



Essays in Labour Economics

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Thesis submitted for assessment with a view to obtaining the degree of
Doctor of Economics of the European University Institute

Florence, 21 February 2019

European University Institute
Department of Economics

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Abstract

The elderly are an ever-growing group of the population of western countries. Increasing their low employment rates is one of the greatest challenges we face in labour market policy today and is the subject of the first chapter of this thesis. I evaluate the labour market effects of partial retirement - that is a scheme that subsidises part-time work for older workers. It was introduced as an attempt to extend working lives by incentivising part-time employment after a certain age. I find that this policy had overwhelmingly negative effects on old-age labour supply as most workers substituted full-time work with part-time work in partial retirement without actually extending their active lives.

Chapter 2 of this thesis is a reflection on the labour market situation of young workers with parental backgrounds that make it difficult for them to achieve their potential. When and where they are held back and whether an open labour market can compensate for this disadvantage is the subject of this chapter. I find that after entering the labour force, workers from disadvantaged backgrounds "catch-up" in terms of wages with respect to their privileged peers with the same educational achievement. I explain this phenomenon in a setup of education signalling with noise and subsequent employer learning.

In the third chapter my co-authors and I focus on the consequences of national wage setting mechanisms in countries with large geographic differences in labour productivity. We confront Germany with relatively flexible wage bargaining mechanisms and Italy with very rigid ones. We find that given the large productivity differences in both countries, Italy's highly centralised bargaining system generates significant inefficiencies and high costs in terms of aggregate earnings and employment particularly in the South.

Acknowledgements

I thank everyone who provided intellectual or emotional support in the making of this thesis. In Florence, Zurich and elsewhere.

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Preface

What I present here are three chapters in labour economics that differ in their focus, method and implications for labour market policy. What connects them is perhaps that they provide insights into the labour market realities of three groups of workers that have somewhat particular policy needs and experiences. The first group is the elderly. They are a growing share of the population of western countries, leading to both social change and a significant burden to welfare states. Their continued inclusion in the work force is arguably one of the greatest challenges we face in labour market policy today, and is the subject of the first chapter. The second group are young workers with parental backgrounds that make it difficult for them to achieve their potential. When and where they are held back and whether an open labour market can compensate for this is the subject of the second chapter. The third group are workers living in structurally weak and unproductive geographic regions. To what extent labour market institutions like collective bargaining can be helpful or harmful to their economic success is the subject of the third chapter.

The first chapter of this thesis deals with the effects of a policy meant to encourage part-time work among older workers. It allows workers to reduce working hours to part-time after reaching a certain age - and thus retire "partially". At the same time workers draw a benefit that can be interpreted as a partial pension, that complements their income to ensure a smooth transition into full retirement. This programme seeks to extend working lives through a reduction of working hours at older ages using pensions or subsidies to compensate for at least part of the loss in earnings. In chapter 1, I evaluate the employment effects of partial retirement using the quasi-experimental variation of a policy

reform in Austria. The 2004 reform of the partial retirement scheme in Austria and the corresponding cut-off at the eligibility age allow me to estimate its causal effect using a regression discontinuity design on the birthdate of individual workers. I find that subsidising partial retirement - in the Austrian context - had an overall large negative effect on employment in terms of hours. This indicates that most workers substituted full-time work with part-time work in partial retirement. I estimate that only 15% of the days old workers spent in part-time work due to the policy can be attributed to days otherwise spent in non-employment for the population overall. Some heterogeneity analysis reveals however, that employment effects are significantly more positive for the part of the population who - due to their characteristics - has a high risk of withdrawing from the labour force before the official retirement age. For this group I find that the availability of partial retirement reduces in particular the uptake of disability insurance. This indicates that the part-time policy could increase its effectiveness by focussing on individuals who have or are at risk of developing a disability. I also provide some evidence with respect to spillover effects of partial retirement on the firm level, that is its impact on both hiring of new workers as well as retention of existing ones. I find that partial retirees tend to be replaced by newly hired workers into the firm relatively quickly. Other co-workers do not seem to be affected by the policy.

In Chapter 2 of this thesis, I argue that children of parents with low educational achievement face an additional - unobserved - cost of investing in signals like education. Therefore, they obtain less education than their counterparts from a more educated family even if they have the same innate abilities. If this is the case, then out of a group of people with the same educational achievement those from less privileged background have a higher innate ability. Then, upon entry in the labour market that rewards this ability, they will have a steeper wage growth than individuals from a "better" parental background who invested in the same signals. I find empirical support for the implications of this hypothesis in a setup of employer learning with statistical discrimination using data from the German Socio-Economic Panel.

In Chapter 3 my co-authors and I study the local and aggregate effects of national wage

bargaining systems. In many European countries, wages are determined by collective bargaining agreements. These agreements are intended to improve wages and reduce inequality. We study the local and aggregate effects of two versions of collective bargaining systems: Italy and Germany. Italy sets wages based on nationwide contracts that allow for limited local wage adjustments, while Germany has moved toward a more flexible system that allows for local bargaining. We show that while the Italian system is successful at reducing nominal wage inequality, it also creates costly geographic imbalances due to geographic differences in firm productivity. We show that the North-South productivity gap in Italy is similar to the West-East productivity gap in Germany. As a result of their different collective bargaining systems, Italy exhibits almost no relationship between local productivity and local nominal wages, while Germany has larger geographic wage differences and a tighter link between local wages and local productivity. In Italy, low productivity provinces have significantly higher non-employment rates than high productivity provinces, because employers cannot lower wages, while in Germany the relationship between non-employment and productivity is weaker. In Italy, the relationship between real wages and productivity is negative, with lower real wages in the North compared to the South, since the latter has low housing costs but similar nominal wages. Thus, conditional on having a job, Italian workers have higher purchasing power in the South, but the probability of having a job is higher in the North. We conclude that the Italian system has significant costs in terms of forgone aggregate earnings and employment because it generates a spatial equilibrium where workers queue for jobs in the South and remain unemployed while waiting. If Italy adopted the German system, we estimate that aggregate employment and earnings could increase by 12%, and 7% , respectively. Our findings are relevant for several other European countries with systems similar to Italy's.

Chapter 1

Can Partial Retirement Increase Old-Age Employment? Evidence from Austria

1.1 Introduction

Aging populations and low labour force participation among the elderly is creating an ever increasing share of the population that does not work. This is a strain for welfare states, where retirement expenditures constitute a large share of public spending especially in European countries. It is also a social issue in societies where work is at the core of social interaction and identity. Against this background, finding new ways of labour market integration for the elderly is arguably one of the greatest challenges we face in labour market policy today.

In recent years there has thus been increasing pressure on governments and social security systems to address these issues. Many countries have raised or are planning to raise the normal retirement age, abolish early retirement options and have implemented special programmes to help the elderly unemployed to reintegrate in the labour market.

As a part of this reform process, some countries have also experimented with policies meant to encourage part-time work among older workers. These "partial retirement"

policies are the focus of this paper. They allow workers to reduce working hours to part-time after reaching a certain age - and thus retire "partially". At the same time workers draw a benefit that can be interpreted as a partial pension, that complements their income to ensure a smooth transition into full retirement. This type of programme seeks to extend working lives through a reduction of working hours at older ages using pensions or subsidies to compensate for at least part of the loss in earnings.

Austria introduced partial retirement in 2000 and will be the empirical setting of this paper. However, similar schemes are (or have been) in place in several countries including Australia, Belgium, Czech Republic, Denmark, France, Germany and the Netherlands. Despite its popularity, we yet lack quasi-experimental evidence on the effects of partial retirement on the labour supply of older workers.

From an economic theory perspective it is not ex-ante clear what the employment effects of such programmes will be. In the absence of part time options, any population of elderly workers will consist of two groups: the ones that choose to still work full-time and the ones that stop working altogether. Whether an increase in part-time work among the elderly increases or decreases total labour supply ultimately depends on how it affects the composition of these two groups.

In this paper, I evaluate the employment effects of partial retirement using the quasi-experimental variation of a policy reform in Austria. The 2004 reform of the partial retirement scheme in Austria and the corresponding cut-off at the eligibility age allow me to estimate its causal effect using a regression discontinuity design (RDD) on the birth-date of individual workers. I find that subsidising partial retirement - in the Austrian context - had an overall large negative effect on employment in terms of hours. This indicates that most workers substituted full-time work with part-time work in partial retirement. I estimate that of only 15% of the days old workers spent in part-time work due to the policy can be attributed to days otherwise spent in non-employment over a ten year horizon for the population overall. Some heterogeneity analysis reveals however, that positive employment effects are significantly larger for the part of the population who - due to their characteristics - has a high risk of withdrawing from the labour force

before the official retirement age. For this group I find that the availability of partial retirement reduces in particular the uptake of disability insurance. This indicates that the part-time policy could increase its effectiveness by focussing on individuals who have or are at risk of developing a disability.

A second argument often put forward in support of part-time schemes for the elderly is that they encourage a "phasing-in" of younger replacement workers and are thus good for business and for young job seekers. Others argue that in times of low demand older workers are often the first affected by redundancies and generous social security provisions are merely an attractive tool for these job cuts, without benefiting other workers at all (Hutchens (1999)). In this paper, I will also provide some evidence with respect to these spillover effects of partial retirement on the firm level, that is its impact on both hiring of new workers as well as retention of existing ones. In terms of spillovers to other workers in the same firm, I find that partial retirements do seem to cause replacements and therefore increase hirings in the firms that send their workers to partial retirement. I find no effects on the firm's probability to remain in business. Partial retirements also do not seem to affect the tenure probability or wages of co-workers.

This is the first study to evaluate the effects of a partial retirement policy in the context of a natural experiment. The advantage of the research design of this paper is that it measures the causal effects of the policy even in the presence of selection into partial retirement based on unobservables of either firms or workers. That being said, the literature on partial retirement so far, finds consistently negative overall employment effects. While due to the different design of other papers in the literature, it is difficult to compare the results quantitatively, my findings are qualitatively in line with what has been found in other settings.

Börsch-Supan et al. (2018) find only small increases in labour force participation by the 55-64 year olds in a cross-country study of flexible retirement reforms. The negative effects on hours in the same group are large. Graf et al. (2011), evaluate the policy in Austria matching individuals who take vs. individuals that do not take up partial retirement. They thus evaluate the treatment effect of reducing hours directly. They find small

positive effects on days employed in the first four years after take-up, but large negative effects on full-time equivalent employment. They also find small negative effects on the days spent in unemployment. Huber et al. (2013) look at a partial retirement policy in Germany that shares the key features of the Austrian policy. The study is more similar to this one in that they evaluate an intent to treat effect of working in a firm that offers partial retirement. They then match on both worker and firm characteristics to compare similar workers working in similar firms some of which self-report that they offer partial retirement and some don't. They find in particular differences between workers in the East and West of Germany. While in the West the policy seems to have almost no effects on labour force participation, they find that in the East the option of partial retirement does reduce the probability of entering retirement through unemployment. They also look at the effect on other workers in the same firms not directly affected by partial retirement. They find that the option of partial retirement for older workers has positive spill-over effects on the employment of younger workers.

1.2 Institutional Setting

Like many other European countries, Austria has an effective retirement age that lies well below the OECD average. In particular, workers tend to retire on average long before the statutory retirement age, which is 60 for women and 65 for men. In 1999 the average age at which retirement benefits were claimed among women was 58.2 (1.8 years before the normal retirement age), among men 61.9 (3.1 years before the normal age). Here again, Austria is not an isolated case. The effective age of labour market exit is lower than the official retirement age for both men and women in 13 out of 35 OECD countries (OECD (2017)).

It is important to note that in the Austrian context along with early retirement options, unemployment and disability benefits also play a significant role for a premature withdrawal from the labour force. Overall in 1999, 16.5% of men and women who entered

retirement entered through unemployment, an additional 23.2% came through some form of disability benefits (in some cases: early retirement due to limited ability to work). In addition, of those retiring in 1999 48.4% were early retirees (among which the share of previously unemployed was twice as large as it was among all retirees).¹ Counteracting this development was an important motivation for the introduction of partial retirement in the year 2000.

Figure 1.1 gives a quick overview of the options Austrian workers at the time faced in terms of early retirement. In the rest of this section I will describe the details of the institutional setting regarding retirement in Austria. For an even more detailed account of the Austrian public pension system and its evolution in the early 2000s see Staubli and Zweimüller (2013).

The Austrian social security system covers all private sector workers and provides early retirement pensions, old age pensions, and disability pensions. These pensions provide the main source of income in retirement. The level of benefits depends both on an average of earnings in the highest earning 15 years of the working life as well as the insurance years (though insurance years include periods of parental leave, unemployment and military service). On average, the replacement rate of pensions after income and payroll taxes is 75% of the pre-retirement net earnings. The normal retirement age is 60 for women and 65 for men conditional on at least 15 insurance years in the past 30 years. The access age to early retirement was 60 for men and 55 for women before 2000 conditional on 37.5 insurance years. In 2000, around 88% of men and 54% of women satisfied this criterion when they reached the specified age (Staubli and Zweimüller (2013)). Recipients of an early retirement pension lose their benefit in each month that they earn above a pre-defined threshold (around 380 euros per month). No such restrictions apply for normal old-age pensions (after the normal retirement age). Two pension reforms enacted in 2000 and 2003 raised the minimum age for early retirement for those individuals gradually from 55 for women and 60 for men to 60 and 65 respectively. This took place in two-month and later monthly steps per quarter of birth. Note that these gradual changes

¹Own calculations on the basis of social security registry

affected cohorts before and after the cut-off for the partial retirement reform in the same way and therefore is unlikely to confound the results of this paper. The reforms did not affect men with at least 45 insurance years and women with at least 40 insurance years who could still retire earlier if they met the requirement. It also did not affect men and women qualifying for a special heavy labor pension for people doing hard physical work. In 2005 an additional corridor pension was introduced which allowed retirement at 62 conditional on having 37.5 insurance years.

Disability insurance can be claimed by individuals with a health impairment that reduces the capacity to work by more than 50% who do not qualify for any old-age pension. Because medical criteria for disability classification are relaxed starting at age 57, disability pensions make up a large share of early withdrawals from the labour force (Staubli (2011)). With a replacement rate of about 55% of last net earnings unemployment benefits can be drawn for up to a year. After that year, they can be extended in a slightly reduced version de-facto indefinitely though they are reduced by the earnings of the spouse or other family member in the same household.

1.2.1 Partial Retirement

Partial retirement (*Altersteilzeit* in German) was introduced in Austria after a period of decreasing employment of the elderly population. As mentioned above, a significant share of elderly Austrians withdrew from the labour force prematurely claiming unemployment or disability benefits or entering other forms of non-employment. The objective of partial retirement was thus to increase attachment to the labour force in the immediate pre-retirement period. The idea was to do this by making part-time work particularly attractive and thereby incentivise working part-time relative to dropping out of the labour market completely. The policy allowed male workers above 55 and female workers above 50 to reduce their hours to 40-60% of full-time. These workers would receive the corresponding wage plus a top-up of 50% of their foregone earnings (lost through the reduction of hours). This top-up was financed by the government. Figure 1.2 is a graphical representation of the budget set of an elderly worker resulting from the option of partial

retirement. Partial retirement could only be taken up in mutual agreement of employer and employee – this means there was no legal claim to it on either side. The maximal length of partial retirement agreements was initially set to 6 1/2 years. Although it was later varied, a continuation until the first possible age of early retirement was always ensured. There were two possible versions of partial retirement: On the one hand the "phased" option which implied exactly a reduction of hours for the whole duration of the agreement. On the other hand, there was the "block" version that involved a full-time and a leisure phase. So for instance a partial retirement agreement of 5 years, reducing to 50% would imply 2.5 years full-time work and 2.5 years of leisure. In this version the policy essentially amounted to early retirement.

1.2.2 2004 Reform

Partial retirement was fairly popular among workers and firms in the first three years. Among the cohort reaching the eligibility age in 2001 and fulfilling basic eligibility criteria, 11.7% would enter partial retirement within one year from becoming eligible. This led to broad criticism of the high cost of the programme. Facing pressure to reduce the scope of the programme the Austrian government thus implemented a reform that made access more stringent from January 1, 2004. In particular, the reform required firms to employ substitute worker if they sent someone to partial retirement. This substitute worker had to be employed for above 12 hours a week and had to be a previously unemployed person or an apprentice. If they failed to do so, firms would have to pay 50% of the wage top up themselves for the phased version and 100% for the block version². The second change implemented with the 2004 reform was the initiation of a step-wise increase of the eligibility age by a half year every year between 2004 and 2009. The eligibility age was thus raised from 50/55 in 2003 to 53 for women and 58 for men in 2009.

This reform, paired with the age required for eligibility provides my empirical set-up for the evaluation of this policy. Individuals born so that they reached the eligibility age

²In the block version, employers had the option of hiring the substitute only during the leisure phase. In that case, they didn't have to pay for the top-up in the leisure phase, but still 50% of it during the full-time phase.

to partial retirement (50 for women, 55 for men) *before* January 2004 were eligible to take up partial retirement before the reform and thus the old an less restrictive version of the policy. Individuals reaching this age one month later, were subject to the new restrictions introduced by the reform. For workers born just after the cut-off two things changed therefore: First, they were allowed to enter partial retirement only a half year later (when they were 50.5 or 55.5). Secondly, their employers had to potentially incur a higher cost for signing the partial retirement agreement because of the substitute worker requirement or alternatively the increased wage load.

Uptake reacted strongly to the reform. There was a significant last-minute effect in December 2003 followed by a sharp drop in uptake (see Figure 1.3). The last-minute spike in take-up is driven by workers across the eligibility age range and not just by those whose eligibility is affected by the increase in the eligibility age (see Figure 1.4 for take-up behaviour of above 51/56 year olds). This indicates that firms considered the substitute requirement as a significant constraint and reacted strongly to it. While this is an interesting fact as such, for the purpose of this paper it suffices to acknowledge that the access of workers to partial retirement was suddenly and exogenously made more difficult. This is the fact that I will use to evaluate the policy causally.

1.3 Data and Summary Statistics

1.3.1 Austrian Social Security Database and Partial Retirement Data

I use the Austrian Social Security Data (Zweimüller et al. (2009)). These data contain very detailed longitudinal information for all private sector workers in Austria. At the individual level the data include gender, nationality, month and year of birth, blue-collar or white-collar status, daily labor market history, and earnings. The data also record firm identifiers for employers and their location and industry. I obtained additional data on the individual take-up of partial retirement from the Austrian Ministry of Social Affairs.

1.3.2 Sample Construction

My sample of individuals consists of women of the birth cohorts 1951-1956 and men from 1946-1951. That means all individuals born within three years of the threshold as defined by the partial retirement reform. I restrict my attention to those in stable employment at age 53³, working in a firm with more than 3 employees. I also restrict the sample to firms outside of seasonal industries (tourism, construction, agriculture, mining)⁴. Individuals in the sample have to fulfill the criteria of eligibility in terms of working history at the eligibility age in principle⁵. This leaves a sample of 82 497 women and 95 267 men. The firms for which I evaluate spillover effects will be the employers of those individuals on their 53rd birthday. For the firm and peer sample I look at firms with less than 40 employees. Peers are the co-workers of critical workers in the same firm in December 2002. My final sample consists of 25 180 firms with an average of 15 co-workers per sample individual.

1.3.3 Descriptives and Selection into Partial Retirement

Tables 1.1 and 1.2 show the composition of my sample by a range of characteristics. Regardless of treatment status (pre/post reform). I also show the composition of the takers and non-takers group. Table 1.3 shows the marginal effects of a probit model of ever being observed in partial retirement on different variables. Men are overrepresented in the sample and also among those taking up partial retirement. As can be seen in Table 1.3 men are actually less likely to take up partial retirement once earnings and employment history are controlled for. While one may think the target population of partial retirement are those less able to work fulltime, surprisingly, having experienced unemployment or extended sickness⁶ does not increase (rather decreases) an individual's

³Defined as working more than 300 days in the year they turn 53.

⁴This is because the large employment fluctuations in these industries make evaluating spillover effects particularly difficult.

⁵To be eligible, workers have to have been employed at least 15 years in the past 25 years at the time they reach the eligibility age. I restrict the sample to individuals who at age 48/53 have cumulated days of employment are such that with continuous full-time work they could reach the eligibility requirement of 15 years

⁶Sickness is reported in the social security registry only when it exceeds 30 days

probability to reduce to part-time. Among the industries, Manufacturing, Energy and Financial services seem to employ the highest fraction of partial retirees. In general, the population most likely to enter partial retirement are high earning, white collar workers with very stable working arrangements (long tenure) in firms with a high general wage level. This may already be a first indicator, that in fact those individuals who take up partial retirement are not the ones with the least attachment to the labour force and that the counterfactual labour market behaviour in the absence of the partial retirement policy may in fact be continued full time employment. It is this counterfactual scenario that I will determine in a causal framework in the next sections.

1.4 Estimation

1.4.1 Employment Effects of Partial Retirement for Elderly Workers

Objective

The main objective of this paper is to evaluate the employment effects of the availability of partial retirement on the elderly workers it was designed for. I will exploit the 2004 reform combined with the eligibility age cut-off for identification. In particular, workers who had turned 50/55 *before* January 2004 were still eligible to take up the old "cheaper" version of partial retirement, while those who did not reach the age of eligibility in time could not. There is thus a strict cut-off in terms of month of birth of who could be in the old system and who was eligible only to the new system. This results in a significant jump in the take-up rate of partial retirement right at this birthmonth cut-off. I will exploit this cut-off to implement a regression discontinuity design where the running variable is the birthmonth of the worker. Figure 1.5 shows the jump in partial retirement uptake at this cut-off. Since no other policy changes at this particular birthmonth cut-off, the differences of workers close to either side of the cut-off can be causally attributed to the effect of the easier availability of partial retirement. I will estimate the effect of easier access to

partial retirement on days in partial retirement, days in employment, days in full-time equivalent employment, days in unemployment, on disability and in old-age retirement. All this on a 5 and 10 year horizon as well as in a yearly dynamic framework. I also look at the retirement age and the age that the individual is last observed in employment (effective retirement age).

RD Estimation Framework

I estimate an ITT effect of being born such that one was eligible to partial retirement before the reform vs just after. Specification for individual level outcomes:

$$Y_i = \alpha + \beta T_i + \gamma_1 f(bm_i) + \gamma_2 T_i \times f(bm_i) + \gamma_3 X_i + \varepsilon_i$$

where T_i is an indicator whether the birthmonth is lower than the cut-off, meaning that the individual was eligible to partial retirement *before* the reform. bm_i is the month of birth of the worker. $f(bm_i)$ is a linear trend. β is the parameter of interest in this case and represents the ITT effect of partial retirement. It can be interpreted as the effect of being born such that the easy access partial retirement policy was just still available. X_i is a vector of controls including gender, white/blue collar, firm size, earnings at 48/53, days tenure at 48/53, working history of the last 10 years before turning 48/53 (sick days, days unemployed). The optimal bandwidth is estimated following the methodology proposed by Calonico et al. (2014b) and Calonico et al. (2014a), though results are robust to the use of a predetermined bandwidth of 12 months. Standard errors are clustered on the firm level. In Table 1.4 I show the balancing results of running this specification on the predetermined outcomes later used as controls (gender, white/blue collar, firm size, earnings at 48/53, days tenure at 48/53, working history of the last 10 years before turning 48/53 (sick days, days unemployed) and the wage level in the firm).

1.4.2 Spill-over Effects of Partial Retirement on Firms' Employment Behaviour and Peer Effects

Objective

While the objective of partial retirement was primarily an increase in the labour force attachment of the elderly workers themselves, it is also often argued that this kind of phasing out of the workforce may facilitate a slow phasing in of new younger workers in their place. Given my treatment variation on an individual level I have the opportunity to test also this proposition. By looking at firms employing treated or non-treated workers, I extend the treatment status to firm level. In my empirical setting, I have a strong sudden shock to partial retirement uptake in December 2003 for individuals born before the cut-off. This allows me to look at the causal effect of a worker going into partial retirement on both the hiring of new workers by the firm as well as the retention rate and wages of already employed peers at the firm.⁷

It should be noted that the firm and peer level outcomes does not come without caveat: First, because the reform in 2004 influenced the way in which partial retirement workers had to be substituted directly, the treatment assignment may itself influence hiring behaviour. Untreated - post-reform individuals - are less likely to enter partial retirement but if they do enter they may be more likely to be substituted. This concern is alleviated by the fact that the untreated, born such that they couldn't take up partial retirement pre-reform, were also affected by an increase in the eligibility age by half a year. Therefore the earliest possible time when any untreated individual could take up partial retirement with substitution requirement is in July 2004. Any hirings before that can thus be attributed to the spike in partial retirements pre-reform.

The second concern would be that if untreated individuals are indeed more likely to leave the labour force early than pre-reform individuals - that is, if partial retirement indeed has positive effects of employment - then this could have its own effect on hirings and thus dampen any effect we see on substitute hirings of partial retirement takers. Ex-post this

⁷Peers may be affected by a sudden loss of a worker because market frictions prevent an immediate substitution with a newly hired worker (Jäger (2016)).

seems to be less of an issue as the actual effects on non-employment I find are very small. It is nevertheless noted that all estimated spillover effects may be downward biased.

RD Estimation Framework

The obvious challenge with my estimation framework is that individual workers are treated, but I want to look at outcomes on the firm level. For that purpose, I restrict my sample to firms who employ at least one worker who has been born somewhere within the selected bandwidth around the cut-off. For the purpose of this description I will call such a worker "critical worker". This worker will be either "treated" (born such that she had access to the old system of partial retirement) or not treated. Some firms will employ only one "critical worker". In this case the firm is unequivocally treated or not. For firms with more than one such worker it is less clear. I construct my dataset in a way that I will have one observation for every pair of critical worker and firm. Thus, if a firm has two critical workers, its outcome will be recorded twice in my dataset with the corresponding treatment assignment of each of the critical workers. I will then weight the observation of firms by the inverse of the number of critical workers. The coefficient of the treatment variable can thus be interpreted as the contribution of one treated worker compared to a non-treated worker to the firms' hiring outcomes. For the outcomes of peer workers, I employ the same strategy except that this time there is one observation for each peer-critical worker pair. The regression is then weighted by the inverse of the number of peers in a firm \times critical workers. Otherwise the specification is identical to the one for individual level outcomes. To be able to detect spill-overs, I restrict my sample to firms with less than 40 employees in December 2002 for the spill-over analysis.

1.5 Results

1.5.1 Elderly Workers

I study the ITT effects of availability of partial retirement on elderly workers' days in partial retirement, days in employment, days in full-time employment, days in unem-

ployment, on disability and in old-age retirement. I also look at the retirement age (first observed old-age pension), the age that the individual is last observed in employment (effective retirement age). The last two outcomes are shown in Table 1.5. Table 1.6 reports the effects on the probability of experiencing a certain state of employment or non-employment over a 5 year horizon after reaching age 50 for women and 55 for men. Table 1.7 looks at the total days in employment and different forms non-employment over the five years. Tables 1.8, 1.9 do the same thing for a horizon of 10 years after turning 50/55. In columns (2) and (4) of each table I have added controls for gender, white/blue collar, firm size, earnings at 48/53, days tenure at 48/53, working history of the last 10 years before turning 48/53 (sick days, days unemployed). Treated individuals are the ones born before the cut-off and thus had easier access to partial retirement.

Overall, being born on the left of the threshold (treated) increases the probability of taking up partial retirement consistently by 10 percentage points. Neither the official, nor the effective retirement age are significantly affected by this increase in the take-up of partial retirement. It does however decrease the probability of ever being out of employment by 2.3%⁸ and decreases the probability of experiencing unemployment and disability by about 1.6 percentage points each over a horizon of five years. The unemployment effect is not significant at a 10 year horizon. Table 1.7 shows that the 10 pp increase in the probability of take-up translate into 224 days more in partial retirement over 5 years, 277 over 10 years. As we will see in the dynamic estimation (Figures 1.6 and 1.7) the small difference between 10 and 5 year results is due to a decrease in the effect on days spent in partial retirement after year 5 from the eligibility age.

The additional days spent in partial retirement by the treated group can be split quite precisely into days that were worked full time (in employment but not partial retirement) by the counterfactual (non-treated) group and days that were instead spent not employed. These numbers mark the fundamental trade-off of this policy: Some people will use the subsidised part-time work to extend their working lives and increase attachment to the labour force when they would have otherwise dropped out. Some will use the subsidy on

⁸I will always cite the numbers of the preferred specification with controls here.

part-time to reduce hours earlier than they would have done incurring a smaller income loss.

