UNIVERSITY COLLEGE LONDON

DOCTORAL THESIS

Essays in Public Economics

ENRICO MIGLINO

A thesis submitted in fulfillment of the requirements for the degree of Doctor of Philosophy in Economics

I, Enrico Miglino, confirm that the work presented in my thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

Abstract

My thesis combines experimental and quasi-experimental evidence with structural models to study the causal effects and welfare consequences of institutional changes and public policies. The thesis focuses on incentives for policymakers, policies for the disadvantaged, and housing taxation.

In the first chapter, I analyze whether a tenure requirement for a parliamentary pension in Italy changed policymakers' behavior in confidence votes. Using a difference-in-discontinuities design and newly-collected data, I find that the pension incentive increased political stability but also party control over members of Parliament, ultimately reducing voters' welfare.

In the second chapter, I study the educational outcomes of a randomized policy in Chile that granted college admission to the top 15% students in disadvantaged high schools. The policy decreased students' pre-college effort, likely due to belief biases about grade ranking. The initial positive effect on college enrollment gradually decreases in the five years after high school. A dynamic model shows that eliminating the belief biases would improve the academic preparation of college entrants. Expanding admission to disadvantaged students can improve their college attainment, but preparation matters and responds to pre-college incentives.

In the third chapter, I study whether a basic pension increased the life expectancy of the elderly poor in Chile. Using administrative and survey data in a regression discontinuity design, I find that the pension was a cost-effective measure to reduce recipients' mortality thanks to an increase in food consumption and visits to health centers.

In the fourth chapter, I study the equilibrium effects of taxing property investors. Using a difference-in-differences estimator I find that a transfer-tax on UK investors reduced prices, but also transaction volumes and real-estate liquidity. After documenting strong search frictions in the market, I build a housing-search model and show that taxing investors increased welfare by offsetting the crowding-out externality they impose on owner-occupiers.

Impact Statement

My first chapter analyzes how pension incentives can change policymakers' behavior in confidence votes upon which the life of a government depends. An important policy implication is that parliamentary benefits that incentivize legislators to remain in power should be designed carefully, as they entail a trade-off between political stability and party polarization. They can reduce the policy uncertainty associated with unexpected government crises but they can also become a hidden tool for parties to enhance their control over legislators, inducing them to vote following party directives rather than voters' interest. Recently, the political-economy literature has shown that party discipline is an increasingly important driver of polarization in parliamentary institutions. This chapter shows that parliamentary benefits can be an effective tool for parties to tighten their control over the legislators and suggests that changes in parliamentary benefits can be one of the drivers of polarization in parliamentary voting.

My second chapter examines the impacts of preferential college admissions on a population of disadvantaged students in Chile. Young adults from better-off families are much more likely to attend college than those from worse-off families. One policy response to this intergenerational inequality is to provide college admission advantages to students from disadvantaged contexts. This chapter finds that preferential admissions can improve college enrollment of disadvantaged students but they also reduce their pre-college effort and academic preparation due to students' biased beliefs. These results suggest that policymakers wanting to expand admissions to disadvantaged students should reckon with the reality of the school environments such policies would encounter, and consider pairing the admission rules with tailored tutoring and information interventions.

My third chapter studies whether an income increase for the elderly poor in Chile can improve their health. The main policy implication of this chapter is that non-contributory pensions, intended to improve the living standards of the elderly poor, can also be a cost-effective measure to improve their life expectancy thanks to an increase in food consumption and visits to health centers. The results are informative for policymakers who aim to introduce income transfers that target subpopulations similar to the treatment group, which is composed primarily of elderly, low-income women in a middle-income country.

My fourth chapter studies the effects of a transfer tax that targets property investors, but not owner-occupiers in the UK. This tax increased home-ownership and decreased property prices without discouraging housing supply, satisfying the main objectives of the housing policy. However, my housing-market model offers an important caveat for policymakers: if the surcharge becomes excessively high, the effects can be reversed. Not enough buy-to-let investors will enter the market, the rental price will increase and too many households will search for a property to buy, inducing property prices to rise. Previous papers find that unconditional transfer taxes lead to deadweight losses. My study shows that a moderate transfer tax targeting investors can increase social welfare by offsetting the crowding-out externality that investors impose on owner-occupiers while competing for the same properties.

Acknowledgements

I would like to thank the *Fondazione Rodolfo Debenedetti* and the *Fondazione Openpolis* for kindly granting access to their data for Chapter 1.

I would like to thank the *Chilean National Institute of Pensions* and the *Ministry of Health* for the data provided for Chapter 3 and gratefully acknowledge the financial support of *CAF-development Bank of Latin America*.

The data for Chapter 4 have been provided by the Consumer Data Research Centre, an ESRC Data Investment, under project ID CDRC 604-02, ES/L011840/1; ES/L011891/1.

I would like to thank the American Economic Association for granting permission to publish Chapter 3. The full citation for this paper is: Miglino, Enrico; Navarrete, H. Nicolás; Navarrete, H. Gonzalo; Navarrete, H. Pablo 'Health Effects of Increasing Income for the Elderly: Evidence from a Chilean Pension Program' *American economic journal. Economic policy* 2023, Vol.15 (1), p.370-393.

Contents

Abstract 3

Impact Statement 4

Acknowledgements 6

1 Parliamentary pensions, Government Stability and Party Control 11

1.1 Introduction 11
1.2 Theoretical framework 15
1.3 Institutional background 21
1.3.1 The Parliament and the votes of confidence 21
1.3.2 The parliamentary pension reforms 23
1.4 Empirical strategy 25
1.4.1 Identification 25
1.4.2 Estimation 28
1.5 Data 30
1.6 Empirical results 33
1.6.1 The pension incentive increased government stability 33
1.6.2 Validity tests 35
1.6.3 The pension incentive increased party discipline 38
1.6.4 Back-of-the-envelope calculations on government stability 41
1.6.5 Effects over time 42
1.7 Conclusions 45

2 College Access When Preparedness Matters: New Evidence from Large Advantages in College Admissions 47

2.1 Introduction 47
2.2 Contributions to the Literature 53
2.3 Context, Randomization and Data 55
2.3.1 Context and PACE Policy 55
2.3.2 Randomization and Balancing Tests 57

2.3.3 Data Construction 58

2.3.4 Descriptive Analysis 60

2.4 Experimental Policy Evaluation 64

2.4.1 Findings 64

2.4.2 Discussion 67

2.5 Mechanisms 68

2.5.1 Students' Response to Incentives 68

2.5.2 Changes in Teachers' Behaviors and School Practices 74

2.5.3 Reduction in Perceived Returns to College 75

2.6 A Dynamic Model of Education Choices 75

2.6.1 From Experimental Evidence to a Model 75

2.6.2 Model 76

2.6.3 Identification 81

2.6.4 Estimation 83

2.7 Model Results 84

2.7.1 Estimation Results 84

2.7.2 Counterfactual Experiments: Improving the College Preparedness of College Entrants through School Interventions 86

2.8 Conclusions 90

3 Health Effects of Increasing Income for the Elderly: Evidence from a Chilean Pension Program **92**

3.1 Introduction 92

3.2 The basic pension 95

3.3 Data and empirical strategy 96

3.3.1 Pension and health datasets 96

3.3.2 Regression discontinuity design 97

3.3.3 Treatment effect on the treated 98

3.3.4 Descriptive statistics 100

3.4 RD validity 101

3.4.1 First stage 101

3.4.2 Continuity of applicants' density and pre-determined covariates . . 103

3.5 Results 105

3.5.1 The effect of receiving a pension on applicants' health 105

3.5.2 Discussion on the mortality effect 107

3.5.3 The heterogeneous effects of receiving a pension on applicants . . 109

3.5.4 Mechanisms behind the effects 111

3.6 Cost-benefit analysis . . . 115
3.7 Concluding remarks . . . 116

4 The Equilibrium Effects of Taxing Property Investors: A Welfare Analysis 118
4.1 Introduction . . . 118
4.2 Policy background and data . . . 123
4.2.1 Data . . . 126
4.3 Empirical strategy . . . 127
4.4 Reduced-form results . . . 132
4.4.1 Evidence of search frictions . . . 137
4.5 The model . . . 139
4.5.1 Timing and meeting probabilities . . . 141
4.5.2 Agent values . . . 142
4.5.3 Prices and mortgage negotiations . . . 145
4.5.4 A financial accelerator . . . 147
4.5.5 A recursive equilibrium . . . 148
4.5.6 Comparative statics: the short run . . . 150
4.5.7 Comparative statics: the long run . . . 152
4.6 Identification and calibration . . . 153
4.7 Quantitative results . . . 156
4.7.1 Model validation . . . 156
4.7.2 Welfare analysis . . . 158
4.8 Conclusions . . . 160

A Appendix: Parliamentary pensions, Government Stability and Party Control 162
A.1 Voting against party directives . . . 162
A.2 Additional Tables . . . 165
A.3 Additional Figures . . . 174

B Appendix: College Access When Preparedness Matters: New Evidence from Large Advantages in College Admissions 184
B.1 Fieldwork Information . . . 184
B.2 Additional Figures . . . 185
B.3 Robustness analysis . . . 189
B.4 Additional Tables . . . 190
B.5 Additional Details on Analysis of Mechanisms . . . 202
B.5.1 Construction of Teacher Variables . . . 202
B.5.2 Beliefs over Returns to College Degree . . . 203

B.6 Technical Appendix . . . 204
B.6.1 Structural model parameterizations . . . 204
B.6.2 Additional identification details . . . 206
B.6.3 Auxiliary Regressions and Moments . . . 207
B.6.4 Equilibrium of the Tournament Game in the Counterfactual . . . 210
B.7 PISA score re-scaling . . . 212

C Appendix: Health Effects of Increasing Income for the Elderly: Evidence from a Chilean Pension Program 214
C.1 The pension score . . . 214
C.2 Anticipating behavior . . . 217
C.3 Serial applicants . . . 218
C.4 Set of controls used in the robustness estimations . . . 219
C.5 Sensitivity and placebo checks on the direct health effects . . . 220
C.6 Spillover effects on applicants' household members . . . 222
C.6.1 Spillover results . . . 222
C.6.2 Robustness of fertility results . . . 227
C.6.3 Discussion on the spillover effect on fertility . . . 230
C.7 Additional tables . . . 234
C.8 Additional figures . . . 246

D Appendix: The Equilibrium Effects of Taxing Property Investors: A Welfare Analysis 259
D.1 Robustness checks . . . 259
D.2 Stocks and flows . . . 261
D.3 Derivations . . . 262
D.3.1 Prices and loans . . . 262
D.3.2 The financial accelerator . . . 265
D.3.3 Shares of buyers' types . . . 266
D.3.4 Wealthy households' and investors' mortgage choice . . . 267
D.3.5 Welfare analysis for households and investors . . . 268
D.4 Additional tables . . . 270
D.5 Additional figures . . . 280

Bibliography 286

Chapter 1

Parliamentary pensions, Government Stability and Party Control

1.1 Introduction

> If a man wants to join our ranks, if he wishes to accept my modest program, to transform himself and become a progressive, how could I reject him?
>
> Agostino Depretis (Italian prime minister, 1882)

The power to unseat a government is one of the checks and balances upon which parliamentary democracies are based. But the policy uncertainty associated with unexpected government crises discourages investment, hiring, lending, and ultimately hinders economic growth (Alesina et al., 1996; Baker, Bloom, and Davis, 2016; Bloom, 2014; Bloom, Bond, and Van Reenen, 2007; Bordo, Duca, and Koch, 2016). The critical decision to topple a government lies in the hands of few members of Parliament, who can either be *vocational* and vote to maximize voters' welfare, or *opportunistic* and vote to extract tangible rents for themselves (Persson and Tabellini, 2000). Whether legislators follow their vocation 'for politics' or 'off politics' in high-stakes voting decisions is crucial to understand the functioning of modern democracies (Weber, 1965).

This paper asks whether monetary incentives can change parliamentary voting behavior, increasing government stability but also party control over the members of parliament. To address this question, I study an unusual change in parliamentary benefits occurred in Italy in 2008 (legislature XVI): the introduction of a minimum parliamentary tenure of 4.5 years required to obtain a parliamentary pension. For this analysis, I have digitized the stenographic records of all 427 confidence votes occurred in the Italian Parliament between 2001 and 2022 (legislatures XIV-XVIII), which contain the list of members of parliament (MPs) who voted in favor, against or abstained. I merged this data with personal data on all MPs: demographics, education level, previous job, party affiliation, start and end date of each parliamentary term which can be used to compute the parliamentary tenure. I have also digitized the first tax return submitted by each Deputy and Senator in the parliamentary archives to have a measure of their private income in the year prior to entering Parliament.

To study the effects of the policy, I cannot employ a regression discontinuity design because there is another monetary incentive, the parliamentary severance pay, that increases at the threshold of 4.5 years. I do not use a difference-in-differences design because newly-elected MPs may have a different trend in their voting attitude with respect to MPs with a much longer experience in Parliament. Therefore, I combine two sources of variation, before/after the beginning of Legislature XVI and just below/above 4.5 years parliamentary tenure, and implement a 'difference-in-discontinuities' design as in Grembi, Nannicini, and Troiano (2016). I take the difference between the pre-treatment and the post-treatment discontinuity at the tenure threshold in order to net out the effect of the severance pay increase. This strategy requires that the effect of confounding factors at the threshold does not vary over time.

I estimate that the introduction of the tenure requirement for a parliamentary pension increases the probability of expressing a vote of confidence in the government by 3 percentage points, within a tenure bandwidth of one year in each side of the threshold. To confirm the validity of the empirical strategy, I perform a series of diagnostic tests. First, the effect is not significantly different from zero in any of the Houses in the two legislatures (XIV and XV) before the introduction of the tenure requirement, but it becomes significantly positive since the first legislature in which the tenure requirement is in place (XVI). Secondly, the diff-in-disc coefficient is significantly positive and remarkably stable when using different bandwidths around the tenure threshold, from two months up to three years on each side. Thirdly, the Frandsen (2017) test for discrete running variables cannot reject the null hypothesis of absence of manipulation in the distribution of the tenure of MPs in confidence votes, and diff-in-disc estimates on pre-determined variables are not significant. Finally, to assess the possibility that this result arises from random chance rather than from a causal relationship, I perform a set of diff-in-disc estimations at placebo thresholds below and above 4 years and 6 months. All the placebo estimates are lower than the true-threshold coefficient for the confidence vote.

In a heterogeneity analysis, I show that the effect of the tenure requirement is stronger in legislatures in which MPs' private income and the probability of being re-elected are lower. Surprisingly, the policy significantly increases the votes of confidence by MPs elected in parties that support the government (*majority MPs*), but it decreases votes of confidence by MPs elected in opposition parties (*opposition MPs*) even if the latter effect is not significant in all specifications. The tenure requirement significantly reduces the probability that an MP votes against party directives by 3.4 percentage points. These empirical results can be rationalized in a simple political-agency model in which policy-makers are to some extent opportunistic.

First of all, if they were vocational and voted purely in their voters' interest, their voting behavior should not change when their private economic incentives change. Secondly, the model shows that the minimum tenure requirement for a parliamentary pension has two effects on the voting behavior of newly-elected MPs, which affect majority and opposition MPs differently. A 'pivotal' effect for which both majority and opposition newly-elected MPs have an incentive to vote the confidence in case they are the pivotal voter so as to increase the probability of survival for the current government, complete their first legislature and secure a parliamentary pension. A 'party-discipline' effect for which both majority and opposition newly-elected MPs have an incentive to vote following the party directives so as to have a higher probability of being re-elected (Table A.1) and reach the tenure requirement in their second term, in case the government loses the confidence vote and there are early elections. The party-discipline effect increases MPs' loyalty towards their own party, even if this is detrimental to voters' welfare.

Both effects go in the same direction for majority MPs, but they go in opposite directions for opposition MPs because the latter have a party directive to vote against the government. Then, the model predicts that the minimum tenure requirement will have an unambiguously positive impact on the probability to vote confidence for majority MPs, but it will have an ambiguous effect on opposition MPs, depending on which (if any) of the pivotal and party-discipline effect dominates. These predictions are confirmed by the heterogeneity analysis: the estimated effect is positive and highly significant for majority MPs, whereas it is insignificantly negative for opposition MPs (i.e. the party-discipline effect seems to dominate). The model shows that if the party-discipline effect dominates, the minimum tenure requirement is unambiguously distortionary: it induces majority MPs to vote in favor of the government and opposition MPs to vote against the government when it would be in the voters' interest to do otherwise.

Back-of-the-envelope calculations show that one of the eight governments (Berlusconi IV) in legislatures XVI-XVIII would have resigned earlier, had the tenure requirement been absent. Interestingly, Italy experienced a severe sovereign debt crisis in the final months of Berlusconi's government, which the pension incentive helped to prolong.

My paper contributes to several branches of the political economy literature. First, it sheds light on the relationship between institutions and political stability. Theoretical models of government instability feature legislative bargaining between parliamentary parties and include shocks to economic or electoral prospects that can induce renegotiations and no-confidence votes (Diermeier and Merlo, 2000). Empirical studies usually rely on strong identifying assumptions. Notable exceptions are the papers by Gagliarducci and Paserman (2012), Acconcia and Ronza (2022) and Carozzi, Cipullo, and Repetto (2022). They employ RD designs to show that policymakers' gender and political fragmentation can significantly affect local government stability, whereas my analysis shows that monetary incentives based on parliamentary tenure can significantly affect government stability at national level.

Second, my model illustrates the presence of a trade-off between government stability and distortionary party discipline, which speaks to the literature on political polarization. Party discipline has been a growing driver of polarization in US congressional voting since the 1970s (Canen, Kendall, and Trebbi, 2020a). In 2018 party discipline accounted for 65% of the polarization in roll call voting according to estimates by Canen, Kendall, and Trebbi (2020b). In the last decades political polarization have increased dramatically in the EU (Müller and Schnabl, 2021) and in the US (Canen, Kendall, and Trebbi, 2020a), hindering bipartisan cooperation (Hetherington, 2015), increasing the risk of political gridlocks in moments of crisis (Mian, Sufi, and Trebbi, 2014), and even inducing discriminatory behaviour toward opposing party supporters (Iyengar and Westwood, 2015). Partisan differences have widened in congressional voting behavior (McCarty, Poole, and Rosenthal, 2008), congressional speeches (Gentzkow, Shapiro, and Taddy, 2019), campaign finance data (Bonica, 2014), and candidate ideology based on survey responses (Moskowitz, Rogowski, and James M. Snyder, 2022). My paper shows that parliamentary benefits can be an effective tool for parties to tighten their control over the legislators and suggests that changes in parliamentary benefits can be one of the drivers of polarization in parliamentary voting. The design of monetary incentives for legislators must be designed carefully to account for consequences in terms of political stability and polarization.

Third, it contributes to the literature that tests predictions of political-agency models (Besley, 2004; Besley and Case, 1995; Preece et al., 2004). Starting from the seminal models by Barro (1973) and Ferejohn (1986), this literature is now quite extensive and hinges on the assumptions that voters are 'principals' with imperfect information about the state of the world and that policymakers are opportunistic 'agents' working for them (see Besley (2007) for a review). Recently, the focus has been on how relative salaries in the political and private sectors affect politicians' career decisions and their quality (Diermeier, Keane, and Merlo, 2005). Theoretical results are inconclusive: if Caselli and Morelli (2004) find that higher salaries improve the quality of politicians assuming they have uni-dimensional ability, more complex models lead to ambiguous predictions due to free-riding effects (Messner and Polborn, 2004), the simultaneous presence of entry and retention effects (Mattozzi and Merlo, 2008) the presence of different types ('skilled' and 'achievers') (Diermeier, Keane, and Merlo, 2005; Keane and Merlo, 2010). Most empirical analysis find positive effects of higher pay on politicians' quality, proxied by the level of education (Gagliarducci and Nannicini, 2013; Kotakorpi and Poutvaara, 2011), with the exception of Fisman et al. (2015), who estimate that increasing salaries decreases MEPs' college quality.[1]

Rather than studying changes in relative wages, I analyze the effect of a perquisite attached to holding a parliamentary seat: the right for a parliamentary pension. In this sense, the paper is similar in spirit to the cross-sectional analyses by Hall and Van Houweling (1995) and Groseclose and Krehbiel (1994).[2] They provide suggestive evidence of strategic retirement of US congressmen in 1992, the last year in which House members sworn in before 1980 could keep their campaign war chests for personal use once retired ('the golden parachute' provision in the Federal Election Campaign Act).[3] If these papers show that pension incentives can have substantial effects on politicians' retirement decisions, my paper analyzes how pension incentives can change MPs' behavior in high-stakes votes upon which the life of a government depends.

The paper is organized as follows. Section 1.2 describes the political-agency model that guides the empirical analysis. Section 1.3 presents the institutional background of the pension reform. Section 1.4 explains the empirical strategy and Section 1.5 illustrates the data. Section 1.6 presents the results, tests the validity of the empirical strategy and reports back-of-the-envelope calculations on the impact on government stability. Section 1.7 concludes.

1.2 Theoretical framework

This section makes precise how the minimum tenure requirement for a parliamentary pension should affect MPs' voting behavior in a simple model of political agency. This

[1]Other papers have also focused on on how monetary incentives affect politicians' productivity while in office. Fisman et al. (2015) find that higher pay has no significant effect on MEPs' effort or legislation output, whereas Finan and Ferraz (2009) show that higher wages increase legislative productivity, resulting in more legislative bills and public goods provision. Gagliarducci and Nannicini (2013) show that better-paid mayors size down the government machinery by improving efficiency.

[2]Preece et al. (2004) study another perquisite enjoyed by US congressmen, the possibility to write checks on nonexistent balances, which ultimately led to the House Bank scandal in 1992. They show that more entrenched congressmen are more likely to be involved in the excessive consumption of this perquisite, confirming a well-known prediction in agency theory.

[3]Their analysis is affected by two potential confounders: the House Bank scandal (congressmen involved in writing checks on nonexistent balances) and redistricting. Using a maximum likelihood model and data on 1992 retirement decisions, Groseclose and Krehbiel (1994) claims that the golden parachute provision caused nearly twice as many retirements as redistricting and nearly four times as many retirements as the House Bank scandal.

principal-agent model yields several predictions that will be tested in the subsequent empirical analysis.

Suppose that every politician is elected in period $t = 1$, retires at the end of period T and dies at the end of period $T + 1$. For simplicity, assume the parliamentary career of an MP ends after two parliamentary terms.[4] Politicians elected in party i choose one binary action $e_{it} \in \{0, 1\}$ in each term, namely whether to vote confidence in the government or not. Assume that if a government loses the vote of confidence, the legislature ends before its natural term and there are early elections.[5]

Politicians belong to majority parties M in favor of the government or to opposition parties m against the government: $i \in \{M, m\}$. If they vote following the directives of their party ($e_{Mt} = 1,\ e_{mt} = 0$), they are re-elected with exogenous probabilities π_i. If they vote against the directives of their party ($e_{Mt} = 0,\ e_{mt} = 1$), they are re-elected with lower probability $\underline{\pi_i} < \pi_i$. This assumption is based on the empirical evidence that first-term MPs who vote against their party directives are 10-25% less likely to be re-elected (see Table A.1 in Appendix Section A.1). Without loss of generality assume that $\underline{\pi_i} = 0$. If MPs are not re-elected, they abandon the political career. Since the confidence vote outcome of each MP is disclosed only after all MPs have voted, I assume that MPs do not observe the decisions of the other MPs when they vote.

All politicians receive a payoff $\mathcal{I}$ from holding a parliamentary seat (e.g. parliamentary wage, ego rents). If they leave the political career, in each period till retirement they earn and consume a time-invariant private wage w, which can be heterogeneous across politicians. When they retire, all MPs obtain a private pension proportional to their wage γw. Assume politicians receive a parliamentary pension ρ only if they complete the first parliamentary term (i.e. the government wins the vote of confidence) or if they are elected for two parliamentary terms (even if incomplete). When in Parliament, politicians observe the state of the world $s_t \in \{0, 1\}$, which is an indicator on the net welfare gain that the government is producing. If $s_t = 0$, it would be in the voters' interest for the Parliament to exercise its control power over the executive and topple the government. If $s_t = 1$, it would be in the voters' interest for the Parliament to guarantee government stability and vote confidence in the government. Voters receive a payoff Δ only if the politician they have elected votes according to the state of the world: i.e. if $e_{it} = s_{it}$. If $e_{it} \neq s_{it}$, voters receive 0. Assume voters do not observe the states of the world or the payoffs of period t until period $t + 2$. Politicians care about their voters' welfare up to a certain extent,

[4] 74% of the MPs in the sample remained in Parliament for no longer than two parliamentary terms.

[5] One could complicate the model and assume that losing the vote of confidence is associated with a positive (less than one) probability that the legislature ends prematurely. Under this assumption all the effects would be dampened, but the model would yield the same qualitative predictions.

measured by a parameter α. I abstract away from considerations on the relative wage and assume that politicians always prefer the political career to the private sector career: $\alpha\Delta + \mathcal{I} \geq w$.

Politicians have a time-additive intertemporal utility function, discount future by a factor $\beta < 1$ and the within-period utility function $u(\cdot)$ is increasing and concave. Note that, if re-elected, the second-period utility of an MP is independent of her majority-opposition status and politicians always act in the voters' interest since it is their last term: second-period utility for a re-elected MP is always $u(\mathcal{I} + \alpha\Delta)$.

Now suppose there is no minimum tenure requirement and the MP will get a pension at $T + 1$ independently of the time she spends in Parliament (as long as she is elected once). The utility function for an MP elected in period $t = 1$ in a majority party with a private wage w is:

$$\begin{aligned} U_1(e_1|M, w) &= u(\mathcal{I} + \alpha[s_1 e_1 + (1 - s_1)(1 - e_1)]\Delta) + \\ &\beta\frac{1 - \beta^{T-1}}{1 - \beta}u(w) + \beta e_1 \pi_M \left[u(\mathcal{I} + \alpha\Delta) - u(w)\right] + \beta^T u(\rho + \gamma w) \end{aligned} \tag{1.1}$$

If the government is doing well ($s_1 = 1$), the newly-elected majority MP always votes confidence in the government as:

$$u(\mathcal{I} + \alpha\Delta) + \beta\pi_M[u(\mathcal{I} + \alpha\Delta) - u(w)] \geq u(\mathcal{I}) \tag{1.2}$$

If the government is not doing well ($s_1 = 0$), the newly-elected majority MP would vote the confidence and go against voters' interest if

$$u(\mathcal{I}) + \beta\pi_M[u(\mathcal{I} + \alpha\Delta) - u(w)] \geq u(\mathcal{I} + \alpha\Delta) \tag{1.3}$$

Intuitively, the MP would follow the party directives and vote against the voters' interest only if her net utility of being re-elected is higher than the utility she gets from benefiting the voters in the first term.

Suppose now that a minimum tenure requirement is imposed. The utility function for an MP elected in period $t = 1$ in a majority party with a private wage w is:

$$\begin{aligned} &U_1(e_1|M, w) = u(\mathcal{I} + \alpha[s_1 e_1 + (1 - s_1)(1 - e_1)]\Delta) + \beta e_1 \pi_M[u(\mathcal{I} + \alpha\Delta) - u(w)] + \\ &\beta\frac{1 - \beta^{T-1}}{1 - \beta}u(w) + \beta^T\{Pr(e_1) + [1 - Pr(e_1)]e_1\pi_M\}[u(\rho + \gamma w) - u(\gamma w)] + \beta^T u(\gamma w) \end{aligned} \tag{1.4}$$

where $Pr(e_1)$ is the probability the government wins the confidence vote and it is increasing in e_1. Again, if the government is doing well ($s_1 = 1$), the newly-elected majority MP always votes confidence in the government as:

$$\begin{aligned}&u(\mathcal{I}+\alpha\Delta)+\beta\pi_M[u(\mathcal{I}+\alpha\Delta)-u(w)]+\beta^T\{[Pr(e_1=1)-Pr(e_1=0)]+\\&[1-Pr(e_1=1)]\pi_M\}[u(\rho+\gamma w)-u(\gamma w)]\geq u(\mathcal{I})\end{aligned} \tag{1.5}$$

If the government is not doing well ($s_1 = 0$), the newly-elected majority MP would vote the confidence and go against the voters' interest if

$$\begin{aligned}&u(\mathcal{I})+\beta\pi_M[u(\mathcal{I}+\alpha\Delta)-u(w)]+\beta^T\{[Pr(e_1=1)-Pr(e_1=0)]+\\&[1-Pr(e_1=1)]\pi_M\}[u(\rho+\gamma w)-u(\gamma w)]\geq u(\mathcal{I}+\alpha\Delta)\end{aligned} \tag{1.6}$$

Comparing inequalities (1.3) and (1.6), the minimum tenure requirement for the parliamentary pension increases the utility of newly-elected majority MPs for voting confidence in the government against voters' interest by $\beta^T\{[Pr(e_1=1)-Pr(e_1=0)]+[1-Pr(e_1=1)]\pi_M\}[u(\rho+\gamma w)-u(\gamma w)]$. This is composed by two effects. The first term in the curly brackets is a *pivotal effect*: the incentive to vote confidence in case they are the pivotal voter so as to increase the probability of survival for the current government, complete their first legislature and secure a parliamentary pension. The second term in the curly brackets is a *party-discipline effect*: the incentive to vote following the party directives so as to have a higher probability of being re-elected and reach the tenure requirement in the second legislature, in case the government loses the confidence vote and there are early elections. Both the pivotal and the party-discipline effect decrease when the government has a larger majority margin, as each voter is less likely to be pivotal and the probability of a government fall is lower. The sum of the effects increases in age (lower T) and decreases in private wage w because of diminishing marginal utility.

