Opt-in or Opt-out? The Effect of Defaults on Public Pension Enrollment*

Tabea Bucher-Koenen, Luisa Wallossek, Joachim Winter
– Please do not cite or circulate –

February 28, 2022

Abstract

Defaults have been shown to be a powerful tool to enhance enrollment in pension insurance 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's easy to opt-out. We study low-income employees who face a binary choice: to enroll or not to enroll in the public pension insurance. Using administrative data from the German Pension Insurance, we document that enrollment increases significantly after the introduction of automatic enrollment but the majority of individuals actively opt-out. We document that behind this average effect, there is substantial heterogeneity in the power of the default across different groups of individuals. We use linked survey data to show that sticking to the default is not optimal for everyone. While many people for whom the default is not optimal opt-out, there is also a fraction of individuals that stick to the default nevertheless.

Keywords: Retirement, Default-Setting, Auto-Enrollment
JEL Codes: H55, J26, D14

*Bucher-Koenen: ZEW – Leibniz Centre for European Economic Research and University of Mannheim, e-mail: tabea.bucher-koenen@zew.de; Wallossek: Munich Graduate School of Economics, LMU – Ludwig Maximilian University of Munich, e-mail: luisa.wallossek@econ.lmu.de; Winter: LMU – Ludwig Maximilian University of Munich, CESifo, and Munich Center for the Economics of Aging (MEA), email: winter@lmu.de. We are grateful for comments from discussants and participants at IIPF, VfS, Netspar International Pension Workshop, ZEW Conference on Ageing and Financial Markets, Deutsche Bundesbank's International Conference on Household Finance, NTA and seminar participants at MEA and ZEW. We gratefully acknowledge the support from the Research Data Centre of the German Pension Insurance.

1 Introduction

How to improve individual choices is an important question in both economic research and policy making. One important topic is the role of defaults. Applications range from the health sector like organ donation or vaccinations, to personal finance, e.g. by settings of debit card limits, to renewable energy use (Madrian 2014). The application that probably attracted most attention in this context is savings for retirement. Interest has been growing over the past two decades since retirement plans increasingly build on defined contributions that typically feature default options 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 behavior, they are prone to inertia and therefore stick to defaults. In particular, auto-enrollment has been shown to have a large effect on individual savings behavior in defined contribution plans (e.g., Madrian and Shea 2001). However, it is also well known that the default might not be optimal for everyone (Carroll et al. 2009).

In this paper, we document that in a setting where the choice menu is simple, individuals still stick to the default, but the effect is smaller than what is typically observed in more complex settings. After the introduction of automatic pension enrollment for low-income employees in Germany, the enrollment share in the first month of employment increases by 23 percentage points, but a majority of 70 percent opts-out immediately. Using administrative data, we emphasize the heterogeneity of choices that is hidden behind these headline numbers. On the one hand, individuals react to disincentives by being less prone to the default if their enrollment costs are higher. On the other hand, the power of the default is stronger for individuals with presumably lower financial literacy, potentially nudging them into enrollment although it is not optimal for them. To better understand the drivers behind the observed heterogeneity, we use linked survey data from the largest German household panel.[1]

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 menu: 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.

[1]Because the linked data (*SOEP-RV*) is currently in a pilot stage and not published yet, we cannot include results based on this data set in the present draft of this paper. We expect the data set to be published in spring 2022 and we will add the results then.

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 (RDD) 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. Relative to a pre-reform enrollment share of about 5 percent, this is a sizable effect. 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 tenure but remains economically and statistically significant in the medium run: after 12 months in a mini-job, the enrollment share is about 14 percentage points higher under automatic enrollment.

To better understand the high prevalence of opt-outs, 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 under both regimes, is observed for less than 15% of the mini-job employees. Furthermore, we show that a significant share of mini-job employees seems to understand and react to enrollment incentives in general, by analyzing discontinuities in incentives at waiting period thresholds that are decisive for pension eligibility. This paper contributes to the literature on the impact of default options on individual behavior and, in particular, to the literature on auto-enrollment in retirement saving and pension schemes. Much 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 et al. (2004), Beshears, Choi, Laibson, and Madrian (2012), Bernheim et al. (2015) or Beshears, Choi, Laibson, Madrian, and Wang (2016).[2] This literature has a common bottom line: default setting has a substantial 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.

Similar results are found in other settings. For Australia, where the vast majority of the workforce is automatically enrolled in the superannuation system, Butt et al. (2018) find that about 70% of survey respondents stick to the default plan and/or the default investment strategy. Blumenstock et al. (2018) run a field experiment in Afghanistan and find that enrollment rates in mobile savings accounts increased by

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

40% after introducing auto-enrollment. The power of defaults has been observed in settings with complex choice menus. In the present paper, we investigate the impact of default setting for a very simple choice menu. This allows for disentangling the pure impact of default setting from other factors that are likely to play a roll in situations where individuals face more complex choices. Under the 401(k) plans for instance, 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 et al. 2020). Madrian and Shea (2001) argue that parts of the observed default stickiness also stem from employees' interpretation of the default as their employer's investment advice. Additionally, switching costs can account for inertia as well (Gabaix 2019, Heiss et al. 2021).

In the mini-job setting that we study in this paper, none of these complications arise. The employees' choice menu is binary: to enroll or not. No other dimension has to be considered. Choice overload is thus unlikely to explain observed default stickiness. Since employers make no choice for their employees, the default cannot be misinterpreted as their advice either. And 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 a related literature on passive savings behavior. Chetty et al. (2014) classify Danish tax payers into active and passive savers based on their reaction to changes in automatic contribution rates and subsidies for savings. They find that the vast majority of the Danish population 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. For the enrollment behavior of mini-job employees, we find that only a minority of individuals show truly passive behavior which we define as always sticking to the default irrespective of what the default is. The majority of mini-job employees is best described as never takers that never enroll, irrespective of the default. Here, we also relate to Goda et al. (2020) who emphasize that passive behavior is not necessarily an individual trait but depends on the default.

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 set we use in section 3. We analyze the effect of the default on the public-pension enrollment of mini-job employees 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.[3] Both employees and employers make contributions at contribution rates that are defined on a national level.

Pension Points. Contribution payments are translated into so called 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% (150%) of the average annual income obtains 0.5 (1.5) pension points and so on, with contribution being capped for income 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 depends on the average labor income 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. Table A.1 gives an overview about the relevant operands in the German statutory pension insurance and their development for the periods relevant for our analysis.

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 employments with very low monthly gross income, up to 400 € for years prior to 2013 and 450 € ever since. Mini-jobs are exempt from income taxes and also free of most social security contributions, including contributions to the

[3] 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.

public health insurance. They can be either an employee's main employment or a side-job in addition to a regular employment, but we only focus on the first group in this paper because of data availability.

Employers' Pension Contribution. Contribution 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}, which has been at 15% in the last years. For mini-jobs τ_{er} is higher than for regular employment, where employer and employee each contribute at the same contribution rate, e.g. 9.45% in 2013 (see Table A.1 for contribution rates over time). If employees are enrolled, the total contribution rate (employer + employee) is the same as under regular employment.

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. The employees' contribution rate is much smaller than the employers', with τ_{ee} = 3.9% and τ_{er} = 15% in 2013. If an individual enrolls, their total contribution rate is $\tau_{er} + \tau_{ee}$ = 18.9% of their gross income as compared to τ_{er} = 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. For most time of the past, the default was no enrollment. Mini-job employees thus had to actively opt into enrollment. After a reform in 2013, the default changed from an opt-in regime to an opt-out regime, where mini-job employees have to actively opt-out of the default enrollment.

Reform. The reform of the mini-job regulations came into force on January 1, 2013.[4] Going from an opt-in to an opt-out regime, the reform installs auto enrollment for mini-job employees in the statutory pension insurance. This new default applies to all mini-job employees whose employment starts in 2013 or later. Employees whose mini-job started pre-reform (2012 or earlier) initially remain under the old opt-in regime even post-reform. The new default applies to them as soon as their mini-job income surpasses the income threshold of 400 € or they take up a new mini-job.

As a second feature of the reform, the allowed income threshold for mini-jobs

[4]The respective law (Act on Changes in the Area of Marginal Employment) passed the German parliament on December 5, 2012.

increases 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 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 the 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 a one-page form with very basic 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 main incentives for enrolling in the statutory public pension insurance. First, mini-job employees 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 increases 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% in 2013. This first incentive increases future pension entitlements on the intensive margin and is thus relevant for mini-job employees who expect to receive pension payments in the future and want to increase their pension entitlements through enrollment.

