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Essays on Labor Markets and Institutions

Tania Fernández Navia

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Pra miña avoa, Segunda
A muller máis traballadora que coñezo.
Pra miña nai, Teresa
Unha alma chea de bondade.
Pra miña madriña, Carmiña
A muller máis forte e loitadora que hai.
Sodes a verdadeira inspiración da miña vida.

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Chapter 1

Introduction

This thesis consists of three empirical essays studying the relationship between different institutions and two relevant mechanisms for a well-functioning labor market −geographical mobility and hiring processes− in the context of Spain.

The Spanish unemployment rate has been persistently high and unevenly distributed across regions.[1] An efficient mechanism to alleviate such regional disparities is geographical mobility (Blanchard et al., 1992; Dustmann and Preston, 2019). The underlying reason is that workers relocate from depressed areas to those regions with better employment possibilities. Given the large and persistent differences in unemployment rates across Spanish regions, the low mobility rates within the country are puzzling (Bentolila, 1997; Overman and Puga, 2002; Caliendo, Künn and Mahlstedt, 2017). The second and the third chapters of this thesis study how different institutions −labor protection, and the family− affect the mobility decisions within Spain.

The second chapter of the thesis, *Unemployment Insurance and Geographical Mobility: Evidence from a Quasi-Natural Experiment*, looks at the causal effect of the unemployment insurance (UI) generosity on the mobility decisions of unemployed workers. From a theoretical perspective, the UI could both increase or decrease migration. On the one hand, generous UI reduces the opportunity costs of unemployment. In this line, it could decrease job seekers' mobility through a reduction in their job search effort (Mortensen, 1977). On the other hand, generous UI increases reservation wages and reduces liquidity constraints. Thus, a more generous UI may enhance geographical mobility via increases in the job search productivity of unemployed workers (Marimon and Zilibotti, 1999). From an empirical viewpoint, the evidence on how the generosity of UI affects mobility is very scarce and mixed (Nekoei and Weber,

[1] For example, in the first quarter of 2020, the unemployment rate of Badajoz was almost 20 percentage points higher than the one in Álava.

2017).

To study the causal effect of the UI generosity on mobility, the second chapter of this thesis exploits a sudden and unanticipated reform that cut the Spanish UI benefits in 2012. In particular, on July 11, 2012, the Spanish government announced that all workers who started an unemployment spell after July 14, 2012, would have a ten percentage points reduction in their UI after the sixth month of unemployment. For the average worker in the sample, this represents a monthly income loss of €140, 17 percent of their pre-reform pay. Using administrative data from the Social Security records representative of UI recipients and a regression discontinuity design, I compare the mobility decisions of workers who became unemployed just before and just after the policy implementation.

The results show that the ten percentage points cut in the UI generosity increased workers' mobility by four percentage points (24 percent of the pre-reform mean). This effect is driven by young educated men with no family responsibilities moving towards the big cities. Besides, the paper shows that these movements mainly happen in the first six months of unemployment. This suggests that individuals anticipate the effects of the reform and intensify the job search from the beginning of the unemployment spell, rather than waiting until their unemployment benefits drop. These findings are robust to a wide variety of specifications and alternative measures of mobility.

Apart from contributing to the very scarce literature studying the causal effect of UI changes on mobility, this chapter complements previous work by Rebollo-Sanz and Rodríguez-Planas (2018). They show that the ten p.p. drop in the UI decreased the mean expected unemployment duration by 5.7 weeks, without affecting the job match quality. The second chapter of this thesis finds that geographical mobility may be an important mechanism explaining the findings in Rebollo-Sanz and Rodríguez-Planas (2018). Using a propensity score matching, I find *suggestive* evidence that movers affected by the reform find jobs two months earlier than they would have if they had not moved. I also show that the reform remarkably reduced the incidence of long term unemployment.

The findings in this chapter are consistent with the view that generous UI benefits represent important frictions to labor market adjustments (Bertola and Ichino, 1995; Hassler et al., 2005). This has relevant policy implications. As Chichester (2005) explains in an OECD policy paper, albeit promoting mobility is not an end in itself, it is key to reduce the barriers to internal mobility in countries with important asymmetries across local labor markets. The findings of this chapter show that changes in the UI design may encourage the active search for jobs through, among other mechanisms, geographical mobility. This means that people seem to react to the breaks in unemployment benefits. In this line, front-loading the payment of unemployment benefits (i.e., higher replacement rates at the beginning of the unemployment spell, decreasing steeply over time) seems a plausible policy to reduce the moral hazard problems linked to UI. A potential follow-up of this work is the evaluation of a sudden reform front-loading the unemployment benefits in Hungary in 2005 (Lindner and Reizer, 2016). This reform provides a similar policy change, but in a context of economic growth.

The third chapter of this thesis, *Family Types and Migration: Evidence from Spain*, studies how historical institutions affect geographical mobility. In particular, it asks if the family organization in the past still shapes today's migration decisions. The main hypothesis is that different family types create distinct levels of family ties that may persist even after the abolition of the original family structures (Farre and Vella, 2013; Alesina et al., 2015). Assuming this institutional stability, people born in societies where the family structure in the past favored strong ties will still face higher mobility costs as a response to the intrinsic utility they obtain from living nearby their families. Thus, assuming everything else equal, people born in provinces where the historical family organization promoted strong family ties will be less mobile.

Historically, in Spain, there were two main family organizations: stem and egalitarian nuclear. In stem families, several generations shared the household. In egalitarian nuclear societies, children left the family house at adulthood. Apart from differences in the co-residence patterns, these two family types also differed in the inheritance system. In stem families, one child (usually the first-born son) inherited all the family wealth, while in egalitarian nuclear societies, the family wealth was equally distributed among all children. According to previous research, family ties were stronger in societies in which stem families were socially predominant (Salamon, 1982; Bras and Van Tilburg, 2007).

To measure past family structures, I use the number of married and widowed women per household in 1860 (Tur-Prats, 2019).[2] Albeit 1860 is just a point in time, Reher (1998) and Todd (1990) show that family types were very stable since the Middle Age to the second half of the XIX century in Europe and Spain. To account for mobility, I use administrative data from Social Security records. I define mobility as changes in the province of relation with the social security between two consecutive years.

The results from estimating a Linear Probability Model (LPM) show that

[2]Higher numbers are associated to stem families, as several generations lived together in the stem organization system.

going from the province where the egalitarian nuclear family was the most socially predominant (Ourense) to the province with the most stem family structure (Huesca) is associated to a decrease in mobility by 1.28 percentage points. This magnitude represents 42 percent of the sample mobility average.

A potential concern with the previous specification is that it could be that societies with worse attitudes towards migration established stem families to keep the family joint and close. To avoid further problems of reverse causality and/or omitted variables, I follow Tur-Prats (2019) and use the inheritance laws that originated in the Reconquista as an instrument in a Two Stages Least Squares (2SLS) estimation framework. The Instrumental Variable approach leads to similar results than the LPM estimates. This is, people born in provinces with historically predominant stem families are less mobile today. These results are robust to different specifications and alternative definitions of the dependant and the key predictor variables.

To explore the potential mechanisms explaining the previous findings, I use the 2014 survey on *opiniones y actitudes hacia la familia*, from the *Centro de Investigaciones Sociológicas*. Using an LPM, I find suggestive evidence showing that people living in provinces where stem families were socially predominant in the past give more importance to the family and to family responsibilities.

Albeit more research in this line is needed, the results in this chapter suggest that cultural norms are long-lasting, and even in a within-country scenario, these persistent differences may create relevant distortions in the response of centralized policies (e.g., income tax deductions for mobility). In the future steps of this project, I will use Europe as the scenario to carry out this analysis, exploiting more variation in the distribution of the family organization.

Another relevant topic in labor economics is how to optimize the match between employees and employers. According to Hoffman, Kahn and Li (2018), hiring the adequate labor force is among the most important and challenging issues a firm faces. Indeed, recruiting the best possible human capital is key to ensure the firm's outcomes and the economic growth (Huber, Lindenthal and Waldinger, 2018; Heinz et al., 2017; Hoffman, Kahn and Li, 2018). This can be even more crucial in Spain, a country with labor productivity well below the average of the European Union.

The fourth chapter of this thesis, *Cognitive Biases in Selection Processes: Evidence from a Natural Randomized Experiment*, analyzes the presence of cognitive biases in a hiring process for teaching positions. Namely, this joint project with Miquel Serra-Burriel, Jordi Teixidó, and Marc-Lluís Vives, studies how the order in which candidates do a job interview affects their probabilities of passing to the next stages of a recruitment process used to hire permanent teachers in Spain.

We use information from the competitive exams held in Catalonia in 2019 to recruit 5,005 permanent teachers. In this recruitment process, applicants had to pass an oral exam and two written tests. In the oral exam, candidates had 45 minutes to present a syllabus for an academic year and to deliver a unit-plan of their choice. After this presentation, the academic board (composed of five members) evaluated the candidate's performance. Only those who obtained more than 5.0 out of 10.0 in this test could continue with the recruitment process.

To study the causal effect of order on assessment, this chapter uses administrative data on the universe of candidates who enrolled to participate in the recruitment process. Exploiting the random order of presentation, we find that those candidates who were arbitrarily assigned to do the exam in the first position obtained 0.17 points more (3 percent of the mean) and were 3.3 percentage points more likely to pass the exam (5.3 percent of the average success rate). These results are robust to different specifications, and they are not caused by differences in candidates' abilities over the sequence. Albeit, to the best of our knowledge, this is the first study looking at sequential effects in the context of a recruitment process, these findings are consistent with previous literature showing the existence of primacy effects in judicial proceedings (Danziger, Levav and Avnaim-Pesso, 2011) or citation behavior (Feenberg et al., 2017).

After finding evidence of sequential effects, we look at the potential mechanisms. We find suggestive evidence consistent with contrast effects, narrow bracketing, and generosity erosion.

Consistent with contrast effects (Bhargava and Fisman, 2014), we show that the performance of the previous candidates inversely affects the next candidate's evaluation and that the very previous candidate creates the strongest influence. In addition, our data shows the importance of narrow bracketing on jury evaluations. This is, we find that the higher the number of candidates who have already passed, the lower the probability the following candidates have to pass the exam. According to Simonsohn and Gino (2013), this may happen because the evaluators do not want to deviate from the expected results within each bracket. In other words, if they can pass 20 percent of the total candidates, they may avoid to deviate from this 20 percent in any given bracket or day. Finally, we check how a jury being lenient may impact the next candidates' assessment. This idea comes from the dictator game, in which players become less generous as the sequence unfolds (Bó, 2005; Engel, 2011). In our context, we consider that giving a candidate an exact 5.0 (the minimum grade so that he/she can continue the recruitment process) is an act of generosity. We find that the larger the number of previous candidates who obtained an exact 5.0, the lower the probability of the next candidates to pass the exam. We call this the generosity-erosion principle.

The findings in this chapter have relevant policy implications. First, they show that arguably irrelevant factors such as candidate's sorting and ordering can have significant consequences on their future labor market careers. Given that this recruitment process is standard for public servants and high-skilled private-sector jobs in different countries, these results cast serious doubts about the efficiency and fairness of different hiring methods. In this line, it is very important to create neutral recruitment processes. Previous work by Autor and Scarborough (2008), Hoffman, Kahn and Li (2018), or Berson, Laouénan and Valat (2020) show that algorithm-based job testing technologies may be helpful to reduce or correct human subjective biases.

We also try to deepen into the mechanisms that explain the sequential penalty. This is a challenging task and we are aware of its limitations and problems. Yet, we are able to show some evidence pointing towards different explanations (i.e., contrast effects, narrow bracketing, and generosity erosion). Following the work by Alesina et al. (2018), we think that informing decision-makers about the possibility that these cognitive biases are impacting their assessments may help to prevent and reduce their effect.

Finally, the fifth and last chapter summarizes the main results, discusses the policy implications of the findings in the different chapters of this thesis, and provides some proposals for further research.

Chapter 2

Unemployment Insurance and Geographical Mobility: Evidence from a Quasi-Natural Experiment

2.1 Introduction

Labor mobility is an efficient mechanism to alleviate regional disparities in economic outcomes (Blanchard et al., 1992). Therefore, it is puzzling that despite significant and persistent spatial differences in unemployment rates, only about 3.9 percent of European workers live in a country different from that of birth (Overman and Puga, 2002; Kline and Moretti, 2014; Caliendo, Künn and Mahlstedt, 2017). Rigid labor market institutions, such as generous unemployment insurance (UI), may contribute to explain such low mobility rates (Nickell, 1997; Bertola, 1999; Hassler et al., 2005).

But, does the generosity of the UI deter labor mobility? From a theoretical point of view, the impact of the UI benefit on labor relocation is ambiguous. On the one hand, generous UI lowers the opportunity costs of unemployment. This may reduce geographical mobility through a decrease in job search effort (Mortensen, 1977). On the other, generous UI increases reservation wages and reduces liquidity constraints. This can enhance relocation through rises in the productivity of job search (Ben-Horim and Zuckerman, 1987; Marimon and Zilibotti, 1999).

This chapter estimates the causal effect of reducing the UI benefit amount on mobility decisions using quasi-experimental evidence from a recent reform in Spain. Namely, it exploits a sudden and unanticipated ten percentage points (p.p.) drop in the UI replacement rate (RR) that affected workers who started an unemployment spell after July 14, 2012. Before the reform, the RR was 70 percent of previous earnings during the first six months of unemployment, and 60 percent afterward (up to a maximum of two years). The reform reduced the RR to 50 percent after the sixth month of unemployment.[1] This unanticipated change offers an excellent opportunity to causally identify the effect of labor market rigidities on workers' mobility decisions.

The Spanish context also represents an interesting case study. The Spanish unemployment rate has been persistently high and very responsive to economic fluctuations.[2] There are also large regional variations in the unemployment rates.[3] Despite the persistent disparities (see figure A.1b), internal relocation is low. In this scenario, it is important to understand whether the generosity of the Spanish welfare system (in particular, the generosity of the UI) deters workers' spatial mobility and contributes to the persistence of regional economic asymmetries (Jimeno and Bentolila, 1998).

To empirically assess the impact of the UI generosity on workers' mobility, I rely on administrative data from the Social Security records (Muestra Continua de Vidas Laborales, MCVL). The MCVL contains a 4 percent random sample of the population (around one million individuals), and it is representative of UI recipients. Using a regression discontinuity (RD) design, I compare the mobility decisions of workers who started their unemployment spells around the reform date.[4]

The results show that the UI cut increased workers' relocation across provinces by 4 percentage points (24 percent of the pre-reform mean).[5] The heterogeneity analysis indicates that this result is mostly driven by movements towards big cities of young educated men without family responsibilities. Furthermore, I find that these results are not driven by an increase in commuting but by more people changing province.

[1] Notice that the policy did not affect the RR during the first six months of unemployment, which remained at 70 percent.

[2] Over the course of the Great Recession, the unemployment rate dramatically increased from 8.23 percent in 2007 to 26.09 percent in 2013.

[3] Figure A.1a shows an unequal distribution of the local unemployment rates in Spain. In 2017, there was a 20 percentage point difference between the unemployment rate of Cádiz and Gipuzkoa, the two Spanish provinces with the highest and lowest unemployment.

[4] To identify the causal effect of the reform, workers who became displaced around mid-July must be comparable. Section 2.5.1 verifies this assumption by showing that the number of displaced employees is smooth at the discontinuity and that workers' features are balanced at baseline.

[5] Spain consists of 52 provinces. The average population by province is 0.9 million inhabitants.

This chapter contributes to the scarce and mixed empirical evidence on the relationship between the UI generosity and mobility. In their model, Hassler et al. (2005) show that the significant disparities in the generosity of unemployment benefits between Europe and the U.S. account for the different rates in geographical mobility. Looking at European countries, Tatsiramos (2009) finds that among unemployed workers, UI recipients exhibit higher mobility rates than those non-entitled to UI in France, Denmark, and Spain. A closely related paper is Nekoei and Weber (2017). To the best of my knowledge, they present the only other study that identifies the causal effect of the UI generosity on mobility using administrative data and a quasi-natural experiment. In particular, they exploit a reform in Austria that increased the UI duration by nine weeks for workers older than 40 years at displacement. Using an RD design, they estimate a very precise zero effect of the UI extension on regional mobility.[6]

There have also been several previous studies examining the relationship between unemployment benefits and labor mobility in Spain. Antolin and Bover (1997) or Bentolila (1997) suggest that institutional factors such as the duration and coverage of unemployment benefits have a negative impact on inter and intraregional mobility. Jofre-Monseny (2014) analyses the effects of the introduction of an agricultural unemployment assistance program in two lagging regions in Spain. Using a border identification strategy, he finds a substantial decrease in out-migration and an increase in in-migration in the affected areas. De la Roca (2017) estimates a single-exit duration model and shows that unemployed workers' propensity to move jumps when workers exhaust their unemployment benefits.

This study also contributes to the understanding of the relation between the UI generosity and the unemployment duration.[7] In a recent paper, Rebollo-Sanz and Rodríguez-Planas (2018) study the effects of the 10 p.p. reduction in the RR on unemployment duration. They find that the UI cut decreased the

[6] There are several dimensions that separate Nekoei and Weber (2017) paper from the present study: First, the Spanish reform affected at the UI benefit level, rather than at the UI duration. According to Schmieder and Von Wachter (2016), agents react more to the former than the latter. Second, the distribution of unemployment rates across regions is more uniform in Austria than in Spain. Thus, geographical mobility may be less important as a job search mechanism in Austria. Finally, Nekoei and Weber (2017) study the behavioral responses in terms of mobility (among other outcomes) of people who are around 40 years old. For this subgroup of the population, I do not find any effect either. This result goes in line with the migration literature, which shows that younger workers are more prone to relocate geographically.

[7] The fact that generous UI increase unemployment duration is one of the most robust findings in the economic literature (Nekoei and Weber, 2017).

expected nonemployment length by 5.7 weeks (or 14 percent), without affecting the job match quality. The results in this study suggest that geographical mobility can be a potential mechanism contributing to shortening the length of the unemployment spells. Indeed, using a propensity score matching, I find suggestive evidence showing that those unemployed workers who moved find jobs around two months earlier than they would have if they had not moved.

The remainder of the chapter proceeds as follows. Section 2 provides institutional details on the Spanish unemployment benefit system and the reform. Section 3 describes the data used in this paper and the constraints imposed on the original sample. Section 4 outlines the empirical strategy used to identify the effect of interest. Section 5 discusses the results of the econometric analysis and presents some robustness checks. Section 6 concludes.

2.2 Institutional Setting

2.2.1 The UI System in Spain

To be eligible for UI in Spain, individuals need to have worked for at least 360 days in the six years before involuntary lose their jobs.[8][9] For entitled unemployed workers, the UI duration ranges from 120 to 720 days, depending on the length of the previous contribution periods in employment (details in figure A.2).

The UI benefit amount results from multiplying the RR −which is time-variant− by the average gross salary in the 180 working-days preceding the unemployment spell. However, this amount is censored to a floor and a ceiling that depends on the Monthly Public Income Index (IPREM) and the family circumstances (see figure 2.1).

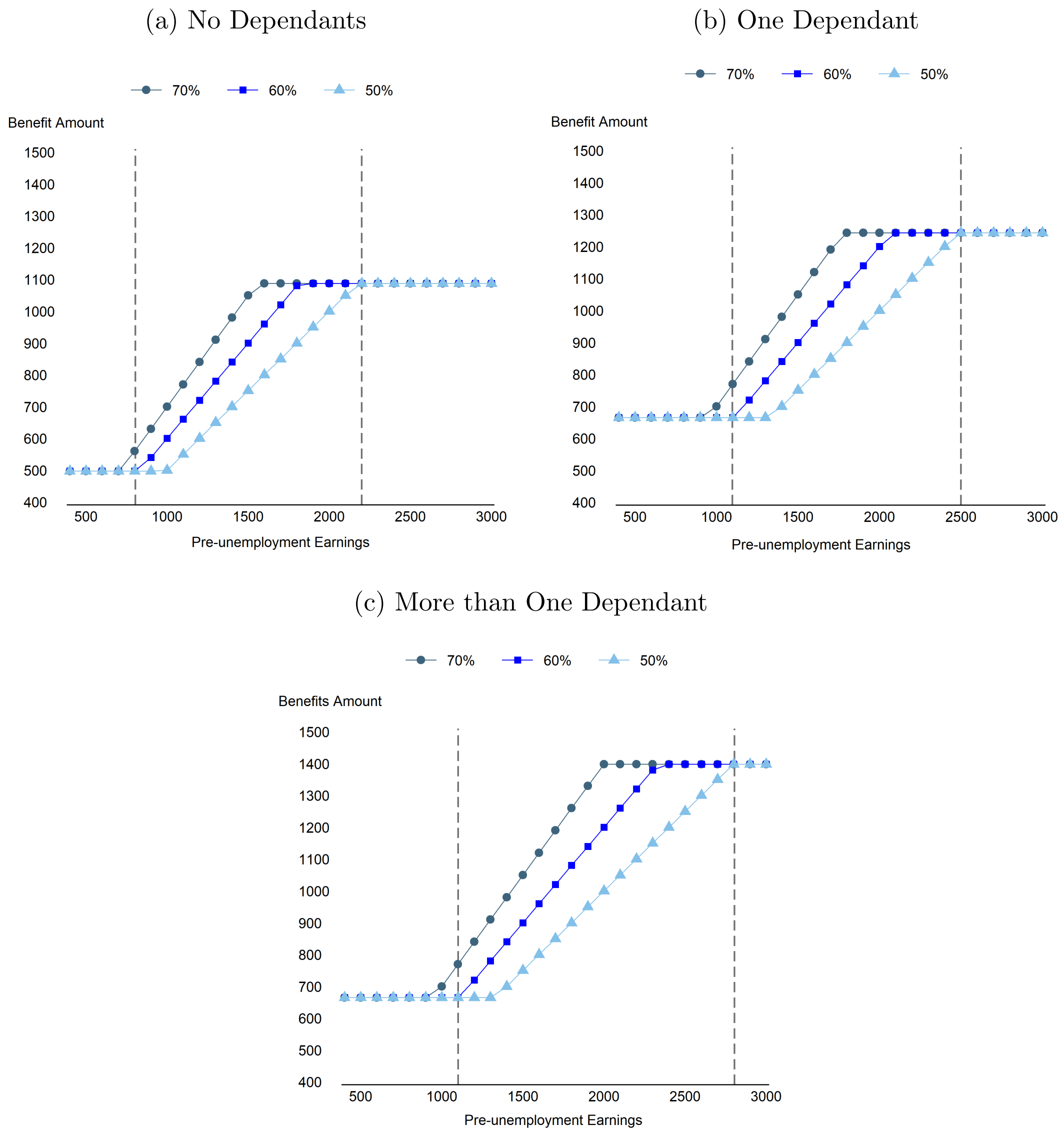
The UI benefit duration in Spain is larger than the average in the OECD countries or the EU-28, while the UI replacement rate is similar (Esser et al., 2013).

[8]If the worker has received another unemployment benefit (UB) during these six years, the period that is considered for the computation of the new UI is the one that elapses between the last day the worker has received the previous unemployment benefit and today's new request for UI. In addition, UI beneficiaries who take a new job before exhausting their previous UI and then return to the unemployment can choose between renewing the original entitlement for the remaining length of time *(option right)* or receiving a benefit based on the new contributions. If the worker chooses to recover the previous UI, the contributions that led to the new benefit will be lost.

[9]Formally, workers are also required to search actively for jobs and to not refuse *adequate* job offers. However, anecdotal evidence suggests that this requirement is not enforced in reality.

Claimants who have not accumulated enough contributions to be eligible for UI or who have exhausted their entitlement can apply for unemployment assistance (UA). The eligibility and duration depend on the length of the previous contributions and family responsibilities. The UA benefit amount has no relation with the previous earnings, and it amounts to 80 percent of the IPREM.

Figure 2.1: UI Benefit Amount



Note: Figure 2.1 shows the UI benefit amount for displaced workers with different family circumstances. The "70%-line" corresponds to the UI that unemployed workers receive in the first six months of unemployment. The "50%-line" ("60%-line") corresponds to the UI in the remaining of the unemployment spell for workers who become unemployed after July 14, 2012 (until July 14, 2012). From 2010 to 2016, the minimum UI benefit amount was €497.01 (€664.75) for workers with no (with) dependants in charge. The maximum benefit amount for individuals with no dependants was €1,087.20. For workers with one dependant in charge, the benefit amount could not exceed €1,242.52. Finally, the maximum UI benefit for workers with more than one dependant was €1,397.84.

2.2.2 The Reform in the UI RR

On 11 July 2012, the former president of the Spanish conservative government, Mariano Rajoy, reported a package of austerity measures aimed at reducing the fiscal deficit in Spain.[10] One of the most unpopular announcements was the reduction of the UI benefit replacement rate. The main purpose of such reform was to encourage the active search of employment. Despite the social discontent, the announcement become law on July 13, 2012 (Law 20/2012).[11]

Before the policy implementation, the RR was 70 percent during the first six months of unemployment, and 60 percent afterward. The reform reduced the RR from 60 to 50 percent −a 16.66 percent− after the sixth month of unemployment for workers who started their unemployment spells after July 14, 2012. Figure A.3 shows that, on average, a job seeker affected by the reform received €140 less per month than a comparable person who was not affected. In terms of magnitude, this income loss represents more than the average monthly expenditure per person in food and non-alcoholic beverages in 2012.

The policy was sudden and unanticipated. The announcement of the reform happened two days before its approval, and four days before its implementation.[12] In addition, the reduction in the benefit replacement rate was introduced in the aftermath of the economic crisis, with a large and increasing unemployment rate and a negative GDP growth. The week after the approval of the law, there were important demonstrations against the cuts in different cities of the Spanish geography (see El País).[13] Nonetheless, the UI RR design is still regulated by Law 20/2012.

[10]There was another policy in February 2012 that affected collective bargaining agreements at the firm level and decreased the dismissal costs for permanent workers. As I consider inflows into unemployment from January 2011 to December 2013, the February 2012 reform affects some individuals in the sample. However, the results in this chapter are robust to use workers dismissed around July, and therefore, who are not differently affected by the policy change in February.

[11]The approval of this reform was via a *law-decree*. A law-decree is a form of legislation limited to cases of extraordinary and urgent need. This type of law can be effective the following day after its publication in the State Official Bulletin (BOE). Within the following 30 days, the law-decree needs the approval of the Congress and Senate. In this case, the law was published in the BOE on July 13, 2012. Regarding the need for approval, the conservative party led by Mariano Rajoy had an absolute majority in both chambers in 2012.

[12]Figure A.4a shows the popularity of the terms *prestación de desempleo (UI) and recortes paro (UI cuts)* in Google Trends in Spain during the year 2012. The figure shows a jump on the popularity of both terms during the week this policy was announced, approved, and implemented. Figure A.4b provides an intuition on how relevant the policy change was.

[13]See https://elpais.com/politica/2012/07/19/actualidad/1342711453_843667.html

2.3 Data

This study uses the Continuous Sample of Working Histories (*Muestra Continua de Vidas Laborales* or MCVL). The MCVL is a microlevel data set provided by the Spanish Ministry of Employment and Social Security since 2004. It is based on administrative records compiled from social security, income tax, and census registers. Each annual wave contains a 4 percent non-stratified random sample of all individuals who have any contact with the Social Security Administration (including both workers and recipients of contributory pensions −such as unemployment insurance−) during at least one day in the sampled year.

The MCVL has a longitudinal design, meaning that if a person is selected in a given wave and remains in contact with the Social Security Administration, such a person continues as a sample member in the subsequent editions.[14] The data also contain complete employment histories for each individual back to the moment they have entered the labor market (or 1967 for earlier entrants).

For each employment spell, the data include its exact start and end dates, the type of contract (fixed-term or permanent; part-time or full-time), the social security contribution group (a proxy for occupation) and regime, an anonymized employer identifier, the type of firm (public or private), and its location, as well as monthly earnings.[15] When the relationship with the firm ends, there is information on whether it is a voluntary or involuntary termination. The MCVL also includes personal characteristics such as age, gender, nationality, province of birth, educational attainment, and individuals' household composition.

The Estimation Sample

The main analysis uses waves 2011-2017 and limits the sample to workers who start receiving UI at some point between 2011-2013.[16] In addition, I exclude workers whose benefit amount does not drop after the reform because (1) they

[14]The requirement for inclusion in the MCVL is constant over the years. The difference across editions is that the most recent ones include individuals who enter the labor force for the first time to compensate for those who disappear from the sample (mostly those who die or leave Spain).

[15]The dataset provides information on the firm location at the municipal level for those municipalities with more than 40,000 inhabitants. For municipalities with less than 40,000 inhabitants, there is information on the firm location at the provincial level.

[16]I limit the sample to workers who start receiving UI around the date of the reform. Some individuals (27.99 percent of the restricted sample) start receiving UI both before and after the policy implementation. Thus, they could make both the treatment and control groups. For those workers with several spells receiving UI, I consider the first as the relevant one.

are entitled to receive UI for less than 181 days, or (2) their benefit amount is above the maximum or below the minimum under both replacement rates (see the dashed grey lines in figure 2.1). I also exclude those workers who are potentially using the *option right*.[17] As in previous studies, I just consider individuals who are alive during the period of analysis, without disabilities, who are between 25 and 50 years old at the moment they start receiving UI, and who have been working in full-time jobs belonging to the general regime of the social security during the 180 working-days before displacement.[18][19] I also exclude workers who leave the sample while entitled to receive UI.[20] This group represents a 3.55 percent of the restricted sample. Albeit it is not a large amount, this may create some risks of sample selection, as the probability of leaving the sample is not equally distributed over observable characteristics. In particular, foreign-born workers are remarkably over-represented in this group.[21] Thus, I limit the analysis to workers with Spanish nationality who were born in Spain.

The MCVL has three characteristics that are key to this study. First, unemployment spells in which workers receive UI are clearly identified. Second, the longitudinal design of the data allows calculating the UI entitlement of each worker, both in terms of duration and benefit level. This permits to recognize those workers who are affected by the drop in the RR. Finally, there is information on the workplace location, which allows tracking individuals across space.

Descriptive Statistics

Table A.1 reports the summary statistics. The main outcome variable is the change of province. This variable indicates whether the individual has changed province during the period he/she was entitled to receive UI (the "baseline" province is where the individual was working before the displacement). Table A.1 shows that the average mobility among UI beneficiaries is 16 percent. It

[17]Those who use *option right* are recovering the unused UI from previous spells, which is not affected by the policy change.

[18]For very young individuals, there may be a problem of representativeness (see García-Pérez, Castelló and Marinescu (2016)). In addition, I limit the age to 50 years because there is a policy at the same time that changes the minimum age to be entitled to a particular retirement pension from 52 to 55 years.

[19]Wages and hours of work are not reliable in jobs that are not included in the general regime. In addition, those workers can have different rules regarding the UI.

[20]Those workers most likely have left Spain. My results are not sensitive to the inclusion or exclusion of this group.

[21]While immigrants represent less than 15 percent of the sample, 60 percent of the unemployed workers who leave the sample are immigrants.

is relevant to indicate that the changes of province do not necessarily imply changes in the province of residence. Instead, they reflect a change in the province where workers have their relationship with the social security administration −in terms of working, receiving unemployment benefits, or receiving a contributory pension−.[22]

Regarding the covariates included in the analysis, 60 percent of individuals in the sample are men. The average person is 36 years old and has no family responsibilities. 14.5 percent of them have tertiary education, and 29 percent have completed secondary education. The average years of experience are 13, and the average earnings during the year prior to the unemployment are €16,832.38.[23] Regarding the characteristics of their pre-displacement employment, 13 percent had a high-skilled occupation job, 22 (44) percent of workers had a medium-high (medium-low) skilled occupation, and 22 percent had a low skilled occupation. In addition, 68 percent of the sample had a permanent contract, and 93 percent worked in the private sector. The average unemployment in the province of last employment was 24 percent, and the average entitlement to UI was 20 months.

2.4 Methodology

To estimate the causal effect of the reduction in the unemployment insurance generosity on workers' mobility decisions, the empirical analysis uses an RD design. This approach exploits the sudden and unanticipated change in the UI replacement rate for those workers who start an unemployment spell after July 14, 2012.[24]

In the baseline specification, I estimate a local linear regression (Gelman and Imbens, 2018) of the form:

$$Y_i = \alpha + \beta T_i + \gamma_1 (c_i - c') + \theta X_i + \epsilon_i \tag{2.1}$$

where Y denotes the outcome variable for individual i. In the main specification, Y is an indicator variable that takes the value 1 if individual i changes

[22] One limitation of this information is that the researcher cannot observe if workers can work from outside the company location or are registered in the social security to gain unemployment benefits in a province different from their province of residence. Another limitation is that it is not possible to observe the job search mechanisms (i.e., I will not be able to distinguish those who moved because they have already found a job in another province from those who moved to look for a job in other geographical areas).

[23] Wages are deflated to 2009 Euro using the CPI.

[24] Notice that, in Spain, workers start receiving UI the same day their unemployment spell begins.

geographical location during the time the worker is entitled to receive UI, and 0 otherwise.[25] The treatment assignment T_i is a deterministic function of the day in which each worker starts the unemployment (c_i), and the cutoff date (c'). In particular, T_i is defined as follows: $T_i = \mathbb{1}\{c_i \geq c'\}$, where $\mathbb{1}\{\cdot\}$ is an indicator function that takes the value 1 if the worker i starts the unemployment spell from the cutoff date c' onwards, and 0 otherwise. In this scenario, the cutoff date is July 15, 2012.

The model also includes a linear trend ($c_i - c'$) that consists of the date each person starts the unemployment spell minus the cutoff date. X_i is a vector of predetermined observable characteristics. It includes a set of worker traits (e.g., gender, age, number of family members younger than 18 living in the household, three educational dummies (i.e., less than high school, high school, and college), years of experience in the labor market, and annual earnings in the year before the unemployment spell), and a set of employment pre-displacement characteristics (e.g., indicator variables for permanent or fixed-term type of contract, indicator variables for private or public sector job, fourteen industry dummies, and four skill dummies (i.e., high skill occupation, medium-high skill occupation, medium-low skill occupation, and low skill occupation)). I also control for the potential benefit duration, and for the unemployment rate in the province of last employment.[26] ϵ_i is the unobserved error term.

In the main specifications, I estimate equation 2.1 using a local linear regression with the MSE optimal bandwidth (Calonico, Cattaneo and Titiunik, 2014) and a triangular kernel density function (Porter, 2003).[27] Standard errors are clustered at the day of entry at the UI.[28] To assess robustness, I also consider alternative bandwidths and different orders of the polynomial in the running variable. The model is estimated with and without including control variables.

The main advantage of the RD design is that, as long as individuals do not have precise control on the day they become unemployed, the variation in the treatment is *as good as random* in a neighborhood around the discontinuity

[25] I estimate the effect of the reduction in the UI on mobility across provinces, urban areas, and regions *(CC.AA.)*.

[26] Albeit in the RD context conditioning for observable characteristics is not required for consistency, it improves precision.

[27] All results presented in Section 2.5. are robust to (1) the use of a CER optimal bandwidth instead of MSE optimal bandwidth, and (2) the use of a uniform rather than a triangular kernel density function.

[28] I cluster the standard errors at the day workers start receiving UI in order to account for potential correlation in the *day of entry* unobservable characteristics (Lee and Card, 2008).

threshold (Lee and Lemieux, 2010). In this scenario, the parameter of interest $-\beta-$ measures the causal impact of the reform. In Section 2.5.1., I provide evidence that the random assignment assumption is satisfied.

2.5 Results

As discussed, to estimate the causal effect of the UI generosity on workers' mobility decisions, I employ an RD approach.[29] This identification strategy relies on comparing the behavior of workers who start the unemployment spell around the date of the policy change. Before moving to the RD results, I present the standard validity checks (Cattaneo, Idrobo and Titiunik, 2018).

2.5.1 The Validity of the RD Approach

The main threat to the validity of the RD design is the possibility that workers or employers manipulate the date of the layoffs so that the UI spells start non randomly before or after the reform date. However, stratification is very unlikely in this case.

First, workers have no control over the timing of their dismissals.[30] Second, the reform was implemented four days after its announcement. This leaves no room for manipulation to employers, who are obliged to give a 15-day written notice to the employees who are being fired. Anyhow, I can empirically test the absence of manipulation around the cutoff looking at the density of observations and balance in covariates around the reform date.

Figure A.5 shows the number of UI entries before and after the UI benefit cut. There is no graphical evidence of manipulation on the timing of the layoffs around the cutoff date. This visual impression of continuity is supported by the results of implementing the density test of the running variable proposed by Cattaneo, Jansson and Ma (2019), which indicates that the discontinuity at the cutoff is equal to 0.1615 (p-value 0.8717).[31]

In order to test whether there is endogenous sorting around the threshold, I estimate equation 2.1 using as dependent variables the background covariates I include in the analysis as controls. The results (see table A.2) show that there are no systematic differences in any of the observable characteristics between

[29]Recall that Rebollo-Sanz and Rodríguez-Planas (2018) study the effect of this reform on the unemployment duration and job-match quality. A replication of their findings using the years I am considering in this chapter is available in the Appendix.

[30]Recall that workers who voluntarily quit their jobs are not eligible for UI.

[31]Figure A.5a also shows that the number of entries in the UI is noisy, with special peaks at the beginning of each month.

