 0.01; * 0.05; ^{x} 0.1. t: month of observation, 1/2011 = 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. *cons*: constant from the OLS estimation. X_{Jit}: vector of individual control variables. Controls include age, age squared, gender, citizenship, east/west and experience with mini-job employment in the past. See Figure A.2 for mean values for the control variables over time. Table A.3 replicates the results using Logit.

percent of individuals that are employed in a mini-job for at least 3, 6 or 12 months (panel a, b and c, respectively), are enrolled for all m and over the entire post-reform period. This is in line with some individuals being partly inert and thus opting out only with some delay. Pre-reform enrollment shares increase with m, indicating that those who are employed in a mini-job for a longer time are more likely to actively opt in if not enrolled automatically.

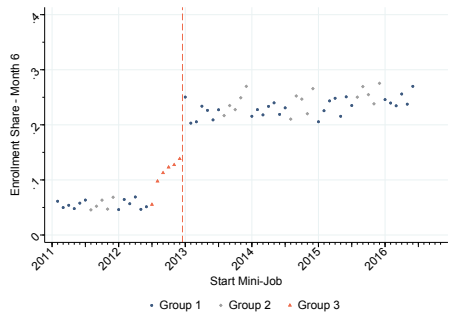
Figure 2 also documents a clear increase in enrollment for some months right before the reform date and the number of months is increasing with m. To be precise, there is an increased enrollment share if mini-jobs that started in this pre-reform month are observed post reform after m months (group 3). For $m = 3$, this concerns the last 2 months of 2012, for $m = 6$ the last 5 months and for $m = 12$ all months in 2012 except for January. As described above, we cannot distinguish

Figure 2: Enrollment Share in the Medium Run – Unconditional Means

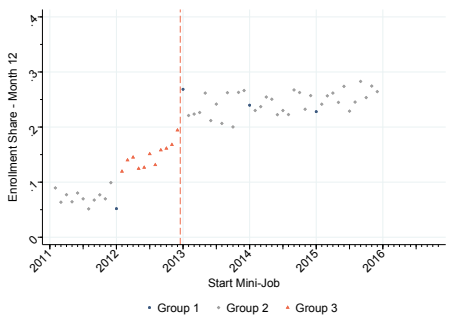
(a) m = 3



(b) m = 6



(c) m = 12



Notes: Weighted data. Scatter plots display 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. *Group 1*: no uncertainty about the default or the starting date of the employment. *Group 2*: no uncertainty about the default but uncertainty about the starting date. *Group 3*: uncertainty about both the default and the starting date. In each panel, a marker 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 only, is only considered for Panel a. The corresponding absolute numbers are shown in Figure A.1

between individuals who remained in their old mini-job under the old default and those who started a new mini-job under automatic enrollment on January 1, 2013. The increased enrollment shares pre reform are in line with some individuals starting a new mini-job under the new default (see subsection 4.4 for more details).

We exclude group 3 for estimating the medium-term impact of automatic enrollment because we do not know their default with certainty. We deviate from the standard RD design here because the discontinuity does no longer occur between

adjacent months.

Table 4: Medium-Term Effects of Automatic Enrollment

	m = 3		m = 6		m = 12	
D_t	0.1648***	0.1667***	0.1484***	0.1483***	0.1344***	0.1359***
	(0.0061)	(0.0059)	(0.0086)	(0.0083)	(0.0150)	(0.0145)
t_{pre}	0.0001	0.0000	0.0001	0.0001	-0.0006	-0.0001
	(0.0002)	(0.0002)	(0.0004)	(0.0004)	(0.0010)	(0.0010)
t_{post}	0.0005***	0.0004***	0.0007***	0.0007***	0.0008**	0.0008**
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0003)	(0.0003)
cons	0.0448***	0.0272***	0.0543***	0.0244***	0.0758***	0.0266**
	(0.0027)	(0.0038)	(0.0042)	(0.0055)	(0.0085)	(0.0103)
X_{Jit}	No	Yes	No	Yes	No	Yes
R2	0.0531	0.1187	0.0462	0.1191	0.0356	0.1187
N	196696	194893	110173	109306	47297	46923

Notes: Effect on the enrollment E_{it}^m of individual i in the m^{th} month of their mini-job in month t. Only observations without uncertainty about the default (excluding group 3 from Figure 2). Coefficients from the OLS regression specified in Equation 2 with (right columns) and without individual characteristics (left columns). Robust standard errors in parentheses. Weighted data. Significance level: *** 0.001; ** 0.01; * 0.05; ^{x} 0.1. t: month of observation, 1/2011 = 1. D_t: Dummy for the default, $D_t = 1$ under auto-enrollment and 0 else. *cons*: constant from the OLS estimation. X_{Jit}: vector of individual control variables. Controls include age, age squared, gender, citizenship, east/west and experience with mini-job employment in the past. Intuitively, the sample size N decreases with increasing m because not all individuals remain 3, 6 or 12 months in their mini-job employment and we exclude an increasing number of months with uncertain default information (group 3 from Figure 2). See Table A.4 for the same table without excluding group 3. Table A.5 replicates the results using Logit.

Table 4 provides the results from the estimation of Equation 2 for $m = 3$, 6, and 12 and confirms the graphical evidence in Figure 2: automatic enrollment significantly increases enrollment shares in the medium run, but relative to the first month, the effect decreases over time, to about 14 percentage points after 12 months. Again, including individual characteristics (right columns) does not change magnitude or significance of the effects. Table A.4 provides results from estimating the medium-term effect including all observations.

There are two explanations for the difference between the instantaneous and medium-run effect. First, inertia delays the response and individuals take some time to deviate from the default. Using within-subject variation in the default, we investigate the prevalence of such partly inert behavioral types in subsection 5.1 and

find that between 5 and 8 percent of individuals are either delayed never takers or delayed always takers who take some time to deviate from the default. A second explanation is attrition. Individuals who remain longer in the sample because they have longer employment periods are more likely to make an active choice and deviate from the default, both pre and post reform. With increasing m, short-time employees drop out of the sample and longer-term employees account for larger parts of the sample which mechanically increases the enrollment share over m.

4.3 Heterogeneous Effects of the Default

We now analyze the effect of the default in more detail and investigate heterogeneity in the impact of automatic enrollment. To account for potentially heterogeneous effects, we interact the default dummy D_t with individual characteristics X_{Jit}, thereby allowing the treatment effect of the default to be heterogeneous. Adding the interaction terms $D_t \times X_{Jit}$ to Equation 2 yields:

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

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

Table 5 displays the results from estimating Equation 3. For each characteristic J, the table displays the estimated coefficient $\hat{\delta}_J$ as well as the coefficient from the interaction with the default $\hat{\zeta}_J$. The results show that there is significant heterogeneity in the impact of the default across demographic groups, while the overall effect of the default is persistent when allowing for heterogeneous effects.

Demographic Characteristics Understanding the interplay between automatic enrollment and different demographic characteristics is of great relevance for policy makers, since it allows for understanding the impact of the default for different groups of the population. For example, policy makers might be particularly interested in the default's impact on enrollment for women, who constitute the vast majority of mini-job employees with longer periods of mini-job employment (Table 1), while at the same time facing a substantial gender pension gap and an increased old-age poverty rate (OECD 2019).