Waiting Periods. While the earnings points determine the pension level, the pension eligibility depends on the individual waiting period. Under enrollment, one month of mini-job employment is equivalent to one additional month for the waiting period. Without enrollment however, mini-jobs are not fully considered for the waiting period. Precisely, the waiting period for non-contributors depends on the earnings points and is determined by $\frac{EP}{0.0313}$. This second incentive increases future pension entitlements on the extensive margin and is thus relevant if insurees want to increase their waiting period to become eligible for pension payments. This is typically the case if the accumulated waiting time is below but fairly close to 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 month, they will become eligible for monthly pension payment during their entire retirement period.[5]

We illustrate the two different enrollment incentives with an exemplary mini-job employee in subsection A.3. 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.

Note that employers always contribute the mandated τ_{er} of 15%, 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 virtually no compliance cost on them.

[5]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.

3 Data

Our analysis is based on administrative data from the German statutory pension insurance. The so-called *Versichertenkontenstichprobe* (VSKT) is a sample of the universe of insurees in the German Statutory Pension Insurance.[6] It is a monthly panel data set with information for the entire biography of the insurees. The sample is drawn as of December 31, 2016 and contains data for individuals born between 1949 and 2001. Information for employees is reported directly by the employers.

Our main variable of interest records the insuree's 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 the mini-job is their main employment. Demographic characteristics such as date of birth, gender, citizenship or region are 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.

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 further exclude mini-job observations if the reported average monthly income exceeds the applying upper bound threshold $\bar{y}$ for mini-job income.[7] Until 2012, $\bar{y}$ is 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 2013 or later are under 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. To get the cleanest possible set-up, we focus our analysis on the first month of mini-job employments, for which we can identify the starting date.[8]

[6] Data set: German Pension Insurance (Deutsche Rentenversicherung Bund 2017).

[7] 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.

[8] A mini-job employment is considered to have its start in month t if the recorded starting date

Table 1 provides sample characteristics for the final sample in the first (third, sixth, 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 6-year period. The number of observations decreases significantly with increasing m, representing the short average tenure for mini-jobs.

Table 1: Sample Characteristics

	$m = 1$	$m = 3$	$m = 6$	$m = 12$
(A) Number of mini-jobs				
Observations	337,109	201,808	119,554	57,353
Weighted	25,621,597	15,575,701	9,369,103	4,581,213
(B) Basic characteristics				
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

Notes: Number of new mini-jobs in the period 01/2011– 11/2016. 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.

4 Estimating the Impact of the Default Setting

We use the natural experiment of introducing automatic enrollment for mini-jobs in 2013 to estimate the causal impact of the default on the individual enrollment status. Before 2013, all mini-job employees are under the opt-in regime. After the 2013 reform, new mini-jobs are always under the new default with automatic enrollment. Mini-job employees remain under the old opt-in regime post reform if their employment started pre 2013 and their monthly income does not surpass 400 €.

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.

4.1 Empirical Strategy

We use a regression-discontinuity (RD) approach to determine the causal effect of the default on the enrollment, 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 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 who started 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 is β that 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 time trends 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. The full model, including X_{Jit} an different time trends is estimated as:

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

We use ordinary least squares (OLS) to estimate these equations. As discussed in subsection 4.4, we replicate the analysis with non-linear Logit regressions. The results are very similar, with the marginal effects from the Logit regressions being close to the OLS estimates.

Identification Assumptions. The main identification assumption for our RDD approach is that assignment in the neighbourhood of the January 1, 2013 cutoff 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.

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 graphical evidence for discontinuities for these control variables. In addition, panel f of Figure A.2 shows that there is also no discontinuity in the error term η_{it} from Equation 2. Consequently, there is no reason to assume a discontinuity for any unobserved characteristics.

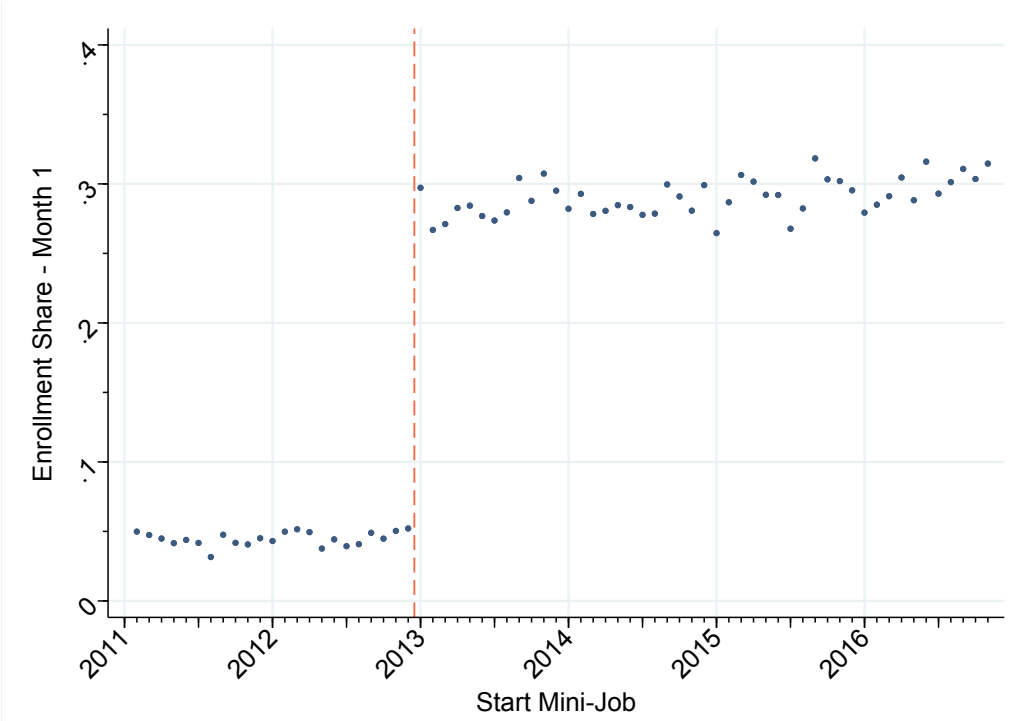
A second concern for RD designs is individuals' ability to manipulate the running variable, leading to non-random assignment around the cut-off (Imbens and Lemieux 2008; Lee 2008). In our setting, the running variable is the starting date of the mini-job employment. This is clearly non-deterministic and individuals as well as employers could theoretically manipulate the starting date around the cut-off. If there was manipulation, this would imply a discontinuity in the density 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 in line with the fact that there is 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 shows graphical evidence for the instantaneous effect of the default on the enrollment share in the first month of a mini-job ($m = 1$). The graph shows the unconditional mean of the enrollment share of individuals who started their mini-job in month t. The discontinuity at the reform date, indicated by the dashed line, is clearly visible. Under the opt-in regime pre-reform, the enrollment share is about 5% for all months. Starting with automatic enrollment for new jobs in January 2013, the enrollment share jumps to a share of almost 30% and stabilizes at that level for all post-reform months with a gentle increase over time.

While Figure 1 visualizes the clear and sizable effect of automatic enrollment, it also shows that the majority of mini-job employees is not affected by it. Most individuals who start their mini-job under automatic enrollment are not enrolled

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.

in the first month of employment. Put differently, about 70% of them opt out immediately after being automatically enrolled.

Table 2 reports the corresponding results from the OLS estimations for the first month of employment ($m = 1$) 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. Column 1 of Table 2 refers to the baseline specification defined in Equation 1, column 1a and 1b add individual characteristics X_{Jit} and different time trends, respectively. Column 2 reports results from estimating Equation 2 that includes both. Both magnitude and significance remain the same over the different specifications.

Medium-Term Effects. We have shown that automatic enrollment has a clear instantaneous effect on enrollment in the first month of employment. To better un-

Table 2: 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.4 replicates the results using Logit.

derstand the impact of automatic enrollment on public pension entitlements, we now widen the time horizon and analyze the medium-term effect of automatic enrollment. We do so by tracking individuals over 3 different points in the first year of employment, in the third, sixth and twelfth month ($m = 3, 6, 12$).