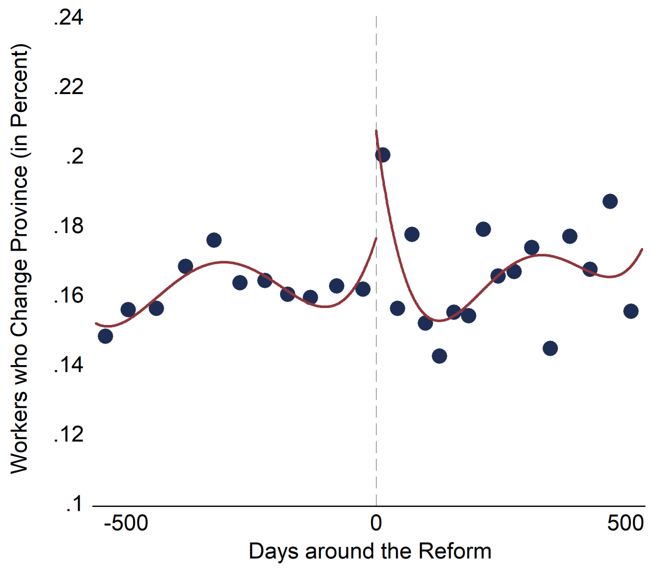
those workers who start an unemployment spell just before and just after July 15, 2012.

Overall, these checks support the validity of the RD approach.

2.5.2 Main Results

I begin with a graphical illustration of the research design. Figure 2.2 plots the proportion of workers who have changed province while being entitled to UI by day of entry in unemployment. The illustration reveals a positive jump in mobility at the discontinuity.

Figure 2.2: Graphical Illustration of the RD Design



Note: The figure plots the proportion of workers who have moved to a different province during the time they are entitled to receive UI (y-axis), and the day workers start receiving UI (x-axis). The dashed vertical line represents the day of the policy implementation. The solid lines represent the fitted values based on a fourth-order polynomial without covariates. The IMSE-optimal number of quantile-spaced bins is 11 bins below the cutoff and 16 above it. The average bin length is around 51 days below the cutoff 33 days above it. Within each bin, there are 1,414 people in the pre-reform part and 785 persons in the post-reform part.

Table 2.1 presents the results of estimating equation 2.1 using as the outcome a binary variable that takes the value 1 if a UI recipient has changed province while being entitled to UI, and 0 otherwise.

The first column in table 2.1 estimates β in equation 2.1 using a local linear approach with MSE-optimal bandwidth and without including controls. The RD estimate indicates that the reduction in the UI generosity increases mobility by 3.8 (≈ 4) percentage points (s.e. $= 0.019$) on average. In terms of magnitude, this represents a 24 percent increase with respect to the pre-reform mean. Columns 2 and 3 estimate equation 2.1 controlling for second and third-order polynomials in the running variable. The point estimates become slightly larger and remain statistically significant at the 5 percent level. The inclusion of control variables in columns 4, 5, and 6 does not change the results.[32]

Table 2.1: Effect of the Reform on Geographical Mobility

Outcome	Mobility across Provinces					
Bandwidth	MSE Optimal Bandwidth					
Days around the reform	111	175	212	109	171	209
Reform (T_i)	0.04**	0.05**	0.06**	0.04**	0.05**	0.06**
	[0.019]	[0.022]	[0.026]	[0.020]	[0.023]	[0.028]
Control Function	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
Covariates				✓	✓	✓
Eff. N	6,037	9,817	11,943	5,893	9,543	11,653

Note: The outcome variable is categorical and takes the value 1 if workers have changed province during their UI entitlement length, 0 otherwise. All results are calculated using the MSE-optimal bandwidth suggested by Calonico, Cattaneo and Titiunik (2014). Column 1 estimates β from the local linear model specified in equation 2.1. Columns 2 and 3 include higher-order polynomials; and columns 4 to 6 control for covariates. Robust standard errors (in brackets) are clustered at the day of entry in unemployment. *** p<0.01, ** p<0.05, * p<0.1

Notice that the measure of mobility is based on the province where workers have their relationship with the Social Security Administration. Therefore, the previous results can be due to an increase in the number of people who commute or to an increase in the number of workers who change their province of residence. In order to shed some light on this, I estimate equation 2.1, looking at whether the policy has affected the probability of migrating to non-neighboring provinces.

The results in the first column of table 2.2 show that the UI drop has increased mobility towards non-neighboring provinces by 3 percentage points (s.e. = 0.014) on average. The positive effect of the reform on mobility towards non-bordering areas is robust to the inclusion of higher-order polynomials in the running variable and covariates.

Given that commuting distances across non-neighboring provinces are very

[32]In all specifications, controls include personal characteristics such as gender, age, age squared, level of education (below secondary education, secondary education, and tertiary education), years of experience in the labor market, number of dependants in the household, and real annual earnings in the year prior displacement (in logs). I also include information on the pre-displacement job: type of firm (public or private), type of contract (permanent or fixed-term), four occupational dummies (high skill job occupation, medium-high skill job occupation, medium-low skill job occupation, or low skill job occupation), and fourteen industry dummies. Finally, I control for the unemployment rate in the last province of employment, and the UI potential duration.

large in Spain (the smallest province occupies almost 2,000 squared kilometers), the findings presented in table 2.2 suggest that the effect of the UI drop on geographical relocation is mainly due to actual changes in the province of residence rather than to more people commuting to other provinces.[33]

Table 2.2: Effect of the Reform on Mobility across Non-Neighboring Provinces

Outcome	Mobility across Non-Neighboring Provinces					
Bandwidth	MSE Optimal Bandwidth					
Days around the reform	97	132	189	88	132	193
Reform (T_i)	0.03**	0.05***	0.06***	0.04**	0.05***	0.06***
	[0.014]	[0.016]	[0.018]	[0.015]	[0.017]	[0.019]
Control Function	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
Covariates				✓	✓	✓
Eff. N	5,225	7,090	10,470	4,677	6,995	10,551

Note: The outcome is a binary variable that takes the value 1 if workers have moved to a non-neighboring province during their UI entitlement length, 0 otherwise. All results are calculated using the MSE-optimal bandwidth suggested by Calonico, Cattaneo and Titiunik (2014). Column 1 estimates β from the local linear model specified in equation 2.1. Columns 2 and 3 include higher-order polynomials, and columns 4 to 6 also include controls. Robust standard errors (in brackets) are clustered at the day of entry in the UI. *** p<0.01, ** p<0.05, * p<0.1

2.5.3 Further Results

Heterogeneity

To better understand the effect of the UI benefit cut on geographical mobility, this section looks at the heterogeneity of the effect across individuals with different observable characteristics. I present the results in table 2.3.

Panel A shows the outcomes of estimating equation 2.1, dividing the sample by gender. The results reveal important asymmetries: while the reform increased men's mobility by 6 percentage points (s.e. = 0.028), we do not observe any change in the migration patterns for women after the policy change. This finding is consistent with the migration literature, that shows that men have larger disparities in their wage distribution across locations, their average wage offers are larger for each location, and they derive less utility of leisure than women (Gemici, 2011).

[33] Note that the smallest province in Spain is Gipúzkoa. To cross it from east to west (minimum distance) takes an hour and a half by car. Ceuta and Melilla are two exceptions. These two provinces in the North of Africa occupy 20 and 28 squared kilometers respectively. Yet, the distance from one to the other is equivalent to 6 hours by car. Albeit Ceuta is nearer to the Iberian Peninsula, it would also require more than two hours by car to arrive at the nearest location.

Panel B divides the sample into two groups depending on the age at displacement: those who started the UI before they turned 36 years old, and those who started the unemployment spell when they were 36 or older.[34] The results indicate a 6 percentage points (s.e. = 0.034) increase in mobility due to the UI drop for the youngest group and small and statistically insignificant effects for those who become unemployed after they turned 36. This finding is in line with the previous literature, which shows that younger workers benefit more from migration as they have not yet accumulated geographic-specific human capital, and their expected wage gains are greater as they have more years to collect the benefits from moving (Borjas, Bronars and Trejo, 1992).

Panel C looks at the effects of the policy on mobility across groups with different care duties. Namely, it divides the sample between those workers who have and who have not family responsibilities.[35] The results show that the reform increased mobility by 8 percentage points (s.e. = 0.032) for the group of workers with no dependents, while the policy did not affect the mobility decisions of workers with family responsibilities. This finding is also consistent with the migration literature, which shows that dependants remarkably increase the costs of migration (Mincer, 1978).

Previous literature has identified education as an additional important determinant of mobility (Malamud and Wozniak, 2012). More educated workers obtain larger net expected gains from relocating. This can explain the differences in migration rates across different educational groups. The results in Panel D show that workers with tertiary education react more to the policy in terms of magnitude. However, these results are imprecisely estimated, probably because the sample size for workers with college is relatively small (4,043 persons).

Finally, Panel E exploits the presence of heterogeneity in mobility between those living in or outside their provinces of birth. According to previous research, workers displaced outside their place of birth are more mobile (see Cadena and Kovak (2016)). Albeit the results go in this direction (the estimated coefficients for the parameter β in equation 2.1 are larger for those living outside their provinces of birth at the moment of displacement), they are imprecisely estimated. This could be related to the fact that just 23 percent of the sample were working outside their provinces of birth.

[34] 36 is the median age in the sample.

[35] Family responsibilities are defined as descendants younger than 26 or with a disability greater than 33 percent; or ancestors older than 65 or with a disability greater than 33 percent who live with the UI recipient.

Table 2.3: Effect of the Reform on Geographical Mobility by Group

Outcome	Mobility across Provinces					
A: Gender						
Only Men	0.06**	0.08**	0.10**	0.06**	0.07**	0.10**
	[0.028]	[0.034]	[0.041]	[0.028]	[0.034]	[0.041]
Only Female	0.00	0.01	0.01	-0.00	0.01	0.02
	[0.029]	[0.034]	[0.038]	[0.029]	[0.033]	[0.039]
Panel B: Age						
≤ 35	0.06*	0.15***	0.17***	0.09**	0.17***	0.18***
	[0.034]	[0.048]	[0.051]	[0.040]	[0.050]	[0.052]
> 35	0.00	-0.03	-0.05	-0.01	-0.03	-0.05
	[0.023]	[0.032]	[0.038]	[0.024]	[0.032]	[0.037]
Panel C: Family Responsibilities						
With dependents	0.02	0.03	0.01	0.02	0.02	0.01
	[0.024]	[0.029]	[0.036]	[0.022]	[0.029]	[0.036]
Without dependents	0.08***	0.11***	0.11***	0.08**	0.11***	0.13***
	[0.032]	[0.036]	[0.039]	[0.034]	[0.039]	[0.041]
Panel D: Education						
Below secondary	0.04*	0.04	0.03	0.03	0.03	0.04
	[0.020]	[0.028]	[0.038]	[0.021]	[0.028]	[0.039]
Secondary education	0.02	0.05	0.04	0.01	0.04	0.03
	[0.039]	[0.046]	[0.046]	[0.040]	[0.046]	[0.047]
Tertiary education	0.06	0.08	0.10*	0.06	0.09*	0.12**
	[0.045]	[0.053]	[0.060]	[0.043]	[0.048]	[0.061]
Panel E: Province of Last Employment						
Province of birth	0.01	0.02	0.02	0.01	0.01	0.02
	[0.017]	[0.019]	[0.022]	[0.017]	[0.019]	[0.023]
Not the province of birth	0.06	0.08	0.10	0.06	0.07	0.09
	[0.042]	[0.053]	[0.066]	[0.039]	[0.049]	[0.059]
Control Function	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
Covariates				✓	✓	✓

Note: The table reports the coefficient of the parameter β based on estimating equation 2.1 separately for each of the different groups. The outcome is a binary variable that takes the value 1 if workers have changed province during their UI entitlement length, 0 otherwise. The bandwidth is calculated using MSE-optimal bandwidth, as suggested by Calonico, Cattaneo and Titiunik (2014). Standard errors (in brackets) are clustered at the day of entry at the UI. In column 1, I estimate the local linear regression specified in equation 2.1. The next two columns include second and third-order polynomials. Columns 4 to 6 add covariates. *** p<0.01, ** p<0.05, * p<0.1

Mobility towards Big Cities

This part of the analysis focuses on the destination of movers. A sensible assumption is that unemployed workers move back to where they originally came from or to smaller areas with cheaper living costs (Huttunen, Møen and Salvanes, 2011; Kaplan, 2012). Table A.3 looks into this hypothesis. We see −in panel A− that the reform has not affected the probability of returning to the province of birth (point estimate = 0.02; s.e. = 0.013). Besides, panel B shows that the policy implementation did not change the probability of moving towards municipalities with less than 40,000 inhabitants (point estimate = 0.01; s.e. = 0.007). However, the UI cut did increase the likelihood of moving towards municipalities with more than 40,000 inhabitants by 4 percentage points (s.e. = 0.020). Looking more in detail, table 2.4 presents the results of estimating equation 2.1 using as the outcome a categorical variable that takes the value 1 if workers have moved towards one of the six biggest cities in Spain, and zero otherwise.[36]

Table 2.4: Effect of the Reform on Mobility towards Big Cities

Outcome	Mobility towards Big Cities					
Bandwidth	MSE Optimal Bandwidth					
Days around the reform	127	161	200	131	157	201
Reform (T_i)	0.03**	0.04***	0.05***	0.02**	0.04***	0.05***
	[0.010]	[0.013]	[0.014]	[0.010]	[0.013]	[0.014]
Control Function	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
Covariates				✓	✓	✓
Eff. N	6,786	8,761	11,228	6,949	8,436	11,257

Note: The outcome variable is a dummy that takes the value 1 if workers have moved to one of the six biggest cities in Spain, 0 otherwise. All results are calculated using the MSE-optimal bandwidth suggested by Calonico, Cattaneo and Titiunik (2014). Column 1 estimates β from the local linear model specified in equation 2.1. Columns 2 and 3 include higher-order polynomials, and columns 4 to 6 also include controls. Robust standard errors (in brackets) are clustered at the day of entry in the UI. *** p<0.01, ** p<0.05, * p<0.1

The results show that the policy increased mobility towards the biggest cities by 3 percentage points (s.e. = 0.010). This result is in line with the evidence in Roca and Puga (2017), which show that workers obtain greater gains by moving towards the more dynamic and large areas. Given that the UI cut took place in the aftermath of the Great Recession, a moment when the job demand was scarce and the unemployment rate was almost 26 percent,

[36] Spain just has six cities with a population of over 500,000 inhabitants. They are Madrid, Barcelona, Valencia, Sevilla, Zaragoza, and Málaga. De la Roca (2017) or Roca and Puga (2017) also use this definition to classify the "big cities".

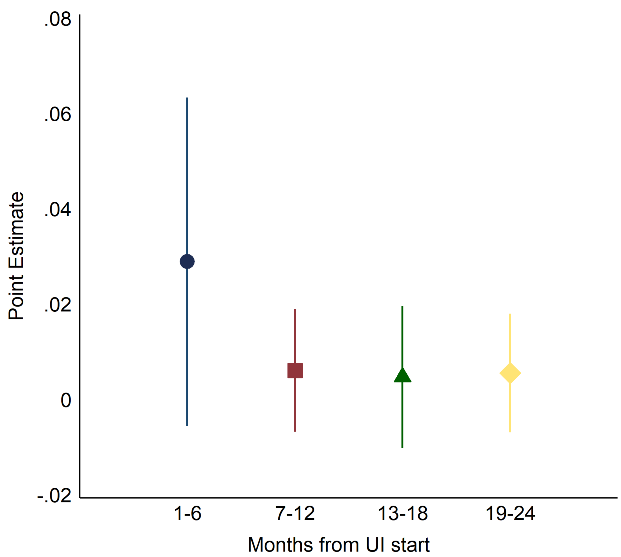
it is not unexpected that individuals decided to relocate into the big cities, where firms are located.

Time of Mobility

An interesting question is the timing of the movements. Recall that the reduction in the UI generosity does not happen from the beginning of the unemployment spell. Instead, the 10 percentage points drop in the RR happens after 6 months of unemployment. Yet, affected workers know their UI will be reduced since they are fired.

In this section, I analyze whether workers anticipate their behavior to the change in the UI benefit. Figure 2.3 plots the coefficients of estimating equation 2.1 using as dependent variables the probability of changing province during different periods (e.g., in the plot represented with a square in $x = [7-12]$, the outcome variable equals 1 if the UI recipient has moved between the seventh and twelfth month of unemployment -conditional on not having moved before-, and 0 otherwise). The results show that unemployed workers anticipated the change in the UI, as the main response to the reform in terms of mobility happens during the first six months of unemployment.

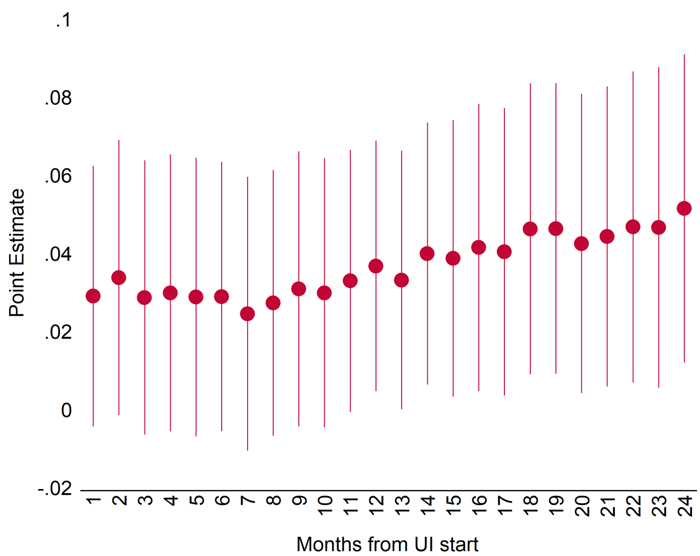
Figure 2.3: Mobility Decisions: Flow



Note: Figure 2.3 plots the coefficients of estimating equation 2.1 using as dependent variables the probability of moving during different periods (i.e., in the plot represented with a square in $x = [7-12]$, the outcome variable equals 1 if the UI recipient has moved between the seventh and twelfth months of unemployment -conditional on not having moved before-, and 0 otherwise).

Figure 2.4 plots the cumulative probability of moving during the two years after the person started receiving UI (e.g., the point represented in $x = 8$ results from estimating the local linear model in equation 2.1 using as dependent variable a categorical variable that takes the value 1 if the person has changed province during the first 8 months of unemployment, and 0 otherwise). The results also suggest that workers react to the policy by moving from the beginning of the unemployment spell. These findings are in line with Rebollo-Sanz and Rodríguez-Planas (2018). They show that the decrease in the nonemployment duration happens from the beginning of the unemployment spell, suggesting that affected workers anticipate the effect of the policy and do not wait until their UI benefit amount drops.

Figure 2.4: Mobility Decisions: Stock



Note: Figure 2.4 plots the cumulative probability of moving during the UI spell (e.g., the point represented in $x = 8$ results from estimating the local linear model in equation 2.1 using as dependent variable a categorical variable that takes the value 1 if the person has changed of province during the first 8 months since the start of the unemployment spell, 0 otherwise).

Job Finding Probability for Movers and Stayers

This section attempts to identify if workers who changed province have spent less time unemployed compared to the hypothetical state of having not moved. Ideally, one would like to estimate the following model:

$$\Delta = E(Y_1|D = 1) - E(Y_0|D = 1) \tag{2.2}$$

where D is a categorical variable equal 1 if the unemployed worker changed province during the UI length, and 0 otherwise. Y represents the unemployment duration, which is defined as the number of days that elapse between the start of the UI and the day of entry in the next job. In this context, Y_1

indicates the outcome for movers, and Y_0 indicates the outcome for stayers.

The problem with equation 2.2 is that I cannot observe the second term on the right-hand side (i.e., I am not able to know the unemployment duration of workers who have moved if they had not moved). In this scenario, a solution is to compare movers with stayers. A simple t-test on the duration of unemployment with the sample of workers affected by the policy suggests that movers spend 71 days (s.e. $= 9.2$) less unemployed in comparison with stayers.[37] However, mobility is a decision variable, and previous studies show that movers may not be a random sample in the pool of job seekers (Gabriel and Schmitz, 1995). To address this potential selection bias, I make use of a Propensity Score Matching (PSM) methodology.

In the first stage of the PSM, I estimate a binary model that analyzes the propensity of individuals to move given some predetermined observable characteristics X^*. I follow the migration literature to select the variables that may be relevant to determine whether a worker decides to move or to stay. Accordingly, I use gender, age, age squared, education level, earnings, dependants, and an indicator variable that distinguishes those workers who have already moved from those who have never changed province. All variables have the expected sign: men are more likely to move than women. The probability of changing province is larger for workers with higher educational attainment. Those job-seekers with dependants and who had never moved before are less likely to relocate. In addition, workers with higher earnings are more prone to migrate.[38]

Once I have estimated the first step, I compare the outcomes of workers who have similar propensities to migrate. To keep it simple, I estimate the PSM using the nearest neighbor method with replacement.[39] The results show that the estimated average treatment effect equals -61 (s.e. $= 13.35$). In other words, the findings suggest that movers find a job 2 months earlier than they would have if they had stayed in the province of displacement.

Yet, it is important to stress that this method relies on a very strong assumption: the conditional independence assumption. This is, conditional on the selection of observables X^*, the assignment into treatment (i.e., to move or not to move) is random (Rosenbaum and Rubin, 1985). Given this, I consider these findings as *suggestive evidence* that movers find jobs earlier than comparable stayers.

[37] The average unemployment duration for stayers is 393 days, and for movers, it is 322 days.

[38] Results from the probit model are available upon request.

[39] Using other methods, I obtain similar results.

2.5.4 Robustness

In the next paragraphs, I test the robustness of the main results.[40] To do so, I first estimate equation 2.1 using alternative bandwidths (from 7 days to 365 days before and after the cutoff date). Figure A.6 presents the results. They show that the findings in section 2.5.2 are not due to the bandwidth selection.

I also perform several placebo tests. First, I artificially move the cutoff date to every day of 2012. Figure A.7 presents the coefficients of estimating the parameter β in equation 2.1 for the 365 placebo regressions. Just four out of these 365 estimated coefficients are larger in absolute magnitude than the estimated 0.0389 in column 1 of table 2.1. This test reinforces the validity of the main results.

Table A.4 presents additional evidence of the robustness of the findings in section 2.5.2. In particular, it shows the results of estimating equation 2.1 using unemployed workers entitled to receive UI between 4 and 6 months.[41] The coefficients for the parameter β are smaller in magnitude than the point estimates in table 2.1, and they are statistically indistinguishable from zero.[42] Overall, these checks support the idea that the previous findings are, in fact, due to the reduction in the UI benefit generosity.

Yet, one additional concern about the results presented in table 2.1 is that they may be driven by seasonality. In order to rule out this possibility, I complement the RD approach with the following specification in equation 2.3.

$$Y_i = \alpha + \beta T_i + \gamma_1(c_i - c') + \theta X_i + \sum_{i=1}^{12} Month_i + \sum_{i=1}^{3} Year_i + \epsilon_i \qquad (2.3)$$

The sample for this estimation contains those workers who start receiving UI between 2011 and 2013. Including several years allows controlling for seasonality by adding calendar month and year fixed-effects. The results −in

[40]These robustness checks are available for all the results presented in sections 2.5.2 and 2.5.3 under request.

[41]The drop in the RR happens after 180 days of unemployment. Thus, workers entitled to perceive UI during six or less than six months are not affected by the cut in the UI benefit amount.

[42] Table A.4 −panel B− presents the outcomes of estimating equation 2.1 for the sample of workers who were entitled to receive UI for more than six months but whose UI benefit amount did not drop after the reform (see figure 2.1). The results show a 10 p.p. increase in mobility for this group. Because these results are surprising, I replicate the previous exercise and estimate the coefficient for parameter β for every day in 2012. 57 out of the 365 additional regressions presented a point estimate larger in magnitude to the 10 p.p. (see figure A.8). Thus, the results in table A.4 −panel B− are not very informative.

figure A.9− show the estimated coefficients of the parameter β in equation 2.3 for different samples. We can see that the findings are robust to the inclusion of month and year fixed-effects.

It could also be that those workers affected by the policy were *by chance* more mobile than those who were not. Figure A.10 shows the results of estimating equation 2.1, looking at mobility before the reform. The results indicate that there are no statistically significant differences in mobility rates for the treated and control groups in the months that preceded the policy implementation.

Finally, I analyze whether the previous findings are robust to alternative definitions of geographical units. Namely, I look at mobility across urban areas and autonomous regions (*Comunidades Autónomas* or CC.AA.).

Table A.5 −panel A− presents the results of estimating equation 2.1 using as the dependent variable mobility across urban areas. This definition is interesting as urban areas are a good approximation for local labor markets (Roca and Puga, 2017).[43] The MCVL has information on the municipality where the firm is located for each job and (un)employment spell. Therefore, I can assign each individual to an urban area conditional on the municipality to have more than 40,000 inhabitants. Given this threshold limitation, I lose 50 percent of the baseline sample when defining mobility as changes in urban areas. Still, the results presented in table A.5 show that the reform increased relocation across urban areas by 6 percentage points. In terms of magnitude, this represents a 28 percent increase with respect to the pre-reform mean.

Table A.5 −panel B− presents the results of estimating equation 2.1, looking at mobility across autonomous regions. The RD estimates indicate that the reduction in the UI generosity increased mobility across CC.AA. by 4 percentage points. Because interregional mobility before the reform was about 11 percent, the results indicate a 36 percent increase with respect to the pre-reform mean.[44]

As the effect of the UI drop on mobility across CC.AA. is so large, I analyze the possibility that the increase in mobility was mainly caused by more workers moving to provinces outside the regions where they were last em-

[43]The definition of urban areas is constructed by Spain's Ministry of Development since 2008. In Spain, there are 85 urban areas. Albeit they account for just 10 percent of the surface, more than 68 percent of the population lives in urban areas. In addition, less than 25 percent of employment happens outside urban areas. The average population per urban area is around 150,000 inhabitants.

[44]Spain consists of 17 autonomous regions (which are comparable to states in other countries). Seven out of the seventeen regions cover just one province. The average population per CC.AA. is around 2.5 million inhabitants.

ployed. Table A.5 −panel C− shows the results of estimating equation 2.1 using as the outcome a categorical variable that takes the value 1 if the worker changes province but not CC.AA., and 0 otherwise. The results suggest that the increase in mobility is due to people moving outside their regions of pre-displacement work.

2.6 Conclusion

Labor mobility is an efficient mechanism to reduce labor market disparities across regions. Yet, geographical relocation in the EU -across and within countries- is limited. This paper studies the effect of the UI generosity explaining the low propensity of workers to relocate.

To establish a causal link, I exploit an exogenous reduction in the Spanish UI benefit amount introduced in July 2012. The results show that the cut in the UI generosity increased unemployed workers' mobility by 4 percentage points. In terms of magnitude, this represents a 24 percent increase with respect to the pre-reform mean. Educated and young men without family responsibilities moving towards the biggest cities drive the results.

This paper is closely related to Rebollo-Sanz and Rodríguez-Planas (2018), which exploits the same reform. They find that the 10 percentage points drop in the UI replacement rate reduced the expected nonemployment duration by 5.7 weeks, without affecting the subsequent job-match quality. The results presented in this study point towards geographical mobility as a relevant job-search mechanism to reduce the unemployment length.

This work has important policy implications. As Chichester (2005) explains in an OECD policy paper, promoting mobility may not be an end in itself, but it is important to reduce the barriers to internal mobility in countries where local asymmetries in the labor market are remarkable. The findings of this paper show that changing the UI design may reduce the moral hazard problems observed in the data by encouraging the active search for jobs.

In particular, the results seem to indicate that front-loading the payment of the unemployment benefits (i.e., large RR at the beginning of the unemployment spell, but decreasing steeply throughout the nonemployment spell) can potentially increase search-effort (e.g., intensifying geographical mobility), and reduce the nonemployment duration. This policy prescription was already made in Spain in the *Manifiesto de los 100 economistas*.[45] To the best of my

[45]The *Manifiesto de los 100* is a document signed in 2009 by one hundred leading Spanish economists that contains economic measures to reactivate the Spanish labor market.

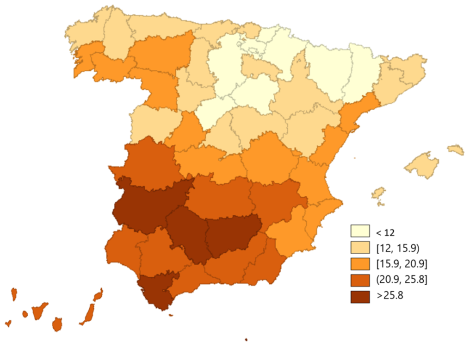
knowdlege, Lindner and Reizer (2016) present the only empirical causal evidence on the effects of front-loading the UI. Albeit Lindner and Reizer (2016) does not look at the mechanisms, their study shows that front-loading the unemployment benefit payments in Hungary reduced the nonemployment duration, increased re-employment wages, and improved the government's budget balance. Future work evaluating the effects of this policy on geographical relocation and subsequent labour market outcomes could be very informative and complementary to this work.

2.7 Appendix

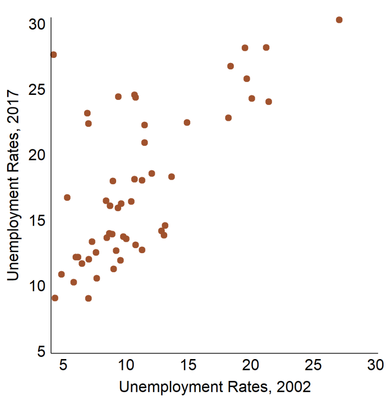
The Spanish Labor Market

Figure A.1: Local Unemployment Rates in Spain

(a) Unemployment Distribution, 2017

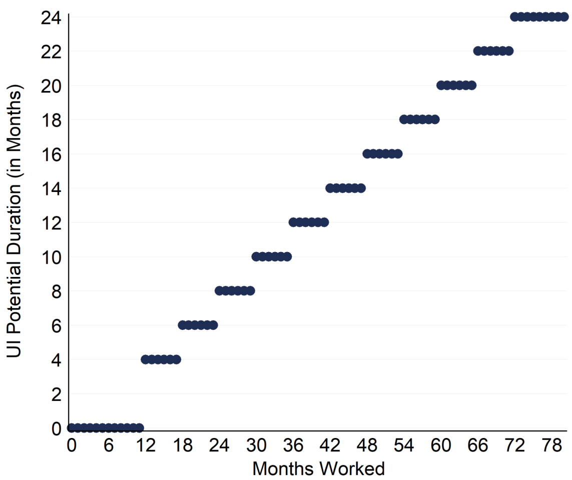


(b) Unemployment Persistence



Note: Figure A.1a shows the unemployment rate by province or NUTS-3 regions (this represents territorial units with an average population of about 0.9 million inhabitants) in Spain in 2017. The map shows large disparities in unemployment, with some provinces in the south having an unemployment rate twice as large as some provinces in the north. In addition, figure A.1b shows that local unemployment rates in 2017 were highly correlated (0.68) with those in 2002. *Source: Spanish National Institute of Statistics (INE).*

Figure A.2: UI Duration and Days Worked

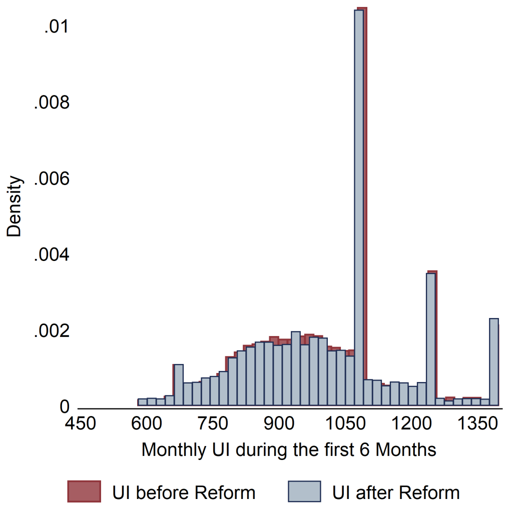


Note: Figure A.2 shows that just employees who have worked under a Social Security regime that covers the situation of unemployment during at least 360 days (12 months) in the six years previous to the displacement are entitled to receive UI. Workers who have contributed just those 12 months are entitled to the minimum UI length of 4 months. For each additional 6 months contributed, the UI duration increases around 2 months, up to a maximum of 24 months. *Source: Servicio Estatal de Empleo, SEPE.*

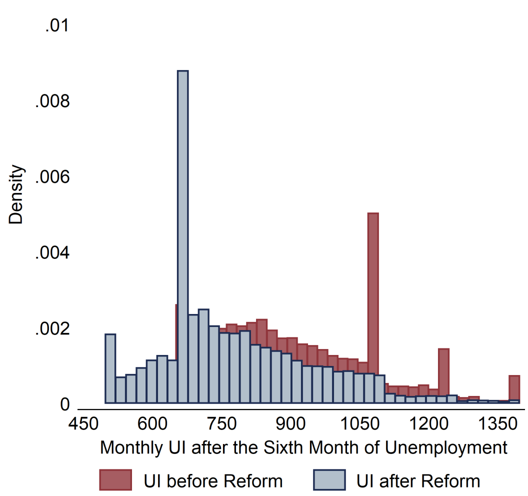
Unemployment Insurance in Spain: UI Benefit Amount

Figure A.3: Effect of the RR Drop on the UI Benefit Amount

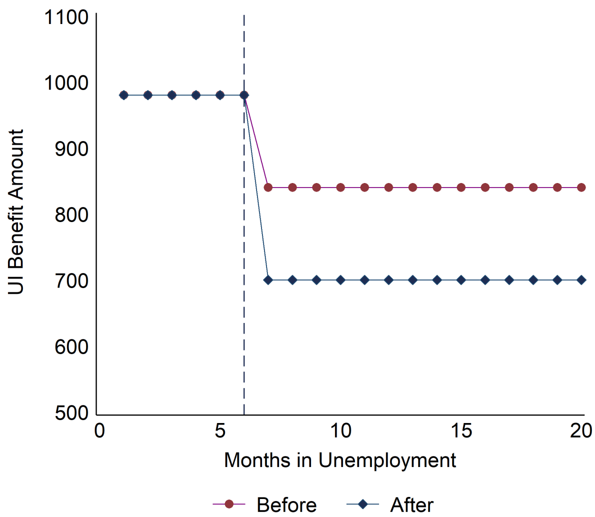
(a) UI Distribution in first 6 Months



(b) UI Distribution after 6 Months



(c) Effect of the Reform for the Average Worker

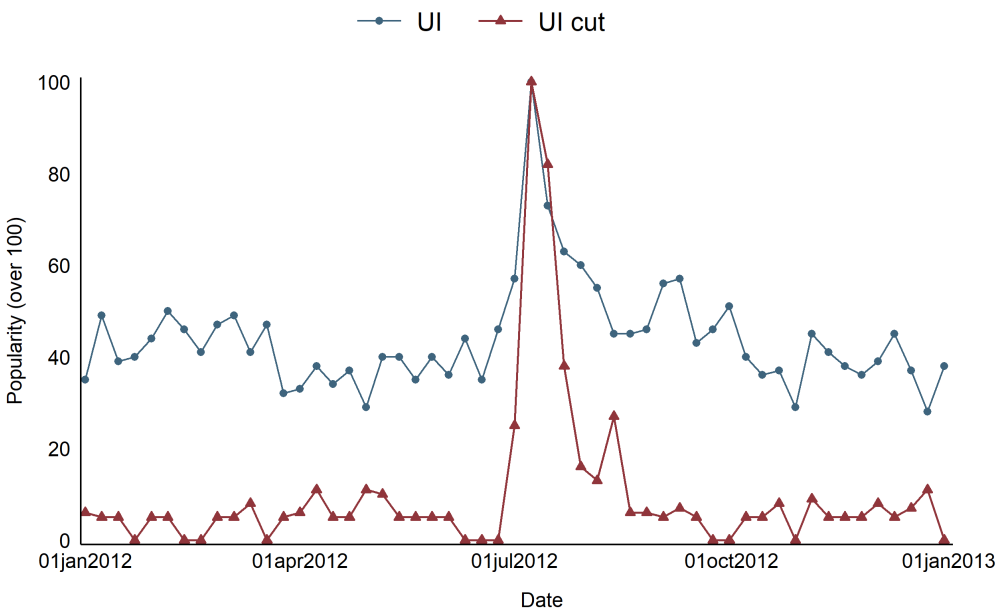


Note: Figures A.3a and A.3b present the distribution of the UI benefits during the first six months of UI and after the 6th month of UI for the treated (in blue) and control (in brown) groups. As expected, during the first 180 days receiving UI, there are no differences between the UI benefit amount for those workers who started the unemployment spell before or after July 15, 2012. However, after the sixth month, UI recipients in the treated group receive substantially lower UI benefits than those in the control group. We also observe important peaks indicating the maximum and minimum UI benefits. Figure A.3c represents the effect of the policy for two identical individuals with an average wage in the 6 working-months preceding the unemployment of €1,400, with no family responsibilities, and who are entitled to 20 months of UI (average person in the sample). The only difference between these two workers is that one of them (worker A – represented with the brown line) started the unemployment spell on July 14, 2012, while the other person (worker B – represented with the blue line) started the unemployment on July 15, 2012. We see that during the first 6 months of unemployment, both workers receive €980. However, from the seventh until the last month of UI entitlement, worker A receives €840 in terms of UI per month, while worker B receives €700.