Note that if they are uninterested in voters' welfare ($\alpha = 0$) MPs always vote confidence following the party directives, whereas if they are sufficiently interested in voters' welfare ($\alpha \to \infty$) MPs always vote in the voters' interests. In these extreme cases (fully 'opportunistic' or fully 'vocational' politicians), the minimum pension requirement should have no effect on their voting behavior.

Prediction 1: *The minimum tenure requirement for the parliamentary pension increases votes of confidence by newly-elected majority MPs if they are sufficiently, but not fully opportunistic. This effect increases in age and in the probability of being re-elected, whereas it decreases in private wage and in the government majority margin.*

Note that the minimum tenure requirement is a distortionary incentive for majority MPs because it induces them to vote in favor of the government, when it would be in the voters' interest to vote against.

In absence of a minimum tenure requirement, the utility function for an MP elected in period $t = 1$ in an opposition party with a private wage w is:

$$U_1(e_1|m, w) = u(\mathcal{I} + \alpha[s_1 e_1 + (1 - s_1)(1 - e_1)]\Delta)+ \\ \beta(1 - e_1)\pi_m[u(\mathcal{I} + \alpha\Delta) - u(w)] + \beta\frac{1 - \beta^{T-1}}{1 - \beta}u(w) + \beta^T u(\rho + \gamma w) \tag{1.7}$$

If the government is not doing well ($s_1 = 0$), the newly-elected opposition MP never votes confidence in the government as:

$$u(\mathcal{I} + \alpha\Delta) + \beta\pi_m[u(\alpha\Delta + \mathcal{I}) - u(w)] \geq u(\mathcal{I}) \tag{1.8}$$

If the government is doing well ($s_1 = 1$), the newly-elected opposition MP would vote the confidence (and satisfies voters' interest) if

$$u(\mathcal{I} + \alpha\Delta) \geq u(\mathcal{I}) + \beta\pi_m[u(\alpha\Delta + \mathcal{I}) - u(w)] \tag{1.9}$$

Intuitively, the MP would follow the party directives and vote against the voters' interest only if her net utility of being re-elected is higher than the net utility of benefiting the voters in the first term.

Suppose now that a minimum tenure requirement is imposed. The utility function for an MP elected in period $t = 1$ in an opposition party with a private wage w is:

$$U_1(e_1|m, w) = u(\mathcal{I} + \alpha[s_1 e_1 + (1 - s_1)(1 - e_1)]\Delta)+ \\ \beta(1 - e_1)\pi_m[u(\mathcal{I} + \alpha\Delta) - u(w)] + \beta\frac{1 - \beta^{T-1}}{1 - \beta}u(w)+ \\ \beta^T\{Pr(e_1) + [1 - Pr(e_1)](1 - e_1)\pi_m\}[u(\rho + \gamma w) - u(\gamma w)] + \beta^T u(\gamma w) \tag{1.10}$$

The change in behavior for newly-elected opposition MP is ambiguous as it depends on the sign of $\{[Pr(e_1 = 1) - Pr(e_1 = 0)] - [1 - Pr(e_1 = 0)]\pi_M\}$. Now the pivotal and party-discipline effects have opposite signs. On one hand, opposition MPs would like to vote the confidence because if the government wins they would obtain the pension. On the other hand, opposition MPs are afraid to vote the confidence because if the government loses, they would be less likely to be re-elected and to secure the pension in a second term.

With the minimum tenure requirement, the newly-elected opposition MP might vote the confidence even if the government is not doing well ($s_1 = 0$). This occurs when

$$u(\mathcal{I}) + \beta^T\{[Pr(e_1 = 1) - Pr(e_1 = 0)] - [1 - Pr(e_1 = 0)]\pi_m\}[u(\rho + \gamma w) - u(\gamma w)]$$
$$\geq u(\mathcal{I} + \alpha\Delta) + \beta\pi_m[u(\alpha\Delta + \mathcal{I}) - u(w)] \tag{1.11}$$

A necessary condition for this to occur is that the pivotal effect dominates the party-discipline effect.

If the government is doing well ($s_1 = 1$), the newly-elected opposition MP would vote the confidence in the voters' interest if

$$\begin{aligned} &u(\mathcal{I} + \alpha\Delta)+ \\ &\beta^T\{[Pr(e_1 = 1) - Pr(e_1 = 0)] - [1 - Pr(e_1 = 0)]\pi_m\}[u(\rho + \gamma w) - u(\gamma w)] \\ &\geq u(\mathcal{I}) + \beta\pi_m[u(\alpha\Delta + \mathcal{I}) - u(w)] \end{aligned} \tag{1.12}$$

Comparing inequalities (1.9) and (1.12), the minimum tenure requirement for the parliamentary pension has an ambiguous effect on the utility of newly-elected opposition MPs for voting confidence. If the incentive to increase the chances of a government victory to immediately obtain the pension right (*pivotal effect*) is lower than the fear of losing the possibility of being re-elected and obtain the pension later in case of government defeat (*party-discipline effect*) ($\{Pr(e_1 = 1) - Pr(e_1 = 0) < [1 - Pr(e_1 = 0)]\pi_M\}$), then the minimum tenure requirement incentives newly-elected opposition MPs to vote *against* the confidence, even if this is against the voters' interest. If the government has a larger majority margin, both the positive pivotal effect and the negative party-discipline effect are weaker.

Prediction 2: *The minimum tenure requirement for the parliamentary pension has an ambiguous effect on the votes of confidence by newly-elected opposition MPs. They are less likely to vote confidence if they are more likely to be re-elected. A larger government majority margin and a larger private wage weaken both the pivotal and the party-discipline effects, whereas age magnifies these effects.*

Note that if the party-discipline effect dominates the pivotal effect (i.e. the tenure requirement reduces the number of opposition MPs voting confidence in the government), the tenure requirement incentive is distortionary. It incentivizes opposition MPs to vote against the government, even though it would be in the voters' interest to vote in favor. The expected social surplus produced by majority MPs is $\Delta\{Pr(s_1 = 1) + Pr(s_1 = 0)[(1 - Pr(e_1 = 1)) + Pr(e_1 = 1)\beta\gamma_M]\}$ which decreases in $Pr(e_1 = 1)$. Since the tenure requirement increases $Pr(e_1 = 1)$ for majority MPs, it decreases the expected social surplus produced by majority MPs. The expected social surplus produced by opposition MPs is $\Delta\{Pr(s_1 = 0) + Pr(s_1 = 1)[Pr(e_1 = 1) + (1 - Pr(e_1 = 1))\beta\gamma_M]\}$ which increases in $Pr(e_1 = 1)$. Since the tenure requirement decreases $Pr(e_1 = 1)$ for opposition MPs when the party-discipline effect dominates, it also decreases the expected social surplus produced by opposition MPs. According to the definition by Besley (2007), the minimum tenure requirement is a political failure as it produces a negative ex-ante social surplus, as long as the party-discipline effect dominates. The model shows an examples in which political survival considerations can be a source of real inefficiencies, as summarized by Besley and Coate (1998).

1.3 Institutional background

1.3.1 The Parliament and the votes of confidence

Italy is a parliamentary democracy with a bicameral structure. Until legislature XVIII, the Chamber of Deputies was composed by 630 elected Deputies and the Senate was composed 315 elected Senators with the same legislative power. Following a constitutional referendum, in 2019 the Parliament approved a reform which reduced the number of Deputies to 400 Deputies and the number of Senators to 200 starting from the following legislature (XIX) (Dipartimento per le Riforme Istituzionali, 2022). This reform reduced the probability that an MP will be re-elected and this might affect the impact of the minimum tenure requirement for a pension in legislature XVIII.

All MPs are elected simultaneously during general political elections, except for Senators with a life tenure. These are former presidents of the Republic or citizens directly appointed by the president of the Republic ‘for outstanding patriotic merits’ (at most five). Regardless of the party or electoral district, MPs have the legal duty to represent the interests of all Italian citizens. The parties form parliamentary groups whose heads determine the calendar of Parliament and the issues to be discussed during each parliamentary session. Yet, the parties have no formal control over the voting behaviour of the MPs while they are in Parliament (Merlo et al., 2010).

A majority in each House is required to pass a bill before it becomes a new law. Beyond the legislative power, the Parliament exercises control over the executive power of the Council of Ministers, primarily by means of a vote of confidence. There are several instances in which a vote of confidence can occur. First, before being officially in power, every Government must obtain the majority in each House through a vote of confidence.

Second, each House can cast a vote of no confidence at any moment during a parliamentary term (legislature) as long as the no-confidence motion is signed by at least one tenth of the House members (Senato della Repubblica, 2022a). Third, the Government may call a vote of confidence in order to compel the House to reconfirm its support in relation to a specific text being considered by the House and speed up the legislative procedure.

If the government loses a vote of confidence in any of the two Houses, the President of the Council of Ministers has to resign and the government falls (Camera dei Deputati, 2022). No-confidence votes are not 'constructive' as in Spain and Germany: MPs do not propose an alternative candidate President of the Council who has to have a parliamentary majority and takes charge if the incumbent loses the vote. In this sense, the Italian confidence vote is similar to the confidence vote in the majority of parliamentary and semi-presidential democracies (Rubabshi-Shitrit and Hasson, 2022).[6]

The constitutionally mandated duration of a legislature is five years. The President of the Republic can dissolve the Parliament before the natural end of a legislature and call for early elections if the Parliament is unable to form a stable majority in each House in support of a government. This occurs generally after a loss in a vote of confidence (as for Romano Prodi's second government in January 2008) or after a win in a vote of confidence by an excessively narrow margin (as for Giuseppe Conte's second government in August 2019). Article 88 of the Italian constitution states that the President of the Republic cannot dissolve the Parliament "during the final six months of the presidential term, unless said period coincides in full or in part with the final six months of Parliament.". There were three presidents in the period of analysis: Ciampi (1999-2006), Napolitano (2006-2013; 2013-2015) and Mattarella (2015-2022; 2022-). The last semester of the first two presidents coincided with the final six months of Legislature XIV and XVI, respectively, so these presidents were allowed to dissolve the Parliament at any moment during their seven-year terms. Mattarella's last semester occurred between August 2021 and February 2022, which corresponds to the period between the fifth and the eleventh month of the third year of Legislature XVIII. The empirical results are robust to excluding Legislature XVIII from the analysis or to reducing the bandwidth to six months around the threshold: from the fourth to the fifth year of Legislature XVIII.

Early elections have been relatively frequent in Italy. There have been eighteen legislatures between 1948 and 2022 and half of them ended before the natural term. The high

[6]The constructive vote of no-confidence is currently present in seven countries: Germany, Spain, Hungary, Poland, Slovenia, Belgium and Israel. The vast majority adopts a regular vote of no-confidence as in Italy: Australia, Austria, Bulgaria, Canada, Czech Republic, Croatia, Denmark, Estonia, Finland, Iceland, India, Ireland, Latvia, Lithuania, Netherlands, New Zealand, Norway, Portugal, Romania, Slovakia, Sweden and the United Kingdom (Rubabshi-Shitrit and Hasson, 2022).

degree of political instability in Italy resulted in high executive turnover: there have been 69 governments in the last 75 years, with an average duration of 1.1 years.

As we can see in Table 1.1, there were 223 votes of confidence/no confidence in the Chamber of Deputies and 204 votes of confidence/no confidence in the Senate from 2001 to 2022 in the legislatures XIV-XVIII. Votes of confidence are quite frequent: the average distance between two votes of confidence is 38 days for the Senate and 35 for the Chamber, the maximum distance is 364 days for the Senate and 306 for the Chamber. Motions of no-confidence votes are not frequent: there were only five in the Senate and only one in the Chamber in the analyzed period. Each legislature had between one and three different governments. Among the five legislatures under study, legislature XV and legislature XVIII ended before their natural end. The last vote of legislature XV was one year and nine months after its beginning, whereas the last vote of legislature XVIII was four years and four months after its beginning.

TABLE 1.1: Number of votes of confidence in legislatures XIV-XVIII

Government	Legislature	Chamber votes	Senate votes	Date first vote	Date last vote
Berlusconi II	14	19	10	21/06/2001	28/12/2004
Berlusconi III	14	12	9	28/04/2005	09/02/2006
Prodi II	15	17	16	23/05/2006	24/01/2008
Berlusconi IV	16	32	22	15/05/2008	14/10/2011
Monti I	16	34	19	18/11/2011	21/12/2012
Letta I	17	10	6	30/04/2013	04/02/2014
Renzi I	17	31	43	25/02/2014	07/12/2016
Gentiloni I	17	14	20	14/12/2016	23/12/2017
Conte I	18	10	6	06/06/2018	05/08/2019
Conte II	18	21	23	10/09/2019	19/01/2021
Draghi I	18	23	30	18/02/2021	21/07/2022

Notes: Number of votes of confidence and motions of no confidence for each government in legislatures XIV-XVIII from 2001 to 2022. Each government is identified by the President of the Council and its ordinal number.

1.3.2 The parliamentary pension reforms

In the entire period of analysis, the parliamentary pension was cumulative with respect to any non-parliamentary pension the MP received from another job (Rizzo, 2018).

Before 1997, MPs received a parliamentary pension at the age of 60, after paying pension contributions for at least a five-year term in Parliament. The pension scheme was quite favorable: the pension amount corresponded to a share of the final salary (up to 85.5%), only in small part financed by the pension contributions. An MP that did not complete a five-year term could also obtain a pension by simply paying the missing monthly pension contributions. The facility to qualify for a parliamentary pension led to distortionary practices such as 'rotating resignations', which allowed multiple MPs to obtain a pension even after spending only few days in Parliament.

Since 1997, only MPs with more than 2.5 years of parliamentary tenure were allowed to pay the missing monthly pension contributions to obtain a pension (De Santis, 2020). The minimum pension age was raised to 65 and decreased by one year for each year of parliamentary tenure after the first five years, down to a minimum of 60 years of age. The pension contribution was 8.6% of the gross parliamentary wage and the pension varied between 25% and 80% of the parliamentary monthly wage, increasing in tenure (from 5 to 15 years of tenure). In 2006 the gross parliamentary wage was €$12,434$ and therefore, the minimum pension was €$3,108$ with only 5 years of paid contributions.

In 2007, the right to pay the missing pension contributions to obtain a parliamentary pension was suppressed. The MPs elected for the first time since 2008 (from legislature XVI onwards) would receive a parliamentary pension only if they had more than 4.5 years of parliamentary tenure over their lifetime (Camera dei Deputati, 2007; Senato della Repubblica, 2022b).[7] The suppression of the pension redemption scheme for MPs followed a surge of an anti-politics sentiment in the Italian public opinion targeting MPs' privileges and has attracted considerable attention from the media (Fusani, 2007; Sesto, 2021). If this reform was aimed at cutting the high costs of the Italian Parliament, several commentators warned about its distortionary incentive of voting in favor of a government to avoid the end of a parliamentary term before 4.5 years of tenure (De Santis, 2020; Osservatorio sui Conti Pubblici Italiani, 2021).

In addition, the monthly pension amount was changed, ranging between 20% and 60% of the final gross parliamentary monthly wage, increasing in tenure (from 5 to 15 years of tenure) (Camera dei Deputati, 2007). The gross parliamentary monthly wage in legislature XVI was €$10,435$ and the pension contribution was 8.8% of the gross parliamentary wage. If the parliamentary term ended just before the MP could reach 4.5 years of tenure, the MP would lose a monthly pension of €$2,087$ from age 65 to the rest of his life as well as €$49,587$ in already paid pension contributions (Camera dei Deputati, 2013). The minimum parliamentary pension was 57% higher than the average gross monthly pension in Italy in 2011 (ISTAT, 2013).

[7] Officially the minimum tenure is 5 years (i.e. one complete parliamentary term), but the eligibility threshold is actually 4 years, 6 months and 1 day as the tenure calculation approximates to the next semester (Osservatorio sui Conti Pubblici Italiani, 2021).

For MPs elected after January 1, 2012, the pension scheme changed from a final-salary pension scheme to a fully contributory pension scheme, in which the pension amount depends on the pension contributions. This resulted in a substantial pension cut from legislature XVI to legislatures XVII and XVIII. An MP reaching the minimum tenure threshold in legislature XVIII will earn only €970 when reaching 65 years of age (Daconto, 2022).[8]

The parliamentary pension is not the only parliamentary benefit that changes at 4.5 years of tenure. At the end of a parliamentary term an MP receives a severance pay that increases by 80% of the monthly wage of the MP at 4.5 years of tenure (Camera dei Deputati, 2001, 2022). The severance pay has remain fixed at 80% of the monthly wage of the MP, multiplied by the number of years of parliamentary tenure, over the entire period of analysis.

1.4 Empirical strategy

1.4.1 Identification

I closely follow the identification strategy by Grembi, Nannicini, and Troiano (2016). Given the institutional background described above, there are three different treatments: the severance pay that increases at the threshold, the pension eligibility that changes at the threshold only after 2008 and the pension amount that changes in 2008 but does not vary between the two sides of the threshold. Define: D_{it} as the first treatment for MP i at time t, equal to one if the severance pay is lower and zero otherwise; R_{it} as the second treatment, equal to one if the MP does not obtain the parliamentary pension and zero otherwise; A_{it} as the third treatment, equal to 1 if the monthly pension amount is 20% of the final gross parliamentary monthly wage and 0 if it is 25%. The additional confounding treatment A_{it} differentiates this setting from the one in Grembi, Nannicini, and Troiano (2016).

MPs with parliamentary tenure Z_{it} at or below the threshold Z_c (4.5 years) have a lower severance pay, while the pension eligibility requirement is introduced at time t_0 (year 2008) for MPs with tenure at or below the same threshold. Finally, the monthly pension amount decreased from 25% to 20% of the final gross parliamentary monthly wage at time t_0 on both sides of the threshold. The assignment mechanism for the three treatments can be formalized as below:

[8]MPs who were already in parliament before 2012, a pro-rata system is applied: a final-salary pension scheme up to January 1, 2012 and a contributory pension scheme thereafter (Senato della Repubblica, 2022b).

$$D_{it} = \begin{cases} 1 & \text{if } Z_{it} \leq Z_c \\ 0 & \text{otherwise,} \end{cases}$$

$$R_{it} = \begin{cases} 1 & \text{if } Z_{it} \leq Z_c \text{ and } t \geq t_0 \\ 0 & \text{otherwise,} \end{cases}$$

$$A_{it} = \begin{cases} 1 & \text{if } t \geq t_0 \\ 0 & \text{otherwise.} \end{cases}$$

Define $Y_{it}(d, r, a)$ as the potential policy outcomes if $D_{it} = d$, $R_{it} = r$, and $A_{it} = a$ with $d, r, a \in \{0, 1\}$. The observed outcome is equal to

$$\begin{aligned} Y_{it} = & D_{it}R_{it}A_{it}Y_{it}(1,1,1) + D_{it}R_{it}(1 - A_{it})Y_{it}(1,1,0) + \\ & D_{it}(1 - R_{it})A_{it}Y_{it}(1,0,1) + D_{it}(1 - R_{it})(1 - A_{it})Y_{it}(1,0,0) + \\ & (1 - D_{it})R_{it}A_{it}Y_{it}(0,1,1) + (1 - D_{it})R_{it}(1 - A_{it})Y_{it}(0,1,0) \\ & + (1 - D_{it})(1 - R_{it})A_{it}Y_{it}(0,0,1) + (1 - D_{it})(1 - R_{it})(1 - A_{it})Y_{it}(0,0,0) \end{aligned}$$

The objective is to identify the causal effect of R_{it} on Y_{it}. For ease of notation, let $W^- \equiv \lim_{z \to Z_c^-} E[W_{it}|Z_{it} = z, t \geq t_0]$ and $W^+ \equiv \lim_{z \to Z_c^+} E[W_{it}|Z_{it} = z, t \geq t_0]$ with $W \in \{Y, Y(1,1,1), Y(1,1,0), Y(1,0,1), Y(0,1,1), Y(1,0,0), Y(0,1,0), Y(0,0,1),$ $Y(0,0,0)\}$.

In this setting standard continuity conditions are not sufficient for identification because of the confounding treatment D_{it}. Even assuming that all potential outcomes $E[Y_{it}(d, r, a)|Z_{it} = z, t \geq t_0]$ with $w, r, a \in \{0, 1\}$ are continuous in z at Z_c, we have that the cross-sectional RD estimator after t_0, $\hat{\tau}_{RD} \equiv Y^- - Y^+$, does not identify an average treatment effect of R_{it} at the threshold:

$$\begin{aligned} \hat{\tau}_{RD} \equiv Y^- - Y^+ &= Y(1,1,1)^- - Y(0,0,1)^+ \\ &= [Y(1,1,1)^- - Y(1,0,1)^-] - [Y(1,0,1)^+ - Y(0,0,1)^+] \\ &= E[Y(1,1,1)_{it} - Y(1,0,1)_{it}|Z_{it} = Z_c, t \geq t_0] \\ &- [Y(1,0,1)_{it} - Y(0,0,1)_{it}|Z_{it} = Z_c, t \geq t_0] \end{aligned}$$

where the first term in the right-hand-side captures one of the potential causal effects of interest (namely, the average treatment effect of establishing a minimum tenure requirement for a parliamentary pension for MPs in 2008 with a severance pay equal to $4 \cdot 80\%$

of the final wage and a monthly parliamentary pension that is 20% of the final wage) and the second term captures the 'bias' (namely, the average treatment effect of increasing the severance pay from $4 \cdot 80\%$ to $5 \cdot 80\%$ of the final wage for MPs with a monthly parliamentary pension that is 20% of the final wage). Accordingly, the cross-sectional RD estimate is biased because the effects of the two treatments D and R cannot be disentangled from each other.

Information on the pre-treatment period ($t < t_0$) allows to remove the selection bias under local assumptions. Similarly to the post-treatment period, for the pre-treatment period let $\tilde{W}^- \equiv \lim_{z \to Z_c^-} E[\tilde{W}_{it} | Z_{it} = z, t < t_0]$ and $\tilde{W}^+ \equiv \lim_{z \to Z_c^+} E[\tilde{W}_{it} | Z_{it} = z, t < t_0]$ with
$\tilde{W} \in \{Y, Y(1,1,1), Y(1,1,0), Y(1,0,1), Y(0,1,1), Y(1,0,0), Y(0,1,0), Y(0,0,1), Y(0,0,0)\}$.

To identify the causal effect of eliminating the parliamentary pension under a certain parliamentary tenure, I exploit both the discontinuous variation at Z_c and the time variation after t_0 using a 'difference-in-discontinuities' estimator $\hat{\tau}_{DD}$:

$$\hat{\tau}_{DD} \equiv (Y^- - Y^+) - (\tilde{Y}^- - \tilde{Y}^+) \tag{1.13}$$

The identification assumptions for the 'difference-in-discontinuities' design are:

Assumption 1 *All potential outcomes* $E[Y_{it}(d,r,a) | Z_{it} = z, t \geq t_0]$ *and* $E[Y_{it}(d,r,a) | Z_{it} = z, t < t_0]$ *with* $d, r, a \in \{0,1\}$ *are continuous in* z *at* Z_c

Assumption 2 *The effect of the confounding policy* D_{it} *at* Z_c *in the case of no treatment* ($R_{it} = 0$) *is constant over time and does not depend on the pension amount:* $Y(1,0,1) - Y(0,0,1) = \tilde{Y}(1,0,0) - \tilde{Y}(0,0,0)$.

Assumption 2 requires that the effect of the severance pay discontinuity D_{it} at the threshold Z_c does not vary with time nor with the pension amount. This is similar to the standard identifying assumption for diff-in-diff: it requires observations just below and just above Z_c to be on a local parallel trend in the absence of the policy of interest R_{it}. To indirectly test for this assumption, I estimate the pattern of the discontinuities in Y_{it} before t_0 and show that observations just below and just above Z_c were not on differential trends before the adoption of the minimum tenure requirement.

Under these two assumptions, the diff-in-disc estimator identifies the local causal effect of eliminating the parliamentary pension in a neighborhood of the tenure threshold ($Z_{it} = Z_c$), for MPs with a severance pay that is $4 \cdot 80\%$ of the final wage ($D_{it} = 1$) and with a monthly parliamentary pension equal to 20% of the final wage ($A_{it} = 1$):

$$
\begin{aligned}
\hat{\tau}_{DD} &\equiv (Y^{-} - Y^{+}) - (\tilde{Y}^{-} - \tilde{Y}^{+}) \\
&= [Y(1,1,1)^{-} - Y(0,0,1)^{+}] - [\tilde{Y}(1,0,0)^{-} - \tilde{Y}(0,0,0)^{+}] \\
&= [Y(1,1,1) - Y(0,0,1)] - [\tilde{Y}(1,0,0) - \tilde{Y}(0,0,0)] \\
&= [Y(1,1,1) - Y(1,0,1)] \\
&= E[Y(1,1,1)_{it} - Y(1,0,1)_{it} | Z_{it} = Z_c]
\end{aligned}
$$

Proposition 1 *Under Assumption 1 and Assumption 2, the diff-in-disc estimator* $\hat{\tau_{DD}}$ *identifies the average treatment effect at* Z_c*:* $E[Y_{it}(1,1,1) - Y_{it}(1,0,1) | Z_{it} = Z_c]$.

This result allows to identify a causal effect of the treatment of interest under plausible conditions. Yet, the estimand only refers to MPs with a severance pay equal to $4 \cdot 80\%$ of the final parliamentary wage. To identify a more general estimand with the diff-in-disc estimator, we can introduce an additional assumption.

Assumption 3 *The effect of the treatment* R_{it} *at* Z_c *does not depend on the confounding policy* D_{it}*:* $Y(1,1,1) - Y(1,0,1) = Y(0,1,1) - Y(0,0,1) \equiv E[Y_{it}(1) - Y_{it}(0) | Z = Z_c, A_{it} = 1]$

Assumption 3 states that there must be no interaction between the effect of the severance pay policy and the effect of the parliamentary pension policy. In my institutional setting, this assumption would be violated if MPs just below and just above Z_c, who receives a different severance pay, reacted to the minimum tenure requirement for the pension in different ways. In Section 1.6.2 I indirectly test this assumption by showing that the confounding policy (the severance pay) has no meaningful impact on the voting behavior of MPs. There is no significant discontinuity in the MPs' voting behavior at the 4.5 year threshold before the introduction of the minimum tenure requirement for a parliamentary pension.

1.4.2 Estimation

Let t_0 be the time in which the reform came into force (May 14, 2008). First, I can restrict the panel to the confidence votes occurred after (before) the reform $t \geq t_0$ ($t < t_0$) and implement a local linear regression:

$$Y_{ipgt} = \delta_0 + \delta_1 \tilde{Z}_{it} + D_{it}(\pi_0 + \pi_1 \tilde{Z}_{it}) + \eta_{pg} + \phi_i + \varepsilon_{ipgt} \tag{1.14}$$

where D_{it}, is an indicator for tenure of MP i at time t less than or equal to 4.5 years capturing treatment status, $\tilde{Z}_{it} = Z_{it} - Z_c$ is the parliamentary tenure of the MP centered at the threshold, η_{pg} are party-by-government fixed effects, which control for the average support that the party p in which the MP was elected gives to the government g, and ϕ_i are MP fixed effects. The coefficient π_0 is the RD estimator and identifies the local treatment effect of risking to lose the parliamentary pension. These separate regressions allow to test whether the severance pay had a significant impact on the MPs' voting behavior before t_0 or whether the change in voting behavior is driven by the introduction of the tenure requirement for a parliamentary pension after t_0.

To disentangle the two effects, I implement a difference-in-discontinuities following Grembi, Nannicini, and Troiano (2016). The method consists in fitting linear regression functions to the votes distributed within a tenure window h on either side of the tenure threshold Z_c, both before and after t_0. Formally, we restrict the sample to votes in the tenure interval $Z_{it} \in [Z_c - h, Z_c + h]$ and estimate the model:

$$Y_{ipgt} = \zeta_0 + \zeta_1 \tilde{Z}_{it} + D_{it}(\theta_0 + \theta_1 \tilde{Z}_{it}) + Post_t[\alpha_0 + \alpha_1 \tilde{Z}_{it} + D_{it}(\beta_0 + \beta_1 \tilde{Z}_{it})] + \eta_{pg} + \phi_i + \varepsilon_{ipgt} \tag{1.15}$$

where $Post_t$ is an indicator for being elected in the post-treatment period $t \geq t_0$.[9] The coefficient β_0 is the diff-in-disc estimator and identifies the treatment effect of risking to lose the parliamentary pension, as the treatment is $D_i \cdot Post_t$. In all the tables, I show the results without fixed effects, with party-by-government fixed effects only, and with all fixed effects. My preferred specification is the latter, because it does not depend on the variation in the composition of the Parliament: it can control for all time-invariant unobserved heterogeneity across MPs, taking care of any potential unobserved imbalance. This specification captures the change in voting behavior within each MP voting for the same government at the tenure threshold, which is the aim of the empirical analysis.[10]

As recommended by Kolesár and Rothe (2018), I do not cluster standard errors by the running variable as this results in confidence intervals with poor coverage property. Following their suggestion, I use conventional Eicker-Huber-White heteroskedasticity-robust standard errors. I also show that significance of the main estimates does not change if standard errors are clustered at MP level to account for within-MP serial correlation in

[9] I cannot use the 2.5-year threshold because of data limitations: my dataset does not contain votes of confidence before 1997, when this threshold was introduced. Therefore, at this threshold I cannot disentangle the minimum tenure requirement effect from the severance pay effect.