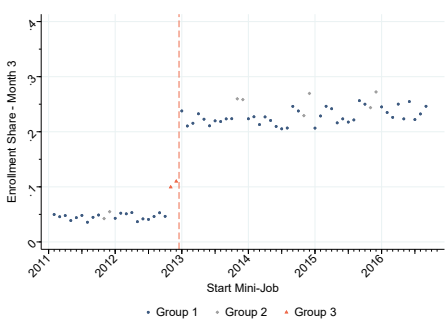
Intuitively, the sample size decreases with increasing m because not all individuals remain 3, 6 or 12 months in their mini-job employment. With increasing m, β is also expected to decrease if we assume that at least some individuals are partly inert and take some time to deviate from the default and to actively opt-in (under the old default) or opt-out (under automatic enrollment).

Widening the time horizon comes at the cost of loosing precise information. As soon as we track individuals for $m > 1$ months, the employment history will include a turn of the year for at least some individuals. For $m = 3$ for instance, every indi-

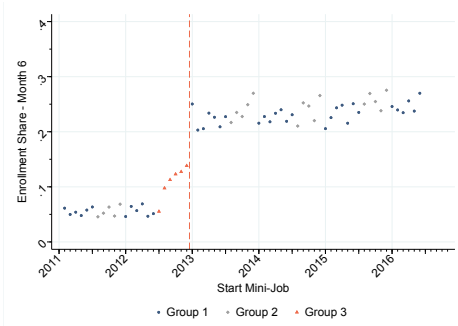
vidual who started their job in November or December is now observed in the next year (January and February, respectively). This threatens identification because we cannot distinguish individuals who remained in the same job over the turn of the year from those who started a new mini-job at the beginning of the new year. Consequently, we cannot determine the default for individuals whose employment period includes the turn of the year 2012/2013. For employment periods over the turns of the year pre or post reform, we know their default but there remains uncertainty about the precise length of their current employment.

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

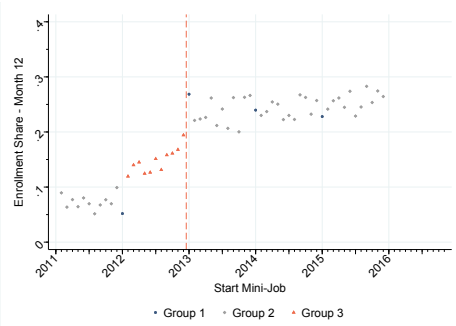
(a) m = 3



(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

Figure 2 shows the unconditional mean of the enrollment share for individuals from Figure 1 that remain in a mini-job for at least 3, 6 or 12 months (panel a, b and c, respectively). There are three groups of individuals and their prevalence changes with increasing m. For group 1 (dark blue dots), there is no uncertainty about their default or the starting date of their employment. The information for them is as precise as for $m = 1$. Group 2 contains those for whom we know their default with certainty but there is uncertainty about the starting date of their employment (grey diamonds). For group 3 (orange triangles), there is uncertainty about both the default and the starting date.

Four patterns emerge. First, there is a clear positive mid-run effect of auto-enrollment with enrollment shares above 20% for all m and over the entire post reform period. Second, enrollment post reform for $m = 3, 6, 12$ is somewhat lower than in the first month which is in line with some individuals being partly inert and thus opting out only after some time. Third, pre-reform enrollment shares increases with m.

Lastly, there is 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 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 observed increased pre-reform enrollment shares are in line with parts of the individuals having started a new mini-job under the new default as discussed in subsection 4.4. 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. In Table A.5, we provide results from estimating the medium-term effect including all observations.

Table 3 provides the results from the estimation of Equation 2 for $m = 3$, 6, and 12 when restricting the sample to observations with no uncertainty about the default (excluding group 3 from Figure 2). The results confirm 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.

There are two explanations for the divergence between the instantaneous and

Table 3: 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.5 for the same table without excluding group 3. Table A.6 replicates the results using Logit.

medium-run effect. First, inertia delays the response and individuals deviate from the default only after some time. 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 realize 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}. Adding the interaction terms $D_t \times X_{Jit}$ to Equation 2 yields:

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

For each X_J, the interaction term ζ_J captures heterogeneity in the effect of automatic enrollment for different values of characteristic J. A positive value indicates a higher increase in enrollment under the new default.

Table 4 displays the results from the estimation of Equation 3. For each characteristic J, the table displays the estimated coefficient δ_J as well as the coefficient from the interaction with the default ζ_J. The results show that the overall effect of the default is persistent when allowing for heterogeneous effects but that there is significant heterogeneity in the impact of the default.

Demographic Characteristics. Understanding the interplay between automatic enrollment and different demographic characteristics can be considered 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).[9].

The administrative data provides a set of basic individual demographics (age, gender,[10] citizenship, region) and Table 4 documents substantial heterogeneity in enrollment behavior 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

[9]The average pension for women in Germany is 46 percent lower than for men and the income poverty rate at age 66 or older of 10.6% is increased by 43 percent as compared to 7.4% for men (OECD 2019).

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

Table 4: Heterogeneous Effects of Automatic Enrollment

	(1)	(2)	(3)
D_t	0.2640***	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.

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. 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 an East German state.

Stylized Effects. To better understand the extend of heterogeneity, we provide stylized effects 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 who has 10 years of experience as a mini-job employee. Person B is a first-time mini-job employee, a 25 year old men who has never before worked in a mini-job. Both are in the first month of a new mini-job, live in a West German state, have the German citizenship and income above the minimum assessment threshold. 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 4. The predicted enrollment share pre-reform is 16.1% for A and 2.6% for B. Post-reform, the predicted enrollment rises to 32.3% for A and 31.9% 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.

Salience & Financial Literacy. One potential explanation for the heterogeneous enrollment effects is heterogeneity in the salience and individual understanding of the default. If the default regime and the options for deviation are less salient for certain groups, those groups may be more likely to stick to the default. Salience may be lower for non-German citizens because they are less familiar with the setting and may face language barriers. Furthermore, women are often shown to be less financially literate than men (Bucher-Koenen et al. 2017) which could explain a stronger effect of the default for women.[11] Financial literacy is also lower in East Germany (Bucher-Koenen and Lusardi 2011), in line with the positive interaction effect for the east dummy.[12]

[11]Bucher-Koenen, Alessie, et al. (2021) find that a part of the gender gap in financial literacy can be explained by a gap in confidence rather than in knowledge, but the gap is persistent even when accounting for the differences in confidence.

[12]Even decades after the German reunification, former East and West differ among many dimensions. See Becker et al. (2020) for an overview of existing evidence on east-west differences in economic outcomes, political preferences, culture and gender roles.

Naturally, the administrative data does not include a direct measure for salience or financial literacy. To overcome this, we use past experience with mini-jobs as a proxy for salience, assuming that exposure to the mini-job system increases institutional knowledge and the salience of the default setting. Column 2 of Table 4 shows that a longer past experience is associated with higher enrollment for $D_t = 0$, but the interaction with the default dummy is negative. This is consistent with the default being more salient for more experienced mini-job employees. To better understand the role of salience and financial literacy, we link a subsample of the administrative data to survey data and study the enrollment behavior for individuals based on their knowledge about their own enrollment status. We provide the results of this analysis in subsection 5.3.

(Dis-)Incentives. Heterogeneity in the power of the default is of high relevance for policy makers. Specifically, it raises the question whether automatic enrollment nudges "the right" individuals into enrollment. A key dimension of interest here is heterogeneity in costs or benefits of enrollment. Individuals may face different (dis-)incentives for enrollment. For example, non-German mini-job employees may have a lower expected return from enrollment if they are less likely to claim old age pension in Germany because they reach the minimum contribution period of five years less often, i.e. because they may only live in Germany for a relatively short period.

While financial (dis-)incentives are likely to play an important role for enrollment, but the administrative data do not provide information on such costs on the individual level. We make use of the institutional setting to overcome this and identify a group that experiences higher costs: individuals with income below the minimum contribution threshold. 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 clearly decreases incentives for enrollment by imposing higher costs. We add a dummy for this group in column 3 of Table 4 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, it's absolute value amounts to almost half of the effect of D_t alone. In subsection 5.3, we use survey data to better understand the role of individual (dis-)incentives for enrollment.