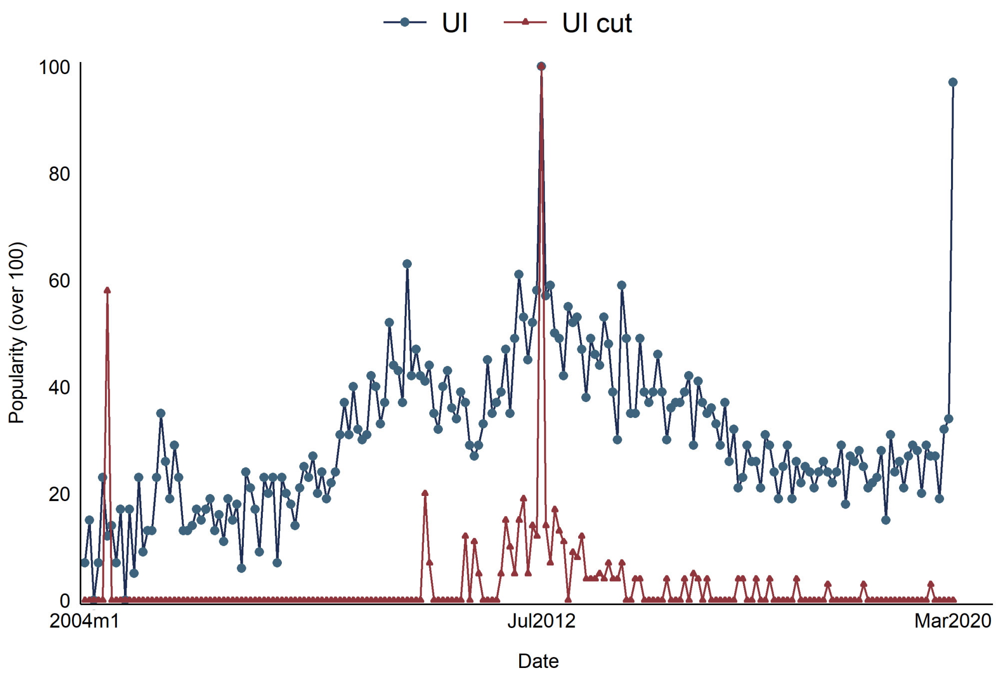
The Salience of the Reform

Figure A.4: Popularity in Google Trends of the Terms “UI” and “UI Cut”

(a) Year 2012



(b) From 2004-2020



Note: Figure A.4a represents the popularity of the terms “prestación de desempleo” (*Unemployment Insurance*) and “recortes paro” (*UI cuts*) over the year 2012 in Spain. We can observe an important jump on the searches for both terms in Google on the week the change in the RR was announced, approved, and implemented. Figure A.4b shows the searches for the terms above in Spain between 2004 and 2020. We can see a clear peak in July 2012, suggesting the relevance and magnitude of the policy change. The only exception is the second peak for the term UI in March 2020, caused by the destruction of hundreds of thousands of jobs due to the covid-19.

Descriptive Statistics

Table A.1: Descriptive Statistics

	All (1)	Pre-reform (2)	Post-reform (3)	Difference ((3)-(2))
Main Outcome Variable				
Change Province	0.163	0.161	0.166	0.004
	[0.370]	[0.368]	[0.371]	(0.004)
Covariates				
Panel A: Worker Characteristics				
Male	0.599	0.612	0.584	-0.029***
	[0.490]	[0.487]	[0.493]	(0.006)
Age(years)	36.48	36.29	37.71	0.427***
	[6.764]	[6.769]	[6.750]	(0.081)
Dependents	0.502	0.499	0.505	0.006
	[0.500]	[0.500]	[0.500]	(0.006)
Below secondary education	0.564	0.583	0.541	-0.04***
	[0.496]	[0.493]	[0.498]	(0.006)
Secondary education	0.291	0.285	0.298	0.01**
	[0.454]	[0.451]	[0.457]	(0.005)
Tertiary education	0.145	0.132	0.161	0.03***
	[0.352]	[0.338]	[0.367]	(0.004)
Experience(years)	12.87	12.67	13.13	0.468***
	[6.396]	[6.419]	[6.358]	(0.077)
Earnings (in log)	9.680	9.688	9.670	-0.017***
	[0.358]	[0.365]	[0.350]	(0.004)
Panel B: Last Employment Characteristics				
High skill occupation	0.128	0.120	0.137	0.016***
	[0.334]	[0.326]	[0.343]	(0.004)
Medium-high skill occupation	0.219	0.211	0.228	0.016***
	[0.413]	[0.408]	[0.419]	(0.005)
Medium-low skill occupation	0.437	0.448	0.422	-0.026***
	[0.496]	[0.498]	[0.494]	(0.006)
Low skill occupation	0.217	0.220	0.213	-0.06
	[0.412]	[0.414]	[0.410]	(0.005)
Private firm	0.930	0.931	0.929	-0.002
	[0.255]	[0.254]	[0.257]	(0.003)
Permanent contract	0.681	0.669	0.696	0.03***
	[0.466]	[0.471]	[0.460]	(0.006)
Panel C: Local labor markets				
Unemployment rate	23.92	22.54	25.61	3.071***
	[6.465]	[6.121]	[6.477]	(0.075)
Panel D: UI characteristics				
UI duration	20.28	20.31	20.25	-0.060***
	[5.303]	[5.184]	[5.447]	(0.064)
Observations (N)	28,125	15,557	12,568	28,125

Note: Columns 1-3 report means and standard deviations in brackets. Column 4 reports differences of groups means between columns 3 and 2 with standard errors in parenthesis. ***,**, and * denote significance at the 1, 5 and 10 percent levels.

Validity Checks

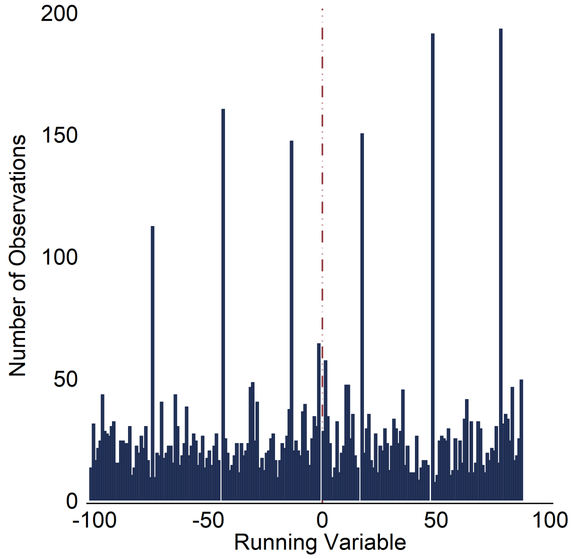
Table A.2: Balanced Test on Covariates

	RD Estimate	Standard Errors	Eff. N
Panel A: Worker Characteristics			
Male	-0.02	0.022	6,184
Age (in years)	-0.13	0.233	6,140
Less than secondary education	0.00	0.022	8,436
Secondary education	-0.01	0.028	5,669
Tertiary education	-0.00	0.015	5,339
Dependants	0.11	0.072	5,158
Experience (in years)	-0.09	0.154	6,995
Earnings in the year prior the displacement (in logs)	0.01	0.023	5,339
Panel B: Last Employment Characteristics			
High skill occupation	-0.01	0.019	5,669
High-Medium skill occupation	-0.00	0.023	5,262
High-Low skill occupation	0.02	0.027	4,809
Low skill occupation	-0.01	0.019	6,284
Private firm	-0.00	0.009	3,908
Permanent contract	0.00	0.020	5,921
Agricultural sector	0.00	0.005	4,583
Manufacturing	-0.02	0.018	6,515
Utilities	0.00	0.004	5,921
Construction	0.02	0.025	5,960
Trade	0.02	0.021	4,360
Transport and storage	0.01	0.013	5,230
Accommodation and food services	-0.02	0.013	3,908
Information and communication	-0.02	0.014	6,465
Finance, insurance, and real state activities	-0.00	0.004	6,643
Professional, scientific, and technical activities	0.00	0.012	6,140
Administrative and support activities	-0.01	0.014	6,284
Education, human health, and social work	0.02	0.016	3,547
Other services	-0.01	0.010	8,008
Public administration sector	0.01	0.011	3,677
Panel C: Local labor market			
Unemployment rate	-0.15	0.543	4,677
Panel D: UI characteristics			
Maximum UI entitlement (months)	-0.24	0.231	5,339

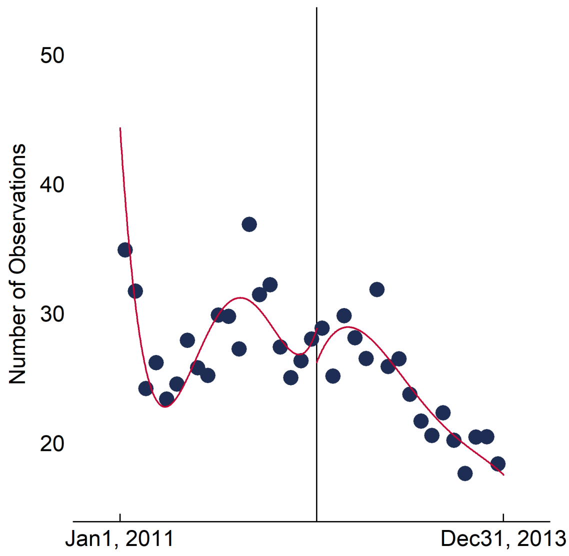
Note: The table shows the RD results of estimating equation 2.1 using as outcome variables the control variables of the original model. The bandwidth for each regression is estimated separately using CER-optimal bandwidth (see Cattaneo, Idrobo and Titiunik (2018)). All regressions include covariates. Standard errors are clustered at the day of starting the UI. Panel A focuses on workers' characteristics, and Panel B on the features of the workers' last employment; Panel C looks at the unemployment rate on the province of last employment for each individual during the term they become unemployed; and panel D at the UI duration each worker in the sample is entitled to receive. None of the coefficients is statistically distinguishable from 0. This test supports the validity of the RD design. Albeit I follow Cattaneo, Idrobo and Titiunik (2018) to perform this balanced test, these results are robust to estimating each regression using the optimal bandwidth derived from the main results (111 around the reform) with and without including control variables. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Figure A.5: Distribution of UI Inflows

(a) Histogram



(b) RD Plot



Note: Figure A.5a shows the graphical representation of running the density test for the running variable proposed by Cattaneo, Jansson and Ma (2019) (see Cattaneo, Jansson and Ma (2018) for a deep explanation of how to implement the test in Stata). Formally, the value of the statistic is positive (0.1615), and statistically indistinguishable from 0 (p-value of 0.8717). Figure A.5a also indicates that the distribution of UI entries is not random, as there are important peaks at the beginning of each month (the average number of workers who start an unemployment spell per day is 26 (see figure A.5b), while the average amount of workers who start receiving UI on the first day of the month is 154). Figure A.5b plots the average number of workers on the y-axis and the day they start receiving UI on the x-axis. The MV-optimal number of evenly distributed bins is 19 below the cutoff and 17 above it. The average bin length is around 30 days on both sides. Both figures suggest that there is no bunching around the cutoff date of July 15, 2012. Figure A.5b also points towards a decrease in the number of new UI recipients over time, reflecting both the recovery of the Spanish economy after the second term of 2013 and the fact that many workers may have exhausted their UI entitlements and are not eligible for new ones.

Movers' Destinations

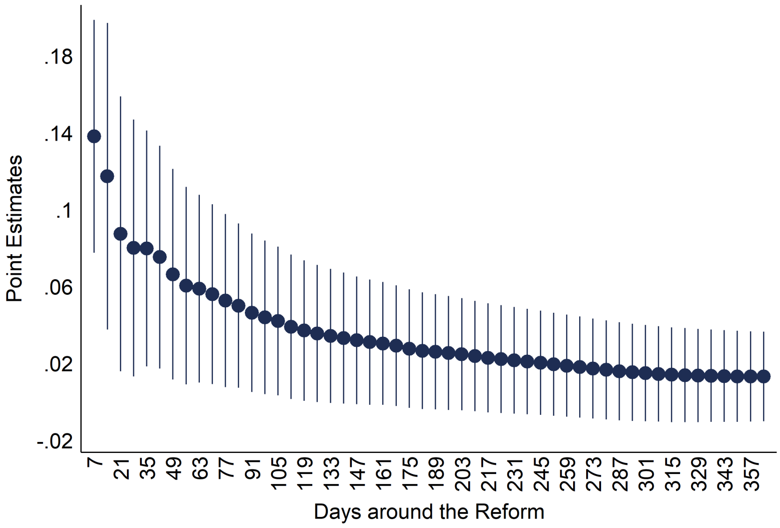
Table A.3: Effect of the Reform on Mobility towards ...

Panel A						
Outcome	**Province of Birth**					
Bandwidth	MSE Optimal Bandwidth					
Days around the reform	137	155	228	114	140	230
Reform (T_i)	0.02	0.03*	0.03*	0.02	0.04**	0.04**
	[0.013]	[0.017]	[0.018]	[0.014]	[0.017]	[0.017]
Eff. N	7,405	8,448	12,808	6,094	7,609	9,801
Panel B						
Outcome	**"Small" Municipalities**					
Bandwidth	MSE Optimal Bandwidth					
Days around the reform	203	198	229	187	192	231
Reform (T_i)	0.01	0.01	0.00	0.01	0.01	0.00
	[0.007]	[0.010]	[0.013]	[0.007]	[0.011]	[0.013]
Eff. N	11,461	11,161	12,941	10,275	10,444	12,837
Panel C						
Outcome	**"Large" Municipalities**					
Bandwidth	MSE Optimal Bandwidth					
Days around the reform	104	154	205	96	146	197
Reform (T_i)	0.04*	0.05**	0.06**	0.04*	0.05**	0.07***
	[0.020]	[0.023]	[0.026]	[0.021]	[0.024]	[0.027]
Eff. N	5,496	8,396	11,169	5,084	7,925	10,991
Control Function	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
Covariates				✓	✓	✓

Note: The outcome variable in panel A is categorical and takes the value 1 if workers have moved back to their province of birth, and 0 otherwise. The dependent variable in panel B is categorical and takes the value 1 if workers have moved to small municipalities, and 0 otherwise. I consider a municipality to be small if it has less than 40,000 inhabitants. Recall that in the MCVL, I cannot identify those municipalities with less than this amount of people (i.e., I cannot know the size of municipalities of less than 40,000 inhabitants). This explains the threshold I consider to identify small vs. non-small locations. However, it is important to say that 50 percent of the people in Spain live in this type of "small" locations. Finally, panel C looks at mobility towards bigger places. All results are calculated using the MSE-optimal bandwidth suggested by Calonico, Cattaneo and Titiunik (2014). Column 1 estimates β from the local linear model specified in equation 2.1. Columns 2 and 3 include higher-order polynomials, and columns 4 to 6 also include controls. Robust standard errors (in brackets) are clustered at the day of entry in the UI. *** p<0.01, ** p<0.05, * p<0.1 The results show that people move, especially, towards medium-sized and large provinces.

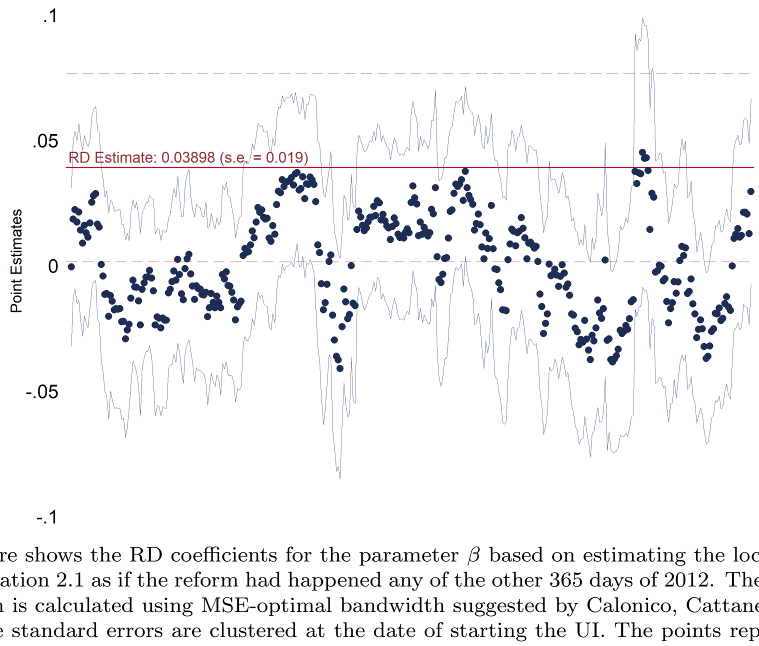
Robustness Checks

Figure A.6: Alternative Bandwidths



Note: This figure shows the coefficients of the parameter β from estimating equation 2.1 using as outcome variable an indicator that takes the value 1 if workers have changed province, and 0 otherwise. The coefficients are estimated using a local linear model with different bandwidths: from 7 days around the reform to 365 days. The standard errors are clustered at the day of entry in the UI. The solid blue lines represent the 95 percent confidence interval around the coefficients. These results show that the findings in section 2.5.2 are not due to bandwidth selection.

Figure A.7: Placebo Test: Artificial Reform Dates



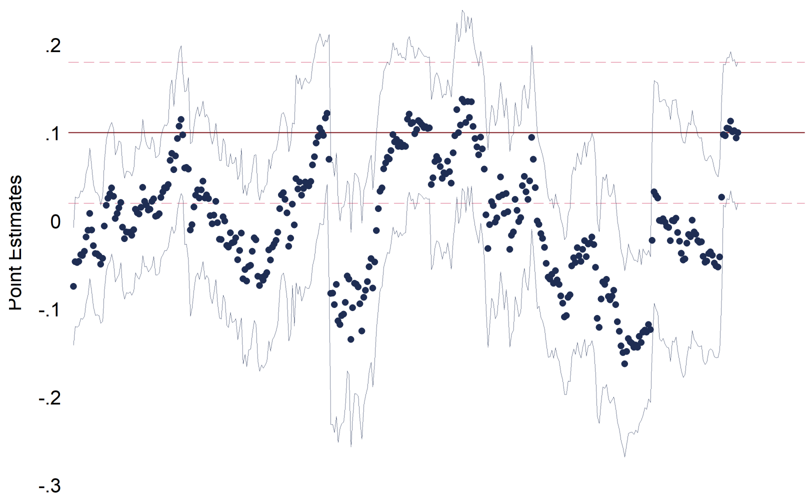
Note: The figure shows the RD coefficients for the parameter β based on estimating the local linear model specified in equation 2.1 as if the reform had happened any of the other 365 days of 2012. The bandwidth for each estimation is calculated using MSE-optimal bandwidth suggested by Calonico, Cattaneo and Titiunik (2014), and the standard errors are clustered at the date of starting the UI. The points represent the estimated coefficients for each of the 365 regressions, and the solid blue lines show the 95 percent confidence intervals around each coefficient estimate. The solid red line represents the estimated coefficient for the parameter β presented column 1 of table 2.1, and the dashed red lines represent the 95 percent confidence interval around such coefficient. There are four specifications out of the 365 in which the coefficient for the parameter β is higher in absolute terms to 0.0389. This placebo test reinforces the results presented in section 2.5.2.

Table A.4: Placebo Test: Non-Affected Workers

Panel A: Entitled to Less than 6 Months of UI						
Bandwidth			Optimal Bandwidth			
Days around the reform	204	217	203	127	229	197
Reform (T_i)	0.02	0.01	0.01	0.01	-0.00	-0.00
	[0.029]	[0.041]	[0.055]	[0.036]	[0.040]	[0.056]
Eff. N	2,750	2,876	2,734	1,637	2,951	2,585
Panel B: No Drop in RR						
Bandwidth			Optimal Bandwidth			
Days around the reform	227	166	214	187	187	221
Reform (T_i)	0.10**	0.14***	0.14**	0.07**	0.08**	0.08*
	[0.041]	[0.054]	[0.064]	[0.028]	[0.036]	[0.044]
Eff. N	4,365	3,252	4,170	3,629	3,629	4,194
Control Function	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
Covariates				✓	✓	✓

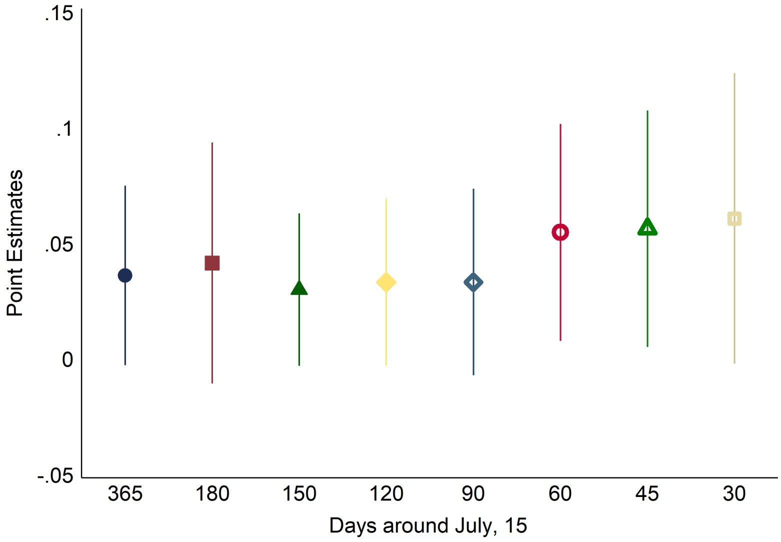
Note: Panel A reports the estimated coefficients for the parameter β based on estimating equation 2.1 for a group of workers entitled to UI from 4 to 6 months. Panel B looks at the effect of the UI cut on the group of workers for whom the RR did not change after the reform. The first column estimates β from the local linear model specified in equation 2.1 using the MSE-optimal bandwidth suggested by Calonico, Cattaneo and Titiunik (2014). Columns 2 and 3 add second and third-order polynomials respectively. Columns 4 to 6 incorporate covariates to the previous specifications. Robust standard errors (in brackets) are clustered at the day of entry in the UI. *** p<0.01, ** p<0.05, * p<0.1

Figure A.8: Placebo Test: Workers whose RR did not Drop



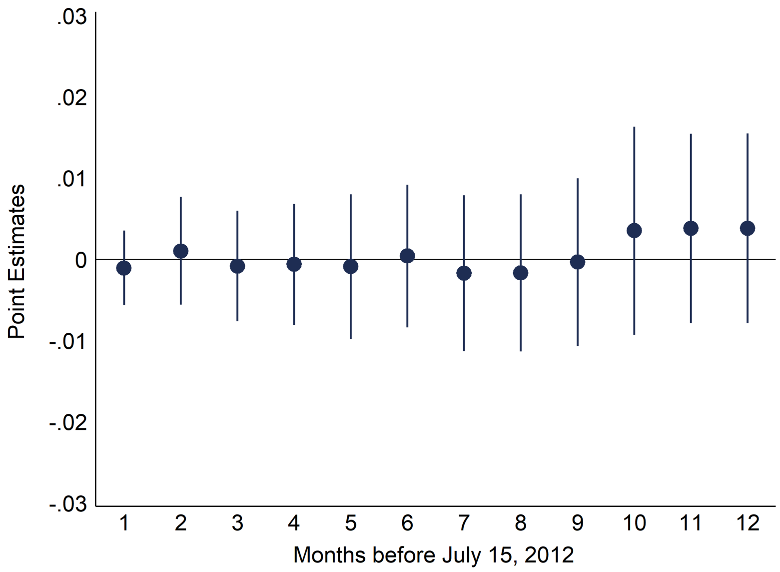
Note: The figure shows the RD coefficients for the parameter β based on estimating the local linear model specified in equation 2.1 as if the reform had happened any of the other 365 days of 2012 for the group of workers whose UI did not drop. There are 57 specifications out of the 365 in which the coefficient for the parameter β is higher in absolute terms to 0.104 in column 1 of table A.4, panel B. This suggests that the estimates in table A.4 should be interpreted with caution.

Figure A.9: Effects of the Reform on Mobility, DiD



Note: The figure shows the estimated coefficients for the parameter β in equation 2.3. The outcome variable is categorical and takes the value 1 for workers who change province during their UI entitlement, and 0 otherwise. Robust standard errors are clustered at the day of entry in the UI. For the regressions where the number of days around the reform are not enough to have several years repeated, I use as a control group the year 2011. This is, the point estimate for 60 days around the reform uses from May 15 to September 15 of both 2011 and 2012. The solid lines represent the 95 percent confidence interval around the coefficients. The results suggest that the findings in section 2.5 are not due to seasonality.

Figure A.10: Effects of the Reform on Pre-Reform Mobility



Note: This figure shows the coefficients of the parameter β from estimating equation 2.1 using as outcome variable the cumulative mobility in the months before the reform was implemented. For example, the last point indicates that there are no statistically significant differences in the mobility rates within the last 12 months for those who were and who were not affected by the UI drop. As in the other specifications, the coefficients are estimated using the MSE-optimal bandwidth suggested by Calonico, Cattaneo and Titiunik (2014), and clustering the standard errors at the day of entry in the UI. The solid blue lines represent the 95 percent confidence interval around the coefficients, and the black horizontal line is 0.

Table A.5: Effect of the Reform on Geographical Mobility...

Panel A						
Outcome	Across Urban Areas					
Bandwidth	MSE Optimal Bandwidth					
Days around the reform	98	150	197	102	147	196
Reform (T_i)	0.06**	0.08**	0.09**	0.06**	0.08**	0.10**
	[0.030]	[0.035]	[0.039]	[0.030]	[0.035]	[0.040]
Eff. N	2,670	4,156	5,666	2,699	4,025	5,585
Panel B						
Outcome	Across CC.AA.					
Bandwidth	Optimal Bandwidth					
Days around the reform	87	138	192	80	138	195
Reform (T_i)	0.04***	0.06***	0.06***	0.05***	0.06***	0.06***
	[0.017]	[0.019]	[0.020]	[0.018]	[0.020]	[0.021]
Eff. N	4,686	7,466	10,572	4,309	7,366	10,597
Panel C						
Outcome	Across Provinces within CC.AA.					
Bandwidth	Optimal Bandwidth					
Days around the reform	226	181	275	167	177	262
Reform (T_i)	0.01*	0.01	0.00	0.01	0.00	0.00
	[0.009]	[0.013]	[0.014]	[0.009]	[0.013]	[0.014]
Eff. N	12,560	10,102	15,320	9,111	9,793	14,497
Control Function	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
Covariates				✓	✓	✓

Note: The outcome variable in panel A is categorical and takes the value 1 if workers have changed urban area during their UI entitlement length, 0 otherwise. Panel B looks at mobility across different CC.AA; and the dependant variable in Panel C takes the value 1 for those workers who change province but not CC.AA., and zero otherwise. All results are calculated using the MSE-optimal bandwidth suggested by Calonico, Cattaneo and Titiunik (2014). Column 1 estimates β from the local linear model specified in equation 2.1. Columns 2 and 3 include higher-order polynomials, and columns 4 to 6 add control variables. Robust standard errors (in brackets) are clustered at the day of entry in the UI. *** p<0.01, ** p<0.05, * p<0.1

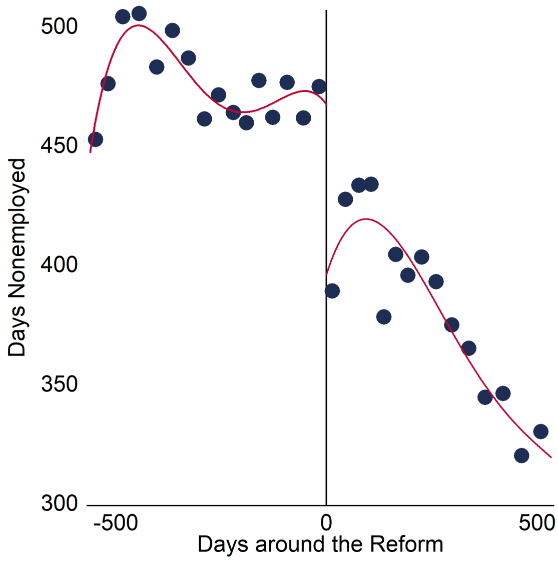
The Effect of the UI Reduction on Nonemployment Duration

Rebollo-Sanz and Rodríguez-Planas (2018) use a difference-in-differences (DiD) strategy to study the causal effect of the 10 p.p. cut in the UI replacement rate on the nonemployment duration and subsequent labor market outcomes. In particular, their study compares workers who start an UI in 2012 −before and after July 15− to similar workers who started a UI at the same time but who were entitled to less than 181 days of UI. Rebollo-Sanz and Rodríguez-Planas (2018) show that reducing the RR by 10 p.p. increases workers' probability of finding a job by 41 percent relative to similar workers who were not affected by the reform (i.e., the reform reduced the mean expected duration of the unemployment spell by 5.7 weeks).

In this section, I aim at replicating these findings using an RD design and a different sample.[46] Thus, I estimate equation 2.1 using as outcome variables the (1) nonemployment duration, and (2) different measures of the post-displacement job match quality.

Figure A.11 and table A.6 present the results of estimating equation 2.1 using as outcome variable the nonemployment length. The findings show that the policy reduced the mean expected nonemployment duration by 88 days. In terms of magnitude, this represents 18 percent of the pre-reform mean (475 days). These results are consistent with the findings in Rebollo-Sanz and Rodríguez-Planas (2018) and with the literature that studies the causal effect of the UI changes on the nonemployment duration (Nekoei and Weber, 2017).

Figure A.11: Effects of the Reform on Nonemployment Duration



Note: The y-axis in figure A.11 represents the days that elapsed since the start of the UI until the beginning of the next job. The x-axis represents the day of entry in UI, normalized to 0 at July 15, 2012. The vertical line represents the day of the policy implementation. The solid lines represent the fitted values based on a fourth-order polynomial without covariates. The IMSE-optimal number of quantile-spaced bins is 16 below the cutoff and 15 above it. The average bin length is around 35 days below the cutoff, and 35 days above it.

[46] I have information until the end of 2017, while Rebollo-Sanz and Rodríguez-Planas (2018) use information up to 2013.

Table A.6: Effect of the Reform on Nonemployment Duration

Outcome	Non-employment Duration					
Bandwidth	Optimal Bandwidth					
Days around the reform	120	160	215	112	159	204
Reform (T_i)	-87.81***	-99.37***	-102.48***	-90.23***	-102.42***	-103.02***
	[23.137]	[29.758]	[34.390]	[23.080]	[28.389]	[33.149]
Control Function	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
Covariates				✓	✓	✓
Eff. N	6,073	8,233	11,395	5,662	8,074	10,807

Note: The outcome variable is the number of days from the beginning of the UI until the next employment spell. All results are calculated using the MSE-optimal bandwidth suggested by Calonico, Cattaneo and Titiunik (2014). Column 1 estimates β from the local linear model specified in equation 2.1. Columns 2 and 3 include higher-order polynomials, and columns 4 to 6 also include controls. Robust standard errors (in brackets) are clustered at the day of entry in the UI. *** p<0.01, ** p<0.05, * p<0.1

There are two potential explanations consistent with the reduction in the nonemployment duration. First, as the opportunity cost of being unemployed increases, workers reduce their reservation wages and increase their job-offer acceptance. All equal, this would imply a reduction in the job-match quality. Second, if the UI was subsidizing leisure, workers may now accept jobs that otherwise will have rejected without decreasing the quality of the matches. To shed some light on this, I estimate equation 2.1 looking at different measures of job-match quality (i.e., wages, tenure in the next job, and occupation). Table A.7 presents the results. They show that the reduction in the nonemployment duration did not affect the job-match quality. These findings, consistent with the evidence in Rebollo-Sanz and Rodríguez-Planas (2018), suggests that the UI system in Spain is partially used to subsidize unproductive leisure.

Table A.7: Effect of the Reform on Subsequent Labour Market Outcomes

Outcome	Wages		Duration		Occupation	
Bandwidth	Optimal Bandwidth					
Days around the reform	167	152	138	125	209	126
Reform (T_i)	-37.73	-48.49	-42.27	-66.45*	-0.03	-0.01
	[29.762]	[40.889]	[30.078]	[38.177]	[0.018]	[0.028]
Control Function	Linear	Linear	Linear	Linear	Linear	Linear
Covariates		✓		✓		✓
Eff. N	8,711	7,755	7,053	6,256	9,680	5,480

Note: Table A.7 looks at how the policy affected workers' subsequent labor market outcomes in terms of wages (columns 1 and 2); duration of the next employment (columns 3 and 4); and occupation (columns 5 and 6). All results are calculated using the MSE-optimal bandwidth suggested by Calonico, Cattaneo and Titiunik (2014). For each dependent variable, the first column estimates β from the local linear model specified in equation 2.1, and the second adds covariates. Robust standard errors (in brackets) are clustered at the day of entry in the UI. *** p<0.01, ** p<0.05, * p<0.1

Finally, figures A.12a and A.12b look at the effect of the reform on the probability of becoming a long-term unemployed worker. To look at this, I measure how the UI cut affected the probabilities of spending more than one year and more than two years out of employment. Importantly, before the reform, 48(28) percent of the sample were unemployed for more than one (two) year(s). As Schmieder, Von Wachter and Bender (2012) state, the studies that look at the short term effects of the UI may be underestimating the true costs of the UI generosity if it increases the incidence of long-term unemployment.

Figures A.12a and A.12b and table A.8 show that the UI cut remarkably decreased the incidence of LTU. Namely, the UI cut reduced the probability of spending more than one (two) year(s) out of employment by 9 (7) percentage points. In terms of magnitude, this represents 19 (28) percent decrease of the pre-reform means. This result is in line with Bentolila, Pérez and Jansen (2017). They find that workers entitled to receive unemployment benefits are more likely to become LTU. The results also go in line with the models of stigma, skill depreciation, and supply-side hysteresis. Given that more than half of the unemployed workers in 2018 in Spain were long-term unemployed, I consider this result especially relevant.

Figure A.12: Effect of the Reform on Labor Market Outcomes

(a) LTU (> 1 year) (b) LTU (> 2 years)

Note: The y-axis in figures A.12a and A.12b represents the percentage of workers who are long-term unemployed (defined as those who are nonemployed for more than 1 and 2 years, respectively). The x-axis in all figures represent the day of entry in UI, normalized to 0 at July 15, 2012. The vertical lines represents the day of the policy implementation. The solid lines represent the fitted values based on a fourth-order polynomial without covariates. The IMSE-optimal number of quantile-spaced bins in each of the figures is 21 and 24 bins below the cutoff and 12 and 12 above it. The average bin length is around 27 and 23 days below the cutoff, and 44 and 44 days above it.

Table A.8: Effect of the Reform on the LTU

Panel A						
Outcome	Long Term Unemployment (> 1 year)					
Bandwidth	Optimal Bandwidth					
Days around the reform	133	183	186	127	187	169
Reform (T_i)	-0.09***	-0.09***	-0.07	-0.09***	-0.09***	-0.04
	[0.028]	[0.034]	[0.045]	[0.027]	[0.031]	[0.040]
Eff. N	7,138	10,191	10,350	6,694	10,275	9,177
Panel B						
Outcome	Long Term Unemployment (> 2 year)					
Bandwidth	Optimal Bandwidth					
Days around the reform	146	156	200	144	149	192.
Reform (T_i)	-0.07***	-0.09***	-0.10***	-0.07***	-0.09***	-0.10***
	[0.021]	[0.028]	[0.033]	[0.019]	[0.026]	[0.030]
Eff. N	8,029	8,496	11,228	7,820	8,051	10,444
Control Function	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
Covariates				✓	✓	✓

Note: The outcome variable in panel A (B) is categorical and takes the value 1 if the worker spends more than 1 (2) year(s) out of employment, and 0 otherwise. All results are calculated using the MSE-optimal bandwidth suggested by Calonico, Cattaneo and Titiunik (2014). Column 1 estimates β from the local linear model specified in equation 2.1. Columns 2 and 3 include higher-order polynomials, and columns 4 to 6 also include controls. Robust standard errors (in brackets) are clustered at the day of entry in the UI. *** p<0.01, ** p<0.05, * p<0.1

Chapter 3

Family Types and Migration: Evidence from Spain

3.1 Introduction

There are sizeable differences in labor market outcomes between and within countries (Blanchard et al., 1992). However, mobility flows are negligible. According to the United Nations, only 272 million persons were living outside their country of birth in 2019, −3.5 percent of the total population−. Besides, in a survey carried out by Gallup in 2013, just eight percent of the world's adults declared they have internally relocated in the past five years (Esipova, Pugliese and Ray, 2013).[1]

Previous literature looking at the determinants of migration shows that cultural and language differences (Levinsohn, 2007; Adsera and Pytlikova, 2015), labor regulations (Bertola, 1999; Hassler et al., 2005), legal restrictions to migration (Hanson, 2009), geographical-specific human capital accumulation (Borjas, Bronars and Trejo, 1992; Bloomfield et al., 2017), or family ties (Huttunen, Møen and Salvanes, 2011; Alesina et al., 2015) may discourage labor relocation. Yet, a missing piece in this literature is the potential effect of past institutions on today's mobility decisions.[2]

This chapter studies how the historically predominant family organizations affect today's mobility decisions in the context of Spain. In particular, this

[1] In particular, the estimations in Esipova, Pugliese and Ray (2013) suggest that just 381 million people changed of city or municipality within their country between 2008 and 2012.