Table 5: Heterogeneous Effects of Automatic Enrollment

	(1)	(2)	(3)
D_t	0.2640***	0.2546***	0.3235***
	(0.0061)	(0.0061)	(0.0061)
t_{pre}	0.0002	0.0001	0.0001
	(0.0001)	(0.0001)	(0.0001)
t_{post}	0.0006***	0.0006***	0.0004***
	(0.0001)	(0.0001)	(0.0001)
Age	0.0020***	0.0016***	0.0015***
	(0.0001)	(0.0001)	(0.0001)
Age $\times D_t$	-0.0016***	-0.0008***	-0.0009***
	(0.0001)	(0.0001)	(0.0001)
Female	0.0324***	0.0260***	0.0258***
	(0.0015)	(0.0015)	(0.0015)
Female $\times D_t$	0.0006	0.0110***	0.0081**
	(0.0028)	(0.0029)	(0.0028)
Non-German	-0.0287***	-0.0253***	-0.0266***
	(0.0013)	(0.0013)	(0.0013)
Non-German $\times D_t$	0.0202***	0.0137***	0.0112***
	(0.0025)	(0.0026)	(0.0025)
East	-0.0027	0.0031	0.0065*
	(0.0026)	(0.0026)	(0.0026)
East $\times D_t$	0.0370***	0.0274***	0.0320***
	(0.0045)	(0.0045)	(0.0044)
Experience		0.0006***	0.0006***
		(0.0001)	(0.0001)
Experience $\times D_t$		-0.0010***	-0.0011***
		(0.0001)	(0.0001)
HigherCost			-0.0422***
			(0.0016)
HigherCost $\times D_t$			-0.1418***
			(0.0027)
cons	-0.0363***	-0.0325***	-0.0142***
	(0.0030)	(0.0030)	(0.0029)
r2	0.0836	0.0847	0.1171
N	333707	333707	333707

Notes: Effect on the enrollment of individual i in the first month of their mini-job in month t. Coefficients from the regression specified in Equation 3, robust standard errors in parentheses. Weighted data. Significance level: *** 0.001; ** 0.01; * 0.05; ^{x} 0.1. t: month of observation, 1/2011 = 1. D_t: Dummy for the default, $D_t = 1$ under auto-enrollment and 0 else. *Age*: Age of individual i at the time of observation. $Female$: Dummy for the gender of i, $Female = 1$ if female and 0 else. $Non\text{-}German$: Dummy for the citizenship of i, $Non\text{-}German = 1$ for individuals without the German citizenship and 0 else. $East$: Dummy for the state of residence of i, $East = 1$ for East Germany and 0 else. *Experience*: Number of months with mini-job employment in the past. *HigherCost*: Dummy for very low-income employments with higher relative enrollment costs. *cons*: constant.

The administrative data contains a limited set of individual demographics (age, gender,[9] citizenship, region) and Table 5 documents substantial heterogeneity in the response to the default for different demographic groups along these dimensions. The negative coefficient for the interaction term ζ_{Age} implies that the effect of automatic enrollment ($D_t = 1$) decreases with age. In terms of magnitude, being one year older dampens the effect of $D_t = 1$ by about 0.1 percentage points. The estimate for δ_{Age} however is positive, indicating that age is associated with higher enrollment for $D_t = 0$. The opposite pattern emerges for non-German mini-job employees. Compared to German employees, they are significantly less likely to enroll at the baseline but the effect of the default is larger for them. There are also systematic differences in the enrollment behavior of women and men. Women are in general more likely to enroll and automatic enrollment increases their enrollment more than for men (in absolute terms). The effect of the default is also higher for individuals living in East Germany.

Illustrating the Effect Heterogeneity To better understand the heterogeneity, we illustrate the effect of the default for two different hypothetical mini-job employees, person A and B. Person A is a long term mini-job employee, a 50 year old woman with 10 years of experience as a mini-job employee. Person B is a 25 year old man, who has never before worked in a mini-job. Both are in the first month of a new mini-job, live in West Germany, are German citizens, and have an income above the minimum assessment threshold y_{min}. We predict their enrollment just before and after the reform, for December 2012 and January 2013, based on the specification of Equation 3 with all characteristics as described in column 3 of Table 5. The predicted enrollment probability pre reform is 0.161 for A and 0.026 for B. post reform, the predicted enrollment probability rises to 0.323 for A and 0.319 for B. A has a higher enrollment probability pre reform, but the effect of the default is stronger for B, both in absolute and in relative terms.

Suggestive Evidence: Role of Financial Literacy The heterogeneous effects reported in Table 5 show a stronger default effect for women and for employees in East Germany, both demographic groups that have been shown to have lower financial literacy (Bucher-Koenen and Lusardi 2011; Bucher-Koenen, Lusardi, Alessie, and Rooij 2017).Furthermore, the default effect is also larger for non-German citizens, a group whose specific knowledge of German social insurance is arguably lower and who may in addition also face language barriers. In addition, column 2 of Table 5 shows

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

that the default effect decreases with more past experience with mini-jobs. Being exposed to the mini-job system for a longer period is likely to increase institutional knowledge. These results suggest that the the default is more powerful for those with lower levels of financial literacy. We use linked survey data to study the impact of financial literacy in subsection 5.3 further.

Financial Incentives 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_{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 5 and show that their enrollment share is much lower compared to individuals who face the regular τ_{ee}. The estimated interaction effect is by far the most sizable, its absolute value amounts to almost half of the effect of D_t. This implies that individuals are more likely to deviate from the auto-enrollment default if facing larger financial incentives.

4.4 Corroborating Evidence and Robustness Checks

Unknown Default When we track the enrollment behavior of individuals for longer than the first month of their mini-job, we lose information whenever the time period contains the turn of a year. Figure 2 shows that enrollment behavior changes for those for whom the default is unknown. One explanation is that a sizable share of these individuals actually starts a new job with the beginning of the new year and is thus automatically enrolled if the starting point is in the post-reform period.

With a simple back-of-the-envelope calculation, we can quantify the share of mini-jobs employees that would have to start a new mini-job on January 1, 2013, in order to explain the observed pattern. For $m = 3$, we see an average enrollment share of about 5 percent pre reform and about 23 percent post reform. For those who started their mini-jobs in December 2012, the enrollment share in the third month (February 2013) is about 9 percent. Assuming that this increase is only driven by a share x of individuals who started a new mini-job under auto-enrollment on January 1 2013, we need $x \cdot 0.23 + (1 - x) \cdot 0.05 = 0.09$. Solving for x leads to $x = 0.22$. Thus, if 22 percent of these individuals started a new mini-job in January 2013, this would fully explain the increased enrollment share of 9 percent that we observe for this group in February 2013.

To assess the plausibility of $x = 0.22$, we use additional data from the central agency that is handling administration for mini-jobs (*Minijob-Zentrale*).[10] We use the quarterly data from March 31, 2013 and look at the subsample of individuals that have started a mini-job in the past 180 days.[11] Out of these mini-jobs, the share of those that started within the past 90 days, thus post reform, is 77 percent. Having $x = 0.22$ for our subsample in February 2013 thus seems to be within a plausible range. Thus, we conclude that the observed patterns for the medium term are likely driven by individuals who actually started a new mini-job in January 2013, which is not observable in the data.

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 a higher income threshold increases the probability of enrollment. Two potential mechanisms are possible, but we provide suggestive evidence that none of them is at play.

First, individuals might respond to the increased income threshold by adjusting their labor supply at the extensive margin. In particular, individuals with a monthly reservation wage between 400 € and 450 € will supply labor after the reform, but not before. If those individuals are more likely to enroll (irrespective of the default), this would increase the post-reform enrollment share. If extensive margin responses were driving the observed enrollment patterns, we would expect to see a sizable increase in the number of mini-jobs after the reform. Figure A.1 documents that this is not the case. Compared to 2012, the monthly numbers of new mini-jobs is only somewhat higher in 2013 and 2014 and then decreases again in 2015 and 2016.

Second, labor supply at the intensive margin, coupled with liquidity constraints, could potentially lead to increased enrollment post reform. Assume that the 400 € threshold is binding for the labor supply of individual i, i.e., they would like to work more in their mini-job. Let's further assume that with a monthly income of 400 €, liquidity constraints hinder i from enrollment. In this case, the increased income threshold will increase i's labor supply post reform, potentially lifting liquidity constraints, and as a result, i may now enroll. In this scenario, i does not enroll because of the change in default but because they are no longer liquidity constrained.

Figure A.3 shows that the income distribution for mini-jobs peaks at 400 € pre

[10] The data is not publicly available and was provided upon request.

[11] The data provides information on the mini-job tenure on a quarterly level. Tenure is categorized in multiples of 90 days (1– 90 days, 91– 180 days and so on) that do not necessarily coincide with the turn of a month and the information refer to all mini-job employees, while we are focusing on a specific subsample in our analysis.

reform, but there is also substantial mass below that threshold. This suggests that the threshold is not binding for most employees. Furthermore, many mini-job employees still have a monthly income of 400 € post reform, as indicated by the persistence of the peak at 400 € for post-reform years.[12] To provide further evidence, we rerun the main analysis for a subsample of mini-jobs with monthly income ≤ 400 € post reform. The estimated effects are of comparable size as shown in Figure A.4 and Table A.6. We thus argue that the increased income threshold cannot explain the increased enrollment after the reform and does not constitute a threat for identification.

Logistic Regression In addition to the OLS estimation in the main analysis, we replicate our findings using logistic regression estimations (Logit). Table A.3 shows the corresponding marginal effects for the instantaneous effect ($m = 1$). The results are very similar to those from the OLS estimation as provided in Table 3. The estimated marginal effect of automatic enrollment is 0.24 compared to 0.23 in the OLS estimation. Table A.5 shows that for the medium term ($m = 3, 6, 12$), marginal effects estimated from Logit are somewhat larger than those from OLS as shown in Table 4. All effects remain highly significant at the 0.001 level.