4.4 Robustness Checks

Logistic Regression. In addition to the OLS estimation in the main analysis, we replicate our findings using logistic regression estimations (Logit). Table A.4

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 2. The estimated marginal effect of automatic enrollment is 0.24 compared to 0.23 in the OLS estimation. Table A.6 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 3. All effects remain highly significant at the 0.001 level.

Unknown Default. When we track the enrollment of individuals for more than the first month of their mini-job, we lose information whenever the time period contains the change of a year. Figure 2 shows that enrollment behavior changes for those for whom the default is unknown. One explanation is that a significant part of these individuals actually start a new job with the beginning of the new year and are 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 needed 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% pre reform and about 23% post reform. For those who started their mini-jobs in December 2012, the enrollment share in the third month (February 2013) is about 9%. 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% of individuals who we treat as if they remained in the same employment from December 2012 onwards actually started a new mini-job in January 2013, this would fully explain the increased enrollment share of 9% that we observe for these group in February 2013.

We use additional data to assess the plausibility of $x = 0.22$ from the central agency that is handling administration for mini-jobs (*Minijob-Zentrale*).[13] We use the quarterly data from March 31, 2013 and look at those who have started a mini-job in the past 180 days.[14] Out of these mini-jobs, the share of those that started within the past 90 days, thus post reform, is 77%. 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.

[13]The data is not publically available and was provided upon request.

[14]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.

Change in Income Threshold. Mini-job regulation applies to employments with income below a certain income threshold. In 2013, this threshold increased from 400 € to 450 €. This change coincides with the introduction of automatic enrollment and thus constitutes a potential confounder for our analysis. If a higher income threshold increases the probability of enrollment, we overestimate the effect of the change in default. Two mechanisms are possible.

First, there may be extensive labor supply responses to the increased income threshold. Individuals who did not supply labor pre-reform may do so post-reform if their monthly reservation wage lies between 400 € and 450 €. 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, intensive margin labor supply and liquidity constrains could increase 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 constrains hinder i from enrollment. The increased income threshold will increase i's labor supply post reform, while at the same time potentially lifting liquidity constrains, leading to a change in enrollment.

Figure A.3 shows that the income distribution for mini-jobs peaks at 400 € pre reform, but there is also substantial mass below that threshold, making it not binding for most employees. Furthermore, a smaller peak at 400 € remains post reform.[15] To provide further evidence, we rerun the main analysis for a subsample of mini-jobs with monthly income $\leq$ 400 €. The estimated effects are of comparable size as shown in Figure A.4 and Table A.7. We thus argue that the increased income threshold cannot explain the increased enrollment post-reform and does not constitute a threat for identification.

[15]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.

5 Understanding Individual Behavior

We have shown in section 4 that automatic enrollment has a significant effect on new mini-job employees' public pension enrollment. However, under automatic enrollment, only a minority of individuals stick to the default while the majority opts 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 (Blumenstock et al. 2018; Cribb and Emmerson 2020; Madrian and Shea 2001). We now want to better understand the comparably low enrollment rates and what may drive the individual enrollment behavior.

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 individuals or savers. This terminology has been heavily influenced by Chetty et al. (2014). Following Chetty et al. (2014), active savers are typically defined as individuals whose behavior is in line with neoclassical 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 default setting. Passive savers show the exact opposite behavior. They do not react to changes in savings incentives but are heavily influenced by automatic enrollment.

Chetty et al. (2014) find that the vast majority (85%) of the Danish population can be described as passive savers, who do not respond to savings subsidies and who save more (less) when their automatic contribution rates increase (decrease). Only 15% 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 (see Butt et al. 2018, Goda et al. 2020 or Goodman 2020).

To examine the prevalence of active and passive behavior in the simple choice framework for mini-jobs, we make use of the panel structure of the data set and compare the enrollment behavior of individuals observed under both defaults. We define behavioral types based on the combination of the individual enrollment under the opt-in and the opt-out regime. We define a total of 6 behavioral types, as shown in Table 5.

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

Table 5: 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.

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* are the exact opposite, they always enroll irrespective of the default.

For both never and always takers, the default does not affect their enrollment status and they do not contribute more under automatic enrollment. Following the terminology of Chetty et al. (2014), 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.

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 being enrolled by default. Again following the Chetty et al. (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 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. Such partly inert behavior can be squared with different explanations. 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 making an active choice. The remaining behavioral patterns are summarized as *inconsistent* behavior.

Table 6: Prevalence of Behavioral Types

	CaseI	CaseII
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	47,765.00	57,628.00

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 5 for the definition of the behavioral types.

In order to examine the taxonomy of individual behavior under different defaults, we restrict the sample to individuals for whom we have at least one monthly observation under each default. 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 in question 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.

By changing the default, policy makers can consistently change the enrollment behavior of passive individuals, who always stick to the default and start enrolling only under automatic enrollment. Table 6 shows that only a minority of individuals exhibit this purely passive behavior, between 8 and 13 percent. Put differently, by going from an opt-in to an opt-out regime, only about 1 in 10 individuals can be consistently nudged into enrollment in the public pension insurance.

The majority are never-takers, who never enroll neither under the opt-in nor under the opt-out regime. For both cases, we find the share of never takers to be about two thirds. Thus, the majority does not show any inertia under the new default and opts-out immediately after starting their mini-job with auto-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 periods: One pre-reform without enrollment, a second period post-reform without enrollment and under unknown default and a third period 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 (period 1 and 2) but always under the new default (period 3). In case II, the behavior is classified as inconsistent because the enrollment status under the new default (period 2 and 3) is not consistent.

5.2 Additional Evidence: Individual Comprehension of the Pension System

A key finding of our analysis in section 4 is that most individuals opt-out and we have shown above that this is driven by a majority of never-takers who never enroll.

An open question that remains is why are individuals so little prone to automatic enrollment in this setting? We provide one potential explanation: when facing a simple choice menu, costs and benefits from enrollment may be more salient for individuals. We provide suggestive evidence for this by studying enrollment behavior of individuals who are close to reaching a threshold for waiting periods.

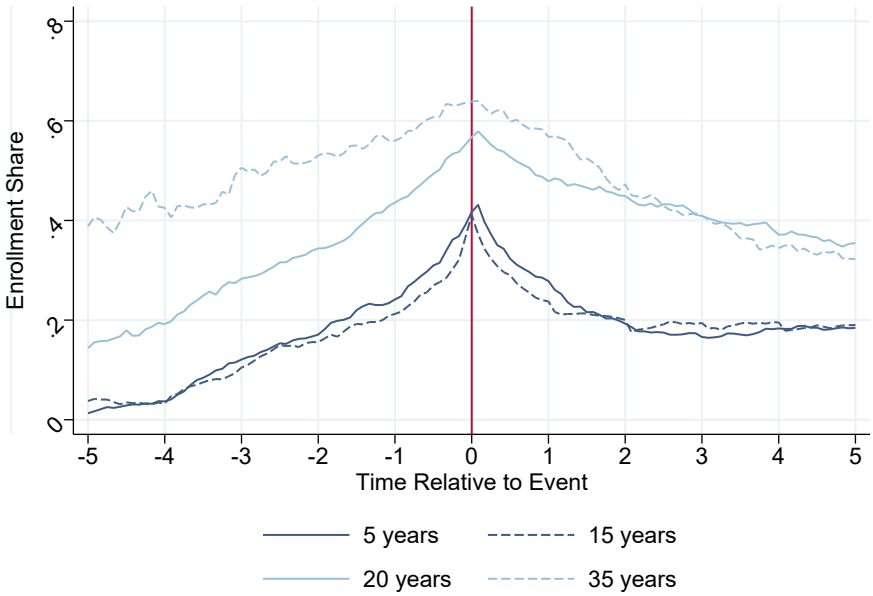
As described in section 2, enrollment increases the share of tenure that is credited as waiting period to 100%. Pension eligibility requires certain amounts of these waiting periods with different thresholds for different types of pension. For example, general eligibility for old age pension requires a minimum of 5 years and at least 35 years are required for early retirement (see Table A.2 for more details).

Being close to a waiting period threshold increases benefits from enrollment, which may decide about eligibility at the extensive margin for the respective pension type. For instance, an individual with an insurance record of 4 years who works one additional year in a mini-job, will become eligible for a basic public pension payment after this year only if they enroll in the public pension insurance. Without enrollment, less than 12 months will be considered as waiting periods and they will not reach the five year threshold. The same logic applies for the other thresholds. Consequently, we expect higher enrollment for individuals close to a threshold if waiting period regulations are salient to them.

