Labor Supply Effects of Changes in Pensions – Regression Discontinuity Evidence from Low-Skilled Workers

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Abstract: To estimate the effects of large cuts in pensions on labor force participation, we exploit three natural experiments in which such cuts affect a group of mostly low-skilled repatriated ethnic German workers. In two of these natural experiments, the pensions were cut by between 8 and 16%, yet, according to our regression discontinuity estimates, there was no significant delay in retirement age (with estimated confidence interval bounds of -72 and +49 days). In the third natural experiment, the workers were given an incentive to avoid a pension cut by retiring earlier, but we find no significant effect for earlier retirement. All these results are consistent with low-skilled workers in Germany being frozen in a corner-solution equilibrium in which the optimal choice is to retire as early as possible. This German case is thus an example of older low-skilled workers' facing few incentives to supply labor in European labor market/social security institutions.

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1 Introduction

Governments have been making pension systems less generous in many industrialized countries. Increasing life expectancies, low retirement ages in many continental European countries like France, Germany, Italy or Spain (OECD, 2011 p. 43), and the fiscal implications of the current financial crisis will keep pension systems in the focus of policy reforms. We might expect these reforms to have positive effects on the labor supply, notably for older workers. This should especially be true in the presence of myopic savings behavior, liquidity constraints, or unexpected pension cuts (cf. Card et al., 2007; see also the illustration in Figure A1). These potential labor supply effects should in turn induce important fiscal effects by increasing tax and social security revenues and decreasing pension fund payouts. The size of these effects, however, depends on the labor supply elasticity of mostly older workers, a factor that is hard to determine empirically because of the rarity of exogenous shocks to budget constraints (wages, pension rights).

Our paper is one of the very few studies to investigate large exogenous cuts in pensions by way of natural experiments. We use administrative data from the German pension register to estimate labor supply reactions to a series of large pension cuts during the 1990s affecting only repatriated ethnic Germans. This group of older repatriated ethnic Germans resembles low-skilled (i.e. without vocational training/apprenticeship) workers in Germany (i) in terms of their job distribution, (ii) in terms of their retirement age distribution, and (iii) in terms of their labor force participation and employment rates before and after retirement (see Section 4 below). As to the job distribution (i), 54% of the male repatriated ethnic Germans aged 55–65 work in blue-collar jobs compared to 56% of low-skilled male German workers aged 55–65. These numbers differ significantly from the 29% figure for all German workers in that age group (see Table 1 for occupational distributions and dissimilarity indices).[1] As to the retirement age distribution (ii), Figure 1 plots the

[1] Many repatriated ethnic Germans might have been regarded as skilled in their source countries, but differences in production methods and working cultures and lack of recognition of educational degrees from former socialist countries, combined with

survival estimates for age at retirement for men born between 1932 and 1936. As the figure shows, the retirement behavior of repatriated ethnic Germans is similar to that of low-skilled Germans. Both these groups have a modal retirement age of 60, with the second most common retirement age being 63. In contrast to qualified Germans, very few men in these groups retire at age 65.[2]

Older low-skilled workers constitute a sizable enough group that they should interest policy makers: 35% of adult men and 39% of adult women are over 55, and 12% of men and 27% of women aged 55–65 are low skilled, as defined by not even having completed an apprenticeship (author calculations based on the German Microcensus 2005).

Low-skilled workers are a key target group of labor market and social policies, because they face low wages so that they either risk belonging to the working poor or—as in many European countries—have limited incentives to work because their potential earnings hardly exceed social benefits.

Our study demonstrates this situation for older low-skilled workers in a continental European economy. More specifically, the three natural experiments investigated provide two types of incentives: The first two experiments reduced pension rates and hence increased the price of leisure, meaning that we would expect workers to retire later. The third provided incentives for early retirement to avoid a pension cut. Because the first set of cuts was based on the repatriation date and enacted retrospectively, it can be analyzed using regression discontinuity designs. However, we find no significant effects of these reforms on labor supply and estimate an upper bound for the extensive life-time Marshallian labor supply elasticity of 0.07 for men. Based on the third experiment, we also observe that workers do not react to incentives for early retirement to avoid a pension cut and thus argue that low-skilled German workers are already retiring as young as is

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language problems, devalued much of their human capital. The attachment of the repatriated ethnic Germans to German culture varied considerably, with some people still speaking German at home, whereas others spoke no German at all so that some repatriated ethnic Germans were seen as "foreign" immigrants by some German observers. According to Bauer and Zimmermann, 1997, p. 365, between 41 and 53 percent of ethnic Germans arriving between 1989 and 1993 were enrolled in German language courses.

feasible according to administrative rules. We therefore conclude that low-skilled men are bogged down in a "corner solution" made up of incentives to retire as early as possible.

The German case investigated here can be seen as an example of how some European welfare systems provide few labor supply incentives/opportunities for older low-skilled workers. Given the high replacement rates of state pensions in many European countries, our study therefore exemplifies how generous social security benefits can impact the labor supply.[3]

Ours is one of the few causal studies on the effects of pension rights reduction on retirement behavior, an analysis made possible by the fact that two of the pension cuts we consider were enacted retrospectively for repatriated ethnic Germans after immigration into Germany, the cuts being dependent on the immigration date. Krueger and Pischke (1992) analyze a pension cut of similar size as that in our study by exploiting the 1977 amendments to the Social Security Act in the United States. Over a transitional period of a few years, these legislative changes gradually decreased the average social security pensions by about 13% for the 1920 birth cohort compared to those of the 1916 birth cohort. Yet the authors find no effect for this large cut on retirement behavior and conclude that the continuing downward trend in male labor force participation in the United States cannot be explained solely by increasing social security benefits, even though these variables are negatively correlated over a long time period in the post-war era.

The two factors used by Krueger and Pischke (1992) to explain their findings—private pensions or private wealth substituting for pensions—cannot explain the absence of any pension cut effect on retirement age in our study. Having returned to Germany from former socialist countries, the repatriates we analyze can safely be expected to have had almost no private wealth or company pension. Rather, their alternative income sources are their spouse's pension/earnings (most women worked in socialist countries)

[2] For women, the corresponding graph is given in Figure A2: both repatriated ethnic German women and low-skilled German women have a modal retirement age of 60, in contrast to skilled German women for whom the modal retirement age is 65.

or support from their children. Although a social welfare program for the elderly was in place when the individuals in our sample retired, the Federal Statistical Office and the German Parliament report that take-up rates were generally low because many elderly shied away from asking their children—who were required by law to support parents in need—to disclose their financial situations (Statistisches Bundesamt 2008; Deutscher Bundestag 2001).[5]

To sum up, given our low labor supply elasticity estimates, we conclude that significant changes in pensions for low-skilled workers in either direction seem mostly to have redistributive consequences, without any significant changes in the labor supply of the affected workers.

The paper is structured as follows: Section 2 sketches the retirement system in Germany, as well as the pension situation for repatriated ethnic Germans and its reforms. Section 3 describes the data source, Section 4 presents the results, and Section 5 concludes the paper.

2 Institutional background: the German public pension system and special rights for repatriated ethnic Germans

2.1 Retirement in the German public pension system

We briefly sketch the key features of the German pension system before explaining the particular rules pertaining to repatriated ethnic Germans. For the cohorts we study, the system was characterized by considerable flexibility concerning the age of retirement, with built-in incentives to retire early. In Germany, the most important component of

[3] It is also worth noting that Germany relies mostly on a mandatory pension system for all employees (except many self-employed workers and civil servants), with company pension plans acting only as supplements.

[5] ISG (1999) also reports that the share of pensioners overall who received social welfare in 1997 was only 1.3% and for repatriated ethnic Germans who migrated before 1993, that figure was as low as 3.3% (ISG 2002). There is no separate figure for repatriated ethnic Germans who are pensioners. However, it is important to note that when the individuals in our sample retired, eligibility determination for social security took into account both spouse's and children's income, as well as other income sources. Hence although pensions for people in our sample were generally low, high labor force participation rates for both men and women in former socialist countries generally resulted in "family pensions" above the subsistence level. Based on these observations, the pension cuts analyzed here could not simply have been cushioned by higher social welfare receipt.

income in old age is the mandatory 'public pension insurance', which covers about 85% of workers (generally excluding civil servants, who have a separate pension system, and self-employed workers, who are mostly voluntarily self-insured; Berkel and Börsch-Supan, 2004). By international comparison, this system is characterized by a high replacement rate of about 70% (according to Börsch-Supan, 2000, p. F29; only 58% according to Boeri and van Ours, 2008, p. 123), meaning that public pension benefits constitute by far the most important source of income for elderly Germans (over 80% of income for households headed by persons over 64 years of age; Börsch-Supan, 2000).

Although the statutory retirement age for men in Germany during our observation period was 65, under certain preconditions, some workers could receive public pension payments earlier, most notably at ages 63, 60 or even earlier. For instance, any individual whose employment history (as far as relevant to the pension insurance) exceeded 35 years could retire flexibly between the ages of 63 and 65. Several other arrangements also allowed workers to receive pension payments as early as age 60; most particularly, the so-called 'reduced earnings capacity' of, for example, workers who were administratively classified as not being 'appropriately employable' because of 'health or labor market reasons'. The eligibility criteria for such pensions were also met if no vacancies were available at the labor office for the worker's specific job description and changing to a different job type would cost the worker an earnings loss of at least 50%.[6] During the 1990s (our observation period), these rules were interpreted liberally enough that Berkel and Börsch-Supan (2004) term them 'soft eligibility rules'. As a result, Arnds and Bonin (2002) suggest that at that time, many individuals had at least some discretionary power to retire as early as 60.[7] In fact, under these same rules, male workers could receive a

[6] More precisely, when these criteria were met, an individual would not necessarily receive a full pension but could be awarded a reduced pension. However, during the 1990s, the underlying rules were interpreted so generously that most workers who retired because of 'reduced earnings capacity' received a full pension. Another pathway to early retirement was a worker's having been unemployed for at least one year out of the previous 1.5 years after having contributed payments to the pension insurance for at least 8 out of the previous 10 years. Individuals could also retire at any age in case of severe disability.

[7] Other possibilities for retiring before age 60 were by way of so-called 'partial retirement plans' or the disability retirement allowable at any age for sufficiently severe disabilities.

pension due to 'reduced earnings capacity' even before reaching 60 as long as they had contributed to the insurance system for at least five years and had worked three out of the last five years (Riphahn, 1997). Women, on the other, hand, could generally retire at age 60 provided they had worked for at least 10 years since age 40.

None of the above retirement schemes, however, were accompanied by an actuarial adjustment of the monthly pension benefit (Arnds and Bonin, 2002). Rather, in the case of early retirement, pension benefits were lowered only because during the years remaining until the regular retirement age of 65, the individual accumulated no additional pension rights. No additional actuarial adjustment was made, however, to take into account the fact that by retiring earlier, individuals increased their expected duration of pension receipt, which, in turn, increased social security wealth (i.e. the expected present value of cumulative pension payments). The lack of any such adjustment created very strong incentives for early retirement, and empirical studies of retirement over time suggest that actual retirement behavior was strongly influenced by changes in these incentives (Börsch-Supan and Schnabel, 1998; Börsch-Supan, 2000).

2.2 Repatriated ethnic Germans

In this study, we evaluate a population that was affected by large cuts in pension rights; namely, repatriated ethnic Germans (*Aussiedler*). Germany, like Israel, has a 'right-of-return' law that allows ethnic Germans to settle in the Federal Republic of Germany as German citizens immediately after arrival. More specifically, according to the German constitution, both citizens *and* refugee ethnic Germans are 'Germans'.

Ethnic German immigration into (West) Germany from 1950 to 2005 amounted to about 4.5 million people (*Wikipedia*), about 5.5% of Germany's total population, many of whom came from the former Soviet Union (2.3 million), Poland (1.4 million), and Romania (0.4 million). Although the criteria for who is an ethnic German and who may immigrate as a citizen have recently been made stricter, the laxer rules in place during

the cold-war period were still in effect during our observation period of the 1990s.[8]

Yet, despite these generous immigration and naturalization laws for ethnic Germans, initially, few ethnic Germans settled in West Germany because the Iron Curtain prevented them from exercising their right to West German citizenship. This situation changed radically in the late 1980s, however, when the law finally felt the effect of the Iron Curtain's fall.[9]

Our telephone calls with both German ministries and organizations representing repatriated ethnic Germans suggest that whole families with two or three generations (grand-parents, parents and children) migrated to leave the economic conditions in Eastern Europe, which looked bleak at the time. Although we could not obtain a statistic on family migration, Krieger *et al.* (2006, p. 32) provide statistics that suggest that by 2006, virtually all ethnic Germans had left the former Soviet Union: Krieger *et al.* (2006, p. 29) report that there were 2,039,341 ethnic Germans according to the Soviet census of January 12 1989. "German nationality" or "Jewish nationality" might even have been mentioned in a Soviet passport. To this day, some former Soviet states mention "nationality" of the person in addition to

[8] The main motivation for the right-of-return law enacted in the (West) German constitution were considerable settlements of people of German decent in central and eastern Europe, as well as in other territories of the former Soviet Union. Many of these settlements have in one way or another remained German in culture and even language. The historical reasons for settlement include Russia's invitation in the 18th century for Germans to settle on its territory and the fact that in the 19th century, large parts of central Europe were part of either Germany or Austria-Hungary, which produced pockets of German-speaking settlements all over central Europe (see Figure C1 for a map). Although many ethnic Germans were forced to leave these territories after the Second World War, some remained for personal reasons like intermarriage or simply because the countries 'forgot' to expel them, meaning that German minorities remained in the Soviet Union, Poland, Romania, and several other countries. Because the Federal Republic of Germany felt that these minorities were disadvantaged in their countries of residence by their German ethnicity, it granted them the right to settle in Germany and become citizens immediately upon arrival. The Federal Republic of Germany, unlike the Republic of Austria, also assumed responsibility for ethnic Germans from former territories of Austria-Hungary. As a result, ethnic Germans from Romania could become citizens of modern-day Germany but not of modern-day Austria, and all East German residents were likewise regarded as West German citizens.

[9] After the initial heavy population movement immediately after World War II, ethnic Germans moved to Germany in relatively small numbers, but with the collapse of the Soviet Union, their number quickly increased again, with 1.5 million immigrating from 1989 to 1993 (see Figure C2). As a result, during the 1990s, German legislation gradually changed until the criteria for approval as an ethnic German refugee became stricter and fewer potentially ethnic German migrants set out for Germany (Bauer and Zimmermann, 1997). For example, although ethnic Germans living abroad can still migrate to Germany, they must now pass a language test. The population studied here, however, immigrated to Germany before 1997 and is hence part of the large influx of the early 1990s.

Soviet "citizenship", according to the so-called "fifth paragraph"—the fifth line in the Soviet passport, abolished by Russia in 1997, but kept, for example, by Kazakhstan (*Wikipedia*). During the years 1989 through 1996, almost three quarters of these, that is 1,391,122 ethnic Germans, had left the former Soviet Union for Germany (Krieger *et al.*, 2006, p. 32). During the early 1990s, about 200,000 ethnic Germans migrated per year. In the year 2000 and after, this number was already below 100,000 and from 1989 through 2005, altogether 2,177,158 ethnic Germans had arrived in Germany from the former Soviet Union, a number that roughly matches the Soviet Census of January 12 1989. In 2005, only 35,386 people migrated, substantiating the view that within the stated period, virtually all ethnic Germans quit the former Soviet Union.

2.3 Pensions for repatriated ethnic Germans

Given that many repatriated ethnic Germans spent large parts of their working lives outside of Germany without paying contributions to the German public pension insurance, an Alien Pension Law (*Fremdrentengesetz*, FRG) was legislated in West Germany in 1959. Under this law, the pension system acknowledged the period of employment in the previous country of residence (e.g. Soviet Union) exactly as if the individual had worked in the same occupation in West Germany. Based on this recognition, it granted repatriated ethnic Germans generous pension rights. Hence, an ethnic German coming to Germany at age 65 after having worked in the Soviet Union for 40 years could go straight into retirement and receive a full pension just like a German-born individual who had worked in Germany for 40 years in the same type of job. Retirement earlier than 65 (i.e. at age 63, 60, or earlier) was similarly possible, because the same rules applied to repatriated ethnic Germans as applied to native Germans: time worked in the source country (e.g. Soviet Union) counted just like time worked in Germany for application of the rules outlined in Section 2.1.