My results indicate that the shares of the former are small while there is ample evidence for the latter behaviour. Among the average number of additional days the treatment group spent in partial retirement with respect to the control group in my preferred specification with controls, almost 92% are spent in full time employment by the control group over a 5 year horizon (this can be seen in Panel A, column 4 of Table 1.7). There is only a very small decrease of days spent out of employment of the treated group. It is only over the 10 year horizon in the specification with controls that we can reject the change in days spent not employed to be zero at a 10% significance level. The number of days spent in unemployment and disability are not statistically distinguishable from zero over the 5 year horizon. Over the 10 year horizon the number of days spent in disability decreases slightly. This means that despite the fact that partial retirement reduces the probability of experiencing a spell of unemployment or going on disability slightly, quantitatively these spells are small especially when compared to the large reductions in full-time work. It can thus be said that overall, the effects of partial retirement on elderly employment are overwhelmingly negative.

Nevertheless, to study a bit more in detail what is going on and when, the results of an event-study type dynamic specification are shown in Figures 1.6 and 1.7. These figures show the ITT estimates for each year (measured in age from 50/55). We can clearly see the strong impact of the birthmonth cut-off on the take-up of partial retirement at the eligibility age (Panel a) Figure 1.6) that disappears after around year 8 (age 58/63). Panel c) shows the impact on full-time employment, that almost mirrors Panel a). Here however, we also see that the effect on the extensive margin (days out of employment) is not consistently zero. Especially in years 4-6 after reaching the eligibility age, individuals with better access to partial retirement tend to be employed for more days than their untreated counterparts. These effects vanish once age 62 is reached for men and 57 for women. These are the ages where early retirement becomes available (for women it starts from 57 and 9 months of age). This is consistent with the idea that partial retirement

may keep employment intact as long as the outside option is unemployment, disability or other forms of non-employment. Once early retirement becomes available, partial retirees transition to that at the same rate as people with no access to partial retirement. This version of events is also supported by Figure 1.7. While the days in retirement are not significantly different between the two groups at any point (we could expect an effect starting from year 7 after eligibility in panel c), partial retirement does seem to decrease the amount of days spent in disability insurance (particularly after period 2 of panel a, which is when men reach the age of easy access to disability). Even though there seem to be some small positive employment effects of partial retirement at some ages, it does not compensate for the large negative ones that come through the reduction of hours. This is clear from panel c in Figure 1.6.

Individuals with High Propensity to Retire Early

Earlier in this paper, I have highlighted that the net employment effects of a policy like partial retirement depend on the counterfactual labour supply of the people drawn to partial retirement. The ideal part-time policy helps individuals to stay attached to the labour market when they are not able or willing to work full-time anymore. These people would - in the absence of partial retirement - prematurely withdraw from the labour force completely. The results presented in section 1.5.1 are indicative that this is not the kind of person primarily attracted to partial retirement. It seems that an overwhelming majority of people who choose to take up the part-time scheme would have otherwise continued to work full-time. This raises the question whether there are positive labour supply effects of the policy on people with low labour force attachment that are overshadowed by the large-scale take-up of people who are really at no risk to stop working before formal retirement. If this were the case then doing a better job at targeting the policy to people with low attachment might significantly improve the effectiveness of partial retirement at increasing old-age labour supply.

To assess this possibility, I repeat the analysis above for those individuals in the original

sample that I identify as having a high propensity for early effective retirement. I identify "high risk" people in my sample by looking at the characteristics of early retirees of earlier cohorts, not exposed to partial retirement. In particular I use a sample of 125 605 individuals from the cohorts of 1930 to 1934 (men) and 1935 to 1939 women, selected in the exact same way as the original sample with respect to their working history etc.⁹ These people have reached the normal retirement age *before* partial retirement was introduced and are thus unaffected by the policy. I define early effective retirement in this setting as a withdrawal from the labour force (last employment spell) more than three months before reaching the earliest legal pension age (the ERA at the time was 55 for women and 60 for men). I run a linear regression of an indicator of whether the individual is an early effective retiree on some baseline characteristics (Gender, white/blue collar status and log earnings age 48/53, log firm size, log tenure at the firm, dummies for ever having experienced unemployment or extended sickness in the past 10 years, Industry). I then use these coefficients and the same characteristics to predict the "risk" of early effective retirement of the original sample. I classify "high risk" individuals above median predicted probability of early labour force withdrawal. Tables 1.10 and 1.11 show the characteristics of the "high risk" vs. the low risk in the original sample. Most notably, the high risk sample consists predominantly of men and blue collar workers. Many of them have experienced unemployment or extended sickness in the past. The dominant industries are manufacturing and retail. Tables 1.12 and 1.13 show the labour market effects focussing exclusively on these "high risk" individuals of the original sample. Table 1.12 shows the effects on the retirement age of this group, while Table 1.13 shows the effects on labour market status over a ten year horizon.

Comparing the results in Table 1.13 to those in Table 1.9 discussed in section 1.5.1, one can see that indeed the positive participation effects of partial retirement are more pronounced focussing on this group of individuals with low predicted labour market at-

⁹This means that they are in stable employment at age 53, working in a firm with more than 3 employees, outside of seasonal industries and fulfill the criteria of eligibility in terms of working history for partial retirement in principle. The only difference being that because employment spells are recorded only from 1972 onwards, I cannot observe a working history of 25 years, so I use individuals having worked 60% of the 10 years before reaching age 48/53.

tachment. While the effects on the take-up of partial retirement are very similar to those of the entire sample (282 over 10 years), the number of days spent out of employment are reduced much more for the treated of this group. The days spent out of employment are reduced by 80 days in the preferred specification over 10 years. This corresponds to 28% of the additional partial retirement days, almost twice as large as for the entire sample where this percentage was only 15%.¹⁰ Most of this positive employment effect is caused by a reduction of the days spent in disability. The days spent in disability of the treated group is reduced by about 14% relative to the already high mean over 10 years of the control group of the "high risk" sample. This percentage is below 10 for the general sample. The effective retirement age for the high risk sample is significantly increased by about 2 months on average.

The take-away from this exercise should be that effective targeting can improve the effectiveness of part-time policies significantly. While we have seen that they are attractive to a lot of different workers - many of whom are at a low risk to exit the labour force prematurely - part-time can have positive employment effects acting as an alternative for instance to disability insurance.

1.5.2 Spill-over Effects

As mentioned earlier, it is also interesting in this setting to look at the reaction of the employer to partial retirement. In my empirical setting, I have a strong sudden shock to partial retirement uptake in December 2003 for individuals born before the cut-off. Looking at the employer of these individuals¹¹ Figure 1.8, panel a) shows the monthly treatment effects on total hires around this date. Despite the relatively large standard errors, we see an increase in the hiring of treated firms in months 0-2 after the reform-induced spike in partial retirements. This suggests that replacement workers are hired to replace some of the labour lost through the retirements. The exact amount of labour that

¹⁰In fact, when I look at the other half of the sample, having only low risk of premature exit of the labour force, the relation of the increase of the days spent in partial retirement and decrease in full-time employment days is almost one-to-one. This means that among this group, partial retirement caused almost exclusively a reduction in full-time work, with no positive effects on the extensive margin.

¹¹For a detailed explanation on how I translate individual level treatment to firm or peer level outcomes please refer to the methodology section of this paper.

is being replaced is difficult to pinpoint because of the imprecise estimates. However, if we take the point estimates in Figure 1.8 seriously then we have an increase in hires in treated firm of about .1 in the first 2 months after the reform (a little less but still sizable in month 3). If we consider that treated individuals at the cut-off are about 17% more likely to enter partial retirement in a window of 3 months prior to the reform, then it looks like full replacement is achieved already within 2 months after the spike in take-up. There seems to be no effect of partial retirement on the probability of a firm to remain in business.

Figure 1.9 shows the monthly effect of peers' outcomes. If workers going on partial retirement were not easily replaced but their specific human capital valuable to the firm, we would expect the treatment effect on peers' employment and wages to be positive right after the reform. None of these things seem to be the case. Co-workers seem to be rather unaffected by the partial retirement of their elderly colleagues. In part this may of course be due to the fact that retirements can be planned long in advance. Therefore replacements (seen in the increase in hires) can be arranged in advance. This explains the contrast to Jäger (2016) who finds spillovers to co-workers in the case of the unexpected death of a worker.

1.6 Conclusion

This paper estimates the effects of a policy that allows "partial" retirement through a subsidised part-time scheme for elderly workers. The policy was implemented in the Austrian context of the 2000s where older workers tended to retire prematurely through various forms of non-employment. Partial retirement was an attempt to make employment more attractive to those individuals. I show that in the Austrian context the policy attracted considerably more people from full-time work into partial retirement than those who would have otherwise not been employed. To be precise, only 8% of the increase in days spent in partial retirement can be attributed to days otherwise spent in non-employment

over a five year horizon. Over a ten year horizon it is only slightly more at 15%. This means that in the Austrian setting partial retirement had overall strong negative effects on labour supply. I do however find more positive employment effects among individuals with characteristics associated with a high risk of early retirement. This suggests that more effective targeting (for instance as a part of the disability insurance scheme) could improve the effectiveness of part-time policies significantly.

I also evaluate spill-overs of partial retirement to other labour market participants. I find that the policy increased hirings in the firms that sent workers to partial retirement and that elderly workers were in fact replaced quite quickly. I do not find any effects in terms of earnings or tenure for their co-workers in the same firms.

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Appendix: Tables

Table 1.1: Summary statistics and selection into partial retirement

	Ever Partial retirement					
	Row %			Column %		
	No	Yes	Total	No	Yes	Total
Gender						
Men	77,36	22,64	100,00	52,46	57,87	53,59
Women	80,97	19,03	100,00	47,54	42,13	46,41
Total	79,03	20,97	100,00	100,00	100,00	100,00
Blue/White collar						
Blue collar	82,89	17,11	100,00	41,62	32,40	39,69
White collar	76,50	23,50	100,00	58,38	67,60	60,31
Total	79,03	20,97	100,00	100,00	100,00	100,00
Monthly earnings						
0 - 1300	94,87	5,13	100,00	12,23	2,49	10,19
1300 - 1999	84,41	15,59	100,00	26,09	18,17	24,43
2000-2999	76,26	23,74	100,00	32,25	37,84	33,43
≥ 3000	72,78	27,22	100,00	29,43	41,50	31,96
Total	79,03	20,97	100,00	100,00	100,00	100,00
Tenure at firm						
less than 1 year	83,94	16,06	100,00	7,20	5,19	6,78
1-5 years	82,31	17,69	100,00	20,93	16,96	20,10
5-10 years	81,08	18,92	100,00	19,43	17,09	18,94
over 10 years	76,49	23,51	100,00	52,44	60,76	54,18
Total	79,03	20,97	100,00	100,00	100,00	100,00
Has been sick last 10 yr						
No	77,89	22,11	100,00	77,18	82,60	78,32
Yes	83,18	16,82	100,00	22,82	17,40	21,68
Total	79,03	20,97	100,00	100,00	100,00	100,00
Has been unemployed last 10 yr						
No	77,06	22,94	100,00	75,61	84,83	77,54
Yes	85,84	14,16	100,00	24,39	15,17	22,46
Total	79,03	20,97	100,00	100,00	100,00	100,00

Note: This table describes the composition of the total sample of 177,764 individuals.

Table 1.2: Summary statistics and selection into partial retirement

	Ever Partial retirement					
	Row %			Column %		
	No	Yes	Total	No	Yes	Total
Firm size						
4 - 40	83,88	16,12	100,00	26,26	19,02	24,75
40-199	79,51	20,49	100,00	25,35	24,64	25,20
200-999	74,91	25,09	100,00	26,00	32,84	27,44
≥ 1000	78,21	21,79	100,00	22,38	23,50	22,62
Total	79,03	20,97	100,00	100,00	100,00	100,00
Industry						
Manufacturing	73,41	26,59	100,00	26,61	36,34	28,65
Energy	55,49	44,51	100,00	1,33	4,03	1,90
Water and Sewage	77,78	22,22	100,00	0,51	0,55	0,52
Retail	82,02	17,98	100,00	17,78	14,70	17,14
Transportation	87,74	12,26	100,00	6,04	3,18	5,44
Information and Communication	80,90	19,10	100,00	1,62	1,45	1,59
Finance and Insurance	72,77	27,23	100,00	6,40	9,03	6,95
Real estate	89,18	10,82	100,00	2,01	0,92	1,78
Scientific and Technical Services	76,19	23,81	100,00	3,87	4,56	4,01
Other economic services	90,58	9,42	100,00	3,22	1,26	2,81
Public Administration, Defense	84,76	15,24	100,00	19,24	13,04	17,94
Education	87,66	12,34	100,00	1,22	0,65	1,10
Health and Social services	78,03	21,97	100,00	6,18	6,56	6,26
Art, Entertainment, Recreation	88,46	11,54	100,00	0,82	0,41	0,74
Other	78,03	21,97	100,00	3,14	3,34	3,18
Total	79,03	20,97	100,00	100,00	100,00	100,00
Region						
Burgenland	77,21	22,79	100,00	2,15	2,39	2,20
Kaernten	81,52	18,48	100,00	5,38	4,60	5,21
Niederoesterreich	74,61	25,39	100,00	14,44	18,53	15,30
Oberoesterreich	70,25	29,75	100,00	15,45	24,67	17,38
Salzburg	81,50	18,50	100,00	7,14	6,11	6,93
Steiermark	81,86	18,14	100,00	12,75	10,65	12,31
Tirol	84,31	15,69	100,00	6,61	4,64	6,19
Vorarlberg	84,02	15,98	100,00	4,69	3,36	4,41
Wien	82,22	17,78	100,00	29,53	24,07	28,39
Unknown	87,66	12,34	100,00	1,85	0,98	1,67
Total	79,03	20,97	100,00	100,00	100,00	100,00

Note: This table describes the composition of the total sample of 177,764 individuals.

Table 1.3: Selection into partial retirement - Probit model

	(1)	
Woman	0.0209***	(0.00425)
White collar	0.0425***	(0.00802)
Log yearly earnings	0.0657***	(0.00328)
Log tenure in firm	0.00541**	(0.00169)
Has been sick last 10 yr	-0.00893*	(0.00357)
Has been unemployed last 10 yr	-0.0310***	(0.00375)
Log firm size	0.00424	(0.00331)
Avg wage in firm 2002	0.00177***	(0.000229)
Industry		
— <i>Manufacturing</i>	0	(.)
— <i>Energy</i>	0.0887	(0.0472)
— <i>Water and Sewage</i>	-0.0237	(0.0331)
— <i>Retail</i>	-0.0463***	(0.00844)
— <i>Transportation</i>	-0.103***	(0.0174)
— <i>Information and Communication</i>	-0.0919***	(0.0271)
— <i>Finance and Insurance</i>	-0.0356*	(0.0149)
— <i>Real estate</i>	-0.115***	(0.0183)
— <i>Scientific and Technical Services</i>	-0.0408	(0.0211)
— <i>Other economic services</i>	-0.0925***	(0.0134)
— <i>Public Administration, Defense</i>	-0.0910***	(0.0179)
— <i>Education</i>	-0.113***	(0.0307)
— <i>Health and Social services</i>	-0.00998	(0.0125)
— <i>Art, Entertainment, Recreation</i>	-0.117***	(0.0195)
— <i>Other</i>	-0.00260	(0.0169)
Region		
— <i>Burgenland</i>	0	(.)
— <i>Kaernten</i>	-0.0595*	(0.0250)
— <i>Niederoesterreich</i>	0.0197	(0.0222)
— <i>Oberoesterreich</i>	0.0517*	(0.0218)
— <i>Salzburg</i>	-0.0540**	(0.0204)
— <i>Steiermark</i>	-0.0607*	(0.0258)
— <i>Tirol</i>	-0.0867***	(0.0227)
— <i>Vorarlberg</i>	-0.100***	(0.0219)
— <i>Wien</i>	-0.0862***	(0.0210)
— <i>Unknown</i>	-0.0704*	(0.0304)
Observations	177764	

Table 1.4: Individuals' pre-treatment balancing regressions

PANEL A				
	(1)	(2)	(3)	(4)
	Woman	White collar	Log yearly earnings	Log tenure in firm
RD_Estimate	-0.012 [0.015]	-0.010 [0.017]	0.004 [0.015]	0.056 [0.035]
Observations	177764	177764	177764	177764

PANEL B				
	(1)	(2)	(3)	(4)
	Sick last 10 yr	Unemployed last 10 yr	Log firm size	Avg wage in firm 2002
RD_Estimate	0.006 [0.011]	-0.004 [0.010]	-0.043 [0.183]	0.279 [0.828]
Observations	177764	177764	177764	177764

Note: This table contains the the ITT estimates of outcomes measured pre-treatment of being born such that pre-reform policy was available. The optimal bandwidth and robust standard errors (in parentheses) are calculated following the methodology proposed by Calonico et al. (2014a) and Calonico et al. (2014b), though results are robust to the use of a predetermined bandwidth of 12 months.

Table 1.5: ITT effect, individuals' effect on age at retirement

	(1)	(2)	(3)	(4)
	Retirement age	Retirement age	Age last employed	Age last employed
RD_Estimate	0.083 [0.081]	0.015 [0.064]	0.094 [0.070]	0.074 [0.069]
Mean control group	60.386	60.386	58.709	58.709
Observations	177764	177764	177764	177764
Controls	No	Yes	No	Yes

Note: Age at retirement is measured in distance from the normal retirement age (60 for women, 65 for men). This table contains the the ITT estimates on retirement age of being born such that pre-reform policy was available for individuals' outcomes of the linear RDD framework as described in section 1.4.1. The optimal bandwidth and robust standard errors (in parentheses) are calculated following the methodology proposed by Calonico et al. (2014a) and Calonico et al. (2014b), though results are robust to the use of a predetermined bandwidth of 12 months. Controls include gender, white/blue collar, firm size, earnings at 48/53, days tenure at 48/53, working history of the last 10 years before turning 48/53 (sick days, days unemployed).

Table 1.6: ITT effect, individuals' cumulative outcomes over 5 years

PANEL A				
	(1)	(2)	(3)	(4)
	Ever in partial ret	Ever in partial ret	Ever not employed	Ever not employed
RD_Estimate	0.102*** [0.012]	0.102*** [0.012]	-0.021* [0.011]	-0.023** [0.010]
Mean control group	0.136	0.136	0.328	0.328
Observations	177764	177764	177764	177764
Controls	No	Yes	No	Yes
PANEL B				
	(1)	(2)	(3)	(4)
	Ever in disability	Ever in disability	Ever unemployed	Ever unemployed
RD_Estimate	-0.013* [0.007]	-0.016** [0.007]	-0.016 [0.011]	-0.016* [0.009]
Mean control group	0.118	0.118	0.173	0.173
Observations	177764	177764	177764	177764
Controls	No	Yes	No	Yes

Note: This table contains the the ITT estimates of outcomes cumulated over 5 years (measured in age from 50/55) of being born such that pre-reform policy was available for individuals' outcomes of the linear RDD framework as described in section 1.4.1. The optimal bandwidth and robust standard errors (in parentheses) are calculated following the methodology proposed by Calonico et al. (2014a) and Calonico et al. (2014b), though results are robust to the use of a predetermined bandwidth of 12 months. Controls include gender, white/blue collar, firm size, earnings at 48/53, days tenure at 48/53, working history of the last 10 years before turning 48/53 (sick days, days unemployed).

Table 1.7: ITT effect, individuals' cumulative outcomes over 5 years

PANEL A				
	(1)	(2)	(3)	(4)
	Days in partial ret	Days in partial ret	Days in full-time	Days in full-time
RD_Estimate	223.109*** [16.375]	224.256*** [15.937]	-210.416*** [21.135]	-205.136*** [19.038]
Mean control group	103.346	103.346	1467.070	1467.070
Observations	177764	177764	177764	177764
Controls	No	Yes	No	Yes
PANEL B				
	(1)	(2)	(3)	(4)
	Days not employed	Days not employed	Days unemployed	Days unemployed
RD_Estimate	-12.141 [13.063]	-17.351 [12.361]	-3.148 [5.386]	-3.641 [4.912]
Mean control group	255.830	255.830	67.961	67.961
Observations	177764	177764	177764	177764
Controls	No	Yes	No	Yes
PANEL C				
	(1)	(2)	(3)	(4)
	Days in disability	Days in disability	Days retirement	Days retirement
RD_Estimate	-5.215 [7.715]	-9.063 [7.627]	-1.352 [5.894]	-1.751 [5.589]
Mean control group	103.534	103.534	13.240	13.240
Observations	177764	177764	177764	177764
Controls	No	Yes	No	Yes

Note: This table contains the the ITT estimates of outcomes cumulated over 5 years (measured in age from 50/55) of being born such that pre-reform policy was available for individuals' outcomes of the linear RDD framework as described in section 1.4.1. The optimal bandwidth and robust standard errors (in parentheses) are calculated following the methodology proposed by Calonico et al. (2014a) and Calonico et al. (2014b), though results are robust to the use of a predetermined bandwidth of 12 months. Controls include gender, white/blue collar, firm size, earnings at 48/53, days tenure at 48/53, working history of the last 10 years before turning 48/53 (sick days, days unemployed).

Table 1.8: ITT effect, individuals' cumulative outcomes over 10 years

PANEL A				
	(1)	(2)	(3)	(4)
	Ever in partial ret	Ever in partial ret	Ever not employed	Ever not employed
RD_Estimate	0.096*** [0.013]	0.097*** [0.013]	0.001 [0.008]	-0.003 [0.008]
Mean control group	0.165	0.165	0.888	0.888
Observations	177764	177764	177764	177764
Controls	No	Yes	No	Yes
PANEL B				
	(1)	(2)	(3)	(4)
	Ever in disability	Ever in disability	Ever unemployed	Ever unemployed
RD_Estimate	-0.012 [0.008]	-0.016** [0.008]	-0.004 [0.013]	-0.005 [0.011]
Mean control group	0.152	0.152	0.215	0.215
Observations	177764	177764	177764	177764
Controls	No	Yes	No	Yes

Note: This table contains the the ITT estimates of outcomes cumulated over 10 years (measured in age from 50/55) of being born such that pre-reform policy was available for individuals' outcomes of the linear RDD framework as described in section 1.4.1. The optimal bandwidth and robust standard errors (in parentheses) are calculated following the methodology proposed by Calonico et al. (2014a) and Calonico et al. (2014b), though results are robust to the use of a predetermined bandwidth of 12 months. Controls include gender, white/blue collar, firm size, earnings at 48/53, days tenure at 48/53, working history of the last 10 years before turning 48/53 (sick days, days unemployed).

Table 1.9: ITT effect, individuals' cumulative outcomes over 10 years

PANEL A				
	(1)	(2)	(3)	(4)
	Days in partial ret	Days in partial ret	Days in full-time	Days in full-time
RD_Estimate	275.665*** [22.118]	277.828*** [22.183]	-256.356*** [38.918]	-238.668*** [31.636]
Mean control group	185.048	185.048	1983.929	1983.929
Observations	177764	177764	177764	177764
Controls	No	Yes	No	Yes
PANEL B				
	(1)	(2)	(3)	(4)
	Days not employed	Days not employed	Days unemployed	Days unemployed
RD_Estimate	-27.406 [26.259]	-43.633* [23.832]	1.167 [8.723]	-0.141 [8.268]
Mean control group	1483.722	1483.722	122.473	122.473
Observations	177764	177764	177764	177764
Controls	No	Yes	No	Yes
PANEL C				
	(1)	(2)	(3)	(4)
	Days in disability	Days in disability	Days retirement	Days retirement
RD_Estimate	-22.288 [19.999]	-32.556* [19.430]	5.771 [24.334]	-2.618 [21.308]
Mean control group	336.079	336.079	846.249	846.249
Observations	177764	177764	177764	177764
Controls	No	Yes	No	Yes

Note: This table contains the the ITT estimates of outcomes cumulated over 10 years (measured in age from 50/55) of being born such that pre-reform policy was available for individuals' outcomes of the linear RDD framework as described in section 1.4.1. The optimal bandwidth and robust standard errors (in parentheses) are calculated following the methodology proposed by Calonico et al. (2014a) and Calonico et al. (2014b), though results are robust to the use of a predetermined bandwidth of 12 months. Controls include gender, white/blue collar, firm size, earnings at 48/53, days tenure at 48/53, working history of the last 10 years before turning 48/53 (sick days, days unemployed).

Table 1.10: Summary statistics and selection into early effective retirement

	High risk of early LF withdrawal					
	Row %			Column %		
	No	Yes	Total	No	Yes	Total
Gender						
Men	28,38	71,62	100,00	30,42	76,76	53,59
Women	74,97	25,03	100,00	69,58	23,24	46,41
Total	50,00	50,00	100,00	100,00	100,00	100,00
Blue/White collar						
Blue collar	23,28	76,72	100,00	18,48	60,89	39,69
White collar	67,58	32,42	100,00	81,52	39,11	60,31
Total	50,00	50,00	100,00	100,00	100,00	100,00
Monthly earnings						
0 - 1300	60,86	39,14	100,00	12,40	7,97	10,19
1300 - 1999	49,04	50,96	100,00	23,96	24,89	24,43
2000-2999	45,33	54,67	100,00	30,31	36,55	33,43
≥ 3000	52,15	47,85	100,00	33,34	30,59	31,96
Total	50,00	50,00	100,00	100,00	100,00	100,00
Tenure at firm						
less than 1 year	50,26	49,74	100,00	6,81	6,74	6,78
1-5 years	47,22	52,78	100,00	18,98	21,22	20,10
5-10 years	49,60	50,40	100,00	18,79	19,09	18,94
over 10 years	51,14	48,86	100,00	55,42	52,95	54,18
Total	50,00	50,00	100,00	100,00	100,00	100,00
Sick last 10 yr						
No	57,61	42,39	100,00	90,24	66,39	78,32
Yes	22,51	77,49	100,00	9,76	33,61	21,68
Total	50,00	50,00	100,00	100,00	100,00	100,00
Unemployed last 10 yr						
No	52,80	47,20	100,00	81,89	73,20	77,54
Yes	40,32	59,68	100,00	18,11	26,80	22,46
Total	50,00	50,00	100,00	100,00	100,00	100,00

Table 1.11: Summary statistics and selection into early effective retirement

	High risk of early LF withdrawal					
	Row %			Column %		
	No	Yes	Total	No	Yes	Total
Firm size						
4 - 40	58,77	41,23	100,00	29,09	20,41	24,75
40-199	43,91	56,09	100,00	22,13	28,27	25,20
200-999	41,26	58,74	100,00	22,64	32,23	27,44
≥ 1000	57,79	42,21	100,00	26,14	19,09	22,62
Total	50,00	50,00	100,00	100,00	100,00	100,00
Industry						
Manufacturing	6,18	93,82	100,00	3,54	53,76	28,65
Energy	88,95	11,05	100,00	3,38	0,42	1,90
Water and Sewage	34,95	65,05	100,00	0,36	0,68	0,52
Retail	33,55	66,45	100,00	11,50	22,77	17,14
Transportation	50,75	49,25	100,00	5,52	5,36	5,44
Information and Communication	48,55	51,45	100,00	1,54	1,63	1,59
Finance and Insurance	92,39	7,61	100,00	12,85	1,06	6,95
Real estate	55,60	44,40	100,00	1,98	1,58	1,78
Scientific and Technical Services	61,63	38,37	100,00	4,94	3,08	4,01
Other economic services	40,65	59,35	100,00	2,28	3,33	2,81
Public Administration, Defense	88,91	11,09	100,00	31,91	3,98	17,94
Education	96,31	3,69	100,00	2,12	0,08	1,10
Health and Social services	87,87	12,13	100,00	11,00	1,52	6,26
Art, Entertainment, Recreation	87,70	12,30	100,00	1,29	0,18	0,74
Other	91,07	8,93	100,00	5,80	0,57	3,18
Total	50,00	50,00	100,00	100,00	100,00	100,00

Table 1.12: ITT effect, high risk individuals' effect on age at retirement

	(1)	(2)	(3)	(4)
	Retirement age	Retirement age	Age last employed	Age last employed
RD_Estimate	0.087 [0.118]	0.023 [0.092]	0.180* [0.096]	0.158* [0.086]
Mean control group	61.375	61.375	59.128	59.128
Observations	88882	88882	88882	88882
Controls	No	Yes	No	Yes

Note: The results in this table are for individuals with above median propensity to withdraw from the labour force before reaching the earliest formal retirement age. This table contains the the ITT estimates on retirement age of being born such that pre-reform policy was available for individuals' outcomes of the linear RDD framework as described in section 1.4.1. The optimal bandwidth and robust standard errors (in parentheses) are calculated following the methodology proposed by Calonico et al. (2014a) and Calonico et al. (2014b), though results are robust to the use of a predetermined bandwidth of 12 months. Controls include gender, white/blue collar, firm size, earnings at 48/53, days tenure at 48/53, working history of the last 10 years before turning 48/53 (sick days, days unemployed).