5 Understanding Individual Behavior

We find that, even though there is a positive and sizable effect of automatic enrollment on pension contribution, only a minority of individuals stick to the default while the majority opt out from enrollment immediately. This is in contrast to findings from the existing literature, that typically documents enrollment for the majority of individuals who are enrolled automatically (e.g., Blumenstock, Callen, and Ghani 2018; Cribb and Emmerson 2020; Madrian and Shea 2001). To better understand the comparably low enrollment rates as well as the underlying heterogeneity, we study individual enrollment behavior in more detail.

5.1 Behavioral Types of Enrollment

When assessing the impact of default setting or other retirement savings policies, individuals are often classified into active and passive savers. This terminology

[12] 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-€-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.

has been heavily influenced by Chetty, Friedman, Leth-Petersen, Nielsen, and Olsen (2014). Following their terminology, active savers are typically defined as individuals whose behavior is in line with standard economic models and who react to changes in savings incentives (e.g., subsidies for pension contributions). Active savers are not expected to change their savings behavior in response to a change in the default as long as incentives remain unchanged. Passive savers on the other hand stick to the default and do not react to changes in savings incentives.

Chetty, Friedman, Leth-Petersen, Nielsen, and Olsen (2014) find that the vast majority (85 percent) of the Danish population can be described as passive savers, who do not respond to savings subsidies and who save more (less) when the default contribution rates increase (decrease). Only 15 percent in their setting are active savers, reacting to subsidies by shifting savings to the subsidized accounts and setting off changes in automatic contributions by adapting their contributions in other accounts. More recent research has shown that individual behavior is not always consistently active or passive, but may change depending on the setting (see e.g., Butt, Donald, Foster, Thorp, and Warren 2018; Goda, Levy, Manchester, Sojourner, and Tasoff 2020; Goodman 2020).

Definition Behavioral Types To examine the prevalence of active and passive saving behavior in the simple choice framework for mini-jobs, we exploit the panel structure of the administrative data set by comparing the enrollment behavior of individuals observed under both defaults. We define a total of 6 behavioral types, as shown in Table 6.

Never takers never enroll, neither under the opt-in nor under the opt-out regime. They always stick to the default when the default is no enrollment and always opt out when they are enrolled by default. For never takers, there is no inertia under the new default, neither inattention nor switching costs prevent them from opting out. *Always takers* on the other hand always enroll irrespective of the default. For both never and always takers, the default does not affect their enrollment status, i.e., they do not contribute more under automatic enrollment. Both groups can be described as active individuals who either actively chose to enroll (always takers) or not to enroll (never takers), but whose choice is not affected by the default setting.

The third group comprises individuals that always stick to the default: they do not enroll under the opt-in regime and they do not opt out when automatically enrolled. Again following the Chetty, Friedman, Leth-Petersen, Nielsen, and Olsen (2014) terminology, we refer to them as *passive* individuals. They exhibit perfect default stickiness under both the opt-in and the opt-out regime and their behavior can thus be described as true passive behavior. Their enrollment follows the

Table 6: Definition of Behavioral Types

		Opt-in Regime (Old Default)	Opt-out Regime (New Default)
Active	Never Taker	✗	✗
	Always Taker	✓	✓
Passive		✗	✓
Partly Inert	Delayed Never Taker	✗	✓→ ✗
	Delayed Always Taker	✗→ ✓	✓
Inconsistent		✓/✗	✓/ ✗

Notes: Definition of behavioral types based on the enrollment behavior of individuals that are observed under both defaults. ✓: enrollment for all months under the corresponding default. ✗: no enrollment for all months under the corresponding default. ✓→ ✗: enrollment for the first $x < n$ consequent months and no enrollment for the last $n - x$ months under the corresponding default. ✗→ ✓: no enrollment for the first $x < n$ consequent months and enrollment for the last $n - x$ months under the corresponding default. ✓/✗: none of the above patterns.

default-setting and a default-changing reform thus influences their future pension entitlements.

The remaining individuals do neither consequently pursue enrollment or no enrollment (active behavior), nor do they always stick to the default (passive behavior). If they stick to the default first but then deviate after some time, we refer to their behavior as *partly inert*. *Delayed never takers* never actively enroll in the pension insurance under the opt-in regime, stick to the default for the first $x < n$ months under auto-enrollment, then opt out and never enroll again for the remaining $n - x$ months. Analogously, we define *delayed always takers* as individuals who do not enroll for the first $x < n$ months under the opt-in regime, then start enrollment and continue enrollment for all future $n - x$ months pre reform and never opt out from automatic enrollment. There are different explanations for partly inert behavior like this. Mini-job employees may be inattentive regarding their default and/or the possibility to deviate from it and may only learn about it after x months. Alternatively, they may be attentive but other factors, like (perceived) switching costs prevent them from immediately engaging in active behavior. The remaining behavioral patterns are summarized as *inconsistent* behavior.

Sample In order to examine the taxonomy of individual behavior under different defaults, we restrict the sample to individuals that we observe at least once under

Table 7: Prevalence of Behavioral Types

	Case I	Case II
NeverTakers	62.43	67.04
AlwaysTakers	5.35	5.30
Passive	12.70	8.26
DelayedAlwaysTakers	3.70	1.44
DelayedNeverTakers	4.02	3.80
Inconsistent	11.81	14.15
Total	100.00	100.00
N	47,765	57,628

Notes: Sample: all individuals that are observed under both defaults for at least one month between 01/2011 and 11/2016. Weighted data. Case I and II account for the uncertainty of the default for some post-reform observations as described in the text. They mark the two most extreme cases and thus provide a corridor for the true value, which has to lie between the two extremes. See Table 6 for the definition of the behavioral types.

each default regime. As discussed in section 4, we cannot unambiguously determine the default for all post-reform observations. For mini-jobs that started pre reform, the old default applies post reform as long as a certain income threshold is not surpassed (see section 2 for details). However, in the data, those individuals are not distinguishable from individuals whose mini-job ended on December 31, 2012 and who started a new one, under the new default, on January 1, 2013. Consequently, the default is unknown for post-reform observations of these individuals.

We account for this uncertainty by showing the results for the two most extreme cases. For case I, we assume that all individuals remained in their mini-job and none of them started a new mini-job on January 1, 2013. For case II, we assume that all individuals in question started a new mini-job on January 1, 2013. With these two extreme cases we provide a corridor for the true values.

Results Table 7 indicates that the majority of individuals are never-takers, meaning they never enroll, whether under the opt-in or the opt-out regime. In both cases we consider, we observe that approximately two-thirds of individuals fall into this category. Therefore, the majority of individuals exhibit no inertia under the new default setting and opt out immediately after commencing their mini-job with auto-enrollment. Policy makers do not influence their enrollment behavior by altering

the default option. The default setting has a consistent effect solely on passive individuals, who constitute only a minority of individuals. As shown by Table 7, this purely passive behavior is exhibited by approximately 8 to 13 percent of individuals, i.e., transitioning from an opt-in to an opt-out regime nudges only about 1 in 10 individuals into enrollment. There are a few always takers and individuals whose behavior is in line with delayed opting in or opting out, but they only constitute a small minority.

The differences between case I and case II are driven by two factors. First, since we restrict the sample to individuals with at least one observation under each default, we consider different samples for the two cases. An individual for whom the actual default is unknown for all post-reform observations is considered for the old default in case I and for the new default in case II. In case I, this leads to no observations under the old default for this individual which is why we do not include them in the sample here. This explains the smaller sample size for case I. Second, the same behavior is classified differently under the two cases for some individuals. This can be illustrated with an individual for whom we observe three mini-job spells: One spell without enrollment pre reform, a second spell post reform without enrollment and under unknown default and a third spell post reform with enrollment under the new default. In case I, this is classified as passive behavior since the individual never enrolls under the old default (spell 1 and 2) but always under the new default (spell 3). In case II, the behavior is classified as inconsistent because the enrollment status under the new default (spell 2 and 3) is not consistent. However, while the precise shares differ between case I and II, the overall patterns remain unchanged.