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 actually 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, at least a part of, the mini-job employees understanding the incentives for enrollment that are tied to the waiting period thresholds.

There are two potential mechanisms that may be driving 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 from automatic enrollment.

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 needed one more year (12 more months) of contribution time before they reached the 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 individual are observed 5 years after they reached the threshold.

5.3 Additional Evidence from Survey Data (to be completed)

To better understand the heterogeneity in individual enrollment behavior, we use additional information from linked survey data. The project *SOEP-RV* provides record linkage for a subsample of the administrative data we use for our main analysis and the largest German household survey (Lüthen et al. 2021).

The *SOEP-RV* project is currently in a pilot stage. We cannot include results based on this data set in the present draft of this paper. We anticipate that the data set will be published in spring 2022 and we will add the results then.

6 Conclusion

A large body of literature has studied inertia in settings where a default defines the outcome under passive behavior, including old age savings settings. We know that individuals often do not consider all available information when making decisions and are at least partly inattentive (Gabaix 2019), which leads to inertia. In more complex settings, there are a lot of additional behavioral explanations for observed inertia. Inertia may stem from switching costs (Gabaix 2019, Heiss et al. 2021) if the (perceived) costs for deviating from the default are high. For situations where individuals have to take complex decisions with many potential outcomes, it may root in choice overload (Goda et al. 2020, Iyengar and Lepper 2000). And in automatic enrollment settings specifically, inertia may occur because employees (mis-)perceive a default as an employer's investment advice (Madrian and Shea 2001).

This paper examines the impact of default setting for the public pension enrollment of German low income earners (so-called *mini-job* employees) when the choice menu is simple. Specifically, most of the above channels for inertia are shut down, which allows for a clear analysis of the default's impact on enrollment. Building on administrative data, we show that introducing automatic enrollment significantly increases take-up in this less complex setting. However, compared to the existing literature on auto-enrollment, the effect of going from opt-in to opt-out is rather small.

Only few individuals exhibit true passive behavior defined as always sticking to the default. The majority is best described as never-takers who never enroll. In addition, significant parts of the 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 fruitful than policy makers may hope.

We also observe sizable heterogeneity in the power of the default, which should be taken into account when designing default settings. 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 nudge the "wrong" individuals into enrollment, for example because they are less financially literate. Once the linkage between the administrative pension enrollment data and the survey data from the SOEP becomes available, we will be able to characterize this heterogeneity in much more detail.

References

Becker, Sascha O., Lukas Mergele, and Ludger Woessmann (2020). "The Separation and Reunification of Germany: Rethinking a Natural Experiment Interpretation of the Enduring Effects of Communism." en. *Journal of Economic Perspectives* 34, 143–171. DOI: 10.1257/jep.34.2.143.

Benartzi, Shlomo and Richard H. Thaler (2007). "Heuristics and Biases in Retirement Savings Behavior." *Journal of Economic Perspectives* 21, 81–104. DOI: 10.1257/jep.21.3.81.

Bernheim, B. Douglas, Andrey Fradkin, and Igor Popov (2015). "The Welfare Economics of Default Options in 401(k) Plans." en. *American Economic Review* 105, 2798–2837. DOI: 10.1257/aer.20130907.

Beshears, John, James J. Choi, David Laibson, and Brigitte C. Madrian (2012). *Persistence at the Default Among Low-Income Individuals*. en. Tech. rep., 45.

Beshears, John, James J. Choi, David Laibson, Brigitte C. Madrian, and Sean (Yixiang) Wang (2016). *Who Is Easier to Nudge?* Tech. rep.

Blumenstock, Joshua, Michael Callen, and Tarek Ghani (2018). "Why Do Defaults Affect Behavior? Experimental Evidence from Afghanistan." en. *American Economic Review* 108, 2868–2901. DOI: 10.1257/aer.20171676.

Bucher-Koenen, Tabea, Rob J. Alessie, Annamaria Lusardi, and Maarten van Rooij (2021). *Fearless Woman: Financial Literacy and Stock Market Participation*. Working Paper. National Bureau of Economic Research. DOI: 10.3386/w28723.

Bucher-Koenen, Tabea and Annamaria Lusardi (2011). "Financial literacy and retirement planning in Germany." en. *Journal of Pension Economics & Finance* 10, 565–584. DOI: 10.1017/S1474747211000485.

Bucher-Koenen, Tabea, Annamaria Lusardi, Rob Alessie, and Maarten van Rooij (2017). "How Financially Literate Are Women? An Overview and New Insights." en. *Journal of Consumer Affairs* 51, 255–283. DOI: https://doi.org/10.1111/joca.12121.

Butt, Adam, M. Scott Donald, F. Douglas Foster, Susan Thorp, and Geoffrey J. Warren (2018). "One size fits all? Tailoring retirement plan defaults." en. *Journal of Economic Behavior & Organization* 145, 546–566. DOI: 10.1016/j.jebo.2017.11.022.

Carroll, Gabriel D., James J. Choi, David I. Laibson, Brigitte Madrian, and Andrew Metrick (2009). "Optimal Defaults and Active Decisions." *Quarterly Journal of Economics* 124, 1639–1674. DOI: 10.1162/qjec.2009.124.4.1639.

Chetty, Raj, John N. Friedman, Søren Leth-Petersen, Torben Heien Nielsen, and Tore Olsen (2014). "Active vs. Passive Decisions and Crowd-Out in Retirement Savings Accounts: Evidence from Denmark." *Quarterly Journal of Economics* 129, 1141–1219. DOI: 10.1093/qje/qju013.

Choi, James J., David Laibson, Brigitte C. Madrian, and Andrew Metrick (2004). "For Better or for Worse. Default Effects and 401(k) Savings Behavior." *Perspectives on the Economics of Aging*. Chicago: Wise, David A., 81–126.

Clark, Robert L and Denis Pelletier (2019). *Impact of Defaults in Retirement Saving Plans: Public Employee Plans*. Working Paper. National Bureau of Economic Research. DOI: 10.3386/w26234.

Cribb, Jonathan and Carl Emmerson (2020). "What happens to workplace pension saving when employers are obliged to enrol employees automatically?" en. *International Tax and Public Finance* 27, 664–693. DOI: 10.1007/s10797-019-09565-6.

Deutsche Rentenversicherung Bund (2017). *Datensatz SK 79 für die Versicherungskontenstichprobe gemäß § 1 Abs. 2 RSVwV ab der Erhebung zum 31.12.2016 und für die Sondererhebung über vollendete Versichertenleben sowie für die Sondererhebung zum Projekt SHARE*.

Gabaix, Xavier (2019). "Behavioral inattention." en. *Handbook of Behavioral Economics: Applications and Foundations 2*. Vol. 2. Elsevier, 261–343. DOI: 10.1016/bs.hesbe.2018.11.001.

Goda, Gopi Shah, Matthew R. Levy, Colleen Flaherty Manchester, Aaron Sojourner, and Joshua Tasoff (2020). "Who is a passive saver under opt-in and auto-enrollment?" *Journal of Economic Behavior & Organization* 173, 301–321. DOI: 10.1016/j.jebo.2019.08.026.

Goodman, Lucas (2020). "Catching up or crowding out? The crowd-out effects of catch-up retirement contributions on non-retirement saving." en. *Journal of Public Economics* 188, 104221. DOI: 10.1016/j.jpubeco.2020.104221.

Heiss, Florian, Daniel McFadden, Joachim Winter, Amelie Wuppermann, and Bo Zhou (2021). "Inattention and Switching Costs as Sources of Inertia in Medicare Part D." *American Economic Review* 111, 2737–2781. DOI: 10.1257/aer.20170471.

Imbens, Guido W. and Thomas Lemieux (2008). "Regression discontinuity designs: A guide to practice." en. *Journal of Econometrics*. The regression discontinuity design: Theory and applications 142, 615–635. DOI: 10.1016/j.jeconom.2007.05.001.

Iyengar, Sheena S. and Mark R. Lepper (2000). "When choice is demotivating: Can one desire too much of a good thing?" en. *Journal of Personality and Social Psychology* 79, 995–1006. DOI: 10.1037/0022-3514.79.6.995.