Table 1.13: ITT effect, high risk individuals' cumulative outcomes over 10 years

PANEL A				
	(1)	(2)	(3)	(4)
	Days in partial ret	Days in partial ret	Days in full-time	Days in full-time
RD_Estimate	272.565*** [25.607]	282.287*** [24.929]	-208.555*** [38.121]	-204.905*** [35.081]
Mean control group	173.837	173.837	1702.812	1702.812
Observations	88882	88882	88882	88882
Controls	No	Yes	No	Yes
PANEL B				
	(1)	(2)	(3)	(4)
	Days not employed	Days not employed	Days unemployed	Days unemployed
RD_Estimate	-63.781** [29.481]	-80.255*** [30.126]	-20.809 [12.969]	-23.769* [13.344]
Mean control group	1776.081	1776.081	159.185	159.185
Observations	88882	88882	88882	88882
Controls	No	Yes	No	Yes
PANEL C				
	(1)	(2)	(3)	(4)
	Days in disability	Days in disability	Days retirement	Days retirement
RD_Estimate	-62.633* [36.705]	-70.572** [32.838]	0.697 [35.091]	-5.170 [29.982]
Mean control group	493.988	493.988	928.131	928.131
Observations	88882	88882	88882	88882
Controls	No	Yes	No	Yes

Note: The results in this table are for individuals with above median propensity to withdraw from the labour force before reaching the earliest formal retirement age. This table contains the the ITT estimates of outcomes cumulated over 10 years (measured in age from 50/55) of being born such that pre-reform policy was available for individuals' outcomes of the linear RDD framework as described in section 1.4.1. The optimal bandwidth and robust standard errors (in parentheses) are calculated following the methodology proposed by Calonico et al. (2014a) and Calonico et al. (2014b), though results are robust to the use of a predetermined bandwidth of 12 months. Controls include gender, white/blue collar, firm size, earnings at 48/53, days tenure at 48/53, working history of the last 10 years before turning 48/53 (sick days, days unemployed).

Appendix: Figures

Figure 1.1: Timeline of retirement in Austria

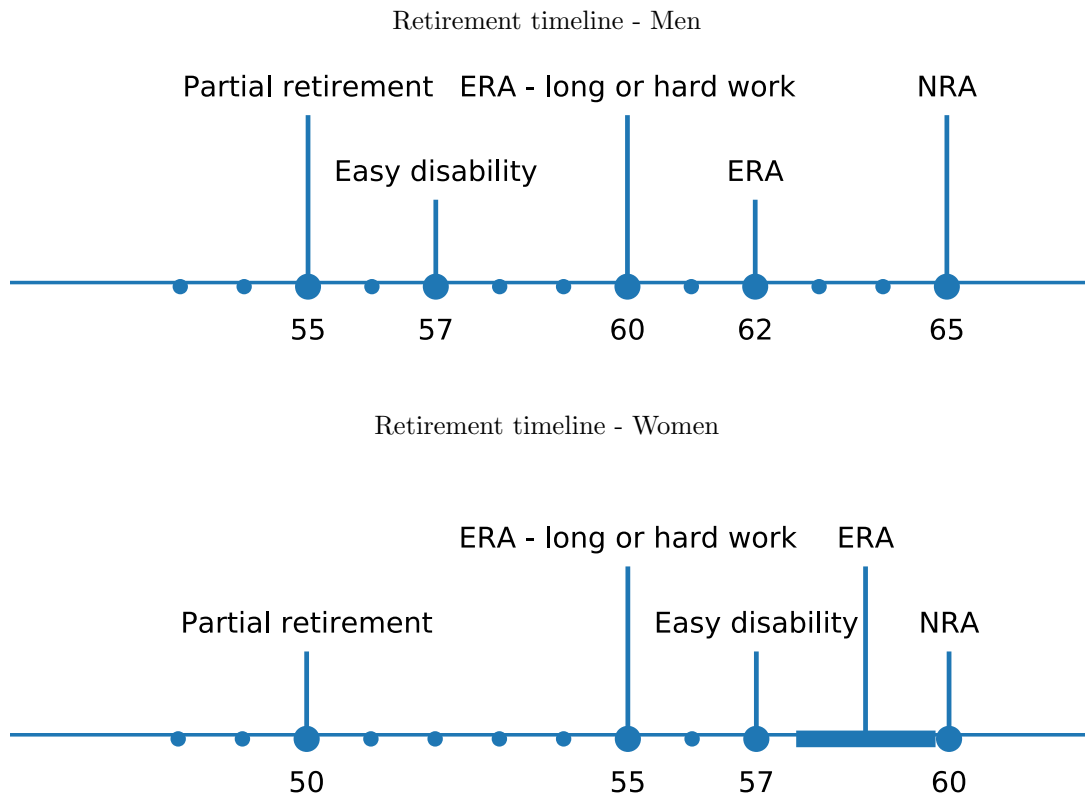
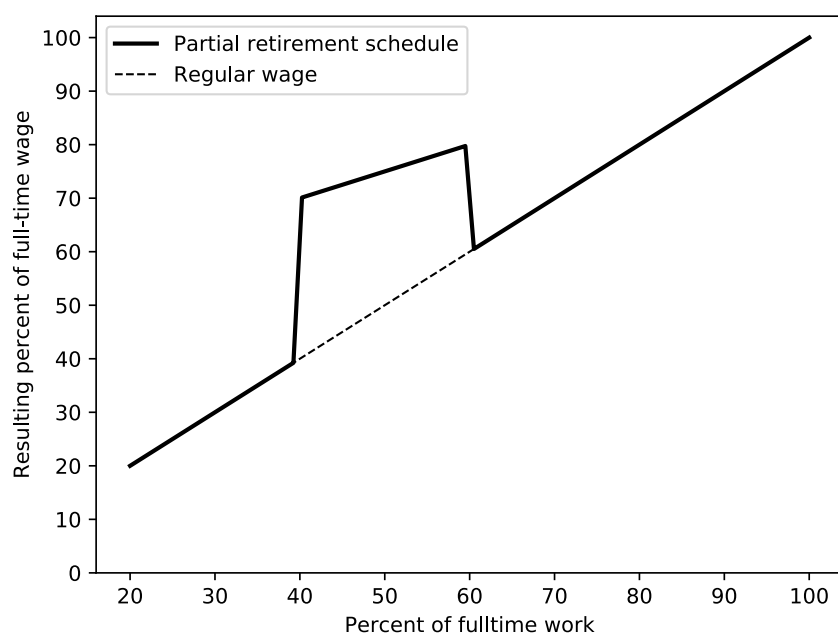
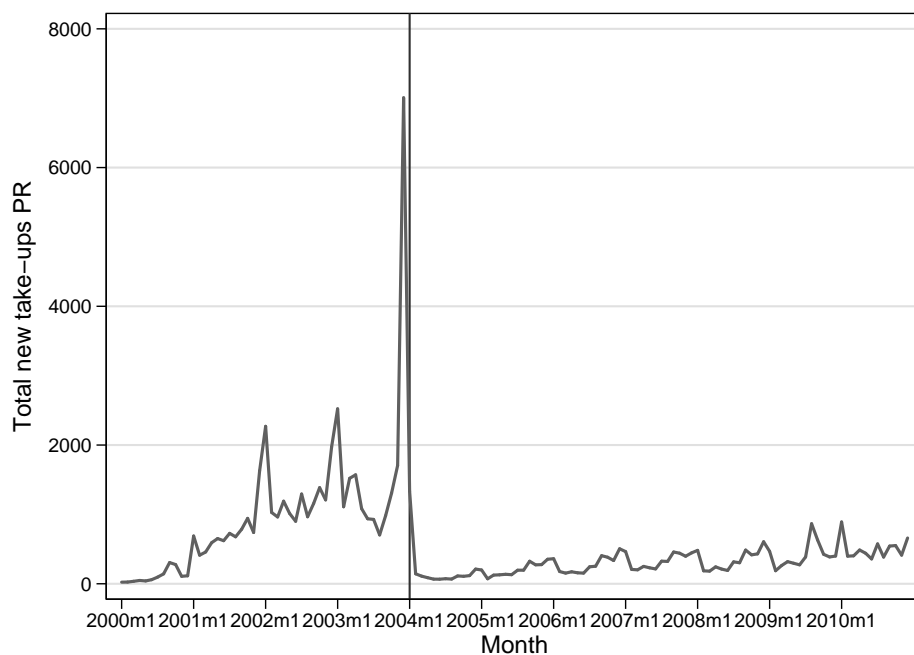


Figure 1.2: Replacement rate of partial retirement



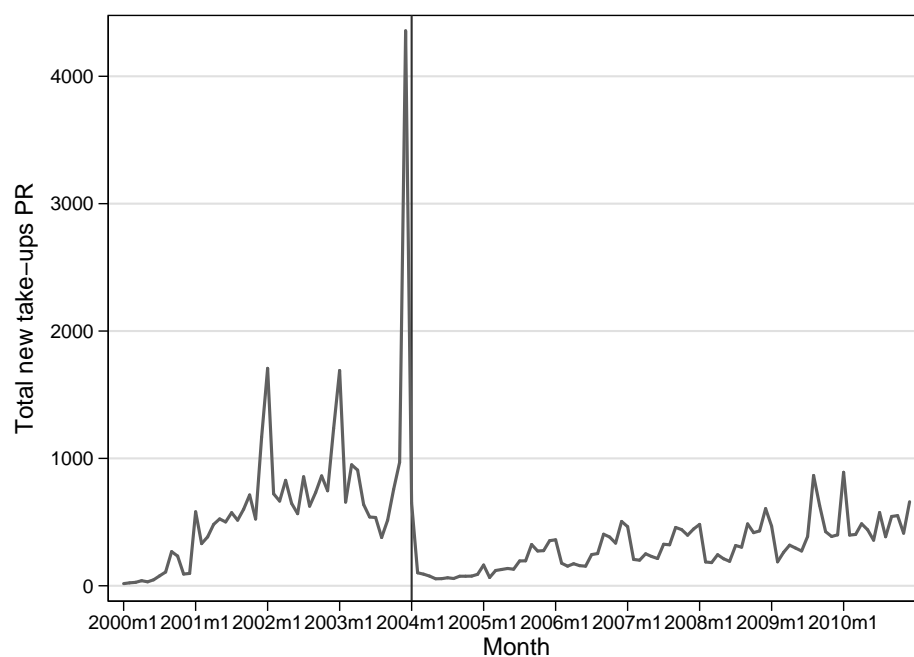
Note: This graph shows the expected fraction of fulltime wage received by the fraction of fulltime hours worked. In particular it shows the size of the partial retirement subsidy if hours are reduced to 40-60% of fulltime.

Figure 1.3: Total entries to partial retirement per month



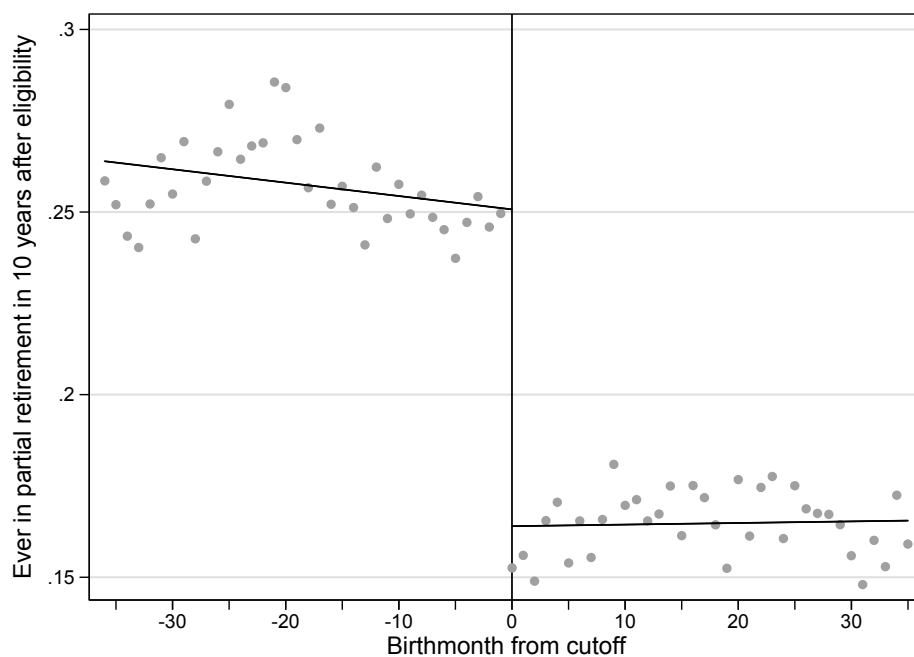
Note: This graph shows the total number of people newly entering a partial retirement scheme in Austria by month. January 2004 is the date the reform described in section 1.2.2 was enacted.

Figure 1.4: Total entries to partial retirement per month - above 51/56 year olds



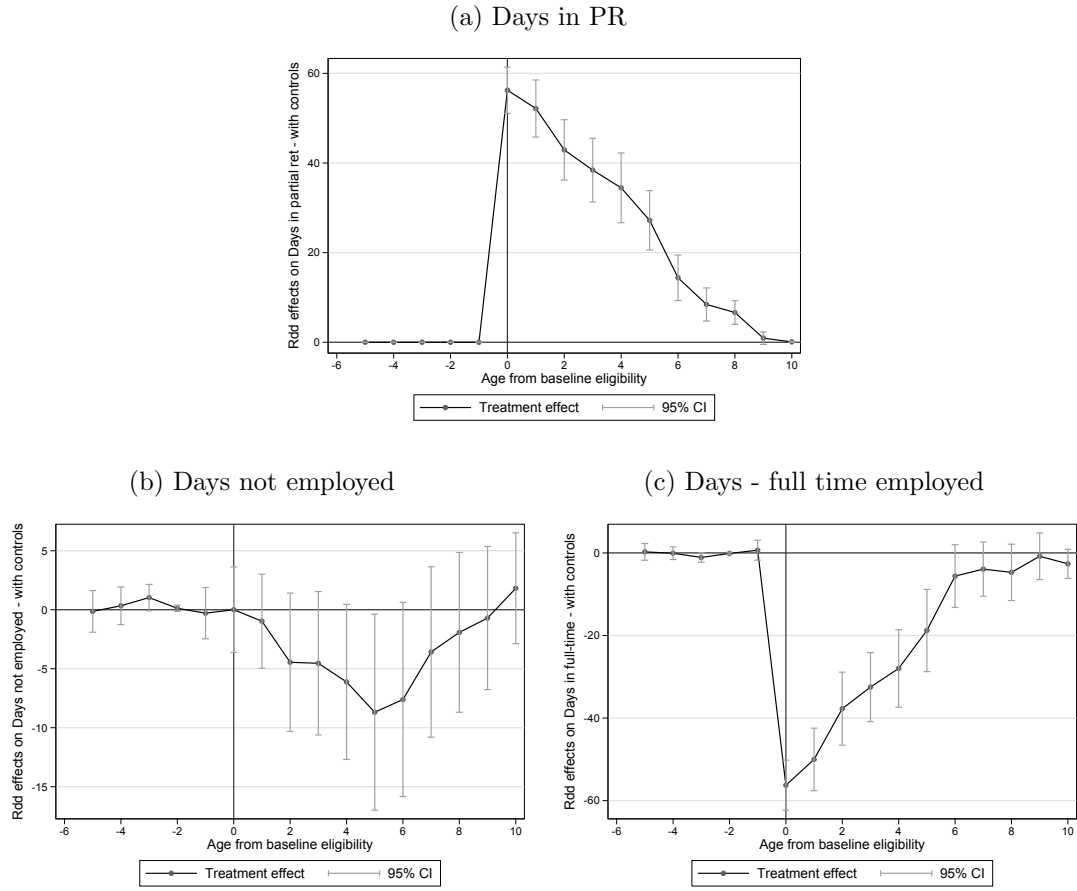
Note: This graph shows the total number of women above the age of 51 and men above the age of 56 (one year after original eligibility age) newly entering a partial retirement scheme in Austria by month. January 2004 is the date the reform described in section 1.2.2 was enacted. This graph shows that the last minute effect is not only driven by people who experienced a hike in the eligibility age at the time of the reform (from 50 to 50.5 or 55 to 55.5) but that in fact the requirement of hiring a substitute worker was an important cause of the last minute effect in the take-up rate.

Figure 1.5: Ever on partial retirement by birthmonth



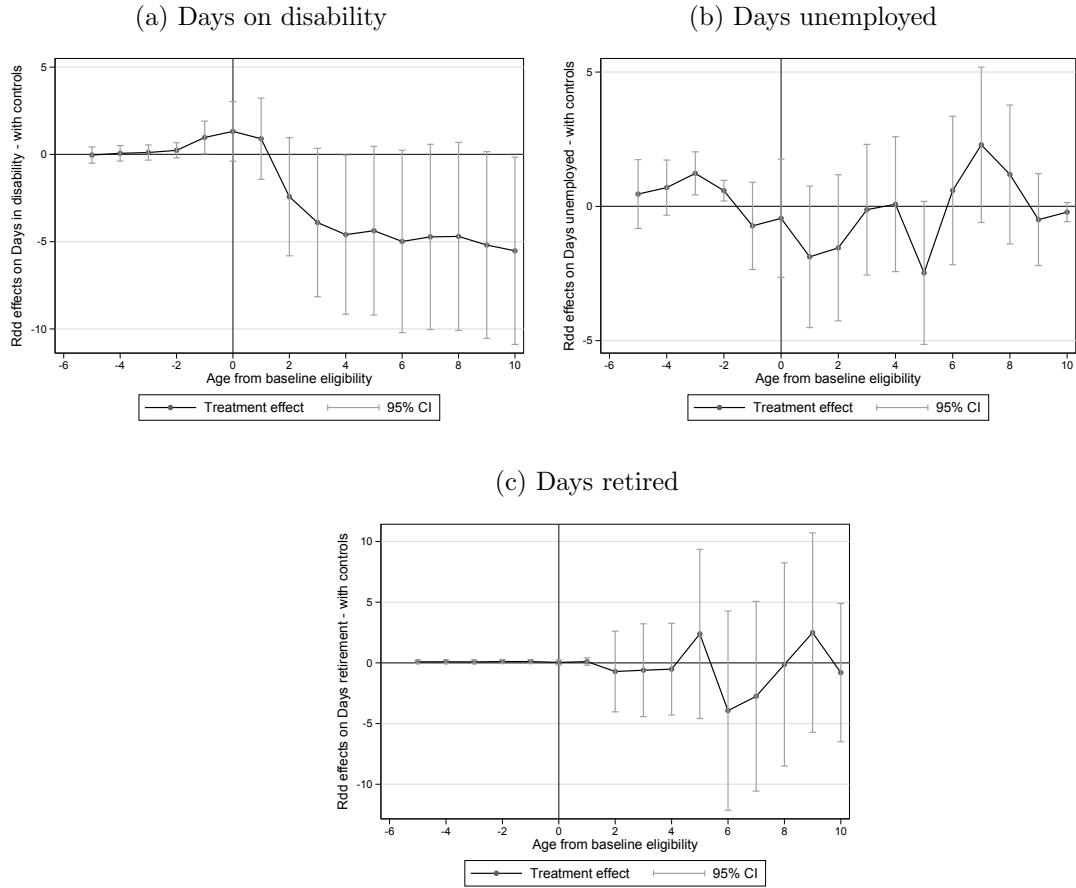
Note: This graph shows the average probability of ever taking up the partial retirement policy within 10 years from reaching the baseline eligibility age by birthmonth. It is based on the sample of individuals born within three years from the cut-off as described in section 1.3. Persons left of the cut-off could take advantage of the more lenient pre-reform version of partial retirement. Persons born right of the cut-off were subject to the substitute worker requirement and had an eligibility age increased by 6 months.

Figure 1.6: Yearly event study RDD - Employment



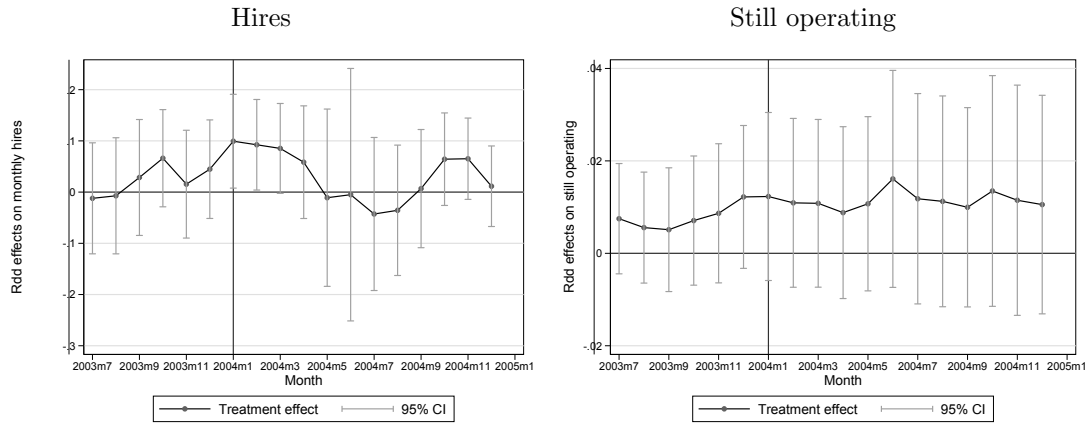
Note: This figure shows the ITT estimates of the linear RDD framework as described in section 1.4.1 for each year of age (measured age from 50/55). Model includes as controls: Gender, white/blue collar status, log earnings age 48/53, log firm size, log tenure at the firm, dummies for ever having experienced unemployment or extended sickness in the past 10 years. The optimal bandwidth and robust standard errors (in parentheses) are calculated following the methodology proposed by Calonico et al. (2014a) and Calonico et al. (2014b), though results are robust to the use of a predetermined bandwidth of 12 months.

Figure 1.7: Yearly event study RDD - Non-employment



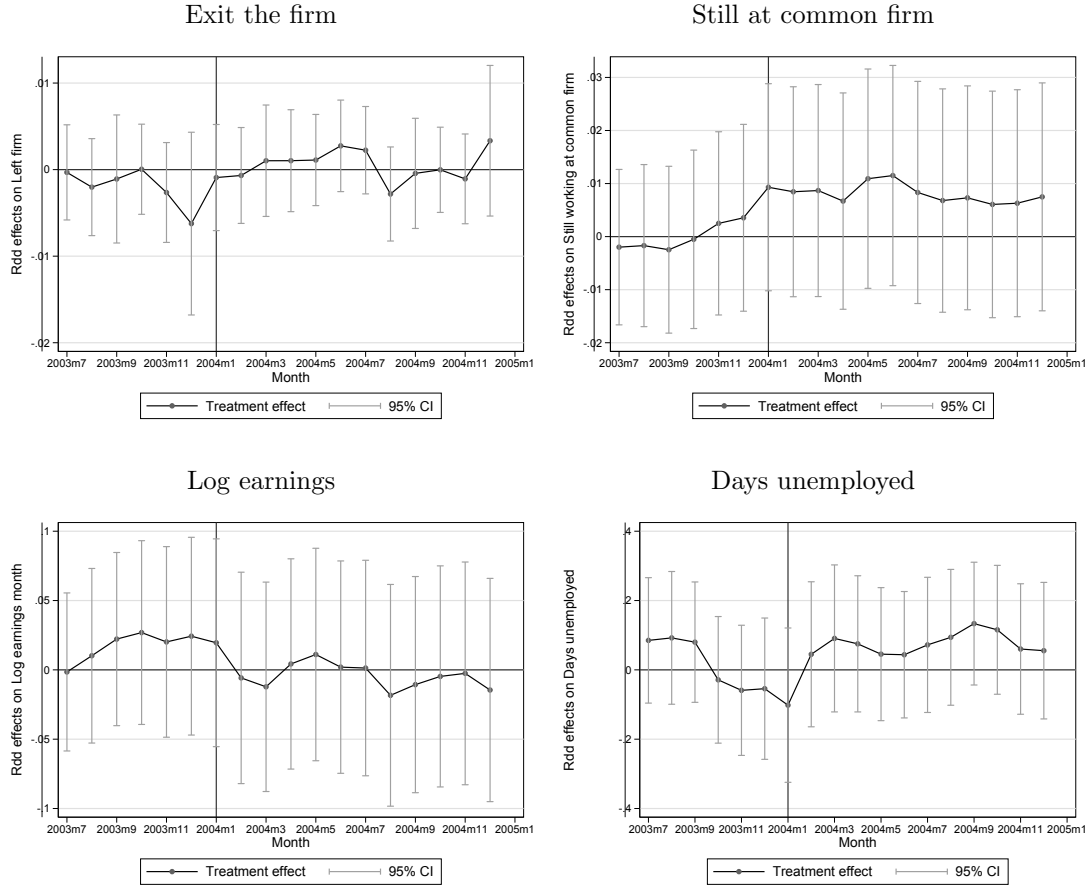
Note: This figure shows the ITT estimates of the linear RDD framework as described in section 1.4.1 for each year of age (measured age from 50/55). Model includes as controls: Gender, white/blue collar status, log earnings age 48/53, log firm size, log tenure at the firm, dummies for ever having experienced unemployment or extended sickness in the past 10 years. The optimal bandwidth and robust standard errors (in parentheses) are calculated following the methodology proposed by Calonico et al. (2014a) and Calonico et al. (2014b), though results are robust to the use of a predetermined bandwidth of 12 months.

Figure 1.8: Monthly firm's outcomes around reform



Note: This figure shows the ITT estimates for total hires and the probability of operating of the firm of the linear RDD framework as described in section 1.4.2 for each month around the reform. There are as many observations as there are workers within the bandwidth in a firm, and each of those observations is weighted by the inverse of that number of workers. In addition to the restrictions discussed in section 1.3 firms are part of the sample if they have 3–40 employees. This specification controls for size and industry of the firm. The optimal bandwidth and robust standard errors (in parentheses) are calculated following the methodology proposed by Calonico et al. (2014a) and Calonico et al. (2014b), though results are robust to the use of a predetermined bandwidth of 12 months.

Figure 1.9: Monthly event study RDD - Peers



Note: This figure shows the ITT estimates for peers' outcomes of the linear RDD framework as described in section 1.4.2 for each month around the reform. For each peer, there are as many observations as there are workers within the bandwidth in a firm, and each of those observations is weighted by the inverse of peers \times critical workers. Exit the firm is a binary variable turning 1 when the peer leaves his/her current firm (regardless where he/she transitions to. Still at common firm is 1 as long as the peer is still employed at the same firm as the firm where he/she was working with the "critical worker". Log earnings are the log earnings received in that month, days unemployed the days spent in unemployment. The specification controls for gender, age and pre-treatment earnings of the peer as well as the size of the initial firm in 2002. For computational reasons these estimates are only computed for a predetermined bandwidth of 12 months. Standard errors are clustered on the common firm level.)

Chapter 2

Learning from Privilege: Parental Background, Education and Wage Dynamics.

2.1 Introduction

A young person's educational and professional success depends in many ways on the one of her parents. Human capital endowments of children from different backgrounds can differ because some qualities and characteristics are genetically transmitted or shaped in very early childhood. Moreover, the amount of time and money that parents choose to invest in their child's skills, learning, motivation or "credentials" depends both on their economic and cultural preferences as well as their intellectual and financial capabilities. In this paper I will focus on the extent and effects of these latter differences. In particular, I am trying to understand whether differences in the parental background of children can be interpreted as a determinant of the cost to obtain education that is independent of their ability endowments. Specifically I argue that having parents with a higher level of education reduces the cost of obtaining a higher education for their children. I will show that if this is the case, a situation arises in which young workers with the same innate ability obtain different levels of schooling or - put differently - young workers with the

same level of schooling but from different parental background have a different level of innate ability.

Unfortunately ability is something that is very rarely observed directly in the data. I therefore use a framework of employer learning - in which employers adapt their wages to the true productivity of the worker over time - to test whether the implications of my assertion hold true in the data.

When young workers enter the labour market it is in general difficult for employers to directly observe their productivity. Instead they will rely on observable characteristics of the worker to form expectations about her productive capabilities. Some of these observable characteristics are exogenously given - like a person's gender, race etc. - while others can be altered or invested in by the individual in question - like for instance education. Spence (1973) calls the former an index and the latter a signal. A signal is a costly action that allows the worker to reveal information about her abilities. The reason this signal is useful is that the cost of investing in a signal like education depends on an individual's innate ability - which is private information the employer would like to have.

The cost of obtaining signals like education naturally depends on other factors aside from ability. For instance, a child that has to travel far to go to school may be less likely to go there than one that lives right next to the school. Even if they have the same ability. This does not mean that the signal is uninformative: On average, it will still be true that children with higher ability invest more in schooling than those with low ability. However, if one does not know which of the two children with the same level of schooling lived far from the school and which lived closer one has to - in the absence of more information - assign the same expected level of ability to both of them.

I argue that parental background constitutes such an unobservable cost to signal. In a world in which - independently of their own abilities - it is more costly for young people coming from a less educated family to obtain education themselves parental background will introduce noise in the education signal. In particular, a person with high ability but less advantaged parental background may end up investing in the same level of education as a person with lower ability but very privileged family background.

Employers who are trying to assess the ability of a newly hired worker but are not able, allowed or willing to observe these differences in the family background will treat both of them the same as they are indistinguishable from their point of view.