[10] To identify and estimate the coefficient of interest the specification with all fixed effects exploits the variation of votes within MPs that participated in at least one confidence vote in each side of the threshold within the bandwidth. These constitute 82% of the observations in the regression sample. If I keep only these MPs in the sample, the estimates are virtually unchanged.

the data. Each regression uses uniform kernels. Because there is no clear way to determine the optimal bandwidth in the case of a discrete running variable, I present the robustness of the results to multiple bandwidths from two months up to three years on each side of the cutoff (Iizuka et al., 2021). Following Gelman and Imbens (2019) and Lee and Lemieux (2010), my preferred estimation method is local linear regression, with different linear terms on the running variable estimated at either side of the threshold, but the main results are qualitatively unchanged when using local quadratic regressions.[11]

1.5 Data

Each of the two Houses provide information on demographics and other characteristics of all members elected in each legislature since the inception of the Italian Republic in 1948. This data includes the name, surname, gender, date and town of birth, level of education and previous job, start date and end date of each parliamentary term, the parliamentary group (party) to which each MP is affiliated and the start-date and end-date of the party affiliation during a legislature. To compute the parliamentary tenure of an MP, the days spent in both Houses are cumulated. Therefore, I have merged the Deputies' and Senators' datasets using the name and surname of the MPs as the linking variable in order to construct an accurate measure of the parliamentary tenure of each MP, by summing the duration of all their parliamentary terms in both Houses.

In addition, the two Houses kept the stenographic record of each parliamentary session in all the legislatures from XIV to XVIII. I have systemically searched for each vote of confidence and motion of no confidence occurred during these legislatures. The votes of all MPs are revealed at the end of a confidence vote. Therefore, the stenographic records contain a list of all the MPs who voted in favor, against or abstained for each vote of confidence. The non-listed MPs decided not to vote. I have digitized these votes from the original stenographic records and created a dataset that contains the voting behavior of all Deputies and Senators during the 427 votes of confidence in the Houses of Parliament from 2001 to 2022, during legislatures XIV-XVIII. Stenographic record data also provide information on the voting intention of each party, which can be used to construct an indicator variable for whether the MP was elected in a party that supports the government (majority party) or is against the government (opposition party). To determine the party in which the MP was elected, I use the first parliamentary group in the legislature

[11] Gelman and Imbens (2019) show that controlling for high-order polynomials in regression discontinuity analysis leads to noisy estimates, sensitivity to the degree of the polynomial, and poor coverage of confidence intervals. They recommend instead to use estimators based on local linear or quadratic polynomials.

to which they belong. I exclude Senators with a life tenure from the analysis as they continue to receive a parliamentary wage until the end of their life and they never receive a parliamentary pension.[12]

Finally, I have collected data on the annual before-tax income of all Deputies and Senators from 1981 to 2022. Since 1982, Italian elected officials are required to publicly disclose their annual tax returns (Merlo et al., 2010). As tax returns refer to the previous fiscal year, I have information on each Deputy's and Senator's income in the year prior to entering Parliament. I consider this variable as the data analogue of the private wage w in the model discussed in Section 1.2. Data from 1981 to 2005 and in 2013-2014 was kindly provided by the Fondazione Rodolfo Debenedetti (2009) and by the Fondazione Openpolis (2017), respectively. For the remaining years, I have digitized the copies of the tax returns of each Deputy and Senator contained in the archive at the 'Servizio Prerogative e Immunità' of the Chamber and the Senate. Only 17 out of 3,032 MPs (0.56%) in the analyzed five legislatures have a missing tax return in their first year of parliament, mostly because they started their parliamentary career before 1982 or because their parliamentary career lasted only few months.

The Italian parliament is by no means an outlier with respect to age, gender and education with respect to the parliaments of other European countries (see Merlo et al. (2010) for a comparison with the US congress). Table 1.2 contains basic descriptive statistics for the outcome variables and predetermined covariates for confidence-votes by MP with parliamentary tenure in a window of one year below and above 4.5 years. Tables A.2 and A.3 disaggregate these statistics for MPs elected in majority-opposition parties and by house of parliament. There were 41,189 potential confidence votes at the MP level in the 2001-2022 period from legislature XIV to legislature XVIII. Around 70% of these were votes of confidence in favor or against the government (the rest being abstentions or MPs not voting). Mechanically, the average number of parliamentary terms below 4.5 years of tenure (1.51) is lower than the number of terms above (2.09). Likewise, age mechanically differs by approximately 1.5 years. The education level and geographic origin of the MPs do not vary substantially around the threshold, whereas the number of female MPs decreases by 6 percentage points. This is because newly-elected MPs are more likely to be female than re-elected MPs. The annual before-tax income in the year prior to entering parliament is almost €$10,000$ lower below 4.5 years of tenure. This is because in the last legislatures there was a large influx of newly elected MPs with very low income (see

[12] A parliamentary group has to have at least 10 members in the Senate or 20 members in the Chamber of Deputies (Camera dei Deputati, 1997; Senato della Repubblica, 2017). MPs belonging to very small parties are categorized as belonging to the same parliamentary group 'Mixed'. The main results of the analysis are robust to the exclusion of these MPs.

Figure 1.5 in Section 1.6.5). Interestingly, in the year before reaching the tenure threshold the percentage of MPs voting confidence in the government is substantially higher than in the year after (74 vs 64%), whereas the probability of voting against party directives is lower (5 vs 6%).

TABLE 1.2: Summary statistics

	Tenure below 4.5 years	Tenure above 4.5 years
Abstains/no vote	.18	.21
Confidence if voting	.74	.64
Vote against party directives	.05	.06
Number of terms	1.51	2.09
Tenure (years)	4.00	4.82
Age (years)	51.17	52.71
Female	.30	.24
Pre-parliament income (€)	94384	104054
High school	.98	.98
University degree	.71	.70
Born in southern Italy	.37	.36
Born in central Italy	.22	.24
Born outside Italy	.02	.02
Observations	28084	13105

Notes: The sample is restricted to tenure between 3.5 and 5.5 years for votes of confidence between 2001 and 2022 (legislatures XIV-XVIII).

1.6 Empirical results

In the main regressions the dependent variable is defined as:

$$\textit{Confidence}_{ipgt} = \begin{cases} 1 & \text{if MP i elected in party p votes confidence in government g at time t,} \\ 0 & \text{if MP i elected in party p votes no confidence in government g at time t.} \end{cases}$$

Up to the end of legislature XVII, an abstention vote in the Senate counted as a vote against the proposed motion, so I set $Y_{ipgt} = 0$ for these particular cases. In all other cases (abstentions in the Chamber, abstentions in the Senate in legislature XVIII and no vote), I set Y_{ipgt} to missing. Tables A.8 and A.9 show that the decision to abstain or to not vote appear to be negatively affected by the minimum tenure requirement, but the decrease is only marginally significant in one specification.

1.6.1 The pension incentive increased government stability

Table 1.3 shows the diff-in-disc estimates of a minimum tenure requirement for a parliamentary pension on the probability of voting confidence in the government. In Columns (1)-(3), we see that the effect is positive and significant in all specifications for the entire sample of Deputies and Senators. As expected, the estimates become more precise when we add party-by-government fixed effects and individual MP fixed effects. Exploiting the variation in votes for the same government by the same MP, we can see that the tenure requirement significantly increases the probability of voting confidence in the government by 3 percentage points. When we add all fixed effects, the estimates are significantly positive and very similar for the Chamber of Deputies and for the Senate (Table A.5). Tables A.6 and A.7 show that these results are robust to using clustered standard errors at MP level and local quadratic regressions, respectively.[13]

[13] The effect is insignificant in the local quadratic regression without any fixed effect, but it becomes significantly positive when adding the fixed effects.

TABLE 1.3: Diff-in-disc estimates of minimum tenure requirement on confidence, all sample.

	All		
	(1)	(2)	(3)
Post*Tenure under 4.5 years	0.060***	0.019**	0.030***
	(0.023)	(0.009)	(0.004)
N	33,330	33,330	33,330
R-squared	0.02	0.76	0.95
Average outcome	0.71	0.71	0.71
MP FE	NO	NO	YES
Party-by-gov. FE	NO	YES	YES

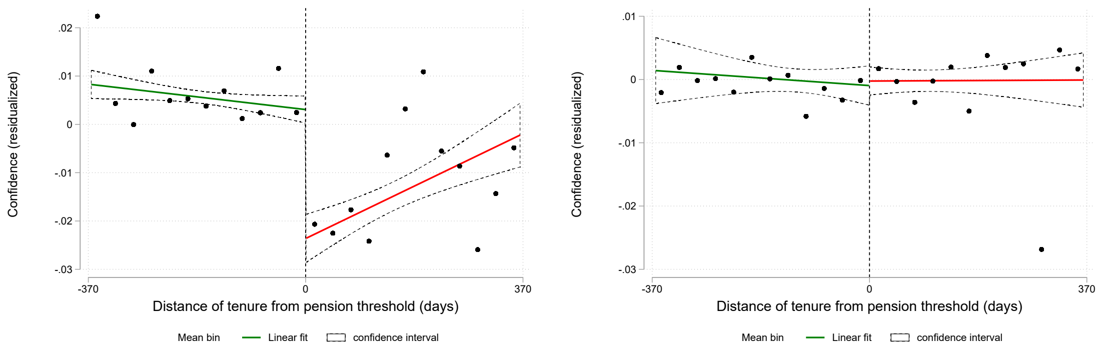
Notes: The regression sample is restricted to a bandwidth of 12 months on each side of the cutoff. Standard errors are Eicker-Huber-White heteroskedasticity-robust. Average outcome is the average of the outcome variable after reaching the tenure threshold.

Given that the regressions include MPs' and party-by-government fixed effects, Figure 1.1 plots the residuals from a regression of the outcome of interest (vote of confidence) on party-by-government fixed effects and individual MP fixed effects in order to net out these fixed effects, as suggested by Lee and Lemieux (2010).[14]

The residualized outcomes are averaged in monthly bins and plotted on the distance (in days) to the 4.5 year-tenure threshold. The figures include the predicted values of a local linear regression of the residualized outcomes on (normalized) tenure, separately for each side of the cutoff. Confidence intervals are constructed on the linear fit, with errors clustered at the MP level. The fitted lines best illustrate the trends in the data and the size of the jump, whereas the binned averages provide a sense of the underlying variability in the data. This exercise is performed for the entire sample (both Houses) and separately for each House, in the period before and after the introduction of the minimum tenure requirement for a parliamentary pension. Figure 1.1 confirms the qualitative results of columns (3) of Table 1.3. Reassuringly, the positive effect on confidence is large and significant only after the introduction of the minimum tenure requirement whereas it is very close to zero and insignificant in the pre-treatment period.

[14]Controlling for covariates or residualizing the outcome yields the same consistent estimate of the RD parameter of interest as long as the order of the polynomial of the running variable is correctly specified and the covariates are not discontinuous at the threshold Lee and Lemieux (2010). In this case, the estimates are robust to linear and quadratic specifications and all pre-determined observables are balanced. Reassuringly, Table 1.3 and Figure 1.1 produce the same qualitative results.

FIGURE 1.1: Regression discontinuities, post-treatment and pre-treatment



(A) Both Houses, post-treatment

(B) Both Houses, pre-treatment

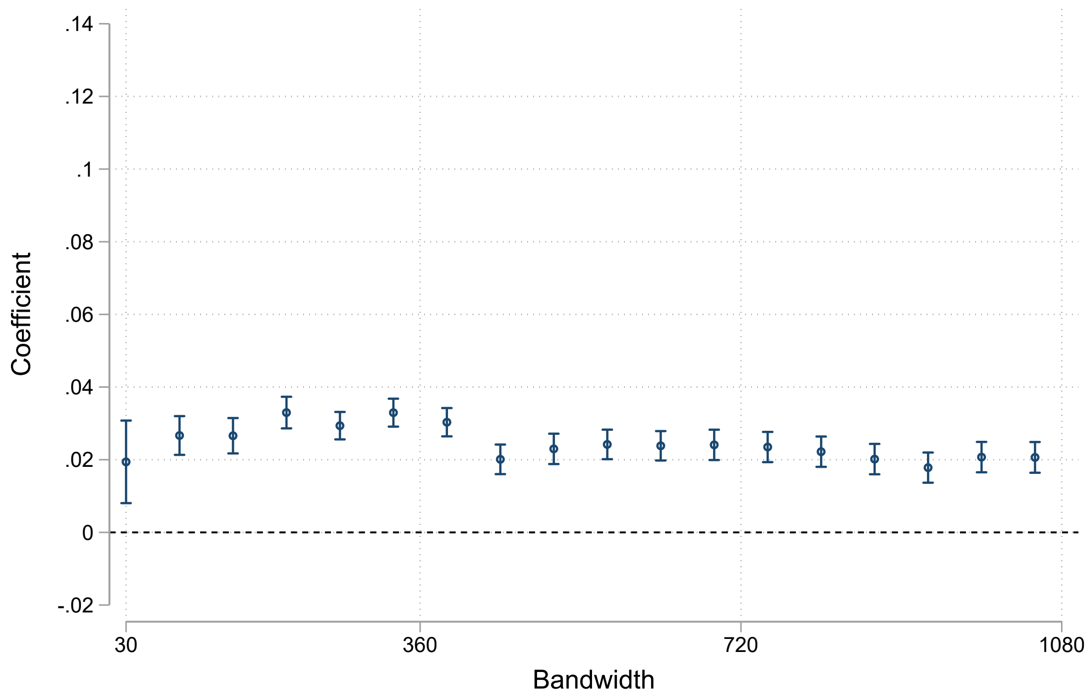
Notes: These figures show the effect of the parliamentary tenure distance from the 4.5 year-cutoff on the MP's probability of voting confidence in the government. The circles are averages across monthly bins on either side of the threshold, while the solid and dashed lines represent the predicted values and confidence intervals of a local linear regression of the outcome on (days of tenure, normalized) and the fixed effects, separately for each side of the cutoff. The bandwidth includes observations within one year from the 4.5 year-cutoff.

1.6.2 Validity tests

To confirm the validity of the empirical strategy, I perform a series of diagnostic tests. Assumption 2 requires that the effect of the severance pay discontinuity at the threshold does not vary with time nor with the pension amount. The severance pay increase does not seem to play a relevant role considering that the effect is not significantly different from zero prior to the introduction of the 4.5-year tenure requirement (Figure 1.4). This is understandable considering that the bonus for an extra year of tenure consists only in a small increase of the one-off severance payment equal to 80% of the last wage. Reassuringly, the effect becomes significantly positive in the first legislature in which the minimum tenure requirement was introduced.

Secondly, Figure 1.2 shows that the magnitude of the effect for the entire Parliament is not sensitive to the bandwidth chosen. The diff-in-disc coefficient is significantly positive and remarkably stable when using different bandwidths around the tenure threshold, from two months up to three years on each side. Figure A.5 confirms that the effect for the Senate is robust to the choice of bandwidth. The estimates for the Chamber of Deputies are always positive and significant, except when the bandwidth is very small (two months).

FIGURE 1.2: Diff-in-disc coefficients: bandwidth sensitivity



Notes: The graph shows the point estimate and the 95% confidence interval of the Diff-in-Disc coefficients on the entire sample (both Houses), estimating the regression specified in Equation (1.15) using different bandwidths. The horizontal axis shows the number of tenure days from the 4.5-year-cutoff in each side of the bandwidth.

Identification of the treatment effect requires that the MPs or the government do not manipulate the date of the confidence votes to exploit the incentive of the minimum tenure requirement for the parliamentary pension.[15] Manipulation of the timing of confidence votes may occur for two reasons. First, motions of no-confidence may be postponed after 4 years and 6 months of a parliament term in order to let newly-elected MP secure a parliamentary pension and be more willing to vote against the government. This is not the case: there are only two motions of no-confidence in the analyzed period (one in 2010 by the Chamber and one in 2014 by the Senate) and they are both before the 4.5-year threshold. Second, the government may anticipate confidence votes to speed up the legislative procedure before the 4.5-year threshold, trying to exploit the economic incentive for MPs to prolong the legislature. This does not seem to be the case. Figure

[15] In a diff-in-disc design, this requirement can be relaxed: it is sufficient that the manipulation of the running variable does not increase over time. However, the absence of any effect in the pre-treatment period allows us to concentrate in the post-period as in a standard RD.

A.2 shows that there is no bunching of confidence votes before the tenure cutoff at 0. There is some bunching around 40 days after the cutoff. However, this is only due to the fact that the government called for a vote of confidence on 5 different articles of the same law in the Senate (DDL 2941) in October 25, 2017 and for a vote of confidence on 3 different articles of the same law (Atto Camera 2352) in October 11, 2017. If anything, the government should have anticipated those votes before the threshold to get advantage of the minimum tenure requirement for a parliamentary pension.

When the running variable has a finite number of fixed support points as in this setting (tenure only change in increments of one day and there are several mass points), the standard McCrary (2008) test can falsely reject the null of no manipulation at an excessively high rate or can fail to detect actual anomalies in the running variable's density (Frandsen, 2017). Therefore, I test for the presence of manipulation around the 4.5 years cut-off using the test proposed by Frandsen (2017) in the context of regression discontinuity designs with a discrete running variable. This test relies only on support points at and immediately adjacent to the RD threshold when the running variable is discrete. The test cannot reject the null hypothesis (p-value= 0.342) of absence of manipulation in the distribution of the tenure of MPs in confidence votes, using a value for the test parameter as low as $k = 10^{-8}$ (p-value=0.342).[16]

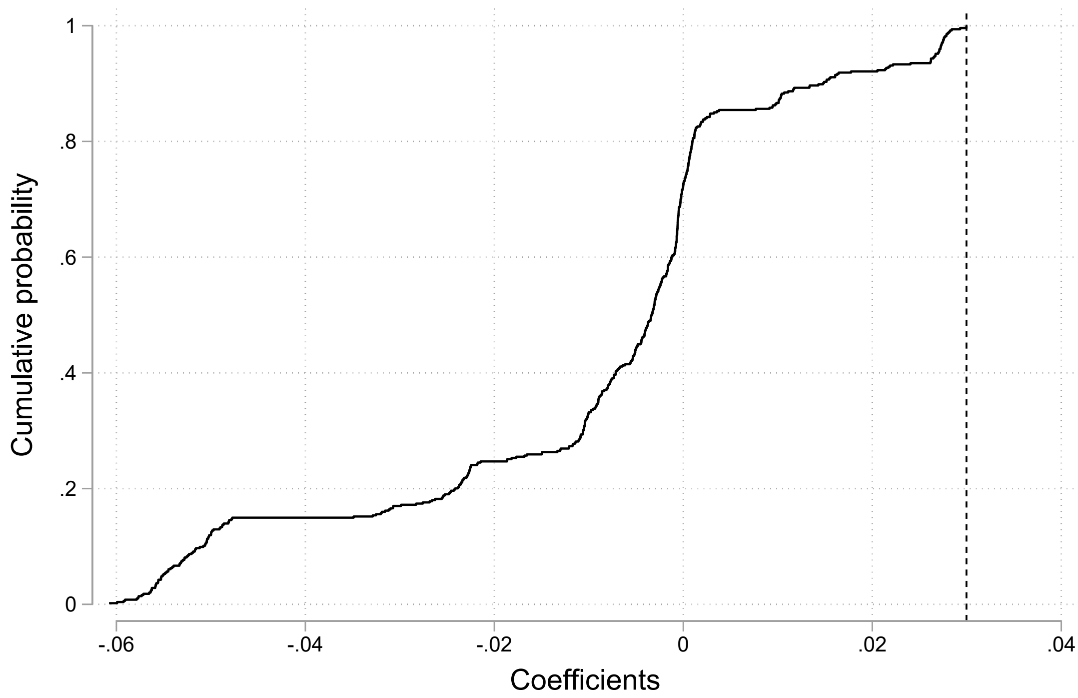
Tables A.4 and A.13 further evaluate the absence of manipulation. I implement diff-in-disc estimations with pre-determined variables characteristics (gender, education, birth place, foreign, pre-parliament income) as outcome variables without adding any fixed effects. No pre-determined characteristics show a statistically significant jump at the threshold for the entire sample and for the sample of Deputies. Only one pre-determined characteristic (gender) shows a marginally significant decrease at the threshold for Senators.

To assess the possibility that the main result arises from random chance rather than from a causal relationship, I perform a set of diff-in-disc estimations at placebo thresholds below and above 4 years and 6 months. I place the placebo thresholds at every day from 3 years and 8 months to 4 years and 4 months and at every day from 4 years and 8 months to 5 years and 4 months, in order to stay sufficiently away from the policy thresholds of 3.5, 4.5 and 5.5 years at which the severance payment, the pension age and the pension payment change discontinuously. At these false thresholds, we should not find treatment

[16] I implement the test using the Stata command *rddisttestk* (Frandsen, 2017). The parameter k determines the maximal degree of nonlinearity in the probability mass function that is considered to be compatible with absence of manipulation. A high k allows the mass at the cutoff to deviate substantially from linearity before the test can reject with high probability, whereas a low k means that even with small deviations from linearity the test will reject with high probability. A higher k implies a lower power of the test to detect manipulation.

effects similar to the estimate at the true threshold. Figure 1.3 shows the cumulative density function of these 488 placebo point estimates obtained using local linear regressions. All these placebo estimates are lower than the true-threshold coefficient for the confidence vote and the cumulative distribution function is much steeper around 0. This placebo test provides evidence that the main result is not driven by mere random noise in the data.

FIGURE 1.3: Placebo estimates



Notes: This graph shows the cumulative distribution of Diff-in-Disc estimates on votes of confidence for both Houses, from placebo local linear regressions in which the cutoff is set in different parts of the tenure distribution. Estimates are computed using the regression in Equation (1.15) within a 1-year bandwidth. Cut-offs are located at every day from 4 years and 8 months to 5 years and 4 months, in order to stay sufficiently away from the policy thresholds of 3.5, 4.5 and 5.5 years at which the severance payment, the pension age and the pension payment change discontinuously. The vertical dashed line shows the coefficient estimated using the true 4.5-year tenure threshold.

1.6.3 The pension incentive increased party discipline

An analysis of the heterogeneous effects of the policy provides further tests on the predictions of the political-agency model presented in Section 1.2. In particular, according to

Prediction 1, we expect the minimum tenure requirement to have an unambiguously positive impact on the probability to vote confidence for majority MPs, whereas, according to Prediction 2, the effect on opposition MPs is ambiguous, depending on which (if any) of the pivotal and party-discipline effect dominates.

TABLE 1.4: Diff-in-disc estimates of minimum tenure requirement on confidence, by type of party.

	Majority party			Opposition party		
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Tenure under 4.5 years	0.049*** (0.009)	0.046*** (0.009)	0.041*** (0.005)	-0.088*** (0.018)	-0.056*** (0.015)	-0.002 (0.006)
N	23,064	23,064	23,064	9,226	9,226	9,226
R-squared	0.01	0.06	0.79	0.03	0.22	0.90
Average outcome	0.96	0.96	0.96	0.09	0.09	0.09
MP FE	NO	NO	YES	NO	NO	YES
Party-by-gov. FE	NO	YES	YES	NO	YES	YES

Notes: The regression sample is restricted to a bandwidth of 12 months on each side of the cutoff. Standard errors are Eicker-Huber-White heteroskedasticity-robust. Average outcome is the average of the outcome variable after reaching the tenure threshold.

As we can see in Table 1.4, the model predictions hold. The effect of the tenure requirement is positive and highly significant for majority MPs, whereas it is negative for opposition MPs. The party-discipline effect appears to dominate the pivotal effect, even though the significance of the negative effect disappears when we control for party-by-government and MPs' fixed effects (column 6), as confirmed by Figure A.10.[17] Note that according to the model presented in Section 1.2, these empirical results imply that the minimum tenure requirement is unambiguously distortionary because it reduces voters' welfare.

Table 1.5 confirms that the minimum tenure requirement increased party discipline. This table reports the estimates of the diff-in-disc coefficient β_0 in (1.15) when as dependent variable I use an indicator variable equal to 1 if the MP voted against party directives at time t and 0 otherwise. The pension incentive reduced the probability of voting against party directives by 3.4 percentage points, from an average of 5 percentage points.

[17]As we can see in Figure A.6, the significantly positive treatment effect on majority members is robust to the choice of different bandwidths, whereas the effect on opposition parties appears to be sensitive to the choice of the bandwidth.

TABLE 1.5: Diff-in-disc estimates of minimum tenure requirement on voting against party directives.

	All		
	(1)	(2)	(3)
Post*Tenure under 4.5 years	-0.019**	-0.034***	-0.034***
	(0.010)	(0.010)	(0.006)
N	32,707	32,707	32,707
R-squared	0.01	0.09	0.68
Average outcome	0.05	0.05	0.05
MP FE	NO	NO	YES
Party-by-gov. FE	NO	YES	YES

Notes: The regression sample is restricted to a bandwidth of 12 months on each side of the cutoff. Standard errors are Eicker-Huber-White heteroskedasticity-robust. Average outcome is the average of the outcome variable after reaching the tenure threshold.

Further model predictions. According to Prediction 1 and 2, if the party-discipline effect dominates, a larger government majority should weaken the pension incentive. This is because the probability to secure the pension in a first parliamentary term is higher and MPs are less induced to be loyal to their own party, reducing the party-discipline effect. In addition, with larger majorities MPs are less likely to be pivotal voters, reducing the pivotal effect. To test this prediction, I define the government 'majority margin' in each house as the difference between the number of MPs voting confidence in the first vote of confidence (government confirmation) and the minimum number of MPs to obtain a majority (315 in the Chamber and 158 in the Senate). I restrict the sample to the period after the introduction of the minimum tenure requirement and perform the estimation interacting all the regressors in (1.16) with the majority margin, separately for majority and opposition MPs.[18] These estimations rely on the conditional independence assumption that the interaction is not capturing the effects of correlated unobservables.

Table A.12 shows the estimates of the relevant interaction: $D_i \cdot Margin_{it}$. According to the model, a larger majority margin should dampen the positive effect for majority MPs and the negative effect for opposition MPs. The empirical estimates in Table A.12

[18] Let Margin_{ig} be the majority margin of government g in the first confidence vote in the parliamentary house of MP i. I estimate

$$Y_{ipgt} = \psi_0 + \psi_1 \tilde{Z}_{it} + D_{it}(\omega_0 + \omega_1 \tilde{Z}_{it}) + \mathit{Margin}_{ig}[\psi_2 + \psi_3 \tilde{Z}_{it} + D_{it}(\omega_2 + \omega_3 \tilde{Z}_{it})] + \nu_{ipgt} \quad (1.16)$$

Table A.12 reports the coefficient of interest ω_2, as well as the baseline coefficient ω_0.

validate the model predictions: the estimates of the interaction coefficient are significantly negative for majority MPs and positive for opposition MPs. Controlling for MP and party fixed effects, I find that one hundred additional MPs sustaining the government dampens the effect of the pension by 2.7 percentage points on majority MPs and by 0.5 percentage points on opposition MPs, even though the latter effect is not significant.

According to Prediction 1 and 2, we also expect that closeness to retirement age should amplify the effect of the pension incentive.[19] In Table A.11, I perform the same exercise as above using age as the interaction variable. Age should amplify the positive effect for majority MPs and the negative effect for opposition MPs. The model predictions appear in line with the empirical estimates in Table A.11. as the estimates of the interaction are significantly positive for majority MPs and significantly negative for opposition MPs. An additional year of age amplifies the effects on majority MPs by 0.1 percentage point and on opposition MPs by 0.3 percentage points.

1.6.4 Back-of-the-envelope calculations on government stability

To have a sense of the magnitude of these results, we can perform a back-of-the-envelope calculation on the percentage of confidence votes Italian governments would have lost in the absence of the minimum tenure requirement. Based on column (3) in Table 1.4, I assume that the effect of the policy was a homogeneous increase of 4.1 percentage points in the confidence votes by majority MPs with a tenure below 4.5 years over the entire post-treatment period. Based on column (6) in Table 1.4, I assume that the policy did not have any effect on opposition MPs. This exercise hinges on the strong assumptions that the effect is homogeneous across majority MPs (under 4.5 years of tenure) and across opposition MPs and that we can extrapolate the treatment effect far from the tenure threshold, while in fact it is identified only locally.