After the fall of the Iron Curtain, however, this rule led to a significant drain on the pension system because repatriated ethnic Germans (and East Germans) could receive pensions without ever having paid into

the system. The outcome was a series of reforms cutting these repatriated ethnic Germans' pensions. For instance, for most of the immigration cohorts in our study, although all years worked in the source countries (e.g. Soviet Union) still counted as active work, the pension level was calculated based on East German rather than West German pay scales.[10]

In the German public pension system, pension rights are usually based on the contributions made by employees over their working life, which are translated into so-called 'earnings points' that reflect the employee's earnings position relative to other workers in the economy. One earnings point corresponds to the average earnings in the economy in a given calendar year. Therefore, depending on individual earnings in any given year, the individual may gain more or less than one earnings point per calendar year, depending on his or her position in the wage distribution. The pension level is calculated based on the total number of earnings points collected. The reforms we investigate reduced pensions by cutting the number of earnings points obtained by a repatriated German through previous employment in the original country of residence (hereafter, source country).

Having immigrated mostly at a relatively high age (55 and older), the repatriated ethnic Germans studied in this paper spent most of their working lives outside Germany and their pension rights, rather than being based on actual contributions to the system, were mostly calculated by type and length of employment in the source country. The reforms we investigate that involve cuts in these pension rights, therefore, translate into large reductions of the repatriated immigrants' total pension rights.

The pension level is not, however, a linear function of the earnings points, which explains why the pension cuts observed in the data are smaller than the original cuts in earnings points. That is, after being cut according to the described legislative changes, the earnings points earned before 1993 (the date after which this rule was repealed) were increased

[10] First, repatriated ethnic Germans are assigned to a 'qualification group' according to a supplement to the German social security law that classifies the educational attainment of repatriated ethnic Germans into five categories. The worker's former job is then allocated to one of 23 industries. To simulate the earnings points for the pension rights, each qualification-industry combination has a hypothetical income assigned for each calendar year since 1950. The data used for analysis, however, do not include

again so as not to fall below a certain threshold. In other words, the German public pension insurance 'beefed up' low pension levels by raising part of an individual's pension by up to 50%.[11]

3 Pension reforms for repatriated ethnic Germans during the 1990s and corresponding administrative data

During the 1990s, repatriated ethnic Germans effectively faced several cuts in the pension rights they had accumulated outside Germany. In order to exploit these pension reforms as natural experiments that allow estimation of low-skilled workers' reactions to unexpected cuts in pension benefits, we first briefly describe both the reforms and the corresponding administrative data. More detailed descriptions of the reforms are provided in German by both Polster (1990, 1992, 1997) and Heller (1997).

Our administrative data are taken from the Federal German Pension Insurance (Deutsche Rentenversicherung Bund, DRV-Bund), the mandatory state pension system for most German workers, which began providing access to a sample of its administrative data in 2005. We obtained remote access to the complete population of pension data on repatriated ethnic Germans for the calendar year 2008, the only year for which 'date of immigration' (accurate to the day) – a necessary variable for our regression discontinuity analyses – was available. We base our analysis on the full population of ethnic German immigrant birth cohorts covered in the data set.

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information on the qualifications and industries used for this simulation, only the number of earnings points accumulated by each individual.

[11] This rule, which increased only the part of a pension based on social security-relevant activities (primarily, dependent employment) before the year 1993, has not yet been replaced by any other variant of a minimum pension for pension earnings points gained after 1993.

More specifically, if the average of the 'earnings points' per year of dependent employment accumulated before 1993 is below 0.75 (i.e. 75% of the average wage), these earnings points are either increased by 50% (if 1.5 times the average earnings points is less than 0.75) or are raised so that the average is exactly equal to 0.75 (if 1.5 times the average earnings points is more than 0.75). These rules imply an attenuation of any cut in earnings points that would cause the average earnings points per year of dependent employment before 1993 to fall below 0.75 for a given pension. However, because the effective pension floor depends on a person's years of social security-

These administrative data provide personal information on the entire population of repatriated ethnic Germans who retired before 2008 and were still alive in 2008. This raises the question whether sample selection due to differences in mortality between treatment and control groups bias our results (see the studies by Snyder and Evans, 2006 and Jensen and Richter, 2003, on mortality effects of pension cuts in the United States and Russia, respectively). For lack of precise information on the date of immigration, the mortality file of the administrative data does not lend itself to the same kind of regression discontinuity design that we employ below. Our rough attempt—based on year of immigration—suggests that mortality was either not affected or decreased slightly due to the reforms. We do, however, not present the results here, because we question their causal interpretation. The results are available upon request.

The administrative pension data include variables such as the pension level in euros, year and month of retirement, individual's age, date of immigration into Germany, and source country. Unfortunately, however, they include no additional socioeconomic characteristics. For the first two natural experiments, we must also exclude from the sample repatriated ethnic Germans who immigrated from Poland. First, this is because a special regulation prevented them from being affected by any of the first two natural experiments. Second, the number of people immigrating from Poland was at a very low level from 1991 onwards (see Figure C2). Hence, the sample sizes for the cohorts we consider are too small for immigrants from Poland to act as a control group in a difference-in-differences identification strategy in the first two natural experiments, but they do act a as control group in the third natural experiment.

In the following, we describe the three reforms that provide the natural experiments analyzed in this study. Some complications arise due to a succession of reforms. Figure 2 therefore provides a timeline with a description of the samples corresponding to each reform. The treatment and control samples have to be defined based on the timing of immigration and the birth years of repatriated ethnic Germans.

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relevant employment (or other activities regarded as equivalent to employment) before 1993, there is no uniform and unconditional minimum pension.

Natural Experiment 1: On July 25, 1991, earnings points acquired abroad (and used to calculate the pension level) were cut by 30% for all repatriated ethnic Germans who had immigrated on January 1, 1991 or later (according to *Renten-Überleitungsgesetz,* TÜG, Art. 14,20a and Art. 15). Due to the nonlinear relationship between earnings points and pensions, actual pensions were reduced by about 8 to 11 percent, only a little less than the 13 percent in the reform analyzed by Krueger and Pischke (1992). Because the legislation was passed after the date of immigration, it amounted to an ex-post reduction in pension rights. Hence, the effect of the reform can be evaluated using a regression discontinuity design that compares the retirement behavior of immigrants arriving shortly before and after January 1 1991. Because the 1st of January is often a date when new laws or regulations are implemented, we checked whether there were any other rule changes affecting the budget constraint of immigrants arriving after that date: we found no such changes.

The oldest cohorts in our estimation samples are individuals who turned 60 in 1992 (and were thus 76 years of age when observed in 2008) and had not yet reached an age when retirement was easily possible when the reform was implemented (i.e. cohorts born on or after January 1 1932). The youngest cohorts are individuals born in March 1936 (who were 72 years of age in 2008) because the retirement behavior of cohorts younger than these were potentially affected by another reform irrespective of their immigration date (see the description of Natural Experiment 3 below).

As part of the regression discontinuity design implementation, we use the immigration date to define a sample that is a subset of the population of repatriated ethnic Germans in these birth cohorts. This subset is restricted to individuals who immigrated between July 1990 and June 1991. Those who immigrated between January and June 1991 comprise the treatment group and those who immigrated between July and December 1990 make up the control group. Two additional discontinuity samples use 'tighter' immigration date windows around the cutoff: workers that immigrated between October 1990 and March 1991 (a 6-month window) and those who immigrated in December 1990 or January 1991 (a 2-month window). Although our administrative data contain the population of repatriated ethnic German pensioners, the sample restrictions by cohort and

immigration date leave us with sample sizes of 2,554, 1,083, and 348 for the three regression discontinuity samples, respectively (see Table A1 and Table A2; the figures for women are 3,405, 1,479 and 482, respectively, see Table A3). We do, however, have to rely on the regression discontinuity design as an identification strategy for lack of sufficient socio-economic control variables in the administrative data. Tobit estimates will take account of the censoring.

When evaluating Natural Experiment 1, we also censor the retirement date relative to May 1 1996 because after the announcement of the reform associated with Natural Experiment 3 (see Figure 2 and the description below in this section; Natural Experiment 3 provided an incentive to retire before October 1 1996), strategic behavior may have occurred to avoid it. Thus, in an attempt to isolate the effects of Natural Experiment 1, we censor retirement date observations for all individuals who had not yet retired by the beginning of May 1996, which results in the censoring of about 20% of our estimation sample's retirement ages.

Natural Experiment 2: On September 25 1996, an upper bound for earnings points (acquired abroad) was introduced for all repatriated ethnic Germans who immigrated after May 6 1996 (according to the *Wachstums- und Beschäftigungsförderungsgesetz,* WFG, Art. 3 and Art. 4, September 25 1996). The limit was 25 earnings points, which, as shown below, effectively amounted to a reduction in actual pensions of between 10 and 16%, similarly to the reform analyzed by Krueger and Pischke (1992).[13] The causal effect of the cut in pension rights on retirement behavior can thus be derived using a regression discontinuity design as long as those immigrating just before versus just after the cutoff date do not differ systematically on other characteristics.

Natural Experiment 2 was generated by the same law as Natural Experiment 3 (see below). In order to separate the effects of these two regulation changes, we consider only men who turned 60 in 1997 or later – that is, the cohorts born on or after January 1 1937. We thereby minimize the number of individuals who could strategically retire and avoid the reform associated with Natural Experiment 3. The youngest cohorts in the

[13] The limit of 25 earnings points applied to singles, the calculation for married couples was more complicated.

sample are those born in December 1941 because individuals in all succeeding birth cohorts might not yet have retired by 2008.[14] For this analysis, the discontinuity samples consist of 12-month (immigrated between November 6 1995, and November 6 1996), 6-month (immigrated between February 6 1996 and August 6 1996), and 2-month (immigrated between April 6 and June 6 1996) sampling windows (with May 6 1996 as the cutoff date). Table A1 and Table A4 show how this leaves us with 1,902, 849 and 319 observations for men in our regression discontinuity samples. The corresponding numbers for women are 2,687, 1,191, and 474, respectively (Table A5).

Note that although the law was passed by the parliament majority on September 25 1996, the government announced the first draft on May 8 1996 (according to the German daily newspaper *Frankfurter Allgemeine Zeitung* of that day). This raises the question whether there was a strategic reaction in terms of changes in migration to Germany after the announcement. However, as we have described in Section 2.2 above, massive differences in economic conditions between Germany and the former Soviet Union led to a mass exodus of virtually all former Soviet ethnic Germans. In order to motivate such a massive population movement over all generations, other factors than pension levels must have played a role, so that we believe that the strategic migration reactions to pension cut announcements were small, because living standards and pensions were still much higher in Germany than in the former Soviet Union (see Jensen and Richter, 2003, on the Russian pension crisis).

Natural Experiment 3: This reform cut earnings points acquired in the source country by 40% for all repatriated ethnic Germans retiring after October 1 1996 irrespective of immigration date. The reform was generated by the same law as Natural Experiment 2 (the *Wachstums- und Beschäftigungsförderungsgesetz, WFG,* Art. 3 and Art. 4).[15] Hence, in contrast to Natural Experiments 1 and 2, which provided incentives for *later* retirement, it provided incentives for men who would normally have

[14] Extending the sample somewhat by including adjacent birth cohorts changes neither the point estimates nor the standard errors in any relevant way.

[15] This reform complemented the reform associated with Natural Experiment 1 in that for immigrants who had not yet retired before October 1 1996, it replaced the 30% rule with a 40% rule. For other immigrants, earnings points were simply cut by 40%.

retired later (i.e. after October 1996) to retire *before* that date to avoid the pension cut.

Since the government announced the plan to cut earnings points by 40 percent already on May 8 1996, workers had enough time to react to the planned reform. This is because, for the effective date of retirement, the date of application, not the date of the administration's decision is decisive (the German term for this administrative rule is *Vertrauensschutz*). For example, if a person applied to retire on August 1 1996, but it took until October to process the application, the date of retirement would still be August 1 1996; this would be decided retrospectively and the more generous regulations valid for August, when the reform was not in place yet, would still apply.[16]

We use two different identification strategies to evaluate this natural experiment, which will be described in Section 4.3 (see also Figure 2). Both strategies rely on immigration cohorts arriving between 1980 and 1990 (most arrived during the second half of the 1980s when perestroika began). Hence, these immigrants were neither affected by Natural Experiment 1 nor by Natural Experiment 2, so that they had most to gain from retiring before October 1, 1996. We include the birth cohorts 1927 through 1942 in our estimation sample, birth cohorts similar to the ones analyzed in Natural Experiments 1 and 2 who were at or close to retirement age around the reform cutoff date. To be included in our sample, a repatriated ethnic German had to be at least 55 years old at the time of retirement. Retirement before the age of 55 is excluded because it is mostly governed by more severe medical conditions and not relevant for the cohorts investigated in Natural Experiments 1 and 2.

Because so many women retire at age 60 and it is difficult to retire earlier, we expect it to be much more likely to observe an effect for men, so that we limit ourselves to this group.

[16] We thank Andreas Dannenberg from the German Pension Insurance (*DRV*) for telephone and written information on these and other pension regulations.

4 Results

To gauge how retirement correlates with labor force participation, we draw additional data from the German Microcensus, because our administrative data set only provides information on the formal act of retirement and not on labor supply.[17] Using 2005 Microcensus data, we find that among repatriated ethnic German men who were not receiving a pension—and who had immigrated since 1990 and were aged 55 to 65—88(66)% were participating (working) in the labor market (sample size n = 393). By contrast, among repatriated ethnic German men who were receiving a pension, 7(5)% were participating (working) in the labor market (sample size n = 299). These numbers are almost identical to those observed for low-skilled workers overall in Germany: among low-skilled German men not receiving a pension—and who were aged 55 to 65—85(68)% were participating (working) while among those receiving a pension only 7(6)% were participating (working) (sample sizes n = 1637 and n = 1525, respectively).[18][19]

Koller (1997) reports employment rates for repatriated ethnic Germans aged 50-59 of 61 and 35 percent for men and women, respectively. These figures are similar to our own calculations based on the 2005 Microcensus (for both older repatriated ethnic Germans and older low-skilled German workers in general). From these sources, we derive that about half of the people in our samples are likely to have taken up employment in Germany before retiring.

[17] The only information provided in the administrative pension data is whether a pensioner earns more than €400 per month, at which point the pension is reduced. Only 0.7% of repatriated ethnic Germans who had retired in the previous three years (as of 2008) had had their pension reduced because they were earning more than €400 through work.

[18] Among repatriated ethnic German women who were not receiving a pension—and who had immigrated since 1990 and were aged 55 to 65—64(52)% were participating (working) in the labor market (sample size n = 434). By contrast, among repatriated ethnic German women who were receiving a pension, 13(12)% were participating (working) in the labor market (sample size n = 407). For low-skilled German women, however, the corresponding labor force participation (employment) rates are somewhat lower at 45(39)% and 10(9)%, with sample sizes n = 4314 and n = 3668, respectively (the samples sizes for women are much higher than the ones for men, because women of this generation were much more likely to be low-skilled than men).

[19] For lack of data, we do not analyze the relationship between unemployment benefit receipt and (early) retirement here. Tatsiramos (2010) shows that low employment rates of older workers in Europe can partly be explained by generous unemployment benefits which often act as a "pathway [in]to early retirement". Similar effects are found by Lalive (2008) for Austria and Kyyrä and Ollikainen (2008) for Finland.

The employment shares of repatriated ethnic Germans who are not and who are retired suggest that measuring labor supply based on retirement might overestimate labor supply elasticities, meaning that the labor supply elasticities reported below can be seen as upper bounds on the true elasticities. It seems that for both men and women, the decision to retire is highly, albeit not perfectly, correlated with the decision to stop supplying labor.

4.1 Effect of the two pension cut reforms on age at retirement

Table A2 through Table A5 list the population means for men and women for the first two natural experiments that consist of ex post pension cuts for people having immigrated after the corresponding cutoff date. Because the administrative data cover so few sociodemographic characteristics, we must rely on the regression discontinuity design to identify the causal effect of pension cuts. The only sociodemographic characteristic that allows assessment of the 'balancing quality' of the regression discontinuity sampling windows is the source country. As Table A2 and Table A3 demonstrate, for Natural Experiment 1, the samples with a 12-month window for an immigration date around the cutoff point are not well balanced in terms of source country (i.e. the treatment group is more likely to immigrate from the former USSR than from Romania). As soon as we consider a 6-month (or 2-month) sampling window, however, the treatment and control groups are well balanced for this variable. Nevertheless, although we control for source country and immigration date in the estimates reported below, the paucity of socioeconomic information in the administrative data leads us to regard the 6-month sampling window as more reliable for the estimation of causal effects than the 12-month window. By the same token, the 2-month sampling window provides an even more credible regression discontinuity design identification strategy, although the standard error is comparatively large because, compared to the 6-month sampling window, the number of observations is limited.