In this paper I want to test empirically whether the above hypothesis is likely to be true. In the absence of a reliable measure of true "ability" I invoke a theory on employer learning due to Harris and Holmstrom (1982) that assumes that workers are paid relative to their expected marginal product at all times. As in the previous discussion at the point of hiring a young worker the employer can only form expectations about her productivity given observables and pays her accordingly. The idea of employer learning however is that over time, the employer learns about the true ability of the worker so that after some time the employee's wages come closer and closer to her true marginal productivity. If the above hypothesis is true, then the implication of this model is that given a set of initial observable characteristics the wages of the worker with less privileged background will rise while the wages of the worker from a privileged family will fall (or rise less steeply). I use data on the employment and education history of young adults from the German Socio-Economic Panel to perform a first empirical assessment of this theory. I find indeed that controlling for other factors at the beginning of the career the slope of the wage curve is negatively related to the level of education of the father.

The rest of this paper is structured as follows: Section 2 provides a brief overview on related literature, Section 3 formalises the ideas outlined above about education signalling with noise, Section 4 discusses the empirical implementation and results, Section 5 concludes.

2.2 Literature Review

This work contributes to the extensive literature on social mobility. Theorised for instance by Becker and Tomes (1994) intergenerational income mobility has been empirically estimated among others by Solon (1992), Zimmerman (1992) and Björklund and Jäntti (1997) who have documented the correlation between the earnings of fathers and sons.

More specifically, in the context of education Ermisch and Francesconi (2001) find parent's educational attainment to be strongly correlated to their children's educational attainment in British households between 1991-1997. Feinstein and Symons (1999) show that factors indicating parental interest in the education of their children play a particularly important role in children's secondary school performance. In the context of Germany Dustmann (2004) demonstrates the effect of parental education on wages through the early tracking mechanism of the German educational system.

Furthermore my present work draws on the literature on internal labour market dynamics. Models of symmetric employer learning (Harris and Holmstrom (1982) lay the foundation for my empirical framework which relies heavily on Farber and Gibbons (1996), Altonji and Pierret (2001) and Lange (2007). Farber and Gibbons (1996) demonstrate that employers indeed learn about innate ability over time by finding an increasing relationship of wages with an ability measure unobserved by the employer over time. Altonji and Pierret (2001) provide evidence that employers statistically discriminate on the basis of education when they do not have information about the true ability of the worker. Using a structural model of employer learning Lange (2007) estimates the highly relevant and interesting speed of employer learning to be relatively fast. According to his estimates the initial expectation errors about workers productivity decline by 50% in the first 3 years of a worker's career.

2.3 Education Signalling with Noise

In this first part of the theory I will briefly show how an additional factor influencing the cost of signalling can lead to partially separating equilibria in the Spence signalling model (Spence (1973)) where some individuals with different levels of ability nevertheless decide to invest in the same level of education.

This analysis applies the logic of Riley (2001) and Streb (2006). To avoid unnecessary calculations I will do this in the simplest possible setup with two levels of ability $\theta \in \{\underline{\theta}, \bar{\theta}\}$ and two levels of parental background $b \in \{\underline{b}, \bar{b}\}$. The probabilities of the four different

resulting types of agent are given by: $P(\theta = \bar{\theta}, b = \bar{b}) = p_{hh}$, $P(\theta = \bar{\theta}, b = \underline{b}) = p_{hl}$, $P(\theta = \underline{\theta}, b = \bar{b}) = p_{lh}$ and $P(\theta = \underline{\theta}, b = \underline{b}) = p_{ll}$

The cost $c(\theta, b, s)$ of obtaining the signal education s is increasing in s but decreasing in θ - as in the standard signalling model - but also b .

Given s an employer will form a belief μ about the probability of facing a high productivity type. The employer is risk neutral and operates in a competitive market and therefore pays a wage w equal to the expected productivity of the worker.

Agents of all types maximise expected utility given by:

$$U(w, s, \theta, b) = w - c(s, \theta, b)$$

where $c_s > 0$ and $c_{s,\theta}(s, \theta, b) < 0$ and $c_{s,b}(s, \theta, b) < 0$

In my simple setup with four types there are two possibilities:

$$c(s, \bar{\theta}, \bar{b}) < c(s, \bar{\theta}, \underline{b}) < c(s, \underline{\theta}, \bar{b}) < c(s, \underline{\theta}, \underline{b}) \quad (2.1)$$

$$c(s, \bar{\theta}, \bar{b}) < c(s, \underline{\theta}, \bar{b}) < c(s, \bar{\theta}, \underline{b}) < c(s, \underline{\theta}, \underline{b}) \quad (2.2)$$

Note that which of the two cases applies depends both on the relation between $c_{s,\theta}(s, \theta, b)$ and $c_{s,b}(s, \theta, b)$ as well as $\bar{\theta} - \underline{\theta} = \Delta\theta$ and $\bar{b} - \underline{b} = \Delta b$. (1) is a situation that is not far away from the standard scenario in Spence (1973). There is a separating equilibrium in which any high ability type will choose a level of the education $s = s^h$ while any low type chooses $s = 0$ and the employers beliefs are formed accordingly.

Additionally, all pooling and partially pooling equilibria can be eliminated by the intuitive criterion (Cho and Kreps (1987)). In a setting of more types one may need stronger criteria to eliminate these equilibria (Banks and Sobel (1987)) but ultimately only a single separating equilibrium remains (a version of the Riley equilibrium (Riley (1979))). Intuitively, this is a case where b is not able to introduce enough noise in the signal to make it impossible to distinguish high from low ability types. The high ability type will always find it worthwhile to invest just a little more in education than her low ability counterpart in order to unambiguously signal her high ability.

In (2) exactly this is not the case. Clearly, the type $(\bar{\theta}, \underline{b})$ is not in a position to invest more than the $(\underline{\theta}, \bar{b})$ type. In this case, there can be no fully separating equilibrium. However, any full pooling equilibrium can also be discarded: The type $(\bar{\theta}, \bar{b})$ can always deviate to a higher level of education than the others and clearly signal her high ability. In this case, an equilibrium exists where type $(\bar{\theta}, \underline{b})$ and type $(\underline{\theta}, \bar{b})$ are bunched together. In this equilibrium type $(\underline{\theta}, \underline{b})$ will not invest in education, $(\bar{\theta}, \underline{b})$ and type $(\underline{\theta}, \bar{b})$ will invest in an intermediate level s^m while $(\bar{\theta}, \bar{b})$ will again signal her unequivocal superiority with s^h . The corresponding equilibrium beliefs are:

$$\begin{cases} \mu(\theta = \bar{\theta} | s = s^h) &= 1 \\ \mu(\theta = \bar{\theta} | s = s^m) &= \frac{p_{hl}}{p_{hl} + p_{lh}} \\ \mu(\theta = \bar{\theta} | s = 0) &= 0 \end{cases}$$

Why is this type of pooling equilibrium not eliminated by the intuitive or divinity criterion? While the type of high ability but low parental background cannot profitably invest in a higher level of education than the type with low ability and high parental background, the latter has no way of gaining from a deviation. Since he is the one potentially benefiting from the largest set of responses to his deviation, it will always be him who is attached the largest probability of deviation relative to $(\bar{\theta}, \underline{b})$ or $(\bar{\theta}, \bar{b})$. This implies that firms would offer a wage too low for the deviation to be profitable for any type.

The existence of this particular equilibrium both motivates and serves as a starting point for my empirical analysis. Notice that in the above equilibrium:

$$E[\theta | s = s^m] = \frac{p_{hl}\bar{\theta} + p_{lh}\underline{\theta}}{p_{hl} + p_{lh}}$$

but that

$$E[\theta | s = s^m, b] = \begin{cases} \bar{\theta} & \text{if } b = \underline{b} \\ \underline{\theta} & \text{if } b = \bar{b} \end{cases}$$

For an individual sending signal s^m I can write:

$$\theta_i = E[\theta_i | s_i = s^m] + u_i \quad (2.3)$$

where $u_i = \theta_i - E[\theta_i | s_i = s^m]$ - the component of θ that cannot be predicted by s - is decreasing in b_i .

Above, I have stated that an employer pays a newly hired young worker exactly according to her expected productivity so $w_{i0} = E[\theta_i | s_i]$ where w_{i0} denotes the wage of individual i at time 0 of her career.

In the context of employer learning (as in Farber and Gibbons (1996) and Altonji and Pierret (2001)) each period of the working relationship the employer observes additional information $\xi_{it} = \theta_i + \epsilon_{it}$ about the true productivity of worker i (I will suppress the subscript i from this point onwards) which is equivalent to the information $d_t = \xi_t - E[\theta | s] = u + \epsilon_t$ where ϵ_t is a mean-zero error term independent of s or θ .

Wages will further develop in the following way:

$$w_t = E[\theta | s, D_t] = E[\theta | s] + E[u | s, D_t]$$

where D_t is the set of all additional information collected up to point t : $D_t = \{d_1, d_2, \dots, d_t\}$ where $D_0 = \emptyset$.

Note that $E[u | s, D_0] = 0$ while $\lim_{t \rightarrow \infty} E[u | s, D_t] = u$.

In the beginning of this section I have described a situation in which children of parents with a low educational attainment face an additional cost to invest in an educational signal and an equilibrium arises that bunches several types of different ability levels and different parental background together. If this is true, then u is decreasing in b and since $E[\theta | s]$ is constant over time we should expect that wages are decreasing in b with experience as the employer learns about the true ability of the worker.

This analysis has shown that the relation of u and b will be key to determine whether or not the hypothesis discussed in the introduction is true. In the next section I will discuss how I tackle this problem empirically.

2.4 Econometric Setup and Results

The equations above already offer a good starting point for an empirical analysis. However, to gain a more realistic view on the wage determination process I will slightly enrich this specification following Farber and Gibbons (1996) and Altonji and Pierret (2001). I have ignored two important issues so far: The first is human capital acquisition. In the empirical specification it will be useful to allow productivity to grow with experience. To do this a deterministic function of human capital growth is added to the model $H(t)$ with $H(0) = 0$. Practically this will amount to the introduction of a time trend (of possibly higher order) into the regression.

The second issue I have not addressed is the fact that naturally employers do not hire and form expectations about the productivity of their workers purely based on their educational attainment. They may use other public information available such as a worker's gender or age. On the other hand they may decide on entry wage based on information available to them through the hiring process that I cannot identify or observe. This information can, of course be correlated with parental background. In fact, while my analysis builds on the assumption that employers cannot ex ante observe family background perfectly, it is likely that they observe factors that are correlated to it - like an accent or a person's name. The fact that this information is correlated with parental background and omitted in the data may pose a number of problems to my analysis that I will discuss later on. For now let me re-specify the simple model above in the following way:

Let s be a vector of variables both observed by the employer at the point of hiring and observable in the data - like age. Let q be a vector of variables observed by the employer but not contained in the data - like a candidate's communication skills. Let b be a worker's parental background contained in the data but not (fully) observed by the employer and let θ be a worker's innate ability unobserved both by the employer at the point of hiring and by the data. The employer sets the wage w_t according to his expectations about the worker's productivity y_t :

$$w_t = E[y_t | s, q] = \alpha_1 s + \alpha_2 q + \alpha_3 E[\theta | s, q, D_t] + H(t) + \zeta_t$$

Both s and q can directly increase the worker's marginal product y_t but also serve as a signal to form a belief about θ . ζ_t is a mean-zero error term orthogonal to s , q and b .

Estimating the wage equation with the observable data using OLS will be a linear projection E^* of w_t on s and b .

$$w_t = \alpha_1 s + \alpha_2 E^*[q|s, b] + \alpha_3 E^*[E[\theta|s, q, D_t]|s, b] + H^*(t) \quad (2.4)$$

$$= \alpha_1 s + \alpha_2 E^*[q|s, b] + \alpha_3 E^*[E[\theta|s, q]|s, b] + \alpha_3 E^*[E[u|D_t]|s, b] + H^*(t) \quad (2.5)$$

Let the linear approximation of $E[\theta|s, q]$ be

$$E^*[\theta|s, q] = \gamma_1 s + \gamma_2 q + u \quad (2.6)$$

where u is orthogonal to s and q by construction.

Furthermore, because I cannot directly observe q , I have to define what a regression omitting q picks up by defining the linear projection:

$$q = \beta_1 s + \beta_2 b + z \quad (2.7)$$

So what is estimated in a linear projection of w_t on s and b ? From the equations (6), (7) with (5) above we get:

$$w_t = \psi_1 s + \psi_2 b = (\alpha_1 + \alpha_2 \beta_1 + \alpha_3(\gamma_1 + \gamma_2 \beta_1 + \Phi_{st}))s + (\alpha_2 \beta_2 + \alpha_3(\gamma_2 \beta_2 + \Phi_{bt}))b$$

where Φ_{st} and Φ_{bt} are the coefficients of a linear regression of $E[u|D_t]$ on s and b .

If $\text{cov}(s, E[u|D_t]) = 0$ then Φ_{st} and Φ_{bt} can be written as

$$\Phi_{bt} = \nu_t \Phi_b$$

$$\Phi_{st} = \nu_t \Phi_s$$

where $\nu_t = \frac{\text{cov}(b, E[u|D_t])}{\text{cov}(b, u)} \in [0, 1]$ and Φ_b and Φ_s are the coefficients of a regression of u on b and s (see Altonji and Pierret (2001)). This is important, because if ν_t is non-negative it

allows to conclude that the sign of the estimated coefficient Φ_{bt} corresponds to the sign of Φ_b which is the relationship between u and b given s .

2.4.1 Identification

At this point, it is important to note, that u is not the "residual" of $E[\theta|s]$ but of $E[\theta|s, q]$. Unless q is independent of s and b I cannot determine whether it is education that is the signal that is more costly to people from low parental background or whether it is any other signal from q . This is an issue I will not solve in this paper. In addition however one should expect that q is used to some degree to predict b at the beginning of a worker's career. As time goes by and the employer learns about true ability this factor will become less important and ability will get more weight in the wage determination process. If we could control for q as we do for s this process would not matter for our coefficient Φ_{bt} . Given that we cannot do that, b - that is correlated with q - will necessarily pick up part of this "losing importance"¹. To see this in detail, please refer to the technical appendix.

The first strategy to improve identification in this case is to control for individual characteristics that could have an influence on wage growth, like gender or migration background etc. I will present the results of an OLS regression of wage on schooling, parental background and a time trend with and without controls for some of these background characteristics in Table 2.3.

Another solution to these identification problems is to rid b of the part that is "contaminated" by q as in Farber and Gibbons (1996). For this purpose I regress b on education and the set of observables used as controls above as well as the initial wage w_0 . The idea is that w_0 contains the relevant information on q so that the residual b^* is orthogonal to q and s . In the next step, I substitute b with b^* and perform the same estimation as described above. b^* is an estimator of e in the above equations so that $\Phi_{b^*} = \rho_3$. Note however that this procedure may not be fully effective in the likely case that wages are observed with measurement error. Then this "cleansing" procedure may be incomplete.

¹This does not mean that q if included in the regression would have a negative coefficient, rather its sign would depend on $cov(b, q)$ and $cov(b, u)$

The results of these regressions are presented in Table 2.4. To somewhat account for any type of sorting into firms all specifications control for the type of employment (blue collar, white collar), the industry and the size of the company. Individual level controls include the age at the first job, sex, migration background and cohort of the individual as well as their interactions with the time trend. Year fixed effects are also included. Taking into account that the speed of learning as estimated by Lange (2007) is relatively fast I estimate the model for 3 different time horizons. The shortest time horizon observes wages only up to the third year after the entry in the labour force. I also do the analysis for years 0-5 in the labour market and 0-10 years. Because the effect of human capital accumulation as well as employer learning is likely to decrease with experience the time trend included is quadratic for all specifications except the one with the shortest time horizon. This model contains only a linear time trend.

2.4.2 Data and Sample Selection

To perform the estimates described above I use survey data of young adults of the German Socio-Economic Panel². This survey includes yearly information about current occupational status, wages, industry as well as personal characteristics such as education and importantly, detailed account of parental education. The data is available from 1984-2013 and I use all waves. The variable I use to determine parental background b is an indicator on whether or not the father has obtained post-secondary education. For the individual young workers themselves, I use a much more detailed measure of education, that combines both scholastic and vocational training the individual may have gotten. As the relevant measure of working experience I use potential experience, that is the years passed since the entry in the labour force. I consider an entry in the labour force the first time they are observed with a wage out of full-time work. Hence, prior part-time work experience is not accounted for.

I keep individuals who are observed for a minimum of 5 years after they start working and drop all those from the sample that are self-employed or work as a public servant.

²Socio-Economic Panel (SOEP), data for years 1984-2013, version 30, SOEP, 2014, doi:10.5684/soep.v30.

I restrict attention to those who make a permanent transition from non-employment to employment and thus worked full-time for at least $\frac{2}{3}$ of the time observed and consider only positive earnings. My objective is to understand how and to what extent employers adjust their wages to what they learn about individual workers. If an individual is currently unemployed, I do not have information on that individual's potential earnings. This is thus a restriction to obtain meaningful results on the degree to which employers adapt their wages to what they learn about a worker's ability.³ I also use only individuals who do not change their educational attainment during the period of observation. Given these selection criteria, the sample size is 810 individuals.

Tables 2.1 and 2.2 present some summary statistics of the sample. The sample is slightly more male than female. The most common forms of education are lower or intermediate secondary school with an apprenticeship or University. Blue and white collar jobs at first entry into the labour market are pretty evenly distributed. From Table 2.2 it is clear that children of college educated fathers enter the labour market older and at a higher wage and tend to remain there also after 5 years. This is however not accounting for their education as I will in the following exercise.

2.4.3 Estimation Results

Table 2.3 shows the estimation results with "contaminated" b . The coefficient Φ_{bt} is negative throughout but ceases to be significant at the 10 year horizon model. This is coherent with the estimated speed of learning in Lange (2007). After 5 years in the labour market, the learning effect will be very small which means that the return to ability and hence b decreases. This is also reflected in the positive coefficient on experience squared interacted with b . It means that until a five year horizon individuals with the same training but worse parental background have a steeper earnings profile, but decreasingly so. Adding the controls has no large effects on the point estimates which is reassuring considering the discussion about the set of variables q . Looking at Table 2.4 one can see, however, that the coefficients become slightly less sizable for the "clean" b^* . Yet, they

³That being said, the results are qualitatively robust to the inclusion of periods of unemployment. However, due to the increased variation, the estimates are less precise.

are still negative until the 5 year horizon model and remain for the most part statistically significant. Note that the results for b^* without controls imply that also b^* was generated from a regression of b on education and the initial wage without any of the controls.

Overall the short term results tend to be in support of the idea that, given observable characteristics at the entry in the labour market, wages of workers from less educated families grow faster than those of workers from more educated families. The time horizon that seems to be decisive for this process is consistent with a framework of educational signalling, employer learning and statistical discrimination. It is to be noted, however that the results are also consistent with other theories.

Another one of the most widespread interpretations of wage growth over time is the accumulation of human capital (Becker (1962), Mincer et al. (1974)). In this analysis I have allowed for a deterministic time trend in the wages and hence for experience as a relevant factor in wage determination. As I have already discussed, speed of human capital accumulation could in principle depend on q , but also on s or b or the unobservable θ . In the empirical part of this paper, I have accounted for this fact by adding interactions of all observables with the time trends. I have also used b^* in my analysis that should be orthogonal to any of the factors in s or q . This can take into account the fact that q or s - that are correlated with b could be correlated with the speed of learning as well. Therefore, my results should not be driven by human capital accumulation that depends on q or s . If b were directly related to the return to experience then we would expect this effect to be positive. One can think for instance of parents who are pushing their children's careers by intervening in their favour. This is not consistent with my results. What would be consistent with my results is some form of "catching up" by children from less educated backgrounds in terms of non-formal or soft skills not observed in the data. Given my methodology though, it would have to be the case that the initial lack of these "soft skills" is not observed by the employer at the point of hiring or not reflected in the initial wage.

2.5 Conclusion

In this paper, I have presented some support for the hypothesis that workers with identical observable characteristics at the entry in the labour market have different wage dynamics depending on their parental background. In particular, I find that wages of young adults whose parents have a low educational attainment grow faster with respect to the wages of their colleagues from more educated families in their early career. I interpret this finding in an educational signalling - employer learning context and argue that children from less educated families tend to achieve a lower level of education than others even when their abilities are the same. This is because they face an additional cost to invest in education that is independent from their true productivity. The precise nature of this cost as well as the signal it affects should be the subject of further research on this topic.

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Appendix: Tables

Table 2.1: Summary statistics 1

	Percent	Observations
Gender		
Male	58,40	473
Female	41,60	337
Total	100,00	810
Highest level of education or training		
No degree/drop out	1,23	10
Lower secondary	6,54	53
Intermediate secondary	5,31	43
Entrance exam tertiary	1,48	12
Upper secondary	1,85	15
Other schooling degree	2,10	17
Apprenticeship with secondary	15,43	125
Apprenticeship with intermed. secondary	23,21	188
Apprenticeship with upper secondary	6,30	51
Apprenticeship with other degree	6,17	50
Health care school	6,67	54
Special technical school	1,23	10
Other vocational degree	1,23	10
Uni applied sciences	5,80	47
University	15,43	125
Total	100,00	810
Father college		
No	84,69	686
Yes	15,31	124
Total	100,00	810
First job blue collar		
No	58,15	471
Yes	41,85	339
Total	100,00	810

Table 2.2: Summary statistics 2

	Min	Max	Median
<u>Entry wage monthly</u>			
Father college:			
No	164.00	5,271.00	1,344.00
Yes	312.00	6,904.00	1,784.00
Total	164.00	6,904.00	1,400.00
<u>Monthly wage after 5 years</u>			
Father college:			
No	256.00	10,226.00	1,900.00
Yes	578.00	7,000.00	2,679.50
Total	256.00	10,226.00	2,000.00
<u>Age entry to labour market</u>			
Father college:			
No	17.00	34.00	22.00
Yes	19.00	33.00	25.00
Total	17.00	34.00	22.00

Table 2.3: Estimation results - simple b

	(1)	(2)	(3)	(4)	(5)	(6)
Father college	0.071*	0.039	0.074*	0.043	0.042	0.013
	[0.039]	[0.035]	[0.041]	[0.037]	[0.039]	[0.035]
Father college=1 \times Experience	-0.030**	-0.024*	-0.060**	-0.055**	-0.005	-0.001
	[0.014]	[0.013]	[0.024]	[0.022]	[0.014]	[0.014]
Father college=1 \times Experience squ	—	—	0.013***	0.013***	0.001	0.001
			[0.004]	[0.004]	[0.001]	[0.002]
Observations	3044	3044	4537	4537	6887	6887
Horizon years	0-3	0-3	0-5	0-5	0-10	0-10
Controls	No	Yes	No	Yes	No	Yes
Time trend	linear	linear	quadratic	quadratic	quadratic	quadratic

Notes: Clustered standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Controls include year fixed effects, education, blue collar/white collar, industry and size of company, age at the first job, sex, migration background, cohort as well as their interactions with the time trend.

Table 2.4: Estimation results - "cleaned" b^*

	(1)	(2)	(3)	(4)	(5)	(6)
b^*	0.023 [0.039]	0.038 [0.035]	0.024 [0.041]	0.042 [0.037]	-0.005 [0.039]	0.013 [0.035]
Experience $\times b^*$	-0.022 [0.014]	-0.024* [0.013]	-0.046* [0.024]	-0.055** [0.022]	0.004 [0.014]	-0.001 [0.014]
Experience squ $\times b^*$	—	—	0.011*** [0.004]	0.013*** [0.004]	0.000 [0.001]	0.001 [0.002]
Observations	3044	3044	4537	4537	6887	6887
Horizon years	0-3	0-3	0-5	0-5	0-10	0-10
Controls	No	Yes	No	Yes	No	Yes
Time trend	linear	linear	quadratic	quadratic	quadratic	quadratic

Notes: Clustered standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Controls include year fixed effects, education, blue collar/white collar, industry and size of company, age at the first job, sex, migration background, cohort as well as their interactions with the time trend. In this specification father's education b is "cleaned" of everything an employer may have observed at hiring by regressing it on the initial wage and taking only the residual b^* .

Technical Appendix

Following up on the discussion in section 2.4.1 let's say we observed θ as well as s , b and q and that a projection of b on s and q is $b = k_1s + k_2q + e$ and a linear projection of θ on the three other variables is:

$$\begin{aligned}
 \theta &= \rho_1s + \rho_2q + \rho_3b + v \\
 &= (\rho_1 + \rho_3k_1)s + (\rho_2 + \rho_3k_2)q + \rho_3e + v \\
 &= E^*[\theta|s, q] + \rho_3e + v \\
 &= E^*[\theta|s, q] + u
 \end{aligned}$$

So u is related to s and b in the following way:

$$\begin{aligned}
 u &= \rho_3e + v \\
 &= \rho_3(b - k_1s - k_2q) + v \\
 &= \rho_3(1 - k_2\beta_2)b + \rho_3(-k_1 - k_2\beta_1)s - \rho_3k_2z + v \\
 &= \Phi_b b + \Phi_s s + \tilde{v}
 \end{aligned}$$

The parameter of interest for the present analysis is really ρ_3 . It describes how b relates to innate ability that cannot be predicted by the observable signals in s and q . In a case in which young adults with low b have a higher cost of sending these positive signals, ρ_3 will be negative. Note that if I could observe q alongside of s and b then the coefficients of a regression of u on b , s and q would be ρ_3 , $-\rho_3k_1$ and $-\rho_3k_2$ respectively. The coefficients of s and q reflect exactly the fact that the two variables are correlated with b and are hence used by the employer to predict b . Given that we do not observe q , b will pick up part of the q that loses relevance in the regression. This is reflected by $-k_2\beta_2 < 0$.

Note that this bias is generated by the relationship between b and q despite the fact that I am still assuming that $H(t)$ is unrelated to q or b . The omitted variable q constitutes an even more serious problem in my analysis if this assumption does not hold. If q has an influence on the speed of human capital acquisition $H(t)$ and b is related to q , then

the coefficient Φ_{bt} will pick up this effect. It is difficult to sign this bias since the nature q or its relation to b or $H(t)$ is not known.

Intuitively it is hard to imagine that b generates an advantage in terms of human capital accumulation for people with a less educated parental background through this channel. I would tentatively argue that capital accumulation that depends on the part of q that is correlated with b is likely to bias my results upwards. For instance if q contains an accent associated to disadvantages areas, this is not likely to contribute positively to wage growth. However, I cannot exclude a bias in the opposite direction.

Chapter 3

National Wage Equalization and Regional Misallocation: Evidence from Italian and German Provinces

with Tito Boeri, Andrea Ichino, Enrico Moretti.

3.1 Introduction

Wage and earnings inequality are large and rising in many countries. Different countries have adopted different labor market institutions to try to mitigate labor market inequality, including minimum wages, transfers targeting low wage workers like the Earned Income Tax Credit and unions contracts.

In most European countries, collective bargaining agreements are common practice and cover the majority of workers. Typically, firms and unions belonging to a specific sector bargain over an occupation-specific wage schedule. This wage schedule applies to all workers in that sector, irrespective of their location. The objective is to equalize salaries across employers and thus reduce inequality. For example, car manufacturers

bargain with their unions over wages that apply to all car manufacturing establishments in the country; banks bargain with banking sector unions over wages for the entire banking sector, and so on. Collective bargaining are prevalent in Austria, Belgium, Denmark, Finland, France Germany, Greece, Ireland, Iceland, Italy, Netherlands, Norway, Portugal, Slovenia, Spain and Sweden, although countries differ on how binding the national agreements are (European Central Bank, 2008).

In this paper, we investigate an important but under-researched feature of wage equalization systems. We argue that while national wage equalization may be successful at compressing wage inequality in a country, it can have significant unintended consequences, as it can create costly imbalances between cities and regions as a result of geographical productivity differences. Specifically, in the presence of geographic differences in productivity, wage equalization across localities can generate significant misallocations. Firms in areas where productivity is low need to pay wages above the local market clearing level, which may lead to lower employment, investment and output in those areas. From a macroeconomic point of view, this may result in lower GDP, employment, and aggregate earnings.

We study the local and aggregate effects of national wage bargaining systems by comparing Italy and Germany. Italy and Germany represent two useful case studies. Both make extensive use of collective bargaining agreements, but the level of resulting wage flexibility is markedly different. Italian nationwide sectoral contracts are more binding and allow for only limited local wage adjustments. This means that within each sector, firms in high productivity and low productivity areas face largely the same wage schedule.¹ Germany previously had a similar nationwide wage setting system, but after reunification their system was made more flexible. Due to concerns about significantly lower productivity among Eastern firms, since 1996 Germany has implemented so-called “opening clauses” that allow firms to negotiate *locally* with unions, outside the nationwide agreements (Schnabel, 1998).

¹While firms can increase wages above the national contract schedule, they cannot lower them in most cases. National wage floors are binding enough that the amount of wage variation that comes from establishment-level bargaining is empirically small relative to the amount of variation stemming from national contracts. We provide a detailed discussion of these institutions and the exceptions below.