Since the introduction of the minimum tenure requirement (legislatures XVI-XVIII), there have been 344 confidence votes. According to these calculations, in eight of them (2.3%) the government would have lost a vote of confidence had the tenure requirement for a parliamentary pension been absent. Seven of them occurred during the government 'Berlusconi IV' in legislature XVI and one of them during the government 'Conte II' in

[19] Another interesting heterogeneity analysis is the effect by gender. In Table A.10 we see that the effect on women appears to be positive but lower than the effect on men, even if the difference is not significant in all specifications. Estimates for women are more noisy, as the share of female MPs was very low in legislature XIV (11%) and only gradually increased up to 36% in legislature XVIII.

legislature XVI. Taking these calculations at face value, the government 'Berlusconi IV' would have fallen on December 10, 2010 instead of resigning on November 17, 2011.[20]

It is interesting to note that the spread between the ten-year Italian Treasury Bonds and the Bund rose sharply from 1.6% in June 2011 to 5.5% in November 2011, starting to decline only when Berlusconi resigned and was replaced by Mario Monti (Manasse, Trigilia, and Zavalloni, 2013). Italy experienced a severe sovereign debt crisis in the final months of Berlusconi's government, which an earlier loss in a confidence vote could potentially have prevented.

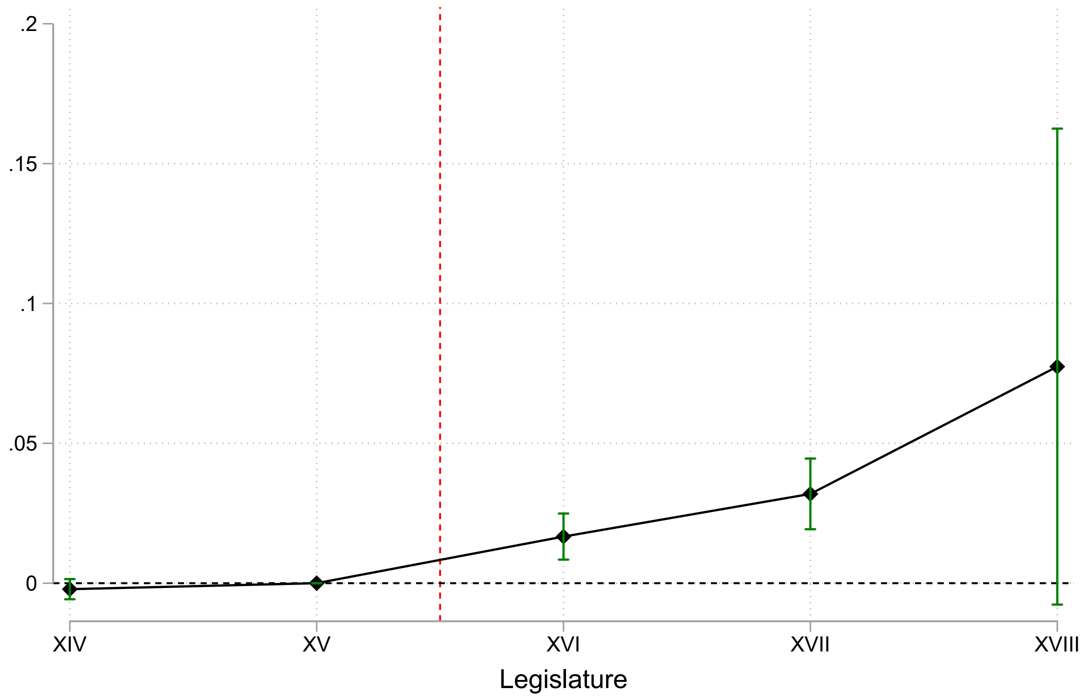
1.6.5 Effects over time

Figure 1.4 shows the timing of the effect performing the RD regression in Equation (1.14), separately for each of the five legislatures in the analyzed period. Reassuringly, the effect is not significantly different from zero before legislature XVI and it becomes significantly positive when the minimum tenure requirement is introduced.[21]

[20] The government 'Conte II' resigned after that vote of confidence, so the tenure requirement would have been inconsequential in this case.

[21] Legislature XV lasted less than two years and does not have a sufficient number of observations around the tenure threshold of 4.5 years to perform the RD estimation.

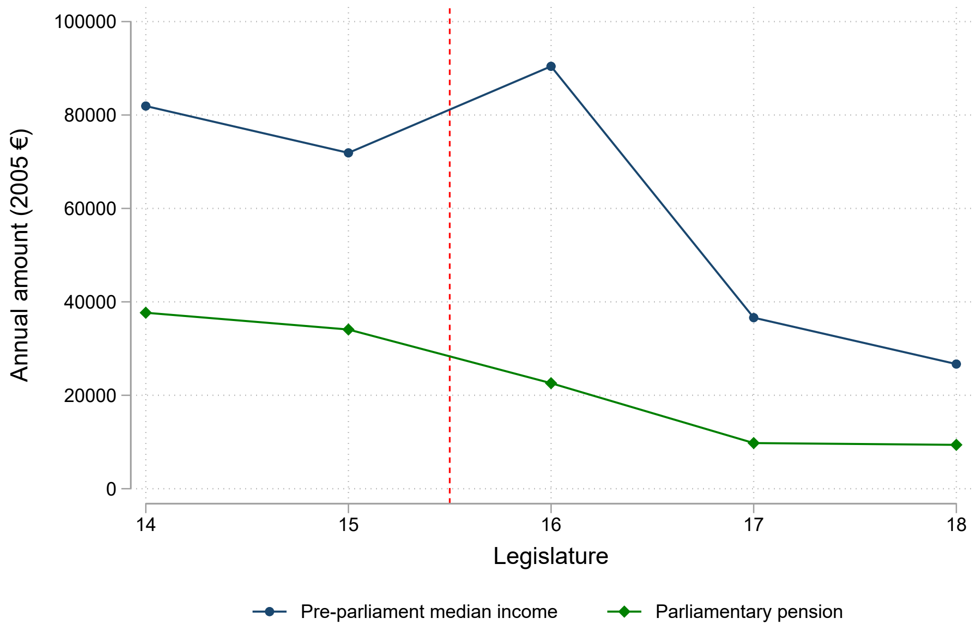
FIGURE 1.4: RD coefficients, by legislature (both Houses)



Notes: These figures show the RD coefficient and its 95% confidence interval estimating regression Equation (1.14) for both Houses, separately for each legislature (XIV-XVIII). The red dashed line indicates the introduction of the minimum pension requirement at 4.5 years.

The effect appears to increase over time, despite the pension amount decreases in the last two legislatures (Figure 1.5). The confidence interval is larger in the last legislature because it ended prematurely and there are less observations in the window around the 4.5 year threshold. Two reasons can explain the increase in the point-estimates over the legislatures: a negative shift in the distribution of newly-elected MPs' private income and a decrease in the probability of being elected.

FIGURE 1.5: Parliamentary pension and pre-parliament income, by legislature



The Italian parliament experienced a drastic turnover between legislature XVI and legislatures XVII-XVIII. The anti-establishment '5-star Movement' elected zero MPs in legislature XVI, but became the party with the largest number of MPs in the following two legislatures. Using newly-collected data on MPs' tax returns for the year prior to entering parliament, we can see that the distribution of pre-parliament income changed substantially between legislature XVI and XVII-XVIII. Figure 1.5 shows that the median pre-parliament annual income (in 2005 €) dropped from 90424€ in legislature XVI to 36613€ in legislature XVII and 26690€ in legislature XVIII. The median age of newly-elected MPs also decreased from 50 years in legislature XVI to 45-46 years in legislatures XVII-XVIII (Figure A.12). Controlling for a quadratic polynomial of age when entering parliament, the decrease in average income from legislature XVI to legislatures XVII-XVIII is significant and amounts to about 50,000 euros (Figure A.13).

Figure A.14 shows that the pre-parliament annual income distribution in legislature XVI first-order stochastically dominates the pre-parliament annual income distributions in legislature XVII and legislature XVIII. The share of MPs earning zero income increased from 0.54% in legislature XVI to 8.48% and 6.62% in legislature XVII-XVIII, respectively. The share of MPs earning income below the poverty line (11,239€ in 2005) increased from 3.78% in legislature XVI to 17.78% and 22.68% in legislature XVII-XVIII, respectively. The parliamentary pension became a larger share of the pre-parliament median income in legislature XVII-XVIII (27-35%) relative to legislature XVI (25%). Assuming diminishing marginal utility, the parliamentary pension represented a stronger incentive for MPs with lower private income and lower expected private pension.

An additional reason for the larger increase in legislature XVIII is that the number of MPs in each House was cut by 36.5% starting from legislature XIX, thus reducing the probability that MPs would be re-elected and would obtain a parliamentary pension in the following legislature. According to the model, a fall in the re-election probability should reduce loyalty to the party by both majority and opposition MPs, resulting in a less positive party-discipline effect for majority MPs and in a less negative party-discipline effect for minority MPs. Figure A.4 is in line with the model predictions, with a drop for the effect on majority MPs and an increase for the effect on minority MPs in legislature XVIII. Overall, the pivotal effect which is unambiguously positive for both majority and opposition MPs becomes predominant with respect to the party-discipline effect and the point-estimate increases.

1.7 Conclusions

This paper employed a difference-in-discontinuities design to test a political agency model of MPs' opportunistic behavior in predicting the impact of the introduction of a minimum tenure requirement to obtain a parliamentary pension in the Italian Parliament. The change in the parliamentary perquisite increases the probability of voting confidence in the government by 3 percentage points. The pension incentive increased confidence votes by MPs elected in parties that support the government, whereas it decreased confidence votes by MPs' elected in opposition parties. These empirical estimates are consistent with the predictions of a political-agency model in which voters have imperfect information about government performance and MPs are opportunistically interested in reaching the tenure requirement to obtain a parliamentary pension. Beyond the direct incentive to keep the current government in power, the policy increases party discipline: it induces opposition (majority) MPs to vote against (for) the government so as to increase the probability of being re-elected and reach the tenure requirement in a second term in case the government falls.

A caveat on these estimates is that the internal validity of the empirical strategy comes at the price of lower external validity, as is always the case in local econometric designs based on policy discontinuities. A theoretical and empirical analysis to understand how tenure requirements for parliamentary perquisites can affect the quality of the elected policymakers and their decisions to enter/exit the political career is an interesting avenue for further research.

An important policy implication of this paper is that parliamentary benefits should be designed very carefully. Monetary incentives for legislators to remain in power entail a trade-off between political stability and party polarization. They can reduce the policy uncertainty associated with unexpected government crises but they can also become a hidden tool for parties to enhance their control over legislators, inducing them to vote following party directives rather than voters' interest.

Chapter 2

College Access When Preparedness Matters: New Evidence from Large Advantages in College Admissions

2.1 Introduction

Young adults from better-off families are much more likely to attend college than those from worse-off families.[1] One policy response to this intergenerational inequality is to provide college admission advantages to students from disadvantaged contexts. Context-based admissions are gaining increasing attention, especially as admissions based on race or ethnicity are proving contentious (Arcidiacono and Lovenheim, 2016).

Most evidence on context-based admissions comes from programs designed to improve the opportunities of disadvantaged students to attend more selective colleges (Black, Denning, and Rothstein, 2023; Bleemer, 2021; Kapor, 2020; Long, Saenz, and Tienda, 2010; Niu and Tienda, 2010). Many of the disadvantaged students they target have sufficient academic preparedness to be admitted to some college.[2] Prior studies have concluded that context-based admissions improve the enrollment in selective colleges of such relatively well-prepared students and that the effects for the most part persist until graduation. An entirely open question is what impacts more extreme forms of admission advantages would have on the college enrollment and persistence of students further down the academic preparedness distribution. Answering it is necessary to build policy recommendations that extrapolate beyond the populations studied so far. Understanding how far

[1] For example, in the United States children from families where at least one parent has attained higher education are 37 percentage points more likely to have a college degree than children from families where neither has. In the United Kingdom the figure is 40, in Chile and Australia 35, in Germany 26 (OECD.Stat).

[2] The same has been argued for race-based preferences in undergraduate admissions (e.g. Arcidiacono, Aucejo, Coate, and Hotz, 2014; Hinrichs, 2014; Machado, Reyes, and Riehl, 2023).

context-based admissions can go while generating persistent educational gains for disadvantaged students is also the starting point for discussions about their optimal design.[3]

This paper answers this question in the context of Chile, which offers three advantages: a policy, called PACE (*Programa de Acompañamiento y Acceso Efectivo a la Educación Superior*), that provided unusually large advantages in admission, detailed longitudinal data from a transparent centralized admission system linkable to survey data, and successive governments willing to collaborate to experimentally evaluate the admission policy. PACE targets disadvantaged schools, and it offers students who graduate in the top 15 percent of their high school guaranteed admissions to colleges participating in the centralized admission system, eliminating the entrance exam score requirement.[4] These colleges offer five-year (and longer) programs of an academic nature, and the PACE admission offerings are guaranteed by an official agreement between the government and the colleges.[5] Students in PACE high schools are considerably disadvantaged: they have 10^{th} grade standardized test scores that are 1.5 standard deviations below those of regular college entrants and 0.49 standard deviations below the OECD average, 77 percent attend vocational high school tracks, 61 percent are categorized as socioeconomically vulnerable by the government, their family income is half the median Chilean income, and their most common choice is to not attend any form of higher education (nearly 60 percent), followed by attending two-year vocational programs (nearly 30 percent). PACE expanded college access dramatically, and more extensively than the context-based admission policies most studied so far.[6]

We would expect PACE to increase the college admissions and enrollments of top performing students in targeted schools. But given the considerable level of disadvantage of the students and the academic nature of the college programs, it is ex-ante unclear how

[3] Critics of preferential college admissions claim that they can induce disadvantaged students to pursue educational opportunities for which they are not academically prepared, leading to a larger dropout from higher education than would have occurred absent such policies, and harming the labor market prospects of these students. This is sometimes dubbed the “mismatch hypothesis" (Arcidiacono et al., 2011; Sander, 2004). This paper examines the persistence of the impacts on higher education enrollment but does not aim to test for the mismatch.

[4] Throughout the paper we describe the PACE policy as it was for our sample. Some changes to the PACE rules were recently introduced, but they did not affect the students in this study’s sample.

[5] Of the 39 institutions participating in the centralized admission system, 29 signed the agreement and offered PACE slots (Figure B.2 shows the quality distribution of PACE seats and of all regular seats offered through the centralized admission system). Higher-education institutions outside the system do not have minimum admission requirements and many provide vocational and shorter degrees.

[6] For comparison, students targeted by the Texas Top Tep (TTT) and Californian Eligibility in the Local Context (ELC) context-based policies on average score comparably or better than other entrance-exam-test takers in Texas and other college applicants in California (already positively selected populations); around 20 percent of those in schools targeted by the TTT are socioeconomically vulnerable (i.e., eligible for reduced or free school meal), and the median income of those around eligibility cutoffs in schools targeted by the ELC is 90% of the median Californian income. See section 2.3.4.

persistent any such impacts would be. Also ex-ante unclear is how PACE could affect students who are not top-performing in their school, and how it could affect students while they are still in high school, for example by inducing a response in teachers' focus of instruction or students' study effort. High school impacts could in turn matter for the college persistence of those induced to enter college and, ultimately, for the persistence of the college enrollment impacts.

To answer all these questions, we construct a new dataset that links administrative data to survey data we collected in schools. The dataset is longitudinal and follows $9,006$ targeted students for 9 years: from 9^{th} grade to five years after high school graduation. Thanks to high-quality administrative records, we observe standardized achievement measures, grades in school, students' socioeconomic and demographic characteristics, and the full path of education choices, from the type of high school to the higher education choices and persistence or graduation up to five years after leaving high school. We further collected survey data in schools, which we linked to the administrative data through student, classroom and school identifiers. Our survey data include information on students' effort and standardized achievement test scores in the last high-school year, on students' beliefs about their relative and absolute ability, on teacher effort, focus of instruction and grading practices, and on school inputs such as remedial and college entrance exam classes.

The dataset is one of the most comprehensive datasets constructed to date on the transition from high school to higher education of a population targeted by an admission policy. Compared to existing studies of admission policies, it contains new variables on high school students and is novel in linking information from multiple actors in schools. For example, virtually nothing is known about the beliefs of high school students targeted by admission policies, or the focus of instruction and effort of their teachers.[7] The dataset, therefore, gives us the opportunity to examine mechanisms of admission policy impacts in the pre-college phase that could not be precisely analyzed before. A further innovation of this study is that we identify policy impacts through the randomization of the policy that took place in 2016 with the explicit purpose of evaluation.[8] Thus, the unique design of PACE, the dataset, and the randomized experiment provide the ideal setting to understand the effects of large admission advantages for the first time.

[7] Kapor, 2020 uses survey data on applications collected in the schools targeted by the Texas Top Ten (TTT). Golightly, 2019 uses administrative data on high school performance of students targeted by the TTT. Akhtari, Bau, and Laliberte, 2022 use administrative data on SAT scores, high school performance and applications before and after bans on race-based affirmative action in the United States, and survey data on students' time on homework and whether they received guidance from a counsellor.

[8] Paper co-author Michela Tincani is leading the experimental evaluation of PACE together with the Ministry of Education and the Ministry of Finance, and co-authored several policy reports, including those officially released by the Ministries (Cooper, Sanhueza, and Tincani, 2020; Cooper, Guevara, Rivera, Sanhueza, and Tincani, 2019, 2022).

The first set of findings is that PACE increased the college admissions and enrollments of disadvantaged students by 4.1 and 3.1 percentage points (p.p.), corresponding to a 36 percent increase in admissions and enrollments compared to the control group. Considering the high level of disadvantage of the targeted students, the government considered these effects satisfactory, and chose to keep the policy in place.[9] The effects are concentrated among students who in 10^{th} grade, before the experiment started, were in the top 15 percent of their high school, while students in the bottom 85 percent experienced no significant change in college admissions and enrollments. The enrollment effects, however, decreased significantly over time: five years after leaving high school, the PACE impacts on continuous enrollment or graduation from college were 1.1 percentage points, around a third of the effects four years earlier.[10] For comparison, context-based admission programs such as the Texas Top Ten and the Californian Eligibility in the Local Context achieved substantially more persistent impacts on the relatively better-prepared populations they targeted.[11]

The second set of findings is that PACE had a negative impact of 0.1 standard deviations on study effort and achievement in high school, as measured through our survey. The result is confirmed using administrative data: PACE decreased GPA in the core subjects (e.g. Mathematics, language) in the last high-school year. Crucially, PACE reduced precisely the dimensions of pre-college human capital that predict persistence in college in the very disadvantaged group we study. Therefore, the endogenous response of pre-college effort and achievement could have contributed to the waning enrollment impacts of PACE by affecting the college preparedness of college entrants under PACE.

To understand the PACE impacts in schools, we analyzed all the mechanisms we specified in the pre-analysis plan (students' response to incentives, teacher grading, teachers' effort and focus of instruction, changes in school inputs), and an additional one motivated

[9]Following a presentation by paper co-author Michela Tincani and her collaborators in the Ministries of Education and Finance to the Budget Office of Chile in May of 2019, the then right-leaning Piñera government chose to keep PACE in place, as these early results were considered a success. The policy was first introduced by the left-leaning Bachelet government.

[10]The enrollment effects at five years are significantly positive in the sub-sample of top-performing students, to which the policy was targeted, but significantly lower than the impacts on first-year enrollment in this group too.

[11]The impacts of the Texas Top Ten on enrollment or graduation six years after leaving high school were three-quarters of those in the first year (Black, Denning, and Rothstein, 2023). The near-threshold students enrolling into a selective college under the Eligibility in the Local Context had a 75 percent probability of graduating (Bleemer, 2021), compared to a 58 percent probability of persistence or graduation at five years among all those from the top 15 percent of PACE schools who entered college (which arguably includes better prepared students than the near-threshold ones).

by the finding that the impacts are negative (a reduction in the perceived returns to college). We find evidence in support of students' response to incentives.[12] The evidence is most consistent with students responding to perceived, rather than actual incentives: by linking our survey data on believed outcomes to actual outcomes, we document that most students have large over-optimism about their absolute and relative (within-school) ability, likely mis-perceiving their distance from regular and preferential admission cutoffs. Consistent with the widespread belief biases, the negative impacts on pre-college effort and achievement are widespread too, unlike what would be expected under rational expectations (see e.g. Bodoh-Creed and Hickman, 2018), where the sign of the effect should vary across students depending on their distance from regular and preferential admission cutoffs. The evidence, therefore, suggests many behave as if without PACE an admission is within reach, and with PACE it is guaranteed.

The reduced-form findings so far suggest that college preparedness reduced as an effect of the change in admission rules. This is a novel finding suggesting a potential role for school interventions. But without more structure, it is impossible to measure whether the policy impacts on pre-college effort mattered for the persistence of the PACE impacts: the latter depends on who self-selects into college under PACE, which we do not observe in the data under counterfactual effort responses. For this reason, we develop a dynamic structural model of pre-college effort, entrance-exam taking, admissions and enrollments, with and without PACE, that delivers the college preparedness of college entrants as an endogenous outcome. The model allows us to simulate the impacts of hypothetical school interventions designed to improve the college preparedness of college entrants under large admission advantages, which is of great policy relevance.

In the model, students have heterogeneous preferences for college that vary with their observed and unobserved characteristics, and when choosing pre-college effort, they anticipate the impact it will have on their perceived admission likelihoods. We can relax rational expectations thanks to the high-quality measures of beliefs we collected, which,

[12] We are confident that the GPA and achievement reductions are not the result of a change in the ability composition of students in the treatment group, which could occur when students strategically select into high schools offering admission advantages. First, the announcement that a school was in PACE was made after the deadline for school enrollment in the 11^{th} grade, and as students need to be in a PACE schools for the last two high-school years to benefit from the percent rule, they did not have an incentive to change school at a later time either. Second, the student characteristics are balanced across treatment groups (Table 2.1), indicating lack of strategic high school selection. Third, we further analyzed school transitions in and out of PACE schools around the time of our experiment and we find no systematic relation between baseline test scores and entering or leaving a PACE school (Table B.18). Finally, strategic high school enrollment typically induces more advantaged students to enter schools where preferential admission policies are in place, leading to an observed *increase*, not decrease, in GPA and test scores.

we show, can independently predict high-stake outcomes up to five years after we administered the survey.[13] The perceived likelihood of a regular admission depends on the perceived entrance exam score, and the perceived likelihood of a preferential admission depends on the perceived GPA rank. Both depend on the choice of effort. Rank depends also on the effort of school peers, a strategic interaction.[14] Informed by survey evidence suggesting that students do not expect pre-college effort to affect college persistence, in the model the payoff from college enrollment does not depend on pre-college effort. The model can successfully replicate the experimental findings, including those that would be hard to fit with models that assume rational expectations.

The model allows us to quantify the magnitude of the perceived incentive effects of PACE. We quantify that students believe the policy reduced the returns to effort considerably, by 77%. They believe that one study hour per week increases the admission likelihood by 6.9 p.p. absent PACE, and by only 1.6 p.p. when PACE is introduced.

To understand whether school interventions that affect pre-college effort could improve the college preparedness of college entrants, we perform two counterfactual policy simulations. First, we correct students' over-optimistic beliefs about the entrance exam score and the GPA rank. We assign to students rational expectations, and solve for the Bayesian Nash Equilibrium of the tournament game taking place in PACE schools.[15] We find that eliminating belief biases in high school affects both the baseline ability of the students who self-select into college (the *selection* channel), and the pre-college effort they exerted while in high school (the *effort* channel), both of which predict persistence. Since over-optimism leads high-ability students to incorrectly perceive an admission as guaranteed and under-provide effort, and low-ability students to incorrectly perceive it as within reach and over-provide effort, eliminating over-optimism increases the effort of high-ability students and decreases that of low-ability students. As effort affects the admission credentials, eliminating the effort under- and over-provisions increases the admissions of the high ability and decreases those of the low ability, improving the ability of

[13] The predictive validity exercise is more nuanced than simply showing that our belief measures can independently predict outcomes far in the future. We find that the belief measures behave the way they should if they were capturing what we expect them to capture. The entrance exam score affects the admission likelihood in both the treatment and control groups, and accordingly, we find that the belief about the entrance exam independently predicts entrance exam taking, admission, enrollment and persistence up to five years later in both groups. In contrast, the within-school rank strongly affects the admission likelihood in the treatment group but not in the control group, and accordingly, we find that the belief about the rank independently predicts those same outcomes in the treatment group, but not in the control group.

[14] To capture such strategic interaction in a setting with biased beliefs, we implement an established approach from the behavioral game theory literature. We assume students choose effort to best respond to what they perceive the within-school admission cutoff to be, which we have elicited, without imposing equilibrium beliefs (see e.g. Camerer, Ho, and Chong, 2004; Costa-Gomes and Crawford, 2006; Costa-Gomes and Zauner, 2003; Crawford and Iriberri, 2007; Stahl and Wilson, 1995).

[15] We find that at the estimated parameter values, the BNE is unique.

college entrants by 0.08 standard deviations according to 10^{th} grade test scores, and their pre-college effort by 0.31 study hours per week (or 0.60 standard deviations). Correcting pre-college belief errors about entrance exam scores and GPA rank, therefore, could improve the college preparedness of those who self-select into college under large admission advantages.

In the second counterfactual experiment, we consider an alternative school intervention because some policymakers consider providing rank information controversial. We simulate the impacts of informing high school students in PACE schools of the importance of pre-college effort for persistence in college. We assign to students payoffs from enrolling in college that depend on pre-college effort to the extent it predicts college persistence. Since students are forward-looking, this counterfactual changes the continuation value of effort in high school. We find that this intervention is less effective than correcting belief errors. It would improve the pre-college effort of college entrants by only 0.09 study hours per week (or 0.18 standard deviations), and it would not change the baseline ability of college entrants. Given the widespread over-optimism about admission chances, this intervention would increase also the pre-college effort of those who end up not being admitted, with ambiguous welfare implications.

2.2 Contributions to the Literature

This paper makes three main contributions. First, it provides the first evidence of the impacts of context-based admissions targeted at very disadvantaged students who score very low on high school standardized tests, and finds that college preparedness in this group is elastic to investments and incentives shortly before college. We study education outcomes in high school and up to five years after leaving high school, and extend the previous literature by studying these outcomes jointly.[16] This allows us to show that the policy negatively affected precisely the dimensions of pre-college human capital that matter for college persistence five years later. It increased college admissions and enrollments,

[16]For example, Kapor, 2020 studies impacts of the Texas Top Ten on college attainment abstracting from pre-college achievement, and Golightly, 2019 studies its impacts on pre-college achievement abstracting from college attainment. For other studies of the impacts of percent plans on college enrollment and persistence, see also Long, Saenz, and Tienda, 2010, Niu and Tienda, 2010, Daugherty, Martorell, and McFarlin, 2014. In the context of race- or ethnicity-based admission policies, a large literature studies impacts on college attainment, abstracting from impacts on pre-college achievement (see the review in Arcidiacono, Lovenheim, and Zhu, 2015). For evidence outside of the United States see, for example, Bagde, Epple, and Taylor, 2016, who estimate impacts of caste-based affirmative action in India on enrollment in engineering colleges and graduation at the end of the fourth year. The literature on pre-college impacts of affirmative action is smaller. Akhtari, Bau, and Laliberte, 2022 study impacts on pre-college academic performance of affirmative action bans in the United States, abstracting from college enrollment and later outcomes.

but we found less persistent enrollment impacts compared to previous studies of context-based admissions focusing on relatively better-prepared students (Black, Denning, and Rothstein, 2023; Bleemer, 2021). The results highlight the importance of college preparedness among very disadvantaged populations, and its nuanced role. An old argument against admission advantages is that they can lead under-prepared students to enter college (Arcidiacono et al., 2011; Ichino, Rustichini, and Zanella, 2022; Sander, 2004). Our results demonstrate that college preparedness can be elastic to investments in the last high-school years, which themselves respond endogenously to admission rules. An important policy implication is that expanding admission advantages to very disadvantaged populations could lead to more persistent impacts if combined with school interventions shortly before college designed to improve the preparedness of college entrants.

Second, this paper provides the first experimental evidence of the impacts of admission policies. Previous studies examined the impacts of college admission on college attainment around admission cutoffs (e.g. Goodman, Hurwitz, and Smith, 2017; Niu and Tienda, 2010; Zimmerman, 2014), estimating local average effects. Thanks to our experimental research design, we can, for the first time, extrapolate away from admission cutoffs without relying on structural model assumptions (Bleemer, 2021; Kapor, 2020; Otero, Barahona, and Dobbin, 2021), or on the parallel trend assumptions of difference-in-differences designs (Black, Denning, and Rothstein, 2023; Bleemer, 2022).[17] We show that the positive impacts on college enrollment are concentrated among those who at baseline were in the top 15 percent of their school, but the negative impacts on pre-college effort and achievement were more widespread. Leveraging on the survey data we collected in schools, we find that a plausible explanation is that students responded to incentives under biased beliefs about their absolute and relative ability, a novel finding that suggests new avenues for the design of large admission advantages.