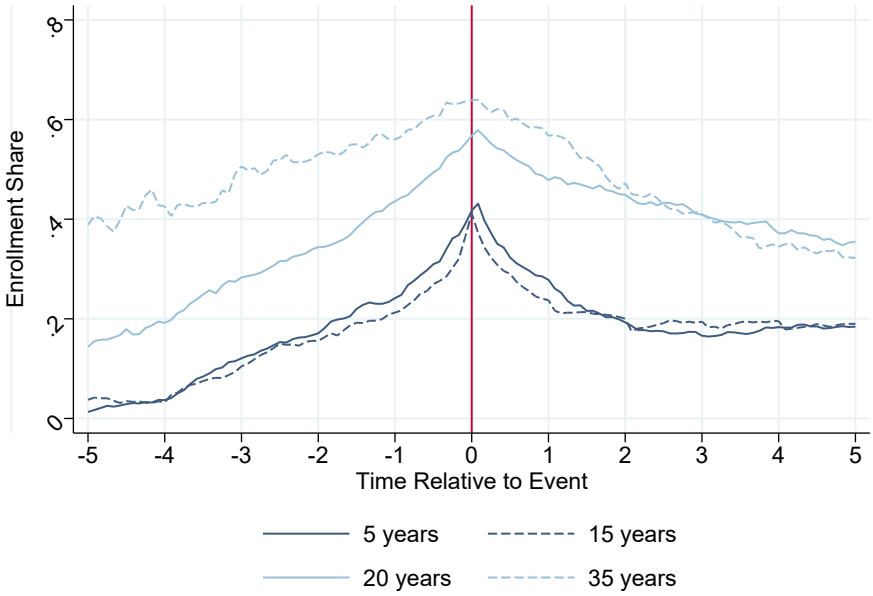
5.2 Responsiveness to Incentives

A key finding of our analysis is that most individuals opt out and we show that this is driven by a majority of never-takers. An open question is, why is the share of individuals influenced by automatic enrollment so small in this setting compared to previous results in the literature? One potential explanation is the simplicity of the setting: when facing a simple choice menu, costs and benefits from enrollment may be more salient for individuals. We provide suggestive evidence for this hypothesis by studying enrollment behavior of individuals who are close to reaching a threshold for waiting periods.

As described in section 2, periods of mini-job employment are fully credited as waiting period only if individuals enroll. Different amounts of these waiting periods are required to meet with eligibility thresholds for different types of pensions. For individuals close to a waiting period threshold, e.g., 35 years for early retirement

(see Table A.2 for more details), benefits from enrollment are larger, potentially being 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 as waiting periods 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.

Figure 3: Enrollment Relative to Waiting Period Thresholds



Notes: Weighted Data. *Event*: the month an individual reaches the threshold for the respective waiting periods. *Time Relative to Event*: position in the individual employment history relative to reaching the threshold. A value of 0 marks the month the given threshold is reached, -1 indicates that those individuals need one more year (12 more months) of contribution time before they reach the respective threshold. *Sample*: For each waiting period threshold (see Table A.2 for details), the sample includes all individuals that have reached that threshold already. For each line, N is thus constant for periods ≤ 0 but decreasing for periods > 0, because not all individuals are observed 5 years after they reached the threshold.

To assess whether mini-job employees understand and react to these incentives, we compare the average enrollment share for mini-job employees over time relative to the

event of reaching an eligibility threshold, e.g. for early retirement. For each threshold, we include all individuals that we eventually observe reaching the respective threshold in the data. The results are shown in Figure 3. For all thresholds, the enrollment share peaks at, or very close to, the threshold. The observed behavior is in line with individuals responding to the incentives for enrollment that are tied to the waiting 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. 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 rather complex 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.

5.3 Financial Literacy

It is well known that financial literacy plays an important role for economic decisions in general and old-age savings in particular, while, at the same time, significant parts of the population are not financially literate (e.g., Lusardi and Mitchell 2014). One potential explanation for the heterogeneous effects of the default on enrollment are differences in individuals' understanding of the default based on their financial literacy. The results from our heterogeneity analysis in subsection 4.3 already suggest that the default is more powerful for groups of individuals who have been shown to have low levels of financial literacy.

Table 8: Direct Measure for Financial Literacy

	Share of individuals
Always aware of own enrollment	0.76
Unaware: enrollment	0.12
Unaware: no enrollment	0.12
N	841

Individual-level data from SOEP-RV. Individuals that are always aware of their own enrollment are classified as having high financial literacy. If an individual is at least once unaware of the own enrollment, i.e. reports an enrollment status that deviates from the observed administrative records, they are classified as having low financial literacy.

Measuring Financial Literacy With Survey Data To directly assess the role of financial literacy for the power of the default, we link the administrative data with GSOEP survey data (see subsection 3.2 for data description). With the linked SOEP-RV data, we construct a direct measure of financial literacy by comparing survey respondents' beliefs about their enrollment status to the true enrollment status observed in the administrative records. Combining survey and administrative data allows us to assess the individual awareness of enrollment, a direct measure of context-specific financial literacy. If the beliefs deviate from the true enrollment at least once, we classify an individual as having low financial literacy. On the contrary, we refer to individuals as having high levels of financial literacy if their beliefs always match their true enrollment status. Since 2018, survey respondents are asked about their enrollment status in the public pension insurance if they report a mini-job. We treat financial literacy as a time-constant individual trait. This allows us to study the enrollment behavior of individuals with high and low levels of financial literacy for all periods observed in the administrative data.

Our sample consists of individuals who report a mini-job and answer the question on their enrollment status for at least one survey year in the GSOEP, while their administrative records show a mini-job employment for at least one month in that year.[13] 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.[14] Our final sample for this exercise consists of 858 individuals (see Table 8).

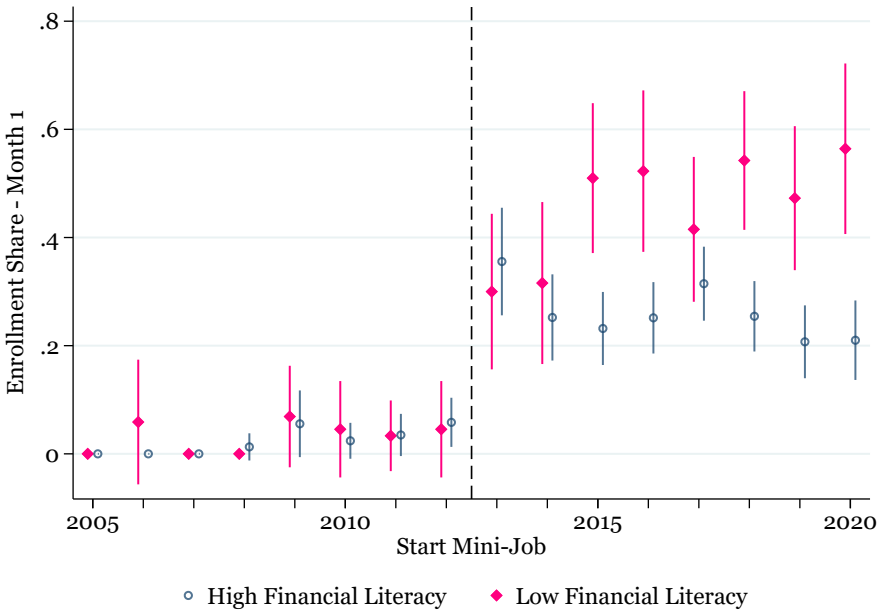
Results Table 8 shows that 24 percent of individuals have low financial literacy and are not always ware of their enrollment status. To assess the differential power of default setting for those with high and low levels of financial literacy, we first provide graphical evidence. Figure 4 plots the enrollment 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 financial literacy. Put differently, individuals are more likely to stick to the auto-enrollment default if their financial literacy is low.

We then support this graphical evidence by interacting the dummy for automatic

[13]We exclude cases where survey respondents report a mini-job, while this is not observed in the administrative records, as well as those who do not self-report a mini-job in the survey, although the administrative records show they have a mini-job.

[14]We do include those who work in a mini-job for less than twelve months though.

Figure 4: Enrollment Share by Financial Literacy



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 *Always aware* subgroup includes those whose belief about the own enrollment status maps the true enrollment observed in the administrative records, while the *Not always aware* group includes those who were unaware of their own enrollment at least once. Note that the financial literacy measure is defined on the individual level, i.e., constant over time if an individual is observed more than once.

enrollment with a dummy for low financial literacy:

$$E_{it}^{m} = \alpha + \gamma t + \beta D_t + \delta literacy_i + \zeta D_t \times literacy_i + \eta_{it} \tag{4}$$

The notation follows the notation for our main analysis, and $literacy_i$ is 1 if individual i has high financial literacy and 0 else. We estimate Equation 4 for the first month of employment ($m = 1$) and report the results in Table 9. This is reduced version of Equation 3, where we do not include the full set of characteristics because of the smaller sample size.