Lee, David S. (2008). "Randomized experiments from non-random selection in U.S. House elections." en. *Journal of Econometrics*. The regression discontinuity design: Theory and applications 142, 675–697. DOI: 10.1016/j.jeconom.2007.05.004.

Lüthen, Holger, Carsten Schröder, Markus M. Grabka, Jan Goebel, Tatjana Mika, Daniel Brüggmann, Sebastian Ellert, and Hannah Penz (2021). "SOEP-RV: Linking German Socio-Economic Panel Data to Pension Records." en. *Jahrbücher für Nationalökonomie und Statistik*. Publisher: De Gruyter Oldenbourg. DOI: 10.1515/jbnst-2021-0020.

Madrian, Brigitte C. (2014). "Applying Insights from Behavioral Economics to Policy Design." *Annual Review of Economics* 6, 663–688. DOI: 10.1146/annurev-economics-080213-041033.

Madrian, Brigitte C. and Dennis F. Shea (2001). "THE POWER OF SUGGESTION: INERTIA IN 401(k) PARTICIPATION AND SAVINGS BEHAVIOR." *Quarterly Journal of Economics* 116, 1149–1187. DOI: 10.1162/003355301753265543.

OECD (2019). *Pensions at a Glance 2019: Country Profiles - Germany*. en. Tech. rep.

Seibold, Arthur (2021). "Reference Points for Retirement Behavior: Evidence from German Pension Discontinuities." *American Economic Review* forthcoming. DOI: 10.1257/aer.20191136.

Appendix

A.1 Tables

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 %).

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: Average Levels of Social Assistance

	State															
Time	01	02	03	04	05	06	07	08	09	10	11	12	13	14	15	16
2009	671	741	637	690	661	684	613	654	667	647	734	597	604	577	581	576
2010	675	755	646	692	670	691	621	662	676	655	643	607	613	587	588	583
2011	692	776	663	707	690	711	636	682	698	670	658	622	628	599	598	601
2012	711	796	685	728	706	732	661	704	722	695	680	641	646	621	640	620
2013	732	816	704	748	731	752	686	727	746	717	695	661	670	646	653	638
2014	752	845	719	760	747	765	700	746	761	727	739	680	691	663	668	654
2015Q1	765	870	732	767	758	782	717	761	775	755	823	689	705	674	686	668
2015Q2	768	868	736	770	763	784	723	766	783	758	830	697	710	682	694	676
2015Q3	771	873	740	772	769	789	730	770	788	763	835	704	714	687	698	679
2015Q4	775	877	742	785	771	791	731	771	789	764	838	705	715	689	700	681
2016Q1	781	884	750	792	779	799	738	784	799	769	845	711	723	695	703	688
2016Q2	783	886	751	791	780	802	739	782	802	772	847	712	724	696	702	688
2016Q3	786	890	753	795	782	805	743	787	804	776	851	720	727	699	706	691
2016Q4	790	876	756	797	784	808	745	789	807	778	852	722	730	700	708	692

Notes: Average gross levels of social assistance in €. 01: Schleswig-Holstein, 02: Hamburg, 03: Lower Saxony, 04: Bremen, 05: North Rhine-Westphalia, 06: Hesse, 07: Rhineland-Palatinate, 08: Baden-Wuerttemberg, 09: Bavaria, 10: Saarland, 11: Berlin, 12: Brandenburg, 13: Mecklenburg-Western Pomerania, 14: Saxony, 15: Saxony-Anhalt, 16: Thuringia. Quarterly data for 2015 and 2016 data stems from `https://www-genesis.destatis.de/genesis/online?sequenz=tabellen&language=en&selectionname=22151-0015` [last accessed: 2020-10-08]. Yearly data for 2009– 2014 was provided by the Federal Statistical Office upon request.

Table A.4: 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 2 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 2 for explanations of variables etc.

Table A.5: 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 3 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 3 for explanations of the variables etc.

Table A.6: 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 3 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 3 for explanations of variables etc.

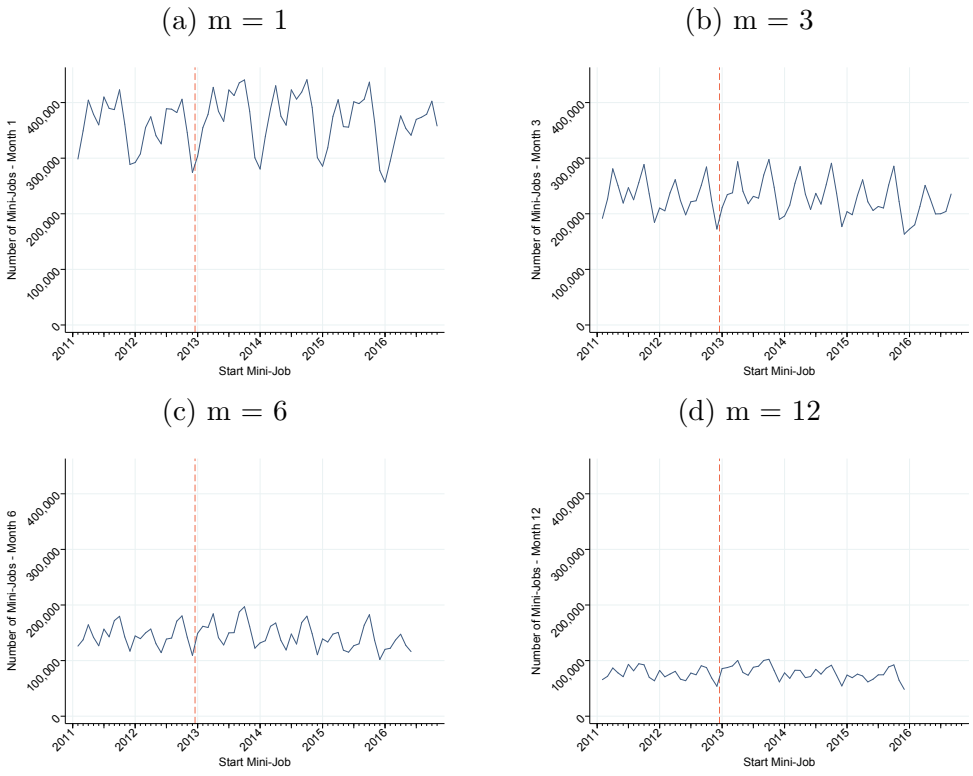
Table A.7: Robustness Check: Income ≤ 400 € ($m = 1$)

	(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 2, but restricting the post reform sample to mini-jobs with income up to the pre-reform income threshold of 400 €, as described in subsection 4.4. See notes in Table 2 for explanation of variables etc.