Third, this paper contributes to the structural literature modelling admission policies (e.g. Arcidiacono, 2005, Kapor, 2020, Otero, Barahona, and Dobbin, 2021) by endogenizing pre-college effort.[18] Building on the results from the survey data, the model

[17]In Texas, the ban on race-based affirmative action and the introduction of the Texas top Ten were nearly simultaneous, making it difficult to isolate the impacts of one of these two policies using difference-in-difference strategies. Parallel trend assumptions are not always satisfied, as shown in Akhtari, Bau, and Laliberte, 2022 for the case of states that did and did not ban affirmative action following the Grutter v. Bollinger court ruling.

[18]To the best of our knowledge, so far this has only been done in Bodoh-Creed and Hickman, 2018, whose structural model includes both pre-college effort and college attainment as endogenous outcomes, and where pre-college effort affects admission likelihoods. The paper estimates the model with data from the United States, assuming that minority and majority students face different admission cutoffs by virtue of extant affirmative action policies. The study does not exploit changes in admission policies to identify their impacts. In contrast, we exploit RCT-based causal estimates of the impacts of PACE to estimate our

relaxes rational expectations assumptions, thus also contributing to the literature on dynamic models of education choices under information frictions. The model extends standard dynamic choice models (e.g. Behrman, Tincani, Todd, and Wolpin, 2016; Keane and Wolpin, 1997) by simultaneously allowing for a subjective value function based on the perceived evolution of the state space, and a true evolution of the state space that follows objective admission likelihoods. It contributes to the literature estimating dynamic models using data on both perceived and actual outcomes. Most relevant to this paper are Stinebrickner and Stinebrickner, 2014 and Arcidiacono, Hotz, Maurel, and Romano, 2020, who model information frictions during college.[19] In contrast, we model information frictions *before* college, and show that in a dynamic setting they can affect later high-stake outcomes such as college enrollment and college preparedness, even when they are short-lived.[20] The model provides entirely novel estimates of the incentive effects of admission advantages as perceived by the high school students they target, and of the likely impacts of informational interventions in schools on the college preparedness of college entrants under large admission advantages.[21]

2.3 Context, Randomization and Data

2.3.1 Context and PACE Policy

In this section we describe the context and policy as they were for our sample.

model. The impacts on pre-college effort in our study, therefore, are not driven by model assumptions but by experimental findings.

[19]Other relevant papers include Bobba and Frisancho, 2019, who use belief and outcome data to estimate a model of the transition from middle to high school; Kapor, Neilson, and Zimmerman, 2020, who use data on beliefs and actual outcomes to estimate a static equilibrium model of school choice in a centralized school admission system; and d'Haultfoeuille, Gaillac, and Maurel, 2021, who develop a test for rational expectations that can be applied to data on perceived and actual outcomes that cannot be matched. Using data on choices and beliefs over the consequences of such choices, Giustinelli, 2016 estimates a model of parent-child choice of high school. Using data on beliefs and expected future choices (but not on actual outcomes), Klaauw, 2012 and Delavande and Zafar, 2019 develop and estimate dynamic structural models of teacher careers and of university choice that do not impose rational expectations. See also Arcidiacono, Aucejo, Maurel, and Ransom, 2016, who estimate (without using belief data) a dynamic structural model of schooling and work decisions where individuals have imperfect information about their schooling ability and labor market productivity.

[20]Boneva and Rauh, 2020 collected survey data in British high schools and showed that first-generation students perceive lower returns to college than those with parents who attended college.

[21]Mounting evidence shows that providing information about absolute and relative ability can successfully and cheaply correct belief errors and choices (Azmat, Bagues, Cabrales, and Iriberri, 2019; Bobba and Frisancho, 2019; Hakimov, Schmacker, and Terrier, 2022). Therefore, the simulations correcting those beliefs can be interpreted as an approximation to the likely effects of best-case informational interventions. See also Hastings, Neilson, and Zimmerman, 2015, who find that a cheap intervention providing information about wages of graduates from different majors in Chile changed students' major choice.

Definition of *selective college*. With selective college we refer to a college that participates in the centralized admission system (*Sistema Único de Admisión*), not to a college that has high admission requirements, which is the meaning attributed to selective college in other countries such as the United States. We refer to these colleges as selective colleges or, simply, colleges. To distinguish them from the colleges that do not participate in the centralized admission system, we use the term non-selective college to refer to the latter.[22]

Regular channel admissions. Students wishing to go to a selective college must take the PSU (*Prueba de Selección Universitaria*) standardized college admission exam. After observing their scores, they decide whether to submit an application to the system. Higher scores increase the likelihood of admission.

PACE. In line with global statistics, college enrollment in Chile is unequal across socioeconomic lines. Students from families in the top income quintile are over three times more likely to enroll than students from families in the bottom income quintile (Figure B.7). PACE was introduced to increase college admissions among disadvantaged students. The government selected the schools to be targeted by PACE using the school-level vulnerability index (*Indice de Vulnerabilidad Escolar*), based on students' socioeconomic characteristics, to indentify schools serving underprivileged students.

Students in high schools participating in PACE can apply to a selective college through the regular channel, like any other student in the country. Moreover, they receive a guaranteed admission to a selective college, that can be used only in the year immediately after graduating from high school, if they satisfy three conditions. First, the grade point average in grades 9 to 12 must be in the top 15% of the high-school cohort.[23] Second, like in the Texas and California percent plans (Horn and Flores, 2003), the student must take the entrance exam, even though the score does not affect the likelihood of obtaining a PACE admission. When students decide whether to take the exam, they have not yet been told whether they have graduated in the top 15% of their school. Third, the student must attend the PACE high school continuously for the last two high-school years (eleventh

[22]Selective colleges offer five-year (and longer) programs. They include the 23 public and private not-for-profit colleges that are part of the Council of Rectors of Chilean Universities (CRUCH) and 14 additional private colleges. Higher-education institutions outside this system do not have minimum admission requirements, and most provide vocational and shorter degrees.

[23]The central testing authority computes the score used to rank students, called *Puntaje Ranking de Notas* (PRN), by adjusting the raw four-year grade point average to account for the school context. The Pearson's correlation coefficient between the unadjusted four-year grade point average and the PRN is 97.44%. Details of how the score is calculated can be found at: https://demre.cl/psu//proceso-admision/factores-seleccion/puntaje-ranking.

and twelfth grade), and participate in light-touch orientation classes (two hours per month on average) that are offered to all students in PACE high schools.[24]

Other features of PACE include the following. i) Unlike the percent plans in Texas and California (see Table 1 in Horn and Flores, 2015), there are no coursework requirements in addition to graduating in the top 15%. ii) Optional tutoring sessions in college are available to those who enroll via PACE. iii) PACE college seats are supernumerary: they do not replace regular seats but are offered in addition to them. Therefore, the introduction of PACE did not make it mechanically harder to obtain regular admission. iv) PACE seats span the same majors as regular seats and are of similar quality, as measured by the average entrance exam score of regular entrants into each college-major pair (Figure B.2). v) A student can obtain both a PACE and a regular admission. vi) If a student does not accept a PACE admission, that PACE seat remains vacant.

2.3.2 Randomization and Balancing Tests

Randomization. The government introduced the PACE program in 69 disadvantaged high schools in 2014 and later expanded it to more schools. In 2015, it identified 221 high schools that were not yet PACE schools, but that met the eligibility criteria for entering PACE in 2016, per students' socioeconomic status. Using a randomization code written by PNUD Chile (United Nations Development Program), it randomly selected 64 of the 221 eligible schools to receive the PACE treatment. The randomization was unstratified.

When a school first enters PACE, only the cohort of eleventh graders is entered into the program. The randomized expansion concerned the cohort who started eleventh grade in March 2016. Before starting the school year, students who were enrolled in schools randomly selected to be treated were informed their school was in the PACE program. This announcement was made after the school enrollment deadline; thus, we did not observe strategic selection into high schools (see footnote 12). The control schools were not entered into the PACE program; they were not promised participation. Figure B.1 illustrates the timeline. Grades in the first two high-school years (9 and 10) were already determined when students in treated schools were informed they were in a PACE school. But students who wished to affect their four-year GPA average had two school years to do so.

Sample and balancing tests. We collected data on the experimental cohort. We sampled all the 64 schools randomly allocated to treatment. For budget reasons, we randomly

[24]The Texas top ten percent plan shares this feature. The PACE orientation classes cover the college application process and study techniques and often replace orientation classes already offered by the schools (MinEduc, 2018).

selected 64 of the 157 schools randomly allocated to control. Table 2.1 presents the balancing tests for the 128 sampled schools using background information collected when the cohort was in the tenth grade. The students in treated and control schools did not differ significantly at baseline on gender, age, socioeconomic status (SES), academic performance or type of high school track attended (academic or vocational). Given the low SES, nearly all students in the sample, across treatment groups, were eligible for a full tuition fee waiver.

TABLE 2.1: SAMPLE BALANCE ACROSS TREATMENT AND CONTROL GROUPS

	Control (1)	Difference between Treatment and Control (2)	p-Value (Difference equals zero) (3)	N (4)
Female	0.476	0.001	0.988	9,006
		(0.054)		
Age (years)	17.541	0.031	0.561	9,006
		(0.052)		
Very-low-SES student	0.602	0.014	0.489	9,006
		(0.020)		
Mother's education (years)	9.553	0.081	0.631	6,000
		(0.168)		
Father's education (years)	9.320	0.115	0.517	5,722
		(0.178)		
Family income (1,000 CLP)	283.950	14.335	0.265	6,018
		(12.794)		
SIMCE score (points)	221.355	7.600	0.151	8,944
		(5.256)		
Never failed a year	0.970	-0.010	0.101	8,944
		(0.006)		
Santiago resident	0.140	0.051	0.482	9,006
		(0.073)		
Academic high-school track	0.229	0.055	0.451	9,006
		(0.073)		

Notes: Standard errors clustered at the school level are shown in parentheses. Very-low-SES student is a student that the government classified as very socioeconomically vulnerable (*Prioritario*). SIMCE is a standardized achievement test taken in 10^{th} grade.

2.3.3 Data Construction

Table 2.2 lists the administrative and primary data sources. We linked them through unique student, classroom and school identifiers and built a longitudinal dataset that follows $9,006$ students for nine years, from ninth grade to five years after leaving high school.

For all $9,006$ students enrolled in the 128 sampled schools, we obtained administrative information on baseline socioeconomic characteristics, baseline standardized test scores, school grades in high school (years 9 to 12), grade progression, college entrance exam scores, regular and PACE channel admissions, enrollments and persistence or graduation

TABLE 2.2: OVERVIEW OF DATA

DATASET	VARIABLES	COLLECTED	SOURCE
1. *SIMCE*	Achievement test scores, background characteristics	Grade 10	Admin
2. *SEP*	Very-low-SES classification (*Prioritario* student)	Grade 10	Admin
3. School records 1	High-school enrollment	Grades 9-12	Admin
4. Student survey	Study effort, beliefs about self and others	Grade 12	Primary
5. Teacher survey	Effort and focus of instruction of Mathematics and language teachers	Grade 12	Primary
6. School-principal survey	Support classes, assessment methods, classroom formation	Grade 12	Primary
7. Achievement	Achievement test scores	Grade 12	Primary
8. School records 2	GPA (overall and by subject), grade progression	Grades 9-12	Admin
9. Higher education records	Entrance exam (PSU) scores, applications, admissions, enrollments and graduation or persistence at five years in selective colleges via regular channel, seat selectivity, enrollments and graduation or persistence at five years in vocational higher-education institutions and non-selective colleges	Years 1-5 after high school graduation	Admin
10. PACE program records	Allocation of PACE seats in selective colleges, applications, admissions, enrollments and graduation or persistence via PACE channel, seat selectivity	Years 1-5 after high school graduation	Admin

Notes: SIMCE: *Sistema Nacional de Evaluación de Resultados de Aprendizaje*, SEP: *Subvención Escolar Preferencial*.

up to five years after high school graduation. To gain insights on outside options, we collected administrative data on enrollments and persistence or graduation up to five years after leaving high school in all higher education programs outside of selective colleges. These are vocational programs (typically lasting up to two years), and academic programs in mostly private non-selective colleges, which do not participate in the centralized admission system.[25]

To complement the administrative data, we collected primary data in all 128 sampled schools between September and November 2017, when students were completing 12^{th} grade (Appendix B.1 describes the fieldwork). Our primary data contain four main pieces of information. First, we measured pre-college achievement. As standardized achievement tests are not administered universally at the end of high school, we administered a

[25] See Kapor, Karnani, and Neilson, 2022 for a description of these off-platform options.

20-minute mathematics achievement test to all students (see Behrman et al., 2015 for a similar approach), developed for us by professional testing agencies. Without this skill measure, it would be difficult to estimate policy impacts on pre-college achievement: using the scores on the entrance exam could introduce selective attrition bias, because the decision to take the exam could be affected by the policy, and using GPA could give results that are hard to interpret, because GPA is not comparable across schools. Second, we elicited study effort through the survey instruments used in Mexican high schools by Behrman et al., 2015 and Todd and Wolpin, 2018, complemented with questions on entrance exam preparation. Third, we elicited subjective beliefs about future outcomes (i.e., college graduation and wages) and returns to effort (i.e., the productivity of effort for entrance exam scores and GPA). Finally, we surveyed mathematics and Spanish teachers, and school principals, to obtain information on the policy response of schools.

We surveyed $6,094$ students, approximately 70% of those enrolled in the 128 sample schools. Attrition was not selective across the treatment and control groups (Appendix B.3). Our response rate compares favorably with that of ministerial surveys (MinEduc, 2015, 2017), and it reflects dropout in the last weeks of the last high-school year (schooling is compulsory until then). We account for survey attrition in two ways. For the regression analyses, we built inverse probability weights using baseline administrative data. For the estimation of the structural model, we let the distribution of unobservable characteristics depend on whether a student was surveyed, to allow for survey-non-response based on unobservables.

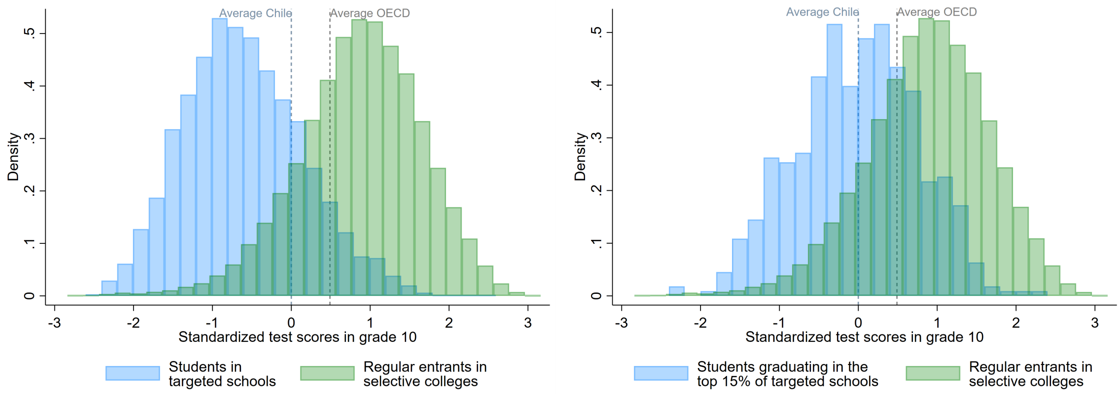
2.3.4 Descriptive Analysis

We now describe the disadvantaged students targeted by PACE, and their higher education choices absent preferential admissions.

Fact 1: Students targeted by PACE score substantially worse on high school standardized tests than regular entrants in selective colleges, and come from poorer households. Figure 2.1 shows the distribution of standardized tests scores in 10^{th} grade among students targeted by PACE and among regular college entrants, standardized in the population of 10^{th} graders. Students in targeted schools score 1.47 standard deviations below regular entrants on average. Their median score corresponds to the fourth percentile of scores among regular entrants. Even those who graduate in the top 15% of targeted schools score substantially worse than regular college entrants. They score 0.88 standard deviations below regular entrants on average. Their median score corresponds to the fourteenth percentile of scores among regular entrants. For reference, we draw the average high school standardized test scores in OECD countries: the majority of targeted students score below the OECD mean, the majority of regular entrants score above the OECD mean.

Table B.2 shows that students in targeted schools are substantially more disadvantaged than the average Chilean student along several dimensions of socioeconomic status, for example, their family income is half that of the average Chilean student. Family income in this group is 53% of the median household income in Chile (54% if focusing on students graduating in the top 15% of targeted schools), and 31% of the family income of regular entrants (32% if focusing on top-graduating students), whose average family income of CLP 904,354 per month is 70% above the median Chilean income.

FIGURE 2.1: Distributions of standardized test scores in 10^{th} grade.



Notes: Test scores are standardized in the population of 10^{th} grade students in 2015. Targeted students are from the control schools. Every bar represents 0.20 standard deviations. The average score in the OECD is calculated using PISA scores, re-scaled to be comparable to the SIMCE scores. Details of the re-scaling can be found in Appendix Section B.7.

For comparison, the students in California around the Eligibility in the Local Context (ELC) preferential admission cutoff have family incomes that are 90% of the median Californian income (Bleemer, 2021, Table 1). High school standardized test scores of these students are not reported, but their entrance exam (SAT) scores are above the average score among all college applicants (Bleemer, 2021, Table 1), which is a positively selected population.[26] Of the students targeted by the Texas Top Ten (TTT) policy, $22-23\%$ are

[26] The most likely ELC compliers were near- or above-threshold students from schools with below-median SAT scores. Within this group, incomes of near-threshold students were around 6.5% above the Californian median income as per Table 3 in Bleemer, 2021. Regarding SAT scores, students near the ELC cutoff score 137 points above the average Californian applicant. Among students near the eligibility cut-off from below-median SAT score schools, SAT scores were 158 SAT points below the average applicant (Bleemer, 2021, Table 3). Results for the SAT in standard deviations are not reported.

eligible for free or reduced school meals (Black, Denning, and Rothstein, 2023, Table 1). In contrast, 61% of the students targeted by PACE are eligible for welfare programs due to their extremely vulnerable socioeconomic circumstances (*Alumno Prioritario*). The students in all schools targeted by the TTT are representative of all SAT test takers in Texas, and those induced to enroll in a more selective college by the policy have entrance exam scores corresponding to the 89^{th} statewide percentile. Therefore, the targeted students score favourably within the already positively selected population of Texan SAT test takers. It should be clear from these statistics that PACE targets a substantially more disadvantaged population than the two most well-known context-based programs in the United States.

Fact 2: Only few students targeted by PACE attend college absent the policy. The most common outside option is not enrolling in any higher-education institution. Among those who attend higher education, the most common outside option is attending vocational programs, followed by attending non-selective (off-platform) colleges. Table 2.3 describes the educational choices of the typical students targeted by PACE absent PACE, i.e., the choices of students in the control group. Around two thirds of students take the college entrance exam (first row of Table 2.3), which aligns nicely with our survey data, where 63% report preparing for it. Even students with very low admission likelihoods prepare for and take the entrance exam (Figure B.4). But, as the second row of the table shows, exam scores are well below the national average (-0.6 standard deviations). Upon observing their exam scores only 21.0% apply to college (third row). 11.4% of students are admitted and 8.5% enroll in college the first year after high school graduation.[27] Among students who graduate in the top 15% of schools targeted by PACE (Panel B in the Table), 90% take the entrance exam; their scores are 0.21 standard deviations below the average test taker's. Upon observing their score, just over half of those who took the exam apply to selective colleges. A minority of high school students graduating in the top 15%, 28.7%, enroll in college the first year after high school graduation. We construct a variable capturing continuous enrollment or graduation in a selective college five years since first enrolling, which is necessary for on-time graduation.[28] Panel A of Table 2.3 shows that 58 percent (i.e., $\frac{0.049}{0.085}$) of the students who enroll in the first year are still continuously enrolled or have graduated after five years. Panel

[27]For the students in this study the PACE slot could only be used in the year immediately after high school graduation. Therefore, we do not examine PACE impacts on later first-year enrollments.

[28]Theoretically, it could be possible for a student to take a one-year gap from a selective college, re-enroll again, and graduate in time. But this is highly unlikely in practice. Graduation refers to graduation in or before 2021 (the fourth year) as graduation data for 2022 (the fifth year) is not yet available. The Ministry will make it available during 2023.

B shows that this figure is similar ($\frac{0.182}{0.287} = 63$ percent) in the sample of students who graduate in the top 15% of their school.

TABLE 2.3: DESCRIPTION OF CHOICES AND OUTCOMES IN THE CONTROL GROUP

	Mean (1)	Std. Deviation (2)	N (3)
A. ALL STUDENTS			
Took college entrance exam	0.655	0.475	4,231
College entrance exam score | took exam	-0.602	0.611	2,773
Applied to college	0.210	0.407	4,231
Admitted to college	0.114	0.318	4,231
Enrolled in college	0.085	0.279	4,231
Still enrolled or graduated from college five years later	0.049	0.217	4,231
Enrolled in vocational institution	0.270	0.444	4,231
Enrolled in non-selective (off-platform) college	0.061	0.238	4,231
B. STUDENTS GRADUATING IN TOP 15%			
Took college entrance exam	0.901	0.299	628
College entrance exam score | took exam	-0.214	0.641	566
Applied to college	0.481	0.500	628
Admitted to college	0.364	0.481	628
Enrolled in college	0.287	0.453	628
Still enrolled or graduated from college five years later	0.182	0.386	628
Enrolled in vocational institution	0.250	0.433	628
Enrolled in non-selective (off-platform) college	0.118	0.323	628

Notes: Sample of students enrolled in the 64 control schools. The standardized test scores in 10^{th} grade is measured in standard deviations of test scores in the population of 10^{th} graders. The college entrance exam score is designed to have mean 500 and standard deviation 110 among all exam takers, we report the standardized score. A student is coded as being enrolled or having graduated in the 5^{th} college year if he/she enrolled in the first year and stayed continuously enrolled every year up until and including year 5, or if he/she enrolled in the first year and graduated in a year prior to year 5.

Panel A shows that, absent the policy, 27% of students in targeted schools enroll in vocational higher education programs, 6.1% in non-selective colleges, and 58.4% do not enroll in higher education. Panel B shows that, absent the policy, among the top performing students in targeted schools, 25.0% enroll in vocational higher education programs, 11.8% in non-selective colleges, and 34.5% do not enroll in higher education.[29]

[29]For comparison, 88.9% of the students around the ELC cutoff in California attended college, 3.9% community college, and only 7.2% did not enroll (Bleemer, 2021 Table 1). Of the students in schools targeted by the TTT in Texas, absent TTT 25% enrolled in a 4-year college, 32% in a community college, and the remaining 43% did not enroll in college in Texas (Black, Denning, and Rothstein, 2023 Table 1). Among TTT compliers, absent TTT 49% enrolled in a 4-year college, 18% in a community college, and the remaining 35% did not enroll in college in Texas (Black, Denning, and Rothstein, 2023 Table 2). Therefore, the students targeted by PACE are less likely to enroll in college absent preferential admissions than those targeted by the two most well-known context-based programs in the United States.

2.4 Experimental Policy Evaluation

To identify the policy impacts, we exploit the randomized assignment of schools to PACE, and estimate the following linear regression model:

$$Y_{is} = \alpha + \beta T_s + \lambda X_i + \eta_{is}, \tag{2.1}$$

where Y_{is} is the outcome of student i in school s, T_s is the treatment status of school s, and X_i is a vector of student i's baseline characteristics. The parameter of interest is β. The standard errors are clustered at the school level.

2.4.1 Findings

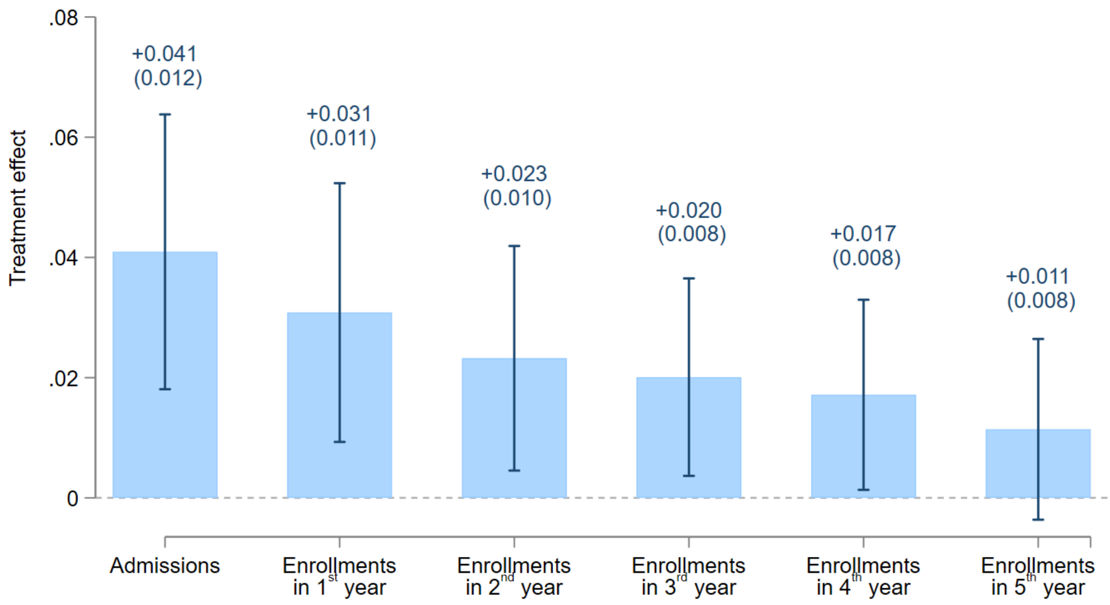
Experimental Finding 1: PACE increased college admissions and enrollments, but the enrollment effects decreased substantially over time. Figure 2.2 shows that students in schools randomly assigned to the treatment are 4.1 percentage points (p.p.) more likely to be admitted to college and 3.1 p.p more likely to enroll than students in control schools. These effects correspond to a 36% increase compared to admissions and enrollments in the control group. The enrollment effect tapers off over time. The effect on continuous enrollment in the fifth year or graduation by such time (which is an upper bound for the effect on on-time graduation) is 1.1 p.p. (p=0.140), corresponding to a 23% increase compared to the control group, and it is significantly different (p=0.001) from the treatment effect on first-year enrollments.

The enrollment effects are concentrated among students who, at baseline, were in the top 15% of their school according to GPA in grades 9 and 10; those in the bottom 85% experienced no change in their college enrollment (as shown in Tables B.6 and B.7). Among top-performing students, PACE increased college applications, admissions, and first-year enrollments, and the college enrollment impacts are still significant and positive in the fifth year since leaving high school, but significantly and substantially smaller than the impacts in the first year (Tables B.4 and B.6). Table B.6 also shows that PACE lowered the enrollment of top-performing students in the outside options (vocational institutes and non-selective colleges), and had no impacts on first-year enrollments in higher education overall, nor on continuous enrollment in or graduation from higher education after five years.

For comparison, the Texas Top ten policy increased by 5.3 percentage points the likelihood that top-performing students from schools that do not normally send their graduates to selective colleges (the most likely compliers) enroll in the selective UT Austin, and by 3.9 percentage points the likelihood that they graduate from UT Austin within 6 years

(Black, Denning, and Rothstein, 2023, Table 3). The effect after six years is 74% of the effect in the first year. In contrast, among top-performing PACE students, the treatment effect on continuous enrollment or graduation after five years is 45% of the enrollment effect in the first year.[30]

FIGURE 2.2: Effects of PACE on admissions and on enrollment or graduation of targeted students.



Notes: The Figure reports OLS estimates from the estimation of parameter β in equation (2.1). The controls are: gender, age, indicator for very-low-SES student, baseline SIMCE test score, never failed a grade, and high school track (academic or vocational). The standard errors clustered at school level are reported in parenthesis, and the 95% confidence intervals constructed from them are shown. The enrollment variables capture continuous enrollment or graduation: a student is coded as enrolled (or as having graduated) in the t^{th} college year if he/she enrolled in the first year and has been continuously enrolled every year up until and including year t, or if he/she enrolled in the first year and has graduated in a year prior to t.

Experimental Finding 2: PACE lowered study effort and achievement before college. Given the decreasing impacts of PACE on college enrollment (Experimental Finding 1), we examine whether PACE had any impacts on pre-college academic preparedness. Columns (1) and (2) of Table 2.4 present results on the pre-specified outcomes. Students

[30] We could not find as easily comparable statistics for the ELC program impacts in California, but 75 percent of those around the ELC admission cutoff graduated from selective colleges (Bleemer, 2021), while only 58 percent of students from the top 15 percent of PACE schools who entered college were still continuously enrolled or had graduated from college in the fifth year. This suggests that the ELC achieved more persistent impacts than PACE.

in treated schools perform 10% of a standard deviation worse than students in control schools on the standardized achievement test we administered. Column (2) shows that the treatment had a negative average effect on study effort of 9% of a standard deviation. The effect is driven by a reduction in study effort towards schoolwork inside and outside the classroom and in entrance exam preparation (Table B.8). Using administrative outcome data, columns (3) and (4) show that the policy had a negative effect on the grades in the subjects tested on the entrance exam, and no effect on the grades in the subjects not tested, suggesting students reduced their study effort towards PSU exam preparation and PSU exam subjects, without reallocating effort to other subjects. To understand whether this reduction could have contributed to the waning enrollment impacts, we examine whether these dimensions of pre-college human capital predict college persistence.