[2] The role of historical institutions on different outcomes such as economic development (Acemoglu, Johnson and Robinson, 2001; Guiso, Sapienza and Zingales, 2006), income inequality (Acemoglu, Johnson and Robinson, 2002), fertility decisions (Fernandez and Fogli, 2009), or gender violence (Tur-Prats, 2019), has been documented in previous literature.

chapter focuses on two types of families: *egalitarian nuclear* and *stem*. Two defining principles separate these two different structures: the inheritance systems and the intergenerational house-sharing norms. In stem societies, a child (usually the first-born son) inherits all the family wealth and remains in the parental house with his wife to continue the family line. Contrary, in egalitarian nuclear societies, children receive an even share of the family wealth and leave their parental house at adulthood.

The hypothesis in this chapter is that individuals born in societies in which stem families were socially predominant in the past are less likely to move nowadays. According to Salamon (1982), stem family structures favored stronger family ties, not only through intergenerational house-sharing but also via indivisible inheritance (Bras and Van Tilburg, 2007).[3] If family culture persists over time and organizational changes (Farre and Vella, 2013), we should expect higher mobility costs −and lower relocation rates− in societies where stem families were historically predominant.[4] This argument is consistent with the anecdotal evidence I present below.

Figure 3.1 shows the correlation between family structures in the past and family ties in the present in Spain, and between family ties and mobility rates in the present. Figure 3.1a shows a positive correlation between the importance individuals claim to give to their families and historically predominant stem family structures. This positive association is consistent with the view that culture and institutions are transmitted over generations and their effects are long-lasting (Duranton, Rodríguez-Pose and Sandall, 2009; Fernandez and Fogli, 2009; Farre and Vella, 2013). In addition, figure 3.1b shows a negative correlation between today's family ties and mobility decisions (Alesina et al., 2015).

To empirically test the hypothesis that past family structures affect today's mobility, I exploit the regional variation in historical family types within Spain. Following Todd (1990), Peña (1992), or Tur-Prats (2019), I measure the family organization calculating the average number of widowed and married women per household in 1860 at the provincial level. As coresidence was the norm in stem families, higher numbers of widowed and married women per house suggest a stem family structure, while lower numbers indicate nuclear egalitarian organizations. To look at internal mobility, I use individual data from the Continuous Sample of Working Histories (MCVL).[5] I measure

[3]Researchers justify stronger ties between siblings in indivisible inheritances by arguing that divisible inheritance laws created intra-family tensions associated with strives.

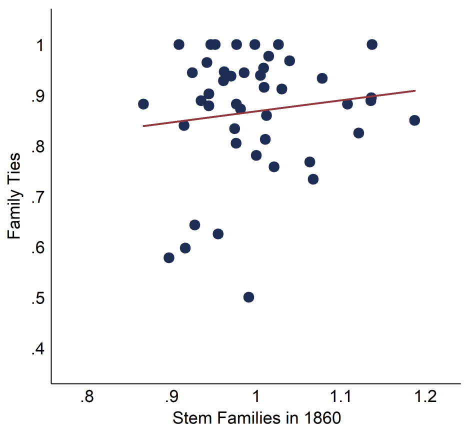
[4]Albeit past family types changed after the Industrial Revolution, they were quite stable between the Middle Ages and the second half of the XIX century (Reher, 1998).

[5]The MCVL is an administrative data with information on a non-stratified random 4

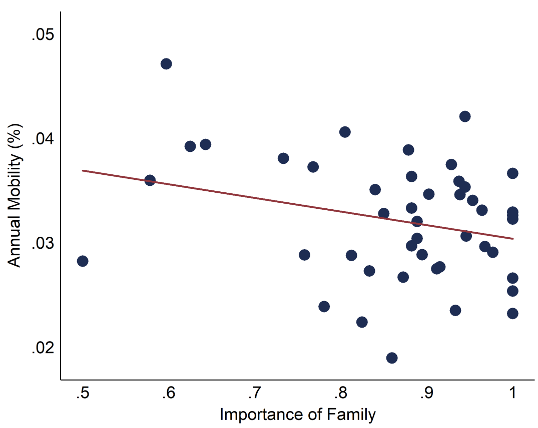
mobility as changes in provinces between two consecutive years. The results of estimating a linear probability model (LPM) suggest that people who were born in provinces with historically predominant stem families are less mobile nowadays. In particular, going from the province where the egalitarian nuclear family was the most socially predominant (Ourense) to the province with the most stem family structure (Huesca) is associated with a decrease in mobility of 1.28 percentage points, which represents 42 percent percent of the sample mobility average. This result is robust to the inclusion of an exhaustive set of individual and provincial controls.[6]

Figure 3.1: Family and Mobility

(a) Family Ties and Family Types



(b) Family Ties and Mobility



Note: The x-axis of figure 3.1a measures family types in 1860 using the average number of married and widowed women per household at the provincial level (Todd, 1990). The y-axis measures the proportion of interviewed individuals who answered that family was very important to them in the 2014 CIS survey of *Opiniones y actitudes sobre la familia.* Figure 3.1b shows a negative correlation between the proportion of people who consider family as very important (x-axis) and the rate of mobility across provinces (y-axis).

A potential concern with the previous estimates is that societies might have organized their family structures based on their attitudes towards migration and family responsibilities. For example, if people who wanted to have their family members nearby or with worse attitudes towards geographical mobility were more likely to establish stem families, the previous LPM estimates would be biased upwards. Alternatively, if societies who wanted to extend the family to other territories to preserve and expand the family name established stem families, the LPM estimates would be biassed towards zero. Besides, there might also be unobserved variables that affected both the family structures

percent sample of the population who, on a given year, have any relationship with the social security in Spain.

[6]Spain consists of 52 provinces (NUTS 3 level), with an average population of 0.9 million inhabitants.

and mobility decisions. In order to rule out further concerns about reverse causality and potential omitted variables, I follow Tur-Prats (2019) and use the inheritance laws that originated in the Reconquista as an instrument in a Two-Stage Least Squares (2SLS) estimation framework. The results from instrumenting family types in 1860 with the inheritance laws derived from the Reconquista are consistent with the LPM estimates.

To understand the underlying mechanisms of this negative relation, the last part of this chapter looks at the persistent effects of the historically predominant family structures on family ties and responsibilities. I find that people born in provinces with a higher intergenerational house-sharing in 1860 still declare to give more importance to the family and to family responsibilities than those who were born in provinces that tended to have an egalitarian nuclear family structure (Galasso and Profeta, 2018). Albeit nowadays stem families are non-existent in Spain, these findings are consistent with the extensive literature showing that social norms are sticky (Alesina et al., 2015).

This chapter contributes to three strands of the literature. First, it relates to the literature studying people's mobility decisions. In particular, this study contributes to the scarce literature aiming at analyzing the family-related roots of migration at origin (rather than at the destination).

Second, it contributes to the recent literature that looks at the role of the family as an important institution affecting economic outcomes. Duranton, Rodríguez-Pose and Sandall (2009) use Todd's classification of family types and identify strong correlations between historical family organizations and regional disparities in household size, educational attainment, social capital, labor participation, sectoral structure, wealth, and inequality. Alesina and Giuliano (2010) look at the behavior of second-generation immigrants and show that persons who came from societies with strong family ties devote more time to home-production, have larger families, lower female and young labor force participation, and lower geographical mobility. Alesina and Giuliano (2011) show, using within-country variation and second-generation immigrants, that individuals coming from societies with strong family ties have lower levels of trust and political engagement. Alesina et al. (2015) find that in countries with strong (weak) family ties, individuals are less (more) mobile and prefer a more (less) regulated labor market. Galasso and Profeta (2018) show that the established retirement systems mimic family support. In societies with historically weak family ties, the pensions act like a safety net, while societies with strong family ties prefer to rely on the government as a provider of old age security through generous retirement benefits.

Finally, this chapter also contributes to the literature studying the consequences of the Reconquista on today's economic outcomes in Spain. The closest study to this chapter is Tur-Prats (2019). She claims that the Reconquista determined family types in Spain through its effects on the land ownership structure and inheritance laws. Using the Reconquista to instrumentalize the family organization, Tur-Prats (2019) finds that territories where the stem family was socially predominant in the past exhibit a lower level of gender violence today. She argues that sharing the house with the mother-in-law reduced the wife's burden of domestic work and increased wife's participation in agricultural production. In this context, husbands reduce violence against their wives to avoid reductions in their production of non-domestic work. Oto-Peralías and Romero-Ávila (2016) show the existence of a strong positive correlation between the "speed of Reconquista" and the degree of structural inequality across provinces. They argue that the fast reconquest of the territory in the first stages of the Reconquista led to the concentration of power in a few hands (nobility). This initial inequalities persisted over time and are still observed nowadays. Finally, Tapia and Martinez-Galarraga (2018) also show that the land access inequality derived from the Reconquista has shaped regional differences in literacy rates. Namely, they find that a ten-point increase in land access inequality reduces male and female literacy rates by 8.5 and 1.3 percentage points, respectively.

The remainder of the chapter proceeds as follows. Section 2 presents the historical classification of family types. Section 3 describes the data used in this chapter. Section 4 outlines the empirical strategy used to identify the effect of interest. Section 5 discusses the results of the econometric analysis and presents some robustness checks. Section 6 concludes.

3.2 Institutional Setting

3.2.1 Family Types

According to Todd (1990), family structures depend on two organizing principles: the vertical relation between parents and children (liberal or authoritarian), and the horizontal relation between siblings (equal or unequal). A relation is considered to be *liberal* if children leave their parental house and become independent when they reach adulthood. Contrary, a relation is *authoritarian* if children still depend on their parents in their adulthood, and even after getting married. Regarding the relation between siblings, in an *egalitarian* society, parents leave a roughly equal share of the family wealth to their children, while in an *unequal* relation, parents leave all the family wealth to one of the children (usually the first-born son).

To measure the vertical relation, Todd (1990) looks at cohabitation patterns between parents and their married children. In societies with authoritarian families, several generations live in the same household, and all children remain under the authority of the father. On the contrary, in liberal families, children leave the parental household and become independent when they reach adulthood. Thus, households with larger numbers of adults are associated with authoritarian families. To measure the horizontal relation between siblings, Todd (1990) looks at the inheritance laws. In some areas, parents had freedom of testation, while in others, they were obligated to divide equally the family wealth among their children or to select an only heir.

The combination of these four family relations leads to the definition of four family types (see table 3.1). In the *absolute nuclear* type of family, children emancipate from their parents when they reach adulthood to form their independent families. When the parents die, one individual, usually the oldest son, inherits all the family wealth. Areas with a predominant *egalitarian nuclear* type of family are characterized by the emancipation of children once they become adults and the even distribution of the family wealth among all descendants. The third type of family, *stem families*, consists of several generations of the family living together. The most common case is a household composed of the parents, the oldest son −who is the main or absolute recipient of the heritage−, the other unmarried children, and the wife and kids of the oldest son. The idea of this type of family is to preserve the lineage. Finally, in *extended families*, parents and children (and children's partners and kids) live in the family house. When the parents die, all children receive an even proportion of the family wealth.

Table 3.1: Family Types

		Vertical Relation	
		Liberal	**Authoritarian**
Horizontal Relation	**Equal**	Egalitarian nuclear	Extended family
	Unequal	Absolute nuclear	Stem

Note: Family types according to Todd (1990) classification.

Todd (1990) tracks these different family structures to the Middle Ages. In his work, he shows that the organization of the family is very stable in time. Namely, he finds few variations between the family structures in the

XIII century and the ones in the 1950 census. In a recent work, Duranton, Rodríguez-Pose and Sandall (2009) update Todd's regional classification of family types to the new territorial distributions of Europe. The map, in figure A.1, shows important heterogeneities in family organizations within and across countries. For example, in southern and eastern England, eastern Scotland, north-west France, Holland, Denmark, and southern Norway, the absolute nuclear family was dominant. Areas with prevalent absolute stem families were located in the west of the UK, northern Spain, south-western France, most of Germany, Austria, German-speaking Switzerland, southern Sweden, and coastal Finland. Egalitarian nuclear families were strongest in northern and eastern France, most of Spain, and southern and north-western Italy. There are also some areas with incomplete stem families (i.e., stem families with more egalitarian inheritance laws) in Belgium or Luxembourg. Communitarian families were only present in some areas of central Italy and large parts of the interior of Finland. In the next section, I focus on the family organization in Spain.

3.2.2 Family Types in Spain

According to historians and anthropologists, only two of the four family structures were present in Spain (Lisón Tolosana, 1976): the stem and the egalitarian nuclear. In stem areas, the oldest son (and his wife and children) lived with his parents and inherited all the family wealth. The heir had an obligation to look after his parents and siblings. The purpose of this family organization was to guarantee the continuity of the family house and the family name to the next generations. In egalitarian nuclear families, all children left the house at adulthood, and the family wealth was equally divided among descendants. However, at least in Spain, parents could reward a particular child with an extra amount of heritage.

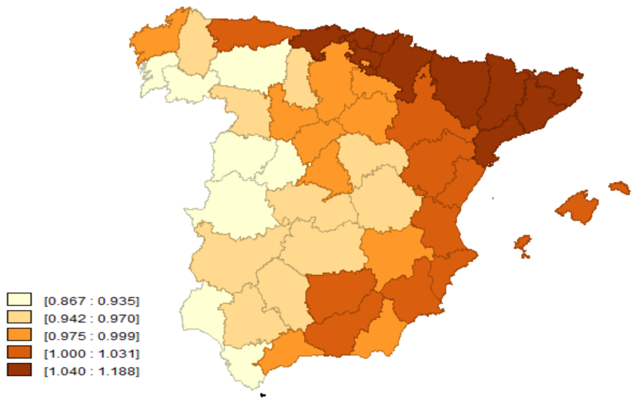
Figure 3.2 shows the geographic distribution of family types in Spain. Following Todd (1990), I define family types looking at the average number of married and widowed women per household. In this exercise, I use the 1860 census.[7] We can see that the stem families were especially predominant in the north and east of Spain.

In figure 3.3, I show the family distributions according to Mikelarena and Pérez-Fuentes (2001). In this case, stem (nuclear) families are those with more than 1.075 (less than 1.000) married and widowed women per household. We

[7]According to Peña (1992), the 1860 census is the most reliable between 1857 and 1930 to measure family structures.

can see that under both definitions, the classification of family types is similar to Todd's division in figure A.1.[8]

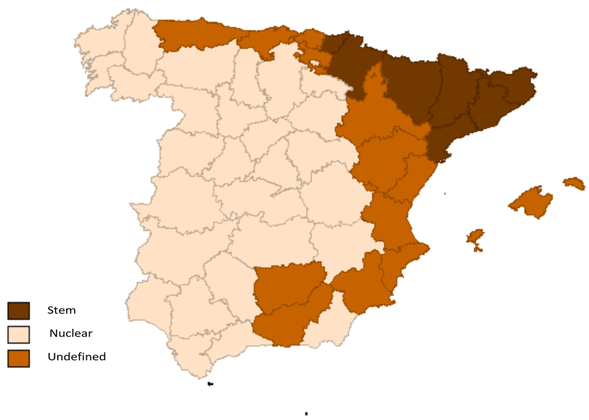
Figure 3.2: Married and Widowed Women per Household in 1860 (I)



Source: 1860 Census

If we look at today's family organization, the 1860 census does not seem representative. This is related to the structural transformations associated with the industrialization, demographic changes, and migration towards the cities that happened during the XX century. However, Todd (1990) and Reher (1998) show that the family types in Europe and Spain were stable from the Middle Ages to the second half of the XIX century. Therefore, albeit the 1860 census may not reflect today's family organization, it is informative about the historically predominant family structures in the different Spanish provinces.

Figure 3.3: Married and Widowed Women per Household in 1860 (II)



Source: 1860 Census

[8]The only differences are Galicia and Valencia. The results are robust to eliminate these two regions.

3.3 Data

This chapter uses data from several sources. Below, I describe them in detail.

MCVL

The Continuous Sample of Working Histories (*Muestra Continua de Vidas Laborales* or MCVL) is a microlevel data set provided by the Spanish Ministry of Employment and Social Security since 2004. Each annual wave contains a 4 percent non-stratified random sample of all individuals who have any contact with the Social Security Administration (SSA) during at least one day in the sampled year. For this chapter, I combine data from the 2005 - 2017 editions of the MCVL.[9]

The MCVL has a longitudinal design. All persons selected in 2004 who remained in contact with the Social Security Administration at least one day in the following years continue in the subsequent editions. In addition, individuals' labor market histories are retrieved to the moment they have entered the labor market (or 1967 for earlier entrants).

For each person, there is information available on the entire employment history, including the exact duration of each employment and unemployment spells, as well as monthly earnings. A crucial feature of the MCVL for this analysis is that there is data on the municipalities where people have their relationship with the social security (be that the location of the firm where they are working, or the area where they are enrolled as recipients of a contributory pension).[10] In addition, the data include individuals' personal characteristics such as age, gender, nationality, province of birth, level of education, or the number of people living in the same household.

For this project, I pool individuals who have a relation with the SSA at least one day in the years 2005, 2010, or 2015. For each year, I assign each sampled person to the province where they received the most income. Related to the sample selection, I drop those workers who were born outside of Spain or for whom there is no information about their birth province, as I cannot associate them with a measure of family types. Finally, I also eliminate individuals who are younger than 16 or older than 65. Table A.1 shows the descriptive

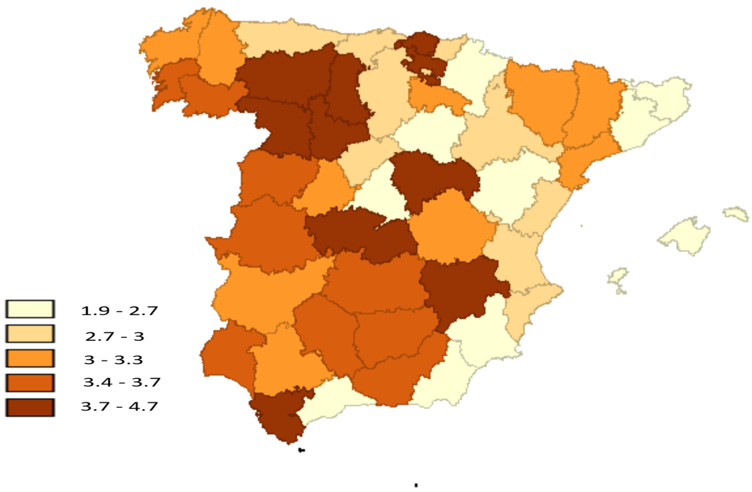
[9]The condition for being included in the sample (i.e., having contact with the social security at least once per year) may create some risks of sample selection for women, immigrants, and young workers (see García-Pérez, Castelló and Marinescu (2016)). Combining 13 waves helps to minimize such risk, as now I have people who have at least one day of relation with the social security in any of the 13 years.

[10]Given that there is only information at the municipal level for municipalities with more than 40,000 inhabitants, I will do this analysis at the provincial level.

statistics for the variables I use in the analysis.[11] The main outcome variable is mobility. It measures the percentage of individuals who have changed province between year t and year $t-1$. We can observe that the average mobility rate is 3 percent. Figure 3.4 shows important differences in relocation rates across provinces. For example, while almost 5 percent of the population born in Cádiz changed province between years t and $t-1$, the annual mobility rates among persons born in the Illes Balears was 1.8 percent.

Regarding the control variables, 56 percent of the sample are men. The average age is 41, and the percentage of workers with primary education is 54 percent. Twenty-six percent of the individuals have secondary education, and 20 percent hold a university degree. The average person in the sample lives with other two individuals and has no children living in the household. In addition, her/his annual earnings are around €16,607, and he/she works 308 days per year.

Figure 3.4: Mobility Flows per Province of Birth



Note: Annual mobility flows in Spain from the MCVL

1860 Census

To measure family structures, I use the 1860 census.[12] This census contains detailed information on the population registers at the municipal and provincial level desegregated by gender, age, and civil status.

Overall, the average province in 1860 had 319,281.3 inhabitants (s.d. = 131,266.6). The smallest province –Álava– had 97,934 inhabitants, while the

[11] I end up with 883,218 different persons and 1,972,483 observations.

[12] According to Peña (1992), from 1857 to 1930, the 1860 census is the most proper to measure the structure and composition of households.

largest −Barcelona− had a population above 700,000 persons. As we can see in tabla A.1, the average number of married and widowed women per household was 1.01 (s.d. = 0.07). The province with the most nuclear family structure (i.e., the province with the lowest number of married and widowed women per household) was Ourense, in the north-west of Spain. On the other hand, Huesca, in the north-east, was the province with the highest number of married and widowed women per household (i.e., the area with the most stem family structure). In figure 3.2, I show the distribution of family structures in Spain according to the 1860 census.

CIS

To measure the importance of family and family responsibilities in the present, I use data from the Centro de Investigaciones Sociológicas or CIS. In 2014, the CIS conducted a survey called *"opinions and attitudes towards the family"* on 2,464 representative individuals. To look at family ties, I use the following question: (1) How important is family to you? Respondents could answer (a) very important, (b) rather important, (c) not very important, or (d) not important at all. As we can see in table A.2, 86 percent of the sample considered that family was very important, and 99 percent believed that family was very or rather important.

Table A.3 shows the summary statistics for different measures of family responsibilities. First, I look at the statement *"When people cannot look after themselves, it is preferable to rely on Social Services than on family"*. Around 27 percent of the sample completely agreed or agreed with that statement, while 60 percent disagreed or completely disagreed with it. Moving to the next question, 51 percent of the people answered that they completely agreed or agreed with the statement *"It is better for children to go to the kindergarten than to be left with their grandparents or other relatives"*, while 32 percent stated that they disagreed or completely disagreed with such a statement. Related to the preferences regarding the care of the elderly relatives, 70 percent agreed or completely agreed that close family should take care of them, while 10 percent disagreed or completely disagreed. In addition, 72 percent of the sample completely agreed or agreed with the statement *"Older people should enjoy their money rather than leave it to their children"*; and 43 percent completely disagreed or disagreed with the statement that *"Parents should not sacrifice their own lives for their children"*.

Apart from information on family values and opinions, the survey also provides data on participants' demographic characteristics. Fifty percent of the individuals are men. The average age is 48 years, 25 percent have primary education, 55 percent have secondary education, and 20 percent have a university degree. In addition, 53 percent of the sample are married, and 43 percent were working at the moment of the interview.

3.4 Methodology

I start the analysis by estimating the following Linear Probability Model (LPM).

$$Y_{i,p,t} = \alpha + \beta Stem_p + \delta X_{i,p,t} + \gamma Z_{p,t} + \theta_t + \epsilon_{i,p,t} \tag{3.1}$$

where $Y_{i,p,t}$ is a categorical variable that takes the value 1 if individual i, born in province p, was living in a different province in the year $t-1$ with respect to his/her province of residence in the year t, and 0 otherwise.[13] $Stem_p$ is defined using the average number of married and widowed women per household in the province individual i was born based on the 1860 census (see Peña (1992) and Tur-Prats (2019)).[14] $X_{i,p,t}$ is a vector of control variables at the individual level that includes gender, age, age squared, level of education, total number of people per household, children younger than 4 at the household, and earnings and days worked in the year t. $Z_{p,t}$ are regional variables at the province of birth level, including the unemployment rate, and the GDP per capita. I also control for year fixed-effects θ_t. Finally, standard errors are clustered at the province of birth level.

A potential concern with the previous LPM estimates is that societies might have organized their family structures based on their attitudes towards migration (Derouet, 1989). For example, it could be that people who wanted to have their family members closer or with worse attitudes towards geographical mobility were more likely to establish stem families. Clearly, this would violate the identifying assumption, and the LPM would overestimate the effect of family types on migration. In addition, there might also be unobserved variables that affect both family structure and mobility decisions. In order to rule out further concerns about potential omitted variables and reverse causality, I follow Tur-Prats (2019) and use the inheritance laws that originated in the Reconquest as an instrument in a Two-Stage Least Squares (2SLS) estimation framework.

[13] $t \in \{2005, 2010, 2015\}$

[14] The results are robust to use the number of married and widowed people per household and the average number of individuals per household.

In particular, Tur-Prats (2019) argues that family types in Spain were determined by the Reconquista via its effect on the land ownership structure and inheritance laws. The more centralized kingdoms in the west of Spain enforced the sharing of the inheritance among children to avoid the expansion of influential landholding families. This led to nuclear families. The exception was in some provinces in the north, were the Christian resettlement of conquered land established small properties held by laborers. These properties needed to remain undivided to be sustainable. This led to the adoption of indivisible inheritance, and thus, to stem families. The eastern monarchies -with a more powerful feudal nobility who wanted to maintain its landholdings undamaged- also enforced indivisible inheritance, which led to stem families. Albeit these inheritance systems are not (*as*) relevant nowadays, their impact on the organization of the family structures is long-lasting (Goody and Goody, 1976).

To quantify this instrument, I follow Tur-Prats (2019), and define a categorical variable $-II_p-$ that takes the value 1 for those provinces that had indivisible inheritance in the XIII century, and 0 for those where the inheritance was equally divided among children (see figure A.2).[15] Equation 3.2 presents the first stage.

$$Stem_{i,p,t} = \alpha + \zeta II_p + \delta X_{i,p,t} + \gamma Z_{p,t} + \theta_t + \xi_{i,p,t} \tag{3.2}$$

For the instrument to be valid, the estimated coefficient for the parameter ζ must be able to explain the effect of the inheritance laws in the medieval era in province p on p having a predominant steam family structure in 1860. In addition, the instrument must only affect the probability of moving through its impact on the family formation. In the next section, I will look at the plausibility of these assumptions.

3.5 Results

The main purpose of this chapter is to establish the causal relationship between the historically predominant family structures in the past and today's mobility decisions. The hypothesis is that societies in which stem families predominated (i.e., those with indivisible inheritance systems and with several generations sharing the household) are less mobile today. The main idea behind this is

[15] Alicante, Barcelona, Bizkaia, Castellò de la Plana, Girona, Huesca, Illes Balears, Lleida, Navarra, Tarragona, Teruel, València, and Zaragoza are the provinces with indivisible inheritance in the XIII century.

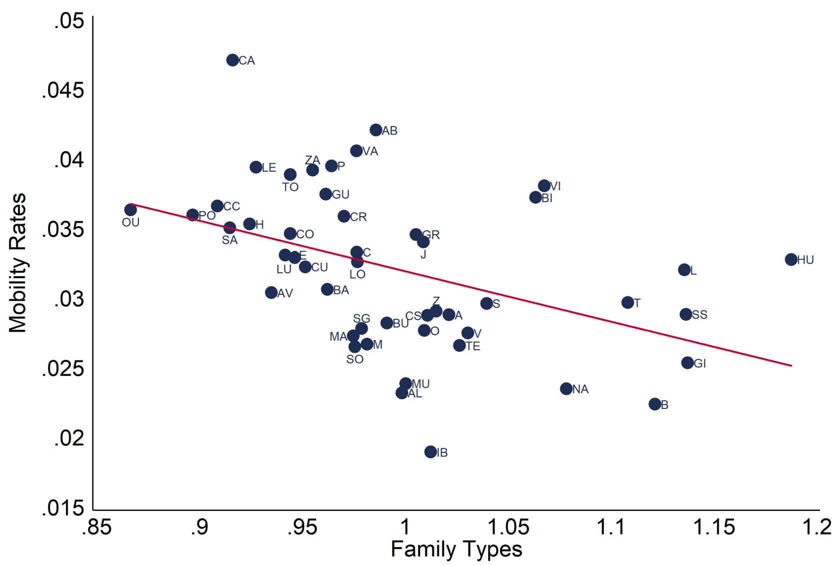
that those societies developed stronger family ties, which still may impact individuals' actual decisions. The next sections aim at answering this research question.

3.5.1 Descriptive Evidence

First, figure 3.5 shows the association between today's mobility rates and past family organizations. In the vertical axis, we can see the percentage of people who changed of province between years t and $t-1$. On average, mobility rates are low: only around 3 percent of the sample geographically relocated. In the horizontal axis, we can see the average number of married and widowed women per household in 1860. Higher numbers are associated with stem families, as they represent several generations sharing the household.

Figure 3.5 shows a strong negative correlation between stem families in the past and nowadays' migration. However, we cannot extract any causal conclusion from this graph (e.g., it could be that provinces where stem families were socially predominant in the past are richer nowadays). In the next sections, I try to deep more into the relationship between these two variables.

Figure 3.5: Mobility Flows and Family Types



Note: The y-axis measures the percentage of individuals who changed the province between years t and $t-1$. The x-axis measures the number of married and widowed women per household at the provincial level in 1860. We can observe an important negative correlation between stem families in the past and mobility rates nowadays.

3.5.2 LPM Results

Table 3.2 shows the results of estimating equation 3.1. The estimated coefficient for the parameter β shows that being born in a province where the predominant family structure in 1860 was stem is associated with less geographical mobility nowadays. More specifically, column 1 shows that if the average number of married and widowed women per household in 1860 increases by 1 unit, the mobility rates decrease 5 percentage points. The results remain stable after the introduction of individual and regional characteristics, and year of reference fixed-effects. The findings in the preferred specification (column 4) show that going from the province where the nuclear family was the most socially predominant (Ourense) to the province with the most stem family structure (Huesca) is associated to a decrease in mobility by 1.28 percentage points, which represents 42 percent percent of the sample mobility average.[16] The last column in table 3.2 also includes regional fixed-effects. In line with the previous findings, people born in provinces with historically predominant stem families are less likely to move. Yet, this result is no longer statistically significant (p-value = 0.24). As figure 3.2 shows, there is not much variation in the family structures at the intra-regional level.[17]

I also show in the table A.5 the results of estimating equation 3.1 using the definition of stem families proposed in Mikelarena and Pérez-Fuentes (2001) (see figure 3.3). The findings show that mobility rates in those provinces characterized by stem families in the past are lower in comparison with those where the nuclear egalitarian organizations were predominant. Table A.6 and table A.7 of the Appendix indicate that the negative correlation between stem families and migration is present using alternative definitions of family structures such as the average number of people per household or the average number of married and widowed persons per household (albeit the results are no longer statistically significant). However, as Peña (1992) argues, these two measures may be less appropriate as they do not account for fertility differences or immigration.

Tables A.12 and A.13 show the results of estimating equation 3.1 looking

[16] Albeit not reported, the other variables impact mobility decisions accordingly to the previous migration literature: men and young persons are more likely to move; individuals with primary and secondary education are less likely to migrate than those with tertiary education. Living with more people, −and especially with children who are younger than 4 years−, reduces mobility, as well as the GDP per capita of the province of residence.

[17] Table A.4 shows the results of estimating equation 3.1 using data from the LFS rather than the MCVL. The findings are the same in terms of sign and magnitude. This is, those people born in provinces where stem families were socially predominant in the past are less likely to move nowadays.

at mobility in the past five years.[18] The results are robust to the change in the dependent variable, and they show that moving from the province with the most nuclear family structure to the one with the most stem family organization in 1860 is correlated with a decrease in today's mobility of 3.2 percentage points (i.e., 42 percent of the average five-years mobility in the sample). Similar results are found when the dependent variable is a binary variable that takes the value 1 if individual i is living outside her province of birth in year t, and 0 otherwise (see A.18).

Table 3.2: Stem Families and Geographical Mobility: LPM Results

Outcome	Mobility across Provinces between t and $t-1$				
	(1)	(2)	(3)	(4)	(5)
$Stem_p$	-0.05***	-0.07***	-0.04**	-0.04***	-0.03
	[0.012]	[0.016]	[0.018]	[0.016]	[0.025]
Individual Controls		✓	✓	✓	✓
NUTS 3 Controls			✓	✓	✓
Year FE				✓	✓
NUTS2 FE					✓
Observations	1,972,483	1,896,700	1,896,700	1,896,700	1,896,700
R^2	0.00	0.03	0.03	0.03	0.03
Mean Dependent Variable	3%	3%	3%	3%	3%

Note: The dependent variable is binary and takes the value 1 if the person has changed province between the year t and the year $t-1$. The model in column 1 only includes the key predictor: the predominant family structure in the past. Particularly, the stem family is defined as the average number of married and widowed women in the household at the province level in 1860. The model in column 2 also includes individuals' characteristics (i.e., gender, age, age square, education, number of people sharing the household, children younger than 4 in the household, and days worked and earnings in year t). Column 3 includes GDP per capita and the unemployment rate in the province of birth. The model in column 4 also includes year fixed-effects (being the years 2005, 2010, and 2015). Finally, the model in column 5 adds regional fixed-effects. Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Overall, the results in this section suggest that people born in provinces where stem families where historically predominant are less likely to relocate nowadays.

3.5.3 IV Results

Main Results

The results from the second-stages regressions are presented in table 3.3.

[18] This new definition of the dependent variable implies that, for each year, I only keep the people that I observe during each of the five previous years. This can create problems related to representativeness, as women and young workers are less likely to be followed during five consecutive years.

Column 1 only includes the historical family structure; column 2 adds individuals' characteristics, column 3 also includes regional macroeconomic indicators, and column 4 controls for year fixed-effects. All the estimates from the aforementioned specifications are negative and slightly higher in magnitudes than the LPM estimates. Taking the results from the preferred specification −column 4−, they show that increasing by 1 the average number of married and widowed women per household in 1860 decreases today's mobility by 7 percentage points. In other words, moving from the province with the most nuclear family structure to the one with the most stem family type reduces contemporaneous migration by 2.2 percentage points (75 percent of the annual mobility rate).[19] Similar results are found when I use as dependent variable mobility during the five past years (see table A.14), or living outside the province of birth in he year t (see table A.20.

Table 3.3: Stem Families and Geographical Mobility: IV Results

Outcome	Mobility across Provinces between t and $t-1$			
	(1)	(2)	(3)	(4)
$Stem_p$	-0.06***	-0.08***	-0.07**	-0.07**
	[0.017]	[0.027]	[0.033]	[0.031]
Individual Controls		✓	✓	✓
NUTS 3 Controls			✓	✓
Year FE				✓
Observations	1,972,483	1,896,700	1,896,700	1,896,700
R^2	0.00	0.03	0.03	0.03
Mean Dependent Variable	3%	3%	3%	3%

Note: This table shows the results from the second-stage. The model in column 1 only includes the key predictor: the predominant family structure in the past. The model in column 2 also includes individuals' characteristics (i.e., gender, age, age square, education, number of people sharing the household, children younger than 4 years living in the household, and days worked and earnings in year t). Column 3 includes GDP per capita and the unemployment rate in the province of birth. The model in column 4 also includes year fixed-effects (being the years 2005, 2010, and 2015). Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1

In the next lines, I will talk about the validity of the IV results.

The Validity of the IV and Robustness Checks

For the IV results to be valid, two assumptions must hold. First, the instrument (indivisible inheritance) must be strongly correlated with the instrumented variable (stem family in 1860). Table 3.4 reports the first-stage

[19] Using data from the LFS, the results show similar findings (see A.8).

regression in equation 3.2. We observe a strong positive correlation between being a province with indivisible inheritance in the XIII century and having a predominantly stem family structure in 1860. In addition, the F-statistics take values between 17.75 and 25.55, well above the conventional threshold of 10 for weak instruments (Stock, Wright and Yogo, 2002). Thus, the inheritance laws derived from the Christian Reconquest are a good instrument for family structures in 1860.

Table 3.4: Stem Families and Geographical Mobility: First-Stage Results

Outcome	$Stem_p$			
	(1)	(2)	(3)	(4)
II_p	0.10***	0.10***	0.09***	0.09***
	[0.021]	[0.020]	[0.020]	[0.021]
F-statistic	25.55	25.46	17.95	17.75
Partial R^2	0.53	0.52	0.46	0.46
Individual Controls		✓	✓	✓
NUTS 3 Controls			✓	✓
Year FE				✓
Observations	1,972,483	1,896,700	1,896,700	1,896,700

Note: This table reports the results of estimating equation 3.2 using an OLS. We can observe that those provinces with indivisible inheritance systems in the XIII century were more likely to have a stem family structure in 1860. I also report the F-statistic and the partial R^2 (Bound, Jaeger and Baker, 1995). Both indicators support the validity of the instrument. The model in column 1 only includes the key predictor: an indicator variable that takes the value 1 if the province p had an indivisible inheritance system in the XIII century, and 0 otherwise. The model in column 2 also includes individuals' characteristics (i.e., gender, age, age square, education, number of people sharing the household, children younger than 4 years living in the household, and days worked and earnings in year t). Column 3 includes GDP per capita and the unemployment rate in the province of residence. The model in column 4 also includes year fixed-effects (being the years 2005, 2010, and 2015). Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Yet, an important limitation is that I cannot empirically test whether the inheritance laws derived from the Reconquest affect today's mobility decisions only through its impact on family formation. It could be, for example, that regions with indivisible inheritance laws in the past became more economically prosperous. In this scenario, inheritance laws would affect mobility decisions through more mechanisms than family formation, violating the exclusion restriction assumption. Indeed, Oto-Peralías and Romero-Ávila (2016) or Tapia and Martinez-Galarraga (2018) show that the differences in the speed Christian Kingdoms reconquered the lands have had a permanent impact on economic development and inequality. I try to address this concern in two ways. First, I control for the contemporaneous unemployment rate and GDP per capita. We can see in columns 3 and 4 of table 3.3 that the results are not sensitive to the inclusion of these variables. Second, I control for the speed in which the Christian Kingdoms reconquered the lands. I follow Oto-Peralías and Romero-Ávila (2016) and divide the Reconquest into five stages.[20] The results, in the first two columns of table A.10, show that the negative effect of the stem family on geographical mobility decisions is robust to the inclusion of this additional control.

Columns 3 and 4 of table A.10 also add a dummy to control for potential structural differences between the north and the south of Spain. We can see that the results are very robust, and they show that people born in provinces with historically predominant stem families are less mobile nowadays.