Table 2 and Table 3 summarize the effects of the reforms in terms of the effective pension cuts for men and for and women combined, respectively (Table A7 presents the results for women only), and Table 4

and Table 5 present the corresponding estimates for age at retirement (Table A8 reports the results for women only).[21] Graphical illustrations of the effective pension cuts due to Natural Experiments 1 and 2 and the survivor estimates for age at retirement for treatment and control groups are exhibited in Figures 3 and 4, respectively. These figures already suggest that whereas pensions were cut significantly in both natural experiments (Figure 3), in neither of them did the retirement age distribution change very much (Figure 4).

In Table 4 and Table 5, we estimate the following regression discontinuity model:

$$y_i = \alpha + \tau 1\left(z_i > c_z\right) + \delta z_i + \beta x_i + \varepsilon_i, \quad E\left[\varepsilon_i \middle| x_i = 0\right] \tag{1}$$

where the outcome variable y is the date of retirement (measured by the actual day, although in Germany retirement is only possible on the first day of each month), and z is the date of immigration into Germany (measured by the day). 1() is the indicator function, which equals 1 if the individual arrived in Germany after the critical date and is thus affected by the reform (treated), and x is a vector of the few available control variables (date of birth measured by the month and dummy variables for the source country). By including the birth date as a control variable, we effectively estimate the reform's impact on age at retirement.

In Table 2 and Table 3, we estimate a variant of Eq. (1) in which the dependent variable is the logarithm of the actual pension paid. These estimates are important for identifying the size of the effective pension cut generated by the two reforms. The estimates differ, however, in the sets of control variables used. Whereas Model 1 includes only the date of birth and the source country as controls, Model 2 adds in the date of immigration, with the treatment indicator defined as an additional control. Model 3 then adds in a quadratic term for immigration date that serves as yet another control (see Angrist and Lavy, 1999, for an application of this approach in a different context).

[21] Figure A5 (Figure A7) plots individual pension levels by date of immigration for men (women) for Natural Experiments 1 (Panel A) and 2 (Panel B). The graphs illustrate the pension cuts, including the pension cap introduced with Natural Experiment 2 (Panel B), but also the significant variation in pension levels both above and below the fitted lines due to the absence of a minimum pension.

The regression discontinuity estimates in Table 2 suggest that Natural Experiment 1 reduced the average pension for men by between 8% (12- and 6-month sampling windows) and 11% (2-month sampling window). The standard errors associated with these estimates are 1, 2 and 4 percentage points, respectively. In order to obtain more precise estimates, we will in the following combine both the samples for men and women and the samples relating to the two natural experiments. In the combined sample for men and women, the point estimates are smaller, ranging between 6 and 10% (Table 5), probably because for women, there are no statistically significant pension cuts; point estimates are about 4% (Table A7). This latter might be attributable to the fact that women, although generally exhibiting high labor force participation rates in former socialist countries, on average have gathered fewer earnings points than men, meaning that they were more greatly affected by the rule for 'beefing up' low pensions (see Footnote 11). This interpretation is substantiated by Figure A3, Panel A, which shows that only women with higher pensions experienced pension cuts.

For Natural Experiment 2, the regression discontinuity estimates (columns 2 and 3) of the effective pension cut reported in Table 2 are somewhat higher than those found for the first, with point estimates for men varying between 11 and 18% and those for men and women combined ranging between 15 and 16%. The more narrowly we define the sampling window, the larger the discontinuity estimates. The precision of the estimates remains at between 3 and 4 percentage points for men and 2 and 3 percentage points for men and women combined.

To obtain smaller standard errors for our estimates, we pool the data from both pension cut experiments. Doing so produces pension cut discontinuity estimates of between 9 and 14% for men, with a standard error of 2 or 3 percentage points, and pension cut discontinuity estimates of between 11 and 14% for men and women combined, with a standard error of 1 or 3 percentage points. The largest point estimate is for the 2-month sampling window.

The question remains, however, of how repatriated ethnic Germans reacted to these pension cuts. We report estimates for the reforms' effects on retirement age in Table 4 (men only), Table 5 (men and women

combined), and Table A8 (women only).[22] The first striking result is that none of the estimates are statistically significant (with a lower and upper bound of the estimated confidence interval of -72 and +49 days for the 6-month sampling window combining both reforms and both genders, see below). For Natural Experiment 1, unexpectedly, most point estimates are negative rather than positive. For men, the results based on the 12- and 2-month sampling windows show point estimates close to zero, with the estimated retirement age changing by between -0.06 and 0.03 years (see Models 2 and 3 for the tobit estimates); that is, between -22 and 11 days. The associated standard errors are between 0.15 and 0.37 years (55 and 137 days), respectively, so that the estimated confidence interval is not very narrow around zero for the smallest sampling window (this is why we combine the samples for both reforms and genders below). For the 12-month sampling window, the estimated confidence interval for Model 2 has lower and upper bounds of -96 (i.e. (0.03 - 1.96 x 0.15) x 365) and 118 (i.e. (0.03 + 1.96 x 0.15) x 365) days, respectively. The point estimate based on the 6-month sampling window is more negative at -0.26 years, with a standard error of 0.22 yielding an estimated confidence interval with lower and upper bounds of -0.70 years (-252 days) and 0.17 years (62 days), respectively. The estimates for Natural Experiment 2 are also insignificantly different from zero.[23]

Combining the estimation samples for Natural Experiments 1 and 2 narrows the confidence intervals around zero, with the largest point estimate for men being 0.19 for the 2-month sampling window (with a standard error of 0.31; Table 4, Model 2, bottom). If the samples for men and women and both natural experiments are combined, the maximum point estimate is 0.036 (with a standard error of 0.078; Table 5, Model 2, bottom). For the 6-month sampling window, the estimated OLS confidence interval for Model 3 has lower and upper bounds of -72 (i.e. (-0.031 - 1.96

[22] In this table, whenever we use data from Experiment 1, we estimate both the OLS and tobit models because individuals in the sample used to evaluate this experiment were also affected by a further pension cut if they decided to retire after September 1996. In order not to confound these two reforms, we censor the date of retirement at May 1 1996 and estimate both OLS models using the censored outcome variable or corresponding tobit models to better take account of the censoring. As Table A1 shows, about 20% of the observations in the sample for Experiment 1 are censored. In the sample used to evaluate Experiment 2, in contrast, no outcome variables are censored.

x 0.085) x 365) and 49 (i.e. (-0.031+ 1.96 x 0.85) x 365) days, respectively. As the sample means show, the 6-month sampling window already balances the distribution of the source country well. By narrowing the sampling window even further, down to two months, we obtain a confidence interval lower and upper bounds of -104 (i.e. (0.016 - 1.96 x 0.154) x 365) and 116 (i.e. (0.016 + 1.96 x 0.154) x 365) days, respectively. For women, none of the regression discontinuity design estimates (Models 2 and 3) is statistically significant and the point estimates are even more consistently close to zero than those for men (Table A8). Hence, as already illustrated graphically by Figures 3 and 4, the significant pension cuts seem to have had no significant effects on the age of retirement.

We also tried to produce estimates depending on the size of the effective pension cut, which required us to calculate counterfactual pension levels for treatment and control groups. Our attempts to achieve this failed for both reforms, because (i) a key variable to make such a simulation has too many missing values and (ii) earnings points gained before and after the 1993 abolition of the "beef up" of small pensions cannot be identified separately. Although workers with higher pension levels experienced larger cuts in both reforms, as shown by Figure 3, workers with lower pension levels still faced sizeable cuts, which are statistically significant. These cuts seem to have had virtually no effect on the distribution of retirement age, as the survivor curves in Figure 4 indicate: the confidence intervals for treatment and control groups almost always overlap.

4.2 Implied extensive labor supply elasticities

We then consider what extensive labor supply elasticities these estimates imply. To this end, we want to know how lifetime labor supply reacts to changes in the price of leisure (w-r). However, what we estimate is the reaction of lifetime labor supply to a change in the pension level r. Assuming that the wage rate is unaffected by the reforms in question, we obtain the following relationship between the labor supply elasticity and the effect of a pension change on the retirement age, i.e. what we estimate:

[23] Individual retirement ages by date of immigration are plotted in Figures A6 and A8 for men and women, respectively.

$$\eta_{LFP,\ price\ of\ leisure} = \frac{d\left(years\ worked\right)}{d\left(w-r\right)} \times \frac{\overline{w-r}}{\overline{years\ worked}}$$
$$\approx \frac{\Delta\left(years\ worked\right)}{\Delta\left(w-r\right)} \times \frac{w_{median,MZ} - \overline{r}_{before}}{\overline{years\ worked}}$$
$$\approx \frac{\Delta\left(years\ worked\right)}{-\Delta r} \times \frac{w_{median,MZ} - \overline{r}_{before}}{\overline{years\ worked}} \qquad (2)$$

To estimate the price of leisure, we need to use a data source containing wage information. To this end, we use pensions and net earnings data for repatriated ethnic Germans from the German Microcensus (MZ) and include these figures into the above formula. Based on a net earnings estimate of €1,000 from the microcensus data minus the average pension from the administrative pension data, we estimate the price of leisure for men in the control group to be around €140 per month in Natural Experiment 1 and €400 per month in Natural Experiment 2. For women, we find that net earnings are around €500, and an average pension is about the same or a little higher, yielding a price of leisure that is zero or negative. We therefore focus on men for the simulation of the labor supply elasticity.

The statistics needed to calculate the extensive life-time labor supply elasticity for men—derived from our estimates, the administrative pension data, and the microcensus data—are given in Table 6. The number of years worked is 39 and 36 for the control groups in Natural Experiments 1 and 2, respectively. The table also reports the price of leisure on an annualized basis before and after the reforms associated with the two natural experiments.

The elasticity estimates are given in Table 7, which summarizes the estimates for the first and second terms (derivative and means ratio) of the product that represents the elasticity given in Eq. (2). Based on our standard error estimates for the reforms' labor supply (retirement) effects, we also provide upper and lower confidence limits, treating the means ratio as non-stochastic. As Table 7 shows, both the point estimates and the confidence limits are close to zero. For the first reform, the estimated elasticity is -0.013, with a lower and upper limit of -0.034 and 0.008, respectively. For

the second, the point estimate is 0.008, with lower and upper limits of -0.050 and 0.065, respectively. All these numbers are very small, as would be regarded any number below, say, 0.5.

4.3 Can workers retire earlier to avoid a pension cut?

Having found that pension cuts do not increase the labor supply significantly, we ask whether the reverse is true: are low-skilled workers willing to decrease their labor supply if given incentive to do so? Such an incentive is provided by Natural Experiment 3 for repatriated ethnic Germans. Specifically, repatriated ethnic Germans having immigrated to Germany before 1991 (and hence being unaffected by the first or second natural experiment) faced a 40% cut in earnings points if they retired on or after October 1 1996, although, as already explained, actual pension cuts were smaller, albeit still significant, because earnings points translate nonlinearly into pension levels.

To investigate whether workers evaded this reform, we use two identification strategies (see Figure 2). In the first strategy, we plot in Figure 5 the transition rates into retirement by calendar time to test whether there is any heaping of transitions into retirement in September 1996, that is immediately before the cutoff date. More specifically, in Figure 5, we compare repatriated ethnic Germans affected by Natural Experiment 3 (treatment group) with repatriated ethnic Germans from Poland, who are not affected due to a German-Polish Accord (control group).

It is shown that there is some heaping of transition into retirement shortly before the cutoff (September 1996 and to some extend August 1996) among the treatment group but not among the control group. However, this heaping is hard to distinguish from the amplitude of the general "noise" (variation) in these data. A difference-in-differences estimator for the transition rate differences between treatment and control groups immediately before and after the cutoff date (September versus October) yields an effect of (0.0130-0.0106) - (0.0098-0.0092) = 0.0018, with a standard error of 0.0016. This would be an increase in the transition rate of about 16(21)% in September compared to the level observed in July

(March) for the treatment group. However, this effect is not statistically significant.

In our second identification strategy for Natural Experiment 3, we compare repatriated ethnic Germans who turned 60 before (treatment) or after (control) September 1996, the rule being that one can retire on the 1st day of the month following one's critical birthday. We motivate this regression discontinuity design—based on date of birth instead of date of immigration as in Natural Experiments 1 and 2—by the fact that retirement before age 60 was much more difficult than retirement at or after that age, a fact confirmed by both the regulations discussed in Section 2.1 and the empirical evidence on the survivor functions that show the retirement age distribution.

Table 8 estimates the effects of retiring before the cutoff date on pensions for sampling windows defined on birth dates within 12, 6, 4 and 2 months' intervals, so that the 12-month interval runs from birth months March 1936 through February 1937, and the 2-month interval only includes persons born in August or September 1936. All estimates show that retiring before October 1, 1996 is associated with a 11-13% higher pension level.

Table 9 provides the regression discontinuity estimates of the reform's effects on retirement age (models 2 and 3), where we define the treatment group based on birth date such that a person turns 60 years of age before September 1996. Model 2 controls linearly for birth month, model 3 includes a quadratic term in birth month. Table 9 reveals no significant regression discontinuity estimates. The size of the point estimates suggests that turning 60 before the cutoff date leads to a retirement age about 0.296 * 12 ≈ 3.6 months younger. The standard error for this estimate is 0.191 * 12 ≈ 2.3 months (in both models 2 and 3 which are identical for the 2-month discontinuity sample). If we assume that those people who retire at age 60 because of the reform would have retired at age 63 in the absence of the reform, our point estimate implies that 3.6 / (3 * 12) ≈ 10% of repatriated ethnic Germans react to Natural Experiment 3. Such a result may be interpreted to mean that, by and large, the vast majority of repatriated ethnic German workers retired as early as the regulations would allow and hardly any were able to retire even earlier to avoid the cut in pension rights.

4.4 Traditional and behavioral explanations for the empirical results

Combining the above observations with the estimates of the labor supply effects of exogenous pension cuts as implemented in Natural Experiments 1 and 2, a standard labor supply model would suggest that low-skilled workers like the repatriated ethnic Germans are mired in a 'corner solution' of retiring at the earliest possible date (as learnt from Natural Experiment 3) and that reducing their pensions did not significantly increase their labor supply in the form of later retirement (as learnt from Natural Experiments 1 and 2). Theoretically, this finding can be explained by the fact that in this corner solution, the marginal rate of substitution between leisure time spent in retirement and consumption is higher than the price of leisure, a fact that still held true even after the cuts in the price of leisure brought about by Natural Experiments 1 and 2. This is shortly outlined in Appendix B.[24]

Behavioral economics can provide an alternative explanation why Natural Experiments 1 and 2 have not increased labor supply. We can think of a reference-dependent utility model, where even after the pension cuts, repatriated ethnic Germans are likely to have had pension incomes above their "aspiration levels", thanks to the windfall gains of obtaining a German pension. These aspiration levels may have been defined earlier on in their lives when ethnic Germans still lived in the Soviet Union or Romania (the main source countries for people in our samples). It is unclear whether in the case of ethnic Germans, aspiration levels would not have increased by moving to Germany. Bertrand et al. (2000) show that networks defined by location and language groups have an impact on welfare participation, a finding that might be interpreted such that incomes of members of these networks define aspiration/reference levels, a fact that might prevent or limit the increase of repatriated ethnic Germans' aspiration levels. On the other hand, we observe that the distribution of occupations and the

[24] We assume that repatriated ethnic Germans were informed about the pension cuts analyzed here. This view has been substantiated by telephone interviews we conducted with the Society of Ethnic Germans from Russia (*Landsmannschaft der Deutschen aus Russland*) and with a Russian language publication for ethnic Germans (*Europa Express*; this publication has been existing under a different name since 1995 and covered the reform associated with the second natural experiment extensively).

retirement age for repatriated ethnic Germans are very similar to the ones of low-skilled ethnic Germans. Ethnic Germans might therefore have adapted their aspiration levels to those of other low-skilled Germans.