In line with the graphical evidence in Figure 4, Table 9 shows that the effect of automatic enrollment is significantly stronger for those with low financial literacy.

Table 9: Interaction Financial Literacy and Default

	Outcome: enrollment in month 1
D_t	0.2095***
	(0.0330)
Low Financial Literacy	0.0076
	(0.0148)
Interaction	0.1965***
	(0.0323)
t	0.0002
	(0.0003)
cons	0.0300**
	(0.0093)
R^2	0.1366
N	2,283

Effect on the enrollment of individual i in the first month of their mini-job in month t. Coefficients from the regression specified in Equation 4, robust standard errors in parentheses. Significance level: *** 0.001; ** 0.01; * 0.05; ^{x} 0.1. t: month of observation, 1/2011 = 1. 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 8.

In addition to the baseline default effect of 21 percentage points, the enrollment of those with low financial literacy increases by another 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 with low financial literacy.

5.4 Liquidity Constraints

When designing automatic enrollment policies, the focus is often on how to increase individual savings for retirement. However, while enrollment may be beneficial for some individuals, others may be better off when not enrolled. One factor here is financial insecurity: If individuals face liquidity constraints, saving more might not be optimal.

Measuring Liquidity Constraints With Survey Data We use the linked SOEP-RV survey data to measure liquidity constraints at the household level. We define a household as liquidity constrained if the household reports 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 between 2005 and 2020, as observed in the administrative records, and answer the question on their savings potential in the GSOEP in the same calendar year (we report sample characteristics in Table 2).

Results Liquidity constraints are common among mini-job employees. When starting a new mini-job, 50 percent of mini-job employees are liquidity constrained. However, this group is not more likely to opt out from automatic enrollment. Rather, Figure 5 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 48 percent of all cases.

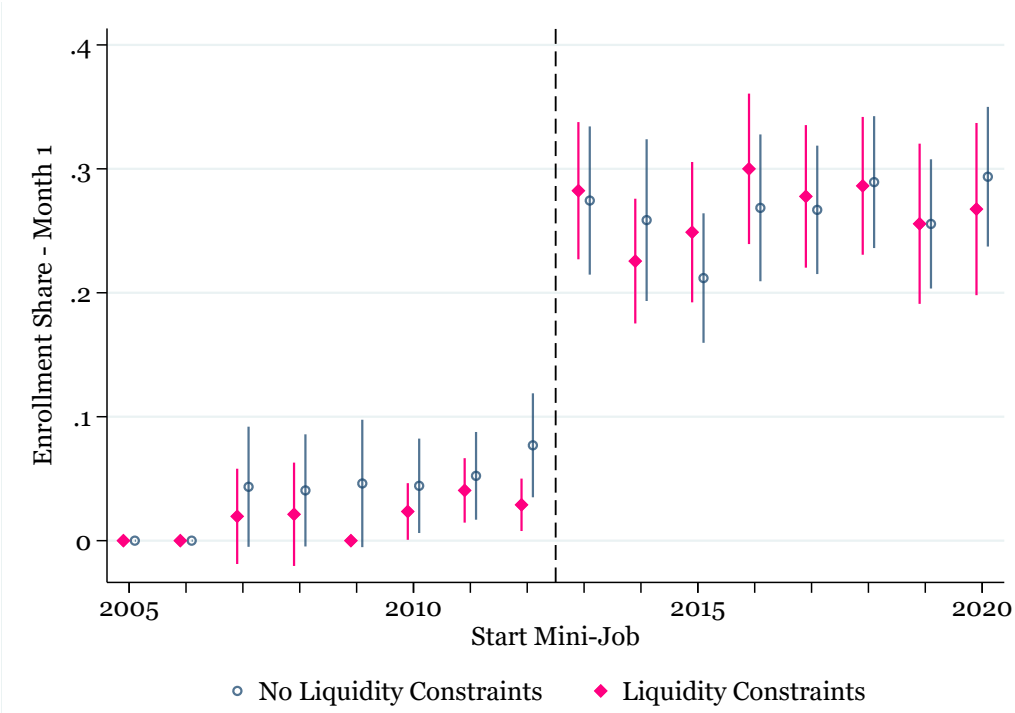
6 Conclusion

It is well established that individuals commonly stick to defaults, particularly in the context of pension enrollment. We show that the power of the default is limited in a pension setting where the choice menu is simple. In this simple setting, many of the explanations for inertia in more complex settings can be ruled out, including switching costs (Gabaix 2019; Heiss, McFadden, Winter, Wuppermann, and Zhou 2021), choice overload (Goda, Levy, Manchester, Sojourner, and Tasoff 2020; Iyengar and Lepper 2000), and (mis-)perceiving a default as investment advice from the employer (Madrian and Shea 2001).

We analyze a natural experiment of introducing automatic enrollment for low-income employees with a binary choice menu in the German PAYG pension system. The setting allows us to rule out the above explanations for inertia, leaving inattention as the main explanation. We find that the introduction of automatic enrollment significantly increases the take-up. However, compared to the existing literature on auto-enrollment, the effect of changing the regime from opt-in to opt-out is rather small, with the majority of individuals opting out immediately.

Only few individuals exhibit true passive behavior defined as always sticking to the default, while the majority is best described as never-takers who never enroll. In addition, a significant share of individuals seem to understand and react to enrollment incentives. Using automatic enrollment to nudge low income earners into higher public pension savings for their retirement may thus be less effective than policy makers may hope.

Figure 5: 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.

We also observe sizable heterogeneity in the effect of the default, which should be taken into account when designing public policies that aim at shaping individual behavior using default setting. Implementing automatic enrollment for everybody does not necessarily enhance future pension entitlements equally for all groups of individuals. And depending on the target group, automatic enrollment may even nudge the "wrong" individuals into enrollment, for example because they are less financially literate.

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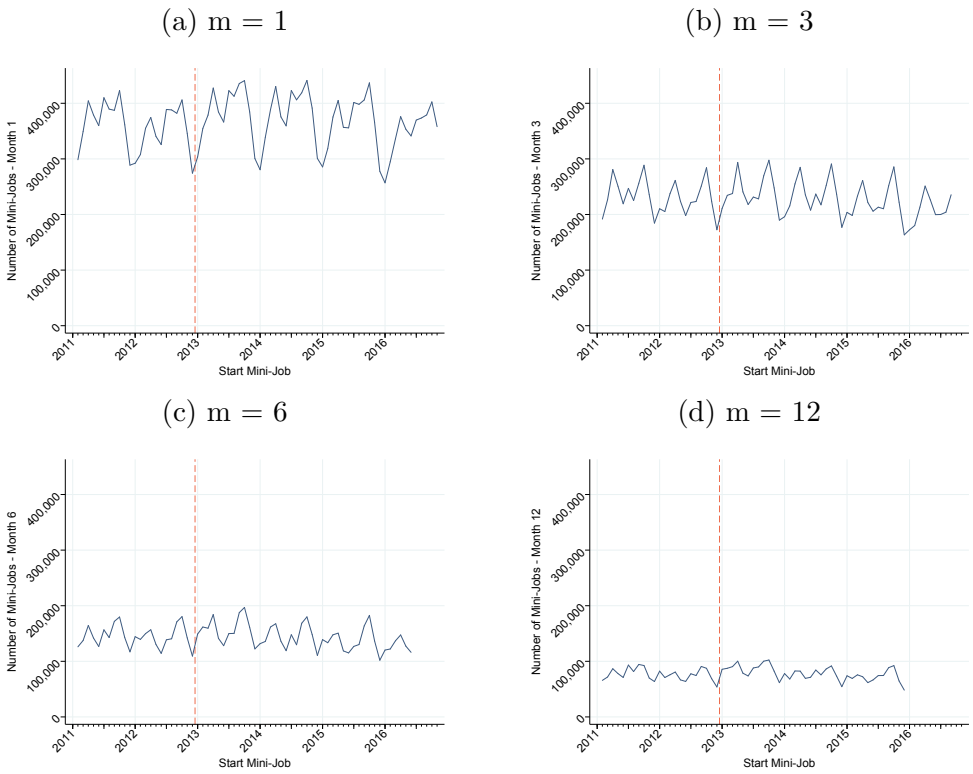
Appendix