Finally, table A.11 shows the results of estimating the reduced form of the IV approach. We can see that people born in provinces that had an indivisible inheritance system in the XIII century are 1 percentage points less likely to move nowadays.

Overall, all outcomes indicate that being from a province where the historically predominant organization was the stem family reduces today's migration.

Tables A.16 and A.17 show similar results when looking at mobility in the past five years rather than mobility the year before.

3.5.4 Heterogeneity Analysis

This section tests whether there are differences in the effect of family types on mobility dividing the sample by gender, education, and family responsibilities.

The first two columns of table 3.5 show the effects of family types on the mobility decisions of men and women based on estimating the LPM in equation 3.1. We can see that moving from Oursense (the province with the most nuclear family structure) to Huesca (the province with the most stem family organization) would decrease mobility by 1.6 (0.9) percentage points for men (women), which represents 48.6 (29) percent of their average mobility rate.

Columns 3-5 of table 3.5 divide the sample by the group of education. Namely, column 3 only considers those persons with less than high school, column 4 looks at workers with high school, and people with a bachelor degree or more are in column 5. The results show that moving from Ourense to Huesca would decrease the mobility of those with primary (secondary) education by 1.9

[20] See figure A.3.

(1.3) percentage points. In terms of magnitude, this represents 74 (46) percent of their average mobility rates. For people with tertiary education, the effect is not statistically significant. This finding is consistent with Bentolila and Ichino (2008), which shows that families behave as an insurance mechanism when needed. This insurance system may arguably be more relevant for low-educated workers.

Table 3.5: Family and Mobility: LPM Results by Group

Outcome	Mobility across Provinces between t and $t-1$						
Group	Men	Women	Prim. Ed.	Sec. Ed.	Ter. Ed.	No Dep.	Dep.
$Stem_p$	-0.05***	-0.03*	0.06***	-0.04***	-0.05	-0.05**	-0.04***
	[0.016]	[0.018]	[0.011]	[0.014]	[0.039]	[0.020]	[0.012]
Individual Controls	✓	✓	✓	✓	✓	✓	✓
NUTS 3 Controls	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Observations	1,061,170	835,530	1,024,568	504,206	367,926	1,018,469	878,231
R^2	0.02	0.03	0.03	0.03	0.04	0.03	0.02
Mean Dependent Variable	3.3%	2.7%	2.6%	2.8%	4.5%	3.4%	2.7%

Note: Columns 1 and 2 look at the effect of family types on migration by gender. Columns 3 to 5 divide the sample by educational category. Columns 6 and 7 look at the heterogeneous effects of family organizations depending on the persons' family responsibilities. All models include the key predictor − the predominant family structure in the past−, individuals' characteristics (i.e., gender, age, age square, education, number of people sharing the household, children younger than 4 years living in the household, and days worked and earnings in year y), GDP per capita and the unemployment rate in the province of birth, and year fixed-effects (being the years 2005, 2010, and 2015). Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table 3.6: Family and Mobility: IV Results by Group

Outcome	Mobility across Provinces between t and $t-1$						
Group	Men	Women	Prim. Ed.	Sec. Ed.	Ter. Ed.	No Dep.	Dep.
$Stem_p$	-0.07**	-0.06**	-0.09***	-0.06**	-0.08	-0.08**	-0.06***
	[0.031]	[0.030]	[0.031]	[0.028]	[0.052]	[0.036]	[0.025]
Individual Controls	✓	✓	✓	✓	✓	✓	✓
NUTS 3 Controls	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Observations	1,061,170	835,530	1,024,568	504,206	367,926	1,018,469	878,231
R^2	0.02	0.03	0.03	0.03	0.04	0.03	0.02
Mean Dependent Variable	3.3%	2.7%	2.6%	2.8%	4.5%	3.4%	2.7%
F-statistic	17.70	17.84	18.36	16.94	17.50	16.67	19.13
Partial R^2	0.46	0.47	0.43	0.49	0.49	0.45	0.48

Note: Columns 1 and 2 look at the effect of family types on migration by gender. Columns 3 to 5 divide the sample by educational category. Columns 6 and 7 look at the heterogeneous effects of family organizations depending on the persons' family responsibilities. All models include the key predictor −the predominant family structure in the past−, individuals' characteristics (i.e., gender, age, age square, education, number of people sharing the household, children younger than 4 years living in the household, and days worked and earnings in year t), GDP per capita and the unemployment rate in the province of birth, and year fixed-effects (being the years 2005, 2010, and 2015). Standard errors clustered at the province of birth level are in brackets. I also report the F-statistic and the partial R^2 (Bound, Jaeger and Baker, 1995). Both indicators support the validity of the instrument. *** p<0.01, ** p<0.05, * p<0.1

Finally, columns 6 and 7 look at how the historically predominant family structure affects the mobility decisions of people with different family responsibilities. Column 6 groups individuals who do not live with children younger than 4 or elderly order than 65, while column 7 includes everyone with at least one dependent. The results show that moving from Ourense to Huesca would decrease internal mobility by for 1.6 (1.2) percentage points for individuals who do not live with dependants (who live with dependants). This represents 47 percent of the average mobility for both groups.

Table 3.6 presents the results of estimating the heterogeneous effect of the past family structure using a 2SLS. We can see that the results are consistent with the estimates from the LPM in table 3.5.

Finally, table A.21 looks more deeply at the effect of family types by gender.

3.5.5 Persistence and Transmission Channels

The analysis in the previous sections shows that people born in provinces with historically predominant stem families are less likely to relocate nowadays. One potential explanation is that the family values associated with the different family structures persist over time (Alesina et al., 2015). In order to shed some light on this, I look at how different family organizations in the past affect today's value of the family using survey data from the *2014 − Opinions and Attitudes about the Family* by the Centro de Investigaciones Sociólogicas (CIS). In particular, I estimate equation 3.3 using an LPM.

$$Y_{i,p} = \alpha + \beta Stem_p + \delta X_{i,p} + \gamma Z_p + \theta_r + \xi i, p \tag{3.3}$$

where $Y_{i,p}$ measures the family ties of individual i, interviewed in province p. $Stem_p$ is defined using the average number of married and widowed women per household in the province individual i was interviewed. $X_{i,p}$ is a vector of control variables at the individual level that includes gender, age, age squared, level of education, income, civil status, and occupation. $Z_{p,y}$ are regional variables at the provincial p level, including the unemployment rate, and the GDP per capita. I also control for region fixed-effects θ_r. Finally, standard errors are clustered at the province level.

I use four different measures of family ties and responsibilities, and present the results in table 3.7. In columns 1 and 2, I measure family importance with the answers to the question "How important is family to you?". In the first column, the dependent variable equals 1 if the individual i said that family was very or rather important, and 0 otherwise. In column 2, the dependent variable only takes the value of 1 for those individuals who said that family was very important to them. Columns 3 and 4 focus more on family responsibilities. In particular, the dependent variable in column 3 is categorical, and it takes the value 1 for individuals who strongly disagree or disagree with at least one of the following statements: “When people cannot look after themselves, it is better to rely on social services than on the family”, “It is better that children attend the kindergarten rather than leave them with their grandparents or other relatives”, or strongly agree or agree with the statement: “Family should take care of their elderly relatives”. Finally, the dependent variable in column 4 takes the value 1 if individuals disagree or strongly disagree with at least one of the next statements: “Elderly relatives should enjoy their money rather than leave it to their children”, or “Parents have their lives and should not sacrifice it for their children”. The results suggest that the individuals from provinces where stem families were socially predominant in the past give more importance to family and to the family responsibilities nowadays.

Table 3.7: Attitudes towards Family

Outcome	Family Importance		Family Obligations	
	(1)	(2)	(3)	(4)
$Stem_p$	0.36	0.79*	0.70**	0.00
	[0.260]	[0.457]	[0.304]	[0.431]
Individual Controls	✓	✓	✓	✓
NUTS 3 Controls	✓	✓	✓	✓
NUTS 2 FE	✓	✓	✓	✓
Observations	2,264	2,264	2,264	2,264
Mean Dependent Variable	0.94	0.48	0.85	0.10

Note: The dependent variable in column 1 is categorical, and takes the value 1 if the individual i declares that family is very or rather important for her/him, and 0 otherwise. In column 2, the dependent variable takes the value 1 for those individuals who declare that family is very important for them, and 0 otherwise. The dependent variable in column 3 is categorical, and it takes the value 1 for individuals who strongly disagree or disagree with at least one of the following statements: “When people cannot look after themselves, it is better to rely on social services than on the family”, “It is better that children attend the kindergarten rather than leave them with their grandparents or other relatives”, or strongly agree or agree with the statement “Family should take care of their elderly relatives”. Finally, the dependent variable in column 4 takes the value 1 if individuals disagree or strongly disagree with at least one of the next statements: “Elderly relatives should enjoy their money rather than leave it to their children”, or “Parents have their lives and should not sacrifice it for their children”. The key predictor variable, $-Stem_p-$ measures the average number of widowed and married women at the household based on 1860 and aggregated. Control variables include: gender, age, age squared, marital status fixed-effects, job status fixed-effects, educational level fixed-effects, GDP per capita and the unemployment rate at the province level, and regional fixed-effects.

In addition, given that this is a within-country analysis and that I include regional fixed-effects in all specifications, the results should not be driven by institutional differences. However, there may be other factors that can contribute to the negative relation between stem families and mobility. More research in the driving mechanisms could be very interesting.

3.6 Conclusions

The low rates of geographical mobility are puzzling, given the large economic disparities across and within countries (Blanchard et al., 1992). Albeit previous literature has studied the contemporaneous factors that affect migration, few is known about how past institutions may shape today's mobility decisions.

This chapter looks at the historical roots of migration. It analyzes how the predominant family structures in the past (stem or egalitarian nuclear) impact today's mobility decisions in Spain. Stem families are characterized by several generations sharing the household, and by indivisible inheritance. Contrary, there was no cohabitation in nuclear families, and the family wealth was equally distributed among all children. The hypothesis in this chapter is that people who are born in places where stem families were socially predominant in the past will be less likely to move, as stem families are associated with stronger family ties (Salamon, 1982). If family institutions are persistent in time (Farre and Vella, 2013), societies with historically prevalent stem families will derive an intrinsic utility from living nearby their families, which increases the cost of mobility.

To look into this hypothesis, I use the 1860 Spanish data to define the prevalent family organization at the provincial level. To avoid problems of omitted variables and reverse causality, I use the inheritance laws derived from the Reconquista as an instrument for family types. The results show that people from provinces with historically predominant stem families are less likely to relocate geographically. This result is robust to the inclusion of a comprehensive set of controls, the use of alternative datasets, different measures of family types, and changes in the definition of geographical mobility.

As a potential mechanism, I find some evidence showing that individuals coming from stem societies give more value to the family as well as to the family responsibilities nowadays. This result is in line with the extensive literature showing that cultural norms are sticky (Alesina et al., 2015).

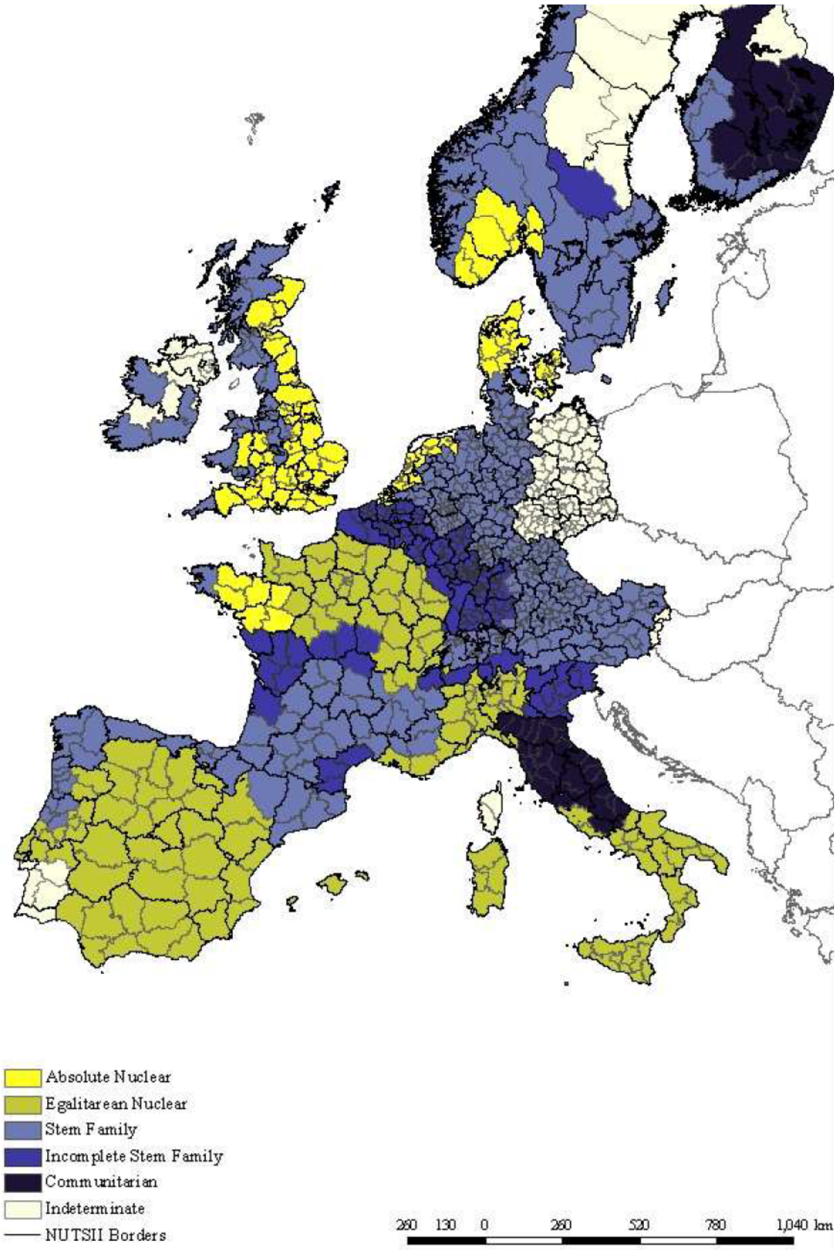
The next step in this research is to extend the analysis to Europe, and see how different family structures in the past affect today's mobility within and also across countries.

From a public policy perspective, this study is very important. A lot of labor policies are decided at the country level. However, if institutions differ across space and they modify individual decisions, general policies are not likely to achieve their objectives.

3.7 Appendix

Family Types in Europe

Figure A.1: Family Types in Europe



Source: Duranton, Rodríguez-Pose and Sandall (2009) based on Todd (1990)

Descriptive Statistics

Table A.1: Descriptive Statistics

Variable	Mean	Std. Dev.	Min.	Max.	Obs.
Main Outcome Variable					
Mobility	0.03	0.17	0	1	1,972,483
Main Covariate					
Stem 1860	1.01	0.07	0.87	1.19	1,972,483
Other Covariates					
Female	0.44	0.50	0	1	1,972,483
Male	0.56	0.50	0	1	1,972,483
Age	41.34	11.7	16	65	1,972,483
Primary Education	0.54	0.49	0	1	1,954,897
Secondary Education	0.26	0.44	0	1	1,954,897
Tertiary Education	0.20	0.39	0	1	1,954,897
Family Size	3.04	1.4	1	10	1,910,894
Children Younger than 4	0.099	0.33	0	7	1,910,894
Yearly Earnings	16,607	10,493	0.01	103,885	1,972,483
Days Worked	308.7	108.2	0	365	1,972,483

Note: This information comes from the MCVL, and it includes all persons aged between 16 and 65, born in Spain, and who have had a relationship with the social security administration at least once in the years 2005, 2010, or 2015. The variable *stem 1860* represents the average number of married and widowed women per household at the province level according to the 1860 census.

Table A.2: Descriptive Statistics: Family Importance

Variable	Mean	Std. Dev.	Min.	Max.	Obs.
Main Outcome Variable					
			Family		
Very important	0.86	0.35	0	1	2,265
Rather important	0.13	0.34	0	1	2,265
Not very important	0.01	0.09	0	1	2,265
Not important at all	0.00	0.05	0	1	2,265

Note: This information comes from the survey *Opinions and attitudes towards family - 2014*, by the CIS. I do not include people who do not answer the question or who were born outside of Spain. As we can see, 99 percent of the sample considered that family was very or rather important.

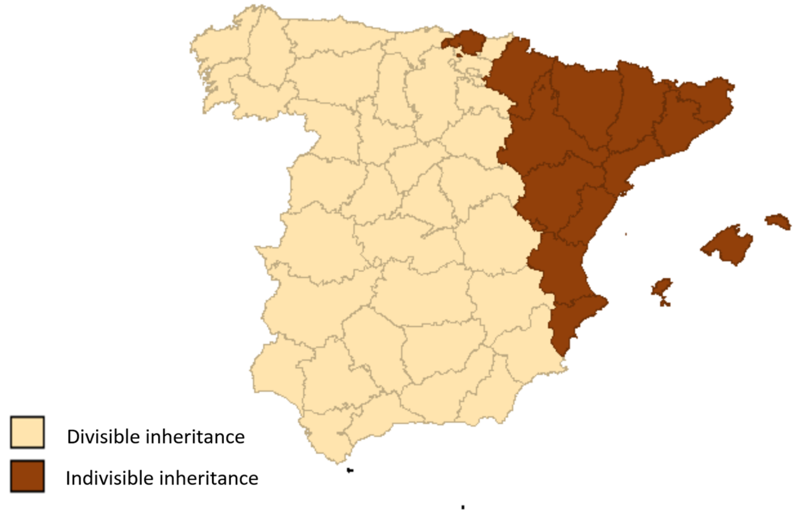
Table A.3: Descriptive Statistics: Family Responsibilities

Variable	Mean	Std. Dev.	Min.	Max.	Obs.
Main Outcome Variables					
To take care of dependent relatives, it is better to rely on					
Social Services					
Completely agree	0.09	0.28	0	1	2,251
Agree	0.18	0.39	0	1	2,251
Not agree nor disagree	0.13	0.33	0	1	2,251
Disagree	0.35	0.48	0	1	2,251
Completely disagree	0.25	0.43	0	1	2,251
To take care of children, it is better					
Kindergarten than family					
Completely agree	0.19	0.39	0	1	2,238
Agree	0.32	0.47	0	1	2,238
Not agree nor disagree	0.17	0.37	0	1	2,238
Disagree	0.22	0.41	0	1	2,238
Completely disagree	0.1	0.30	0	1	2,238
To take care of elderly relatives, it is better					
Close family					
Completely agree	0.24	0.43	0	1	2,249
Agree	0.45	0.50	0	1	2,249
Not agree nor disagree	0.2	0.4	0	1	2,249
Disagree	0.08	0.27	0	1	2,249
Completely disagree	0.02	0.14	0	1	2,249
Older people should enjoy their money					
Rather than leave it to their families					
Completely agree	0.36	0.48	0	1	2,242
Agree	0.36	0.48	0	1	2,242
Not agree nor disagree	0.13	0.44	0	1	2,242
Disagree	0.11	0.31	0	1	2,242
Completely disagree	0.03	0.16	0	1	2,242
Parents should not sacrifice their lives for their children					
Completely agree	0.11	0.35	0	1	2,218
Agree	0.24	0.43	0	1	2,218
Not agree nor disagree	0.22	0.42	0	1	2,218
Disagree	0.31	0.46	0	1	2,218
Completely disagree	0.12	0.32	0	1	2,218

Note: This information comes from the survey *Opinions and attitudes towards family - 2014*, by the CIS. I do not include people who do not answer the question or who were born outside of Spain.

Indivisible Inheritance Laws

Figure A.2: Indivisible Inheritance Laws



Note: The map shows the two different systems of inheritance laws in the XIII century. Areas in dark brown have an indivisible inheritance system, while in areas in yellow, parents should divide the family wealth equally among their children.

Stem Families and Annual Mobility

Table A.4: Family and Mobility: LPM Results using the LFS

Outcome	Mobility across Provinces between t and $t-1$				
	(1)	(2)	(3)	(4)	(5)
$Stem_p$	0.00	-0.01**	-0.01*	-0.02**	0.01
	[0.003]	[0.003]	[0.008]	[0.008]	[0.006]
Individual Controls		✓	✓	✓	✓
NUTS 3 Controls			✓	✓	✓
Year FE				✓	✓
NUTS2 FE					✓
Observations	165,693	165,693	165,693	165,693	165,693
R^2	0.00	0.02	0.02	0.02	0.02
Mean Dependent Variable	1%	1%	1%	1%	1%

Note: The dependent variable is binary and takes the value 1 if the person has changed province between the year t and the year $t-1$. The model in column 1 only includes the key predictor: the predominant family structure in the past. Particularly, the stem family is defined as the average number of married and widowed women in the household at the province level in 1860. The model in column 2 also includes individuals' characteristics (i.e., gender, age fixed-effects, education, number of people sharing the household, employment status, and civil status). Column 3 includes GDP per capita and the unemployment rate in the province of birth. The model in column 4 also includes year fixed-effects (being the years 2005, 2010, and 2015). Finally, the model in column 5 adds regional fixed-effects. Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table A.5: Family (Alternative Definition) and Mobility: LPM Results

Outcome	Mobility across Provinces between t and $t-1$				
	(1)	(2)	(3)	(4)	(5)
$Stem_p$	-0.01***	-0.01**	-0.01*	-0.01**	0.00
	[0.002]	[0.004]	[0.003]	[0.003]	[0.004]
$Incomplete\ Stem_p$	-0.00	-0.00	-0.00*	-0.01**	0.00
	[0.002]	[0.003]	[0.003]	[0.002]	[0.003]
Individual Controls		✓	✓	✓	✓
NUTS 3 Controls			✓	✓	✓
Year FE				✓	✓
NUTS2 FE					✓
Observations	1,972,483	1,896,700	1,896,700	1,896,700	1,896,700
R^2	0.00	0.03	0.03	0.03	0.03
Mean Dependent Variable	3%	3%	3%	3%	3%

Note: The dependent variable is binary and takes the value 1 if the person has changed province between the year t and the year $t-1$. The model in column 1 only includes the key predictor: the predominant family structure in the past. Particularly, I follow Mikelarena and Pérez-Fuentes (2001) and define societies with stem families as those with more than 1.075 married and widowed women per household according to the 1860 census. Egalitarian nuclear families (the omitted category) are those societies with less than 1 married and widow women per household. Finally, those provinces where the average number of married and widowed women per household was between 1 and 1.075 are considered the incomplete stem. The model in column 2 includes individuals' characteristics (i.e., gender, age, age square, education, number of people sharing the household, number of children younger than 4 in the household, and days worked and annual earnings in year t). Column 3 also includes GDP per capita and the unemployment rate in the province of birth. The model in column 4 also includes year fixed-effects (being the years 2005, 2010, and 2015). Finally, column 5 adds state fixed-effects. Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table A.6: Family (Alternative Definition) and Mobility: LPM Results

Outcome	Mobility across Provinces between t and $t-1$				
	(1)	(2)	(3)	(4)	(5)
$Stem_p$	-0.01***	-0.01***	-0.01	-0.01	-0.00
	[0.003]	[0.004]	[0.004]	[0.004]	[0.003]
Individual Controls		✓	✓	✓	✓
NUTS 3 Controls			✓	✓	✓
Year FE				✓	✓
NUTS2 FE					✓
Observations	1,972,483	1,896,700	1,896,700	1,896,700	1,896,700
R^2	0.00	0.03	0.03	0.03	0.03
Mean Dependent Variable	3%	3%	3%	3%	3%

Note: The model in column 1 only includes the key predictor: if the person was born in a province with a stem or a nuclear type of family −stem being defined as the average number of individuals per household at the province level in 1860−. The model in column 2 includes individuals' characteristics (i.e., gender, age, age square, education, number of people sharing the household, number of children younger than 4 in the household, and days worked and annual earnings in year t). Column 3 also includes GDP per capita and the unemployment rate in the province of birth. The model in column 4 also includes year fixed-effects (being the years 2005, 2010, and 2015). Finally, column 5 adds state fixed-effects. Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table A.7: Family (Alternative Definition) and Mobility: LPM Results

Outcome	Mobility across Provinces between t and $t-1$				
	(1)	(2)	(3)	(4)	(5)
$Stem_p$	-0.02***	-0.03***	-0.01	-0.01	0.00
	[0.007]	[0.009]	[0.009]	[0.009]	[0.010]
Individual Controls		✓	✓	✓	✓
NUTS 3 Controls			✓	✓	✓
Year FE				✓	✓
NUTS2 FE					✓
Observations	1,972,483	1,896,700	1,896,700	1,896,700	1,896,700
R^2	0.00	0.03	0.03	0.03	0.03
Mean Dependent Variable	3%	3%	3%	3%	3%

Note: The model in column 1 only includes our key predictor: if the person was born in a province with a stem or a nuclear type of family −stem family is defined as the average number of married and widowed people per household at the province level in 1860−. The model in column 2 also includes individuals' characteristics (i.e., gender, age, age square, education, number of people sharing the household, number of children younger than 4 in the household, and days worked and earnings in year t). Column 3 also includes GDP per capita and the unemployment rate in the province of birth. The model in column 4 also includes year fixed-effects (being the years 2005, 2010, and 2015). Finally, column 5 adds regional fixed-effects. Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table A.8: Family and Mobility: IV Results from LFS Data

Outcome	Mobility across Provinces between t and $t-1$			
	(1)	(2)	(3)	(4)
$Stem_p$	0.00	-0.01*	-0.02*	-0.03**
	[0.007]	[0.006]	[0.013]	[0.011]
Individual Controls		✓	✓	✓
NUTS 3 Controls			✓	✓
Year FE				✓
Observations	165,693	165,693	165,693	165,693
R^2	0.00	0.01	0.01	0.03
Mean Dependent Variable	1%	1%	1%	1%

Note: This table shows the results from the second-stage. The model in column 1 only includes the key predictor: the predominant family structure in the past. The model in column 2 also includes individuals' characteristics (i.e., gender, age fixed-effects, education, number of people sharing the household, employment status, and civil status). Column 3 includes GDP per capita and the unemployment rate in the province of birth. The model in column 4 also includes year fixed-effects (being the years 2005, 2010, and 2015). Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1. The results from the first-stage are in table A.9.

Table A.9: Family and Mobility: First-Stage Results from LFS Data

Outcome	$Stem_p$			
	(1)	(2)	(3)	(4)
II_p	0.10***	0.10***	0.08***	0.07***
	[0.017]	[0.017]	[0.019]	[0.020]
F-statistic	35.16	35.52	16.27	13.22
Partial R^2	0.46	0.45	0.29	0.46
Individual Controls		✓	✓	✓
NUTS 3 Controls			✓	✓
Year FE				✓
Observations	165,693	165,693	165,693	165,693

Note: This table reports the results of estimating equation 3.2 using an OLS. We can observe that those provinces with indivisible inheritance systems in the XIII century were more likely to have a stem family structure in 1860. I also report the F-statistic and the partial R^2 (Bound, Jaeger and Baker, 1995). Both indicators support the validity of the instrument. The model in column 1 only includes the key predictor: an indicator variable that takes the value 1 if province p had an indivisible inheritance system in the XIII century, and 0 otherwise. The model in column 2 also includes individuals' characteristics. Column 3 includes GDP per capita and the unemployment rate in the province of birth. The model in column 4 also includes year fixed-effects. Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table A.10: Family and Mobility: Additional LPM and IV Results

Outcome	Mobility across Provinces between t and $t-1$					
	LPM	IV	LPM	IV	LMP	IV
$Stem_p$	-0.07***	-0.11***	-0.04***	-0.07***	-0.07**	-0.12**
	[0.023]	[0.037]	[0.015]	[0.026]	[0.031]	[0.045]
Individual Controls	✓	✓	✓	✓	✓	✓
NUTS 3 Controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Stages of the Reconquest FE	✓	✓			✓	✓
North-South FE			✓	✓	✓	✓
Observations	1,896,700	1,896,700	1,896,700	1,896,700	1,896,700	1,896,700
R^2	0.03	0.03	0.03	0.03	0.03	0.03
Mean Dependent Variable	3%	3%	3%	3%	3%	3%
F-statistic		18.64		17.42		17.44
Partial R^2		0.34		0.46		0.31

Note: This table shows the results from an LPM and the second stage of the IV of different specifications. All models include the key predictor (the predominant family structure in the past), individuals' characteristics (i.e., gender, age, age square, education, number of people sharing the household, number of children younger than 4 in the household, and days worked and earnings in year t), GDP per capita and the unemployment rate in the province of birth, and year fixed-effects. Columns 1 and 2 include also stage of the reconquest fixed-effects, following Oto-Peralías and Romero-Ávila (2016). Columns 3 and 4 control for potential permanent structural differences between the North and South of Spain, and columns 5 and 6 include both the stages of the Reconquest and North-South fixed-effects. In the columns specifying an IV, I also report the F-statistic and the partial R^2 (Bound, Jaeger and Baker, 1995). Both indicators support the validity of the instrument. Standard errors in all specifications clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table A.11: Family and Mobility: Reduced Form

Outcome	Mobility across Provinces between t and $t-1$				
	(1)	(2)	(3)	(4)	(5)
II_p	-0.01**	-0.01**	-0.01**	-0.01***	0.00
	[0.002]	[0.003]	[0.002]	[0.002]	[0.002]
Individual Controls		✓	✓	✓	✓
NUTS 3 Controls			✓	✓	✓
Year FE				✓	✓
NUTS2 FE					✓
Observations	1,972,483	1,896,700	1,896,700	1,896,700	1,896,700
R^2	0.00	0.03	0.03	0.03	0.03
Mean Dependent Variable	3%	3%	3%	3%	3%

Note: The dependent variable is binary and takes the value 1 if the person has changed province between the year t and the year $t-1$. The model in column 1 only includes the key predictor: an indicator that takes the value 1 if the province p had indivisible inheritance in the XIII century, and 0 otherwise. The model in column 2 also includes individuals' characteristics (i.e., gender, age, age square, education, number of people sharing the household, children younger than 4 in the household, and days worked and earnings in year t). Column 3 includes GDP per capita and the unemployment rate in the province of birth. The model in column 4 also includes year fixed-effects (being the years 2005, 2010, and 2015). Finally, the model in column 5 adds regional fixed-effects. Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Stem Families and 5-years Geographical Mobility

Table A.12: Family and 5-years Mobility: LPM Results

Outcome	Mobility across Provinces between t and $t-5$				
	(1)	(2)	(3)	(4)	(5)
$Stem_p$	-0.10***	-0.16***	-0.10**	-0.10**	-0.07
	[0.028]	[0.040]	[0.045]	[0.038]	[0.066]
Individual Controls		✓	✓	✓	✓
NUTS 3 Controls			✓	✓	✓
Year FE				✓	✓
NUTS FE					✓
Observations	1,725,004	1,660,933	1,660,933	1,660,933	1,660,933
R^2	0.00	0.05	0.05	0.05	0.05
Mean Dependent Variable	7.6%	7.6%	7.6%	7.6%	7.6%

Note: The model presented in column 1 only includes the key predictor: the predominant family structure in the past. Particularly, the stem family is defined as the average number of married and widowed women in the household at the province level in 1860. The model in column 2 also includes individuals' characteristics. Column 3 includes GDP per capita and the unemployment rate in the province of birth. The model in column 4 also includes year fixed-effects (being the years 2005, 2010, and 2015). Finally, in column 5, I also add regional fixed-effects. Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table A.13: Family (Alternative Definition) and 5-years Mobility: LPM Results

Outcome	Mobility across Provinces between t and $t-1$				
	(1)	(2)	(3)	(4)	(5)
$Stem_p$	-0.02***	-0.02**	-0.01*	-0.01*	0.00
	[0.005]	[0.010]	[0.009]	[0.007]	[0.011]
$Incomplete\ Stem_p$	-0.01*	-0.01	-0.01*	-0.01**	0.01
	[0.005]	[0.008]	[0.006]	[0.006]	[0.007]
Individual Controls		✓	✓	✓	✓
NUTS 3 Controls			✓	✓	✓
Year FE				✓	✓
NUTS2 FE					✓
Observations	1,725,004	1,660,933	1,660,933	1,660,933	1,660,933
R^2	0.00	0.05	0.05	0.05	0.05
Mean Dependent Variable	7.6%	7.6%	7.6%	7.6%	7.6%

Note: The dependent variable is binary and takes the value 1 if the person has changed province between the year t and the year $t-5$. The model in column 1 only includes the key predictor: the predominant family structure in the past. Particularly, I follow Mikelarena and Pérez-Fuentes (2001) and define societies with stem families as those with more than 1.075 married and widowed women per household according to the 1860 census. Egalitarian nuclear families (the omitted category) are those societies with less than 1 married and widow women per household. The model in column 2 includes individuals' characteristics. Column 3 also includes GDP per capita and the unemployment rate in the province of birth. The model in column 4 also includes year fixed-effects (being the years 2005, 2010, and 2015). Finally, column 5 adds state fixed-effects. Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table A.14: Family and 5-years Mobility: IV Results

Outcome	Mobility across Provinces between t and $t-5$			
	(1)	(2)	(3)	(4)
$Stem_p$	-0.12***	-0.20***	-0.17**	-0.17**
	[0.042]	[0.068]	[0.083]	[0.075]
Individual Controls		✓	✓	✓
NUTS 3 Controls			✓	✓
Year FE				✓
Observations	1,725,004	1,660,933	1,660,933	1,660,933
R^2	0.00	0.05	0.05	0.05
Mean Dependent Variable	7.6%	7.6%	7.6%	7.6%

Note: This table shows the results from the second-stage. The model in column 1 only includes the key predictor: the predominant family structure in the past. The model in column 2 also includes individuals' characteristics (i.e., gender, age, age square, education, number of people sharing the household, number of children younger than 4 in the household, and days worked and earnings in year t). Column 3 includes GDP per capita and the unemployment rate in the province of birth. The model in column 4 also includes year fixed-effects (being the years 2005, 2010, and 2015). Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1. The results from the first stage are in table A.15.

Table A.15: Family and 5-year Mobility: First-Stage Results

Outcome	$Stem_p$			
	(1)	(2)	(3)	(4)
II_p	0.10***	0.10***	0.09***	0.09***
	[0.021]	[0.020]	[0.021]	[0.021]
F-statistic	25.55	25.73	17.83	17.60
Partial R^2	0.53	0.52	0.46	0.46
Individual Controls		✓	✓	✓
NUTS 3 Controls			✓	✓
Year FE				✓
Observations	1,725,004	1,660,933	1,660,933	1,660,933

Note: This table reports the first stage results. We can observe that those provinces with indivisible inheritance systems in the XIII century were more likely to have a steam family structure in 1860. I also report the F-statistic and the partial R^2 (Bound, Jaeger and Baker, 1995). Both indicators support the validity of the instrument. The model in column 1 only includes our key predictor: an indicator variable that takes the value 1 if the province p had an indivisible inheritance system in the XIII century, and 0 otherwise. The model in column 2 also includes individuals' characteristics (i.e., gender, age, age square, education, number of people sharing the household, number of children younger than 4 in the household, and days worked and earnings in year t). Column 3 includes GDP per capita and the unemployment rate in the province of birth. The model in column 4 includes also year fixed-effects (being the years 2005, 2010, and 2015). Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table A.16: Family and 5-years Mobility: Additional LPM and IV Results

Outcome	Mobility across Provinces between t and $t-5$					
	LPM	IV	LPM	IV	LMP	IV
$Stem_p$	-0.16***	-0.27***	-0.10***	-0.17***	-0.19***	-0.32***
	[0.052]	[0.092]	[0.036]	[0.070]	[0.079]	[0.110]
Individual Controls	✓	✓	✓	✓	✓	✓
NUTS 3 Controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Stages of the Reconquest FE	✓	✓			✓	✓
North-South FE			✓	✓	✓	✓
Observations	1,660,933	1,660,933	1,660,933	1,660,933	1,660,933	1,660,933
R^2	0.05	0.05	0.05	0.05	0.05	0.05
Mean Dependent Variable	7.6%	7.6%	7.6%	7.6%	7.6%	7.6%
F-statistic		18.07		17.31		16.96
Partial R^2		0.33		0.46		0.31

Note: This table shows the results from an LPM and the second stage of the IV of different specifications. All models include the key predictor (the predominant family structure in the past), individuals' characteristics (i.e., gender, age, age square, education, number of people sharing the household, number of children younger than 4 in the household, and days worked and earnings in year y), GDP per capita and the unemployment rate in the province of birth, and year fixed-effects. Columns 1 and 2 include also stage of the reconquest fixed-effects, following Oto-Peralías and Romero-Ávila (2016). Columns 3 and 4 control for potential permanent structural differences between the North and South of Spain, and columns 5 and 6 include both the stages of the Reconquest and North-South fixed-effects. In the columns specifying an IV, I also report the F-statistic and the partial R^2 (Bound, Jaeger and Baker, 1995). Both indicators support the validity of the instrument. Standard errors in all specifications clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table A.17: Family and 5-year Mobility: Reduced Form

Outcome	Mobility across Provinces between t and $t-5$				
	(1)	(2)	(3)	(4)	(5)
II_p	-0.01**	-0.02**	-0.01**	-0.01***	0.01**
	[0.005]	[0.008]	[0.006]	[0.005]	[0.006]
Individual Controls		✓	✓	✓	✓
NUTS 3 Controls			✓	✓	✓
Year FE				✓	✓
NUTS2 FE					✓
Observations	1,725,004	1,660,933	1,660,933	1,660,933	1,660,933
R^2	0.00	0.05	0.05	0.05	0.05
Mean Dependent Variable	7.6%	7.6%	7.6%	7.6%	7.6%

Note: The dependent variable is binary and takes the value 1 if the person has changed province between the year t and the year $t-1$. The model in column 1 only includes the key predictor: an indicator that takes the value 1 if the province p had indivisible inheritance in the XIII century, and 0 otherwise. The model in column 2 also includes individuals' characteristics (i.e., gender, age, age square, education, number of people sharing the household, children younger than 4 in the household, and days worked and earnings in year t). Column 3 includes GDP per capita and the unemployment rate in the province of birth. The model in column 4 also includes year fixed-effects (being the years 2005, 2010, and 2015). Finally, the model in column 5 adds regional fixed-effects. Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Stem Family and Long-Term Mobility

Table A.18: Family and Long-Term Mobility: LPM Results

Outcome	Different than province of birth in t				
	(1)	(2)	(3)	(4)	(5)
$Stem_p$	-0.66***	-0.68***	-0.68***	-0.31*	0.38
	[0.200]	[0.191]	[0.191]	[0.182]	[0.348]
Individual Controls		✓	✓	✓	✓
NUTS 3 Controls			✓	✓	✓
Year FE				✓	✓
NUTS2 FE					✓
Observations	2,089,936	2,008,547	2,008,547	2,008,547	2,008,547
R^2	0.01	0.04	0.04	0.05	0.09
Mean Dependent Variable	25%	25%	25%	25%	25%

Note: The dependent variable is binary and takes the value 1 if the person was living in a province different from the province of birth in the year t. The model in column 1 only includes the key predictor: the predominant family structure in the past. Particularly, the stem family is defined as the average number of married and widowed women in the household at the province level in 1860. The model in column 2 also includes individuals' characteristics (i.e., gender, age, age square, education, number of people sharing the household, children younger than 4 in the household, and days worked and earnings in year t). Column 3 includes GDP per capita and the unemployment rate in the province of birth. The model in column 4 also includes year fixed-effects (being the years 2005, 2010, and 2015). Finally, the model in column 5 adds regional fixed-effects. Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table A.19: Family and Long-Term Mobility: IV Results

Outcome	Different than the province of birth			
	(1)	(2)	(3)	(4)
$Stem_p$	-1.03***	-1.01***	-0.84*	-0.85**
	[0.034]	[0.353]	[0.435]	[0.416]
Individual Controls		✓	✓	✓
NUTS 3 Controls			✓	✓
Year FE				✓
Observations	2,089,936	2,008,547	2,008,547	2,008,547
R^2	0.00	0.04	0.05	0.05
Mean Dependent Variable	25%	25%	25%	25%

Note: This table shows the results from the second-stage. The model in column 1 only includes our key predictor: the predominant family structure in the past. The model in column 2 also includes individuals' characteristics (i.e., gender, age, age square, education, number of people sharing the household, number of children younger than 4 in the household, and days worked and earnings in year t). Column 3 includes GDP per capita and the unemployment rate in the province of birth. The model in column 4 also includes year fixed-effects (being the years 2005, 2010, and 2015). Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1. The results from the first stage are in table A.20.