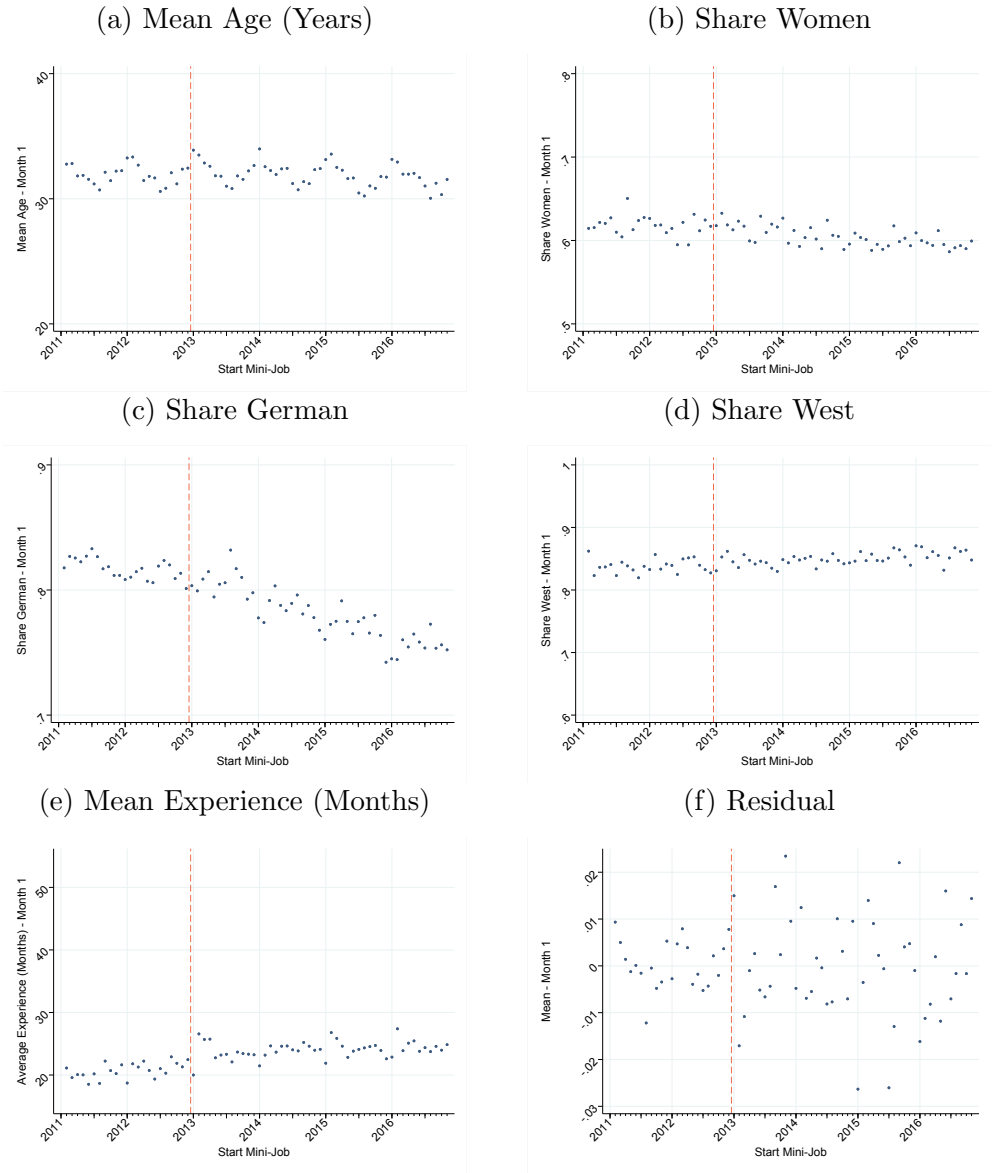
A.2 Figures

Figure A.1: Number of Mini-Jobs



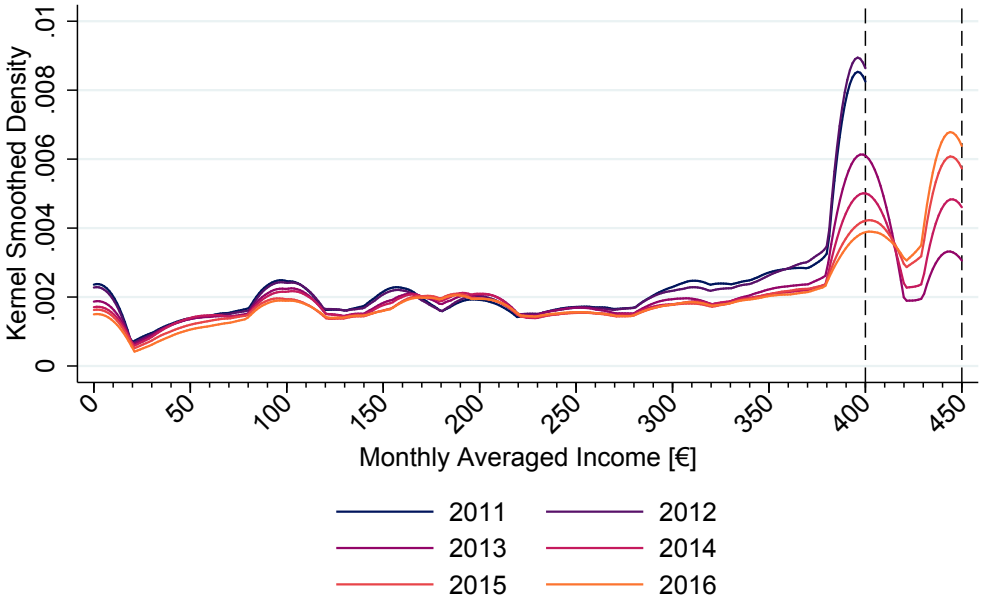
Notes: Weighted data. Graphs display the number of mini-jobs for which we identify a start in month t in the m^{th} month of that mini-job over t. In each panel, the line at given t contains the same group of individuals, exclusive those who dropped out of their mini-job employment before reaching the 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



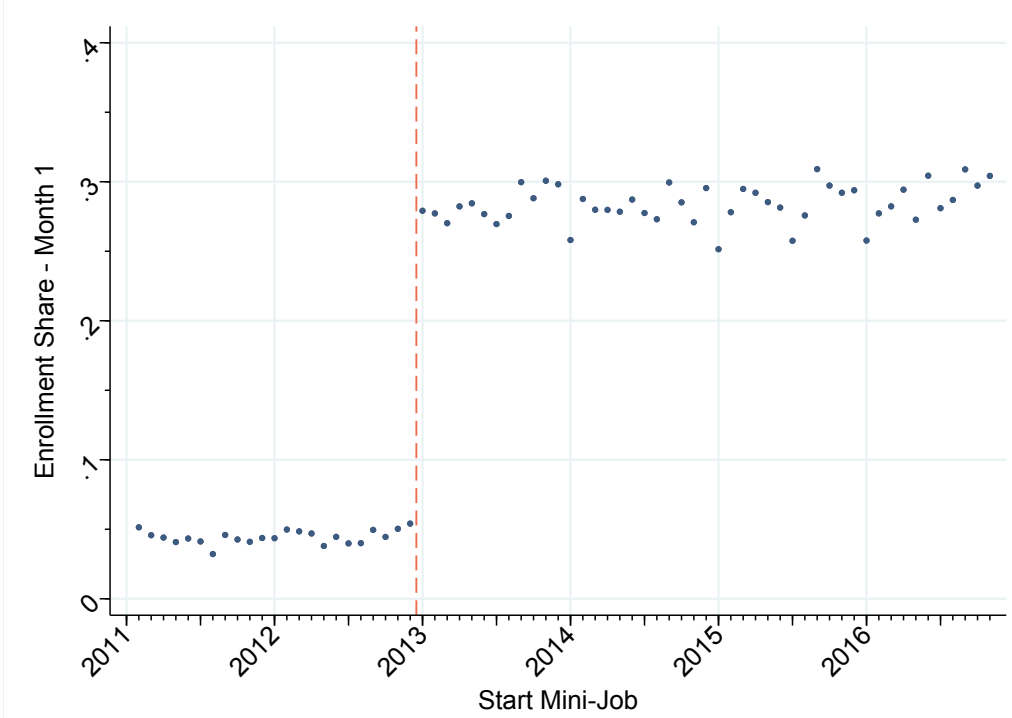
Notes: Weighted data. Scatter plots in panel a to e display the average value for each control variable in the 1^{st} month of the mini-job for individuals who started their mini-job in month t over that t. Panel f displays the average residuals (η_{it}) from Equation 2, again for the 1^{st} month of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