Our empirical analysis comprises two parts. In the first part, we study the relationship between local firm productivity and local nominal wages, non-employment rates, cost of living, and real wages, defined as nominal wages deflated by the local cost of living. Our geographic unit of analysis is an Italian “Province” (103 in total) or German “Spatial Planning Region” or “Raumordnungsregion” (96 in total). In the second part, we quantify the aggregate costs of spatial misallocation in Italy (compared to Germany) in terms of forgone employment, aggregate output and per-capita earnings.

Empirically, Italy and Germany have a similar cross-province standard deviation in mean firm productivity, as measured by firm value-added.² In Italy, firm value added is significantly higher in the North than in the South: In 2014, the gross value-added per worker in a average firm of Milano, for example, was 71% above the value-added in a average firm of Cosenza, in the southern region of Calabria. Similarly, in Germany productivity is significantly higher in the West than in the East: the value-added per worker in a average firm in Munich is 83% above the value-added in a average firm of North Thuringen in East Germany.³ In Italy, the North-South productivity gap reflects long-lasting historical differences in transportation infrastructure, distance from European markets, efficiency of local governments and local policies, criminal activity, and cultural norms, while in Germany, the East-West gap likely reflects half a century of Communist rule in the East as well as other historical factors.⁴

While Italy and Germany have similar geographic distribution of firm productivity, they have important differences in the geographic distribution of nominal wages, likely reflecting wage bargaining differences in the two countries. In Italy, there is a much

²Gross value added is output valued at basic prices less intermediate consumption valued at purchasers prices. The basic price is the amount receivable by the producer from the purchaser for a unit of a product or service minus any tax on the product plus any subsidy on the product.

³Similar geographic differences exist in most countries in Europe and outside Europe. In the US, total factor productivity of firms in cities at the top of the TFP distribution is more than double that of cities at the bottom of the distribution (Hornbeck and Moretti, 2018). Data for gross value added in Italy and Germany from the OECD (2018a).

⁴As we will show, the North-South productivity gap in Italy is remarkably similar to the West-East productivity gap in Germany. In 2014, the difference in mean value-added between the Northern Italian and Southern Italian firms was 19.0%. The corresponding difference between West and East German firms was almost identical: 19.9%. In this paper, we will take these differences as given. Our analysis will focus on the effects of these differences, rather than their causes. The literature on regional productivity differences is immense. Examples for Italy include Banfield, 1958, Putnam et al., 1993, Ichino and Maggi, 2000, Buonanno et al., 2015, and Bigoni et al., 2016). An example for Germany is Burda and Hunt, 2001).

stronger degree of wage equalization across provinces than in Germany. For example, after controlling for worker characteristics the 90-10 percentile difference in mean wages across provinces is 42.9% in Germany, more than four times larger than the 10.3% difference in Italy. The mean wage difference between the North and the South in Italy is 4.2%, while the mean West-East difference in Germany is seven times larger: 28.2%, despite similar productivity differences.

Crucially, we find a marked difference in the relationship between local productivity and local nominal wages in the two countries. If wages can fully adjust, our model indicates that we should see a tight relationship between the two, with areas that enjoy higher firm productivity also having proportionally higher mean nominal wages. This is indeed the case in the US (Hornbeck and Moretti, 2018). By contrast, if wages are prevented from fully adjusting, we should see a weaker relationship. In the extreme case of fully-binding national contracts with complete nominal wage equalization, we should see no relationship at all. When we regress log mean nominal wage (adjusted for workers characteristics) on log value-added across provinces, we find an elasticity of wages with respect to value-added of 0.19 in Italy and 0.73—almost four times larger—in Germany. Thus, German firms appear to be significantly more able to adjust nominal wages to local productivity than Italian firms.

A simple spatial equilibrium model has several predictions for Italy and Germany, which appear consistent with our data. First, our model predicts that in Italy, where wages cannot fully adjust, provinces with low productivity should have higher non-employment rates. This should be less true in Germany, where wages can adjust more to local productivity. Indeed, when we regress local non-employment rate on local value-added we find that the elasticity of non employment rates with respect to value-added is negative in both countries—indicating that provinces with lower value-added have higher non-employment rates—but the elasticity in Italy is -1.41 (0.03), almost six times larger (in absolute value) than Germany’s -0.25 (0.02) elasticity. Our findings are not driven by the existence of an informal sector in Italy.

Second, the model indicates that since workers can move across regions, low produc-

tivity provinces should have lower housing prices, both in Italy and Germany. Empirically this is the case: we find a positive relationship between housing prices and local productivity.

Third, there are striking implications for real wages. In Italy we find a *negative* relationship between real wages and local value-added. Despite having higher productivity, provinces in the North have *lower* real wages than provinces in the South, since the South has low housing costs but similar nominal wages. This means that employed Italians are better off working in the South in terms of purchasing power. However, the probability of having a job is higher in the North. One way to think about geographic differences in Italy is that national wage contracts have created a spatial equilibrium where workers queue for jobs in the South. If they find a job, they are better off than their colleagues in the North in terms of real wages, but while queued they remain not-employed. By contrast, in Germany, we do not see that real wages in the West are lower than the East, since nominal wages are spatially more flexible.

Overall, the current wage-setting system in Italy appears inefficient: firms in provinces where productivity is low face wages well above the local market-clearing level. At the local level, this implies that output and employment in those provinces are below what they could be if wages were flexible. At the national level, this implies losses in GDP and employment.

In the second part of our empirical analysis, we estimate the aggregate costs stemming from spatial misallocation in Italy in terms of forgone aggregate earnings and employment. What would happen if nominal wages were allowed to reflect local productivity? This could happen if Italy adopted a system similar to contemporary Germany's, in which union contracts can be negotiated at the firm or province level instead of the national level.

Our model indicates that while wages would decline in low productivity provinces, output and employment in low productivity provinces would increase, resulting in an overall increase in aggregate output and aggregate employment in the country. Aggregate labor earnings would also increase, but only if the elasticity of labor demand is larger

than one. Intuitively, an elastic labor demand means that the increase in employment in low productivity areas more than offsets the decline in wages.⁵

To empirically quantify the magnitude of these effects, we provide estimates from a counterfactual scenario in which we consider what would happen if the Italian relationships between wages and value added and between non-employment and value added were the same as those observed in Germany. To be clear, we do not assume that wages or employment or value added are the same in the two countries; rather, we apply to Italy the elasticity of wages with respect to value added and the elasticity of non-employment with respect to value added that we have estimated for Germany. For each Italian province, we use those elasticities to quantify counterfactual wages and employment, whatever the value added level of that province.

We find that average wages in Southern provinces would decrease by 5.90%, while Southern employment would increase by 24.6%, or about 2.5 million jobs. On net, aggregate earnings in the South would increase by 16.59%. Nationwide, we estimate that aggregate employment would increase by 11.04% and aggregate earnings would increase by 7.45%. This amounts to around 600 euros per year for each working-age adult.

We conclude that in the aggregate, allowing union contracts some degree of local flexibility would improve the efficiency of labor allocation in Italy, resulting in increased employment, aggregate income, and per capita labor income. There would also be distributional consequences, as currently-employed workers in the South would face lower nominal and real wages.

Our findings are relevant to countries other than Italy and Germany, as the Italian and German system are by no means unique. Broadly speaking, France, Belgium, Portugal, Finland, Iceland, and Slovenia have a system similar to the Italian model, while Austria, Denmark, the Netherlands, Norway and Sweden are closer to the German model (OECD, 2017 and 2018). Countries like Greece, Portugal and Spain have recently moving from a bargaining system similar to Italy's to a "controlled decentralization" which is not unlike the German system. France has long debated the desirability of a more

⁵Labor demand—at least in the traded sector—is likely to be elastic in the case of an open economy like Italy, which is fully integrated in European product markets.

decentralized bargaining system, and such reform was initially part of the labor market reforms proposed by President Macron in 2017, though it was subsequently dropped due to strong union opposition. While the level of macroeconomic benefit from such reforms is likely to vary from country to country depending on the extent of productivity differences across regions, it is safe to conclude that countries with binding national contracts would improve efficiency if they moved toward the German wage-setting model.

Our paper is part of a growing body of work that focuses on the causes and consequences of misallocation.⁶ The US represents an interesting specular example of spatial misallocation. In the US case, little prevents nominal wages from adjusting.⁷ However local employment is de facto constrained in many highly productivity cities, resulting in large spatial misallocation. Hsieh and Moretti (forthcoming) have found large efficiency losses in the form of forgone output and earnings caused by land use regulations that limit housing supply in the most productive cities, thereby constraining the flow of labor toward high-TFP locations. By contrast, in the Italian case nothing constrains local employment or mobility, but local wages cannot adjust to local labor demand conditions.⁸

Our paper is also part of the literature on centralized wage bargaining. While much research has been devoted to the effects of centralized wage bargaining in Italy, Germany and other European countries⁹, the effects on the geography of employment and wages and their aggregate costs have not previously been studied.

The paper is organized as follows. In Section 3.2 we describe the institutional setting and wage determination mechanisms in Italy and Germany. Section 3.3 describes our theoretical model and its predictions. Section 3.4 describes the data. Empirical evidence is presented and discussed in Section 3.5. The aggregate costs of spatial wage rigidity are analyzed in Section 3.6. Section 3.7 concludes.

⁶Restuccia and Rogerson (2017) provides a recent survey.

⁷While the US has a minimum wage, its effects are likely to be different. For one, the federal minimum wage is not as binding as national contracts in Italy. Second, is significant geographic variation in minimum wages, with state- and city-level minimum wages significantly more binding than the federal minimum wage. Third, minimum wages only apply to low-wage workers, while European national contracts define wage floors for all levels of employment, excluding top management.

⁸Other authors have used similar models to measure the effect of state taxes (Fajgelbaum et al., 2015), internal trade frictions (Redding, 2013), infrastructure (Ahlfeldt et al., 2015), and land misallocation (Duranton and Puga, 2014).

⁹For example, Calmfors and Horn (1986), Boeri et al. (2001), and Iversen (1996).

3.2 Wage Setting Mechanisms in Italy, Germany and Other European Countries

We begin by briefly describing the main features of the wage bargaining systems in Italy and Germany. We then discuss which among European countries have wage bargaining systems close to the Italian or German model. We stress that the specifics of a given country's labor market institutions are quite complex. We do not seek to provide a comprehensive description of all the features of the wage-setting systems in each European country, but instead seek to distill the key differences relevant in our analysis, abstracting from many less crucial details.

Italy. Wage bargaining institutions in Italy have been historically designed to achieve strong nominal wage compression.¹⁰ Today, national agreements between unions and employers set wages for each industry and occupational level. Industries are defined narrowly: For instance, there are currently 34 contracts in the chemical industry, 31 in textiles, and 39 in food production. Overall, there are 346 national agreements, and they cover 97.7 per cent of dependent employment in the social security system and 99.3 per cent of firms.¹¹

With limited exceptions, Italian firms cannot pay a salary below the level established at the national level, irrespective of their specific profitability and product demand conditions. Thus, despite large geographic differences in productivity, transportation infrastructure, geographic location, local public goods, and local government effectiveness across different areas of the country, firms in a given industry face the same wage floors.¹²

¹⁰Until 1992, it was mainly the centralized indexation of wages to inflation (*Scala Mobile*) that reduced nominal wage dispersion across sectors, regions and skill levels. The indexation imposed the same absolute (as opposed to proportional) salary increase to all employees, independent of their salary. As a result, wage increases in percent terms were large at the bottom and small at the top of the distribution, resulting in strong compression over time as described by Erickson and Ichino (1994), Manacorda (2004) and Garnero (2018)). This mechanism was abolished in 1992, and in 1993 the Italian government, the national trade unions, and the employers' associations signed a new income policy agreement which is still in effect today.

¹¹By definition, national agreements do not include the informal sector.

¹²In some exceptional circumstances of firms facing particularly severe difficulties, wages lower than those established at the national level may be allowed. These cases are limited by "opening", "hardship" or "inability to pay" clauses to exceptional circumstances such as severe macroeconomic or idiosyncratic shocks that make downsizing unavoidable. These provisions are rarely invoked before a firm is in severe

In theory, the system does allow for some wage bargaining at a decentralized level, either at the firm level or within local industry clusters (*“distretti industriali”*). In practice, decentralized bargaining is limited because it is only allowed to increase wages above the levels set by the national agreements.¹³

In most cases, wages in national contracts are set close to the market clearing levels in Northern regions. One reason is that Southern employers are not well represented in employers associations. Northern regions have a much larger number of firms, especially in manufacturing, and dominate the process. Confindustria, the main employer association in manufacturing, collects almost 80 per cent of its revenues in the North and is typically led by a Northern president.¹⁴ By contrast, Southern employers and workers do not often have the critical mass to have a strong voice in multi-employer bargaining. Empirically, most Northern provinces are generally close to full employment in a typical non-recession year, while unemployment is invariably much higher in the South.

In practice, private sector Italian firms do retain a limited degree of wage flexibility. National contracts allow limited geographic differentiation and some use of merit pay. In recent years, the use of “temporary contracts” has allowed to pay a limited number of employees per firm wages below national contracts. Firms can also pay employees under the table. Thus, while one should expect wage compression, one should not expect nominal wages to be uniformly identical in the private sector. Wages in the public sector (13.6% of employment in 2015), on the other hand, *are* nationally uniform; wages of teachers, doctors, nurses, social security workers, police, and military personnel are the same in every province for a given job description and level of seniority.¹⁵

distress.

¹³Decentralized bargaining is limited to a small number of large firms, since the wage floors imposed by the national contracts are typically high for small and medium size firms. In a 1995-96 survey of a representative sample of 8,000 firms with at least 10 employees in both the manufacturing and service sectors, only 10 per cent of the firms reported engaging in firm-level bargaining (ISTAT, 2000). Since then this share has declined (Casadio, 2003, 2008 and Brandolini et al., 2007).

¹⁴In terms of timing, employers organizations in the four strongest regions (Lombardia, Piemonte, Veneto, and Emilia, which are all in the North) typically sign the leading contracts in metalworking, textiles, chemicals and other manufacturing sectors.

¹⁵As a reaction to the strong nominal wage compression imposed by national agreements, the past few years witnessed an increasing number of so-called “pirate contracts” engineered by a small group of employers and a labor consultant, involving a “fake union” created ad-hoc with the purpose of signing the contract. As long as the contracts are registered at the National Council for Economy and Labor, they allow subscribing employers to pay lower wage floors than those set by traditional unions and employers’

Table 3.1 presents an actual example of an Italian wage agreement. The agreement covers the 2016 construction sector (*"Contratto Collettivo Nazionale per i Lavoratori Edili"*). Entries are based on official data released by the Italian Ministry of Labor and Social Affairs and show the degree of permitted labor cost differentiation across provinces for each specific component of labor costs. The table shows that the main components of labor costs—for example, the floor and the indexation to inflation—have no cross province variation, while other components have some cross province variation. The bottom row shows that, overall, the standard deviation of total labor costs across provinces allowed by the agreement is only Eur 0.62.

Table 3.2 shows examples of geographic wage variations for two specific private sector employers and one public sector employer. The previous table referred to a wage agreement while this table reports wages actually observed in the labor market, but the picture is similar. The first row shows the median monthly salary at a large national bank.¹⁶ We report the median monthly salary of male bank tellers with 10 to 20 years of seniority and find limited geographic variability across North, Center and South Italy. For example, in the Northern city of Milan mean earnings are 1,659 euros per month, while in the Southern cities of Naples, Palermo and Bari they are 1,649, 1,677, and 1,670 euros, respectively. In the second row we show corresponding figures for a large national energy distribution company, inclusive of bonuses and merit pay. In both cases, we uncover limited geographic differences. If anything, wages in the energy company is slightly higher in the South, although for confidentiality reasons we cannot report wages for specific occupations. In the last row we show the salary for an elementary school teacher with 5 years of seniority. As in the rest of the public sector, there is no variation in the nominal wage across areas.

These are motivating examples based on three specific cases. In Section 3.5 we will present more systematic evidence on geographic wage heterogeneity for a representative sample of Italian workers based on labor survey data.

associations. According to CNEL (2018), about one third of the contracts in its database are subscribed by unions that are not represented at the Council and 16 belong to unions that have no role in workers' representation.

¹⁶For confidentiality, we cannot reveal the name.

Germany. Germany offers an interesting counterfactual. Before the Unification of East and West Germany, the country had a wage-setting system not unlike Italy's today. The system was changed after Unification due to the large differences in productivity levels between the combined regions and the many firms' threats to walk out of employers' associations. In particular, "opening clauses" were enhanced to allow firm-level bargaining for wages lower than those established at the national level (see Dustmann et al. (2014) for a discussion of German labor market institutions and their reforms). Opening clauses enable company management and works council to conclude works agreements which deviate from the industry-level collective agreement within certain limits. In the chemical industry, for instance, an opening clause allows companies to reduce the collectively agreed wage by up to 10 per cent for a limited period of time in order to save jobs or improve competitiveness.¹⁷

More precisely, until the mid-1990s the German system was based on a two-tier bargaining structure. First, industry-wide collective bargaining negotiations between unions and employer associations took place at the regional ("*Länder*") level. One of the regions was typically pivotal for negotiations in other regions. Then plant-level bargaining took place, but wages could only increase relative to those established at the regional level. Since collective agreements were often settled uniformly for the whole industry (with only minor regional differences), wage differentiation between regions, sectors, and plants was only achievable if plants paid premiums above the contract wage (Schnabel, 1998).

After German unification, the scope for plant-level bargaining was significantly enhanced by allowing for opting-out and derogation clauses from the wage floors established by higher levels of bargaining. This development was caused by the concern that employers in the new Eastern regions might leave their respective associations in order to separately negotiate their labor contracts. The threat of large-scale association downsizing if less-productive firms in the East had to adopt Western wage floors was a factor in generating union support for "opening clauses". Another threat was the possible reloca-

¹⁷Trade unions and employer associations retain the right to veto such deviating works agreements. Sectoral agreements also impose a number of conditions for derogations to apply: companies have to disclose their financial information allowing workers representatives to have enough time to scrutinize the financial status of the firm, and the derogation must be temporary.

tion of firms to neighboring Eastern European countries.

The decline in the importance of collective bargaining and the rise of opening clauses is quantified in Table 3.3. The ultimate effect of this reform was a significant increase in the decentralization of wage-setting in Germany. The decline in the importance of industry-level contracts in Germany after 1995 and the corresponding increase in the importance of firm-level wage-setting mechanisms have allowed a growing number of German firms to set wages in line with their productivity.

Other countries. For our purposes, the key difference between the Italian and German systems is that the latter permits a much wider scope for decentralized bargaining than the former, allowing wages to vary more as a function of local productivity. An analysis of the Italian and German bargaining systems has implications not just for Italy and Germany, but also for other countries. While the specifics of each country labor market institutions are different, key aspects of the Italian and the German bargaining systems are present in many other European countries.

Within the OECD, countries with systems closer to Germany’s tend to leave considerable room for firm-level bargaining and/or permit deviations or opt-outs from sectoral agreements under a broad set of circumstances. OECD (2018b) calls this system “organized decentralization”. According to the OECD, the group of countries that had a system of organized decentralization in 2015 includes Austria, Denmark, the Netherlands, Norway, and Sweden along with Germany (OECD (2018b)).

By contrast, countries with systems closer to Italy’s are countries where national industry-level agreements play a dominant role and deviations are either not possible or only allowed for wage *increases* relative to sectoral agreements.¹⁸ According to the OECD, this group includes France, Iceland, Portugal, and Slovenia. (OECD (2018b)).

In the wake of the Euro-area crisis, three countries—Spain, Portugal, and (to some extent) Greece—recently transitioned from a highly-centralized system towards a more decentralized, German-style model. A comparison of the Italian and German system can

¹⁸Sometimes this principle extends beyond pay and includes employment features like hours and annual leave, which firms can only improve upon from the perspective of employees.

be informative on the possible effects of these reforms.

More generally, many European countries have a two-tier bargaining structures in which sector-level bargaining can, in principle, be accompanied by plant-level or local area bargaining. For example, in Denmark the proportion of firms carrying out two-tier bargaining more than doubled between 1989 to 1995 (Traxler et al. (2001); Andersen et al. (2003)). Similarly, the number of Belgian firms involved in both industry and plant-level agreements increased tenfold from 1980 and the mid 1990s (Van Ruysseveldt and Visser (1996)). Two-tier bargaining structures are also present in Austria, Finland, the Netherlands, Norway, and Sweden (Boeri et al. (2001)).

3.3 Theoretical Framework

Both Italy and Germany have large spatial productivity differences. In Italy, national contracts limit the ability of local wages to adjust to local productivity, while in Germany the past two decades of labor market reforms have allowed employers to adjust wages to local productivity.

In this section, we present a simplified spatial equilibrium model intended to provide intuition for the effects of the Italian and German wage-setting systems and to guide our empirical analysis. The model is a standard Rosen-Roback model and is kept deliberately simple. The main objective is to compare the spatial equilibria under two extreme cases: (1) local nominal wages can freely adjust to local productivity, and (2) local nominal wages cannot adjust (due to institutional constraints) but workers and firms are free to relocate. Rosen-Roback models of spatial equilibria have traditionally focused on the case of market clearing. The case where the labor market does not clear has not received much attention (see Kline and Moretti (2013) for an exception where unemployment arises from search frictions.)

In interpreting the model, two points need to be clear. First, the focus is on geographic differences. Thus, we abstract from all sources of wage rigidity that are nationwide and affect all provinces equally. While both Germany and Italy have important labor market

rigidities of this variety, their effects are clearly outside the scope of this paper.

Second, while the model provides useful extreme benchmarks, it should be clear that neither Germany nor Italy is exactly described by either of these two extremes. While German firms have more flexibility to set nominal wages more in line with local productivity levels, union contracts ensure that this flexibility is not absolute. Similarly, while Italian firms have more limited flexibility in setting nominal wages, they nevertheless maintain some scope to adjust wages.

3.3.1 Setup

We consider two regions $r = \{n, s\}$ that produce a traded good with a price set on the international market. Production in each region is given by:

$$Y_r = A_r K_r^{(1-\alpha)} E_r^\alpha \quad (3.1)$$

where A_r denotes TFP; E_r is employment and K_r is capital. The two regions are ex-ante identical with the exception of their level of TFP. We assume that n is more productive than s due to exogenous historical factors: $A_n \geq A_s$.

Population of each region is L_r , with the total population of the country $\bar{L} = L_n + L_s$ assumed fixed. The utility of a resident of region r is given by:

$$\Omega_r = \frac{w_r}{p_r^\sigma} (1 - u_r)^\delta$$

where w_r is the nominal wage level, p_r is the housing price in region r ; σ is the weight of housing in the consumption basket; u_r is the non-employment rate in region r : $u_r = 1 - (E_r/L_r)$.¹⁹ We assume that workers can freely move across regions and that they optimally choose where to live. Specifically, we assume zero mobility costs and no heterogeneity in taste for location. Thus, in equilibrium it needs to be the case that workers are indifferent across the two regions: $\Omega_n = \Omega_s$.²⁰

¹⁹For simplicity, we ignore local amenities and assume that workers are renters. Both assumptions can be relaxed.

²⁰In the case of heterogeneity in taste for location, the marginal worker is indifferent between the two

Firms optimally choose how many workers to hire and how much capital to use. As in the standard model, factor demand comes from the first order conditions implying that the price of each factor must be equal to its marginal product. Capital is supplied to firms in a region at an increasing price: $i_r = \mu \ln K_r$.

To close the model, we assume that each resident consumes one unit of housing and that the supply of housing is upward sloping: $\ln p_r = \gamma \ln L_r$. Put differently, housing costs are proportional to regional population.

Nominal wages, employment, capital, housing prices, population, and interest rates in each of the two regions are endogenous: $w_n, w_s, E_n, E_s, K_n, K_s, p_n, p_s, L_n, L_s, i_n, i_s$.

3.3.2 Equilibrium When Wages Are Set by Market.

We first consider the standard free market case with flexible wages. The usual condition that a region's nominal wage equals the region's marginal product of labor follows from firms' first order conditions:

$$w_r^* = \alpha A_r K_r^{*(1-\alpha)} E_r^{*(1-\alpha)} \quad (3.2)$$

where the asterisk denotes an equilibrium variable in the free market case. Similarly, demand for capital in the two regions is determined by the marginal product of capital, obtained by differentiating Equation 3.1 with respect to K . In equilibrium, the marginal product of capital equals the rate of return. Given the additional condition that workers must be indifferent between the two regions, employment, population, capital and housing prices in the two regions are determined. The resulting equilibrium is the standard Rosen-Roback equilibrium, which is well understood in the literature. For our purposes, three features of this equilibrium are worth emphasizing.

First, equilibrium employment, capital and nominal wages are higher in n , which is the region with higher TFP. This can be seen explicitly by expressing equilibrium employment, capital and nominal wages as a function of the model exogenous parameters: locations, so results are qualitatively similar (See Moretti 2010).

$$\ln E_n^* - \ln E_s^* = \frac{(1 + \mu)}{\sigma\gamma(\mu + \alpha) + \mu(1 - \alpha)}(\ln A_n - \ln A_s) > 0 \quad (3.3)$$

$$\ln K_n^* - \ln K_s^* = \frac{(1 + \sigma\gamma)}{\sigma\gamma(\mu + \alpha) + \mu(1 - \alpha)}(\ln A_n - \ln A_s) > 0 \quad (3.4)$$

$$\ln w_n^* - \ln w_s^* = \frac{(1 + \mu)\sigma\gamma}{\sigma\gamma(\mu + \alpha) + \mu(1 - \alpha)}(\ln A_n - \ln A_s) > 0 \quad (3.5)$$

These three equations make intuitive sense. Since TFP is higher in n , profit-maximizing firms hire more workers and use more capital in that region. The differences in labor and capital inputs are proportional to the difference in TFP. The marginal product of labor is also higher in n , and hence the equilibrium nominal wage is higher, with the regional wage gap proportional to the gap in TFP.²¹

Second, housing costs are higher in n because more workers live there in equilibrium:

$$\ln p_n^* - \ln p_s^* = \frac{(1 + \mu)\gamma}{\sigma\gamma(\mu + \alpha) + \mu(1 - \alpha)}(\ln A_n - \ln A_s) > 0 \quad (3.6)$$

The difference in housing costs between the North and the South needs to be large enough to make workers indifferent between the two regions. This follows from the spatial equilibrium assumption. As a result, while nominal wages are higher in n in equilibrium, real wages are equalized in the two regions:

$$\frac{w_n^*}{p_n^{*\sigma}} = \frac{w_s^*}{p_s^{*\sigma}}$$

Finally, there is full employment in both regions, since there are no rigidities preventing the labor market to clear: $u_n^* = u_s^* = 0$.

3.3.3 Equilibrium When Wages Are Set by National Contract.

We now turn to the case of wage rigidity due to collective bargaining. We assume that a national contract forces firms to pay the same nominal wage \bar{w} in the two regions despite

²¹Since more inputs are used in n , it follows that output is also higher in n : $Y_n^* - Y_s^* > 0$.

productivity differences. In particular, we focus for simplicity on the case where nominal wages are set equal to the market clearing wage in n , and thus above the market clearing wage in s :

$$\bar{w} = w_n^* > w_s^* \quad (3.7)$$

As discussed in Section 3.2, this is consistent with the typical contract in Italy. Results are qualitatively similar when nominal wages are set between the market clearing wage in n and s : $w_n^* \geq \bar{w} \geq w_s^*$.

After \bar{w} is set by the national contract, employment, population, and housing prices adjust endogenously in the two regions. Non-employment also adjusts endogenously, since it depends on employment and population. The key difference relative to the free market case is that a national contract results in lower equilibrium employment, capital and output in s . While the wage in s is higher relative to the free market equilibrium, fewer workers are employed and total national employment declines. As a consequence, aggregate output, aggregate earnings and per-capita income are lower. By imposing a wage in s that exceeds s 's productivity, the national contract generates spatial misallocation and causes a national economic loss.

To see this more precisely, consider how firms set employment in this context. The right hand side of Equation (3.2) still represents the region's marginal product of labor, and therefore the labor demand function of firms in that region. But now the region's nominal wage is not endogenously determined by the market, and is instead exogenously set equal to \bar{w} . Firms in each region maximize profits by choosing employment and capital accordingly.

Just like in the free market case, residents reallocate between n and s until utility is equalized in the two regions. Thus in equilibrium:

$$\frac{\bar{w}(1 - u_n^{**})}{p_n^{**\sigma}} = \frac{\bar{w}(1 - u_s^{**})}{p_s^{**\sigma}}$$

where the double asterisk denotes an equilibrium variable in the collective bargaining case. A number of important features of this equilibrium are worth discussing. First, em-

ployment is lower in s . A comparison with Equation (3.3) indicates that the employment gap is larger than in the free market case:

$$\ln E_n^{**} - \ln E_s^{**} = \frac{(1 + \mu)}{\mu(1 - \alpha)}(\ln A_n - \ln A_s) > 0 \quad (3.8)$$

Importantly, unlike in the free market case, now s experiences equilibrium non-employment: $u_s^{**} > 0$. Intuitively, \bar{w} is above the market clearing wage in s and non-employment results from the wedge between the wage and local productivity. In equilibrium, the level of non-employment in s is proportional to the productivity gap:

$$u_s^{**} = \frac{(1 + \mu)\sigma\gamma}{\mu(1 - \alpha)(\sigma\gamma + \delta)}(\ln A_n - \ln A_s) > 0 \quad (3.9)$$

By contrast, n enjoys full employment because \bar{w} is equal to its market clearing wage.