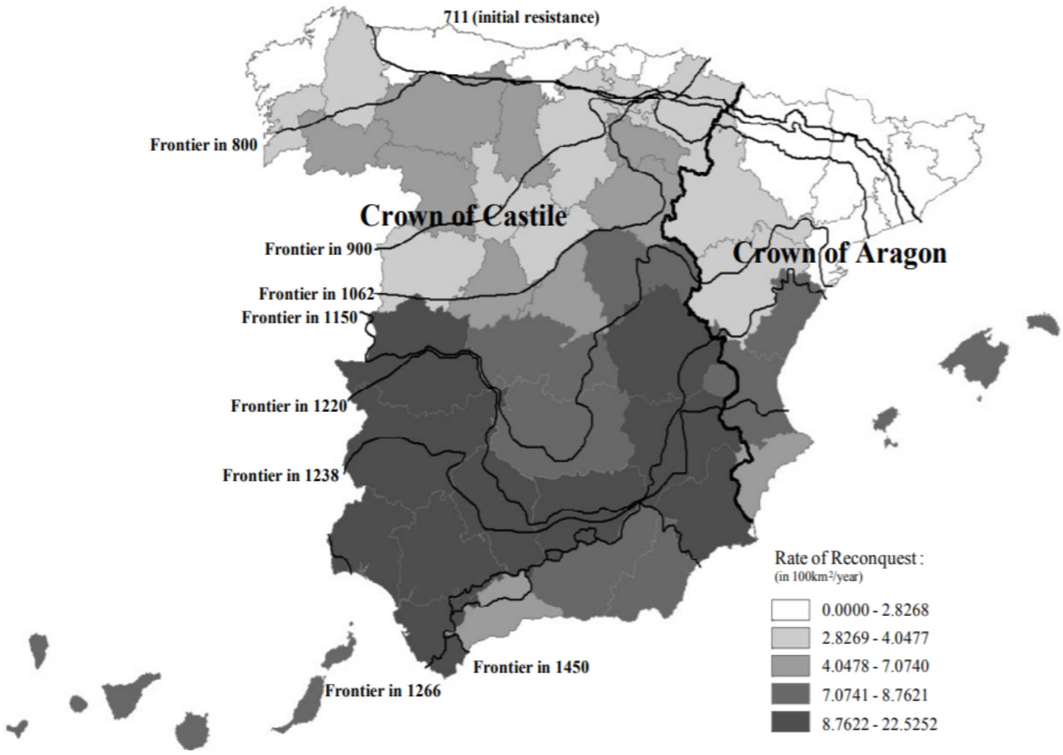
Table A.20: Family and Long-Term Mobility: First-Stage Results

Outcome	$Stem_p$			
	(1)	(2)	(3)	(4)
II_p	0.10***	0.10***	0.09***	0.09***
	[0.021]	[0.020]	[0.020]	[0.021]
F-statistic	25.20	25.34	18.43	17.84
Partial R^2	0.53	0.52	0.46	0.46
Individual Controls		✓	✓	✓
NUTS 3 Controls			✓	✓
Year FE				✓
Observations	2,089,936	2,008,547	2,008,547	2,008,547

Note: This table reports the first stage results. We can observe that those provinces with indivisible inheritance systems in the XIII century were more likely to have a steam family structure in 1860. I also report the F-statistic and the partial R^2 (Bound, Jaeger and Baker, 1995). Both indicators support the validity of the instrument. The model in column 1 only includes our key predictor: an indicator variable that takes the value 1 if the province p had an indivisible inheritance system in the XIII century, and 0 otherwise. The model in column 2 also includes individuals' characteristics (i.e., gender, age, age square, education, number of people sharing the household, number of children younger than 4 in the household, and days worked and earnings in year t). Column 3 includes GDP per capita and the unemployment rate in the province of birth. The model in column 4 also includes year fixed-effects (being the years 2005, 2010, and 2015). Standard errors clustered at the province of birth level are in brackets. *** p<0.01, ** p<0.05, * p<0.1

The Spanish Reconquista

Figure A.3: The Spanish Reconquista



Source: Oto-Peralías and Romero-Ávila (2016).

Heterogeneity Analysis

Table A.21: Family and Mobility: IV Results by Group

Outcome	Mobility across Provinces between t and $t-1$									
Group	Prim. Ed.		Sec. Ed.		Ter. Ed.		No Dep.		Dep.	
	Men	Women	Men	Women	Men	Women	Men	Women	Men	Women
$Stem_p$	-0.11***	-0.07***	-0.05*	-0.07**	-0.07	-0.09*	-0.08**	-0.07*	-0.07**	-0.04**
	[0.035]	[0.022]	[0.026]	[0.027]	[0.047]	[0.056]	[0.035]	[0.040]	[0.028]	[0.021]
Individual Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
NUTS 3 Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	625,255	399,313	273,854	230,352	162,061	205,865	582,875	435,594	478,295	399,936
R^2	0.03	0.02	0.03	0.03	0.04	0.04	0.03	0.04	0.03	0.03
Mean Dependent Variable	0.031	0.020	0.030	0.025	0.45	0.45	0.035	0.032	0.031	0.022
F-statistic	18.72	17.86	16.10	17.98	17.28	17.64	16.58	16.80	19.20	19.08
Partial R^2	0.43	0.43	0.48	0.50	0.49	0.49	0.44	0.45	0.47	0.49

Note: Columns 1 to 6 look at the effect of family types on migration by gender and education. Columns 7 to 10 divide the sample by family responsibilities and gender. All models include the key predictor − the predominant family structure in the past−, individuals' characteristics (i.e., age, age square, education, number of people sharing the household, children younger than 4 years living in the household, and days worked and earnings in year t), GDP per capita and the unemployment rate in the province of birth, and year fixed-effects (being the years 2005, 2010, and 2015). Standard errors clustered at the province of birth level are in brackets. I also report the F-statistic and the partial R^2 (Bound, Jaeger and Baker, 1995). Both indicators support the validity of the instrument. *** p<0.01, ** p<0.05, * p<0.1

Chapter 4

Cognitive Biases in Selection Processes: Evidence from a Natural Randomized Experiment[1]

4.1 Introduction

Many judgments and decisions should and need to be independent of one another. These decisions, however, are rarely faced in isolation; they often come sequentially (Rubinstein and Salant, 2006). One after another, teachers grade students' exams, human resources departments evaluate potential employees, and judges decide whether to grant prisoners parole. Albeit one would expect experts' choices to reflect candidates' real aptitudes (Ginsburgh and Van Ours, 2003), evaluations of options presented in a sequence may be biased by their order of appearance (Neilson, 1998). These so-called *sequential or order effects* can jeopardize the accuracy of relevant deliberation processes (Bruine de Bruin, 2005), and have lifelong consequences for individual outcomes (Ginsburgh and Van Ours, 2003).

Previous literature studying the effects of list ordering on decisions has found evidence that experts tend to disproportionally select the first −primacy effect− or the last −recency effect− options in a list. Examples include settings as diverse as international musical contests (Ginsburgh and Van Ours, 2003), judicial proceedings (Danziger, Levav and Avnaim-Pesso, 2011), sales success

[1]This chapter is a version of a joint research project with Miquel Serra-Burriel, Jordi Teixidó, and Marc-Lluís Vives.

(Wagner and Klein, 2007), editors' choices in academic journals (Orazbayev, 2017), scientific citation behavior (Feenberg et al., 2017), or students' access to selective schools (Jurajda and Münich, 2010).

In this chapter, we investigate the presence of sequential effects in a particularly interesting and unexplored setting: the recruitment process used to hire 5,005 permanent teachers in the Catalan public schools in 2019. The hiring process consisted of three qualifying exams (an oral presentation, a case study, and the development of a random topic), and a merit-based score based on previous experience. During the summer of 2019, 182 groups of five experts each graded and evaluated the 20,254 applicants who enrolled to participate in this recruitment process.

We use administrative data provided by the Catalan government on the universe of candidates enrolled to participate in the recruitment process and focus our attention on the first part of the competitive recruitment process −the oral exam−. In this oral presentation, candidates had 45 minutes to propose a syllabus for an academic year, and to present a unit-plan of their choice. After each oral presentation, the tribunal rated the applicant's performance from 0 to 10. Only those candidates who obtained at least a 5.0 at this stage could continue with the recruitment process. Interestingly for our study, the order in which applicants did this test was random by design.

Exploiting the random order of presentation, we estimate the causal effect of being evaluated in different positions on the candidates' assessments. The results show that those applicants who did the test first obtained around 0.17 points more than those who presented in other slots (3 percent of the mean). In addition, they were 3.3 percentage points more likely to pass the oral exam. In terms of magnitude, this represents 5.3 percent of the average success rate. This result shows that a completely extraneous factor such as the order in which an applicant is interviewed can have severe consequences for his/her labor market career.

The findings of this paper are in line with previous literature showing the presence of primacy effects in sequential decisions. For example, Meredith and Salant (2013) show that contenders randomly listed first in city council elections in California win office 4.8 percentage points more times than expected lacking order effects. Feenberg et al. (2017) find that NBER papers randomly ranked first in the *New This Week* email receive 30 percent more views, downloads, and citations than those ranked in latter positions; and Harris, Novarese and Wilson (2018) find similar results for the papers listed at RePEc.

Once we established the presence of sequential effects in the data, the second part of the chapter aims at exploring its potential mechanisms. We find suggestive evidence of three different cognitive biases: *contrast effects*, *narrow bracketing*, and *the generosity erosion*.

A contrast effect occurs when previous experiences affect current perceptions and decisions (Pepitone and DiNubile, 1976). In our setting, this would mean that candidates' assessment outcomes are influenced by the *performance* of the previous candidates seen by the same evaluators on the same day. Testing for this hypothesis, we find that a one standard deviation increase in the assessment of the former candidate decreases the evaluation of the next candidate by 0.2 points (3.6 percent of the mean), and his/her probability of passing the oral exam by 4 percentage points (6.5 percent of the average success rate). This result contributes to the very scarce literature analyzing the presence of contrast effects on different settings. For example, Pepitone and DiNubile (1976) find that undergraduate students recommend more lenient sentences for crime descriptions that follow a narrative of other particularly severe crimes. Hartzmark and Shue (2018) show that investors wrongly understand earnings news today as more remarkable if yesterday's earnings surprise was bad and less impressive if yesterday's news were good. The closest study to our research is Bhargava and Fisman (2014). They analyze contrast effects in a speed dating context and find that a 1 unit rise in prior target attractiveness leads to a 1.9 percentage point drop in current willingness to date. This effect represents 18 percent of the value of a current target's attractiveness.

The second mechanism we consider is narrow bracketing, and it occurs when a decision-maker who faces many decisions chooses an option in each case without properly consider the other decisions that he/she confronts (Griffin and Tversky, 1992). For illustration purposes, consider teachers who have been grading exams for several days. Today's exams are particularly good and the majority of the students should pass. However, as individuals exaggerate the extent to which small samples or brackets resemble large samples (Tversky and Kahneman, 1971), the evaluator may try to avoid huge deviations from what they expect to be the students' average success rate. In other words, if the teacher expects to pass about 50 percent of the class, he/she may be reluctant to approve all the exams he/she corrects in a given bracket to avoid to severely deviate from the expected rate of 50 percent of approvals (Simonsohn and Gino, 2013). In this chapter, we find evidence of narrow bracketing. In particular, a one standard deviation increase in the proportion of previous candidates who have passed the oral exam decreases applicant's grade in this oral presentation by 0.15 points, and his/her probability of passing by 2 percentage points. This represents 2.7 and 3.2 percent of the respective means. The most similar paper looking at narrow bracketing is Simonsohn and Gino (2013). They show that applicants to M.B.A schools in the U.S. who interviewed after several candidates were highly recommended were less likely to get the interviewers' approval.

Finally, we identify a novel mechanism that has not yet been studied in the literature of sequential effects: the *generosity erosion effect*. By this name, we refer to the fact that tribunal members may become less indulgent as the sequence evolves. This idea comes from behavioral economics. Bó (2005) or Engel (2011) show that, while players tend to cooperate at the beginning of the sequence in the dictator game, they become less cooperative or generous as the sequence unfolds. In our setting, we define as an act of generosity to grade a candidate with an exact 5.0 (i.e., the minimum grade applicants need to be able to continue with the selection process). We observe that a one standard deviation increase in the number of candidates who have received a 5.0 decreases the next candidate's grade by 0.12 points and his/her probability of passing by 2 percentage points. In other words, sparing one candidate negatively affects the probability of the next candidates to pass.

After testing each mechanism individually, we put them together in a model and find that the generosity-erosion effect has the most persuasive and strongest impact on juries' evaluation. We argue that the very large correlation (0.85) between the measures used to proxy for contrast effects and narrow bracketing explains why, when putting together, both mechanisms become insignificant in statistical terms. This is relevant as we follow the common measurements in the previous literature to identify these two cognitive biases. In this sense, our results cast doubts about the possibility of being able to isolate the effect of the two mechanisms.

We consider our work makes four main contributions to the literature on sequential effects and on cognitive biases. First, it is the first paper studying order effects in a recruitment process. If interviewers are prone to suffer from cognitive biases, they can induce firms to select and employ the wrong job seekers. Being aware of order effects in hiring processes may help to design strategies to minimize their adverse impacts and guarantee that outcomes are as fair and efficient as possible. In addition, the structure of this recruitment process is not only common to the hiring process for other public servants and high-skilled private-sector employees, but it also resembles well the admission process in universities, the students' evaluation process, or the investment and project decisions. Therefore, we believe that these findings can be extrapolated to a plethora of contexts.[2]

[2]Besides, our study looks at the selection process to hire teachers in public schools. Given that teachers have a long-lasting impact on student success (Rockoff, 2004; Rivkin,

Second, this paper contributes to the scarce literature looking at sequential effects in a context in which the decisions are not individual but made by a group. Indeed, most of the prior research has examined sequential effects in individual judgments.[3] However, in today's world, crucial decisions are often the result of a collective effort made by a group of experts (Hart, 1985). Critically, Kerr and Tindale (2004), Surowiecki (2004), or Koriat (2012) show that collective decision-making usually results in more accurate judgments −the so-called *the wisdom of the crowd*−. We show that sequential effects are still present and have a significant magnitude both statistically and economically in group-decisions. Unfortunately, at the moment we do not have data on the evaluators' grades at the individual level, which prevents us from being able to study whether the sequential effects are indeed weakened by the join decision process.

Third, we contribute to the literature that studies the cognitive biases explaining sequential effects. Albeit aware of the limitations, we find suggestive evidence of contrast effects and narrow bracketing in our data. However, we show that the usual proxies to measure these two cognitive biases are highly correlated, which hinders the possibility of completely isolating their impacts. Although more research in this area is needed, we propose and find evidence of a potential new mechanism that may have an important role in different settings: the generosity-erosion effect. According to this mechanism, the jury's compassion (in our context, given a candidate an exact 5.0) decreases as the sequence unfolds.

Finally, our paper differs from earlier research on sequential effects and its underlying mechanisms in that we exploit large, quasi-experimental evidence outside the laboratory. This reduces the concerns about external validity and generalisability.

The remainder of the paper is structured as follows: Section 2 describes the teachers' recruitment process. Section 3 presents and summarizes the data, and shows some descriptive evidence on order effects. Section 4 describes the methodology. Section 5 presents the evidence on order effects and the underlying mechanisms. Section 6 concludes.

Hanushek and Kain, 2005; Chetty et al., 2011), hiring the highest-quality teachers can have extensive positive consequences (Biasi, 2019).

[3] Ginsburgh and Van Ours (2003) is among the exceptions.

4.2 Institutional Setting

Competitive Exams to Recruit Teachers in Catalonia

To become a permanent primary, secondary, or vocational training teacher in a public school in Spain, candidates need to qualify in a two-stage merit-based evaluation carried out at the regional level. The first phase, *fase de oposición*, is divided into an oral and a written exam. After this phase, successful candidates move forward to the second stage of the competition, *fase de concurso*. In this second part, applicants are evaluated according to their résumé. The final result is a weighted average of the grades in the two stages of the exam. At the end of the process, the highest-ranked candidates obtain the available positions.

This paper uses evidence from the competitive exams held in Catalonia in 2019. Below, we explain in detail the timings of the competition, how candidates and committee members were selected, and the evaluation process.

Pre-Registration of Candidates

On January 2, 2019, the Catalan government announced the opening of 5,005 positions for permanent teachers (3,604 slots for primary teachers, 759 slots for secondary teachers, and 642 slots for vocational training teachers).[4] Prospective candidates had until February 4, 2019, to register. The application package consisted of a symbolic payment, personal information, the preferred location to take the exam, proof of meeting the academic requirements, and the position(s) candidates were applying to (e.g., secondary teacher of business administration and secondary teacher of commercial management).

Selection of Committees

Once the application deadline closed for the candidates, the five members that formed each committee were selected. The government counselor appointed the president of each committee among the eligible pool of evaluators (the eligibility requirements depending on the specialty). The other four members were chosen among voluntary and other eligible evaluators through a random public draw celebrated on March 7, 2019. The only constraint was that the number of voluntary evaluators could not exceed two per committee. Evaluators oversaw and graded both phases of the competitive exams.

[4]See Resolució edu/1/2019.

Exams' Structure and Evaluation

The first part of the competitive exam for the teaching positions, *fase de oposición*, was divided into two parts, an oral and a written exam. The oral exam lasted between June 5, 2019, and September 11, 2019. Only those applicants who obtained a 5 out of 10 in the oral exam could continue the recruitment process and take the written test.[5]

The order in which candidates did the oral test was random by design. Namely, within each specialty, the firsts to perform were those whose surname started by Y. This letter was determined randomly in a public lottery on December 13, 2018. During this test, candidates had 45 minutes to propose a syllabus for an academic year and to present a unit-plan of their choice. The final mark in the oral exam was calculated using the average grade each of the five evaluators that formed each committee decided. However, when the difference between the maximum and the minimum grades exceeded three points, the most extreme marks were eliminated. This process was only repeated once (i.e., it could be that even after dropping the most distant grades, the differences between the new maximum and the new minimum scores were larger than 3 points).

Those who passed the oral test had to pass a two-stage written exam. The first part was a case study, and the second consisted of the development of a random topic. Candidates who obtained more than 2.5 in each of the parts received an average for the written exam in which the first part weighted 70 percent and the second part 30 percent. The final mark in the *fase de oposición* was the average between the grades in the oral and written exams, conditional on having passed both.

Finally, all candidates that obtained between 5 and 10 in the *fase de oposición* moved forward to the *fase de concurso*. In this stage, applicants were evaluated according to their résumé. Special importance was given to experience and academic training. Other considered merits were the knowledge of different languages and additional training courses.

The final grade was a weighted average between the *fase de oposición* and *fase de concurso*. At the end of the process, the candidates with the highest

[5]In all Spanish regions but Catalonia, the first exam in the *fase de oposición* is the written exam. Those candidates who pass this exam can continue the recruitment process and perform the oral test. In 2019, Catalonia implemented for the first time the change in the structure of the *fase de oposición*, making the first exam the oral test. As we focus on this oral test, this new structure is much more interesting as it reduces the risk of selection (i.e., we have all individuals that participated in the hiring process rather than only those who have passed the written exam).

final scores occupied the available positions.

4.3 Data

The data used in this analysis was provided by the Catalan Department of Education, and it covers the full population of candidates who enrolled to participate in the high stakes evaluations that took place in Catalonia in 2019.

The data includes information on the candidates' gender, experience, aggregate grades in each part of the competitive process (one oral test, and two written exams), an identifier for the area the person was competing for (47 different specialties), and a tribunal identifier (there were 182 separate tribunals). On average, there was one available position for every four enrolled candidates.[6]

This paper focuses on the first part of the competitive exam: the oral test. From the 20,254 initial candidates, just 11,281 attended. The ones who did not participate were automatically excluded from the recruitment process. Related to the selection into participation, we observe that men and those with less experience were less likely to attend the exam.[7] Interestingly, the correlation between the participation in the test And the hour the candidate was expected to present is around 0 (-0.0009).[8]

Table 4.1 presents the descriptive statistics for the sample of candidates who attended the oral test. It shows that, among participants, 80 percent were women, and the average candidate had four years of previous experience.

Looking at how well applicants performed in the oral test (the main variable of interest), we see that the success rate was 62 percent (s.d. = 49 percent) and that the average grade was 5.5 (s.d. = 2.11). Figure 4.1 shows the distribution of the grades in this oral test. We see a clear bunching at 5.0, the minimum grade candidates needed to obtain to be able to continue with the selection process. This suggests that examiners tended to favor the pass when in doubt.[9]

[6] Recall that the number of available positions was 5,005. This call for permanent teaching positions in the Catalan public schools was the largest since the beginning of the Great Recession.

[7] Recall that experience was one of the main inputs for the *fase de concurso*. This means that candidates with no previous experience were in important disadvantage. This is because even if they pass the *fase de oposición*, the *fase de concurso* weights around 50 percent.

[8] Figure A.1 presents the distribution of participants and non-participants over time. We can observe that most of the exams took place in July.

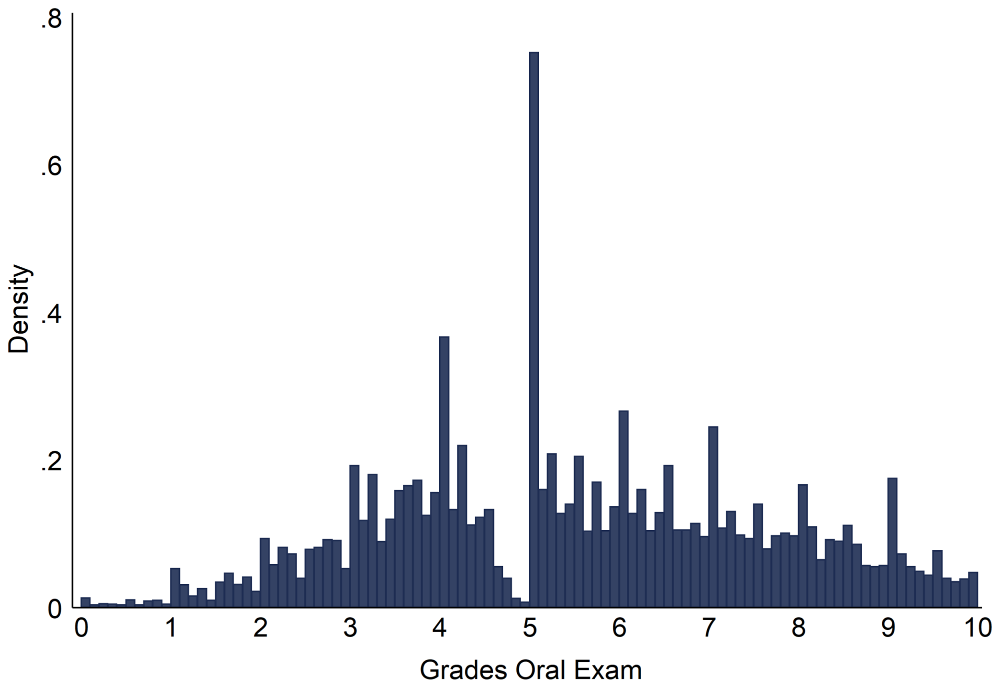
[9] Figure A.2 shows the distribution of grades by gender. We see a larger (lower) mass of male candidates at the bottom (top) of the distribution, in comparison to female candidates. The figure A.2 also shows that the presence of bunching is more salient among female candidates.

Table 4.1: Descriptive Statistics

Variable	Mean	Std. Dev.	Min.	Max.	Obs.
Main Outcome Variable					
Grade oral test	5.5	2.11	0	10	11,281
Pass oral test	0.62	0.49	0	1	11,281
Main Covariate					
Order	3.30	1.82	1	12	11,281
Other Covariates					
Female	0.80	0.40	0	1	11,281
Experience (years)	4.06	4.39	0	30.83	11,281
Grade case-study	5.46	1.87	0.4	10	6,950
Pass case-study	0.60	0.49	0	1	6,950
Grade topic-development	5.96	1.89	0	10	6,923
Pass topic-development	0.71	0.45	0	1	6,923
Total score *fase de oposición*	6.72	1.53	5	9.96	4,973

Note: The table includes information on all candidates who attended the oral exam in Catalonia in 2019. The number of observations in the written exams is lower because only those who passed the oral test could continue with the selection process. The total score in the *fase de oposición* is captured only for those who approved it (i.e., it is the average conditional on having at least a 5.0).

Figure 4.1: Grades Distribution in the Oral Test

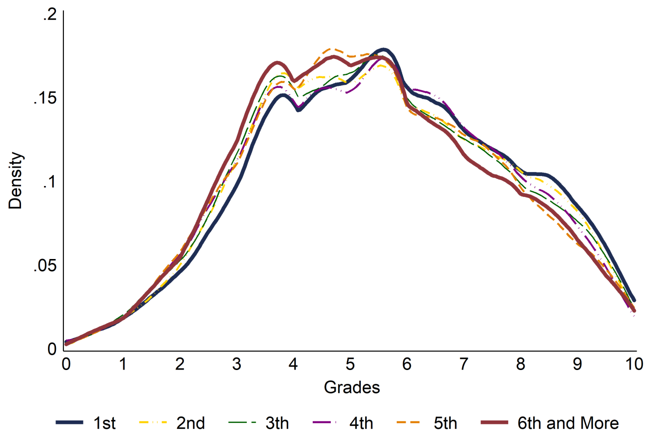


Note: This graph shows the distribution of the grades in the first part of the competitive exam: the oral test. Graphically, we observe an important bunching at 5.0, suggesting that when in doubt, tribunal members may have been benevolent with candidates.

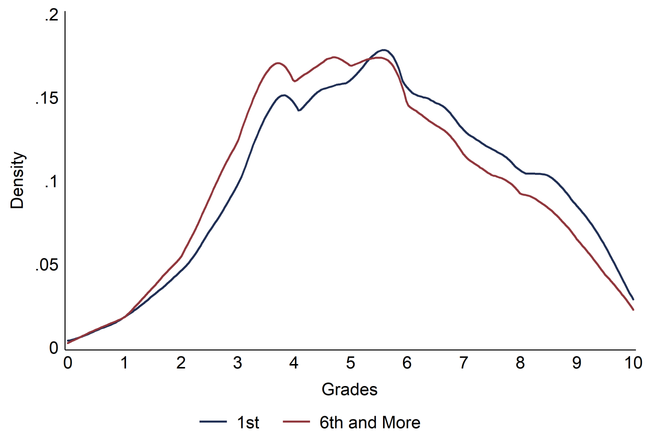
Figure 4.2 shows the distribution of grades in the oral test by the order in which candidates performed the exam.

Figure 4.2: Grades Distribution by Order

(a) All Positions



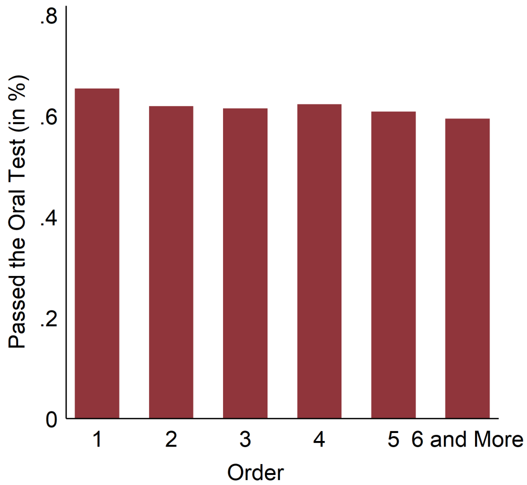
(b) Positions 1 and 6



Note: This graph shows the distribution of the grades in the oral test by the order in which candidates presented. Graphically, we observe that the distribution of grades moves towards the left when the sequential order increases. We show figure 4.2b, comparing the grade distribution of those who presented first with those who did the oral exam at the end of the day, to ease visualization. Notice also that the average grade of those who presented first was 5.66 (s.d. = 2.1), and the one for those who did the exam at the end was 5.36 (s.d. = 2.09). This suggests that order may have an impact on candidates' evaluation. Figure A.3 replicates this exercise dividing the sample by gender.

We observe that the distribution shifts towards the left when the sequential order increases. To ease visualization, figure 4.2b compares the grade distribution for those who presented first with respect to those who did the oral test in the sixth or further positions. We can see a larger (lower) mass of those candidates who went later in the sequence at the bottom (top) of the distribution, compared to those who did the exam in the first spot. Figure 4.3 shows that this order effect is also present when looking at the probability of passing the oral exam.[10]

Figure 4.3: Distribution of Approved by Order



Note: This graph shows the percentage of candidates who approved the oral test by order of presentation. Among those who presented first, the approval rate was 65.02 percent (s.d. = 48). This percentage dropped to 59.15 percent (s.d. = 49) among those who presented sixth or more.

Those candidates who approved this test could continue the selection process with a two-stage written exam. From the 7,005 applicants who obtained a five or more in the oral test, 6,950 attended to the first part of the written exam, a case study. The average grade in this stage was around 5.46, with a success rate of 60 percent. Finally, 6,923 applicants participated in the second part of the written exam, the development of a random topic. The average grade in this test was 5.96, and 71 percent passed it.[11] In the end, 4,973 applicants approved the *fase de oposición* (i.e., 24.6 percent of those who enrolled and 44 percent of those who attended to the oral exam).

4.4 Methodology

The main goal of this paper is to estimate whether there are sequential effects in the oral exams used to hire primary, secondary, and vocational training teachers in Spain. This is, we want to measure if the position in which candidates have presented the oral exam has affected their assessment in such test. To do so, we consider an econometric model where the dependent variable Y_{itd} (1) is categorical and takes the value 1 if tribunal t graded the oral exam can-

[10]See figure A.3 to see how these distributions change by gender.

[11]Figure A.4 shows the distribution of the grades in the two phases of the written exam.

didate i did on day d with a 5.0 or more, and 0 otherwise; or (2) is the grade tribunal t gave candidate i in the oral exam he/she did on day d.

$$Y_{itd} = \alpha + \sum_{n=2}^{6} \beta_n Position_{itdn} + \theta X_i + \gamma_t + \delta_d + \epsilon_{itd} \qquad (4.1)$$

The variable *Position* indicates that the participant i presented the oral exam to tribunal t, on day d, in position n.[12] The vector X_i contains participant-level control variables such as gender and years of experience. γ_t controls for tribunal fixed-effects, and δ_d for the day of the exam fixed-effects. ϵ_{itd} represents the unobserved error term. In all specifications, we cluster the standard errors at the tribunal level to account for correlation in unobserved components of the outcome at the within tribunal level.

Equation 4.1 allows us to ask: Among all candidates who did the exam in a given day d with a given tribunal t, how did the order of presentation affect their evaluations? Particularly, the coefficient β_n measures the effect of being in position n on the probability of passing the exam and on the grade candidates obtained relative to those who did the exam in the first spot.

4.5 Results

4.5.1 Sequential Effects

Table 4.2 presents the results of estimating the parameters β_n in equation 4.1 using OLS. In particular, the table shows the effect of performing in different slots -relative to do the exam first- on applicants' evaluations.[13]

The first column shows the raw estimates. Those who presented second obtained 0.13 points less than those who did the exam first. This *sequential penalty* increases to 0.18 for those who took the test in the third position, to 0.21 for those who did the exam fourth, to 0.24 for those who were in the fifth slot, and to 0.31 for those who performed in position sixth or more. These penalties represent between 2.3 and 5.45 percent of the average candidates' grade in the oral test (5.5). This pattern remains mostly unchanged when we introduce candidates' characteristics in column 2.[14]

[12] We grouped the 642 candidates that presented in positions above 6 in category 6.

[13] These estimates group all candidates in positions 7-12 to 6, but otherwise have a dummy variable for each position.

[14] Candidates' characteristics are gender, experience, and experience squared.

Table 4.2: Effects of Order on Candidates' Evaluations

	(1)	(2)	(3)	(4)
Outcome	**Grade in the Oral Exam**			
Second	-0.13**	-0.11*	-0.11*	-0.11*
	[0.063]	[0.062]	[0.062]	[0.062]
Third	-0.18**	-0.18**	-0.17**	-0.17**
	[0.071]	[0.069]	[0.070]	[0.070]
Fourth	-0.21***	-0.20***	-0.19***	-0.19***
	[0.064]	[0.063]	[0.064]	[0.064]
Fifth	-0.24***	-0.23***	-0.22***	-0.21***
	[0.066]	[0.065]	[0.066]	[0.066]
Sixth and More	-0.31***	-0.29***	-0.19**	-0.18**
	[0.082]	[0.081]	[0.076]	[0.077]
Outcome	**Passed the Oral Exam**			
Second	-0.03*	-0.02	-0.02	-0.02
	[0.015]	[0.014]	[0.015]	[0.015]
Third	-0.04**	-0.04**	-0.03**	-0.03**
	[0.016]	[0.015]	[0.015]	[0.015]
Fourth	-0.03**	-0.03**	-0.03**	-0.03*
	[0.014]	[0.014]	[0.014]	[0.014]
Fifth	-0.04**	-0.04**	-0.03**	-0.03**
	[0.016]	[0.015]	[0.015]	[0.015]
Sixth and More	-0.06***	-0.05***	-0.03*	-0.03
	[0.018]	[0.018]	[0.018]	[0.018]
Observations	11,281	11,281	11,281	11,281
Candidate characteristics		✓	✓	✓
FE tribunal			✓	✓
FE day exam				✓

Note: Column 1 presents the raw estimates of the order of presentation on candidates' grades and the probability of passing the oral exam. Column 2 includes candidate characteristics (i.e., gender, experience, and experience squared). Column 3 adds tribunal fixed-effects, and column 4 includes day of the exam fixed-effects. Robust standard errors corrected for clustering at the tribunal level are in square brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

The inclusion of tribunal fixed-effects in column 3, and day of the exam fixed-effects in column 4 slightly decrease the sequential penalty. Namely, those who presented second obtained 0.11 points less than those who did the exam in the first slot. The ones in the third and the fourth positions got 0.17 and 0.19 points less than those in the first one. The sequential penalty for those who did the exam fifth was 0.21, and those who went sixth or more obtained 0.18 points less than those who presented first. This penalty represents between 2 and 3.8 percent of the average grade in the oral exam.

The bottom panel of table 4.2 shows the results of estimating the parameter β in equation 4.1 with a Linear Probability Model and using as dependent variable the probability of passing the oral exam. The raw estimates in column 1 show that candidates who presented in positions 2, 3, 4, 5, and 6 were between 3 and 6 percentage points less likely to pass the oral test than those who did the exam first. Including participant characteristics, and tribunal and day of the exam fixed-effects leave the pattern mostly unchanged. Overall, candidates who presented later in the sequence were around three percentage points less likely to obtain a 5.0 or more with respect to those who did the exam in the first position. Looking at the magnitude of the coefficients, they represent around 5 percent of the average success rate.