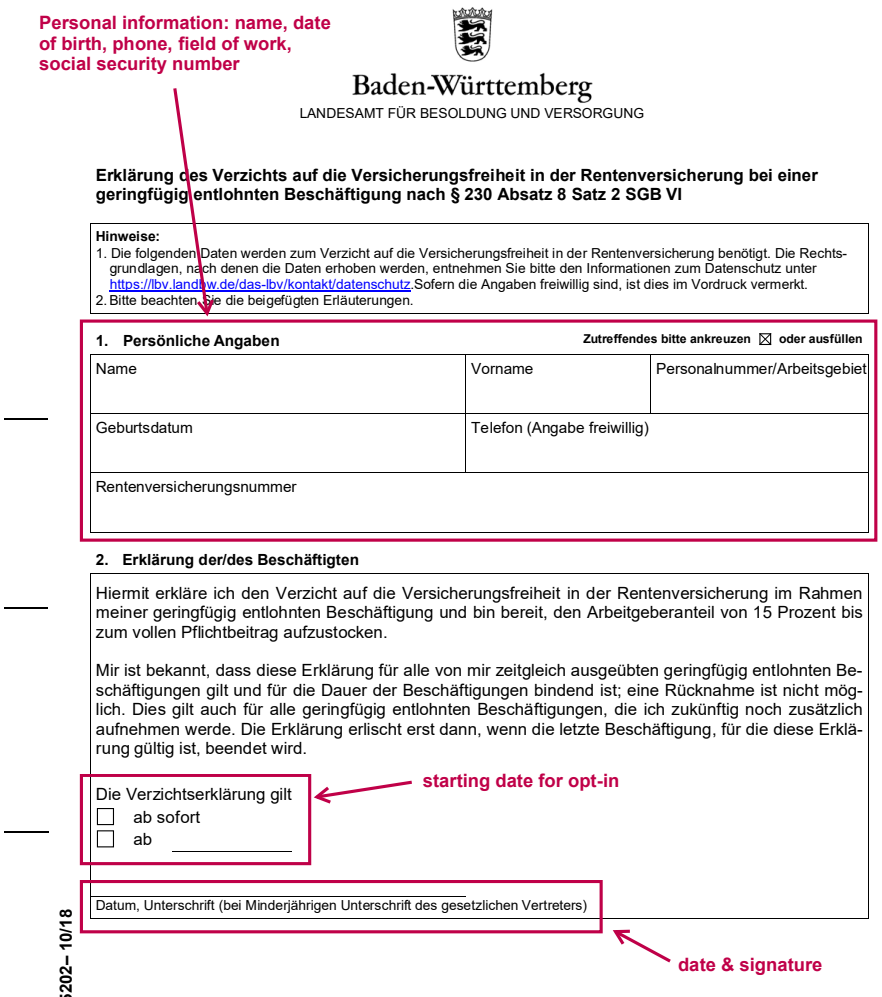


Figure A.5: Example Form for Opting-In for Enrollment

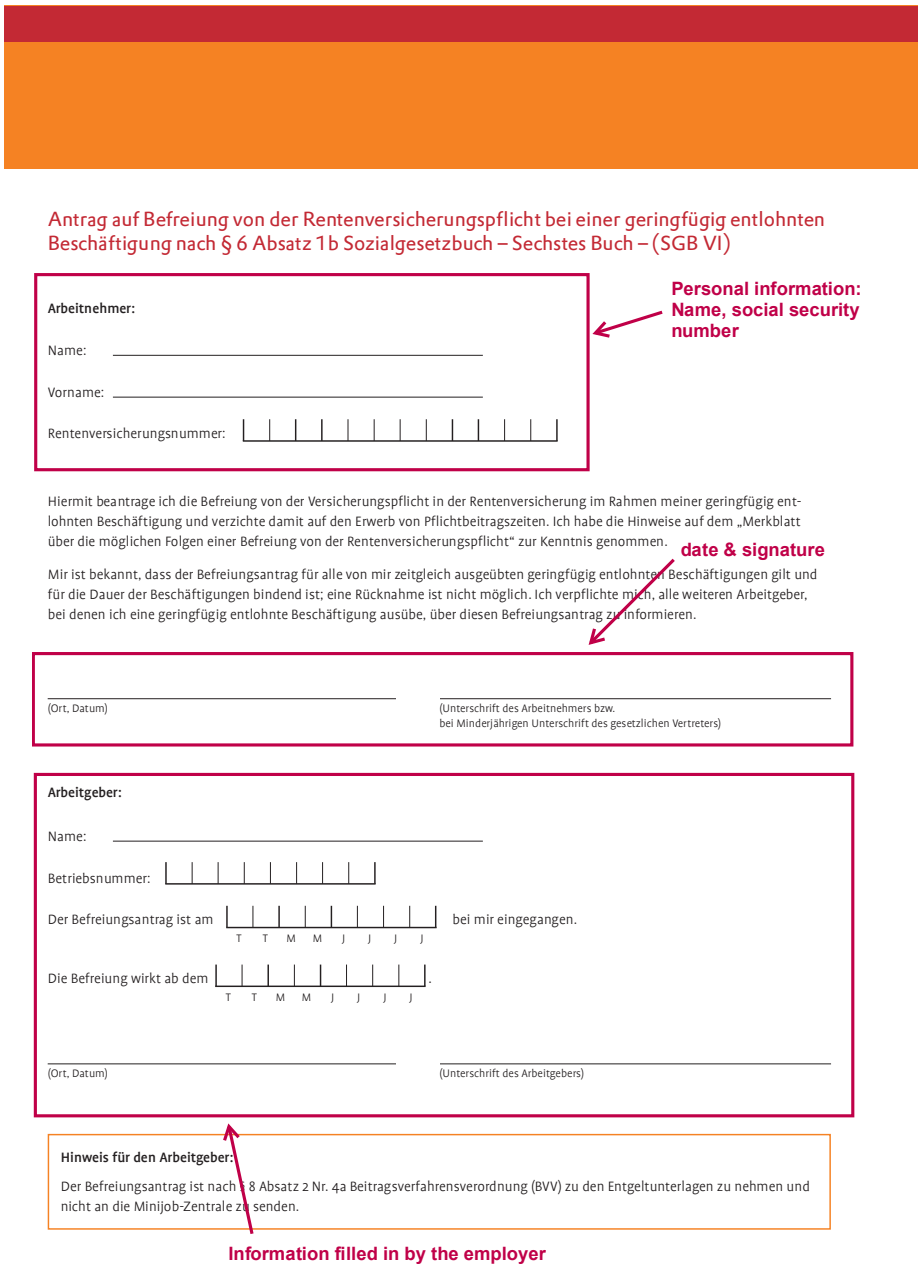


Figure A.6: Example Form for Opting-Out from Enrollment

A.3 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.[16] 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.[17] 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-

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

[17] 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.

A.4 Potential information channels

A significant share of mini-job employees shows no inertia at all when being under the opt-out regime. Two thirds are not enrolled a single month (passive savers, see Table 6), thus opted-out of the default enrollment immediately after taking up their employment.[18] Being attentive to the default situation is a necessary condition for opting out. A natural question to ask is where the employees' attention comes from, or, more specifically, where they learn about the default and how to opt out.

One potential source of information is media coverage. In Germany, it is very common that at the end of a year newspapers and magazines inform their readers about institutional changes that become effective in the subsequent year. These overviews are typically published in late December and feature information on changes in the tax code, social security or other broad interest topics. We studied these overviews for 2012 from three large news papers that included information on the reform for mini-jobs[19]. However, the focus was clearly on the increase of the income threshold

[18] The deadline for opting out for a given month is the Xth of that month.

[19] *Frankfurter Allgemeine Zeitung*: `https://www.faz.net/-gqe-75cu8` (last accessed 2019-12-08); *Welt*: `https://www.welt.de/print/wams/finanzen/article112300864/Alles-neu-macht-der-Januar.html` (last accessed 2019-12-08); *Bild*: `https://www.bild.de/geld/wirtschaft/gehalt/hier-gibt-es-2013-mehr-geld-27784884.bild.html` (last accessed 2019-12-08)

from 400 € to 450 € and much less on the change from opt-in to opt-out. *Bild* (the largest German newspaper) did not even mention the change in default. The *Frankfurter Allgemeine Zeitung* informed about the new default, but without mentioning the possibility to opt-out. In *Die Welt*, the information was more exhaustive, explaining some advantages of being enrolled and that mini-job employees can opt-out.

We investigate Google search volume for "minijob rentenversicherung" (mini-job + pension insurance) and find a clear peak immediately after the reform. The search volume reaches its highest point in January 2013 and has remained constantly above the pre-reform level ever since. However, we cannot say whether this is driven by employers or by employees (or even other individuals).

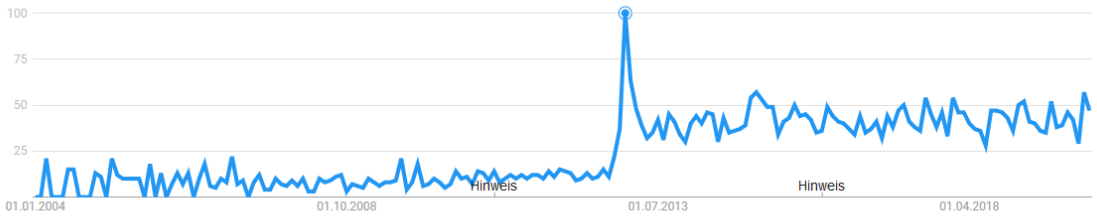


Figure A.4.1: Google Search Volume

Note: Search volume from January 2004 to February 2020. Search term: "minijob rentenversicherung" (mini-job + pension insurance). Highest value for January 2013. Screenshot from https://trends.google.de/trends (last accessed 2020-02-12).