Hence, not only the job and retirement outcomes of these two groups, but also their reactions to reforms as the ones we analyze may be similar. This would imply that the estimates we have obtained for repatriated ethnic Germans are informative for low-skilled German workers in general. Of course, because there was no such reform for other low-skilled Germans, we cannot test whether this reasoning holds empirically.

5 Conclusions

This paper is one of the very few studies to investigate large (between 8 and 16%) exogenous cuts in pensions. We analyze three natural experiments, of which two natural experiments involve ex post cuts in pension levels. The findings of both natural experiments are identical: there is no reaction in terms of later retirement. The third natural experiment provides an incentive to retire earlier rather than later—as do the first two natural experiments. Seen in combination, our findings suggest that low-skilled workers in Germany, as represented here by repatriated ethnic Germans, are bogged down in a 'corner solution' of retiring as early as possible, one in which the price of leisure is so low that even the comparatively large pension cuts analyzed here provide no incentives to work longer.

The pension cut reforms analyzed here only affected repatriated ethnic Germans, most of who arrived in Germany from the former Soviet Union or from Romania (immigrants from Poland being exempted from these reforms). We show that the repatriated ethnic Germans analyzed here resemble low-skilled German workers (i) in their job distributions when working in Germany, (ii) in their retirement age distributions, and (iii) both in their probabilities to participate and to work in the labor market, both before and after their retirement.

Overall, our study demonstrates that because European welfare states, of which Germany is an exemplary case, provide few work incentives for older low-skilled workers; for most, quitting the labor market

as early as possible seems the optimal choice. This finding is consistent with descriptive evidence on low effective retirement ages in many continental European economies as reported in OECD (2011, p. 43).

One major policy implication of this finding is that even significant decreases or increases in the pension level—for example, of between 8–16% as analyzed here—have virtually no incentive effect in terms of labor supply and thus have predominantly distributional consequences (assuming the intensive labor supply elasticity to also be low). There thus seems ample scope for redistribution in both directions through changes in the pension level.

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Tables and Figures

Table 1

Occupational distribution [in percent] of workers aged 55–65: repatriated ethnic Germans versus low-skilled and skilled Germans.

	Men			Women		
	REGs	Low-Sk.	Skilled	REGs	Low-Sk.	Skilled
Self-employed w/o employees	6	10	10	3	5	7
Self-employed w/ employees	6	6	12	2	2	5
Home worker (family business)	0	1	1	2	6	4
Civil servant or judge	4	2	11	1	0	8
White-collar employee	29	25	41	42	43	61
Blue-collar employee	54	56	26	49	44	16
Index of dissimilarity to REGs	-	7	28	-	6	33

Note: REG = repatriated ethnic Germans immigrated in 1990 or later; low-skilled workers = employed individuals without even apprenticeship education; skilled workers = employed individuals with apprenticeship education or higher.

*Source:*German Microcensus 2005; author calculations.

Table 2
Effective log pension changes caused by Natural Experiments 1 and 2: men.

	Model 1	Model 2	Model 3
Natural Experiment 1 - OLS			
12-month sampling window	-0.07***	-0.08***	-0.08***
n = 2554	(0.01)	(0.01)	(0.01)
6-month sampling window	-0.08***	-0.08***	-0.08***
n = 1083	(0.01)	(0.02)	(0.02)
2-month sampling window	-0.07***	-0.11***	-0.11***
n = 348	(0.02)	(0.04)	(0.04)
Natural Experiment 2 – OLS			
12-month sampling window	-0.21***	-0.11***	-0.11***
n = 2217	(0.01)	(0.03)	(0.03)
6-month sampling window	-0.14***	-0.13***	-0.13***
n = 989	(0.02)	(0.04)	(0.04)
2-month sampling window	-0.13**	-0.18***	-0.18***
n = 369	(0.05)	(0.04)	(0.04)
Both Nat. Experiments pooled – OLS			
12-month sampling window	-0.09***	-0.11***	-0.09***
n = 4456	(0.01)	(0.02)	(0.02)
6-monthsampling window	-0.09***	-0.10***	-0.10***
n = 1932	(0.01)	(0.03)	(0.03)
2-month sampling window	-0.08***	-0.14***	-0.14***
n = 667	(0.03)	(0.03)	(0.03)

Note: Model 1 controls for date of birth and source country, Model 2 also controls for immigration date (discontinuity design estimator), and Model 3 additionally controls for the square of the immigration date (discontinuity design estimator). Standard errors in parentheses.
Source: Administrative German pension data; author calculations.

Table 3
Effective log pension changes caused by Natural Experiments 1 and 2: men and women combined.

	Model 1	Model 2	Model 3
Natural Experiment 1 – OLS			
12-month sampling window	-0.068***	-0.067***	-0.065***
n = 5959	(0.010)	(0.019)	(0.019)
6-month sampling window	-0.075***	-0.060**	-0.058**
n = 2562	(0.013)	(0.027)	(0.027)
2-month sampling window	-0.064***	-0.089*	-0.095*
n = 830	(0.024)	(0.050)	(0.053)
Natural Experiment 2 – OLS			
12-month sampling window	-0.198***	-0.150***	-0.150***
n = 5336	(0.008)	(0.017)	(0.017)
6-month sampling window	-0.161***	-0.158***	-0.157***
n = 2376	(0.013)	(0.024)	(0.024)
2-month sampling window	-0.157***	-0.161***	-0.160***
n = 907	(0.025)	(0.032)	(0.032)
Both Nat. Experiments pooled – OLS			
12-month sampling window	-0.129***	-0.111***	-0.114***
n = 11295	(0.007)	(0.013)	(0.013)
6-month sampling window	-0.116***	-0.110***	-0.111***
n = 4928	(0.009)	(0.018)	(0.018)
2-month sampling window	-0.106***	-0.140***	-0.134***
n = 1737	(0.017)	(0.028)	(0.028)

Note: Model 1 controls for date of birth and source country, Model 2 also controls for immigration date (discontinuity design estimator), and Model 3 additionally controls for the square of the immigration date (discontinuity design estimator). Standard errors in parentheses.
Source: Administrative German pension data; author calculations.

Table 4
Effects of pension cuts on retirement age: men.

	Model 1	Model 2	Model 3
Natural Experiment 1 – OLS			
12-month sampling window	-0.06	0.00	-0.05
n = 2554	(0.05)	(0.11)	(0.11)
6-month sampling window	-0.04	-0.21	-0.23
n = 1083	(0.08)	(0.16)	(0.16)
2-month sampling window	-0.09	-0.16	-0.14
n = 348	(0.14)	(0.29)	(0.30)
Natural Experiment 1 – Tobit			
12-month sampling window	-0.12	0.03	-0.03
n= 2554	(0.07)	(0.15)	(0.15)
6-month sampling window	-0.04	-0.26	-0.26
n = 1083	(0.10)	(0.22)	(0.22)
2-month sampling window	-0.12	-0.06	-0.06
n = 348	(0.17)	(0.37)	(0.37)
Natural Experiment 2 – OLS			
12-month sampling window	0.09	-0.09	-0.09
n = 2217	(0.08)	(0.16)	(0.16)
6-month sampling window	-0.00	0.05	0.06
n = 989	(0.12)	(0.22)	(0.23)
2-month sampling window	0.14	0.28	0.28
n = 369	(0.19)	(0.37)	(0.36)
Both Nat. Experiments pooled – OLS			
12-month sampling window	0.00	-0.05	-0.03
n = 4771	(0.05)	(0.10)	(0.10)
6-month sampling window	-0.03	-0.09	-0.08
n = 2072	(0.07)	(0.14)	(0.14)
2-month sampling window	-0.04	0.20	0.08
n = 717	(0.12)	(0.25)	(0.25)
Both Nat. Experiments pooled – Tobit			
12-month sampling window	-0.06	-0.01	-0.02
n = 4771	(0.06)	(0.13)	(0.13)
6-month sampling window	-0.03	-0.14	-0.15
n = 2072	(0.09)	(0.19)	(0.19)
2-month sampling window	-0.04	0.19	0.16
n = 717	(0.15)	(0.31)	(0.31)

Note: Model 1 controls for date of birth and source country, Model 2 also controls for immigration date (discontinuity design estimator), and Model 3 additionally controls for the square of the immigration date (discontinuity design estimator). Standard errors in parentheses.
Source: Administrative German pension data; author calculations.

Table 5
Effects of pension cuts on retirement age: men and women combined.

	Model 1	Model 2	Model 3
Natural Experiment 1 – OLS			
12-month sampling window	0.057	0.073	0.050
n = 5959	(0.036)	(0.075)	(0.075)
6-month sampling window	0.069	0.011	-0.014
n = 2562	(0.053)	(0.108)	(0.108)
2-month sampling window	-0.003	-0.068	-0.074
n = 830	(0.092)	(0.204)	(0.208)
Natural Experiment 1 – Tobit			
12-month sampling window	0.043	0.084	0.057
n = 5959	(0.045)	(0.092)	(0.094)
6-month sampling window	0.074	-0.018	-0.019
n = 2562	(0.064)	(0.132)	(0.132)
2-month sampling window	-0.015	-0.041	-0.041
n = 830	(0.110)	(0.234)	(0.234)
Natural Experiment 2 – OLS			
12-month sampling window	0.087*	-0.029	-0.028
n = 5336	(0.045)	(0.092)	(0.091)
6-month sampling window	0.032	-0.057	-0.055
n = 2376	(0.068)	(0.131)	(0.131)
2-month sampling window	0.009	-0.028	-0.020
n = 907	(0.110)	(0.226)	(0.226)
Both Nat. Experiments pooled – OLS			
12-month sampling window	0.071**	0.013	0.031
n = 11295	(0.029)	(0.060)	(0.059)
6-month sampling window	0.042	-0.018	-0.031
n = 4928	(0.043)	(0.086)	(0.085)
2-month sampling window	-0.035	0.060	-0.016
n = 1737	(0.072)	(0.155)	(0.154)
Both Nat. Experiments pooled – Tobit			
12-month sampling window	0.045	0.036	0.031
n = 11295	(0.038)	(0.078)	(0.078)
6-month sampling window	0.058	-0.083	-0.095
n = 4928	(0.055)	(0.109)	(0.109)
2-month sampling window	-0.064	0.016	-0.012
n = 1737	(0.089)	(0.182)	(0.183)

Note: Model 1 controls for date of birth and source country, Model 2 also controls for immigration date (discontinuity design estimator), and Model 3 additionally controls for the square of the immigration date (discontinuity design estimator). Standard errors in parentheses.
Source: Administrative German pension data; author calculations.

Table 6

Statistics for calculating labor supply elasticity.

	(1) Coeff.	(2) Annual pension change	(3) Annual pension before	(4) PoL before	(5) PoL after	(6) Years worked
Nat. Exp. 1	-0.26	-873	10,308	1,692	2,565	39
(s.e.)	(0.22)					
Nat. Exp. 2	0.06	-1,048	7,183	4,817	5,866	36
(s.e.)	(0.23)					

Note: PoL = price of leisure in euros per year; the tables provides the statistics needed for the calculation of the labor supply elasticity as given in equation (2) of the paper. To calculate the elasticity, first, the estimated coefficient, given in column (1) is divided by the change in the price of leisure, which equals the annual pension change, that is column (2) equals column (4) minus column (5). This ratio is then multiplied by the ratio of the price of leisure before the reform, column (4), divided by the average number of years worked before the reform, column (6). The annual pension before the reform, column (3), is given for descriptive purposes.
Source: Administrative German pension data; German Microcensus; author calculations.

Table 7

Estimates of the extensive labor supply elasticity.

	(1) Difference Ratio	(2) Means ratio	(3) Elasticity
Nat. Exp. 1			
Point estimate	-0.00030	43	-0.013
Lower limit	-0.00079	43	-0.034
Upper limit	0.00020	43	0.008
Nat. Exp. 2			
Point estimate	0.00006	133	0.008
Lower limit	-0.00037	133	-0.050
Upper limit	0.00049	133	0.065

Note: Columns (1) and (2) provide the figures for the first and second terms of equation (2), respectively, whose product equals the extensive labor supply elasticity provided in column (3).
Source: Administrative German pension data; German Microcensus; author calculations.

Table 8

Effect on log pensions of retiring before October 1 1996.

Month-of-birth interval around 1 September 1936		Model 1	Model 2	Model 3
12 months	(n = 2,611)	0.113 ***	0.118 ***	0.118 ***
		(0.010)	(0.010)	(0.010)
6 months	(n = 1,342)	0.107 ***	0.112 ***	0.112 ***
		(0.015)	(0.014)	(0.014)
4 months	(n = 926)	0.126 ***	0.132 ***	0.134 ***
		(0.018)	(0.018)	(0.018)
2 months	(n = 492)	0.129 ***	0.128 ***	0.128 ***
		(0.026)	(0.025)	(0.025)

Note: Model 1 controls for date of birth and source country (raw gap); Model 2 also controls for the month of birth, and Model 3 additionally controls for the square of the month of birth. Because we do not observe the exact birth date, models 2 and 3 are identical for the 2-months sampling window. Standard errors in parentheses.

Source: Administrative German pension data; author calculations.

Table 9

Effect on retirement age of turning 60 before September 1 1996 (with possible retirement before October 1 1996).

Month-of-birth interval around 1 September 1936		Model 1	Model 2	Model 3
12 months	(n = 2,611)	-0.090	-0.086	-0.083
		(0.159)	(0.159)	(0.159)
6 months	(n = 1,342)	-0.272	-0.242	-0.243
		(0.230)	(0.229)	(0.229)
4 months	(n = 926)	-0.377	-0.304	-0.237
		(0.300)	(0.299)	(0.324)
2 months	(n = 492)	-0.339 *	-0.296	-0.296
		(0.192)	(0.191)	(0.191)

Note: Model 1 controls for date of birth and source country (raw gap); Model 2 also controls for the month of birth (discontinuity design estimator), and Model 3 additionally controls for the square of the month of birth (discontinuity design estimator). Standard errors in parentheses.

Source: Administrative German pension data; author calculations.

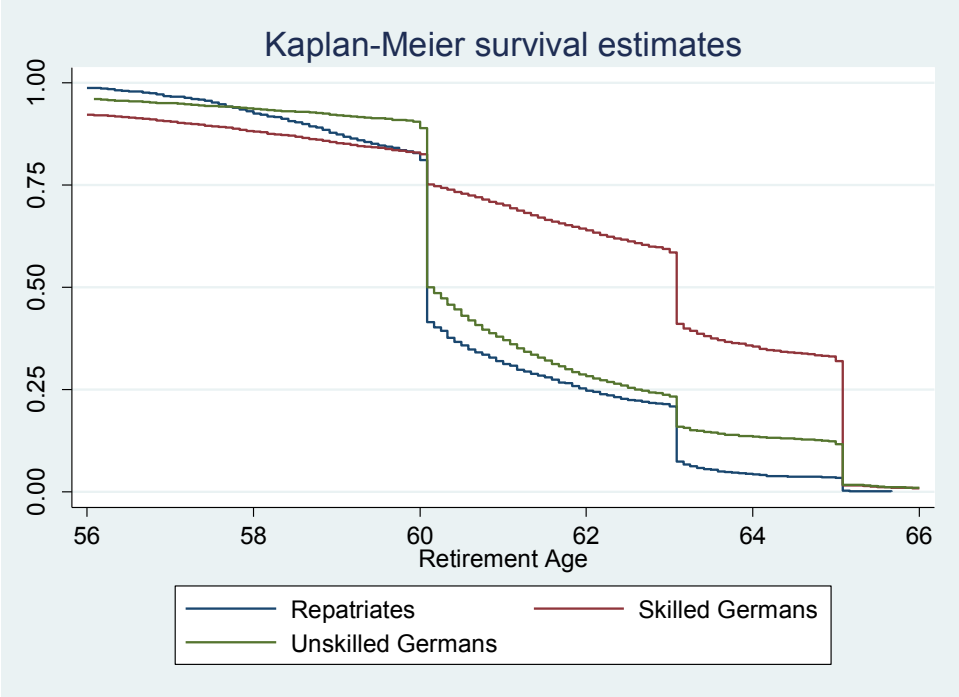


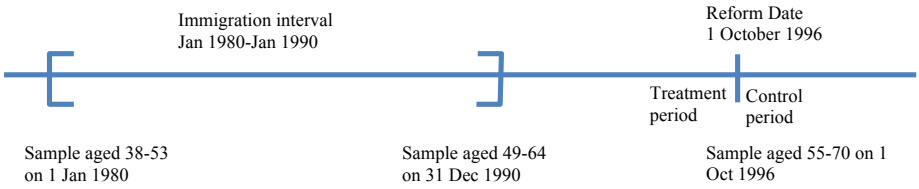
Fig. 1. Survival estimates for age at retirement: men (birth cohorts January 1932 to March 1936, for repatriated ethnic Germans additional restriction: immigration between June 1990 and June 1991).