Second, as in the free market case, housing costs are higher in n since employment and population are higher there²² But unlike the free market case, real wages are now *lower* in n :

$$\frac{\bar{w}}{p_n^{**\sigma}} - \frac{\bar{w}}{p_s^{**\sigma}} = -\frac{(1 + \mu)\sigma\delta\gamma}{\mu(1 - \alpha)(\sigma\gamma + \delta)}(\ln A_n - \ln A_s) < 0 \quad (3.10)$$

This is due to the fact that housing costs are lower in s but nominal wages are the same. This has the interesting implication that conditional on employment, residents of s are better off than residents of n . Residents of the s queue to get a job, and those who earn jobs are better off than their counterparts in n .

Third, in equilibrium firms invest less in s than in n . The gap in the capital stock is

$$\ln K_n^{**} - \ln K_s^{**} = \frac{1}{\mu(1 - \alpha)}(\ln A_n - \ln A_s) > 0 \quad (3.11)$$

²²In particular: $\ln p_n^{**} - \ln p_s^{**} = \frac{(1 + \mu)\delta\gamma}{\mu(1 - \alpha)(\sigma\gamma + \delta)}(\ln A_n - \ln A_s) > 0$.

3.3.4 Aggregate Earning Losses When Wages Are Set by National Contract.

We have found that in the fixed wage equilibrium, a fraction of residents in s are not employed. They optimally choose to stay in the South even if they are idle because if they were to find a job, the real wage would be higher. While employment is higher in n in the fixed wage equilibrium compared to free market equilibrium, this is not enough to offset the employment losses in s . Thus, relative to the free market equilibrium, the fixed wage equilibrium results in lower *aggregate* employment in the country.

Moreover, since capital and labor are imperfect substitutes, the stock of capital in s is also lower in the fixed wage equilibrium.²³ Importantly, the fixed wage equilibrium also results in lower aggregate output

$$Y_n^* + Y_s^* > Y_n^{**} + Y_s^{**} \quad (3.12)$$

If labor demand is elastic, the fixed wage equilibrium also results in lower per capita earnings

$$\frac{(w_n^* E_n^*) + (w_s^* E_s^*)}{\bar{L}} > \frac{(\bar{w} E_n^{**}) + (\bar{w} E_s^{**})}{\bar{L}} \quad (3.13)$$

To see why, notice that we can rewrite the inequality in Equation 3.13 as

$$(\bar{L} u_s^{**}) \bar{w} > E_s^* (\bar{w} - w_s^*) \quad (3.14)$$

The term $(\bar{L} u_s^{**})$ on the left-hand side is the employment loss (or the total number of non-employed) under the fixed wage equilibrium.²⁴ The right hand side $-E_s^* (\bar{w} - w_s^*)$ is the employment level in s under the free market equilibrium times the wage increase experienced by workers in s under fixed wages.

This expression can be seen graphically in Figure 3.1. Point 1 and 2 are the free

²³The equilibrium amount of capital in s is $\ln K_s^{**} = \frac{\ln A_s + \ln(1-\alpha) + \alpha \ln E_s^{**}}{\mu + \alpha}$. Since $E_s^{**} < E_s^*$, it follows that $K_s^{**} < K_s^*$.

²⁴The employment loss $(\bar{L} u_s^{**}) \bar{w}$ is smaller than the change in employment in s because under fixed wages employment in n is higher than under free market.

market equilibrium and the fixed wage equilibrium in s , respectively. The left hand side of Equation 3.14 is the area of the rectangles $A+C$. The right hand side is the area of the rectangles $B + C$. Per capita earnings are larger under free market if and only if the rectangle A is larger than B .

Intuitively, setting the wage above the market wage in s has two effects. On the one hand it raises the wage that employed workers receive by $(\bar{w} - w_s^*)$. On the other it lower overall employment in the country by an amount defined in Equation (3.9). Figure 3.1 clarifies that per capita earnings are larger if the negative effect of employment losses (the area of A) exceeds the positive effect from the higher wage (the area of B). The figure also clarifies that this will be the case if the elasticity of labor demand in region s is larger than one.²⁵

To sum up: setting the wage above its free market equilibrium in s causes non-employment, lower investment and lower aggregate output in s . In n , employment and capital in the fixed wage case are larger than in the free market case, but this only partially mitigates the aggregate output and earning losses. Overall, the wage rigidity created by national union contracts has aggregate costs in terms of forgone output and per capita earnings. In Section 3.6 we will quantify the earning losses in the case of Italy.

3.4 Data

Our empirical analysis is based on data for the labor and housing markets in Italy and Germany. Employment rates are obtained by the national statistical offices and are for the group of 15-64 year old. Our wage data for Italy and Germany are from the National Italian Statistical Office (ISTAT) quarterly labor force statistics and the Institute for Employment Research (IAB), respectively. They include all private and public employees for Italy (individual-level wage data for 2009- 2013); and all private and public employees who are subject to social security contributions for Germany (individual-level wages for 1975-2014). For Italy, housing cost data are from the Osservatorio Mobiliare Italiano and contain transaction-level data on residential real estate sales in Italy. German housing

²⁵For a small open economy, product demand and therefore labor demand are likely to be elastic.

data are obtained from the Bundesinstitut für Bau-, Stadt- und Raumforschung (BBSR), the regional planning authority in Germany. We obtain data on gross value added for all industries on the local level from OECD (2018a).

Geographical Unit of Analysis. Choosing a geographic definition of local labor markets constitutes an important assumption in our study. Ideally, we would like to use a geographic unit akin to US Metropolitan Statistical Areas or Commuting Zones, which are small enough to encompass economically-meaningful units but large enough that most residents both live and work within a single region. Administrative boundaries of municipalities are likely to be too small; Italian and German workers easily commute across municipalities. For Italy, our definition of local labor markets is based on 103 provinces, with working-age populations ranging from 76,884 to 3,418,941 and an average of 495,104. For Germany, we base our definition on 96 ‘Spatial planning regions’ (Raumordnungsregion) with an average working age population of 737,448 and a range of 187,990 to 3,030,240 in 2010.

In Italy, we define North and South by including in the North the following regions: Emilia-Romagna, Friuli Venezia Giulia, Liguria, Lombardia, Marche, Piemonte, Toscana, Trentino-Alto Adige, Umbria, Valle d’Aosta, Veneto. The South is the everything else. In Germany, we define West and East based on the historical Cold War division, with Berlin assigned to East.

Wages and Employment. To account for differences in worker quality and industry characteristics, for the remainder of this paper we will define wages in a locality as *conditional* average wages net of workers’ characteristics (education, age, and gender) and net of industry effects. Specifically, we regress

$$w_i = \alpha + Z_i\beta + u_i$$

where w_i is the hourly (Italy) or daily (Germany) wage of worker i and Z_i contains the following characteristics: gender, age, age squared, education, and industry. We then take the average residual of this regression \hat{u}_i for every year and province and add the

average predicted value across provinces for the year 2010 to each province-year residual. Adding the average 2010 baseline value is useful to maintain the wage *levels* to be able to interpret the province level deviations from the mean in their relative magnitude.²⁶ This procedure produces average wages in each province and year holding constant worker observable characteristics.

In measuring employment in Italy, a potentially important issue is the existence of a relatively large informal sector. Since the informal sector is widely understood to be larger in Southern provinces than in Northern provinces, this has the potential to skew our findings toward finding smaller employment rates in the South. Our wage data should be well suited to deal with this issue, since they are from an anonymous survey of workers and workers have limited incentive to misreport their wages. As for the employment rate - which is taken from official statistics - we present robustness checks using existing estimates on informal employment in Italy and find that our results are not very sensitive.

Housing Costs and Local Cost of Living Indexes. We deflate nominal wages using a measure of local cost of living that takes into account spatial differences in cost of housing and cost of other non-tradables.

To purge the Italian housing data of possible composition effect we regress

$$p_i = \alpha + X_i\beta + u_i$$

where p_i is the price per square meter of the housing object and X_i is a set of characteristics including: size category, presence of a balcony, terrace or cellar, condition, floor, brightness, views, orientation (in terms of cardinal direction), category of the neighborhood, closeness to commercial services, public services, public transport, park or garden, parking, and whether the municipality is located by the sea or in the mountains (these last two variables come from ISTAT). As with the wage residuals above, we then average the housing residuals from this regression by year and province and add them to the baseline.

²⁶Due to data availability, for Italy, we use hourly wages net of taxes, while for Germany we use daily wages gross of taxes. Later, we present robustness checks on whether using wages net or gross of taxes in the two countries matters for the results.

The German housing data are rent prices collected by the regional planning authority BBSR from online or newspaper advertisements. To compute prices per square meter, the BBSR uses non-furnished flats of a size between 40 and 130 sqm in announcements listed for less than half a year. They filter out implausible prices and luxury flats. We compute weighted averages by Raumordnungsregion, using weights that reflect the stock of housing in the area.

To build a local Consumer Price Index (Local CPI) we follow the methodology proposed by Moretti (2013). This measure takes into account that the prices of tradables may also vary regionally with the price of housing (e.g. a sandwich in Milan costs more than a sandwich in Palermo). Both countries' statistical offices provide regional measures of CPI that cannot be used for geographic comparisons as they are normalized to 1 in a given year. Nevertheless, they are useful in understanding whether and to what degree the prices of housing and other goods move together. Regressing the overall change in CPI for a province on the change of its housing component, we find the proportion of tradables' price changes that vary with housing prices. Using this, we can construct a CPI that is a reasonable measure of the geographic differences in prices that consumers face in all their goods.²⁷

Productivity. To our knowledge, the only measure of firm productivity available at a fine geographic level in Italy and Germany is gross value-added per worker. Gross value added is output valued (through capital and labour) at basic prices less intermediate consumption valued at purchasers prices. The basic price is the amount receivable by the producer from the purchaser for a unit of a product or service minus any tax on the product plus any subsidy on the product. Gross value added per worker in each province is obtained by dividing this measure by employment in that province. We obtain data on

²⁷Specifically, local CPI_{pt} is defined as $CPI_{pt} = \omega HP_{pt} + (1 - \omega)NHP_{pt}$ where HP_{pt} is housing price in province p and year t , NHP_{pt} is the price of non housing or non tradables and (ω) is the housing weight. Some part of NHP varies with the housing price so that $NHP = \pi HP + \nu$. Therefore when we regress ΔCPI_{pt} on ΔHP_{pt} , $\beta = (\omega + (1 - \omega)\pi)$. Then, we use ω to compute: $\pi = \frac{\beta - \omega_n}{1 - \omega_n}$. We then use the province specific housing prices obtained through our own calculations and construct the local CPI as:

$$CPI_{pt} = \omega HP_{pt} + (1 - \omega) [\pi HP_{pt} + (1 - \pi)NHP_t]$$

The housing weight in consumption ω is obtained from the statistical office Destatis. For Italy we use consumption weights from Households consumption surveys from 2005-2011.

gross value added per worker for all industries on the local level from OECD (2018a)²⁸

Table 3.4 shows summary statistics of all variables.

3.5 Empirical Evidence

In this section, we first document the degree of productivity differences across provinces in Italy and Germany (subsection 3.5.1). We then turn to wages, studying the relationship between nominal wages and local productivity (subsection 3.5.2). Third, we study the relationship between non-employment rates and local productivity (subsections 3.5.3) and the relationship between real wages and local productivity (subsection 3.5.4).

3.5.1 Value Added

Mean value-added per worker across provinces is Eur 54,837 in Italy and Eur 52,901 in Germany, but these means mask vast geographic differences across provinces in both countries. The maps in the top panel of Figure 3.2 show value-added per worker in Italy (left panel) and Germany (right panel) in 2010. Throughout the paper, all maps are in percent deviations from the unweighted national mean. In Italy, productivity appears higher in the North than in the South. In Germany, it is higher in the West than in the East.

The bottom panel in Figure 3.2 shows the spatial distribution of value-added in the two countries in the same year. Two features of this figure are important. First, the differences in productivity across regions are quite similar in Italy and Germany, with the bulk of the distribution between -20% and 20% in both countries with respect to the country mean. The range, 90-10 percentile difference and 75-25 percentile difference of deviations from the country mean are, respectively, 57.79%, 27.92% and 17.95% in Italy and 70.47.72%, 39.92% and 17.65% in Germany. This level of geographic heterogeneity in productivity is not atypical among industrialized countries and it is not unlike what we see, for example, in the US.²⁹

²⁸This measure has been used by Hall and Jones (1999).

²⁹The 90-10 percentile and 75-25 percentile difference in manufacturing log TFP across metropolitan

Second, while there is some overlap, it is clear from the figure that in Italy Northern provinces are vastly more productive than Southern provinces, and in Germany Western provinces are more productive than Eastern provinces. Interestingly, the differences are comparable in the two countries: the difference between the mean province in Northern and Southern Italy in 2010 is 17.6%, while the difference between the mean province in West and East Germany is 22.7%.

We stress that in this paper, we take these differences as given; our analysis focuses on the *effects* of these differences rather than their *causes*. As we discuss above, in Italy, North-South differences reflect historical differences in many determinants of regional productivity, including transportation infrastructure, distance from European markets, efficiency of local governments and local policies, criminal activity, and cultural norms. These differences are long lasting and largely determined by historical factors. In Germany, East-West differences likely reflect half a century of Communist rule in the East as well as other historical factors. While it is in principle possible to model endogenous regional differences in the long run, such models are outside the scope of this paper.

3.5.2 Nominal Wages

The map in the top panel of Figure 3.3 shows the spatial distribution of nominal wages in the two countries, drawn using the same scale. The main feature to notice is that the distribution is more compressed in Italy than in Germany: while there is geographic wage heterogeneity in Italy, it is significantly smaller than in Germany. The wage bargaining system in the two countries likely partially explains this regional heterogeneity. Conditional on worker characteristics, the wage difference between North and the South in Italy is only 4.2%, while the West-East difference in Germany is seven times larger at 28.2%.

The difference between Italy and Germany is seen more clearly in the bottom panel of Figure 3.3, which shows the spatial distribution across provinces in the two countries. The mass of the distribution in Italy is between -10% and 10% of the country mean,

statistical areas are .71 and .59 in the US in 2000 (Hornbeck and Moretti (2018)).

while in Germany it is between -26% and 22%. In 2010, the 75-25 percentile difference and 90-10 percentile difference were 5.8% and 10.3% in Italy and 13.1% and 42.9% in Germany. The amount of spatial wage dispersion in Germany is lower than what we see in the US, but not by much. By contrast, the amount of spatial wage dispersion in Italy is much lower.

Note that wages are by no means completely uniform across Italian provinces despite national wage contracts at the industry level. There are three reasons for this. First, recall that nationwide contracts offer a wage floor, and while the floor is binding in most cases, firms are free to (and sometimes do) offer higher wages. Temporary contracts offer some additional degree of wage flexibility, although their number is limited by law. Second, some workers are paid under the table. In this case, of course, nationwide contracts are ignored, but our wage data should include these cases, as discussed above. Third, there are data limitations. National contracts specify a given wage for a given occupation and level of seniority in the firm. While we control for a worker work experience, as standard in wage regressions, we do not directly observe seniority. Our occupational categories are not as fine as those used in union contracts.

Overall, while there is some geographical nominal wage dispersion in Italy, it is clear from Figure 3.3 that nominal wage dispersion is empirically much more pronounced in Germany.

In this study, the key empirical relationship is that between local productivity and local nominal wages. If wages can fully adjust, our model indicates that we should see a tight relationship between the two, with provinces that enjoy higher productivity having higher nominal wages. This is indeed the case in the US (Hornbeck and Moretti, 2018). By contrast, if wages are prevented from fully adjusting, we should see a weaker relationship or none at all.

Figure 3.4 presents scatter plots that document the relation between the log conditional mean nominal wage by province on the y-axis and log mean value-added on the x-axis in 2010. Consistent with institutional differences in wage setting mechanisms, German firms are significantly more able to adjust nominal wages to local productivity than

Italian firms. Due to nationwide contracts, there appears to be a significant wedge in Italy between wages and marginal product of labor. Table 3.5 show the corresponding regression coefficients in a model including years 2000-2014 for Germany and 2009-2013 for Italy. Columns 1 and 4 show that in a regression of log conditional mean nominal wage on log mean value-added the elasticity of nominal wages with respect to local value-added is 0.19 and 0.73 across the whole country in Italy and Germany, respectively. In other words, the elasticity is almost four times larger in Germany than in Italy. In columns 2, 3 and 5, 6 we show the same coefficients *within* each of the macro areas. Wages remain strongly correlated with differences in productivity even when eliminating the variation between East and West in Germany. This means that what we are observing is not a pattern between East and West only but a more general flexibility of the German wage setting institutions. In Italy, within the South the correlation between wages and productivity is statistically indistinguishable from zero at a 95% confidence level. The North displays a similar small correlation with productivity as the country overall. This is consistent with the idea that national bargaining contracts tend to cater to Northern firms (leaving the North relatively less constrained) while strongly constraining wage setting in the South of Italy.³⁰

Overall, we find that despite large productivity differences across provinces, Italy's wage-setting mechanism results in nominal wages that are generally compressed across space. Crucially, there is little or no correlation between mean productivity in a province and mean nominal wages. By contrast, Germany has more nominal wage dispersion. Although Germany has the same amount of productivity difference across provinces, the absence of binding national wage contracts allows wages to better adjust to local labor market conditions.

³⁰A similar point is made in Table 3.6. In the first row, we regress individual level log wages on workers characteristics: sex, age, age squared, education and the industry. Entries in the table refer to the R^2 . In the second row, we add province fixed effects. In Italy, the R^2 increases only marginally, from 0.35 to 0.36. By contrast, in Germany the R^2 increases significantly more, from 0.39 to 0.46. This suggests that despite large productivity differences across provinces, local factors play a minimal role in explaining individual level wage variation in Italy, and a larger role in Germany.

3.5.3 Probability of Non-Employment

Our model predicts that in Italy, where wages cannot adjust fully, provinces with low productivity should have higher non-employment rates. This should be less true in Germany, where wages can adjust more to local productivity.

Empirically, this is indeed the case. The maps in the top panel of Figure 3.5 show rates of non-employment in Italy and Germany, by province. This difference between the two countries is more clear in bottom panel of Figure 3.5, which shows the spatial distribution of non-employment rates in the two countries. In Italy there is almost no overlap between the North and the South in non-employment rates. By contrast, despite equally large spatial productivity differences, the East-West differences in non-employment rates are much smaller. Indeed, the distribution for West Germany and East Germany overlap almost completely.

Figure 3.6 shows the relation between the log non-employment rate and log mean value-added for 2010. Unsurprisingly, the elasticity of non-employment with respect to value-added appears negative in both countries, indicating that provinces with lower value-added have higher non-employment. Consistent with our model, the elasticity appears significantly larger for Italy than Germany. Put differently, in Italy, areas with low mean value added have much higher non-employment rates than areas with high mean value added. In Germany, where nominal wages can adjust to local value added, the difference in non-employment rates is smaller.

Table 3.7 shows the corresponding coefficients in a model including years 2001-2015 for Germany and 2004-2015 for Italy. In a regression of local non-employment rates on local value-added, we find a significantly larger coefficient for Italy than Germany. In particular, comparing column 1 with column 4, we see that the elasticity in Italy is almost six times larger in absolute value. As we restrict ourselves to within macro area variation, we can see that a strong correlation between productivity and non-employment remains intact within each macro region in Italy. In Germany, this correlation drops to zero within the East and to a small value within the West.

One plausible concern is that the relatively large informal sector in Italy could bias

our findings. We probe the robustness of our findings by replicating both Figure 3.5 and Table 3.7 using an alternative measure of employment. This measure is based on ISTAT estimates of the proportion of irregular work among all full-time equivalent units of work. We use this measure to inflate the employment rate in each province proportionally to the estimated informal sector.³¹ The results are shown in Figure 3.7 and Table 3.8. They indicate that the relative larger presence of informal work in the South is unlikely to generate large differences with respect to our main results.

3.5.4 Cost of Living and Real Wages

Figure 3.8 and 3.9 show the spatial distribution of housing prices and overall cost of living (local CPI) in the two countries. Recall that geographic differences in overall cost of living reflect both the cost of housing and the cost of other non-tradables, with most of the spatial variation coming from the cost of housing. Housing prices and cost of living are higher in North Italy and West Germany, with the geographic differences slightly more pronounced in Italy.

Figure 3.10 shows real wages, defined as nominal wages deflated by the index of local cost of living. The comparison between nominal wages in Figure 3.3 and real wages in Figure 3.10 is striking. It indicates that real wages in many provinces of the South of Italy are significantly *higher* than the country mean, despite having low productivity. In Germany the same inversion does not occur. This is consistent with the predictions of our model.

Table 3.9 quantifies the North-South and West-East differences in nominal and real wages. Columns 1 and 3 show the nominal wage difference between North-South (column 1) and West-East (column 3), after conditioning on workers characteristics. Despite the fact that productivity differences are similar in the two countries, the difference in nominal wages is much smaller in Italy than in Germany because wages cannot fully adjust in Italy. Columns 2 and 4 show the corresponding real wage difference. Consistent with our model, the difference in real wages becomes negative in Italy, indicating that on average, real

³¹In particular, we inflate the official employment rate by a factor $\frac{1}{1-e_{inf}}$ where e_{inf} is the share of work estimated to be done in the informal sector.

wages are higher in the South.³²

Figure 3.11 presents the province-level relationship between log real wages and log value-added. In Italy, there is a negative relationship, indicating that the most productive provinces tend to have the lowest real wage. In Germany, the relationship is positive.

This finding is striking. Southern Italian provinces are characterized by lower nominal wages but higher real wages as a result of relatively low housing prices and cost of living. As a result, employed Italians are better off living in the South than the North because they have a higher real wage. However, non-employment is higher in the South. Southern jobs are essentially rationed, with residents standing in line waiting to get one. If they succeed, their real wage is higher than in the North. This implies an inefficient spatial allocation of resources and possibly large economic costs in terms of forgone earnings and forgone employment.

In Germany the spatial equilibrium is different. Because nominal wages are more flexible, there are smaller differences in non-employment rates across regions.

Figure 3.12 shows the relationship between nominal and real wages across local areas in 2010. It shows that nominal wages were strongly dispersed in Germany but very compressed in Italy. In Germany, the correlation between real and nominal wages is visible. In Italy, real wages differ considerably but favor the South.

3.6 Aggregate Costs of Spatial Misallocation

Our model in Section 3.3 has shown that the wage rigidity induced by national union contracts has aggregate costs in terms of forgone aggregate earnings and forgone employment. In this Section we estimate the aggregate costs stemming from spatial misallocation in Italy.

Intuitively, under the current wage setting system, firms in provinces where productiv-

³²One difference between the two countries in terms of data is that the wages we use for Italy are net of taxes. Given a progressive tax scheme, taxes could compress wages and thus exaggerate the patterns we are pointing to in this paper. To adjust for this we use the mean wage of all full time workers in social security records (INPS) gross and net of taxes (from 2015) to generate a net/gross ratio for every province. We then construct the net/gross corrected wages dividing the net ISTAT wage of every province by the net/gross ratio for every province. The results base based on corrected wages are in the Table 3.10. The correction does not change our findings.

ity is low are forced to pay wages above the local market-clearing level. As a consequence, output and employment in those provinces are below what they could be if wages were flexible. If nominal wages were allowed to reflect local productivity, output and employment in low-productivity provinces would increase, resulting in an overall increase in national aggregate output and aggregate employment. This could happen if, for example, union contracts were negotiated at the provincial level instead of the national level.

Italian aggregate and per capita earnings would likely increase as well. More specifically, our model shows that total and per capita earnings would increase if the elasticity of labor demand in the traded sector is larger than one. Intuitively, in low-productivity provinces wages would decrease relative to the current status quo. But an elastic labor demand implies that the increase in local employment would be proportionally larger than the decline in wages, insuring a positive effect on aggregate earnings. Labor demand in the traded sector is likely to be elastic in the case of a small open economy like Italy, fully integrated in European continental product markets.

As a result, allowing union contracts to be negotiated at the provincial level would improve efficiency, which would likely increase GDP, total employment, aggregate earnings and per capita labor income. It would also have some distributional consequences, in the sense that it would generate winners and losers relative to the status quo. The main losers would be currently-employed workers in the South, as they would face lower nominal and real wages.

To quantify the magnitude of these effects, we provide estimates from two counterfactual exercises. The two exercises are similar in that they both relax wage rigidity by allowing some local wages to depend on local productivity. They differ in how tight the link between local wages and local productivity is allowed to be.

In the first counterfactual scenario, we assume that wages in each province are allowed to fully adjust to local productivity. That is, we assume that if one province is 10% less productive than another, then its wages are 10% lower. This is indeed the case in the standard market-clearing case, as can be seen by the first order conditions in equation 3.2. It's a useful benchmark, but is not necessarily realistic in a European context.

In the second counterfactual, we assume that the relationship between wages and productivity and the one between employment and productivity are the same as those observed in Germany. Crucially, we do not assume that the average wages or average employment in Italy would be the same as in Germany, nor are we constraining productivity to be similar. Instead, we assume that if in Germany provinces with 10% higher productivity pay wages that are $x\%$ higher, the same would be true for Italy, whatever the productivity level in each Italian province. For conservative estimates on the changes in the employment rate, we use the employment rate corrected for informal employment as described in section 3.5.3 as a baseline for Italy.

We caution that while the empirical results in the previous section were estimated exclusively from data, the quantification of aggregate losses naturally relies on some assumptions. The estimated effects should therefore be considered as an attempt to assess their order of magnitude, rather than as exact figures.

3.6.1 Assumptions

As we argued in Section 3.2, the current Italian nationwide contracts can be thought of as setting wages approximately equal to the market-clearing level in the North. Correspondingly, in our main set of counterfactuals we assume that, in the status quo, wages are set to clear the labor market of the median province in the North. We denote the productivity, wage, and employment rate of the median province in the North as GVA_{max} , ω_{max} and e_{max} , respectively. In the data, the corrected employment rate in this province is 71.23% vs. 66.99% on average over all provinces. The median wage is 8.7 Euros per hour, compared to the 8.5 Euro median across all provinces.

To probe the robustness of our findings with respect to this assumption, later we also try different variants using the median of the top 5, 10 or 20 provinces in the North in terms of productivity.

Scenario 1: The counterfactual wages and employment in this scenario are based on the assumption that wages in provinces outside the North are allowed to fully adjust to local productivity. In practice, this means that for each province outside the North, we

calculate the percent difference between local productivity (GVA_p) and productivity of the North (GVA_{max}): $\delta_p = \frac{GVA_{max} - GVA_p}{GVA_{max}}$. We then compute counterfactual wages in a province as $\omega_{p1}^* = (1 - \delta_p) \omega_{max}$, where ω_{max} is the median wage of the target. Similarly, we compute the counterfactual employment rate as: $e_{p1}^* = e_{max}$. The counterfactual wage and employment for provinces in the North are kept equal to those observed in the data.

Scenario 2: In this scenario, we assume that the relationship between Italian wages and productivity is equal to the relationship between German wages and productivity. Accordingly, we estimate β_ω , the coefficient from a OLS regression of log wage level on log value-added by provinces Germany. The coefficient corresponds to the one in Table 3.5, and is equal to .74. We also assume that the relationship between Italian employment and productivity is the same as the relationship between German employment and productivity, calculating β_e as the coefficient of a regression of log employment rate on log value-added in Germany. It is equal to .25.

The counterfactual wage is defined as $\omega_{p2}^* = (1 - \beta_\omega \delta_p) \omega_{max}$ and the counterfactual employment rate is $e_{p2}^* = (1 - \beta_e \delta_p) e_{max}$. As in Scenario 1, the counterfactual wage and employment for provinces in the North are kept equal to those observed in the data. For both scenarios the expected hourly wage per province is the wage ω times the employment probability e .

3.6.2 Results

Our main findings are in Table 3.11, which reports the resulting changes in employment and aggregate earnings. Letting nominal wages in each province fully adjust to local productivity (Scenario 1) would decrease hourly pay by 9.20% on average—or 77 cents an hour—in Southern provinces. We estimate that these changes would increase employment by 26.62% (about 14 percentage points) in the South, raising income for the average Southern worker, whether employed or not, by 13.86 %. For the country as a whole this increase would amount to a 11.95% increase in employment, corresponding to about 2.5 million additional jobs in the Southern provinces and to an average increase of about 500 euros of yearly income per working-age person in Italy.