TABLE 2.4: EFFECT OF PACE ON PRE-COLLEGE ACHIEVEMENT

	Test Score	Study Effort	12^{th} grade GPA	
			Tested subjects	Untested subjects
	(1)	(2)	(3)	(4)
Treatment	-0.099^{**}	-0.088^{**}	-0.151^{*}	-0.006
	(0.050)	(0.038)	(0.087)	(0.129)
Observations	6,054	5,631	6,046	4,288
R^2	0.259	0.047	0.220	0.109

Notes: The coefficients are OLS estimates. Standard errors were clustered at the school level. The standard set of controls (see notes in Figure 2.2) and Inverse Probability Weights were used. Field-worker fixed effects were used for columns (1) and (2). *Treatment* is a dummy variable indicating whether a student is in a school randomly assigned to be in the PACE program. The outcome variable in column (1) is the number of correct answers on the achievement test, standardized. The outcome variable in column (2) is the standardized score predicted from the principal component analysis of the eight survey instruments reported in Table B.8 of the Appendix. The outcome variables in columns (3) and (4) are the GPA in subjects tested and not tested on the PSU exam, standardized. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

Fact 3: PACE lowered precisely the dimensions of pre-college human capital that predict persistence in college. Table B.3 shows that, after controlling for student demographics, GPA in the last high-school year strongly predicts continuous enrollment or graduation five years after entering a selective college, while the entrance exam score does not (column (1)). More specifically, GPA in core subjects like mathematics and language, which are tested on the entrance exam, is predictive of persistence, while GPA in subjects not tested on the entrance exam is not (column (2)). If GPA at the end of high school is produced by a combination of baseline ability and study effort during high school, the administrative measure of baseline ability and our survey measure of study effort should independently predict continuous enrollment or graduation five years after entering college. This is, indeed, what we find: both measures are predictive, even after conditioning on a rich vector of student characteristics that includes socioeconomic status, demographics, and high school type (columns (3) and (4)). Therefore, academic preparedness, especially competence in the core subjects, captures a combination of baseline ability and study effort in high school and appears important for college persistence in this population.

Validity of the survey-based Experimental Finding 2. While collecting measures of study effort and achievement was necessary because the administrative data lack standardized achievement and effort measures at the end of high school, and while such measures are the student-level outcomes that we specified in the pre-analysis plan, it is important to understand the validity of the results based on the survey measures. First, the negative impacts on the measures we collected and pre-specified are confirmed in the administrative GPA data, as shown. Our achievement test is on mathematics, which is a subject tested on the PSU entrance exam, for which the administrative data shows a negative impact as well.[31] Second, our measures have strong predictive validity: they can independently predict high-stake outcomes up until five years after the data collection, when our data end. For example, Table B.10 shows that, controlling for student characteristics and baseline test scores, a one standard deviation increase in the achievement test score is associated with an increase in the probability that a student is enrolled in the fifth year of college of 3.0 p.p. (p=0.000), or 50% of the sample mean. The study effort measure has equally strong predictive validity. Lastly, the results are robust to using item response theory to calculate the achievement score (Table B.17), and to using Lee, 2009 bounds (Appendix B.3).

2.4.2 Discussion

PACE increased the rate at which disadvantaged students are admitted to and enroll in college. But the impact on continuous college enrollment tapers off over time. This raises the question of whether large admission advantages like PACE may be leading students who lack college preparedness to enroll in college.

Much of the literature on admission advantages treats college preparedness as fixed (Arcidiacono, 2005; Arcidiacono, Lovenheim, and Zhu, 2015; Arcidiacono et al., 2011; Kapor, 2020). Yet, human capital is not a fixed trait, it can respond to the dynamic incentives introduced by the admission rules and to other changes occurring at the school level in response to these policies. Our Experimental Finding 2 establishes, for the first time through a randomized controlled trial, that preferential admission policies can causally

[31] As described in Appendix B.1 on fieldwork, the test questions were developed by professional testing agencies, and after extensive piloting we found that the best way to obtain a reliable measure was to introduce a reward linked to the performance on the test.

change pre-college effort and achievement. Our rich data allowed us to further identify that PACE had a negative impact precisely on the dimensions of pre-college human capital that predict persistence in college. A key question is why this occurred. Answering it is the essential first step to understand whether large admission advantages like PACE *can* generate more persistent impacts on college enrollment. The next section examines the mechanisms behind the reduction in college preparedness.

2.5 Mechanisms

In this section we show results on all the potential mechanisms behind the pre-college human capital response that we specified in the pre-analysis plan, and on an additional mechanism motivated by the finding that the impacts on effort are negative. The pre-specified analysis of mechanisms examines: i) students' response to incentives, analyzed by examining the heterogeneity of the effect on pre-college effort and score on the achievement test by baseline absolute and relative ability and by examining subjective beliefs; ii) teacher grading, analyzed by examining the relationship between grades and standardized measures of achievement across treatment groups, and whether the grading practices differ across treatment groups; iii) teachers' focus of instruction and effort, and school inputs and practices, analyzed using survey measures we collected for this purpose among teachers and principals.[32] The mechanism not pre-specified is a reduction in perceived monetary returns to college, which could have discouraged students from preparing for the entrance exam.

2.5.1 Students' Response to Incentives

Preferential admissions introduce new admission requirements based on pre-college achievement. Since achievement is not a fixed trait but rather an outcome that responds to study effort, the introduction of new requirements can induce an endogenous response in study effort if students value college admission. Did students respond to incentives?

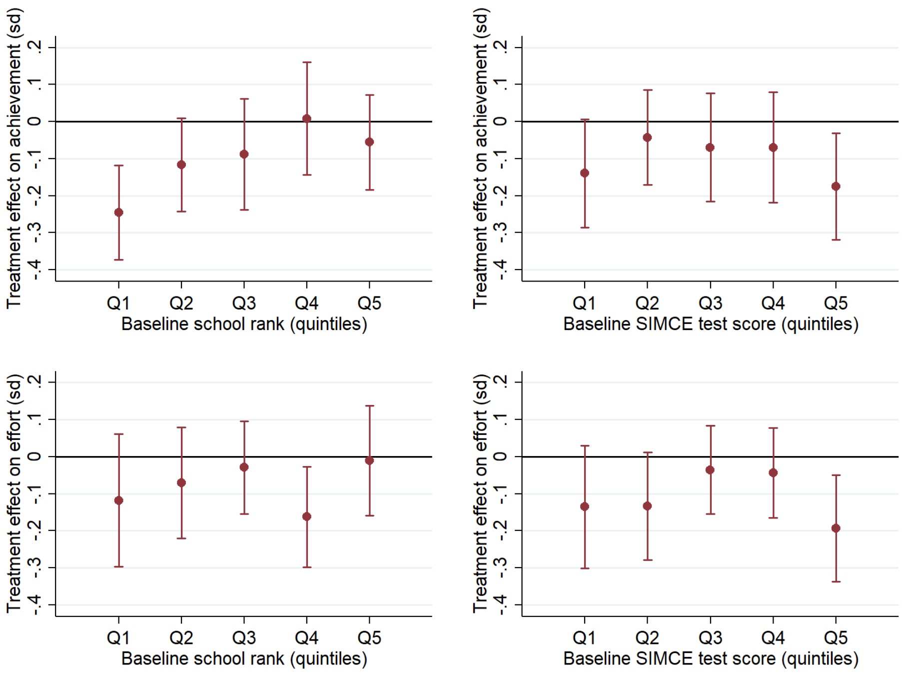
The negative average effects on pre-college effort and achievement are somewhat surprising throgh the lens of incentive response. Given that the students in our sample perform substantially below regular entrants and are admitted at low rates absent the policy (Facts 1 and 2), it is reasonable to expect that the policy brought a college admission within reach, *increasing* the returns to effort, rather than making an admission easier to

[32]We also pre-specified parental involvement in their child education, but for time-budget reasons we could only add two questions to the student questionnaire on parental help: whether the mother and the father help the student with their homework. The treatment had no impact on these variables.

obtain, *decreasing* the returns to effort. Through the lens of incentive response, therefore, the negative average impacts are surprising.

Experimental Finding 3: The negative effects on pre-college effort and achievement are spread across the absolute and relative (within-school) ability distributions. To better understand the effort response, we examine effect heterogeneity along baseline within-school rank and baseline ability. We split the sample into quintiles of baseline ability and baseline within-school rank, and estimate the regression from equation (2.1) on each sub-sample. The results are reported in Figure 2.3. We do not find evidence of encouragement effects on pre-college effort or achievement, anywhere along the baseline relative and absolute ability distributions, and we find the negative impacts are spread across baseline relative and absolute ability.

FIGURE 2.3: Heterogeneity of policy effects on pre-college effort and achievement.



Notes: Each dot is the coefficient on *Treatment* from an OLS regression where: *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program, the controls are the standard set of controls (see Figure 2.2), Inverse Probability Weights and field-worker fixed effects are used, the estimation samples are quintiles in the within-school rank based on 10^{th} grade GPA (left panel) and quintiles in the distribution of 10^{th} grade standardized test scores (right panel). The units of measurement of the treatment effects are standard deviations. The bars are 95% confidence intervals built using standard errors clustered at the school level.

Such patterns of effect heterogeneity are hard to rationalize as a response to incentives under rational expectations. As shown in Bodoh-Creed and Hickman, 2018, when students rationally respond to the incentives embedded in percent rules like PACE, we would expect negative impacts to be concentrated among students near regular admission cutoffs (high absolute ability in our sample of disadvantaged students) and well above the preferential admission cutoff (high relative ability within the school). For this group of students the policy lowered returns to effort by making guaranteed an admission that was previously only within reach under sustained effort. Conversely, we would expect positive impacts among students far from the regular admission cutoff (medium and low absolute ability in our sample) and near the top 15% within-school cutoff. For this group of students the policy increased returns to effort by bringing within reach an admission that was previously unattainable. But these are not the patterns we find.

A potential reason for not finding effects expected under rational expectations is that beliefs about own absolute and relative ability are systematically biased. Therefore, we examine students' beliefs next.

Fact 4: Students' beliefs about their absolute and relative ability are biased. Table 2.5 shows that students display large over-optimism over their PSU entrance exam score (first two lines), on average expecting a score that is 0.6 standard deviations above the score they actually obtain. Students also display large over-optimism about their within-school rank, with over 40% believing that their GPA is in the top 15%. Such relative-rank bias is due to misperceptions about others: students hold accurate beliefs about their own GPA (GPA is measured on a scale from 1 to 7 and on average the GPA students expect differs from the one they obtain by less than 0.1 GPA points), but, as they are never given relative feedback, they have a small belief bias about the 85^{th} GPA percentile in their school, of less than half GPA point (fourth row of the Table). The small belief bias in absolute terms translates into a large belief bias in relative terms because of strong grade compression, that we document in Figures B.8 and B.9.[33]

Examining belief heterogeneity, Figure B.5 shows that students of all (absolute and relative) ability levels are over-optimistic; table B.9 shows that belief biases do not vary systematically by socioeconomic background in our homogeneously disadvantaged sample. The findings align with existing evidence that over-optimism is widespread in many

[33]To document grade compression, first, we show that while grades can range from 1 to 7, effectively the vast majority of the grades are between 5 and 6.5. Second, we link grade data to baseline and endline standardized achievement measures, and show that grades do not discriminate substantially among students of different abilities, and much less than the endline standardized achievement measure does. See Figures B.8 and B.9.

TABLE 2.5: DESCRIPTION OF SUBJECTIVE BELIEFS

	Mean (1)	Std. Deviation (2)	N (3)
Believed entrance exam score (σ)	-0.033	0.920	2,413
Believed minus actual entrance exam score $\mid$ took exam (σ)	0.591	0.916	1,853
Believes regular admission probability ≥ 0.50	0.840	0.367	2,798
Believed minus actual 12^{th} grade GPA (GPA points)	-0.075	0.552	2,558
Actual minus believed top 15% cutoff in school (GPA points)	0.401	0.854	3,326
Believes is in top 15% of school	0.431	0.495	2,469

Notes: Sample of students enrolled in the 64 control schools. This table is based on linked survey-administrative data: we elicited students' beliefs and linked their survey answers to actual outcomes. σ is the standard deviation of PSU entrance exam scores among the population of exam-takers. GPA is a number between 1.0 and 7.0. We define a student as believing she is in the top 15% of her school if her believed GPA is above her believed top 15% cutoff. Appendix Figure B.3 contains an English translation of the survey instruments we used to elicit the beliefs reported in this Table.

contexts, including education (Hakimov, Schmacker, and Terrier, 2022; Stinebrickner and Stinebrickner, 2014).[34]

Remark: Fact 4 and Experimental Findings 2 and 3 are consistent with a response to perceived incentives. As we do not have baseline belief data, we cannot estimate how the effort effects varied by baseline beliefs. However, the belief biases we documented help rationalize the effort response as a rational response to incentives, given biased beliefs: students on average believe they are high ability and high rank, which is the student type for whom we would expect effort reductions under rational expectations. To see why, note that optimism about the entrance exam could lead students to perceive a regular admission as within reach, and study for the entrance exam absent the policy, something most students in the control sample do (Table 2.3). The optimism about the within-school rank could lead students to perceive a preferential admission as guaranteed, and reduce effort when the policy is introduced (Experimental Finding 2). The belief data, therefore, appear consistent with students choosing effort based on perceived incentives. Additional suggestive evidence points to this channel: the negative effect on pre-college achievement is driven by students whose baseline GPA is well above the *perceived* cutoff (Figure B.6), suggesting the negative impacts on pre-college investments were driven by those who perceived a preferential admission as guaranteed.

Why would students interested in college education lower their pre-college study effort, if it matters for persistence in college? One possible explanation is that they do not perceive pre-college effort as important for persistence in college. We have elicited the

[34]We have also collected beliefs about returns to effort, which we describe in section 2.6.3. As actual returns to effort are not directly observed in the data, we do not include them in this section, which describes errors in beliefs.

perceived likelihood of college graduation conditional on enrolling.[35] We find that half of the students are certain they will graduate if admitted, and three quarters believe they have a more than 50% chance of graduating. Crucially, PACE had no impact on this belief, despite its large negative impact on pre-college effort. Since this question was asked when the effort reductions had already occurred, this finding suggests that students do not believe that pre-college effort matters for college persistence.

Remark: likely reasons for the biases in beliefs. The large belief biases about the entrance exam are consistent with the sporadic exam preparation that takes place in these schools, and the limited knowledge of college in the students' families (over 90 percent have parents who did not study beyond secondary education).[36] The large belief biases about school rank, which are relevant for percentile-based plans like PACE (a central form of context-based admissions), could be typical of these schools too. Even though students have correct beliefs about their own GPA and only a small belief bias about the school cutoff in *absolute* terms, such small belief bias translates into a large belief bias about *relative* rank within the school because grades are compressed (as we documented). With compressed grades, biased beliefs about rank are likely whenever relative rank feedback is not provided. While this is a new finding in the literature on admission policies, grades that do not discriminate much between students could be common in schools where academic standing is not particularly salient, such as those that do not normally send students to college.

Virtually nothing is known about the beliefs of high school students targeted by admission policies in other contexts. While more research is required to establish how common belief biases are in this group, the kinds of information frictions we documented may be common among very disadvantaged schools that do not normally lead to college. Therefore, policymakers wanting to introduce large admission advantages should reckon with the reality of the school environments such policies would encounter.

Predictive validity of the belief measures. For the findings depending on belief data to be reliable, it is important that beliefs capture something relevant about choice. We examine their predictive validity in Table B.11, using high-stake outcomes collected up to five years after the data collection, when our data end. Our belief measures correlate with

[35]The question can be translated into English as: *"If I get admitted to a selective college (not a technical institute), I will complete my studies".* The answers are on a 5-point Likert scale, from "Totally sure that I will not" to "Totally sure that I will".

[36]The fact that entrance exam preparation is sporadic was further confirmed to us in several focus groups recently implemented in PACE schools for a different project.

high-stake outcomes as we would expect them to do if they were capturing what they are designed to capture, as the following results show:

1. *The belief over the entrance exam score independently predicts all college-going outcomes, from entrance-taking to persistence in college.* Controlling for baseline characteristics and test scores, an increase in the believed entrance exam score of one standard deviation of the score distribution increases the probability that a students takes the entrance exam, applies to college, and is enrolled in college five years later. The associations are strong, for example, college enrollment five years later is increased by 4.1 p.p. (p=0.000), or 59% of the sample mean (Panel A column (7) of Table B.11).[37] The believed PSU score remains a strong predictor of enrollment and persistence in college even when adding the actual PSU score as a control (Panel B), suggesting that optimism over the score correlates positively with unobserved preference for college, unobserved ability, or both.[38]

2. *The belief over the within-school GPA rank independently predicts college-going outcomes in the treatment group (where rank matters greatly for admission) but not in the control group (where it does not), as expected if students based their college investment decisions on the perceived admission likelihood and if our survey recovered credible measures of beliefs.* In the control group, we do not expect beliefs around the within-school GPA rank to affect whether students take the entrance exam (and later apply, enroll and persist in college), because the rank is not an important determinant of the admission likelihood. Panel C of Table B.11 shows that this is indeed what we find. But in the treatment group, within-school rank affects a student's admission likelihood, therefore, we expect the belief over the rank to predict such outcomes. Panel D of Table B.11 shows that this is indeed what we find. For example, an increase in the perceived lead over the cutoff by one GPA-point, controlling for baseline characteristics and ability, is not associated with persistence in college five years later in the control group (0.4 p.p., p=0.436), but it is strongly associated with it in the treatment group (3.8 p.p., p=0.000, corresponding to 36% of the sample mean). Therefore, the survey measure of belief about relative ability correlates with high-stake outcomes as we would expect it to if it was an accurate measure.

[37] The predictive validity of the belief over the entrance exam score examined in Table B.11 uses the sample of control students, but the conclusions are the same using the sample of treated students.

[38] In this sample of test-takers, we eliminate the causal link between beliefs about the entrance exam and likelihood to take the exam, therefore, predictions within this sample are entirely correlational.

We interpret the predictive validity results as follows. First, subjective beliefs are important in choice. Second, our survey recovered credible measures of these beliefs. Third, subjective beliefs likely correlate with unobserved determinants of college going, such as preferences and unmeasured ability, therefore, the structural model should take such correlation into account.

2.5.2 Changes in Teachers' Behaviors and School Practices

Teacher Grading. Teachers can decide who obtains a preferential seat through their grading. If in response to the percent plan policy they manipulate their grading in a way that weakens the link between achievement and GPA, students in treated schools would have a lower incentive to study to improve their grades. This could explain the negative impacts on effort.

The evidence does not support this mechanism. As shown, pre-college effort reductions resulted in grade reductions (Table 2.4). Accordingly, the mapping between standardized achievement and grades does not differ between treated and control schools (Table B.19), suggesting that grading did not respond to the treatment. Consistent with this result, school principals report similar grading practices across treatment groups (Table B.20).

Teacher Effort and Focus of Instruction. Teachers could change their focus of instruction (i.e., what portion of the ability distribution they target with their teaching), or they could change effort (class preparation hours and absence days) as an effect of percent plans like PACE. Appendix B.5.1 describes how we measured these teacher behaviors, and Table B.12 shows that there is no evidence that such behaviors responded to the policy.

Schools. The curriculum is not a possible margin of policy response because the Ministry of Education mandates it. But school principals in treated schools may decide to offer fewer support classes, especially in regards to entrance exam preparation, as performing well on the exam is less critical for an admission. This, in turn, could directly lower students' pre-college achievement, especially in the subjects tested on the exam.

Using our survey of school principals, we find that treated schools do not differ from control schools regarding the support offered to students (PSU entrance exam preparation support or remedial classes), as shown in Table B.20.

Principals may also choose to change the assignment of students to classrooms. We asked them a set of questions on classroom formation, and found no effects, as shown in B.21.

2.5.3 Reduction in Perceived Returns to College

If the light-touch orientation classes offered to PACE students negatively affected students' beliefs about the net returns to college, they could have generated the negative response of pre-college study effort. In the Chilean setting, Hastings, Neilson, and Zimmerman, 2015 found that providing information about graduate earnings can change students' college choices. Therefore, even though the orientation classes were not designed to provide information about returns to college, this is an important channel to consider.

We elicited beliefs about the monetary returns to a college degree at age 30, and about students' awareness of tuition costs. We find that the policy had no impact on students' beliefs about the monetary returns to college (Appendix B.5.2), which are large at 200% of age 30 earnings, or their awareness of financial aid (83.6% of surveyed students are aware they are eligible for a tuition fee waiver, and there is no statistically significant difference between the treatment and control groups (p=0.618)). Therefore, the treatment did not affect students' perceived net returns to college.[39]

2.6 A Dynamic Model of Education Choices

2.6.1 From Experimental Evidence to a Model

The reduced-form results demonstrated the importance of college preparedness in the context of large admission advantages. Even students who perform at the top of their school score substantially below regular college entrants on high school standardized tests. While PACE achieved persistent impacts on their college attainment, the impacts waned substantially and significantly over time.

The results, however, suggest that college preparedness is not fixed. It is elastic to investments made in the last high-school years, and such investments respond endogenously to the admission rules. This suggests that a promising area for intervention to improve the persistence of large admission advantages is to intervene in targeted high schools to improve the college preparedness of college entrants. As such interventions have not been implemented yet, we develop a structural model that allows us to simulate them.

[39]The perceived returns we measured are similar to those measured among other samples of Chilean students of the same age (Hastings, Neilson, Ramirez, and Zimmerman, 2016).

For the model to be useful it must successfully explain the experimental findings, and deliver the college preparedness of college entrants as an endogenous outcome. To achieve this, we develop a dynamic model of pre-college effort, pre-college achievement, entrance-exam taking, admissions and enrollments that builds upon the reduced-form evidence. We model both a context without admission preferences and one with, and are able to succesfully replicate the experimental findings. The model delivers endogenously the distribution of college seats and the pre-college effort and baseline ability of those who self-select into college.

Informed by the belief data, we do not impose rational expectations but assume that high school students form beliefs about the returns to effort in securing an admission, and choose effort so as to maximize subjective value functions. Based on the admission credentials accumulated by students at the end of high school, admissions are realized according to objective admission likelihoods. Given the admission sets, students choose enrollments. Therefore, the choices students make in high school affect the allocation of college seats and the college preparedness of college entrants. Shaping those choices through strategically designed school interventions can affect the college preparedness of college entrants under large admission advantages.

The survey data highlighted large belief biases about absolute and relative ability. The first intervention we consider, therefore, eliminates such belief errors. The survey data also suggested that students do not perceive pre-college effort as important for college success. The second intervention we consider, therefore, communicates to students the importance of pre-college effort for college persistence.

2.6.2 Model

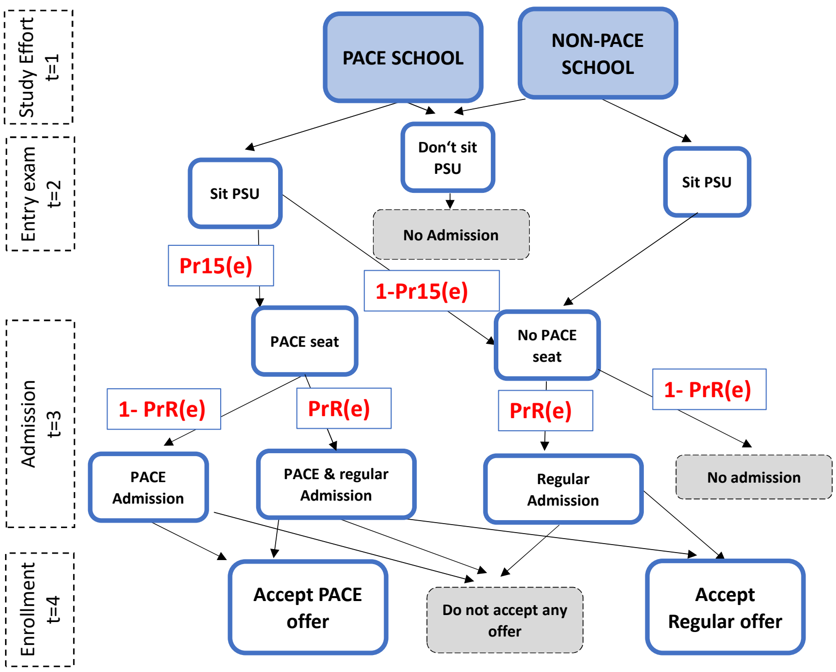
Observed and unobserved heterogeneity. Each student i is characterized by vectors x_i and y_{it-1} of baseline characteristics and baseline achievement measures, respectively, and by $k_i \in \{1, 2, ..., K\}$, a time-constant type unobserved by the econometrician but observed by the student (Heckman and Singer, 1984; Keane and Wolpin, 1994, 1997).[40] The number of types, K, is known to the econometrician. We let parameters that govern the preference for college, achievement and subjective beliefs depend on a student's type, to capture potential correlation between ability, preferences and beliefs that is not explained by observables. Not allowing for such correlation could lead to biased parameter estimates that mischaracterize the role of beliefs in choice (Bobba and Frisancho, 2019;

[40] Vector x_i, measured in 10^{th} grade, includes age, gender, dummy for whether the government classified the student as low-SES, dummy for whether the student repeated a year and dummy for high-school track (vocational or academic). Vector y_{it-1} comprises a standardized test score in 10^{th} grade (SIMCE), GPA in 10^{th} grade and the average of 9^{th} and 10^{th} grade GPA.

Wiswall and Zafar, 2015). This modelling choice means that the model does not assume that the predictive validity of the belief measures we presented in section 2.5.1 is causal.

Timing. Figure 2.4 shows the model timeline. Before the first model period, students form beliefs about the top 15% cutoff in their high school and about how study effort maps into a GPA and an entrance exam score. These determine the *subjective* probabilities of a regular and preferential admission as a function of pre-college effort (represented in Figure 2.4 as $PrR(e)$ and $Pr15(e)$). Based on these beliefs, in period 1 students choose study effort so as to maximize its perceived present value. In period 2, students decide whether to take the PSU entrance exam. As in the real world, students do not yet know their entrance exam score or whether they are in the top 15% of their school, and must base their choices on beliefs about these outcomes. In period 3, admissions are realized according to *objective* admission chances, which depend on the entrance-exam-taking decision and on the entrance exam score and GPA rank actually achieved. In period 4, students make enrollment decisions given their admissions, which depend on the choices they made in previous periods.

FIGURE 2.4: Model timeline.



Parameterization. Below we show how preferences and the objective and subjective production functions and admission probabilities enter the model. In Appendix B.6.1 we show how we parameterize them when we estimate the model.

Objective and subjective admission probabilities. The entrance exam score is produced through effort e_i:

$$PSU_i = PSU(e_i, y_{i,t-1}^{(1)}; \beta^P) + \epsilon_i^P, \tag{2.2}$$

where $y_{i,t-1}^{(1)}$ is a baseline standardized test score and ϵ_i^P is a normally distributed idyosincratic shock. Letting A_i^R be equal to 1 if student i obtains a regular admission and to 0 otherwise, and letting S_i be equal to 1 if student i takes the entrance exam and to 0 otherwise, the objective probability of a regular admission for those who take the entrance exam depends on the entrance exam score, and can be written as:

$$Pr(A_i^R = 1 | PSU_i, S_i = 1; \gamma). \tag{2.3}$$

But students base their pre-admission choices on beliefs about the PSU production function:

$$PSU_i^b = PSU^b(e_i, y_{i,t-1}^{(1)}, k_i; \beta^{Pb}) + \epsilon_i^{Pb}, \tag{2.4}$$

where normally distributed ϵ_i^{Pb} captures belief uncertainty around the expected score, and on beliefs about how the entrance exam score translates into a regular-admission chance (captured by the parameters γ^b):

$$Pr^b(A_i^R = 1 | \overline{PSU}_i^b, S_i = 1; \gamma^b), \tag{2.5}$$

where $\overline{PSU}_i^b$ is the expected score.