Source: Administrative German pension data; author calculations.

Natural Experiment 1: Only Birth Cohorts January 1932 – April 1936 [Age Restriction] and Immigration Cohorts July 1990 – June 1991 [Discontinuity Sample] considered:

Natural Experiment 2: Only Birth Cohorts January 1937 – January 1942 [Age Restriction] and Immigration Cohorts November 1995 – November 1996 [Discontinuity Sample] considered:

Natural Experiment 3 (First Strategy): Only Birth Cohorts January 1927 – January 1942 [Age Restriction] and Immigration Cohorts January 1980 – December 1990 [Not Affected By Reforms 1 and 2] considered:



Natural Experiment 3 (Second Strategy): Only Birth Cohorts March 1936 – February 1937 [Discontinuity Sample] and Immigration Cohorts January 1980 – December 1990 [Not Affected By Reforms 1 and 2] considered:

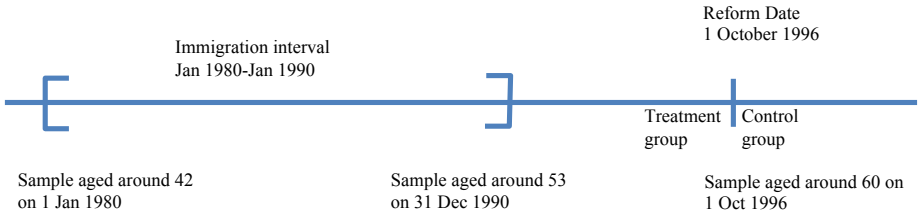
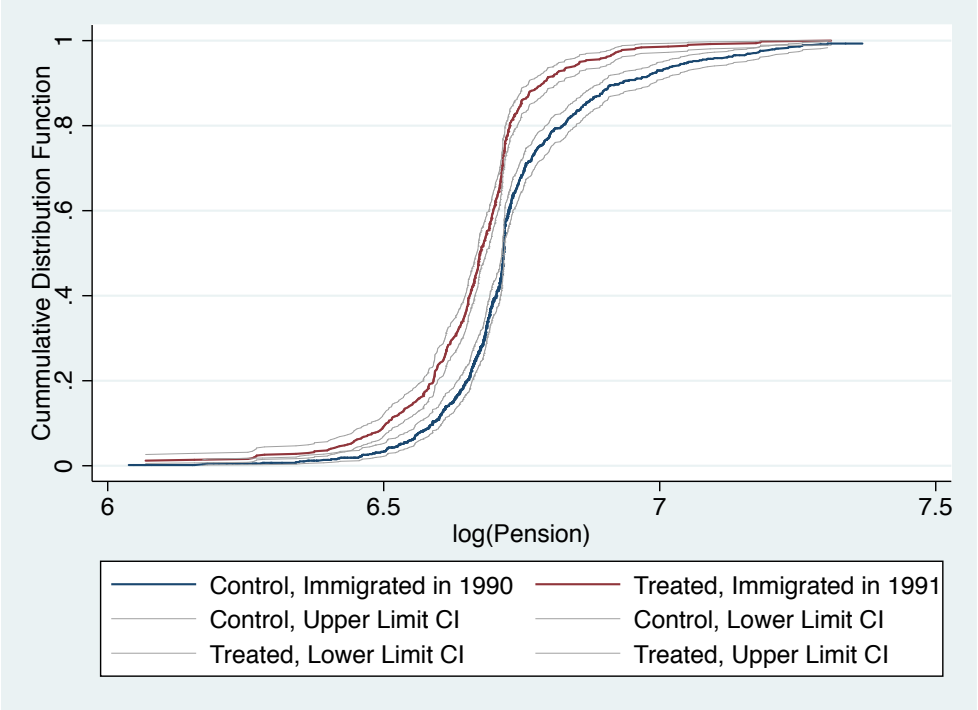


Fig. 2. Timing of Pension Cuts and Sample Restrictions for the Evaluation of Natural Experiments

Note: The birth cohort restrictions are not illustrated graphically, but mentioned in the headings of the panels and in the text boxes concerning age ranges. The square brackets symbolize start and end dates for date of immigration intervals. For the discontinuity designs, these intervals have a width of at most plus/minus 6 months in relation to the cutoff date, that is 12 months at the maximum. This refers to Natural Experiments 1, 2 and 3 (second strategy). Natural Experiment 3 (first strategy) is not a regression discontinuity design.

A. Natural Experiment 1



B. Natural Experiment 2

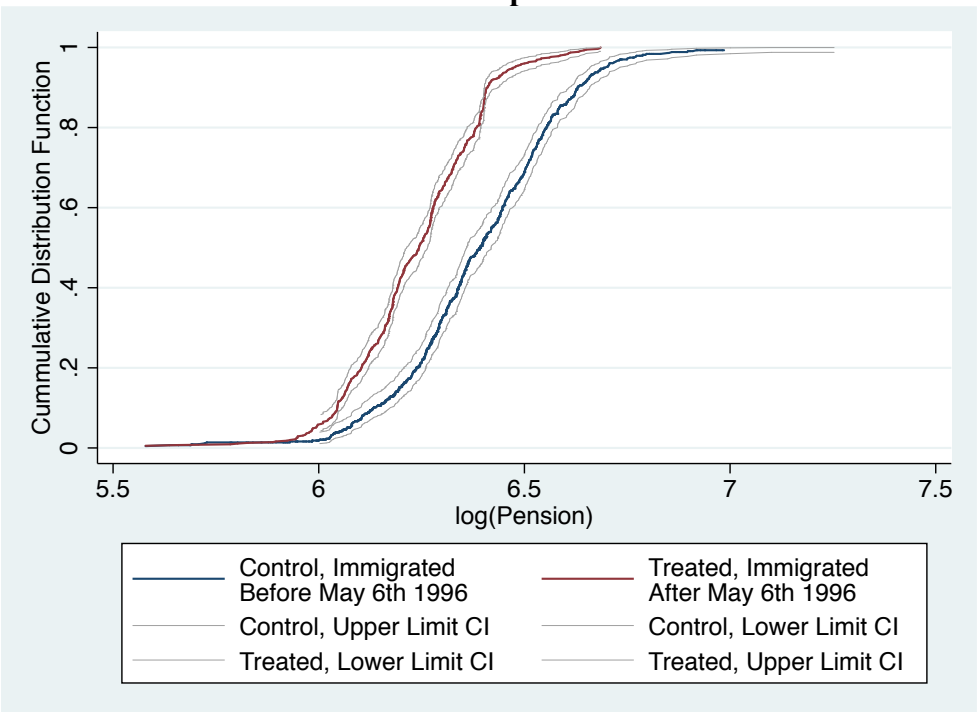
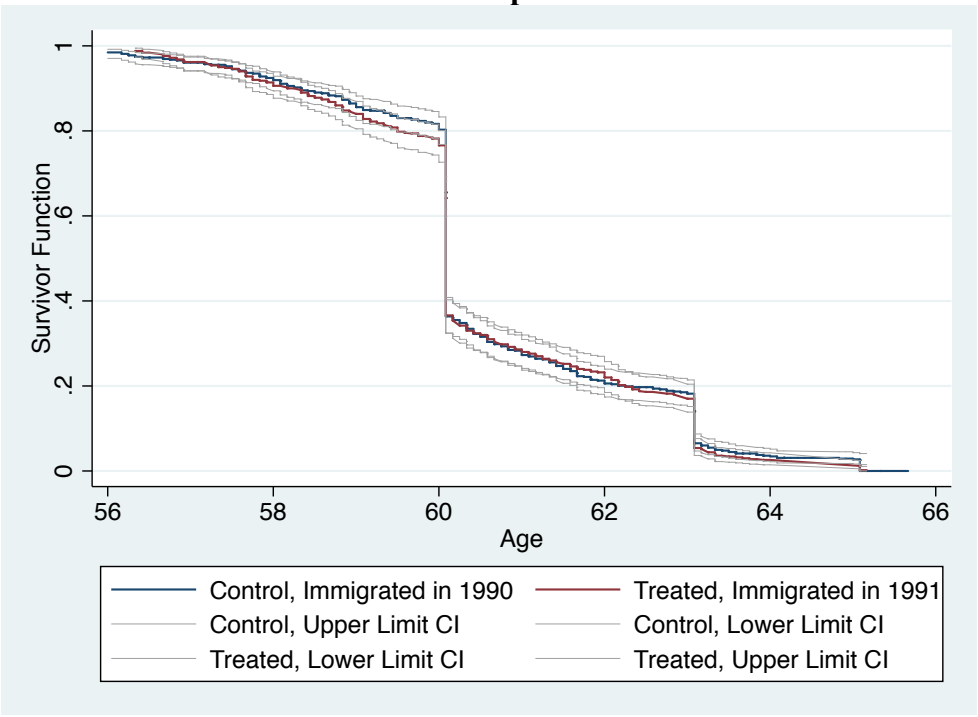


Fig. 3. Effects of pension cuts on the distribution of pension payments. The graphs are based on the data for the 6-month sampling window. The thin grey lines represent 95 percent confidence interval (CI) limits.

Source: Administrative German pension data; author calculations.

A. Natural Experiment 1



B. Natural Experiment 2

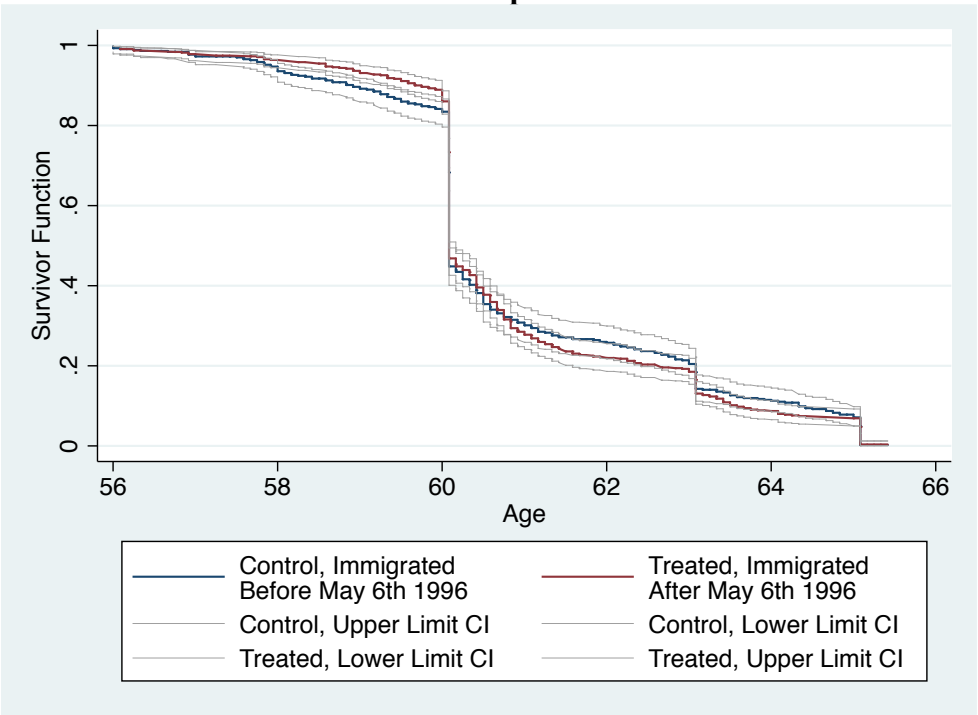
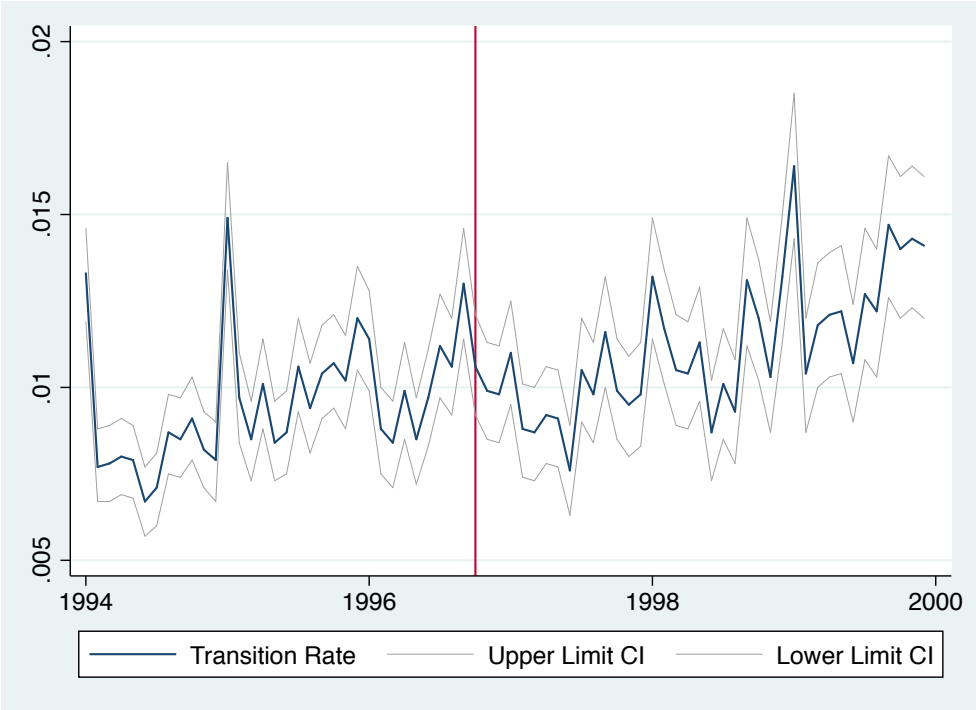


Fig. 4. Effects of pension cuts on retirement behavior. The graphs are based on the data for the 6-month sampling window. The thin grey lines represent 95 percent confidence interval (CI) limits.

Source: Administrative German pension data; author calculations.

A. Treatment Group: Repatriated Ethnic Germans Not Affected By German-Polish Accord



B. Control Group: Repatriated Ethnic Germans Affected By German-Polish Accord

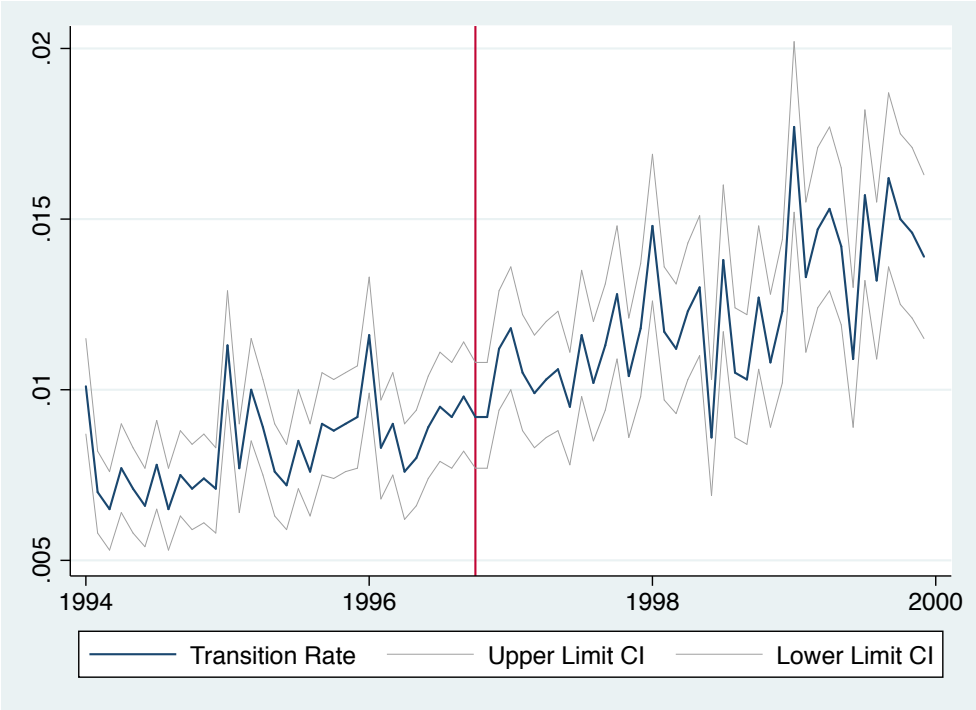


Fig. 5. Transition into retirement rates for repatriated ethnic Germans affected (Panel A) and not affected (Panel B) by Natural Experiment 3. The thin grey lines represent 95 percent confidence interval (CI) limits.