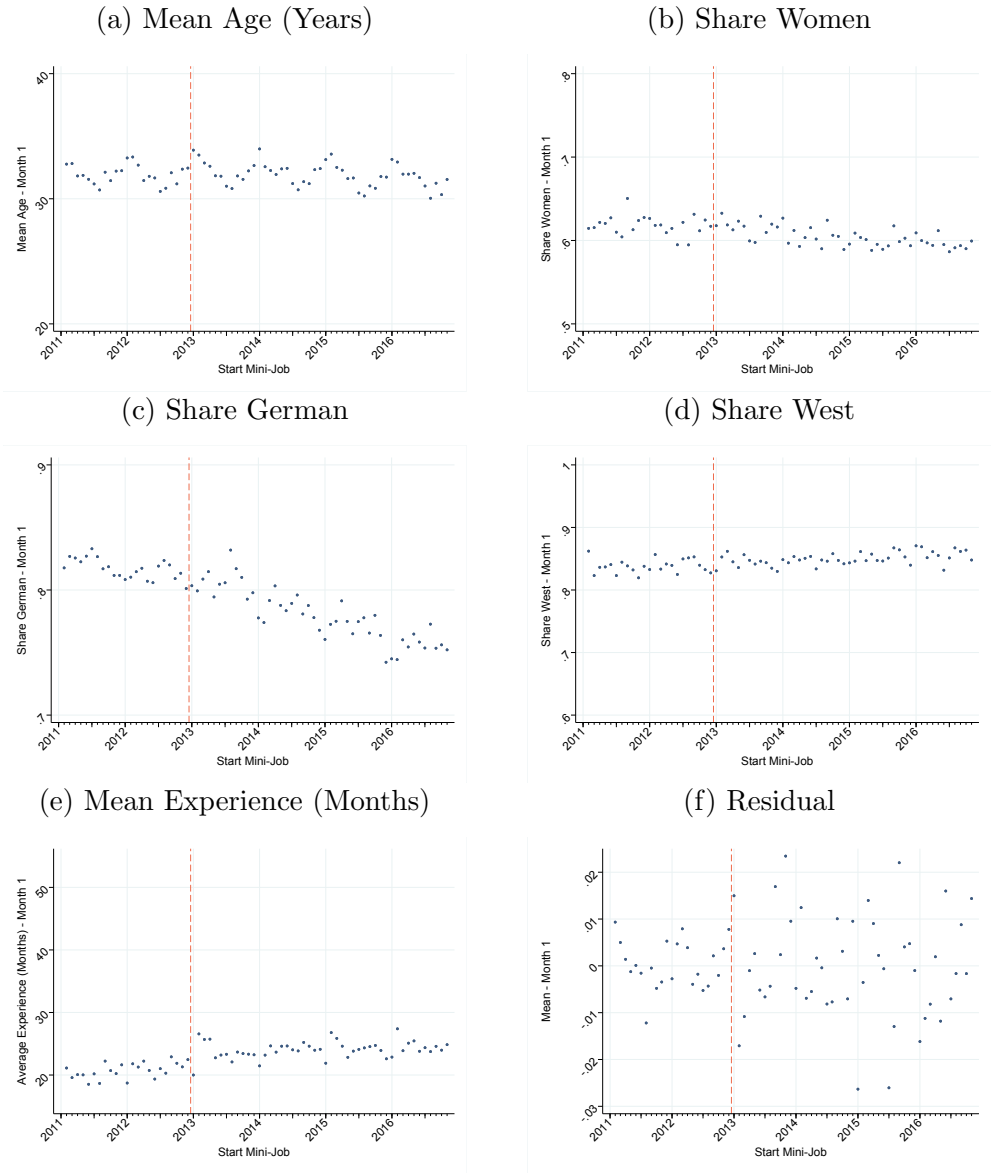
A.1 Additional Figures and Tables

Figure A.1: Number of Mini-Jobs



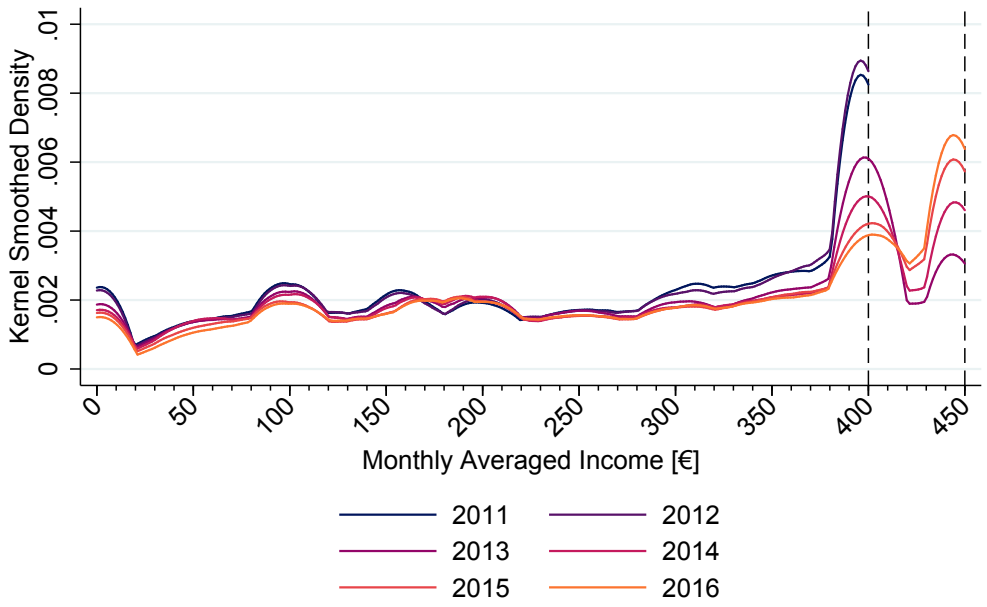
Notes: Weighted data. Graphs display the number of mini-jobs for which we identify a start in month t in the m^{th} month of that mini-job over t. In each panel, the line at given t contains the same group of individuals, exclusive those who dropped out of their mini-job employment before reaching the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

Figure A.2: Control Variables and Residual Over Time



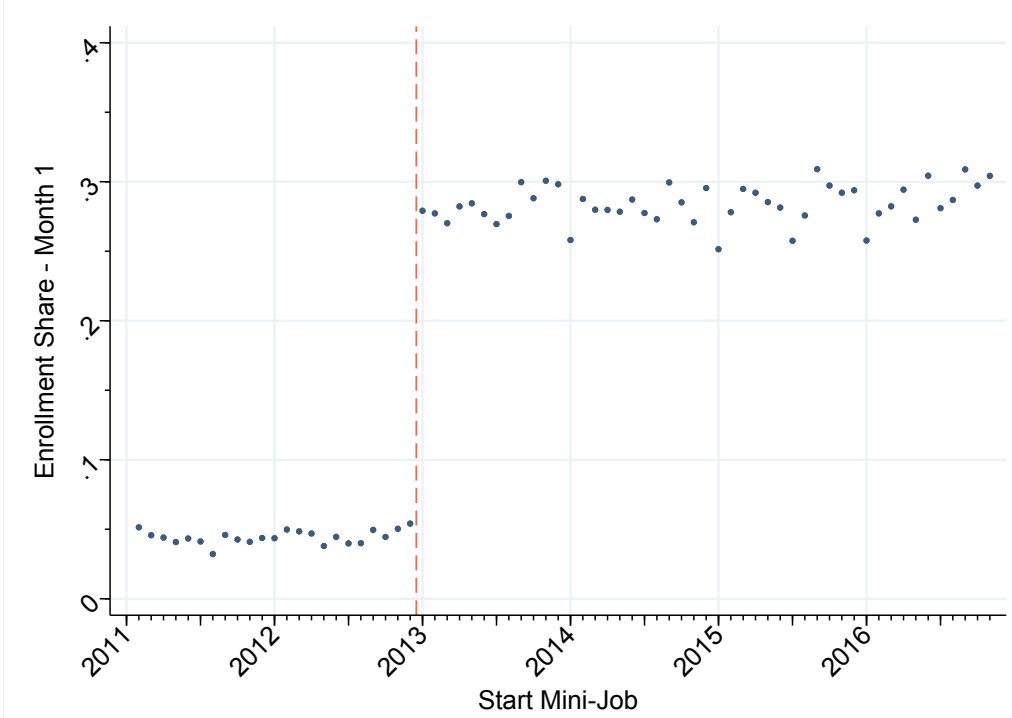
Notes: Weighted data. Scatter plots in panel a to e display the average value for each control variable in the 1^{st} month of the mini-job for individuals who started their mini-job in month t over that t. Panel f displays the average residuals (η_{it}) from Equation 2, again for the 1^{st} month of employment.