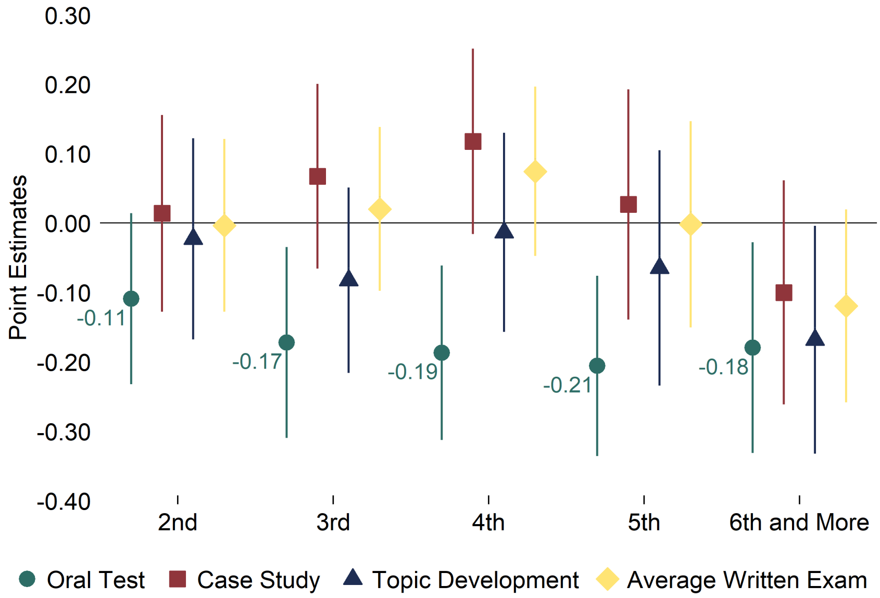
While random assignment guarantees that the order in which candidates did the oral exam is uncorrelated to participants' characteristics, it could be that, *by chance*, those presenting in different slots were unequal in ability or other features. To shed some light on this, we estimate equation 4.1 using as dependent variables the grades candidates obtained in the case-study exam, the development of a random topic, and the average grade in the written test.[15] We also look at the probability of passing the three aforementioned competitions. If candidates' abilities do not differ across the order in which they presented the oral exam, we should see that the estimated coefficients for the parameters β_n in equation 4.1 are indistinguishable from zero when looking at these placebo outcomes.

Figure 4.4 presents the results. Indeed, it shows that the order in which candidates presented the oral exam had no effect on the grades participants obtained in the next stages of the recruitment process, nor in the probability of passing the subsequent tests. This evidences that the results presented in table 4.2 are not caused by inherent differences in candidates' abilities over the sequence.

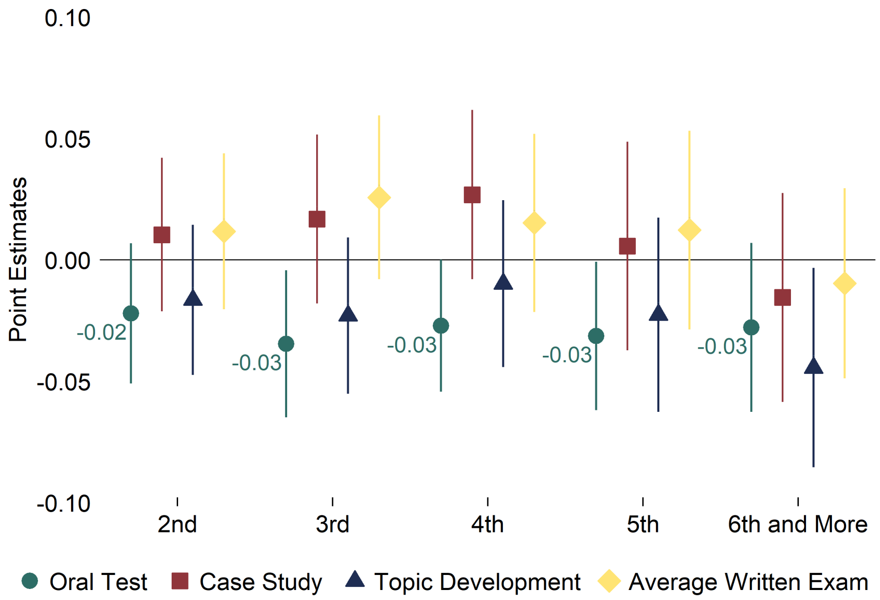
[15]Recall that the average grade in the written exam is a weighted average of the grades candidates obtain in the case-study and the development of a random topic. For this, the former weights 70 percent and the latter 30 percent.

Figure 4.4: Order Effects in Competitive Exams

(a) Candidates' Grades



(b) Candidates' Probability to Pass



Note: The graphs present the results of estimating equation 4.1 for different outcomes. All models include candidates' characteristics and tribunal and day of the exam fixed-effects. Robust standard errors are clustered at the tribunal level. The coefficients represented with a circle are the same as the ones presented in column 4 of table 4.2. The effects of order of presentation in the oral exam on the grade and probability to pass the case-study (the development of a random topic) are represented with a square (triangle). Finally, the diamonds represent the estimated coefficients of the parameter β_n using as dependent variables the average grade and probability of passing the written exam. The solid lines around the point estimates are the 95 percent confidence intervals.

The previous results point towards the presence of *primacy effects* on candidates' evaluations. To deep into this hypothesis, figures 4.5a and 4.5b show the results of estimating equation 4.1 using as the key predictor a categorical variable that takes the value 1 if candidate i presented the oral exam on day d with tribunal t in the first position, and 0 otherwise. The results show that those participants who were first obtained around 0.17 (s.e. = 0.05) points more than those who presented in other slots (i.e., their grades were 3 percent higher than the average). In addition, they were 3.3 (s.e. = 0.01) percentage points more likely to pass the oral exam. In terms of magnitude, this represents 5.3 percent of the average success rate. To put this in perspective, doing the oral exam in the first position has an equal impact than having one additional year of teaching experience.[16]

Figures 4.6a and 4.6b replicate the previous exercise using as the key predictor a binary variable that takes the value 1 for those candidates who did the oral exam with tribunal t on day d in the last spot, and 0 otherwise. Albeit the coefficients are negative, we do not observe any statistical differences in the grades they obtained or in their probability of passing the exam in relation to those applicants who have done the exam earlier within the day. Therefore, candidates in the last positions were not *particularly* penalized.

The results in this study go in line with the scarce but growing literature showing the presence of primacy effects on sequential decisions. For example, Danziger, Levav and Avnaim-Pesso (2011) report that the likelihood that a parole judge rules in favor of the prisoner decreases over the day. Meredith and Salant (2013) use evidence from the randomly assigned ballot order in the California city council and school board elections to study the presence of sequential effects. They find that candidates listed first win office 55.1 percent of the races. This is 4.8 percentage points more races than expected absent of order effects. Feenberg et al. (2017) show that NBER papers randomly ranked first in the *New This Week* email receive 30 percent more views, downloads, and citations than those ranked in latter positions. Harris, Novarese and Wilson (2018) find similar evidence for the papers listed at RePEc. Finally, Fedyk (2018) uses the random positioning of news on Bloomberg terminals to study the effects of being on the front page. She shows that news articles at the front-page induce 280 percent higher trading volumes than other similar news at less prominent positions in the next 10 minutes.

The goal of the next section is to investigate further the nature and mech-

[16] The figures also show that those who did the oral test first did not obtain greater/lower grades in the subsequent parts of the competitive exams, nor were they more or less likely to approve them. This suggests that candidates' abilities did not differ over the sequence.

anisms behind this primacy effect.

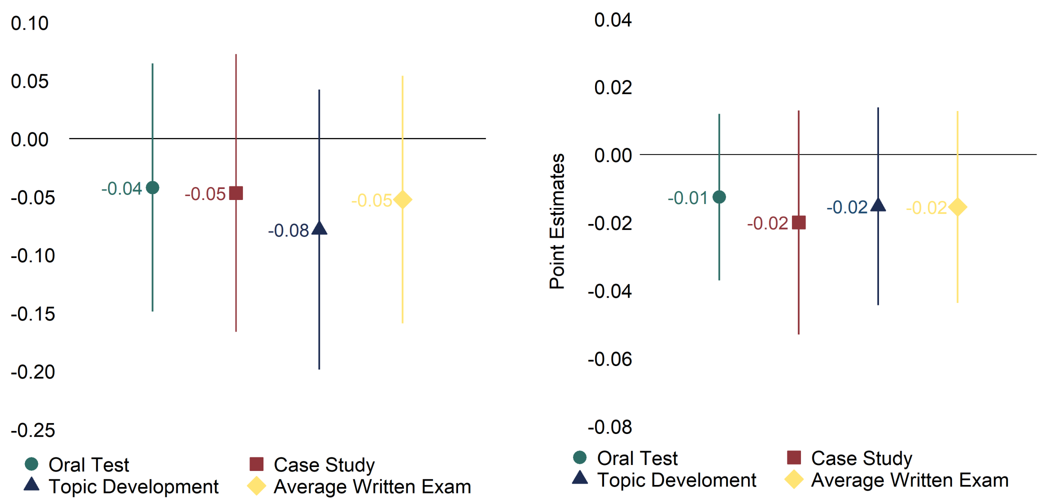
Figure 4.5: Effect of Doing the Oral Exam in the First Slot

(a) Grades (b) Pass

Note: The graphs present the results of estimating the next equation $Y_{itd} = \alpha + \beta First_{itd} + \theta X_i + \gamma_t + \delta_d + \epsilon_{itd}$ for different outcomes using as the key predictor a categorical variable that takes the value 1 if the candidate presented the oral test in the first position, and 0 otherwise. All specifications include applicants' characteristics and tribunal and day of the exam fixed-effects. In all models, robust standard errors are clustered at the tribunal level.

Figure 4.6: Effect of Doing the Oral Exam in the Last Slot

(a) Grades (b) Pass



Note: The graphs present the results of estimating the following equation $Y_{itd} = \alpha + \beta Last_{itd} + \theta X_i + \gamma_t + \delta_d + \epsilon_{itd}$ for different outcomes using as the key predictor a categorical variable that takes the value 1 if the candidate presented the oral test in the last position, and 0 otherwise. All specifications include applicants' characteristics and tribunal and day of the exam fixed-effects. In all models, robust standard errors are clustered at the tribunal level.

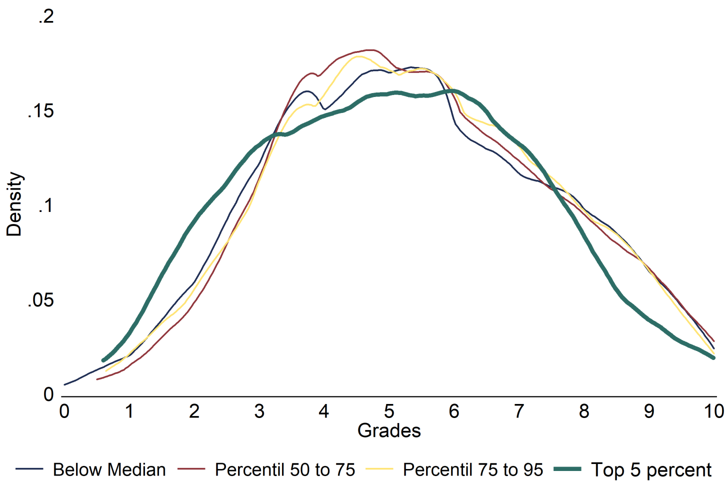
4.5.2 Mechanisms

This section aims at identifying the mechanisms that explain the primacy effects observed in the data. For this part, we only keep those candidates who presented in the fifth or higher positions to be able to identify the drivers of the results in section 4.5.1. This restriction limits the sample to 2,857 candidates.

Contrast Effects

A contrast effect occurs when the assessment of previously evaluated observations inversely biases the perception of the next ones. For example, Pepitone and DiNubile (1976) show that students judge crimes to be less severe following exposure to narratives of very cruel crimes (e.g., an assault is judged to be less serious when a homicide precedes it); Bhargava and Fisman (2014) find that, in a speed dating context, subjects are more likely to reject the next candidate if the previous one was very attractive; or Hartzmark and Shue (2018) find that investors wrongly perceive earnings news today as more (less) impressive if unrelated yesterday's earnings surprise was bad (good).

Figure 4.7: Contrast Effects



Note: This graph shows the distribution of candidates' grades depending on how well the prior applicant performed in the oral test. We see that those who followed a top performer (in this context, the top 5 percent were those who obtained a 9 or more in the exam) are more (less) represented at the bottom (top) of the grade distribution. This could be because they are "less-skilled" candidates, or because evaluators become harsher after seeing a very good candidate.

This section tests the presence of contrast effects on the grades candidates to the position of tenured public teachers obtained in an oral presentation. First, figure 4.7 presents the grades distribution of candidates depending on how well the prior applicant has performed. We can see clear descriptive evidence that the distribution of grades moves to the left for candidates who did the oral presentation after a top-performer.

To formally study the presence of contrast effects on candidates' evaluations, we estimate the following econometric model:

$$Y_{itdn} = \alpha + \beta_k Y_{itdn-k} + \theta X_i + \lambda n_i + \gamma_t + \delta_d + \epsilon_{itdn} \tag{4.2}$$

where Y_{itdn} indicates the evaluation jury t gave to the candidate i, who did the exam on day d in position n. The variable Y_{itdn-k} is the standardized assessment of the four candidates who presented with the same tribunal and within the same day, but in the four slots before the candidate we are interested in.[17] The vector X_i contains participant-level control variables such as gender and years of experience. n_i controls for the position in which candidate i presented the oral exam. γ_t controls for tribunal fixed-effects, and δ_d for the day of the exam fixed-effects. ϵ_{itdn} represents the unobserved error term. In all specifications, we cluster the standard errors at the tribunal level. The parameter of interest in equation 4.2 is β_k. It measures the influence of previous candidates' evaluations on the grade the applicants who did the exam in position n received. In the absence of contrast effects, the estimated coefficients for the parameters β_k should be indistinguishable from 0.

Figure 4.8 presents the results. The coefficients represented with a circle are the ones of interest (as they refer to the evaluation in the oral exam). The first panel (figure 4.8a) shows that if the previous candidates' evaluation increases by one standard deviation, the individual rating decreases by 0.14 points (s.e. = 0.04). The grade that the candidate two slots before received is also important for the applicant's grade. In particular, a one standard deviation increase in the evaluation of the candidate who did the exam in position $n-2$ received decreases applicant's evaluation by 0.11 points (s.e. = 0.04). Previous candidates' evaluations do not impact (or only marginally in the case of the $n-4$ participant) the individual's grade.

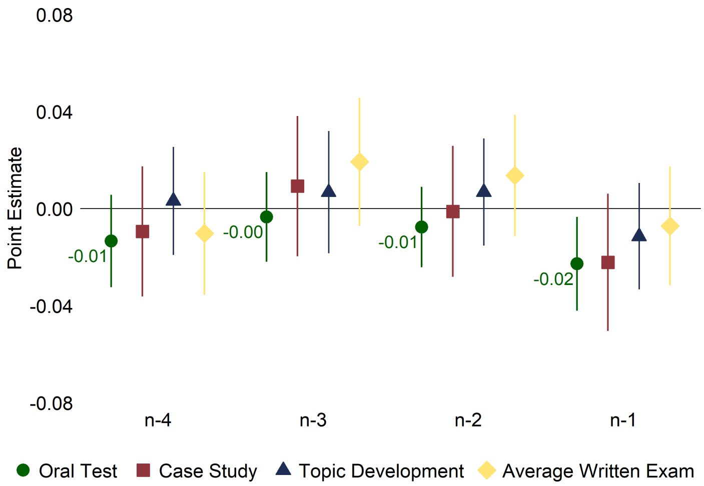
Figure 4.8b looks at how the performance of previous candidates in the oral test affected the probability of passing for the next applicants. We see that a one standard deviation increase in the grade the previous candidate has received decreases in 2 percentage points (s.e. = 0.01) the likelihood of the applicant to pass. The evaluations of candidates who presented earlier in the sequence do not significantly affect the probability of passing for the candidates who presented in the slot n.

[17] $k \in \{-4, -3, -2, -1\}$

Figure 4.8: Contrast Effects in Competitive Exams

(a) Candidates' Grades

(b) Candidates' Probability to Pass



Note: The graphs present the results of estimating equation 4.2 for different outcomes. All models include candidates' characteristics, the order of presentation, and tribunal and day of the exam fixed-effects. Robust standard errors are clustered at the tribunal level. The coefficients represented with a circle look at the effect of previous candidates' evaluations on the evaluation of the oral exam of the applicant who presented in position n. The effects of prior candidates' evaluation in the oral exam on the grade and probability to pass the case-study (the development of a random topic) are represented with a square (triangle). Finally, the diamonds represent the estimated coefficients of the parameter β_k using as dependent variables the average grade and probability of passing the written exam. The solid lines around the point estimates are the 95 percent confidence interval around the coefficients.

One possibility is that these estimates are capturing differences in abilities. This is, it could be that candidates who presented in the fifth slot were systematically worse than those who did the exam in the fourth one. The previous section already shows evidence that this is not the case. Yet, figure 4.8 presents the results of estimating how the evaluation of previous candidates in the oral exam affected the performance of the applicant n in the written tests.

If the effect was still present, then the results could be caused by differences in skills rather than contrast effects. However, the placebo estimations suggest that this negative correlation is not due to differences in abilities.

To show that these results are not driven by limiting the analysis to those who did the exam from the fifth position onwards, figure A.5 shows additional evidence using the whole sample. In this case, we estimate how the evaluation of the previous candidate affects the next candidate's assessment.[18] The results confirm the previous findings. Namely, a one standard deviation increase in the grade of the candidate who did the exam in position $n-1$ decreases the evaluation of applicant in position n by 0.2 points (s.e. = 0.02), and his/her probability of passing the exam by 4 percentage points (s.e = 0.01).

Figure A.6 presents an additional robustness check. In a scenario with contrast effects, candidates following very bad applicants would appear to be better, while candidates following very good applicants would seem worse than they are. Therefore, the effects should be particularly relevant at the extremes (Mussweiler, 2003). To study this relation, we estimate the following econometric model in equation 4.3, where $DY_{k,n-1}$ is a dummy variable indicating the performance quintile of the candidate in position $n-1$.[19]

$$Y_{itdn} = \alpha + \sum_{k=1}^{5} \beta_k DY_{k,n-1} + \theta X_i + + \lambda n_i + \gamma_t + \delta_d + \epsilon_{itdn} \tag{4.3}$$

We see that −being the reference following a person whose grade is in the third quintile− doing the exam after a candidate in the first one (the lowest grades) increases next candidates' assessment by 0.21 points (s.e. = 0.07), and his/her probability of passing by 5 percentage points (s.e. = 0.02). Those who did the exam after someone whose grade was in the second quintile do not obtain higher or lower grades with respect to those who did the exam after someone whose assessment was in the middle quintile. Those who presented after someone in the fourth and fifth quintiles obtained 0.16 (s.e. = 0.07), and 0.38 (s.e. = 0.07) points less than those who took the oral test after someone in the third one, and their probability of passing was 3 (s.e. = 0.02) and 5 (s.e. = 0.02) percentage points lower respectively.

Overall, the results show that a candidate's assessment is influenced by the

[18] As we are just looking at the most recent effect, we can maintain all observations except the first person who presents per tribunal and day.

[19] The grades of those in the first quintile are lower than 3.61. Grades between 3.61 and 5.0 are grouped in the second quintile. The third one encompasses grades lower than 6.0056. Grades in the fourth quintile are between 6.0056 and 7.5. Finally, the last quintile has all the grades higher than 7.5.

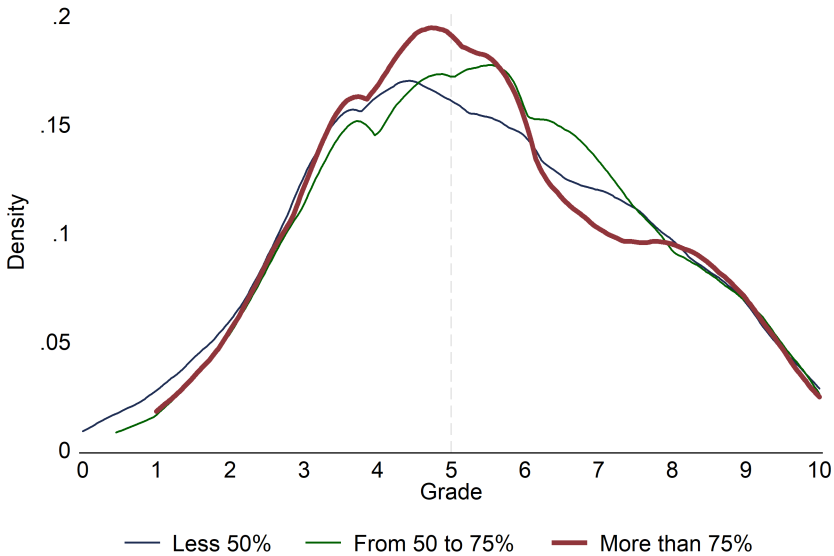
other candidates who have been seen by the same jury on the same day.

Narrow Bracketing

Narrow bracketing occurs when the decision-makers fail to integrate the consequences of many similar choices (Kahneman and Lovallo, 1993; Read et al., 1999; Thaler, 1999). For instance, when grading exams over several days, evaluators may be biased by the tests they saw on a given day rather than using the cumulative information on all the exams they graded over time. And even if the grade of an exam may depend on how well the rest of the class did (it may be an indicator of the difficulty of the exam), it should definitively not depend on the subset of exams the evaluator graded on a given day or *bracket*.

Simonsohn and Gino (2013) argued that evaluators engaging in narrow bracketing might, implicitly or explicitly, keep mental score of their evaluation and try to avoid deviations from what they expect to be the mean-evaluation of the population. For example, if they expect that 70 percent of the students will pass the test, evaluators may be biased to approve around 70 percent of the exams they correct in every bracket. Indeed, using 10 years of data on M.B.A. applications to an American business school, Simonsohn and Gino (2013) find that interviewers who have already recommended three applicants on a given day were less likely to do the same for a fourth one.

Figure 4.9: Narrow Bracketing



Note: This graph shows the distribution of candidates' grades depending on the proportion of previous applicants who have approved the exam.

In this section, we want to analyse whether the proportion of previous candidates who passed the exam affects the success rate of the following candidates in the oral test. Figure 4.9 presents the grades distribution of applicants depending on the percentage of prior candidates who have approved. We can see a larger mass between the 3.5 and the 4.9 of those applicants who did the oral test after more than 75 percent of the previous candidates approved. However, this descriptive evidence is not perfectly clear as there is also a larger mass of candidates who *just passed* who did the oral test after more than 75 percent of the previous candidates approved.

To formally study the presence of narrow bracketing on candidates' evaluation, we estimate the following econometric model:

$$Y_{itdn} = \alpha + \beta Passed_{t,d} + \theta X_i + \lambda n_i + \gamma_t + \delta_d + \epsilon_{itdn} \tag{4.4}$$

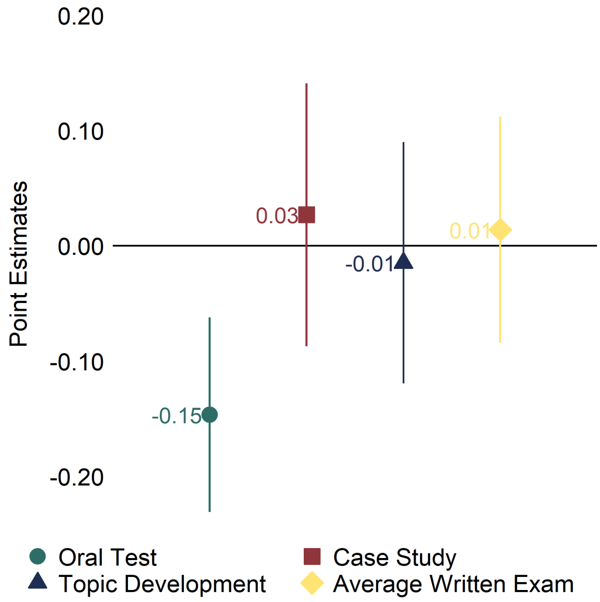
where Y_{itdn} indicates the evaluation jury t gave to the candidate i, who did the exam on day d in position n. The variable $Passed_{t,d}$ it is a standardized measure of the proportion of candidates that were evaluated by jury t on day d before candidate i and who have passed the oral exam. The vector X_i contains participant-level control variables such as gender and years of experience. The variable n_i indicates the order in which candidate i did the oral exam. γ_t controls for tribunal fixed-effects, and δ_d for the day of the exam fixed-effects. ϵ_{itdn} represents the unobserved error term. In all specifications, we cluster the standard errors at the tribunal level. In this model, β is the coefficient of interest, and it measures the influence of the approval rate of previous applicants on candidate i's evaluation.

Figure 4.10 presents the estimated coefficients for the parameter β. We can see that a one standard deviation increase in the proportion of previous candidates who have passed the oral exam decreases applicant's grade in the presentation by 0.15 points (s.e. $= 0.04$), and her/his probability of passing the oral exam by 2 percentage points (s.e. $= 0.01$). As expected, the percentage of candidates who approved the oral exam before candidate i has no effect on i's assessment in the other parts of the recruitment process (the case-study, the development of a random topic, and the final grade in the written exam). This suggests that these results cannot be attributed to differences in abilities.

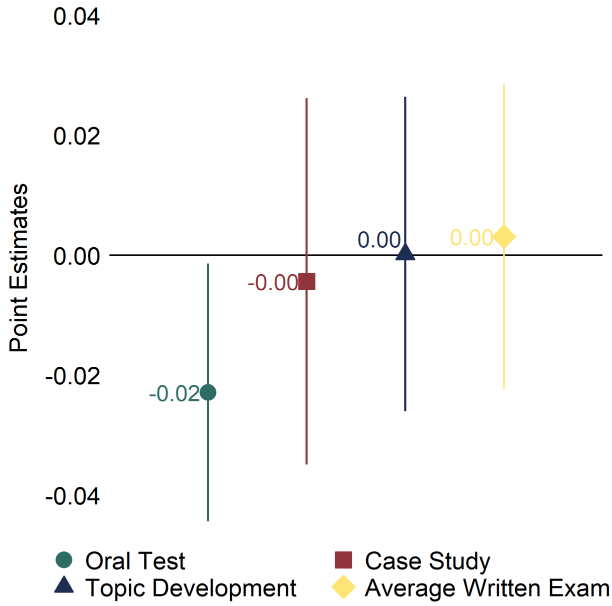
To show that these findings are not driven by limiting the sample to the last candidates, we replicate the previous exercise using all applicants. The results, in figure A.7, show that a one standard deviation increase in the proportion of previous candidates who approved the exam decreases next applicants' assessment by 0.23 points (s.e. $= 0.03$), and his/her probability of passing the oral exam by 5 percentage points (s.e $= 0.01$).

Figure 4.10: Narrow Bracketing in Competitive Exams

(a) Candidates' Grades



(b) Candidates' Probability to Pass



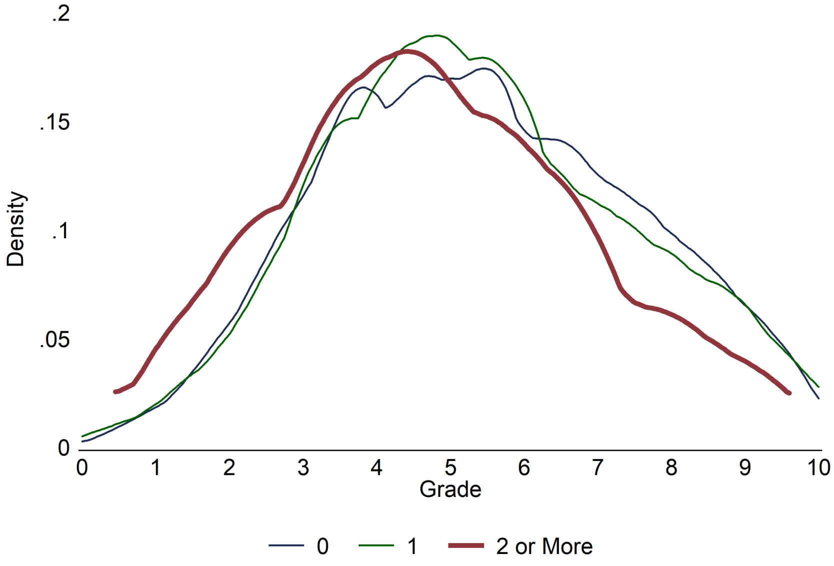
Note: The graphs present the results of estimating equation 4.4 for different outcomes. All models include candidates' characteristics, the order of presentation, and tribunal and day of the exam fixed-effects. Robust standard errors are clustered at the tribunal level. The coefficients represented with a circle study how the proportion of approved candidates who did the exam before i affect i's assessments. The effects of prior approved candidates in the oral exam on the grade and probability of passing the case-study (the development of a random topic) are represented with a square (triangle). Finally, the diamonds represent the estimated coefficients of the parameter β using as dependent variables the average grade and probability to pass the written exam. The solid lines around the point estimates are the 95 percent confidence interval around the coefficients.

Generosity Erosion

We consider a final mechanism that has not been discussed previously in the literature of sequential effects, but that may be very relevant in our context: the *erosion of compassion* over the sequence. A group of evaluators grading candidates can be understood as an interaction between two parties in which one has all the power to decide the outcome, and the other does not, that is, a dictator game (Engel, 2011). Interestingly, people tend to be generous in the first iterations of a sequential dictator game but become less cooperative as more rounds unfold (Bó, 2005; Engel, 2011; Dal Bó and Fréchette, 2018). Thus, repetition erodes generosity. In an evaluation setting, giving a weak candidate a pass when it was unclear whether they deserved it can be understood as an act of generosity. According to the social decision-making literature, as the sequence unfolds, candidates will become more likely to fail because the jury will be less lenient. In other words, evaluators might be willing to spare one candidate from failing, perhaps two or three, but at some point, they will become tired of being too forgiving. We call this the generosity-erodes effect.

Figure 4.11 shows descriptive evidence of this phenomenon. We can see that when two or more candidates obtained an exact 5.0, the grades distribution of the next applicants moved importantly towards the left.

Figure 4.11: Generosity Erosion



Note: This graph shows the distribution of candidates' grades depending on the number of people who received an exact 5.0 before.

To formally study the presence of the generosity erosion effect on candidates' evaluation, we estimate the following econometric model:

$$Y_{itdn} = \alpha + \beta Minpass_{t,d} + \theta X_i + \lambda n_i + \gamma_t + \delta_d + \epsilon_{itdn} \quad (4.5)$$

where Y_{itdn} indicates the evaluation jury t gave to the candidate i, who did the exam on day d in position n. The variable $Minpass_{t,d}$ is a standardized measure of the number of candidates that were evaluated by jury t on day d before candidate i and who passed the oral exam with an exact 5.0. The vector X_i contains participant-level control variables such as gender and years of experience. n_i controls for the position in which candidate i took the oral exam. γ_t controls for tribunal fixed-effects and δ_d for the day of the exam fixed-effects. ϵ_{itdn} represents the unobserved error term. In all specifications, we cluster the standard errors at the tribunal level. β is the coefficient of interest, and it measures the influence of the jury members being *lenient* with previous candidates on candidate i's evaluation.

Figure 4.12 presents the estimated coefficients for the parameter β. Our results demonstrate that candidates' likelihood to pass an exam substantially decreases if previous candidates receive the lowest grade accepted to move forward in the public examination (5.0). Namely, a one standard deviation increase in the number of previous candidates who obtained a 5.0 reduces the next applicants' grade by 0.12 (s.e. = 0.04) points and his/her probability of passing the exam by 2 percentage points (s.e. = 0.01). In other words, sparing one candidate negatively affects the probability of the next candidates to pass.

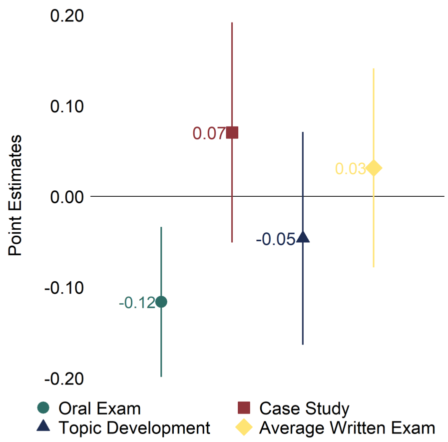
To show that these findings are not driven by sample selection, figure A.8 replicates the previous analysis with all candidates. The results show that a one standard deviation increase in the number of previous candidates who obtained a 5.0 reduces the next applicants' grade by 0.04 (s.e. = 0.02) points and his/her probability of passing the exam by 1 percentage point (s.e. = 0.00).

Finally, as a placebo exercise, we repeat the analysis using as the key independent variable the number of candidates who obtained different grades (1.0, 2.0,...,9.0) on the next applicants' evaluations. Figure A.9 shows that apart from the previously reported effects with the 5.0, there is also something around the 4.0 and the 6.0. In particular, a one standard deviation increase in the number of candidates who obtained a 4.0 increases next candidates' grades by 0.07 (s.e. = 0.04); and a one standard deviation increase in the number of previous applicants with a 6.0 decrease next candidates' average grade by 0.09 (s.e. = 0.04) and his/her probability of passing by 3 percentage points (s.e.= 0.01). We believe that this effect around the 5.0 is explained because the grades we observe are the average of the marks each of the five evaluators decided.

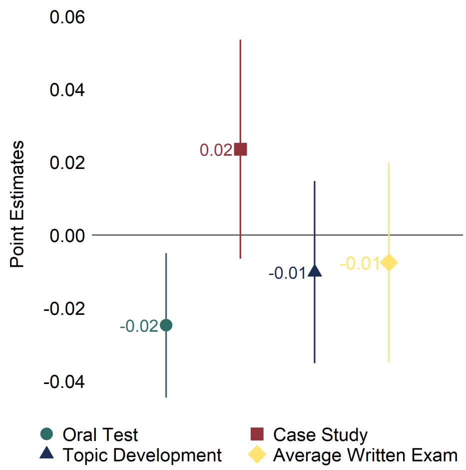
This is, there may be evaluators who though that the candidate with a 4.0 (6.0) deserved to pass (fail), and that the other tribunal members were being very harsh (lenient). This would have made him/her more (less) generous with the following candidate to avoid another *unjustified* fail (pass). Unfortunately, we were not able to obtain the individual grades of each committee member, so we cannot completely disregard other potential reasons that may have caused these findings.

Figure 4.12: Generosity Erosion in Competitive Exams

(a) Candidates' Grades



(b) Candidates' Probability to Pass



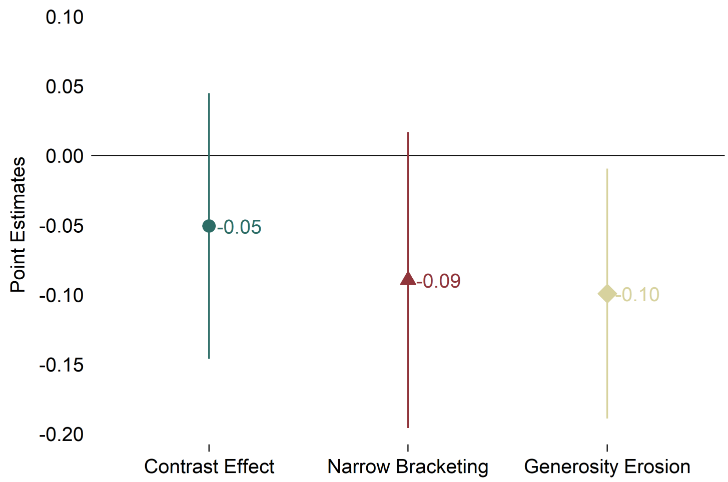
Note: The graphs present the results of estimating equation 4.5 for different outcomes. All models include candidates' characteristics, the order of presentation, and tribunal and day of the exam fixed-effects. Robust standard errors are clustered at the tribunal level. The coefficients represented with a circle look at how the number of previous candidates who obtained a 5.0 affect next applicants' evaluation in the oral test. The effects of prior approved candidates with an exact 5.0 in the oral exam on the grade and probability to pass the case-study (the development of a random topic) are represented with a square (triangle). Finally, the diamonds represent the estimated coefficients of the parameter β using as dependent variables the average grade and probability of passing the written exam. The solid lines around the point estimates are the 95 percent confidence interval around the coefficients.