Source: Administrative German pension data; author calculations.

Appendix A. – Additional Tables and Figures

Table A1
Sample selection for Natural Experiments 1 and 2.

	Nat. Experiment 1		*Nat. Experiment 2*	
	All	Former USSR	All	Former USSR
Born Jan. 1932–Mar. 1936/Sep. 1936–Dec. 1941	128,032		188,424	
Males	56,748		84,765	
Excluding former Polish residents	36,223		55,170	
Date of immigration available	35,829		54,359	
Immigrated Jul. 1990–Jun. 1991/Nov. 1995–Nov. 1996	2,645		2,286	
Date of retirement available	2,640	1,567	2,283	
Retired after immigration (Sample 1A/2A)	**2,554**	**1,547**	**2,217**	**2,097**
Immigrated Oct. 1990–Mar. 1991/Feb. 1996–Aug. 1996 (Sample 1B/2B)	**1,083**	**779**	**989**	**939**
Immigrated Dec. 1990–Jan. 1991/Apr. 1996–Jun. 1996 (Sample 1C/2C)	**348**	**270**	**369**	**348**

Source: Administrative data on the German pension insurance.

Table A2
Sample means for Natural Experiment 1 – treatment and control groups in different discontinuity samples: men.

	12-month window		6-month window		2-month window	
	Treatment	Control	Treatment	Control	Treatment	Control
Age at retirement	60.5	60.8	60.5	60.5	60.3	60.5
	(1.76)	(1.96)	(1.86)	(1.85)	(1.71)	(1.79)
Date of retirement	1994.8	1995.0	1994.8	1994.7	1994.6	1994.6
	(2.21)	(2.41)	(2.29)	(2.27)	(2.20)	(2.28)
Date of retirement (censored)	1993.8	1993.7	1993.7	1993.7	1993.7	1993.7
	(1.41)	(1.51)	(1.46	(1.45)	(1.41)	(1.42)
Share – censored	0.23	0.30	0.24	0.23	0.21	0.21
Retired before October 1996	0.81	0.76	0.80	0.81	0.83	0.82
Pension payment in €	786.0	836.7	789.0	859.0	810.7	874.7
	(118.2)	(161.6)	(114.4)	(165.1)	(109.7)	(195.6)
Date of birth	1934.3	1934.2	1934.3	1934.2	1934.3	1934.1
	(1.24)	(1.26)	(1.24)	(1.26)	(1.25)	(1.27)
Age on January 1 1990	55.8	55.8	55.7	55.8	55.7	55.9
	(1.24)	(1.26)	(1.24)	(1.26)	(1.25)	(1.27)
From Romania	0.25	0.45	0.24	0.27	0.17	0.21
From the former USSR	0.72	0.53	0.73	0.71	0.79	0.76
From another country	0.04	0.02	0.03	0.02	0.03	0.02
Number of observations	1,007	1,547	500	583	145	203

Note: Standard deviations in parentheses.
Source: Administrative German pension data; author calculations.

Table A3
Sample means in Natural Experiment 1 – treatment and control groups in different discontinuity samples: women.

	12-month window		6-month window		2-month window	
	Treatment	Control	Treatment	Control	Treatment	Control
Age at retirement	60.23	60.31	60.17	60.05	60.06	60.06
	(1.88)	(2.2)	(1.9)	(1.99)	(1.74)	(1.94)
Date of retirement	1994.4	1994.52	1994.42	1994.26	1994.24	1994.27
	(2.12)	(2.41)	(2.08)	(2.19)	(2.03)	(2.19)
Date of retirement (censored)	1993.83	1993.71	1993.86	1993.68	1993.76	1993.67
	(1.45)	(1.6)	(1.46)	(1.59)	(1.56)	(1.6)
Share – censored	0.12	0.17	0.13	0.13	0.12	0.14
Retired before October 1996	0.9	0.86	0.9	0.9	0.92	0.9
Pension payment in €	675.68	661.04	679.2	711.64	702.83	737.19
	(211.62)	(249.9)	(207.23)	(226.32	(196.54)	(223.62)
Date of birth	1934.17	1934.21	1934.25	1934.22	1934.18	1934.21
	(1.27)	(1.24)	(1.24)	(1.26)	(1.27)	(1.33)
Age on January 1 1990	55.83	55.79	55.75	55.78	55.82	55.79
	(1.27)	(1.24)	(1.24)	(1.26)	(1.27)	(1.33)
From Romania	0.24	0.44	0.23	0.25	0.16	0.17
From the former USSR	0.72	0.53	0.74	0.7	0.8	0.78
From another country	0.05	0.03	0.03	0.04	0.03	0.05
Number of observations	1,339	2,066	676	803	182	300

Note: Standard deviations in parentheses.
Source: Administrative German pension data; author calculations.

Table A4
Sample means in Natural Experiment 2 – treatment and control groups in different discontinuity samples: men.

	12-month window		6-month window		2-month window	
	Treatment	Control	Treatment	Control	Treatment	Control
Age at retirement	60.88	60.8	60.89	60.89	60.84	60.83
	(1.84)	(2.08)	(1.82)	(2.01)	(1.75)	(2.05)
Date of retirement	2000.03	1999.98	2000.01	2000	1999.85	2000.21
	(2.54)	(2.83)	(2.57)	(2.81)	(2.51)	(2.96)
Pension payment in €	514.58	637.96	518.06	607.43	506.97	601.64
	(86.97)	(131.52)	(86.25)	(132.21)	(84.44)	(134.38)
Date of birth	1939.15	1939.18	1939.12	1939.11	1939.01	1939.38
	(1.49)	(1.47)	(1.47)	(1.49)	(1.44)	(1.51)
Age on January 1 1990	50.85	50.82	50.88	50.89	50.99	50.62
	(1.49)	(1.47)	(1.47)	(1.49)	(1.44)	(1.51)
From Romania	0.04	0.04	0.04	0.04	0.06	0.03
From the former USSR	0.95	0.95	0.95	0.95	0.92	0.97
From another country	0.01	0.01	0.01	0.01	0.02	0.01
Number of observations	1,120	1,097	554	435	191	178

Note: Standard deviations in parentheses.
Source: Administrative German pension data; author calculations.

Table A5
Sample means in Natural Experiment 2 – treatment and control groups in different discontinuity samples: women.

	12-month window		6-month window		2-month window	
	Treatment	Control	Treatment	Control	Treatment	Control
Age at retirement	60.22	60.14	60.25	60.21	60.18	60.2
	(1.32)	(1.43)	(1.36)	(1.49)	(1.41)	(1.55)
Date of retirement	1999.39	1999.34	1999.35	1999.44	1999.33	1999.53
	(1.98)	(2.06)	(1.98)	(2.07)	(1.99)	(2.05)
Pension payment in €	495.97	603.6	493.88	589.41	483.28	576.28
	(97.14)	(144.87)	(99.33)	(136.24)	(105.13)	(134.99)
Date of birth	1939.17	1939.19	1939.11	1939.23	1939.15	1939.33
	(1.49)	(1.51)	(1.49)	(1.49)	(1.48)	(1.49)
Age on January 1 1990	50.83	50.81	50.89	50.77	50.85	50.67
	(1.49)	(1.51)	(1.49)	(1.49)	(1.48)	(1.49)
From Romania	0.04	0.05	0.05	0.06	0.06	0.05
From the former USSR	0.94	0.93	0.93	0.92	0.93	0.92
From another country	0.02	0.02	0.02	0.02	0.01	0.03
Number of observations	1533	1586	758	629	268	270

Note: Standard deviations in parentheses.
Source: Administrative German pension data; author calculations.

Table A6

Sample means for Natural Experiment 3 – treatment and control groups in different discontinuity samples: includes individuals born in September 1936.

	12-month window		4-month window		6-month window		2-month window	
	Treatm.	Control	Treatm.	Control	Treatm.	Control	Treatm.	Control
Age at retirement	61.20	61.46	61.26	61.52	61.26	61.41	61.16	61.43
	(2.11)	(2.21)	(2.12)	(2.14)	(2.13)	(2.13)	(2.25)	(2.09)
Retirement date	1997.32	1998.59	1997.64	1998.4	1997.77	1998.15	1997.74	1998.09
	(2.14)	(2.23)	(2.13)	(2.15)	(2.13)	(2.13)	(2.25)	(2.09)
Retired at age 60	0.33	0.29	0.33	0.28	0.35	0.30	0.36	0.31
Pension in €	891.25	827.57	877.71	832.66	873.98	844.42	870.31	859.15
	(193.59)	(160.36)	(187.20)	(159.62)	(187.10)	(164.66)	(200.48)	(161.18)
Date of birth	1936.12	1937.13	1936.38	1936.87	1936.51	1936.75	1936.58	1936.67
	(0.29)	(0.29)	(0.15)	(0.15)	(0.07)	(0.07)	(0.00)	(0.00)
Age on 1/1/1990	53.88	52.87	53.62	53.13	53.49	53.25	53.42	53.33
	(0.29)	(0.29)	(0.15)	(0.15)	(0.07)	(0.07)	(0.00)	(0.00)
From Romania	0.46	0.46	0.45	0.49	0.45	0.50	0.44	0.46
From ex-USSR	0.40	0.44	0.41	0.41	0.42	0.40	0.42	0.43
Other country	0.14	0.10	0.14	0.10	0.12	0.10	0.13	0.11
Number of obs.	1,248	1,363	646	696	444	482	250	242

Note: Standard deviations in parentheses.
Source: Administrative German pension data; author calculations.

Table A7
Effective log pension changes for women caused by Natural Experiments 1 and 2.

	Model 1	Model 2	Model 3
Natural Experiment 1 – OLS			
12-month sampling window	-0.07***	-0.05*	-0.06**
n = 3405	(0.02)	(0.03)	(0.03)
6-month sampling window	-0.07***	-0.04	-0.04
n = 1479	(0.02)	(0.04)	(0.04)
2-month sampling window	-0.06	-0.03	-0.04
n = 482	(0.04)	(0.07)	(0.08)
Natural Experiment 2 – OLS			
12-month sampling window	-0.19***	-0.18***	-0.18***
n = 3119	(0.01)	(0.02)	(0.02)
6-month sampling window	-0.18***	-0.18***	-0.18**
n = 1387	(0.01)	(0.03)	(0.03)
2-month sampling window	-0.18***	-0.15***	-0.15***
n = 538	(0.02)	(0.04)	(0.04)
Both Nat. Experiments pooled – OLS			
12-month sampling window	-0.13***	-0.12***	-0.13***
n = 6524	(0.01)	(0.02)	(0.02)
6-month sampling window	-0.12***	-0.12***	-0.12***
n = 2856	(0.01)	(0.02)	(0.02)
2-month sampling window	-0.11***	-0.12***	-0.11***
n = 1020	(0.02)	(0.04)	(0.04)

Note: Model 1 controls for date of birth and source country, Model 2 also controls for immigration date (discontinuity design estimator), and Model 3 additionally controls for the square of the immigration date (discontinuity design estimator). Standard errors in parentheses.
Source: Administrative German pension data; author calculations.

Table A8
Effects of pension cuts for women on age of retirement.

	Model 1	Model 2	Model 3
Natural Experiment 1 – OLS			
12-month sampling window	0.15***	0.13	0.13
n = 3405	(0.05)	(0.10)	(0.10)
6-month sampling window	0.15**	0.17	0.15
n = 1479	(0.07)	(0.14)	(0.15)
2-month sampling window	0.06	0.00	-0.04
n = 482	(0.13)	(0.28)	(0.29)
Natural Experiment 1 – Tobit			
12-month sampling window	0.15***	0.13	0.13
n = 3405	(0.06)	(0.12)	(0.12)
6-month sampling window	0.16**	0.14	0.14
n = 1479	(0.08)	(0.17)	(0.17)
2-month sampling window	0.04	0.00	-0.00
n = 482	(0.15)	(0.30)	(0.30)
Natural Experiment 2 – OLS			
12-month sampling window	0.09*	-0.00	-0.00
n = 3119	(0.05)	(0.10)	(0.10)
6-month sampling window	0.04	-0.08	-0.08
n = 1387	(0.08)	(0.15)	(0.15)
2-month sampling window	-0.04	-0.08	-0.08
n = 538	(0.13)	(0.27)	(0.27)
Both Nat. Experiments pooled – OLS			
12-month sampling window	0.12***	0.06	0.08
n = 6524	(0.03)	(0.07)	(0.07)
6-month sampling window	0.09*	0.06	0.04
n = 2856	(0.05)	(0.11)	(0.11)
2-month sampling window	-0.01	0.02	-0.02
n = 1020	(0.09)	(0.19)	(0.19)
Both Nat. Experiments pooled – Tobit			
12-month sampling window	0.12**	0.07	0.07
n = 6524	(0.05)	(0.09)	(0.09)
6-month sampling window	0.11	-0.01	-0.03
n = 2856	(0.07)	(0.13)	(0.13)
2-month sampling window	-0.06	0.01	-0.04
n = 1020	(0.11)	(0.22)	(0.22)

Note: Model 1 controls for date of birth and source country, Model 2 also controls for immigration date (discontinuity design estimator), and Model 3 additionally controls for the square of the immigration date (discontinuity design estimator). Standard errors in parentheses.
Source: Administrative German pension data; author calculations.

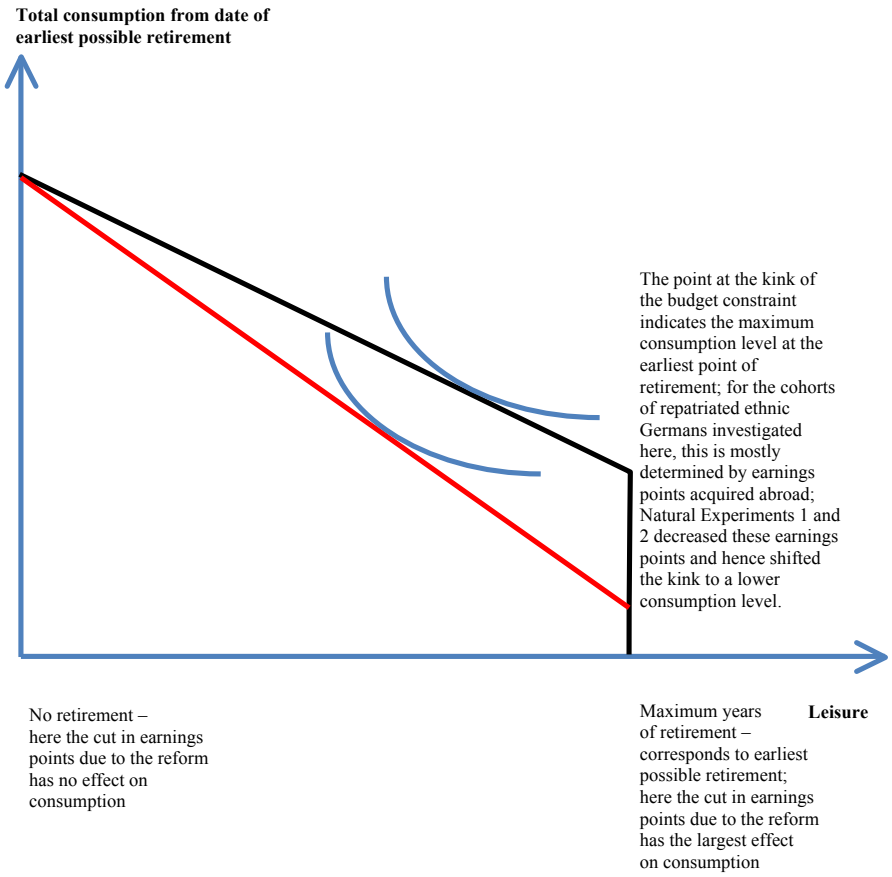


Fig. A1. Illustration why Natural Experiments 1 and 2 theoretically lead to later retirement (less leisure) if workers are initially at an interior solution (note that both substitution and income effects work in the same direction in this case)