Similarly, GPA is produced through effort:

$$GPA_i = GPA(e_i, y_{i,t-1}^{(2)}; \beta^G) + \epsilon_i^G, \tag{2.6}$$

where $y_{i,t-1}^{(2)}$ is baseline GPA and ϵ_i^G is a normally distributed idiosyncratic shock, potentially correlated with the PSU production shock. The objective probability of a preferential admission is determined by the joint distribution of the shocks in the school; preferential admissions are assigned to students in treated schools who take the entrance exam and whose GPA is in the top 15% of their school. But students base their pre-admission choices on beliefs about the GPA production function:

$$GPA_i^b = GPA^b(e_i, y_{i,t-1}^{(2)}, k_i; \beta^{Gb}) + \epsilon_i^{Gb}, \tag{2.7}$$

where normally distributed ϵ_i^{Gb} captures belief uncertainty around the expected GPA, and on beliefs about how the GPA translates into a preferential admission chance (captured by the parameters ξ^b):

$$Pr^b(A_i^P|\overline{GPA}_i^b, c\bar{1}5_i^b; \xi^b), \tag{2.8}$$

where $\overline{GPA}_i^b$ and $c\bar{1}5_i^b$ are the expected GPA and school cutoff and where A_i^P is equal to 1 if student i obtains a preferential admission and to 0 otherwise. Students in PACE schools, therefore, best-respond to their belief about the within-school cutoff ($c\bar{1}5_i^b$), and we do not impose that the beliefs are equilibrium ones. This modelling approach follows an established approach developed in the behavioral game theory literature (e.g. Camerer, Ho, and Chong, 2004; Costa-Gomes and Crawford, 2006; Costa-Gomes and Zauner, 2003; Crawford and Iriberri, 2007; Stahl and Wilson, 1995).[41]

Per-period utilities. In the first period, students derive utility from achievement, produced through effort, and face a cost of exerting effort, such that the per-period utility associated with each effort choice $e_i \in \{0, 1, ..., E\}$ is:

$$u_{i1}(e_i) = y(e_i, x_i, y_{i,t-1}^{(1)}, k_i; \alpha) - c(e_i; \xi), \tag{2.9}$$

where the cost function is assumed to be quadratic: $c(e_i; \xi) = \xi_1 e_i + \xi_s e_i^2$, with a constant normalized to zero because only the difference in utilities is identified. In period 2, students decide whether to take the entrance exam. The per-period utility from taking the exam is the sum of the cost of taking the exam (capturing monetary and non-monetary costs), and a standard logistic shock: $u^{S_i=1} = -c^S + \eta_i$.[42] The per-period utility from not taking the exam is normalized to 0 because only the difference in utilities is identified. In time period 3, admissions are realized.[43] In time period 4, when making enrollment decisions, students derive the following utilities from a regular and a preferential enrollment, respectively:

$$u_i^{ER} = \lambda_{0k_i}^R + \lambda_1 SES_i + \lambda_2 a_i + \lambda_3 q^R(PSU_i) + \nu_i^R, \tag{2.10}$$

$$u_i^{EP} = \lambda_{0k_i}^P + \lambda_1 SES_i + \lambda_2 a_i + \lambda_3 q^P(GPA_i) + \nu_i^P, \tag{2.11}$$

[41] As we explain in detail in Appendix B.6.1, we assume that the survey answers on the expected entrance exam score, GPA and GPA cutoff capture the believed average outcome, and we allow for belief uncertainty around this average, which is absorbed by the γ^b and ξ^b parameters.

[42] The fee is approximately USD 30; most students in the sample can apply for a fee waiver. But disadvantaged students may face non-monetary barriers to taking entrance exams.

[43] We let preferential admissions carry a utility $\delta^A \neq 0$, because in the data we see a null PACE effect on entrance-exam taking that would be difficult to capture without a preferential admission disutility: PACE provides new admission opportunities to those who take the entrance exam, increasing the value of taking it, without increasing its cost. A possible micro-foundation for this parameter is pressure from parents and teachers to enroll through PACE once a PACE admission is obtained, if students would rather avoid enrolling preferentially.

where $\lambda^P_{0k_i} = \lambda^R_{0k_i} + \delta^E$. The utility from not enrolling is normalized to 0. We let the enrollment utilities depend on: the type k_i; the socioeconomic status and ability (SES_i, a_i); the selectivity of the degree-program to which they are admitted (defined as the lowest entrance exam score among all regular entrants), which, approximating the allocation mechanisms, depends on the PSU score in the regular channel and on the GPA in the preferential channel, $q^R(PSU_i), q^P(GPA_i)$; and a standard-logistic utility shock.[44] When making pre-admission choices, students use their expected PSU and GPA to form beliefs about the quality of the degree programs to which they will gain admissions, but realized qualities depend on the objective PSU score and GPA achieved. Keeping selectivity constant, preferential and regular enrollments are allowed to give different utilities (the constants in equations (2.10) and (2.11) can differ), to capture differences across channels not captured by selectivity, as well as any utility cost or premium from enrolling as a preferential student. The enrollment preferences, which are relative to the outside option, capture tastes, barriers and outside options that vary by unobserved student characteristics (k_i) and by background and ability (SES_i, a_i). We do not let the enrollment utilities directly depend on pre-college effort because, as shown in the first remark of section 2.5.1, the data suggest that students do not believe pre-college effort matters for college persistence.

Solution. Students construct a *subjective* value function using their beliefs, which we indicate with a b superscript:

$$V^b_t\left(\Omega_{it}\right) = \max_{d_{it}\in D_{it}} \left\{u(d_{it}, \Omega_{it}) + E^b\left[V_{t+1}\left(\Omega_{it+1}|\Omega_{it}, d_{it}\right)\right]\right\} \tag{2.12}$$

where Ω_{it} is the state vector, which evolves from the initial condition according to *objective* production functions and admission probabilities, and d_{it} is the period choice.[45] We solve the problem by backward induction and find the value of the subjective value function in all decision periods and at all possible state space values. We compute the exact analytical solution, a sequence of optimal, non-randomized decision rules $\{d^*_{it}(\Omega_{it})\}$ that are deterministic functions of the state space Ω_{it}.[46]

[44] SES_i is an indicator for whether the student is identified as with very-low SES by the government; a_i is an indicator for whether a student is above or below median ability at baseline.

[45] The vector of initial conditions is $\Omega_{i1} = [x_i, k_i, y_{it-1}, c\bar{1}5^b_i, T_{j(i)}]$, where $T_{j(i)}$ is a dummy equal to 1 if a student is in a school randomly allocated to the PACE treatment.

[46] The model presumes that college admission is one of the motives behind effort provision in high school, but 9.7% of students report, at baseline, that they do not think they will stay in education beyond high-school, and 97.3% of them do not enroll in college. We assume these students solve a static decision problem in period 1 (effort decision), and allow the treatment to have a direct effect on their cost of study effort.

2.6.3 Identification

We now discuss key measures we use, and how we identify the parameters governing subjective beliefs. In Appendix B.6.2 we discuss permanent unobserved heterogeneity, modelled following Heckman and Singer, 1984, Keane and Wolpin, 1994, 1997, and Wooldridge, 2005.

Pre-college achievement and effort. Pre-college achievement enters the utility of students in the first model period. We assume that the score on the standardized test that we administered, y_i^o, is a noisy measure of pre-college achievement: $y_i^o = y_i + \epsilon_i^{m.e.y.}$, where $\epsilon_i^{m.e.y.} \sim N(0, \sigma^2_{m.e.y.})$ is a classical measurement error. Pre-college effort is a choice of students in the first model period. We assume that reported hours of study per week over a semester are a noisy measure of pre-college effort: $e_i^o = e_i + \epsilon_i^{m.e.e.}$, where $\epsilon_i^{m.e.e.} \sim N(0, \sigma^2_{m.e.e.})$ is a classical measurement error. Using reported hours of study to measure effort allows us to use a common scale to estimate the objective and perceived returns to effort in the production of entrance exam scores and GPA, because we measured the perceived returns using hypothetical study hour scenarios.

Subjective beliefs. We separately identify subjective beliefs from unobserved ability and preferences using the belief data we collected (Manski, 2004). The subjective probabilities of a regular and a preferential admission, conditional on taking the entrance exam ($S_i = 1$), are a function of effort e_i, and depend on the expected believed PSU score, $E[PSU^b_{k_i}(e_i, x_i)]$, the expected believed GPA, $E[GPA^b_{k_i}(e_i, x_i)]$, and the believed top 15% cutoff in the school, $c\bar{1}5^b_i$, as shown in the following equations and, in more detail, in equations (B.6) and (B.7) in the Appendix:

$$Pr^b(A_i^R = 1 | e_{it}, x_i, k_i, S_i = 1) = \Phi\left(\gamma_0^b + \gamma_1^b E[PSU^b_{k_i}(e_i, x_i)]\right), \tag{2.13}$$

$$Pr^b(A_i^P = 1 | e_i, x_i, k_i, S_i = 1) = \Phi\left(\xi_0^b + \xi_1^b (E[GPA^b_{k_i}(e_i, x_i)] - c\bar{1}5^b_i)\right), \tag{2.14}$$

where x_i are baseline student characteristics and k_i is the student's type.

First, we follow a standard approach from the behavioral game theory literature, and assume that students in treated schools best-respond to the perceived cutoff that we have elicited, without imposing equilibrium behavior (Camerer, Ho, and Chong, 2004; Costa-Gomes and Crawford, 2006; Costa-Gomes and Zauner, 2003; Crawford and Iriberri, 2007; Stahl and Wilson, 1995). Therefore, this argument of the function in (2.14) is observed.

Second, to identify the perceived returns to effort in the subjective production functions, in the right-hand side of (2.13) and (2.14), we do not rely on the cross-sectional

relationship between expected outcomes and effort, because it cannot necessarily be interpreted as causal. Instead, we measured perceived returns with our survey. We elicited beliefs about the PSU score and the GPA that students expect to obtain under the actual and hypothetical effort levels. For example, for entrance exam scores, we asked:
Thinking of yourself, how many hours per week do you think you need to study, between August and December, to obtain...
... 600 or more on the PSU
... 450 or more on the PSU
... 350 or more on the PSU.
The answers are hypothetical hours of study, which we assume are affected by measurement error: $h_i^{oj} = h_i^j + \epsilon_i^{m.e.e.}$, where $j = 600, 450, 350$ and $\epsilon_i^{m.e.e.} \sim N(0, \sigma^2_{m.e.e.})$. We convert the answers into the expected increase in PSU score per additional hour of study per week, i.e., the perceived returns to effort in PSU score production. To improve precision of our measure, we combine the answers to the hypothetical questions with those to the questions on how much they actually studied and what PSU score they expect. Let $e_i^o = e_i + \epsilon_i^{m.e.e.}$ denote the hours of study they report, and let $PSU_i^b|_{e_i^o}$ denote the PSU score they expect given those hours. We measure the perceived returns to effort as

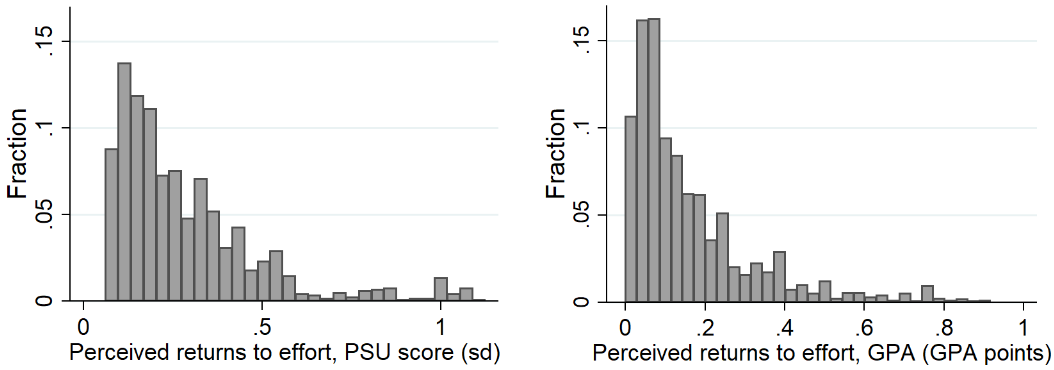
$$\sum_{j \in \{350,450,600\}} \frac{1}{3} \cdot \frac{j - PSU_i^b|_{e_i^o}}{h_i^{oj} - e_i^o}, \quad \text{if } h_i^{oj} \neq e_i^o. \tag{2.15}$$

Figure 2.5 shows the distribution of returns to effort in our sample (Table B.13 summarizes the survey answers used to construct the returns). In estimation, we match moments of these distributions using their model counterparts. Naively matching them would introduce sample-selection bias because perceived returns are not observed among students who were not surveyed. To mitigate the issue we let parameters that govern the perceived returns depend on the unobserved student type, and we let the type distribution vary across students who were and were not surveyed. We then simulate the distributions of perceived returns conditional on being surveyed to build the model counterparts to the empirical moments.

To simulate perceived returns, we simulate the expected PSU score and GPA for each student at various values of hours of study. For example, consider distinct effort levels h_i^z and h_i^j and let $\widehat{PSU_i^b}(h_i)$ be the expected PSU score predicted by the model at effort level h_i. The simulated returns to effort are:

$$\frac{\widehat{PSU_i^b}(h_i^z) - \widehat{PSU_i^b}(h_i^j)}{h_i^{oz} - h_i^{oj}}, \quad \text{where } h_i^{oz} = h_i^z + \epsilon_i^{m.e.e.} \text{ and } h_i^{oj} = h_i^j + \epsilon_i^{m.e.e.}. \tag{2.16}$$

FIGURE 2.5: Distribution of perceived returns to effort, measured as the perceived impact of an additional hour of study per week in the semester (top 1% trimmed).



Having identified the parameters governing perceived returns to effort, we match the distributions of expected PSU scores and GPA to identify the remaining parameters of the perceived production functions. Then, all arguments of the subjective admission probabilities in (2.13) and (2.14) are either observed or identified. The relation between choices and these arguments identify the parameters of the subjective probabilities $(\gamma_0^b, \gamma_1^b, \xi_0^b, \xi_1^b)$. Appendix B.6.2 details how we mitigate potential endogeneity of these arguments by imposing additional exclusion restrictions, exploiting the experimental data variation wherever possible.

2.6.4 Estimation

Aside from the parameters of the regular admission probability (equation (B.3)) and of the selectivity of an admission (equations (B.8) and (B.9)), whose estimates we report in Table B.14, all parameters are estimated within the model. They pertain to the production technologies $(\alpha, \beta^P, \beta^G)$, subjective beliefs $(\beta^{Pb}, \beta^{Gb}, \gamma^b, \xi^b)$, preferences (ξ, c^S, λ), and the distribution of model shocks, measurement errors, and unobserved types $(\Sigma, \sigma^2_{m.e.y.}, \sigma^2_{m.e.e.}, \omega)$. We assume that there are two unobserved types ($K = 2$) that follow a logit distribution that depends on the ninth and tenth grade GPA average ($y_{it-1}^{(3)}$) and on an indicator for whether a student was surveyed in our data collection, D_i^s, to correct for survey attrition based on unobservables. Since the treatment was randomized, we can assume that types are identically distributed across treatment groups (i.e., balanced unobservables). Letting $X_i = [1, y_{it-1}^{(3)}, D_i^s]$:

$$Pr(k_i = \tau | X_i) = \frac{e^{X_i'\omega}}{1 + e^{X_i'\omega}}. \tag{2.17}$$

Estimation is by generalized indirect inference (Bruins et al., 2018), as in Altonji, Smith Jr, and Vidangos, 2013. In a first step, we estimate a set of auxiliary models that summarize the experimental findings and data patterns to be targeted in the structural estimation. In a second step, an outer loop searches over the parameter space, while an inner loop solves the dynamic model at each candidate parameter value and forms the criterion function. The latter is the distance between the auxiliary model estimates from the data and their counterparts from the simulated data. Appendix B.6.3 lists the auxiliary models and moment conditions.

At each parameter iteration θ, we simulate S datasets, where each simulation is a draw for the model shocks and the student type.[47] Let $\bar{\beta}$ denote the vector of auxiliary model parameters and moments computed from the data, and let $\hat{\beta}^s(\theta)$ denote the corresponding values obtained from the s^{th} dataset predicted by the model at the value θ of the structural parameters. Let $\hat{\beta}(\theta) = \frac{1}{S}\sum_{s=1}^{S}\hat{\beta}^s(\theta)$. The structural parameter estimator is obtained as the solution to:

$$\hat{\theta} = \arg\min_{\theta} [\hat{\beta}(\theta) - \bar{\beta}]' W [\hat{\beta}(\theta) - \bar{\beta}] \tag{2.18}$$

where W is a positive definite weighting matrix. Generalized indirect inference, developed for dynamic discrete choice models like ours, ensures that the criterion function is differentiable and allows us to rely on a fast derivative-based optimization method to solve (2.18).[48]

2.7 Model Results

2.7.1 Estimation Results

Parameters. Estimates of the model parameters are in Table B.15. Comparing the perceived and objective production functions shows that students hold overoptimistic beliefs about the returns to effort. In the objective production function of entrance exam score (GPA), the coefficient on effort is 0.161 standard deviations (0.037 GPA points, or 0.065 standard deviations). But students, depending on their type (as defined in section 2.6.2), believe it is larger, between 0.262 and 0.331 standard deviations (0.148 and 0.353 GPA points, or 0.260 and 0.619 standard deviations). Therefore, both student types are overoptimistic. Those of the more optimistic type also have higher unobserved ability and preference for college. Therefore, ability, preferences and beliefs correlate with each other, highlighting the importance to allow for such correlation in estimation.

[47] Following the results in Eisenhauer, Heckman, and Mosso, 2015, we set $S = 20$.

[48] Following Altonji, Smith Jr, and Vidangos, 2013, we use the smoothing function $\frac{exp(\frac{u_i}{\lambda})}{1+exp(\frac{u_i}{\lambda})}$, where u_i is the latent utility, with smoothing parameter $\lambda = 0.05$. We use Knitro to solve the optimization problem (Byrd, Nocedal, and Waltz, 2006).

Model fit. As Table B.16 shows, the model captures key facts and findings from the reduced-form analysis, and additional important data features such as the dynamics of the students' choice problem. The model can rationalize all the facts and findings at the core of our analysis, including those that would be hard to explain with standard rational expectation models.

The model can match the fact that a high proportion of students take the entrance exam, but a much lower proportion is admitted and enrolls in college absent the policy. It matches the positive treatment effects on admissions and enrollments, and negative on pre-college effort and achievement. At the same time, it captures very closely the belief biases over both absolute and relative ability. And finally, it matches correlations in choices over time. For example, it matches very closely the GPA of college entrants, overall and by treatment groups, even though it was not directly targeted in estimation. GPA of college entrants is the outcome of several choices that occur dynamically: the choice of pre-college effort directly affects GPA, and also indirectly affects the selection of college entrants by affecting a student's admission likelihood. The fact the model can capture endogenous outcomes and dynamic self-selection suggests that it provides a reasonable approximation to the dynamic decision process that students face. Finally, the Table specifies which moments were directly targeted in estimation and which were not, and shows that the model can fit both kinds of moments, improving our confidence in the model-based results.

Perceived incentive effect. Having estimated the model, we can use it to simulate the perceived returns to effort in the admission likelihood for students in the treatment and control group, and quantify the perceived incentive effects of PACE.

Absent PACE, the perceived return to effort in the admission likelihood is the derivative with respect to effort of the perceived likelihood of a regular admission. Under PACE, it is the derivative with respect to effort of the perceived likelihood of obtaining either a preferential admission or a regular admission or both. Since this derivative varies with effort, we average it across effort levels. Letting $e = 0, 1, 2, ..., 10$ denote the possible levels of hours of study per week (effort) and $Pr^b(A_i = 1|e, \Omega_{i1})$ the perceived probability of an admission for a student who exerts effort e and has a vector of initial conditions Ω_{i1}, the average perceived marginal returns to effort for student i can be approximated by the numerical derivative:

$$\frac{\partial Pr^b(A_i = 1, e, \Omega_{i1})}{\partial e} = \frac{1}{10}\sum_{e=0}^{9} \frac{Pr^b(A_i = 1|e + \Delta e, \Omega_{i1}) - Pr^b(A_i = 1|e, \Omega_{i1})}{\Delta e},$$

where $\Delta e = 1$. Using the distribution of initial conditions, we average this derivative across students to calculate the treatment effect on perceived returns to effort.

We find that PACE lowered the perceived return to effort in generating a college admission by 5.3 percentage points (p.p.), a 77% reduction compared to the perceived return to effort without PACE. Without PACE, our simulations indicate that students believe one additional hour of study per week in the first semester of the last high-school year increases the likelihood of college admission by 6.9 p.p. on average. With PACE, this figure falls to 1.6 p.p. Therefore, students perceived that the policy considerably undercut their incentive to exert effort in the last high-school year.

2.7.2 Counterfactual Experiments: Improving the College Preparedness of College Entrants through School Interventions

We define college preparedness as a vector containing high-school standardized test scores at baseline (a measure of baseline ability) and the effort exerted in the last high-school year, since they jointly and independently predict college persistence (Table B.3). We simulate two counterfactual scenarios and examine how they change the college preparedness of college entrants under PACE. In the first, we simulate correcting the belief errors that students hold about their absolute and relative ability. In the second, we approximate a policy informing students of the importance of pre-college effort for persistence in college.

The college preparedness of college entrants is determined by the *selection* channel (i.e., the ability composition of college entrants) and the *effort* channel (i.e., how much effort they exerted in high school). Any intervention changing pre-college effort can affect it directly, through the effort channel, and indirectly, through the selection channel. To see how the selection channel works, notice that pre-college effort affects the perceived GPA rank and perceived entrance exam score, which in turn affect the perceived likelihoods of a regular and of a preferential admission and, therefore, the decision to take the entrance exam. Effort, therefore, affects the choice of taking the exam, the actual GPA rank, and the actual entrance exam score, which together determine the objective admission likelihood of each student. Effort, therefore, affects the selection of admitted students and college entrants. Our model captures all of these effects.

First counterfactual experiment: correcting beliefs on ability in PACE high schools. Had the students in PACE high schools had correct information about their relative and absolute ability, they would have exerted different levels of pre-college effort. In turn, both the selection of college entrants and their pre-college effort would have been different.

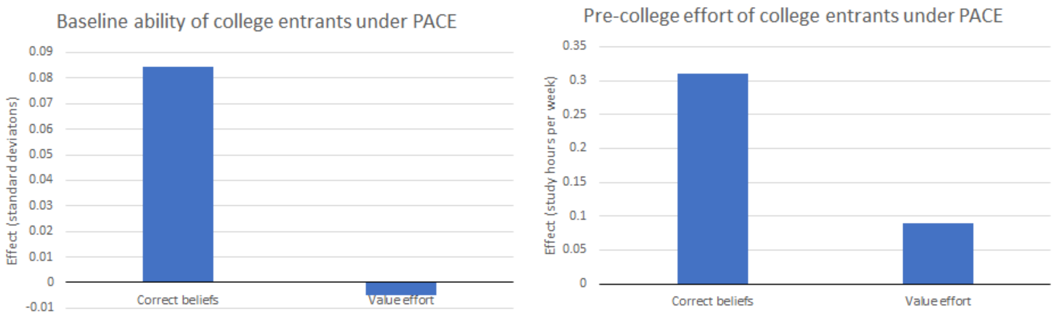
To simulate this counterfactual, we assume students have rational expectations. We assume they use objective rather than subjective production functions (for the GPA and the entrance exam score) and admission likelihood functions (for regular and preferential admissions). We then solve for the rational-expectations equilibrium of the tournament game that takes place in each school to award the preferential admissions, a high-dimensional fixed-point problem. This is a notoriously difficult problem to solve. Previous studies have simplified it by assuming that there is a continuum of individuals and that they differ only along one dimension (Bodoh-Creed and Hickman, 2018, 2019; Cotton, Hickman, and Price, 2020; Hopkins and Kornienko, 2004). But these simplifications are inappropriate in our setting: i) our populations are schools, which are limited in size, and ii) individuals differ in more than one dimension. Therefore, we develop an algorithm that allows us to relax them.[49] Appendix B.6.4 describes it.

The bars labelled "correct beliefs" of Figure 2.6 present the results of the counterfactual experiment. If students in PACE schools had correct beliefs, both the high school test scores at baseline (baseline ability) and the pre-college effort of the sub-sample that selects into college would have been larger, by 0.08 standard deviation and 0.31 study hours per week (corresponding to 0.6 standard deviations of the study hour distribution in the sample). Therefore, the selection and the effort channels are both empirically relevant channels through which informational interventions can affect the college preparedness of college entrants under large admission advantages.

We now examine how this counterfactual policy affects students' choices along the baseline test score distribution. Figure 2.7 shows the effect of eliminating belief errors on the pre-college effort and admissions of students in PACE schools, by 10^{th} grade (baseline) test scores. Recall that beliefs are over-optimistic, on average, at all baseline test score levels in our sample (Figure B.5). Eliminating such over-optimism has opposite effects on effort depending on ability (left panel of Figure 2.7). Over-optimism leads high-ability students to incorrectly perceive an admission as guaranteed and under-provide effort, and low-ability students to incorrectly perceive it as within reach and over-provide effort. Therefore, eliminating it increases the effort of high-ability students and decreases that of low-ability ones. Since effort affects the likelihood of qualifying for an admission, effort under- (over-)provision results in under- (over-)admissions, so that eliminating

[49] We lower the dimensionality of the fixed point and solve for an approximated equilibrium. The intuition is that the strategies of others affect own payoffs only through the probability of a preferential admission. We posit a parametric approximation for this probability and solve for a fixed point in its parameters. We thank Nikita Roketskiy for suggesting this approach.

FIGURE 2.6: Counterfactual experiments simulating interventions in schools to shape the college preparedness of college entrants: the selection and effort channels.



Notes: The panels show the effects of hypothetical interventions that correct belief biases (first bar) or that inform students of the importance of pre-college effort for persistence in college (second bar) on the college preparedness of college entrants under PACE. The left panel shows the effect on the high school standardized test scores at baseline, i.e., the 10^{th} grade, standardized in the population of 10^{th} graders (the selection channel). The right panel shows the effect on study hours per week in the first semester of the last high-school year (the effort channel).

over-optimism increases the admissions of the high ability and decreases those of the low ability (right panel). This explains why correcting belief biases results in a better selection of admitted students in terms of baseline test scores, who have also exerted more effort while in high school. This intervention would also lower the pre-college effort of those who do not enter college (by 0.64 study hours per week, or 1.26 standard deviations).

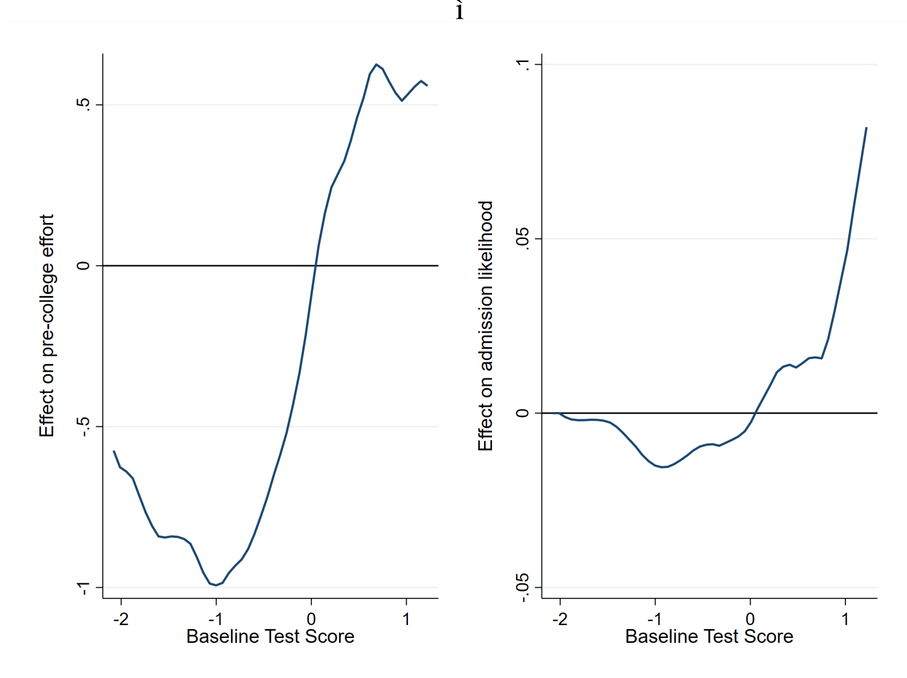
Second counterfactual experiment: informing students of the importance of pre-college effort for college persistence. Some policymakers consider providing rank information controversial; they worry that it could promote unhealthy competition among students and, for this reason, are not actively pursuing this strategy.[50] Therefore, we consider an alternative policy to influence college preparedness: informing high school students targeted by large admission advantages of the importance of pre-college effort for persistence in college.

Recall that in the baseline model we do not allow effort to enter the utility from college enrollment (see the discussion below equations (2.10) and (2.11)), because the data suggest students do not believe pre-college effort is important for college persistence (section 2.5.1). In this counterfactual experiment, we assume that the utility students derive from college depends on pre-college effort. The idea behind this assumption is that a student

[50]This is what policymakers at the Chilean Ministry of Education told us.

FIGURE 2.7: Effects of correcting belief biases in PACE schools on pre-college effort and on admission likelihood along the baseline test score distribution.

ì



Notes: Effort is measured in study hours per week in the first semester of the last high-school year. Baseline test scores (standardized) are measured in 10^{th} grade.

becomes aware that exerting more effort in high school can make it easier to learn in college, reducing the likelihood of dropping out. We let effort enter the utility from college with a coefficient of 0.015, which captures how predictive each additional hour of effort is for persistence in college (see Table B.3).[51] Since students are forward looking, this counterfactual changes the continuation value of study effort in high school. Therefore, it affects how much effort students wanting to go to college exert.