If the geographic differences in the gap between productivity and nominal wages in Italy were set equal to those in Germany instead (Scenario 2), Southern hourly wages would decrease by 5.9% (that is 52 cents), Southern employment would increase by 24.6%, and per capita income would increase by 16.59% in the South and 7.45% across the country.

Figures 3.13 and 3.14 show the geographic dispersion of the percentage change of wages and employment from status quo to counterfactual for both scenarios.

Table 3.12 shows the counterfactual results using the uncorrected employment rate (not accounting for informal work). These results therefore also account for shifts from informal to formal employment. Table 3.13 repeats the counterfactual calculations for the case in which we use the median of the top 5 provinces in the North as the current wage baseline. In this case, average wages in Southern provinces would decrease by 11.41% to 16.96%, depending on the scenario, while Southern employment would increase by 24.8% to 28.05%. Aggregate earnings in the South would increase by 5.61% to 10.18%, and nationwide aggregate employment would increase by 11.8% to 13.74% and aggregate earnings would increase by 0.69% to 3.35%. Tables 3.14 and 3.15 show the results for the cases in which we use the top 10 and 20 top provinces of the North as our status quo productivity baseline.

It is obvious that these back-of-the-envelope calculations are based on rather coarse assumptions and that they are unlikely to be realized in the short run even if flexibilization does take place. Nevertheless, even if - as in Scenario 2 - we allow for a reasonable but persistent relationship between productivity and employment, employment in the South would increase substantially. The purpose of these counterfactual exercises is simply to highlight the existence of potentially large gains in terms of equity and efficiency deriving from a liberalization of wage bargaining across different localities. The costs originating from spatial misallocation in Italy appear to be substantial.

3.7 Conclusions

Centralized wage bargaining systems are common in Europe and have traditionally received substantial attention in the economic literature. But their effects on the geography of employment and wages and their aggregate costs have not previously been studied.

In this paper, we study the local and aggregate effects of national wage bargaining systems by comparing the spatial distribution of wages, non-employment rates, cost of housing, and real wages in Italy and Germany. The two countries have a similar spatial distribution of firm productivity, but have adopted different models of wage bargaining. Italy sets wages based on nationwide sectoral contracts that allow for minimal local wage adjustments, while Germany has moved toward a more flexible system that allows for local bargaining.

We find that, as a consequence, the spatial distribution of nominal wages is very compressed in Italy, and the relationship between local productivity and local nominal wages is weak or possibly zero. By contrast, Germany has significantly more spatially-dispersed wages and a much tighter link between wages and local productivity.

These wage rigidities generate economically costly inefficiencies in Italy. We find that provinces with low productivity have significantly higher non-employment rates than provinces with high productivity, because employers in low productivity provinces cannot lower wages and end up hiring fewer workers.

Notably, we also uncover a *negative* relationship between real wages and local value-added in Italy. Despite having higher productivity, the North has *lower* real wages than the South, since the latter has low housing costs but similar nominal wages. This means that, conditional on having a job, a Italian worker is better off in the South in terms of purchasing power. However, the probability of having a job is higher in the North. Thus, national wage contracts have created a spatial equilibrium where workers queue for jobs in the South and remain unemployed while waiting. By contrast, in Germany real wages in the West are not significantly lower than in the East, since nominal wages are spatially more flexible.

From a macro-economic point of view, we find that the Italian wage bargaining system

generates significant economic costs in terms of forgone aggregate earnings and employment. If nominal wages were allowed to reflect local productivity, aggregate employment and aggregate earnings would significantly increase in Italy. Based on reasonable assumptions, we estimate that aggregate Southern employment could increase by about 14 percentage points. Aggregate earnings in the South would increase by 13.86% to 16.59%. Nationwide, we estimate that aggregate employment would increase by 11.04% to 11.95% or about 2.5 million jobs, and aggregate earnings would increase by 6.22% to 7.45%. In per capita terms, this amounts to around 500-600 euros per capita per year across all working-age adults, not just the employed. These could be the potential gains if Italy adopted a system similar to Germany's, where union contracts can be negotiated at the firm or local level instead of the national level.

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Appendix: Tables

Table 3.1: Example of a Collective Bargaining Contract in Italy: Construction Sector, 2016

Cost component	Mean	Min	Max	SD
Minimum/Floor	4.86	4.86	4.86	0.00
Indexation to inflation	2.96	2.96	2.96	0.00
Cost of living allowance	0.06	0.06	0.06	0.00
Variable component of pay	0.15	0.04	0.50	0.09
Sectoral allowance	1.11	0.91	1.26	0.07
Total : Hourly components of pay	9.02	8.79	9.37	0.09
Remuneration for national holidays	0.61	0.60	0.62	0.00
Compensation for yearly vacation	0.47	0.46	0.48	0.01
Contribution to mutual sectoral fund	1.77	1.73	1.80	0.01
Transport allowance	0.29	0.05	1.40	0.20
Compensation for training	0.18	0.18	0.18	0.00
Contribution to mutual fund for injury	0.21	0.20	0.21	0.00
Total : Additional costs	3.46	3.19	4.64	0.21
Contribution to social security	4.38	4.23	4.78	0.09
Contribution to accident insurance	1.62	1.57	1.77	0.03
Contribution to special construction worker fund	0.78	0.39	1.16	0.15
Total : Social security and accident insurance	6.78	6.34	7.57	0.23
Allowance for meals	0.60	0.12	1.75	0.26
Severance	0.98	0.92	1.39	0.05
Mobility allowance, complementary allowances	3.20	3.13	3.25	0.02
Contribution to pension fund	0.06	0.06	0.06	0.00
Total average hourly cost of labor	24.08	22.98	26.04	0.62

Note: This table shows statistics about the May 2016 dispersion across provinces of the components of the compensation for workers in “level 1” of the bargaining agreement for the construction sector (“Contratto Collettivo Nazionale per i Lavoratori Edili”). Monetary figures are in Euros.

Table 3.2: Median Monthly Wages at Three Employers

	Median North	Median Center	Median South
Private sector: Bank teller	1,666	1,667	1,664
Private sector: Energy company	2,736	2,923	2,931
Public sector: Elementary school teacher	1,305	1,305	1,305

Note: This table shows the median wage for employees working for the same employer with similar seniority and qualification in three macro Italian areas. In the first row, we show the monthly wage for a male bank teller with 10-20 years of experience in a large Italian bank, at level 6 of the internal hierarchy (“Inquadramento unico: impiegato”; the data come from Ichino and Maggi, 2000). The sample size is 423, 140, and 105 individuals for the North, Center, and South respectively. The second row shows the mean wages of a large company in the energy sector operating in almost all Italian provinces. We include men with less than 10 years of experience and with a permanent full-time contract. The sample size is 91, 64 and 130 individuals for the North, Center, and South respectively. In the third row we show the wages of elementary school teachers with 5 years of seniority.

Table 3.3: Percentage of workers covered by collective bargaining and opening clauses - Germany

Year	% workers under industry contract		% workers subject to opening clauses	
	(1)	(2)	(3)	(4)
	West	East	West	East
1996	69.22	56.30	.	.
1998	67.77	50.46	.	.
2001	63.11	44.60	.	.
2003	62.08	42.58	.	.
2005	58.74	41.89	33.36	23.69
2007	56.18	40.57	38.30	28.19
2009	55.46	38.35	.	.
2011	53.70	37.44	47.27	40.01
2013	52.03	35.13	.	.

Note: This table shows the coverage of union contracts and opening clauses ("exceptions to union contracts - on wage or hours"). The data are obtained from a linked employer-employee dataset of the IAB - Institute for Employment Research. The figures in this table show the fraction of workers working for an employer who states to apply industry contracts or to use opening clauses respectively.

Table 3.4: Summary Statistics - 2010

Germany	Mean	SD	N
Value added per worker	52901	8134	96
CPI	100.0	14.18	96
Housing price	100.0	18.53	96
Nominal wage	91.6	11.25	96
Real wage	92.3	10.76	96
Non-empl rate	27.9	3.59	96
Italy			
Value added per worker	54837	6227	103
CPI	100.0	16.19	103
Housing price	100.0	25.17	103
Nominal wage	8.5	0.44	103
Nominal wage - net/gross corrected	12.0	0.74	103
Real wage	8.8	1.47	103
Real wage - net/gross corrected	12.3	1.96	103
Non-empl rate	42.5	9.58	103
Non-empl rate corrected	34.0	7.55	103

Note: Value-added is computed across all industries in the geographic area in question, as calculated by the OECD (Germany) and ISTAT (Italy), and it is divided by employment in that area. Housing prices are average prices for a square meter with similar characteristics in each area. The CPI is constructed using those housing prices according to the method describes in section 3.4. Nominal wages are average wages controlling for individual characteristics such as age, education, gender, and industry. In Italy these are hourly wages net of taxes, in Germany daily wages gross of taxes. Real wages are deflated using Local CPI. Non-employment refers to the number of people age 15-64 out of employment over the total population of that age group.

Table 3.5: Regression of Mean Nominal Wages on Mean Value-Added

	Italy			Germany		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	South only	North only	All	East only	West only
Log value added	0.195	0.0635	0.215	0.736	0.557	0.358
	(0.013)	(0.033)	(0.023)	(0.013)	(0.027)	(0.008)
Provinces:	103	103	103	96	96	96

Note: Entries are the coefficients of mean log value-added in a regression of mean nominal log wage on mean log value-added pooling all years 2000-2014 (Germany) and 2009-2013 (Italy). The regression contains year fixed effects.

Table 3.6: R^2 from a Regression of Individual Wages on Worker Characteristics, Industry and Year Fixed Effects

	(1)	(2)
	Italy	Germany
Without province FE	.352	.389
With province FE	.36	.463
Difference	.008	.074

Note: the table reports the R^2 of a regression of individual wages on the same characteristics as above, industry and year fixed effects. Province fixed effects are then included and the R^2 is compared. The change in R^2 when including the province fixed effects is almost zero in Italy while it is almost 20% in Germany. Here we use data from 2009-2013 (Italy) and for 1992-2014 (Germany).

Table 3.7: Regression of Non-Employment Rate on Mean Value Added

	Italy			Germany		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	South only	North only	All	East only	West only
Log value added	-1.434	-0.658	-0.405	-0.252	0.0437	-0.117
	(0.030)	(0.053)	(0.036)	(0.024)	(0.063)	(0.035)
Provinces:	103	103	103	96	96	96

Note: Entries are the coefficients on mean value-added in a regression of the log non-employment rate among the 15-64 year olds on mean log value-added pooling all years 2001-2015 (Germany) and 2004-2015 (Italy). The regression contains year fixed effects.

Table 3.8: Regression of Corrected Non-Employment Rate on Mean Value Added

	Italy		
	(1)	(2)	(3)
	All	South only	North only
Log value added	-1.259	-0.387	-0.432
	(0.042)	(0.085)	(0.058)
Provinces:	103	103	103

Note: Entries are the coefficients on mean value-added in a regression of the "corrected" log non-employment rate among the 15-64 year olds on mean log value-added pooling all years 2001-2015 (Germany) and 2004-2015 (Italy). The regression contains year fixed effects.

Table 3.9: Average Wage Differences

	North - South		West - East	
	in Italy		in Germany	
	(1)	(2)	(3)	(4)
	nominal	real	nominal	real
% Difference	0.0425	-0.0921	0.282	0.176
	(0.003)	(0.017)	(0.003)	(0.007)
Year FE:	Yes	Yes	Yes	Yes
Provinces:	103	103	96	96

Note: This table reports the coefficient of a regression of conditional mean log wages on an indicator of north/west. We pool all available years. For nominal wages 2000-2014 (Germany) and 2009-2013 (Italy). For real wages 2004-2014 (Germany) and 2009-2011 (Italy). The regression contains year fixed effects.

Table 3.10: Nominal and Real Wages Corrected For Taxes

	North - South			
	Uncorrected		Corrected	
	(1)	(2)	(3)	(4)
	nominal	real	nominal - corr	real - corr
% Difference	0.0425	-0.0921	0.0717	-0.0629
	(0.003)	(0.017)	(0.004)	(0.017)
Year FE:	Yes	Yes	Yes	Yes
Provinces:	103	103	103	103

Note: Because wages in Istat are net of taxes, we use the mean wage of all full time workers in INPS gross and net to generate a net/gross ratio for every province. We then generate corrected wages dividing the net Istat wage of every province by the net/gross ratio for every province. The table shows the coefficient of a regression of conditional nominal and real wage on an indicator of north using the uncorrected and corrected wages respectively.

Table 3.11: Counterfactual Scenarios

	South		North		Total	
Average hourly wage:	Level	Change %	Level	Change %	Level	Change %
Status quo	8.36		8.68		8.54	
Scenario 1	7.56	-9.20	8.68	0.00	8.18	-4.13
Scenario 2	7.84	-5.90	8.68	0.00	8.30	-2.65
Employment rate:	Level	Change %	Level	Change %	Level	Change %
Status quo	57.32		71.00		64.86	
Scenario 1	71.24	26.62	71.00	0.00	71.11	11.95
Scenario 2	70.17	24.60	71.00	0.00	70.63	11.04
Monthly income per capita:	Level	Change %	Level	Change %	Level	Change %
Status quo	766.63		986.68		887.89	
Scenario 1	861.44	13.86	986.68	0.00	930.46	6.22
Scenario 2	881.00	16.59	986.68	0.00	939.24	7.45

Note: This table shows the counterfactual wage, employment and income levels by Macro area. Wages are in Euros per hour, employment rate in % of the 15-64 year olds. The employment rate is corrected for informal work as described in section 3.5.3. The labor income per capita is constructed multiplying the average full-time monthly earnings (assuming 160 working hours per month) in Euros with the employment rate and thus the probability of being in employment. The table also shows the average % changes across provinces between the actual and projected value of each variable. The counterfactual in Scenario 1 reflects a reduction of wages by the percent difference in mean value-added to the median of Northern provinces and an employment rate equal to the median of Northern provinces in all provinces. Scenario 2 adjusts wages according to the relationship between productivity and wages in Germany as well as employment according to the relationship between productivity and employment in Germany. Note that the benchmark group (ie. the Northern provinces) are not adjusted. Averages are weighted by working age population.

Table 3.12: Counterfactual Scenario - Uncorrected Employment Rate

	South		North		Total	
Average hourly wage:	Level	Change %	Level	Change %	Level	Change %
Status quo	8.36		8.68		8.54	
Scenario 1	7.56	-9.20	8.68	0.00	8.18	-4.13
Scenario 2	7.84	-5.90	8.68	0.00	8.30	-2.65
Employment rate:	Level	Change %	Level	Change %	Level	Change %
Status quo	47.02		64.69		56.76	
Scenario 1	64.43	40.74	64.69	0.00	64.57	18.29
Scenario 2	63.46	38.43	64.69	0.00	64.14	17.25
Monthly income per capita:	Level	Change %	Level	Change %	Level	Change %
Status quo	628.16		899.25		777.56	
Scenario 1	779.13	25.88	899.25	0.00	845.33	11.62
Scenario 2	796.82	28.99	899.25	0.00	853.27	13.01

Note: This table shows the counterfactual wage, employment and income levels by Macro area. Wages are in Euros per hour, employment rate in % of the 15-64 year olds. The employment rate is *not* corrected for informal work as described in section 3.5.3. The labor income per capita is constructed multiplying the average full-time monthly earnings (assuming 160 working hours per month) in Euros with the employment rate and thus the probability of being in employment. The table also shows the average % changes across provinces between the actual and projected value of each variable. The counterfactual in Scenario 1 reflects a reduction of wages by the percent difference in mean value-added to the median of Northern provinces and an employment rate equal to the median of Northern provinces in all provinces. Scenario 2 adjusts wages according to the relationship between productivity and wages in Germany as well as employment according to the relationship between productivity and employment in Germany. Note that the benchmark group (ie. the Northern provinces) are not adjusted. Averages are weighted by working age population.

Table 3.13: Counterfactual Scenarios - Variant 2: Top 5 provinces

	South		North		Total	
Average hourly wage:	Level	Change %	Level	Change %	Level	Change %
Status quo	8.36		8.68		8.54	
Scenario 1	6.92	-16.96	8.23	-5.24	7.64	-10.50
Scenario 2	7.38	-11.41	8.39	-3.38	7.94	-6.99
Employment rate:	Level	Change %	Level	Change %	Level	Change %
Status quo	57.32		71.00		64.86	
Scenario 1	71.95	28.05	72.41	2.08	72.20	13.74
Scenario 2	70.17	24.80	71.80	1.21	71.07	11.80
Monthly income per capita:	Level	Change %	Level	Change %	Level	Change %
Status quo	766.63		986.68		887.89	
Scenario 1	795.03	5.61	953.35	-3.31	882.28	0.69
Scenario 2	828.53	10.18	963.96	-2.22	903.16	3.35

Note: This table shows the counterfactual wage, employment and income levels by Macro area. Wages are in Euros per hour, employment rate in % of the 15-64 year olds. The employment rate is corrected for informal work as described in section 3.5.3. The labor income per capita is constructed multiplying the average full-time monthly earnings (assuming 160 working hours per month) in Euros with the employment rate and thus the probability of being in employment. The table also shows the average % changes across provinces between the actual value and the projected value of each variable. The counterfactual in Scenario 1 reflects a reduction of wages by the percent difference in mean value-added to the most productive provinces (in this case the median of the top 5 provinces in terms of value-added) and an employment rate equal to the one in the most productive provinces in all provinces. Scenario 2 adjusts wages according to the relationship between productivity and wages as it is in Germany as well as employment according to the relationship between productivity and employment in Germany. Note that the wages and employment of the top 5 provinces remain unadjusted. Averages are weighted by working age population.

Table 3.14: Counterfactual Scenarios - Variant 3: Top 10 Provinces

	South		North		Total	
Average hourly wage:	Level	Change %	Level	Change %	Level	Change %
Status quo	8.36		8.68		8.54	
Scenario 1	6.93	-16.79	8.25	-4.98	7.66	-10.28
Scenario 2	7.37	-11.56	8.38	-3.46	7.93	-7.09
Employment rate:	Level	Change %	Level	Change %	Level	Change %
Status quo	57.32		71.00		64.86	
Scenario 1	71.34	26.94	71.82	1.24	71.61	12.78
Scenario 2	69.66	23.87	71.32	0.53	70.58	11.01
Monthly income per capita:	Level	Change %	Level	Change %	Level	Change %
Status quo	766.63		986.68		887.89	
Scenario 1	790.23	4.95	948.41	-3.82	877.40	0.11
Scenario 2	821.33	9.19	956.94	-2.94	896.06	2.50

Note: This table shows the counterfactual wage, employment and income levels by Macro area. Wages are in Euros per hour, employment rate in % of the 15-64 year olds. The employment rate is corrected for informal work as described in section 3.5.3. The labor income per capita is constructed multiplying the average full-time monthly earnings (assuming 160 working hours per month) in Euros with the employment rate and thus the probability of being in employment. The table also shows the average % changes across provinces between the actual value and the projected value of each variable. The counterfactual in Scenario 1 reflects a reduction of wages by the percent difference in GVA to the most productive provinces and an employment rate equal to the one in the most productive provinces in all provinces. Scenario 2 adjusts wages according to the relationship between productivity and wages as it is in Germany as well as employment according to the relationship between productivity and employment in Germany. Averages are weighted by working age population.

Table 3.15: Counterfactual Scenarios - Variant 4: Top 20 Provinces

	South		North		Total	
Average hourly wage:	Level	Change %	Level	Change %	Level	Change %
Status quo	8.36		8.68		8.54	
Scenario 1	7.02	-15.74	8.34	-3.99	7.75	-9.27
Scenario 2	7.42	-10.97	8.43	-2.95	7.97	-6.55
Employment rate:	Level	Change %	Level	Change %	Level	Change %
Status quo	57.32		71.00		64.86	
Scenario 1	70.53	25.44	70.94	-0.02	70.76	11.41
Scenario 2	69.00	22.65	70.60	-0.50	69.89	9.89
Monthly income per capita:	Level	Change %	Level	Change %	Level	Change %
Status quo	766.63		986.68		887.89	
Scenario 1	791.41	5.11	946.83	-4.03	877.06	0.08
Scenario 2	819.10	8.89	952.57	-3.43	892.65	2.10

Note: This table shows the counterfactual wage, employment and income levels by Macro area. Wages are in Euros per hour, employment rate in % of the 15-64 year olds. The employment rate is corrected for informal work as described in section 3.5.3. The labor income per capita is constructed multiplying the average full-time monthly earnings (assuming 160 working hours per month) in Euros with the employment rate and thus the probability of being in employment. The table also shows the average % changes across provinces between the actual value and the projected value of each variable. The counterfactual in Scenario 1 reflects a reduction of wages by the percent difference in mean value-added to the most productive provinces and an employment rate equal to the one in the most productive provinces in all provinces. Scenario 2 adjusts wages according to the relationship between productivity and wages as it is in Germany as well as employment according to the relationship between productivity and employment in Germany. Averages are weighted by working age population.

Appendix: Figures

Figure 3.1: Model Equilibria

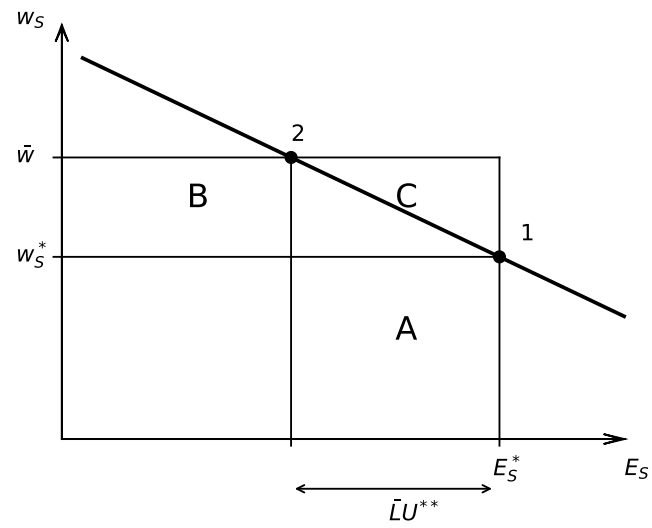
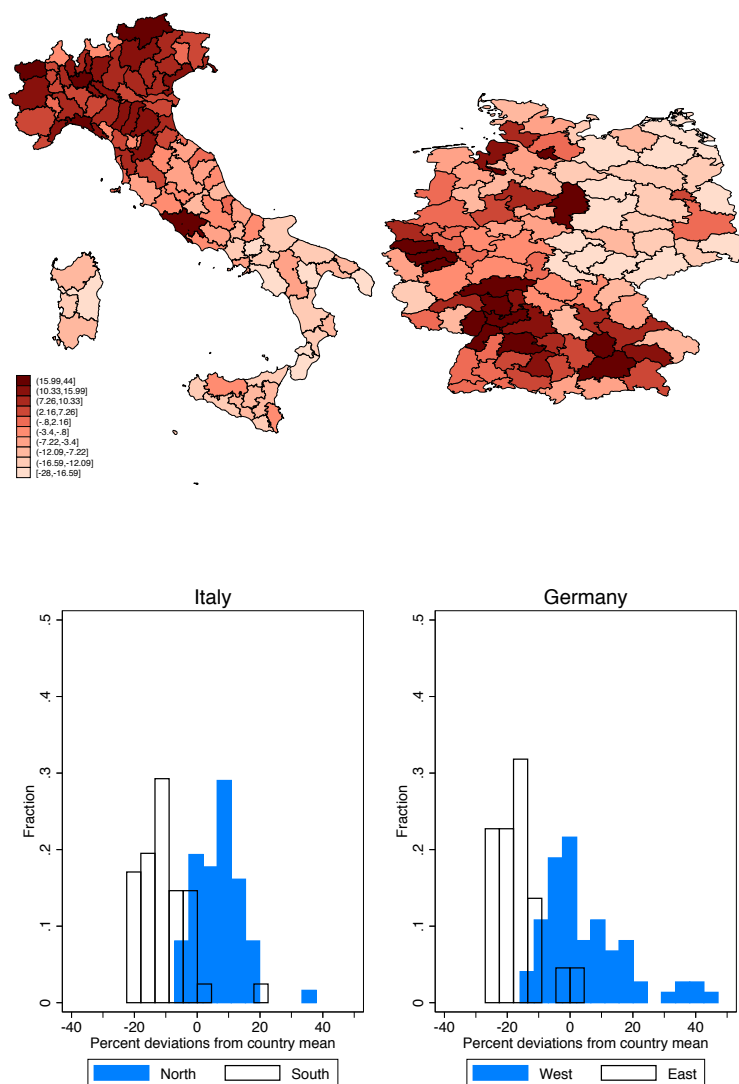
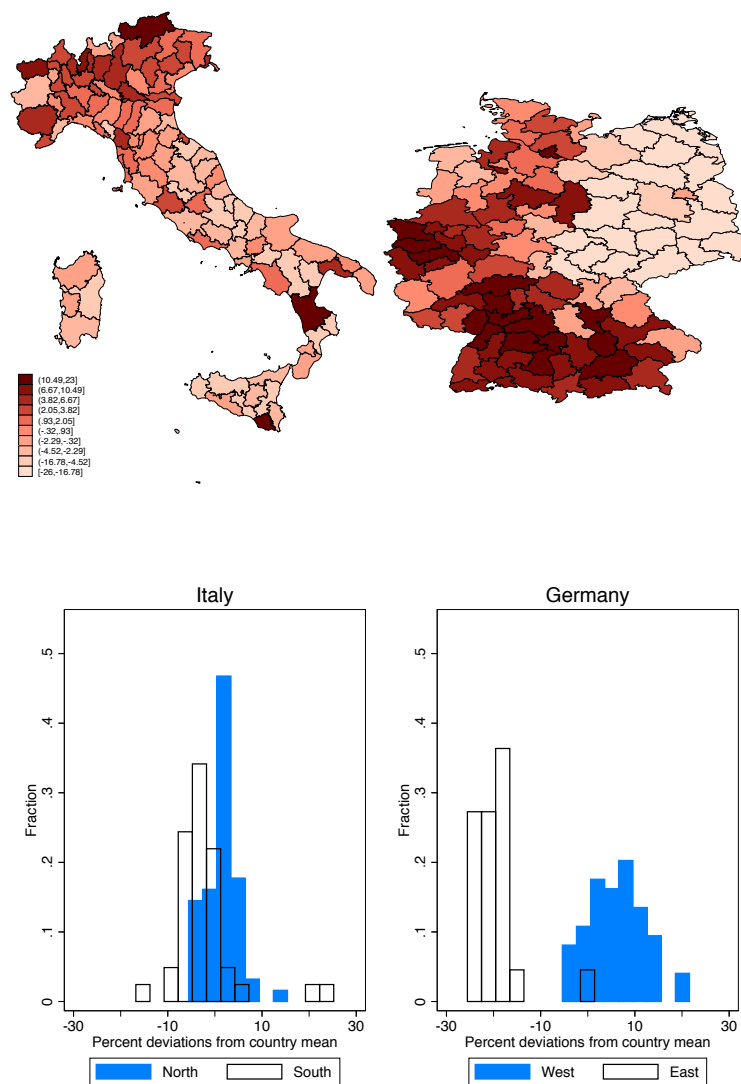


Figure 3.2: Mean Value-Added per Worker



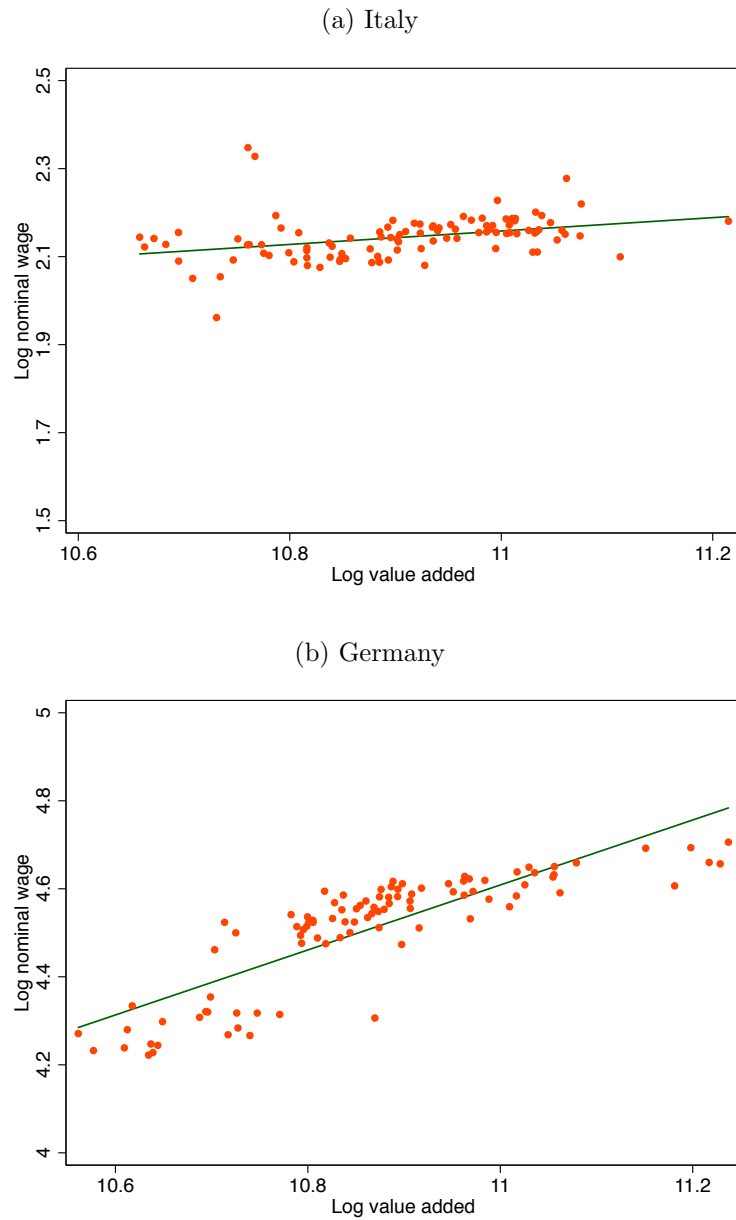
Note: This figure plots deviations from country mean of the gross value-added per worker of a local area in 2010. Means are not weighted by population.