Combining Mechanisms

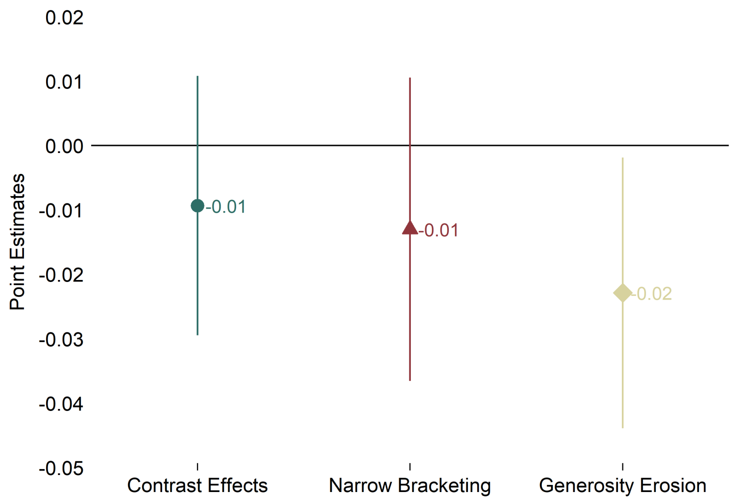
Finally, we combine the three aforementioned mechanisms in the same model. As we can see in figure 4.13, the only cognitive bias that keeps stable and statistically significant is the one referring to generosity erosion. In particular, a one standard deviation increase in the number of previous candidates who obtained an exact 5.0 decreases the next applicant's grade by 0.1 points and her/his probability of passing the oral exam by 2 percentage points.

Figure 4.13: Cognitive Biases in Competitive Exams

(a) Candidates' Grades



(b) Candidates' Probability to Pass



Note: This model includes all the cognitive biases together. All models include candidates' characteristics, the order of presentation, and tribunal and day of the exam fixed-effects. Robust standard errors are clustered at the tribunal level.

Table 4.3 shows a very large correlation between the measures we use to identify narrow bracketing and contrast effects. This could explain why, when putting together, these two mechanisms are not longer statistically significant. Given that the proxies we use to measure contrast effects and narrow bracketing are standard in the literature, this finding cast doubts about the possibility of isolating the effects of these two cognitive biases.

Table 4.3: Correlation Matrix

	Contrast Effects	Narrow Bracketing	Generosity Erosion
Contrast Effects	1	**0.87**	0.08
Narrow Bracketing		1	0.09
Generosity Erosion			1

Note: This table presents the correlation between the measures we use to identify the three explored cognitive biases.

4.6 Conclusions

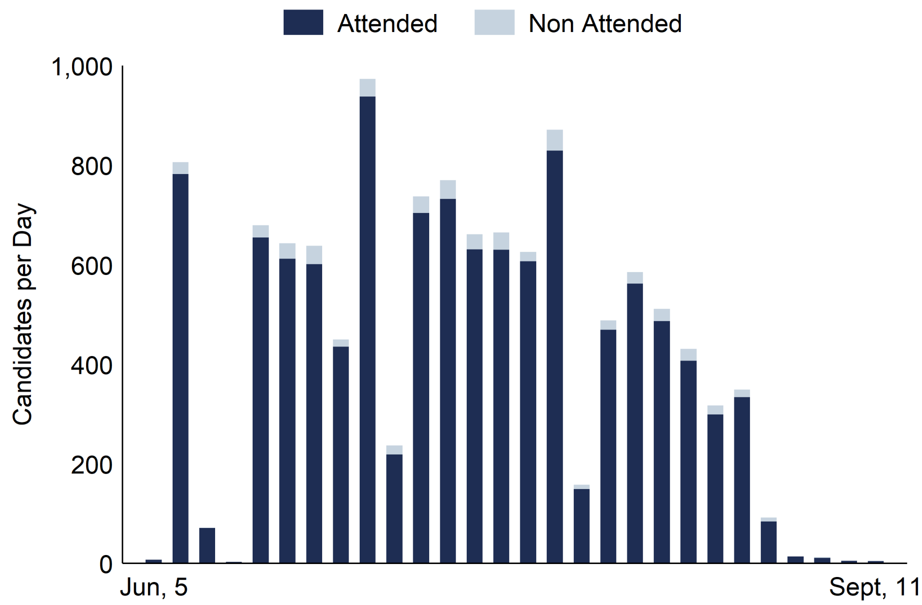
This chapter exploits the random order in which candidates to permanent teaching positions do an oral exam to explore sequential effects in recruitment processes. Our results show evidence of *primacy effects*. Irrespective of ability, applicants who do the oral presentation first obtained 0.17 points more (3 percent of the mean grade) and were 3.3 percentage points more likely to pass (5.3 percent of the average success rate). These findings show that minor changes in the candidate's sorting and ordering can have major consequences on their future labor market careers. This casts serious doubts about the efficiency and fairness of different recruitment processes.

In addition, the chapter contributes to the scarce literature looking at the cognitive biases causing the sequential effects. In line with previous literature, we find evidence on contrast effects −a candidate's assessment is better (worse) if the performance of the previous candidate observed by the same tribunal in the same day is very bad (very good)−, and narrow bracketing −the higher the percentage of previous applicants who have passed the exam, the harsher evaluators are with the following applicants−. Albeit both biases are very relevant in our analysis, we show a very large correlation (0.87) between them, which hinders the ability to isolate their impacts. Besides, we find suggestive evidence on a new mechanisms: generosity erosion. This is, we show that sparing one candidate negatively impacts the assessment of the following applicant. This is consistent with evaluators becoming less cooperative or generous as the sequence unfolds (Engel, 2011).

Albeit we cannot fully disregard other potential explanations (e.g., ego depletion), the results are in line with previous literature and suggest that more research is needed to design efficient and fair recruitment processes. In this line, the key is to create neutral selection processes. Previous work by Autor and Scarborough (2008) or Hoffman, Kahn and Li (2018) show that algorithm-based job testing technologies may be helpful to reduce or correct human subjective biases. A cheaper and potentially useful alternative is just to inform decision-makers about the possibility that these cognitive biases affect their evaluations (Alesina et al., 2018).

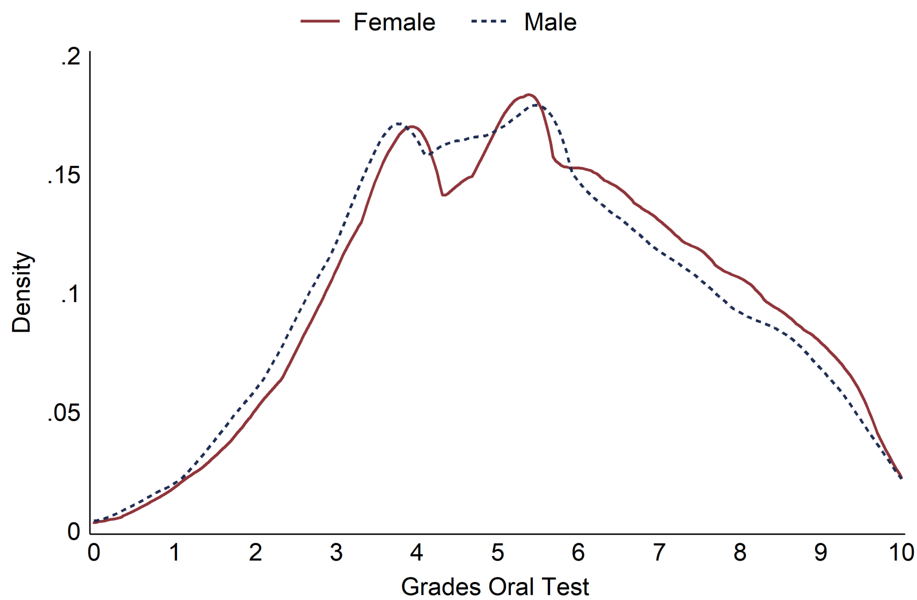
4.7 Appendix

Figure A.1: Distribution of Candidates per Day



Note: This graph shows the distribution of the candidates who were expected to present the oral exam per day. It shows in the dark blue the number of candidates who decided to attend the exam and in light blue the ones who did not attend.

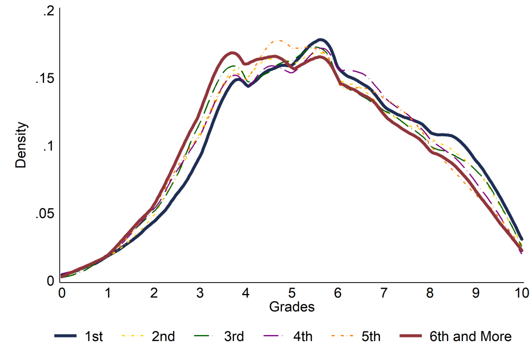
Figure A.2: Distribution of Grades by Gender



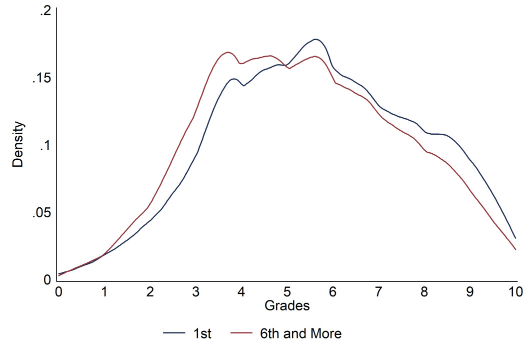
Note: This graph shows the distribution of grades in the oral exam by gender. We observe a larger mass of male candidates at the bottom of the distribution, while there is a larger mass of female candidates at the top. We can also see in this figure the presence of bunching, which is especially relevant in the women's distribution.

Figure A.3: Effect of Order on the Oral Exam by Gender

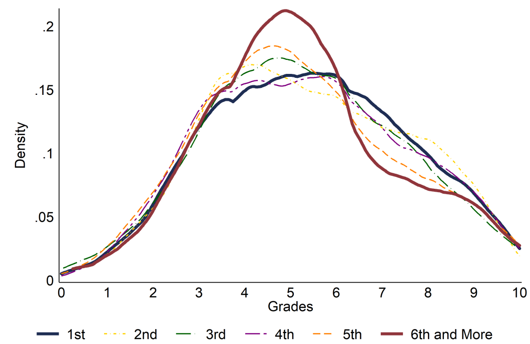
(a) All Positions - Women



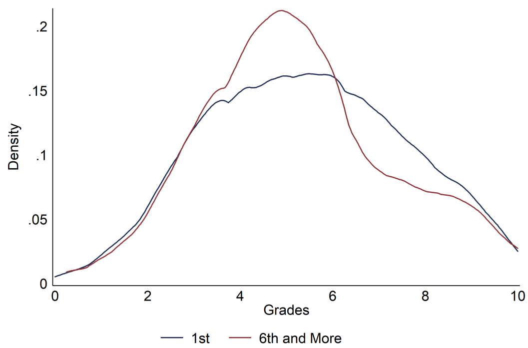
(b) Positions 1 and 6 - Women



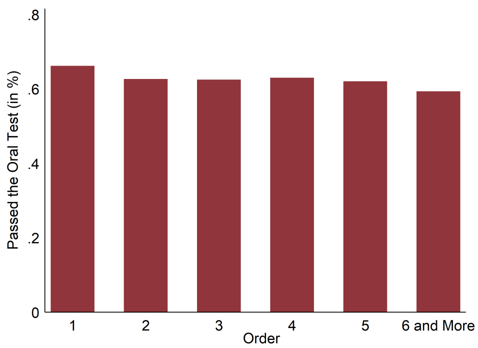
(c) All Positions - Men



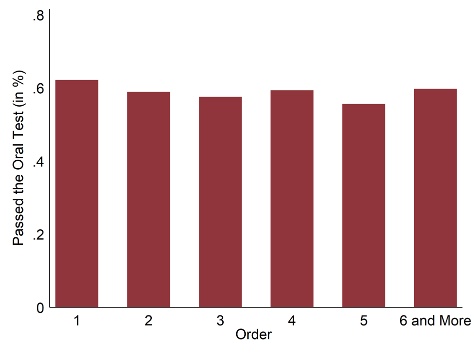
(d) Positions 1 and 6 - Men



(e) Approved by Order - Women



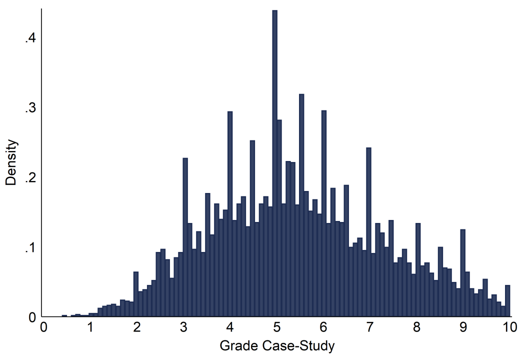
(f) Approved by Order - Men



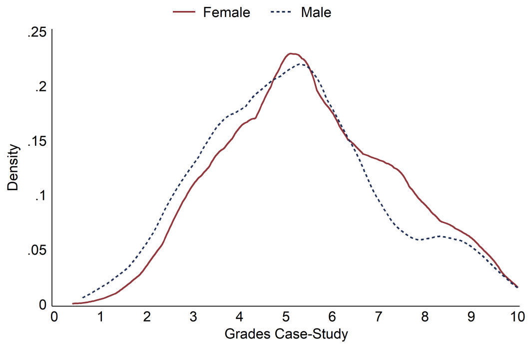
Note: The graphs A.3a - A.3d present the grade distribution by order and gender. We can see that for women, the distribution moves to the left as sequential order increases. For men, there are no such differences at the very bottom (from 0 to 3). Yet, those who present earlier in the sequence outperform those who do the exam later at the top of the grade distribution. For both men and women, the probability of passing the exam is larger among those who do the oral exam in the first slot (see figures A.3e and A.3f).

Figure A.4: Grades Distribution Written Exam

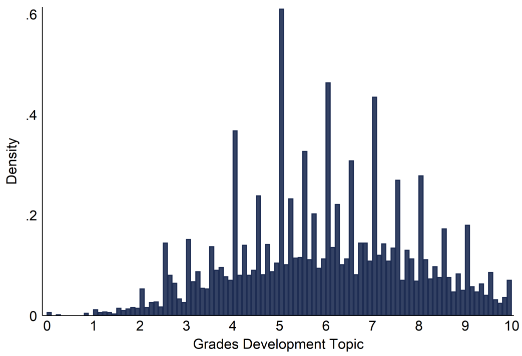
(a) Grades Distribution Case-Study



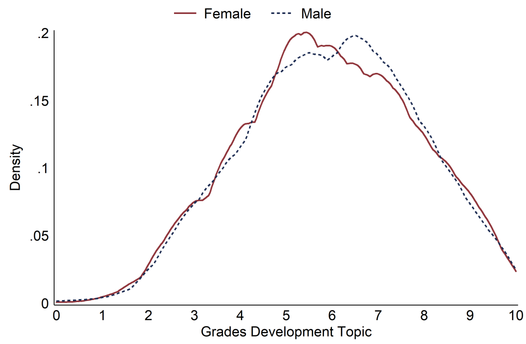
(b) Distribution of Grades by Gender



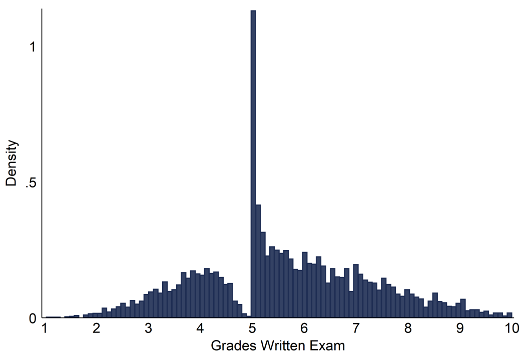
(c) Grades Distribution Topic Development



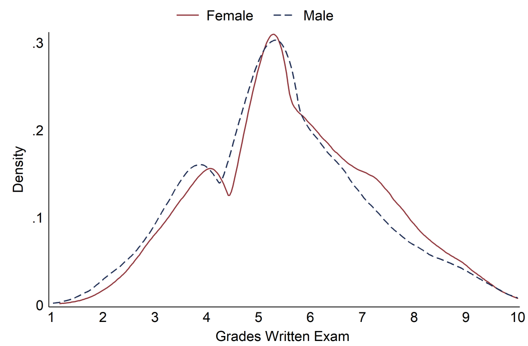
(d) Distribution of Grades by Gender



(e) Grades Distribution Written Exam



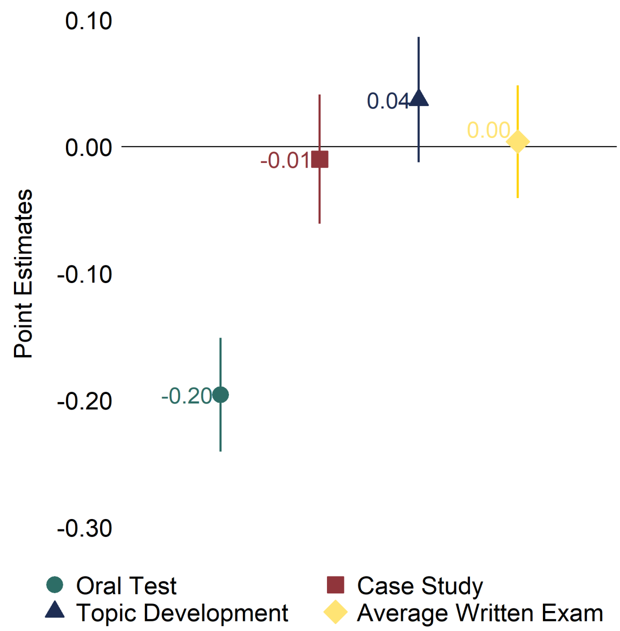
(f) Distribution of Grades by Gender



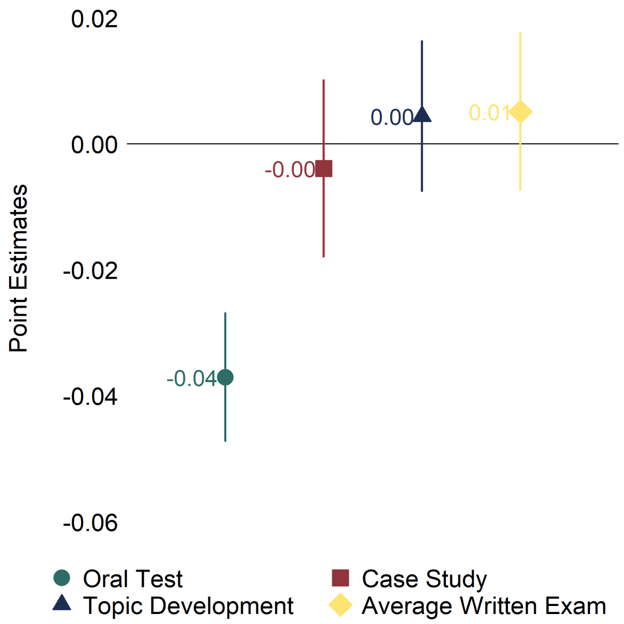
Note: Figures A.4a, A.4c, and A.4e show the distribution of grades in the two stages of the written exam and its average (in which the case-study accounts for 70 percent and the development of a random topic for 30 percent). We still observe important bunching at 5.0. In addition, there seems to be a tendency to round the grades at .0 or .5. Looking at gender differences, we see that women outperform men in the case-study, while men slightly outperform women in the development of a random topic. Interestingly, we do not observe the clear gender differences in bunching that figure A.2 shows (This could be related to the fact that the written exams are anonymous, while on the oral presentation committee members can observe the gender of the participant (Breda and Hillion, 2016)).

Figure A.5: Contrast Effects in Competitive Exams

(a) Grades



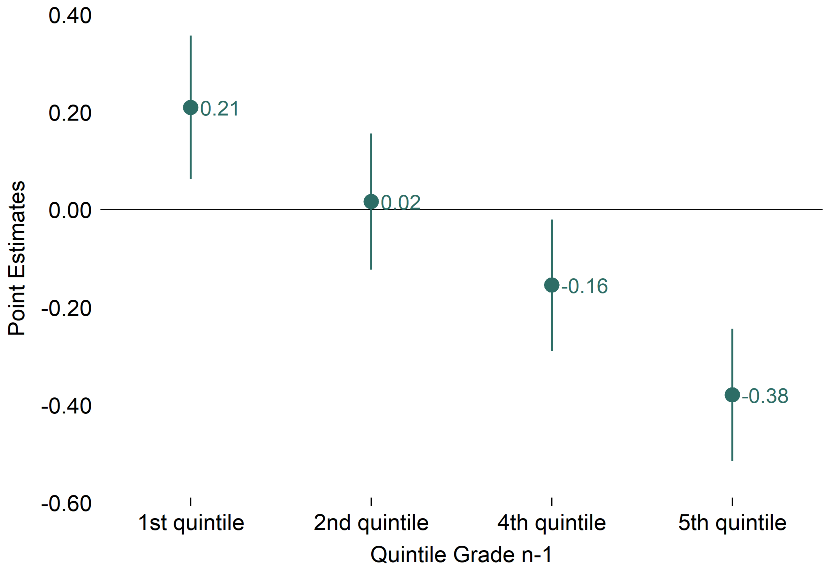
(b) Pass



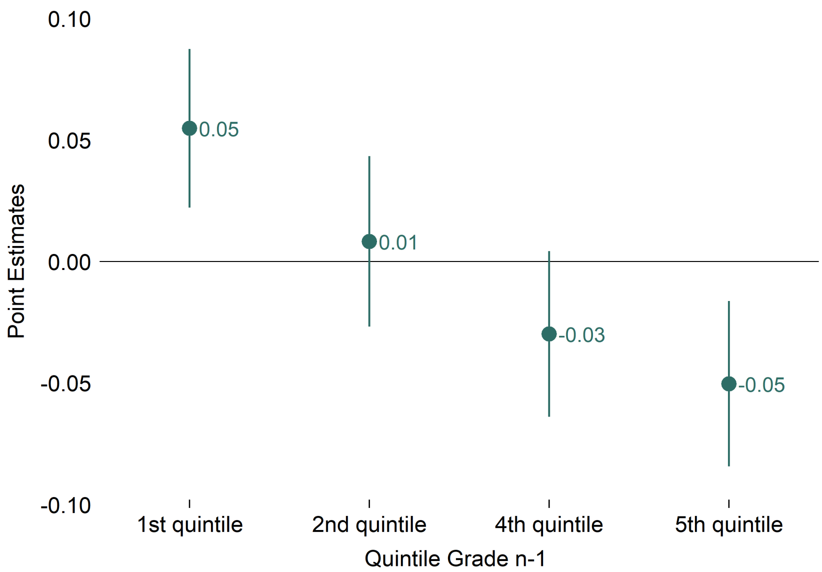
Note: The graphs present the results of estimating the next equation $Y_{itdn} = \alpha + \beta Y_{itdn-1} + \theta X_i + \lambda n_i + \gamma_t + \delta_d + \epsilon_{itd}$ for different outcomes using as the key predictor a standardized measure of the grade the previous candidate obtained in the oral exam. All specifications include applicants' characteristics, the order of presentation, and tribunal and day of the exam fixed-effects. In all models, robust standard errors are clustered at the tribunal level.

Figure A.6: Contrast Effects in Competitive Exams

(a) Candidates' Grades



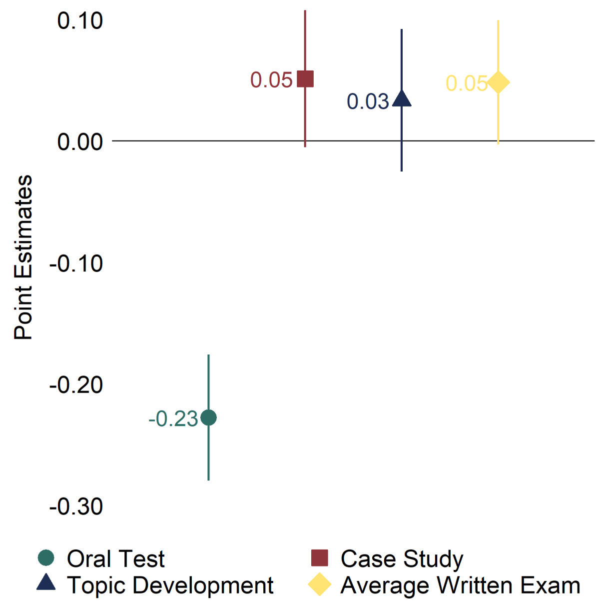
(b) Candidates' Probability to Pass



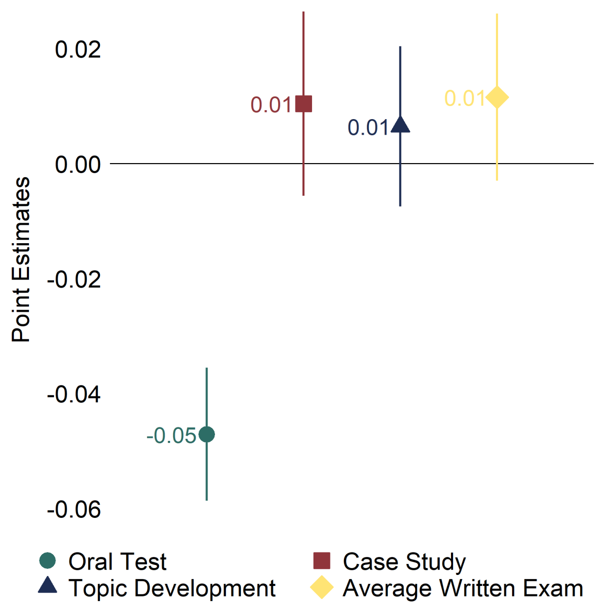
Note: The graphs present the results of estimating equation 4.3. All models include candidates' characteristics, the order of presentation, and tribunal and day of the exam fixed-effects. Robust standard errors are clustered at the tribunal level. The solid lines around the point estimates are the 95 percent confidence interval around the coefficients. The grades of those in the first quintile are lower than 3.61. Grades between 3.61 and 5.0 are grouped in the second quintile. The third one encompasses grades lower than 6.00056. Grades in the fourth quintile represent grades between 6.0056 and 7.5. Finally, the last quintile has all the grades higher than 7.5.

Figure A.7: Narrow Bracketing in Competitive Exams

(a) Candidates' Grades



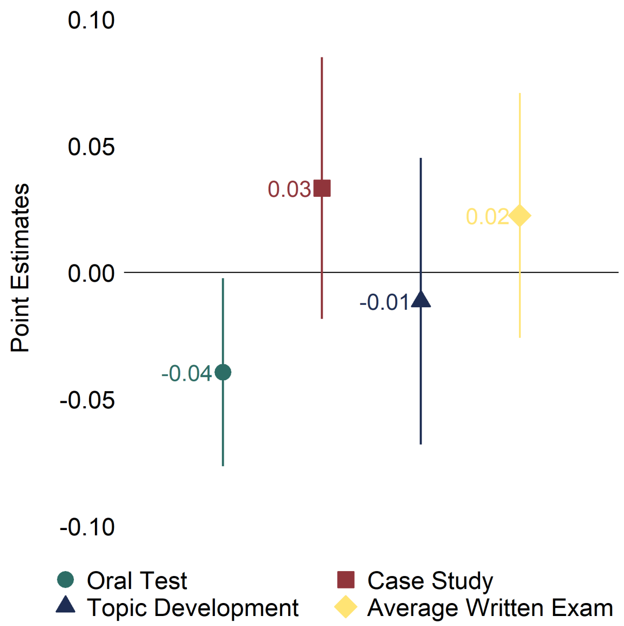
(b) Candidates' Probability to Pass



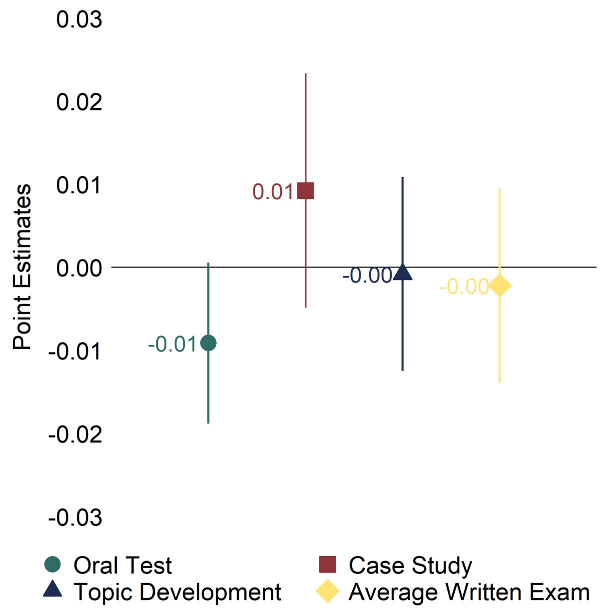
Note: The graphs present the results of estimating equation 4.4 for different outcomes. All models include candidates' characteristics, the order of presentation, and tribunal and day of the exam fixed-effects. Robust standard errors are clustered at the tribunal level. The coefficients represented with a circle study how the proportion of approved candidates who did the exam before i affect i's assessments in the oral test. The effects of prior approved candidates in the oral exam on the grade and probability to pass the case-study (the development of a random topic) are represented with a square (triangle). Finally, the diamonds represent the estimated coefficients of the parameter β using as dependent variables the average grade and probability of passing the written exam. The solid lines around the point estimates are the 95 percent confidence interval around the coefficients.

Figure A.8: Generosity Erosion in Competitive Exams

(a) Candidates' Grades



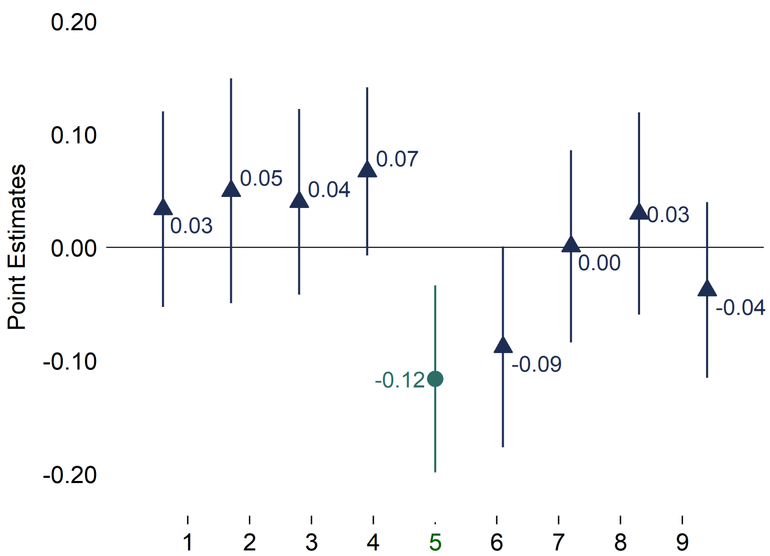
(b) Candidates' Probability to Pass



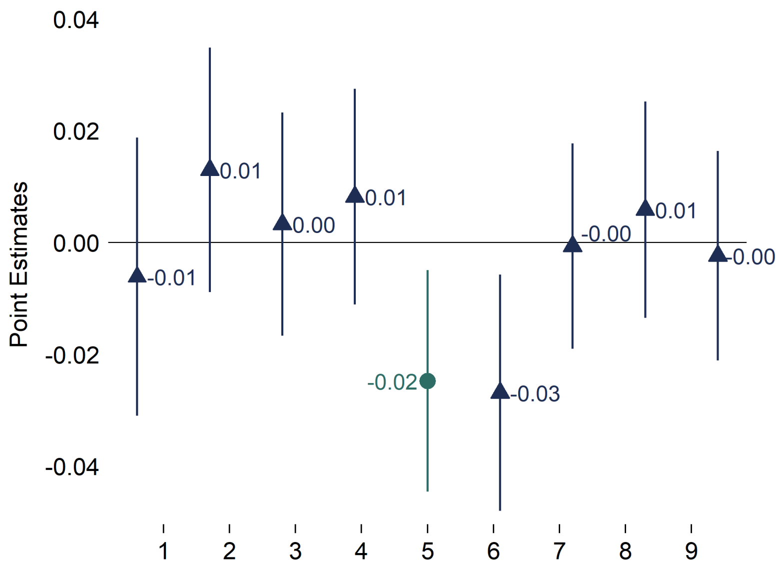
Note: The graphs present the results of estimating equation 4.5 for different outcomes. All models include candidates' characteristics and tribunal and day of the exam fixed-effects. Robust standard errors are clustered at the tribunal level. The coefficients represented with a circle look at how the number of previous candidates who obtained a 5.0 affect applicants' evaluation in the oral test. The effects of priorly approved candidates in the oral exam on the grade and probability to pass the case-study (the development of a random topic) are represented with a square (triangle). Finally, the diamonds represent the estimated coefficients of the parameter β using as dependent variables the average grade and probability of passing the written exam. The solid lines around the point estimates are the 95 percent confidence interval around the coefficients.

Figure A.9: Generosity Erosion in Competitive Exams

(a) Candidates' Grades



(b) Candidates' Probability to Pass



Note: The graphs present the results of estimating equation 4.5 using as the key predictor a standardized measure of the number of each grade in the x-axis previous candidates have obtained. All models include candidates' characteristics and tribunal and day of the exam fixed-effects. Robust standard errors are clustered at the tribunal level. The coefficients represented with a circle look at how the number of previous candidates who obtained a 5.0 affect applicants' evaluation on the oral test. The solid lines around the point estimates are the 95 percent confidence interval around the coefficients.

Chapter 5

Conclusions

This Ph.D. thesis studies two topics very relevant for the well-functioning of the labor markets: workers' mobility and recruitment processes.

Related to labor mobility, the aim is to understand why, despite large differences in unemployment rates within Spain, internal mobility rates are so low. Explanations such as language and cultural differences, or geographic-specific human capital accumulation may not be so important when looking at the within-country level. Therefore, I focus my analysis on labor protection and on the historical organization of the family.

Related to labor protection, the second chapter of the thesis studies the causal effects of a sudden and unanticipated reform that reduced the generosity of the unemployment insurance (UI) benefit on geographical mobility. On July 11, 2012, the government announced that all workers who started an unemployment spell after July 14, 2012, would have a ten percentage points reduction in their UI after the sixth month of unemployment. Using very rich administrative data from social security records and a regression discontinuity design, I show that the UI cut increased internal mobility across provinces by 4 percentage points (24 percent of the pre-reform mean). The increase in mobility was driven by young educated men without family responsibilities moving towards the big cities. Besides, I also find suggestive evidence showing that those workers who moved have found jobs two months earlier than comparable stayers.

These findings are consistent with the view that generous UI benefits represent important frictions to labor market adjustments. The policy implications suggest that front-loading the payment of the unemployment benefits (i.e., large replacement rates at the beginning of the unemployment spell, but decreasing steeply over the duration of the unemployment) can potentially increase job search-effort (e.g., intensifying geographical mobility), and shorten the unemployment spell. In this line, Hungary implemented a reform in 2005 that consisted of the front-loading of the UI (Lindner and Reizer, 2016). Future work studying how this policy affected migration decisions would be a very interesting addition to the second chapter of the thesis.

In the third chapter, I study how the socially predominant family structure in the past affects today's mobility decisions. I depart from the fact that cultural norms are sticky (Alesina et al., 2015), and that different family organizations promote different intensities in family ties (Salamon, 1982). Therefore, even if the past family structures are no longer existent, they may still shape today's decisions. To look at this, I exploit the rich historical regional variation in the organizations of the family within Spain. Namely, there were two main family organizations: stem and egalitarian nuclear (Reher, 1998). Stem families were characterized by intergenerational co-residence and indivisible inheritance. In egalitarian nuclear families, on the other hand, children became independent at adulthood, and the family wealth was equally divided among all descendants. These two family organizations, stable and prevalent in Spain since the Middle Ages until the second half of the XIX century, were associated with different family ties. In particular, according to Bras and Van Tilburg (2007), societies with stem family organizations were characterized by stronger family ties.

Using administrative data from social security to measure mobility, and the 1860 census to measure family types, I estimate the effect of the family structure in the past on internal migration using a linear probability model.

The results show evidence that those people born in areas where stem families were historically predominant are less mobile nowadays. Using an Instrumental Variable approach to correct for potential issues of omitted variables and reverse causality, the results go in the same direction.

Albeit the idea is to continue this study looking at Europe, these first results suggest that cultural norms are very persistent even at the within-country level. Therefore, centralized policies may have very heterogeneous effects if they do not account for the historical variables shaping the different cultures.

The fourth chapter of the thesis explores how cognitive biases affect hiring decisions in a recruitment process. To become a permanent teacher in a public school in Catalonia, applicants have to pass a competitive exam. The first stage of this exam is an oral test. Interestingly, the order in which applicants present this test is completely random. Using data on the universe of candidates, we find that, independent on ability, those applicants who were randomly allocated to present first were five percent more likely to pass than the other candidates.

We propose three explanations for the presence of the so-called *primacy effects*. First, we find evidence of contrast effect (i.e., the performance of the previous candidate inversely affects the next candidate's assessment). Second, the higher the proportion of candidates who have already passed, the less likely the evaluators are to pass the following applicants. This cognitive bias is called narrow bracketing. Third, the evaluator's propensity to cooperate (give the candidate the minimum to continue with the selection process) decreases as the sequence unfolds.

We think these results are very important. First, to the best of our knowledge, this is the first study looking at order effects in the context of a recruitment process. Given that the selection process we study is common to a lot of areas, we believe that our results cast serious doubts about the efficiency and fairness of different hiring processes. In this line, we think that decision-makers (be that a university, a firm, or the government) should put more effort into building hiring processes neutral to the potential biases that could appear. Following Autor and Scarborough (2008), the use of artificial intelligence could help to correct of minimize cognitive biases, albeit it can create other potential problems (Yarger, Payton and Neupane, 2019). Another interesting and not particularly expensive policy could be to inform evaluators about the existence of potential biases that may affect their criteria. According to previous work (Alesina et al., 2018), increasing awareness about implicit biases may help to reduce them.

Finally, the context of our study is very interesting, as the importance of teachers on children's development has been widely documented (Biasi, 2019). It could be very interesting to study whether this selection process actually brings the best teachers to the schools. Unfortunately, we did not get access to any data that would allows us to perform this research.

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