Figure A.3: Income Distribution Mini-Jobs



Notes: Annual income distribution for mini-jobs. Kernel smoothed density, weighted data.

Figure A.4: Robustness Check: Income ≤ 400 € ($m = 1$)



Notes: This figure replicates Figure 1, but restricting the post-reform sample to mini-jobs with income up to the pre-reform income threshold of 400 €, as described in subsection 4.4. See also figure notes for Figure 1.

Figure A.5: Example Form for Opting in for Enrollment

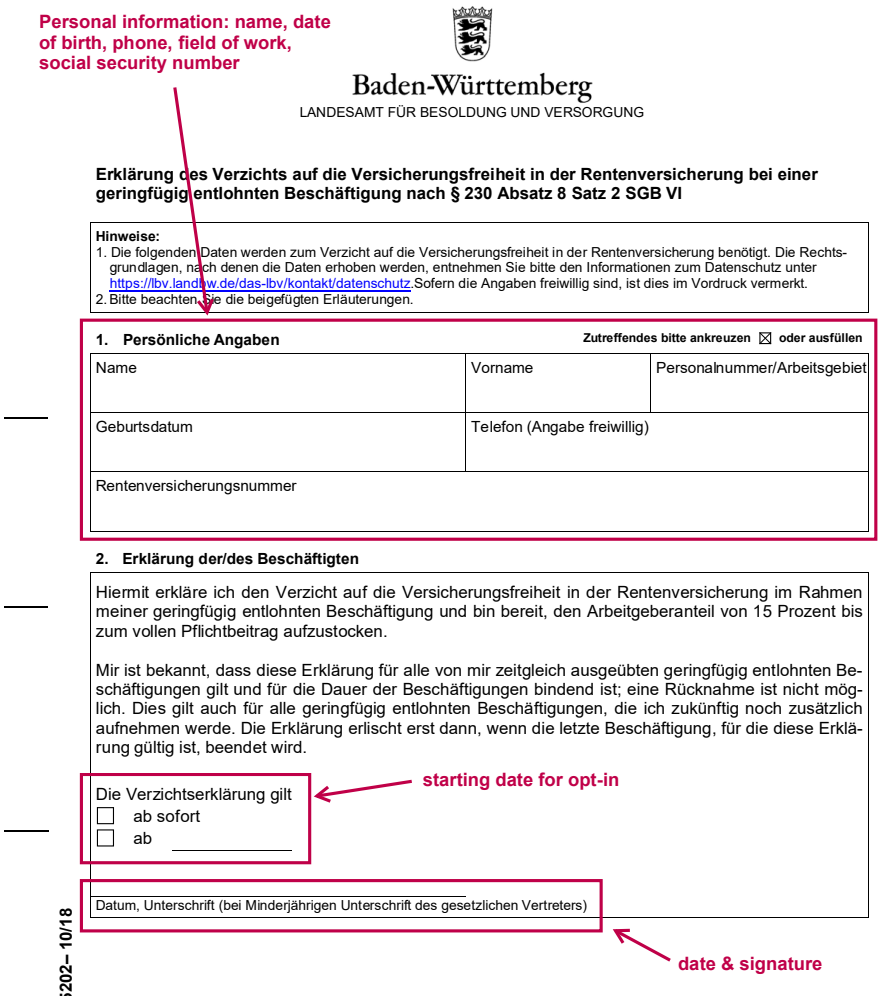
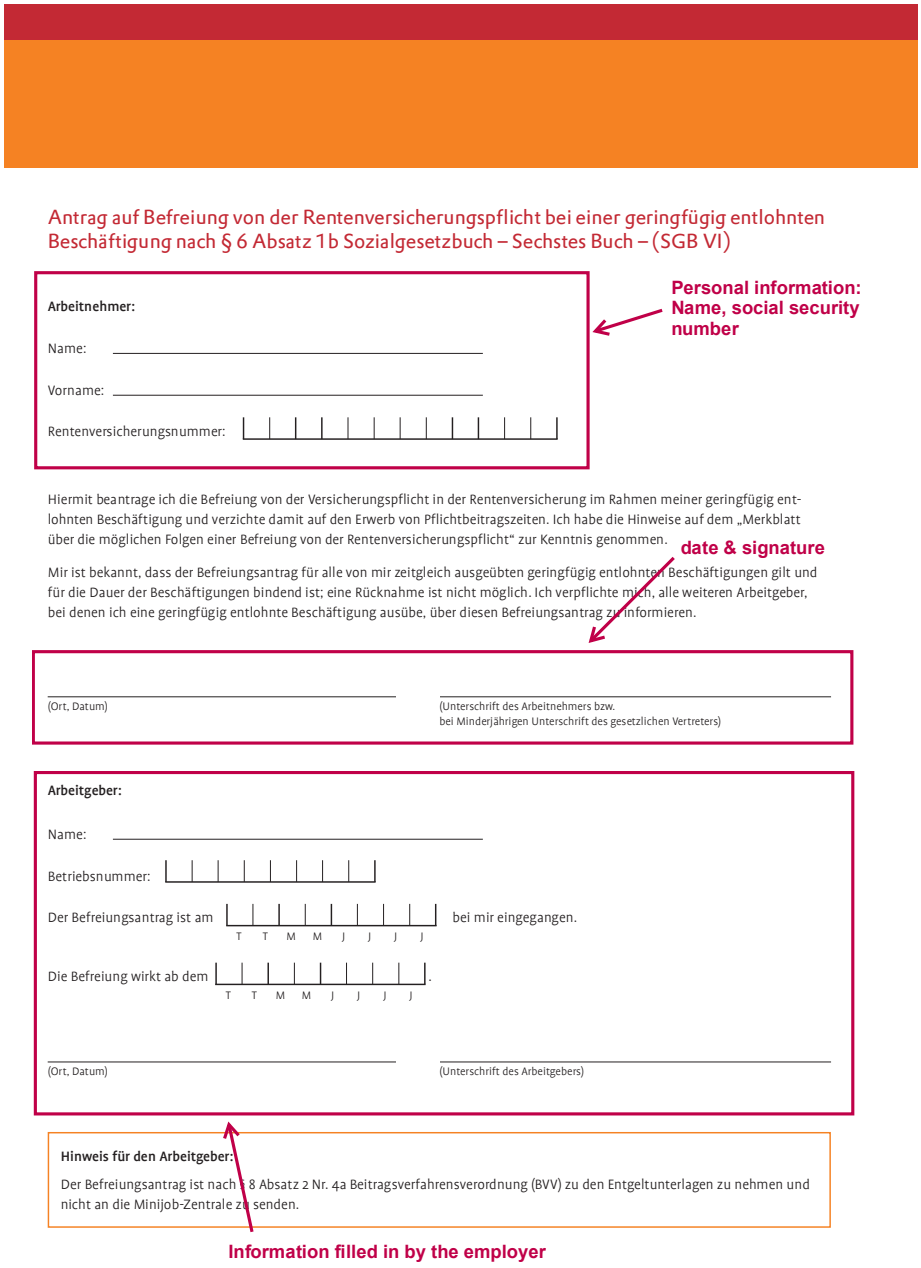


Figure A.6: Example Form for Opting out from Enrollment



Antrag auf Befreiung von der Rentenversicherungspflicht bei einer geringfügig entlohnten Beschäftigung nach § 6 Absatz 1b Sozialgesetzbuch – Sechstes Buch – (SGB VI)

Arbeitnehmer:

Name: _____

Vorname: _____

Rentenversicherungsnummer: | | | | | | | | | | | | |

Personal information: Name, social security number

Hiermit beantrage ich die Befreiung von der Versicherungspflicht in der Rentenversicherung im Rahmen meiner geringfügig entlohnten Beschäftigung und verzichte damit auf den Erwerb von Pflichtbeitragszeiten. Ich habe die Hinweise auf dem „Merkblatt über die möglichen Folgen einer Befreiung von der Rentenversicherungspflicht" zur Kenntnis genommen.