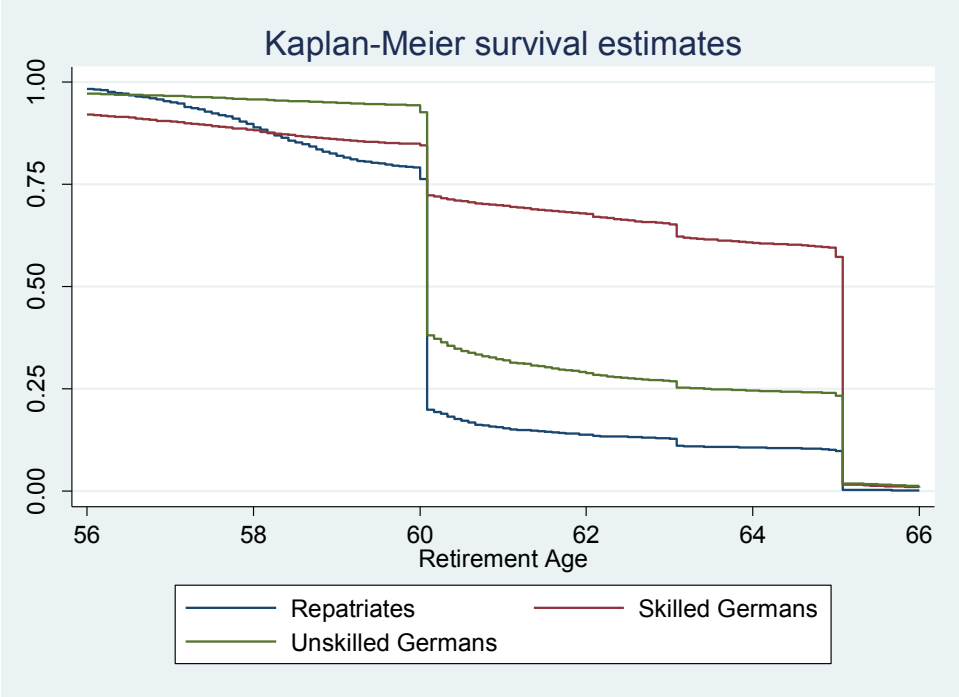
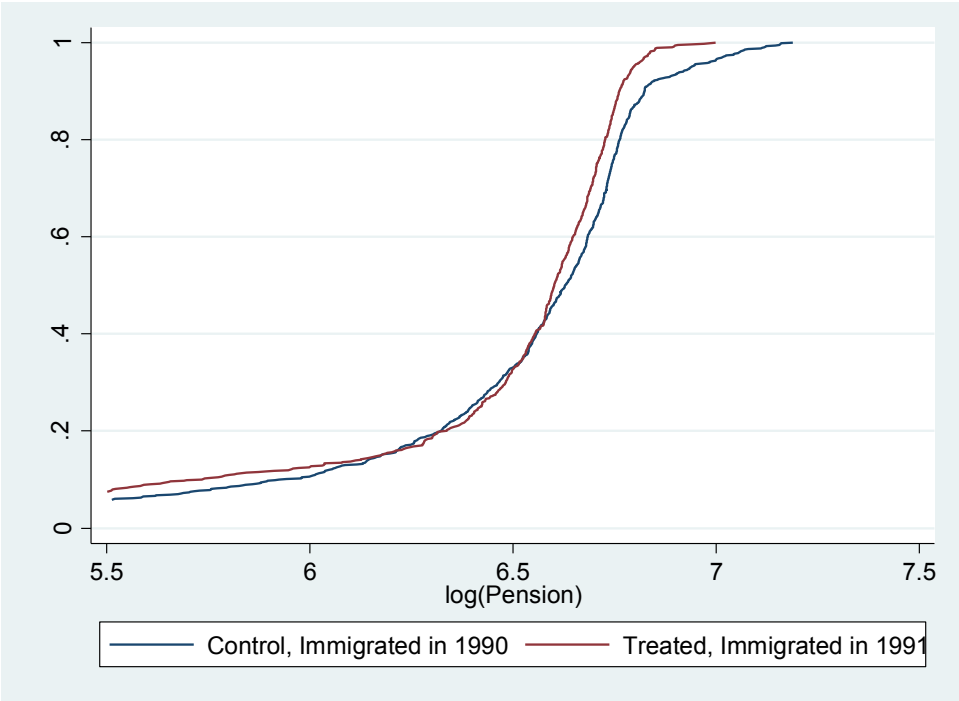


Fig. A2. Survival estimates for age at retirement: women (birth cohorts January 1932 to March 1936, for repatriated ethnic Germans additional restriction: immigration between June 1990 and June 1991).

Source: Administrative German pension data; author calculations.

A. Natural Experiment 1



B. Natural Experiment 2

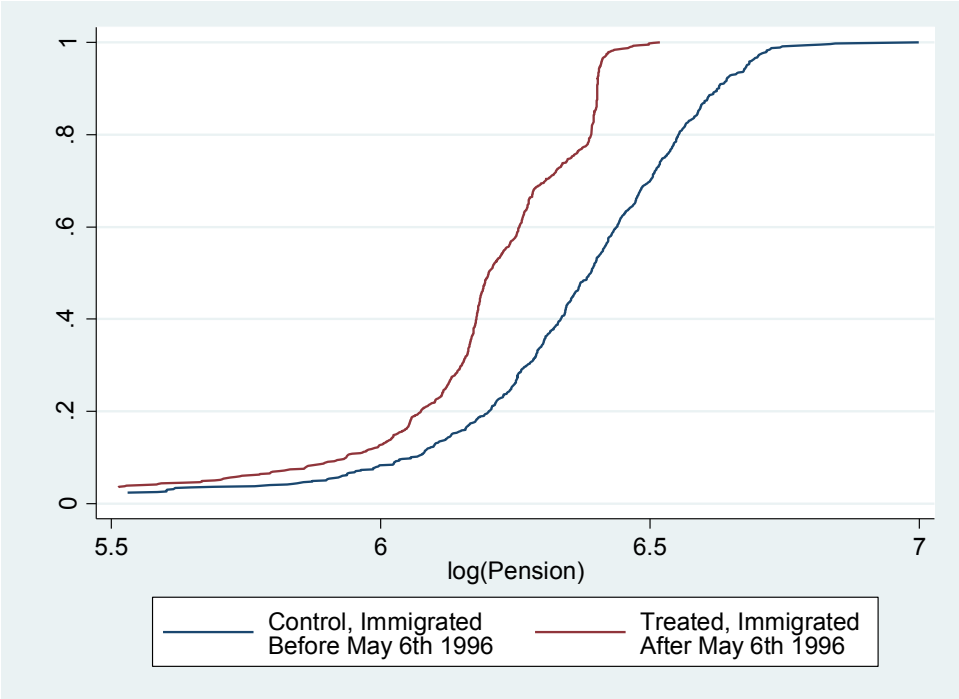
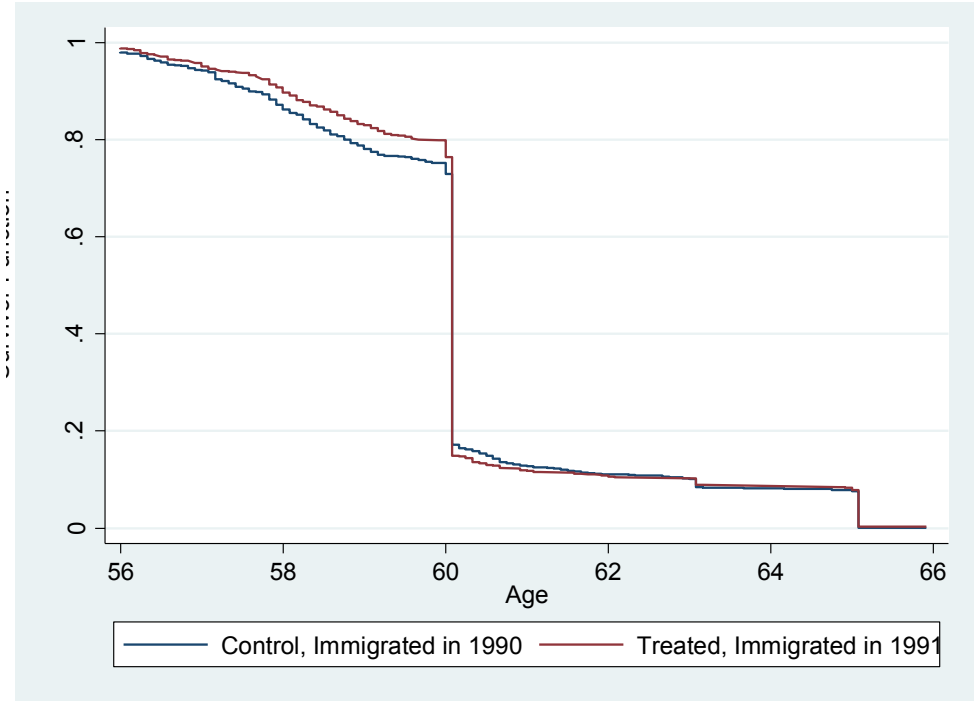


Fig. A3. Effects of pension cuts on the distribution of pension payments for women. The graphs are based on the data for the 6-month sampling window.

Source: Administrative German pension data; author calculations.

A. Natural Experiment 1



B. Natural Experiment 2

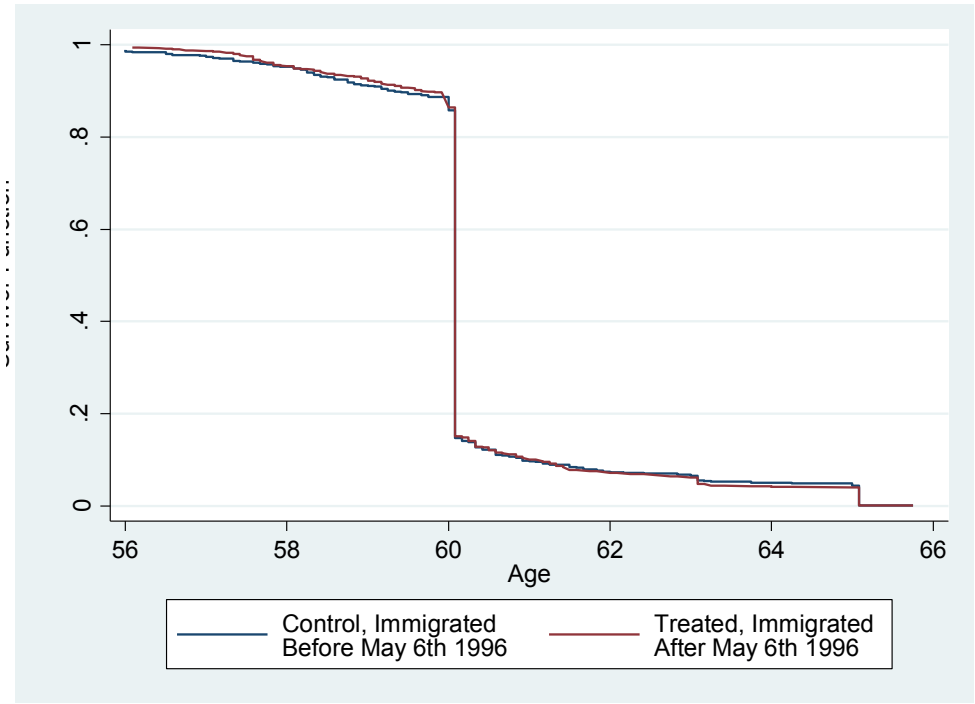


Fig. A4. Effects of pension cuts on retirement behavior of women. The graphs are based on the data for the 6-month sampling window.

Source: Administrative German pension data; author calculations.

A. Natural Experiment 1

B. Natural Experiment 2

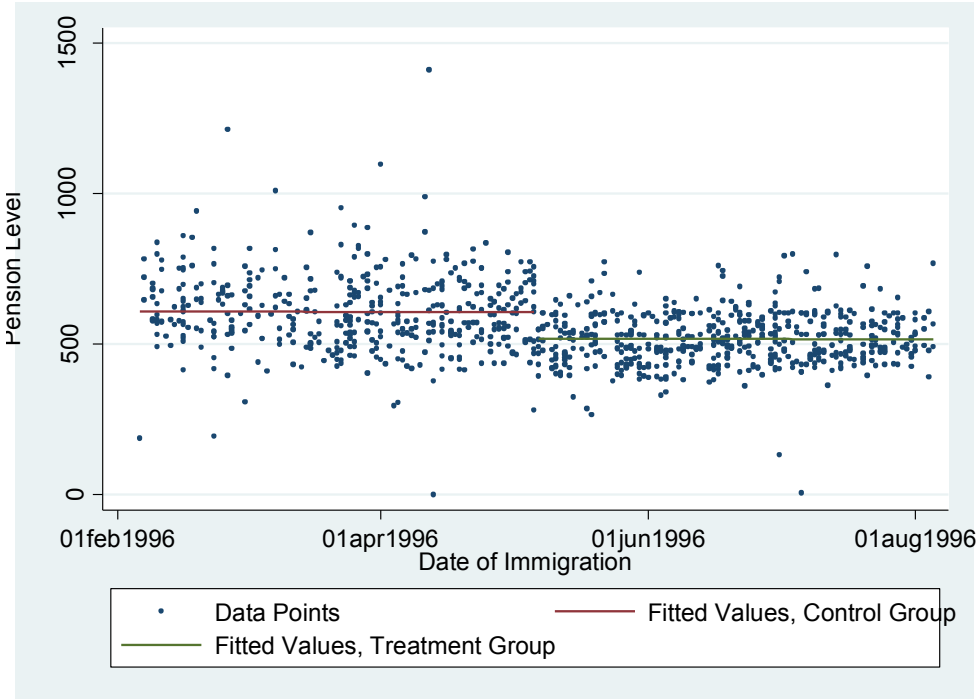
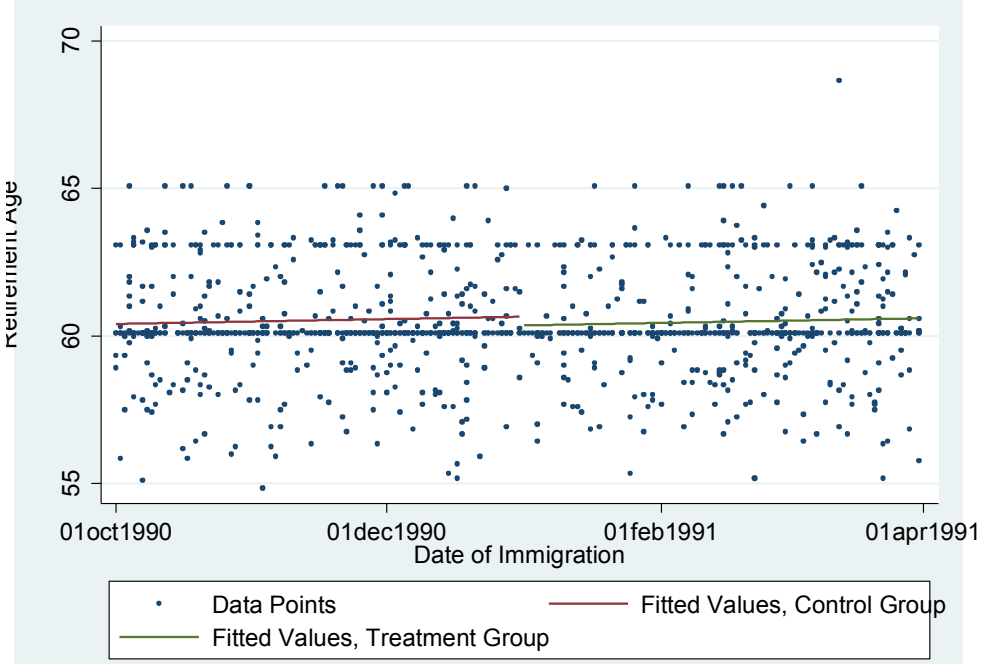


Fig. A5. Pension payments and date of immigration: men. The graphs are based on the data for the 6-month sampling window.

Source: Administrative German pension data; author calculations.