Figure 3.3: Nominal Wages



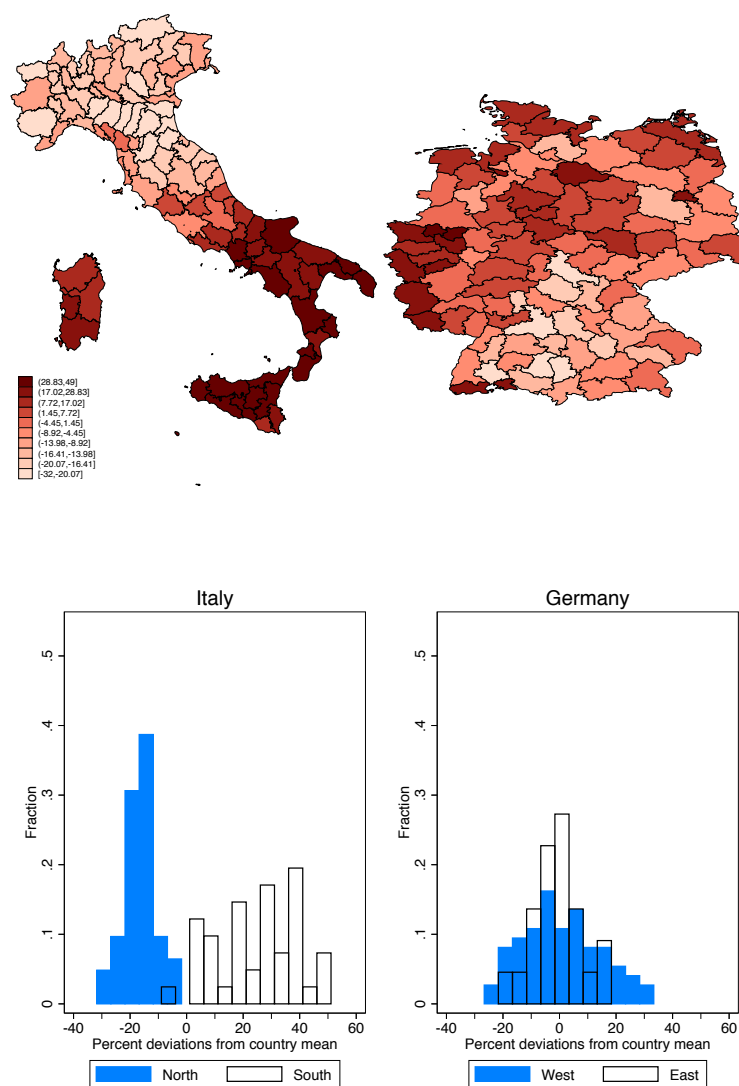
Note: This figure plots deviations from country mean of the nominal wages in euros (corrected for work force composition) in 2010. Means are not weighted by population.

Figure 3.4: Nominal Wage and Value Added



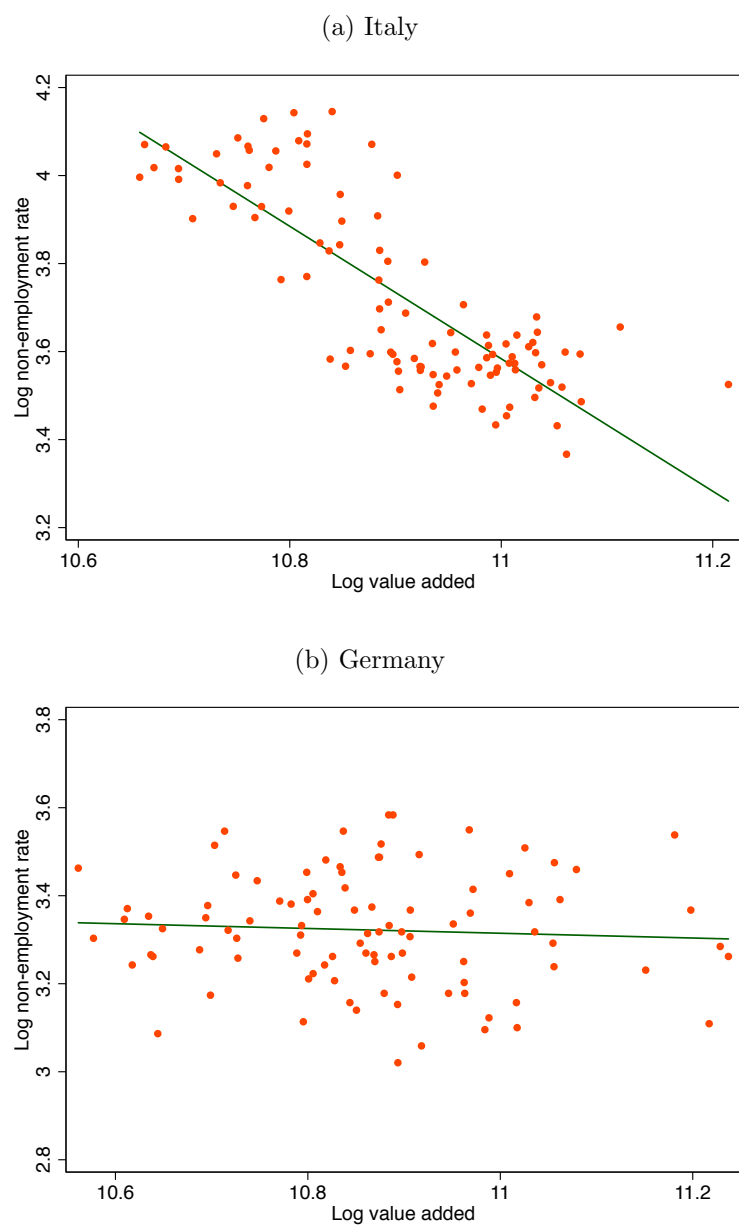
Note: This figure shows the relationship between log conditional nominal wages and log value-added in 2010.

Figure 3.5: Non-Employment Rate



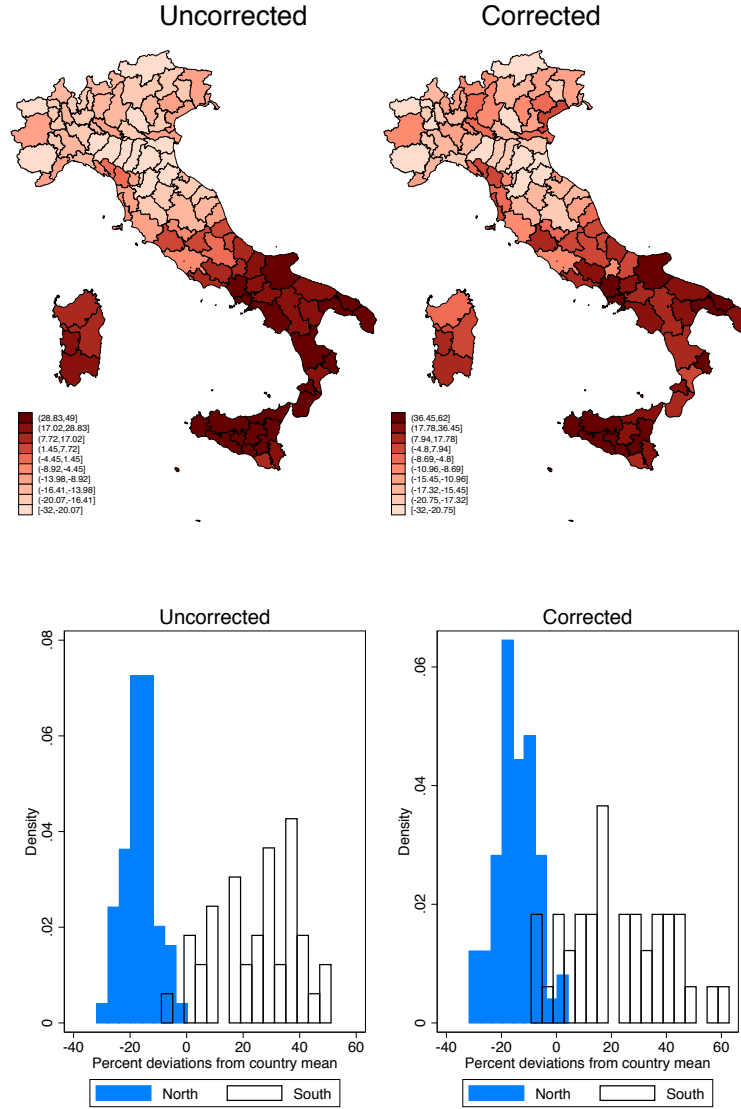
Note: This figure plots deviations from country mean of non-employment rate of the 15-64 year olds in 2010.

Figure 3.6: Non-Employment and Value Added



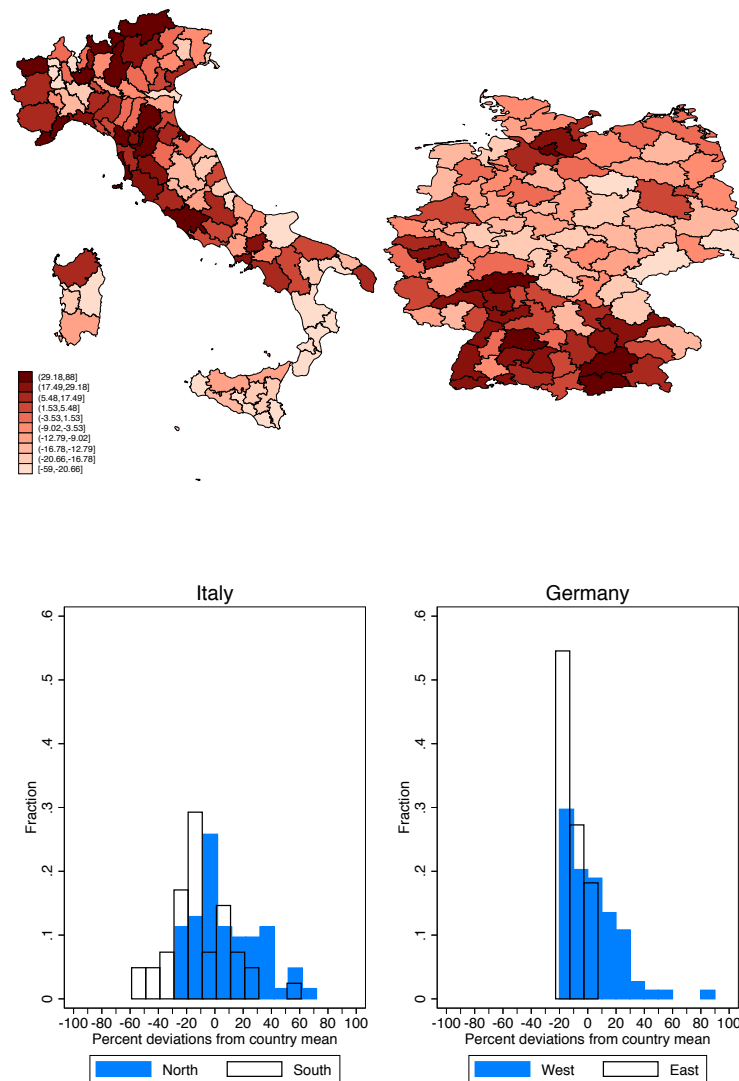
Note: This figure shows the relationship between the log non-employment rate among the 15-64 year olds and log value-added across provinces in 2010.

Figure 3.7: Non-Employment Rate - Corrected for Informal Work



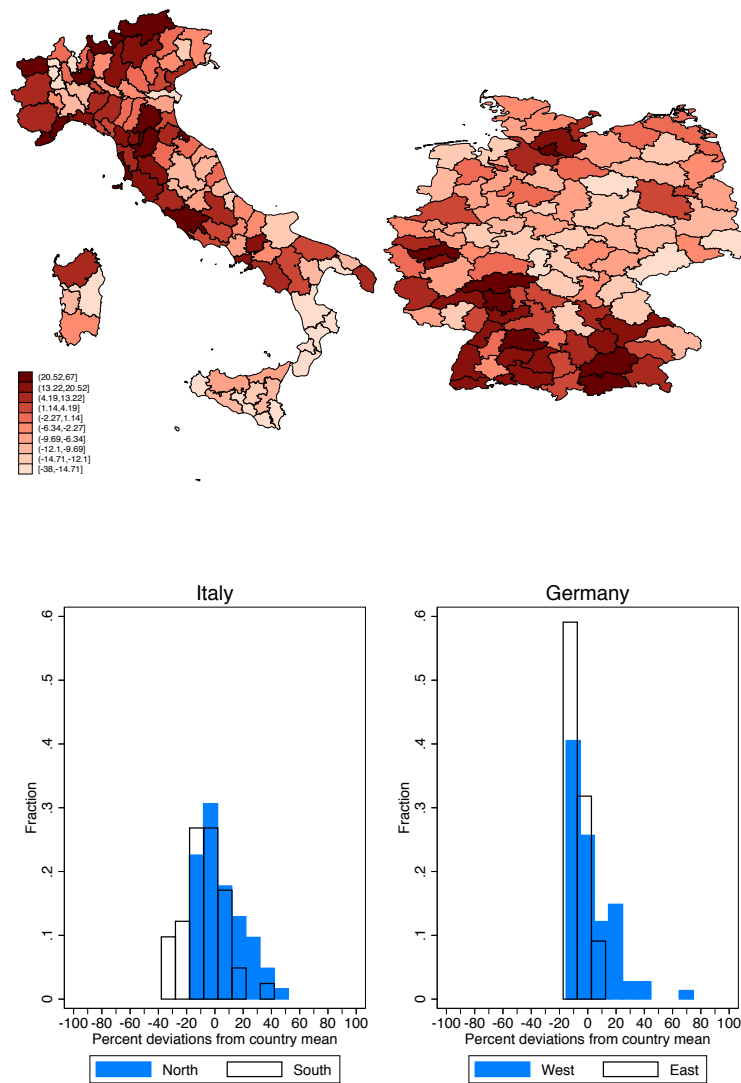
Note: To account for employment in the informal sector in Italy we compute an "informal labour market - corrected" employment rate by adjusting our employment rate to the fact that we only observe proportion $1 - e_{inf}$ (1-rate of informal employment) of actual employment. We thus inflate the official employment rate by a factor $\frac{1}{1 - e_{inf}}$. By computing real expected income on the basis of this measure we implicitly assume that expected wages in the informal sector do not differ from those in the formal sector. Map above expresses deviations from country mean of the non-employment rate among the 15-64 year olds of a local area in the year 2010 - uncorrected and corrected. Means are not weighted by population.

Figure 3.8: Housing Costs



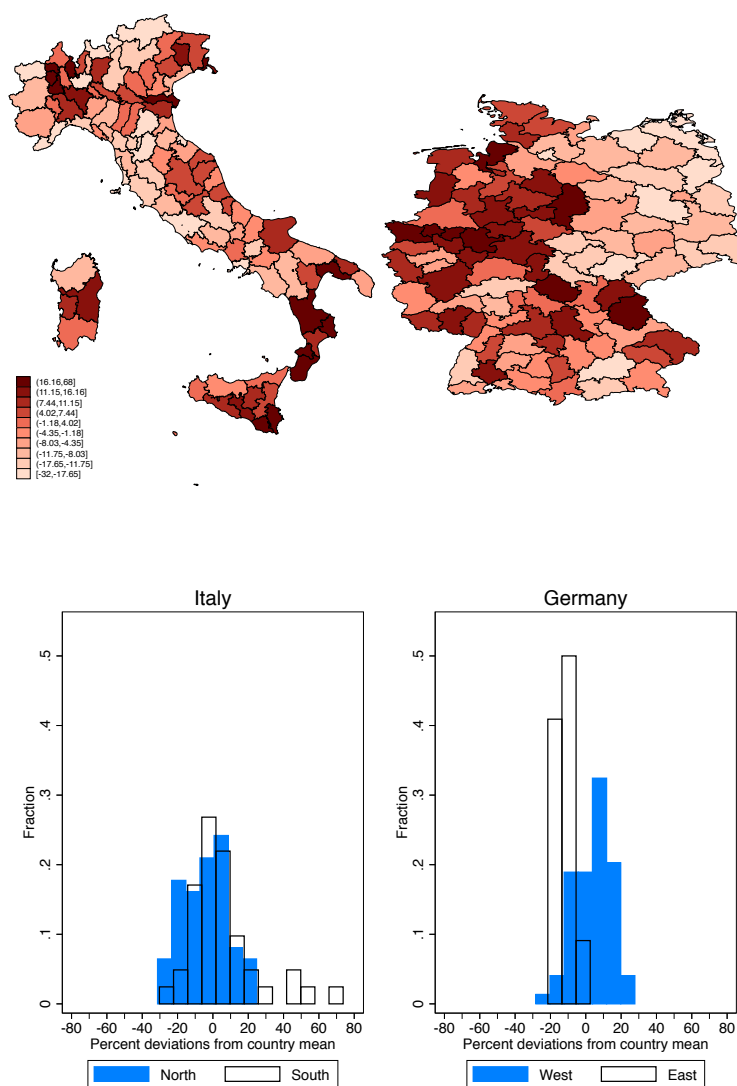
Note: Deviations from country mean of the housing price index in 2010. Means are not weighted by population.

Figure 3.9: Local Cost of Living Indexes



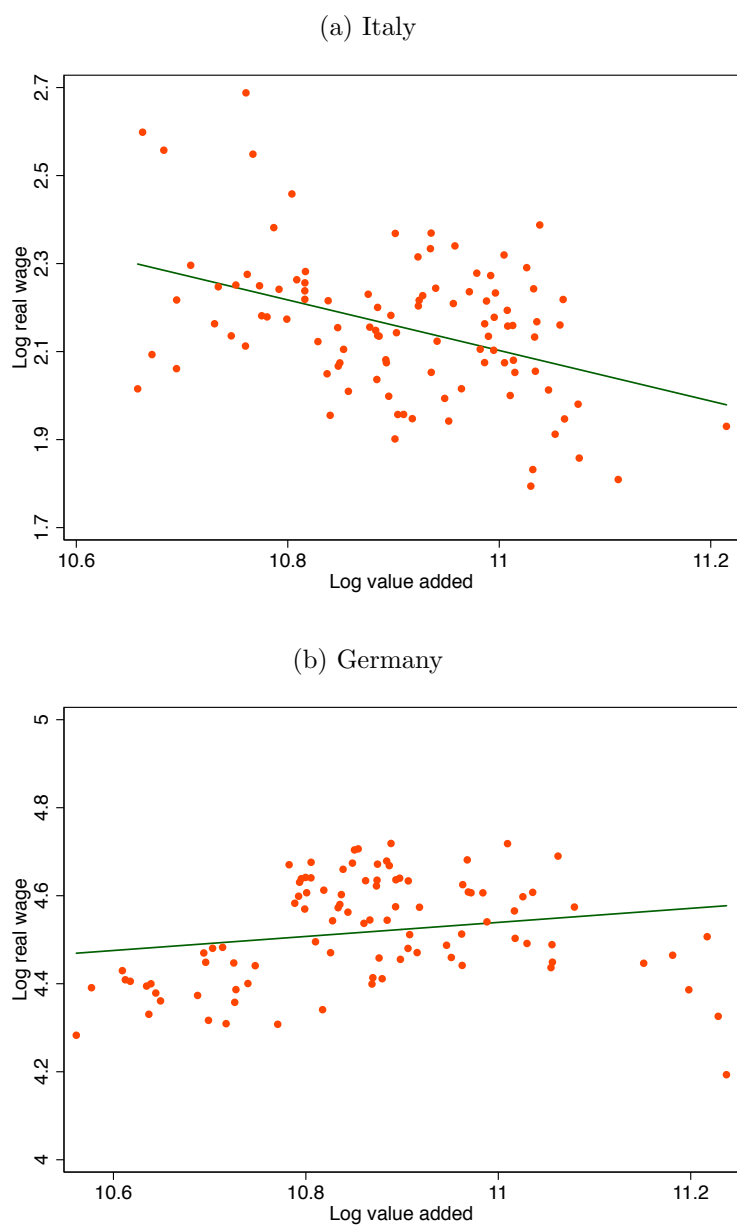
Note: Deviations from country mean of the consumer price index (as constructed by the method described in section 3.4 in 2010. Means are not weighted by population.

Figure 3.10: Real Wages



Note: Deviations from country mean of the real wages in euros (nominal wages adjusted for consumer prices) in 2010. Means are not weighted by population.

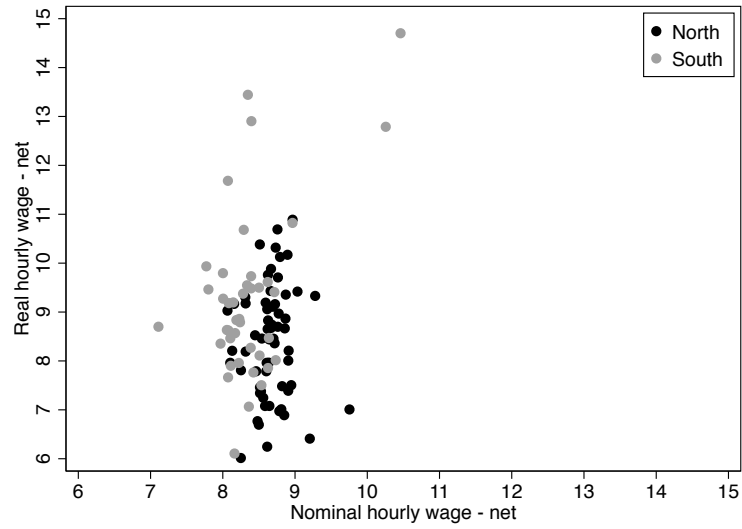
Figure 3.11: Real Wage and Value Added



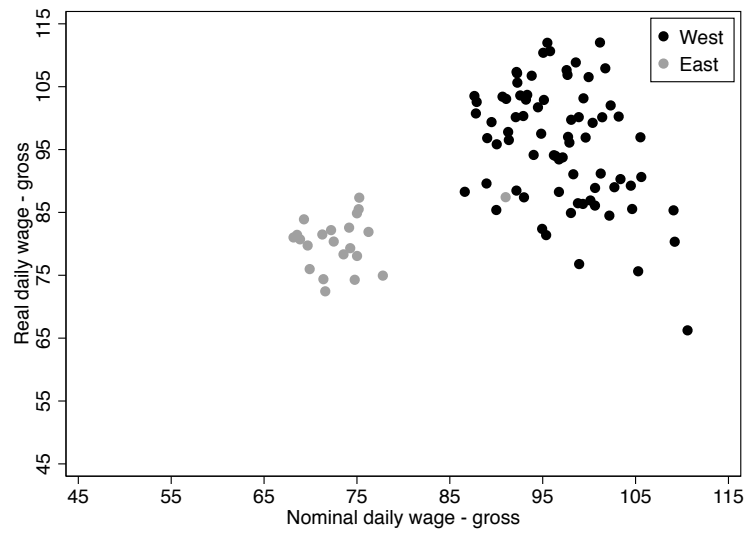
Note: Scatter plots above show the relationship between log real wages and log value-added of provinces in 2010.

Figure 3.12: Nominal and Real Wages

(a) Italy

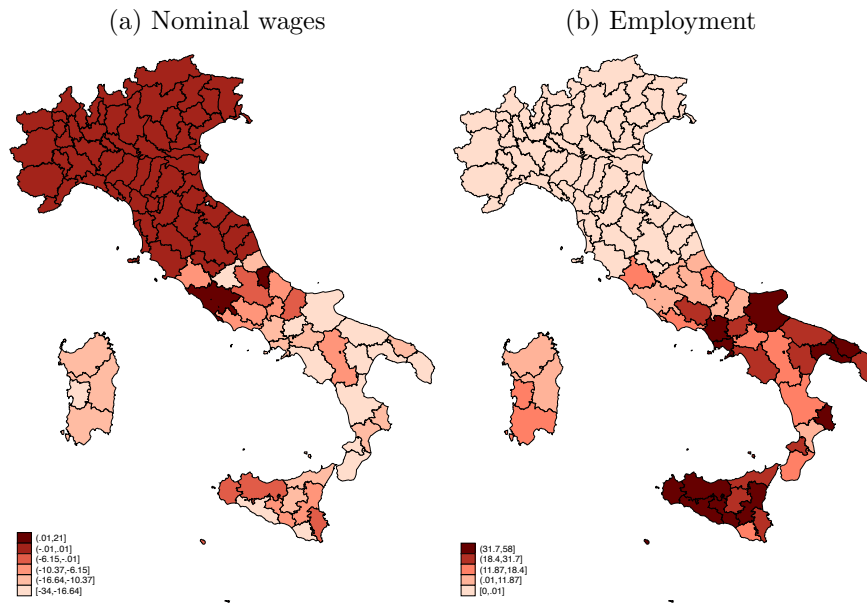


(b) Germany



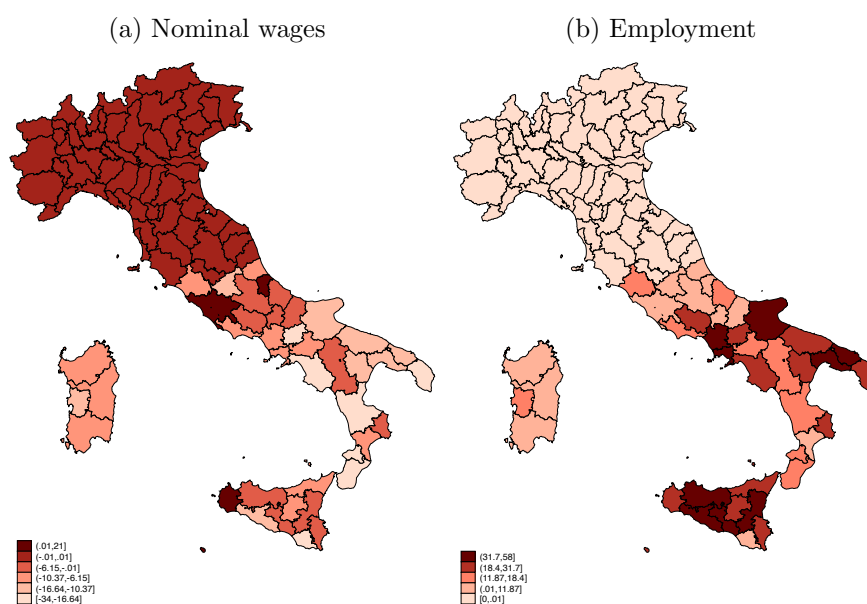
Note: Scatter plots above show the relationship of nominal and real wages across local areas in the year 2010. It shows that in Germany there is a strong dispersion of nominal wages while they are very compressed in Italy (same scale not possible because of hourly/daily wages). In Germany the correlation between real and nominal wages is visible. In Italy southern real wages differ considerably but they favour the south.

Figure 3.13: Changes in Counterfactuals for Scenario 1



Note: This map shows the geographic dispersion of the percentage changes from status quo to counterfactual wage and employment among the 15-64 year olds. The counterfactual in Scenario 1 reflects a reduction of wages by the percent difference in GVA to the median of the most productive provinces (in this case all the Northern provinces) and an employment rate equal to the one in the most productive of all provinces. The employment rate is corrected for informal work as described in section 3.5.3. Note that wages and employment in the North are not adjusted.

Figure 3.14: Changes in Counterfactuals for Scenario 2



Note: This map shows the geographic dispersion of the percentage changes from status quo to counterfactual wage and employment among the 15-64 year olds. Scenario 2 adjusts wages according to the relationship between productivity and wages in Germany as well as employment according to the relationship between productivity and employment in Germany. The employment rate is corrected for informal work as described in section 3.5.3. Note that wages and employment in the North are not adjusted.