Mir ist bekannt, dass der Befreiungsantrag für alle von mir zeitgleich ausgeübten geringfügig entlohnten Beschäftigungen gilt und für die Dauer der Beschäftigungen bindend ist; eine Rücknahme ist nicht möglich. Ich verpflichte mich, alle weiteren Arbeitgeber, bei denen ich eine geringfügig entlohnte Beschäftigung ausübe, über diesen Befreiungsantrag zu informieren.

date & signature

_____ (Ort, Datum)

_____ (Unterschrift des Arbeitnehmers bzw. bei Minderjährigen Unterschrift des gesetzlichen Vertreters)

Arbeitgeber:

Name: _____

Betriebsnummer: | | | | | | | | |

Der Befreiungsantrag ist am | | | | | | | | bei mir eingegangen.
T T M M J J J J

Die Befreiung wirkt ab dem | | | | | | | | .
T T M M J J J J

_____ (Ort, Datum)

_____ (Unterschrift des Arbeitgebers)

Hinweis für den Arbeitgeber:

Der Befreiungsantrag ist nach § 8 Absatz 2 Nr. 4a Beitragsverfahrensverordnung (BVV) zu den Entgeltunterlagen zu nehmen und nicht an die Minijob-Zentrale zu senden.

Information filled in by the employer

Table A.1: 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 th following year; $\overline{Y}$ income threshold in € (no contributions for $Y - \overline{Y}$); 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.2: Waiting 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
	- 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
	- 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: Incomplete and simplifying, see `https://www.bmas.de/EN/Our-Topics/Pensions/old-age-pensions.html` [last accessed: 2020-10-31] for more details. Waiting periods include periods of (regular) employment as well as a variety of other situations, including parental leave or unemployment. Which situations are considered as waiting period differs slightly across the differennt thresholds.

Table A.3: Instantaneous Effect – Logit

	(1)	(1a)	(1b)	(2)
D_t	0.2381***	0.2397***	0.2395***	0.2414***
	(0.0021)	(0.0020)	(0.0047)	(0.0046)
t	0.0004***	0.0003***		
	(0.0001)	(0.0001)		
t_{pre}			0.0005	0.0005
			(0.0005)	(0.0005)
t_{post}			0.0004***	0.0003***
			(0.0001)	(0.0001)
X_{Jit}	No	Yes	No	Yes
N	337109	333707	337109	333707

This table replicates the results from Table 3 using Logit instead of OLS. Marginal effects from Logit regressions, robust standard errors in parentheses. Weighted data. Significance level: *** 0.001; ** 0.01; * 0.05; ^{x} 0.1. See Table 3 for explanations of variables etc.

Table A.4: Medium-Term Effects – All Observations

	m = 3		m = 6		m = 12	
D_t	0.1738***	0.1776***	0.1725***	0.1767***	0.1744***	0.1747***
	(0.0060)	(0.0058)	(0.0084)	(0.0081)	(0.0137)	(0.0131)
t_{pre}	0.0011***	0.0013***	0.0031***	0.0036***	0.0055***	0.0061***
	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0005)	(0.0004)
t_{post}	0.0005***	0.0004***	0.0007***	0.0007***	0.0008**	0.0008**
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0003)	(0.0003)
cons	0.0357***	0.0154***	0.0301***	-0.0076	0.0358***	-0.0212**
	(0.0026)	(0.0037)	(0.0038)	(0.0051)	(0.0059)	(0.0077)
X_{Jit}	No	Yes	No	Yes	No	Yes
R2	0.0525	0.1176	0.0447	0.1163	0.0327	0.1170
N	201808	199970	119554	118629	57353	56913

Notes: This table replicates the findings from Table 4 using all observations (not excluding group 3 from Figure 2). Effect on the enrollment E_{it}^m of individual i in the m^{th} month of their mini-job in month t. Robust standard errors in parentheses. Weighted data. Significance level: *** 0.001; ** 0.01; * 0.05; ^{x} 0.1. See Table 4 for explanations of the variables etc.

Table A.5: Medium-Term Effects – Logit

	$m = 3$	$m = 6$	$m = 12$
D_t	0.1776***	0.1645***	0.1465***
	(0.0055)	(0.0077)	(0.0140)
t_{pre}	0.0002	0.0003	-0.0008
	(0.0006)	(0.0009)	(0.0022)
t_{post}	0.0003**	0.0006***	0.0007**
	(0.0001)	(0.0001)	(0.0002)
X_{Jit}	Yes	Yes	Yes
N	194893	109306	46923

This table replicates the results from Table 4 that include X_{Jit}, using Logit instead of OLS. Marginal effects from Logit regressions, robust standard errors in parentheses. Weighted data. Significance level: *** 0.001; ** 0.01; * 0.05; ^{x} 0.1. See Table 4 for explanations of variables etc.

Table A.6: Robustness Check: Income ≤ 400 € ($m = 1$)

	(1)	(1a)	(1b)	(2)
D_t	0.2312***	0.2406***	0.2295***	0.2379***
	(0.0034)	(0.0034)	(0.0052)	(0.0051)
t	0.0003**	0.0003***		
	(0.0001)	(0.0001)		
t_{pre}			0.0002	0.0001
			(0.0001)	(0.0001)
t_{post}			0.0003**	0.0003**
			(0.0001)	(0.0001)
$cons$	0.0408***	0.0408***	0.0419***	0.0427***
	(0.0014)	(0.0028)	(0.0021)	(0.0032)
X_{Jit}	No	Yes	No	Yes
R2	0.0835	0.1069	0.0835	0.1069
N	281562	278840	281562	278840

Notes: This table replicates the findings from Table 3, but restricting the post-reform sample to mini-jobs with income up to the pre-reform income threshold of 400 €, as described in subsection 4.4. See notes in Table 3 for explanation of variables etc.

A.2 Illustration of Enrollment Incentives

In order to better understand the incentives of enrollment, we illustrate them with an exemplary mini-job employee who worked for 12 months in 2013 with a monthly income of 450 €. That employee acquires 0.1604 EP under enrollment and 0.1273 EP with employer contribution only.[15] They have an incentive to enroll if their return from the increased pension entitlements is higher than their costs from enrollment. By how much they increase their total pension entitlements depends on the length of the retirement period as well as on the earning-points' monetized value upon retirement. In 2013, the equivalent monthly pension value for 0.1604 EP and 0.1273 EP amounts to 4.51 € and 3.58 €, respectively.[16] With a monthly contribution of 17.50 € for one year, the employee thus increases their future monthly pension entitlement by 0.93 € in 2013 numbers. When assuming constant value for the pension points after 2013, the total increase in pension entitlement surpasses the contribution payments after 18.8 years of retirement (17.50/0.93).

In reality, the pension points' value is tied to the development of the market income such that the equivalent monthly pension value from the enrollment in the above mini-job example has increased substantially already since 2013. In 2020, the 0.1604 EP and 0.1273 EP are worth 5.29 € and 4.21 €, respectively. Under enrollment and with a monthly contribution of 17.50 € in 2013, the employee thus increases their monthly pension entitlement by 1.08 € in 2020 numbers. If we again assume constant future value for the pension points, the increase in pension entitlements exceeds the payments from enrollment after 16.2 years.

This oversimplified back-on-the-envelop calculation is abstracting from many relevant factors such as the actual development of the pension point value, life expectancy, risk-aversion, intertemporal discounting, present-bias or outside-options for savings. It is meant to provide a broad idea of how much it pays off for minijob-

[15] $Y_t = 33,659$ (Table A.1). Earning-points for enrolled employees are calculated with $EP = \frac{450 \times 12}{33,659}$. For non-enrolled employees, $EP = \frac{450 \times 12}{33,659} \times \frac{15}{18.9}$.

[16] These are the values for western German states, which account for the vast majority of the German population and also for the majority of mini-job employees. For employees in eastern states, the values are 4.13 € and 3.28 €, respectively. The monthly pension values per earning point over time for east and west are showed in Table A.1

employees to enroll in the statutory pension insurance.

In our example with 12 months mini-job employment and a monthly income of 450 € in 2013, the mini-job employee obtains 12 months for their waiting period when enrolled but only 4 months when not enrolled ($\frac{0.1273}{0.0313}$ rounded to full months). It is important to note that the waiting period is not income related under enrollment, but depends on the income when not enrolled. A mini-job employee with a monthly income of 200 € for 12 months pays 7.80 € and obtains 0.0713 EP as well as 12 months for their waiting period under enrollment in 2013. Without enrollment, they acquire 0.0566 EP and 2 months ($\frac{0.0566}{0.0313}$ rounded to full months) for their waiting period. The enrollment incentive for waiting periods, relative to the costs of enrollment, is thus stronger for lower incomes.