A. Natural Experiment 1



B. Natural Experiment 2

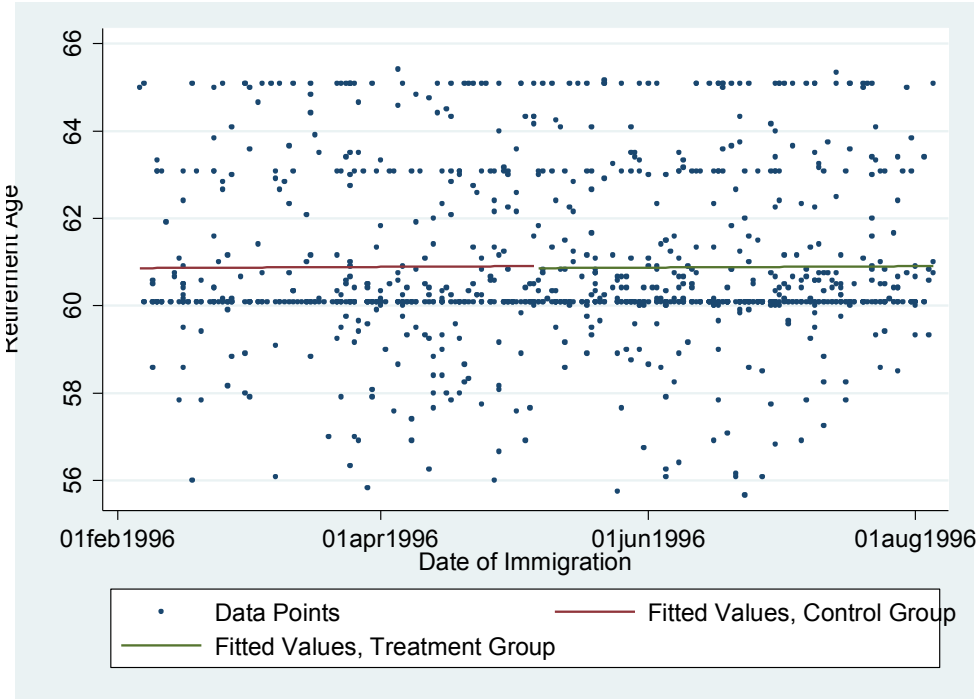
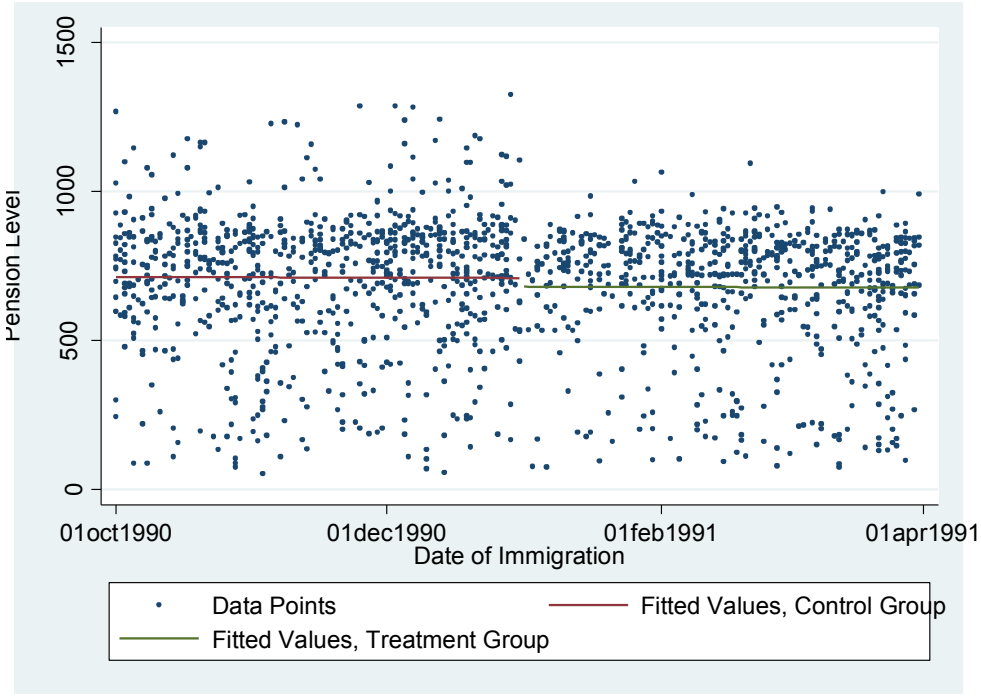


Fig. A6. Age at retirement and date of immigration: men. The graphs are based on the data for the 6-month sampling window.

Source: Administrative German pension data; author calculations.

A. Natural Experiment 1



B. Natural Experiment 2

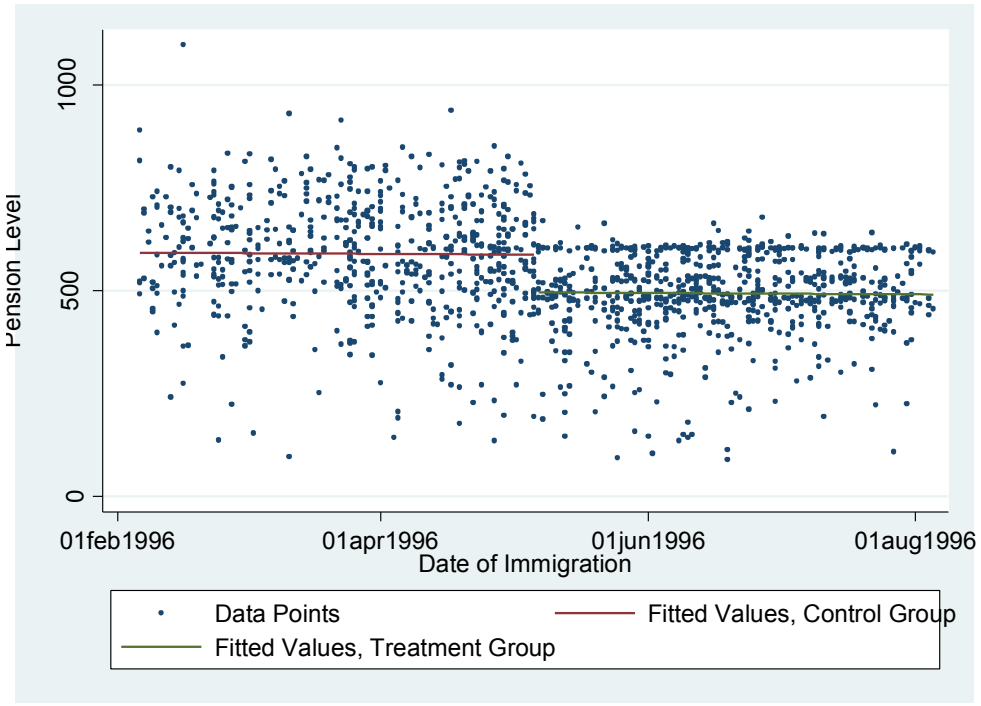
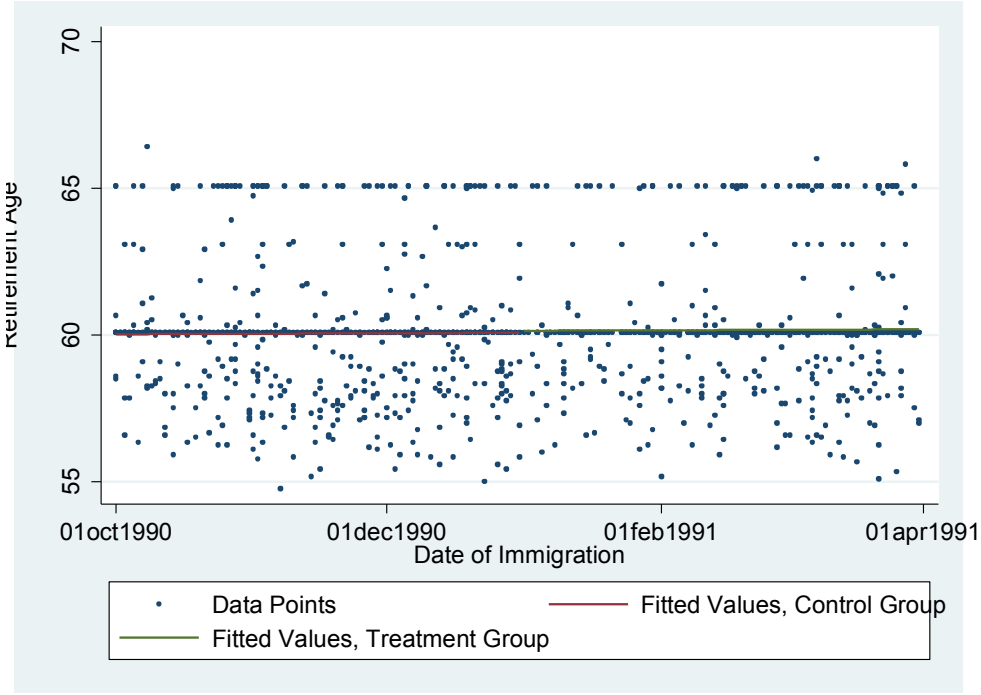


Fig. A7. Pension payments and date of immigration: women. The graphs are based on the data for the 6-month sampling window.

Source: Administrative German pension data; author calculations.

A. Natural Experiment 1



B. Natural Experiment 2

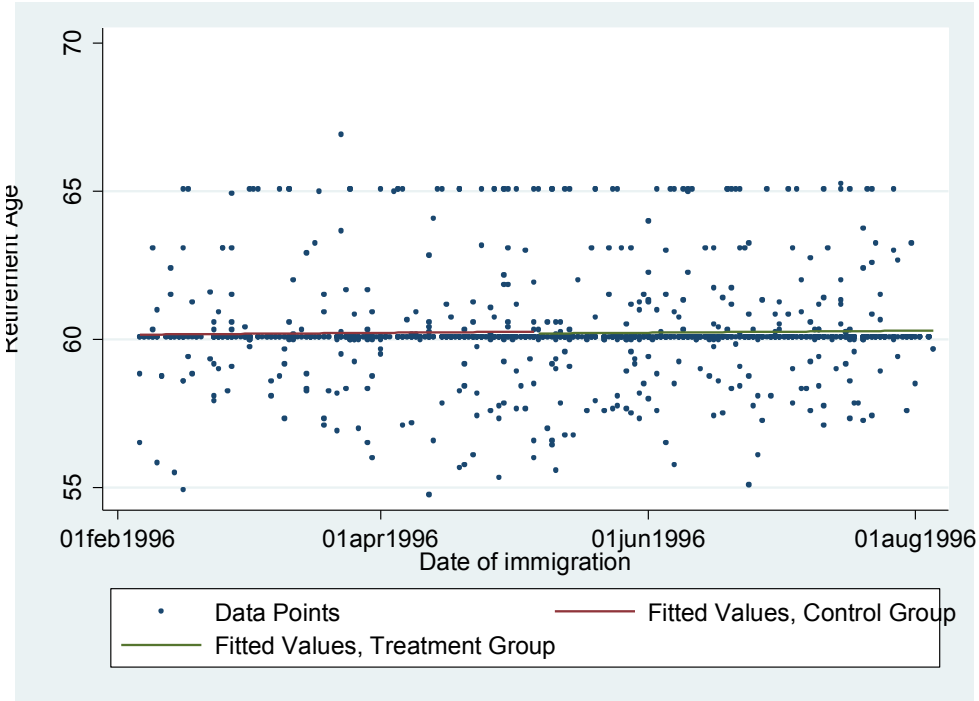


Fig. A8. Age at retirement and date of immigration: women. The graphs are based on the data for the 6-month sampling window.

Source: Administrative German pension data; author calculations.

Appendix B. – Explanation of the Empirical Findings Based on a Simple Labor Supply Model

In the following, we describe the corner-solution outcome, in which workers retire as soon as possible, based on a simply labor supply model. We represent the budget constraint faced by the worker as

$$C = Tw - \left(w - p\right)R \qquad (3)$$

where C is total consumption, T is the time left from the earliest possible retirement date until the expected end of life, w is the wage rate that the individual could earn (per period), p is the pension earned per period, and R is the number of periods spent in retirement (meaning that T-R is the number of periods worked after the earliest point at which retirement is possible).

Although pension p actually depends on the choice of R, we argue that we can ignore this effect because the workers studied spent almost all their working lives abroad, so their years and jobs abroad will predominate in the calculation of p. Such is even more so the case because, as the calculations based on the microcensus data show, the wages offered for this group of low-skilled workers are rather low: the median income of repatriated ethnic German men or women who immigrated after 1997, being older than 55 in 2005 (date of survey) and currently out of the labor market, is around €500 (income data are given only in intervals). For the same group, working men earn between €1,000 and €1,200 and working women around €500.[25] For men, we assume €1,000 to be closer to the accurate number because workers self-select into the working and non-working populations based partly on their labor productivity.

Given a relatively low w and a relatively high p (albeit one still lower than w), we expect the budget constraint to be relatively flat when drawn in C-R space or even, if w is smaller than p, to have a positive slope. This latter, however, is not indicated by the figures discussed above, although these admittedly may be subject to an uncorrectable sample

[25] For comparison, other low-skilled German men not participating in the labor market have a median income around €1,000–1,100 but working men have a median income of between €1,400 and €1,500.

selection bias. A reform that lowers p, like the one considered here, will slope the budget line even more negatively, and even more so if the slope was negative before reform. In the case of an interior solution – that is, if workers did not retire as soon as possible – we would expect both income and substitution effects to delay the retirement date. As regards a corner solution in which workers retire as soon as the administration allows – meaning that $R = T$ – such a solution could still conceivably exist post reform. Therefore, assuming that the reform only affects the pension by lowering it from p to p', we could have the following:

$$MRS_{(R=T,C=Rp)} = \frac{\partial U\left(R=T,C=Rp\right)/\partial R}{\partial U\left(R=T,C=Rp\right)/\partial C} \quad > \quad \frac{\partial U\left(R=T,C=Rp'\right)/\partial R}{\partial U\left(R=T,C=Rp'\right)/\partial C}$$

$$= MRS_{(R=T,C=Rp')} > \left(w-p'\right) \tag{4}$$

If Eq. (4) holds, then lowering the pension from p to p' does not change R (as observed in our estimates); that is, the corner solution remains, with $R = T$.

Appendix C. – Illustration of Ethnic German Settlement in Central and Eastern Europe and Immigration to Germany

Fig. C1. Map of Ethnic German Settlements in Central and Eastern Europe – This Map Excludes Ethnic German Settlement from Regions in Pre-WWI Germany and Cisleithania Austria (Among Which are Today's Czech Republic and Parts of Croatia)

Source: Downloaded from https://www.familysearch.org/learn/wiki/en/File:Germans_in_Eastern_Europe5.png on November 14th 2011. According to this webpage, this map is in the public domain.

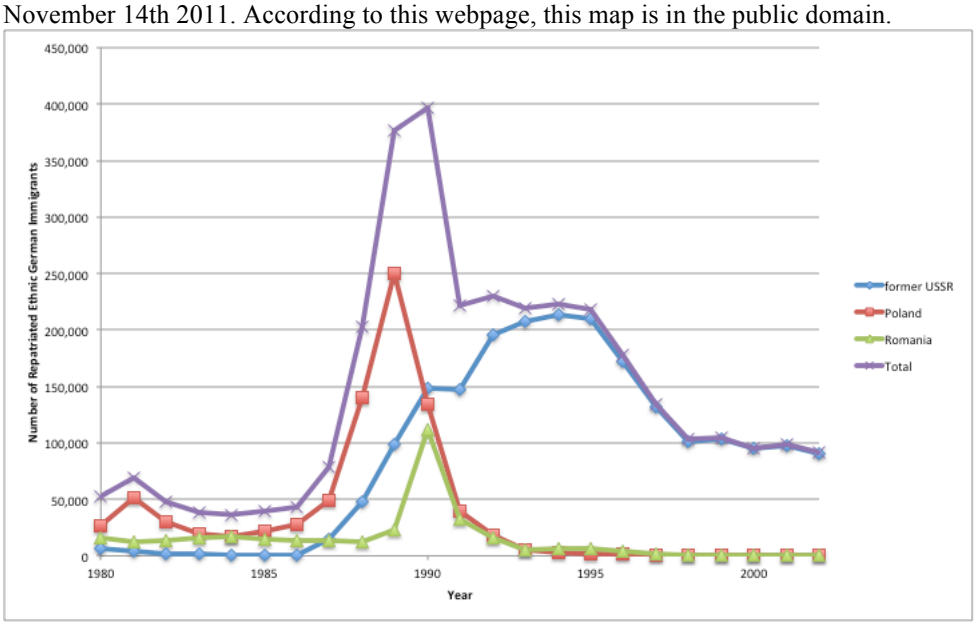


Fig. C2. Number of Repatriated Ethnic German Immigrants by Year and Source Country

Source: German Federal Ministry of the Interior.