We must assume a process for counterfactual beliefs about the school cutoff, because the elicited beliefs about the cutoff were collected at the baseline distribution of effort in each school and are not appropriate beliefs in a counterfactual that changes the within-school distributions of effort. We assume that the belief bias over the rational expectations cutoff remains constant in the counterfactual, which means assuming that students remain as uninformed in the counterfactual as they were in the baseline scenario.[52]

[51]The utility normalization is such that the unit of measurement of utility is the standard deviation of the achievement test score at the end of high school. Therefore, we are assuming that the utility derived from predicted persistence as opposed to predicted dropout is the same as that derived from having achievement that is larger by one standard deviation.

[52]To do so, we calculate the difference between each student's elicited cutoff and the rational expectations cutoff in the baseline scenario (which we simulate), i.e., the belief bias over the rational expectations cutoff.

The bars labelled “value effort" of Figure 2.6 show that the selection of students into college would stay substantially unchanged (left panel), while the pre-college effort of those who self-select into college would improve by 0.09 hours of study per week, corresponding to 0.18 standard deviations of the study hour distribution in the sample (right panel). This intervention, therefore, is not as effective at improving the college preparedness of college entrants as correcting belief errors about absolute and relative ability. Given the widespread over-optimism about admission chances, this intervention would also cause those who do not enroll in college to increase their pre-college effort (by 0.06 hours per week, or 0.12 standard deviations) so as to improve their college persistence, which has ambiguous welfare implications.

2.8 Conclusions

We use an innovative randomized control trial and a comprehensive longitudinal dataset matching detailed administrative records with a data collection in schools developed specifically for this study to provide the first evidence on the impacts of college admission policies targeted at the very disadvantaged. The PACE policy in Chile eliminated the entrance exam requirement for students graduating in the top 15 percent of their school, and it targeted students who score 1.5 standard deviations below regular entrants on 10^{th} grade standardize tests, and who are considerably disadvantaged.

We present several novel findings from this unprecedented empirical setting. This paper focuses on impacts on education outcomes during high school and up to five years after leaving high school. We document that PACE increased college admission and first-year enrollment by 36 percent, but the impacts on continuous enrollment or graduation in the fifth year were around a third of the impacts in the first year. We also show that PACE had negative impacts on pre-college effort and GPA in core subjects (Mathematics, language), dimensions of pre-college human capital that independently predict persistence in college. The experimental research design allows us to examine policy impacts away from admission cutoffs, and we find that the effort impacts are widespread along the baseline ability distribution. Using novel survey data on the beliefs that students have about their entrance exam scores and GPA rank, we show that such evidence is most consistent with students reducing their effort in high school because they perceived that PACE undercut their incentive to exert effort to obtain a college admission. In fact, by matching students’ expected entrance exams and GPA rank with administrative records,

In the counterfactual, we build the believed cutoff as the sum between the rational expectations cutoff at the counterfactual effort distribution (which we simulate) and the belief bias over the rational expectations cutoff.

we document that students of all absolute and relative (within-school) abilities display large belief biases.

Together, the reduced-form findings suggest that college preparedness matters for the impacts of large admission advantages that reach severely disadvantaged populations, and that college preparedness is not fixed by late adolescence, it responds to effort investments made in the last high-school year. This suggests that school interventions designed to shape pre-college effort investments could improve the persistence of the impacts of large admission advantages. The large belief biases we documented also suggest a margin for policy intervention. But without more structure, it is difficult to quantify how pre-college effort investments shape the college preparedness of those who self-select into college. Therefore, we develop and structurally estimate a dynamic structural model that allows us to perform the ex-ante evaluation of informational school interventions designed to shape pre-college effort investments and the college preparedness of college entrants.

The model extends the structural literature modelling admission policies by endogenizing pre-college effort and allowing for biased pre-college beliefs about ability. The model-based results suggest that correcting misperceptions would be effective at improving the college preparedness of college entrants: it would lead to a more positive selection of college entrants, and increase the pre-college effort of those who self-select into college. Informing students of the importance of pre-college effort for college persistence, instead, would have more modest impacts on the college preparedness of college entrants, and it would increase pre-college investments also among overly optimistic students who expect to enter college but who do not get admitted, with ambiguous welfare implications.

This study is the first to examine the impacts of context-based admission advantages on a very disadvantaged population, and it finds that they can improve college enrollment and persistence up to five years, when our data end. Our results can serve as a starting point for discussions about the optimal design of context-based admissions and suggest that such policies can improve the college attainment of students further down the academic preparedness distribution than previously found. Future studies should explore the labor market impacts of PACE. Our results, however, also highlighted challenges that may be specific to these school populations. We documented large biases in beliefs about absolute and relative ability and argue they interacted with policy effectiveness. Directly comparing these findings with other contexts is difficult because data on the beliefs of high school students targeted by admission policies are rarely collected. But our results suggest that policymakers wanting to expand admissions to more disadvantaged populations should reckon with the reality of the school environments such policies would encounter, and consider pairing the admission rules with tailored school interventions.

Chapter 3

Health Effects of Increasing Income for the Elderly: Evidence from a Chilean Pension Program

3.1 Introduction

Researchers and policymakers have documented large and ever-widening life expectancy inequalities across income groups in both developed and developing countries (Brønnum-Hansen and Baadsgaard, 2012; Hoffman, 2008; Tarkiainen et al., 2012).[1] For instance, a recent OECD (2018) report shows that, at retirement age, high-income earners live longer than low-income earners: 1.6 years longer in the US, 3.6 in Chile, 3.25 in the UK and 2.9 in South Korea.[2]

Despite a large body of literature documenting that, at all ages, wealthier people enjoy better health on average (Braveman et al., 2010; Chetty et al., 2016; Marmot, 2005; Waldron, 2013), substantial debate remains on whether an income increase for the elderly poor can improve their health. For instance, unobserved characteristics (e.g. genetic factors) could explain both higher income and better health. Alternatively, better health could be the cause of higher income (reverse causality). Differences in health status may also be the result of cumulative conditions related to income inequalities at earlier ages (e.g. exposure to pollution).

The non-contributory pension program in Chile provides an ideal regression discontinuity (RD) design to identify the causal effect of a large permanent income increase

[1] The preliminary results of this paper first circulated as Miglino et al. (2017). The paper has been published as 'Health Effects of Increasing Income for the Elderly: Evidence from a Chilean Pension Program' Miglino, Enrico; Navarrete, H. Nicolás; Navarrete, H. Gonzalo; Navarrete, H. Pablo; *American economic journal. Economic policy*, 2023, Vol.15 (1), p.370-393

[2] In the report high-income earners are those who earn more than three times the average wage and low-income earners are those who earn half of the average wage or less.

for the elderly poor on their health outcomes. Since 2008, Chileans who are aged 65 or over and do not have a contributory pension can apply to receive a governmental pension, which provides lifelong monthly payments of approximately 40% of the national minimum wage (basic pension). Upon receiving applications, the government calculates a *pension score* and assigns a basic pension to applicants who fall below the 60$^{\text{th}}$ percentile (cut-off) of the score distribution.

Our study uses administrative data on basic pension applicants and their household members in 2011 and 2012.[3] This data is paired with their medical history from 2011 to 2016. We first note that the pool of applicants consists mostly of women without a history of regular paid employment (e.g. former stay-at-home mothers). As individuals can apply multiple times, we define applicants whose *first* application score fell *below* (*above*) the cut-off and within a certain bandwidth, as the intent-to-treat (ITT) 'treatment group' ('control group'). We show that density and balance tests cannot reject the hypothesis that the pension is as good as 'locally' randomly assigned between treatment and control group. We then implement an RD analysis to explore the causal ITT effects of the pension on applicants. To estimates the local treatment effect on the treated (TOT), we use the 'recursive' RD estimator suggested by Cellini, Ferreira, and Rothstein (2010), which explicitly accounts for later successful applications by control group applicants.

Receiving a basic pension reduces applicants' probability of dying by 2.7 percentage points (pp.) within four years of applying, with an ITT income-mortality elasticity of -0.386. The decrease is statistically significant and remains unaffected when using non-parametric estimations and different sets of controls, bandwidths, and polynomial orders.

To shed light on the mechanisms behind this effect, we complement our RD estimation with the analysis of a longitudinal survey conducted by the Chilean Ministry of Labor (Ministerio Trabajo y Previsión Social, 2015). An increase in food consumption and more frequent visits to health centers appear to be relevant drivers of the improvements in recipients' health. Receipt of the basic pension is not associated with a significant change in health insurance coverage or labor supply.

The heterogeneity analysis shows strong health improvements for applicants living without working-age household members and no improvement for those living with working age relatives. A plausible explanation for this last result is that younger relatives reduce their net transfers of income to applicants after pension payments begin. In line with this hypothesis, we also observe an increase in the fertility of working-age relatives of pension recipients, suggesting that transfers of income to applicants may have been diverted to child-raising expenditures.

[3] The program did not systematically collect information on applicants and household members before 2011, making it unfeasible to analyze earlier years.

Our paper provides causal evidence that a permanent income increase for the elderly can improve their health at the present time. Salm (2011) finds that two pension increases in the early 1900s reduced the mortality rates of US veterans. In modern times, the evidence is mixed: studies have estimated negative (Barham and Rowberry, 2013; Jensen and Richter, 2003), insignificant (Cheng et al., 2016), or even positive (Feeney, 2017, 2018; Snyder and Evans, 2006) income elasticities of mortality.

The confidence interval of our estimate encompasses most of the previous negative point estimates of the income-mortality elasticity.[4] To reconcile our results with the positive estimates, note that Snyder and Evans (2006) and Feeney (2017) find that higher pension payments increase the probability of retirement, and that Fitzpatrick and Moore (2018) show that transition to retirement causes a significant rise in mortality, independently of whether income is affected. As the Chilean basic pension is given mostly to people that are already out of the labor force (e.g. former 'stay at home mothers'), it has a limited impact on retirement transitions. Our analysis is then better able to isolate the negative mortality effect of the permanent income increase from the positive mortality effect of the increase in transition to retirement.

The main policy implication of our results is that non-contributory pensions, intended to improve the living standards of the elderly poor, can also improve their health. Furthermore, a cost-benefit analysis suggests that the basic pension is a cost-effective measure to increase pension recipients' life expectancy. Our results are informative for policymakers who aim to introduce income transfers that target subpopulations similar to our treatment group, which is composed primarily of elderly, low-income women in a middle-income country. Income transfers directed to recipients with different characteristics may have different policy implications, as suggested by the large variance of mortality-income elasticities estimated in the literature.

The paper is organized as follows. Section 3.2 presents the basic pension program. Section 3.3 describes the data and explains the empirical strategy. Section 3.4 provides evidence for the validity of the RD assumptions. Section 3.5 presents the results and the potential mechanisms behind the effects. Section 3.6 illustrates the cost-benefit analysis, and Section 3.7 concludes.

[4] Lindahl (2005), Cesarini et al. (2016) and Schwandt (2018) showed mixed results regarding the impact of increases in wealth, such as lottery prizes, on mortality rates amongst the elderly. Although these studies belong to a related literature, the effects of unexpected wealth increases might differ from the effect of a permanent income increase guaranteed by the government.

3.2 The basic pension

Since 1980, Chile has had a full-capitalization pension system in which workers must contribute ten percent of their monthly wage into a private pension fund. Upon retirement, workers receive a pension that is dependent on the amount saved over their working life (*contributory pension*). Until recently, those who had never undertaken paid work received no pension.

This system was judged to be particularly unfair to stay-at-home mothers. To address this issue, President Bachelet signed ACT 20255 into law on March 11th, 2008. This Act established that every citizen aged 65 or above with no retirement savings would be eligible for a pension consisting of lifelong monthly payments provided by the government (basic pension). The introduction of the basic pension took place across Chile simultaneously, and the first payments were delivered on July 1st, 2008. Between 2011 and 2016, our period of analysis, basic pension payments were on average 166 US dollars in 2012 prices (80,961 Chilean pesos), corresponding to approximately 40% of the national minimum wage. Throughout the paper, we present all monetary values converted to 2012 US dollar prices for comparability.

The process for applying for the basic pension is free and identical across Chile. Applicants must apply to the Pension Institute by filling in a form in their municipality of residence. Then, the Pension Institute calculates a pension score that is comprised of two factors: household income from assets (e.g. contributory pensions from household relatives) and labor income from all household members. Administrative data shows that these two factors account for 60% and 40% of total household wealth, respectively. The pension score is then adjusted for household size and household members' disability status. To define a household, the Pension Institute follows the government definition: a group of people, related or not, who live in the same house and share income.

The pension score uses richer data and is computed differently from other governmental indices, such as the *social security score*.[5] The calculation of the pension score relies upon administrative information from public agencies (e.g. Revenue Service) and private companies (e.g. pension fund companies), as well as self-reported information. As the pension score requires information from several public and private offices, it is calculated only for people who apply for the pension.

[5] The social security score ("Puntaje de la Ficha de Protección Social") is a proxy means test based on household composition, potential income and self-reported actual income that allows the government to assign social benefits. The social security score does not use administrative data on labor income or on income from other sources such as contributory pensions. For more details on the pension score see Appendix Section C.1.

Following the assigning of pension scores, the Pension Institute uses an arbitrary cut-off to determine basic pension recipients. The cut-off has gradually increased from covering the poorest 40% of the elderly population in July 2008, to covering the poorest 60% since July 2011. These gradual changes occurred at the same time nationwide.[6]

After the application decision, applicants observe only whether they will receive the basic pension and, if not, the reason for this decision. They can apply more than once, but they never observe the score assigned to them. The government initially considered reassessing basic pension recipients' eligibility every two years. This policy was never enacted and virtually all pension recipients continued to receive payments every month thereafter.

3.3 Data and empirical strategy

3.3.1 Pension and health datasets

Our analysis is based on administrative data provided by the Chilean government. First, we have access to all applications for the basic pension made in 2011 and 2012 (Instituto de Previsión Social, 2017). For each applicant and each of the applicant's household members, the Pension Institute provided us with demographic information regarding their gender, age, town of residency, household social security score, unique identifier number (henceforth *ID number*), and unique identifier number for the household. This dataset also includes the pension score, application date, and the outcome of the application. The Pension Institute collected all the variables mentioned at the moment of application. It also provided us with the outcome of all applications submitted between 2013 and 2016 for those who applied between 2011 and 2012. We do not have access to applicants' data from previous years, as it was not systematically recorded before 2011.[7]

The applicant and household ID numbers allow us to identify the pension applicant in each household and perfectly match each applicant with all household members. Following the Chilean legal minimum working and retirement ages, we define male household members aged 16-64 and females aged 16-59 years as 'working-age household members', while male household members above 64 and females above 59 years of age as 'elderly household members'.

[6]Appendix Figure C.1 shows the timeline of the basic pension reform and the cut-off changes. We find little evidence of applicants delaying their applications to take advantage of the 5% cut-off increase in July 2011 (Appendix Section C.2).

[7]We also obtained household-level data on the factors that determined the pension score and the total household income generated for first applications submitted in 2012. Note that less than 1% of applicants in our working sample share a household with another applicant.

The Ministry of Health also granted us access to the medical history of each applicant and household member in the Pension Institute dataset from 2011 to 2016, which was perfectly matched using individuals' ID numbers. This dataset contains: the date and cause of any deaths; the date of any childbirth for female household members; the date and type of any vaccinations received; and the date, duration, and cause of any hospitalizations, in both private and public health institutions.

Our study analyzes only those applications submitted between July 1, 2011 and December 31, 2012. We do not use applications submitted prior to July 2011, as the 60th percentile cut-off point for eligibility was introduced by the government in July 2011 (Section 3.2). The most recent health data to which we have access extends until December 2016. This allows us to measure health outcomes for up to four years from the date of application. As unsuccessful applicants can submit further applications, we count each applicant as a single observation and accommodate later changes in pension status using the 'recursive' RD estimator presented below.

3.3.2 Regression discontinuity design

To estimate the causal effect of the basic pension on health outcomes, we use a regression discontinuity design. We estimate the local 'intent-to-treat' (ITT) effect, β_t^{ITT}, using the following equation:

$$y_{i,h,a+t} = \alpha + \beta_t^{ITT} D_{h,a} + g_0(Score_{h,a}) + D_{h,a} \times g_1(Score_{h,a}) + \boldsymbol{\gamma' x_{i,h,a}} + u_{i,h,t+a} \quad (3.1)$$

where a is the date of the first application and t is the number of years since the first application. We analyze the outcome y up to four years after the first application, so we can consider the cross-section of first applications and estimate β_t^{ITT} at $t \in \{1, 2, 3, 4\}$. Our main tables report β_4^{ITT}, the ITT effect four years after the first application.[8] $\boldsymbol{x_{i,h,a}}$ is a vector of controls for potentially relevant determinants of the health outcomes, including: gender; whether the applicant is vaccinated for pneumonia and influenza; and month-of-application, health-district and age fixed effects. $Score_{h,a}$ is the distance of the first application score from the cut-off point, for the pension applicant of household h. In our preferred specification, g_j (j=0,1) is a polynomial of order 1 in $Score_{h,s}$. $D_{h,a}$ is an indicator equal to 1 if the applicant of household h obtained a pension score below the cut-off in their first application at date a, and 0 otherwise.

[8] Appendix Figures C.19 and C.22 also show the ITT effect on mortality and fertility within each year following the first application date.

Each regression uses triangular kernels, such that the weight of each observation decreases with the distance from the cut-off. The sample is restricted to a bandwidth of 500 points on either side of the threshold. Standard errors are clustered at the province level.[9] We check the robustness of our results to different specifications using polynomials of order 2 in $Score_{h,a}$, nonparametric estimations, logistic regression, different sets of controls, and the mean-squared error optimal bandwidth approach proposed by Calonico, Cattaneo, and Titiunik (2014).

3.3.3 Treatment effect on the treated

Equation (3.1) estimates the effect of the basic pension on applicants that were 'intended to be treated' at their first application. To estimate the effect of the pension on all applicants that were eventually treated within the four-year period, we need to account for the presence of serial applicants whose first application was rejected but who obtained a basic pension in a successive application. To identify the (local) effect of the treatment on the treated (TOT), we implement the 'recursive' RD estimator suggested by Cellini, Ferreira, and Rothstein (2010) and used by Taylor (2014), which explicitly accounts for the dynamic nature of the treatment.[10]

We can then write health outcomes for any year *t* as a function of the full history of application outcomes:

$$y_{i,h,t} = \sum_{s=0}^{4} \beta_s D_{h,t-s} + u_{i,h,t} \tag{3.2}$$

where $D_{h,t-s}$ is an indicator equal to 1 if the applicant of household *h* obtained a pension score below the cut-off in year $t-s$, and 0 if either the pension score was above the cut-off or they did not apply in that year. $u_{i,h,t}$ represents all other determinants of the outcome (with $E[u_{i,h,t}] = 0$). The TOT in year *t* is the effect of exogenously granting a pension to

[9]There are 33 health districts and 54 provinces in Chile. The standard errors are clustered at the province level in our preferred specification, since health districts are not sufficiently high in number to employ the law of large numbers and make correct use of clustered standard errors. Provinces serve as a good proxy for health districts, while also being suitably high in number. Clustering at the health districts level does not change the results of our estimates.

[10]The usual fuzzy RD is not the appropriate identification strategy in our case, as it assumes that *control* group applicants that receive the pension receive it for the same period as *treatment* group applicants (i.e. four years). With dynamic treatment effects, obtaining a pension in year $a + t$ (with $0 < t < 4$) does not have the same effect on the outcome in year $a + 4$ as obtaining the pension at the first application in year a would have. To obtain the ITT effect, Cellini, Ferreira, and Rothstein (2010) use the entire distribution of the running variable and control for the conditional expectation of the unobserved determinants of outcome given the running variable, by including a high-order polynomial of the running variable. Instead, we obtain the (local) ITT effect by focusing on a small window around the cut-off, as in the paper by Taylor (2014).

applicant i in year $t-s$ and controlling for the outcome of all successive applications (as though subsequent applications were not allowed). In Equation (3.2), this is β_s.

When deriving the TOT, it is important to clarify its relationship with the ITT effect in Equation (3.1). While the TOT is the effect of granting a pension for s years versus not receiving the pension at all for s years, the ITT is the effect of exogenously granting a pension in the first application and allowing unsuccessful applicants to apply again as they wish, potentially obtaining the pension at a later time. Thus, the ITT effect incorporates the effects of $D_{h,t-s}$ operating through the intermediate variables $\{D_{h,t-s+1}, ..., D_{h,t}\}$. The relationship between the ITT effect of $D_{h,t-s}$ on outcome $y_{i,h,t}$ and the corresponding TOT effect is:

$$\begin{aligned}\beta_s^{ITT} = \frac{dy_{i,h,t}}{dD_{h,t-s}} &= \frac{\partial y_{i,h,t}}{\partial D_{h,t-s}} + \sum_{j=1}^{s} \left(\frac{\partial y_{i,h,t}}{\partial D_{h,t-s+j}} \times \frac{\partial D_{h,t-s+j}}{\partial D_{h,t-s}} \right) \\ &= \beta_s^{TOT} + \sum_{j=1}^{s} \beta_{s-j}^{TOT} \pi_j \end{aligned} \tag{3.3}$$

where $\pi_h = \frac{dD_{h,t-s+j}}{dD_{h,t-s}}$ represents the effect of a successful first application on the probability of another successful application j years later. Since only those applicants who had a rejected first application will go on to apply again, we have $\pi_j < 0$ for all j. If $\beta_{s-j}^{TOT} \leq 0$ for all j years, this implies that $\beta_s^{TOT} \leq \beta_s^{ITT}$.

As in Cellini, Ferreira, and Rothstein (2010), the identification of the TOT effects from Equation (3.3) is based on the assumption that the partial effect of a successful application in one year on outcomes in some later year depends only on the elapsed time (s) and not on the application history or the application year. Formally we assume that, although $\frac{\partial y_{i,h,t+s}}{\partial D_{h,t}}$ and $\frac{\partial D_{i,h,t+s}}{\partial D_{h,t}}$ may depend on s, they do not depend on application year or application history $\{D_{h,1}, ..., D_{h,t-1}, D_{h,t+1}, ..., D_{h,t+s-1}\}$. This is a more restrictive condition than the monotonicity and excludability assumptions required by a standard fuzzy RD, because the TOT effects of the pension within a certain period are assumed to be the same between those applicants successful at their first application and those successful at a later application. This would be violated if, for instance, conditional on control variables, serial applicants benefited more (or less) from the basic pension than first-time applicants. The assumption is not required to identify the ITT effects.

To obtain recursive formulas for the TOT effects in terms of β_t^{ITT} and π_t for all t, we can simply invert Equation (3.3):

$$\beta_t^{TOT} = \beta_t^{ITT} - \sum_{j=1}^{t} \pi_j \beta_{t-j}^{TOT} \tag{3.4}$$

The recursive estimator thus proceeds in two steps. First, we estimate the coefficients β_t^{ITT} and π_t using regression Equation (3.1) for each year $t \in \{1, 2, 3, 4\}$.[11] Second, we solve for β_t^{TOT} using recursive Equation (3.4) and obtain its standard error by the delta method.

3.3.4 Descriptive statistics

Appendix Table C.3 reports descriptive statistics for applicants within 500 score points of the cut-off and at the moment of their first application, as well as for their working-age and elderly household members. There are 8,499 applicants in this bandwidth, representing 17.2% of the entire pool of 49,552 applicants.

This table shows that in our bandwidth 87.1% of applicants are female, which is the result of women being less likely to have a contributory pension. The average applicant's age is around 66.8. This suggests that applications are submitted shortly after reaching the minimum application age (75% of applicants are 65 years old) and that we observe the first application ever made for most of our sample. Regarding the typical household composition, the average applicant lives with at least one working-age household member and one elderly male person.

Pension applicants in the bandwidth are on average below the 40th percentile of the social security score distribution, which corresponds to 10,320 social security score points, an indication that applicants are poorer than the median Chilean.[12] Even though the pension score cut-off is set at the 60th percentile of the distribution, the average social security score for applicants close to the cut-off is well below the 60th percentile. This is not surprising, as the pension score considers a more comprehensive set of factors and sources than the social security score (see Section 3.2).

[11] To estimate π_t, we can use regression Equation (3.1) after replacing $y_{i,h,s+t}$ with $D_{h,s+t}$. Ideally we would estimate the ITT and TOT effects for each month after the first application. However, determining the standard errors in the TOT estimation becomes too computationally demanding.

[12] It is unlikely that the basic pension affected applicants' eligibility for other government transfers. To the best of our knowledge, there are three government transfers that could be received by pension recipients' households aside from the basic pension: the rent subsidy 'Subsidio de arriendo de vivienda', the home renovation incentive program 'Programa de Protección al Patrimonio Familiar' and the household allowance 'Asignación Familiar'. The latter is provided to households whose main worker's monthly income is below 1,574 dollars (765,550 Chilean pesos). The basic pension does not affect eligibility for this, as the pension is by definition not received by a worker. The other two are provided to households with a social security score below the sixth decile of the social security score distribution (13,484 score points). While the basic pension can affect the social security score, it is unlikely to affect the eligibility for these two transfers, as our applicants are likely to be infra-marginal. Applicants at the cut-off have a social security score of 9,385 score points, which is around the third decile of the social-security-score distribution. The basic pension would not be sufficient to push applicants' income above the eligibility cut-off for the first two schemes in 2012.

3.4 RD validity

3.4.1 First stage

Panels 3.1a to 3.1d of Figure 3.1 display the probability of receiving a basic pension as a function of the distance of the first-application pension score from the cut-off, within each year following the first application (the 'First Stage'). Virtually all applicants in the bandwidth with a score below the cut-off in their first application (treatment group) received a basic pension in every year following the first application. Conversely, relatively few applicants in the bandwidth with a score above the cut-off in their first application (control group) received a basic pension during the year following the first application, but over the following years, gradually more applicants received the pension.[13] Panel A of Table 3.1 shows that treatment group applicants have a 78.5 pp. higher probability of receiving a basic pension within the first year, which falls to 42.7 pp. within four years following the first application. This dynamic first stage translates into treatment group applicants receiving pension payments for 2.42 more years than control group applicants.

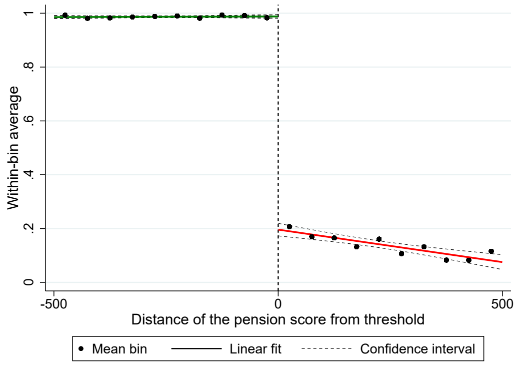
Panel B of Table 3.1 shows that being in the treatment group increases average monthly pension income by USD 103 and total income by USD 102 over the four years following the first application (27% of the minimum wage).[14] In the last two panels of Figure 3.1 we see that an applicant's total income increases below the cut-off because the pension income is constant and non-pension income is positively correlated with the pension score, but decreases above the cut-off because the decrease in average pension income dominates the increase in non-pension income.

[13] Control group applicants who re-submit an application tend to be those with a lower social security score and those who live in larger households (Appendix Section C.3).

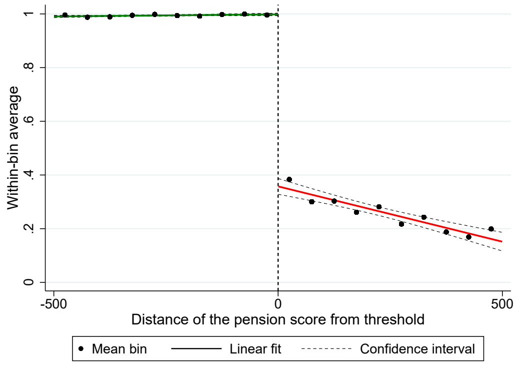
[14] For these estimates, we use only data from applications in 2012 as we do not have non-pension income data for applications in 2011 (see Section 3.3). Results on pension income remain very similar if we use data from applications in 2011. The monthly pension income increase is lower than the basic pension amount ($166) because 42.7% of control applicants obtain the pension at a later application and because, on average, pension recipients receive the pension 2.4 months after their first successful application. This reduces their monthly pension income over four years, since pension payments are divided over 48 months. An applicant's total income includes both pension and non-pension income and takes into account the full trajectory of pension payments. As we do not observe the full trajectory of non-pension income over the four-year period after applying (we have access to non-pension income only at the moment of application), we assume that non-pension income remains stationary in real terms at its 2012 level (nominally changing with the inflation rate).

FIGURE 3.1: First-stage effects.

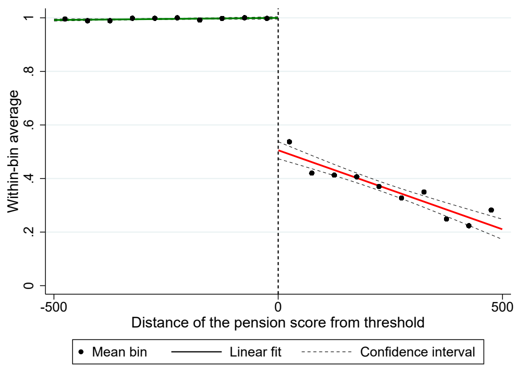
(A) Received a pension within 1 year



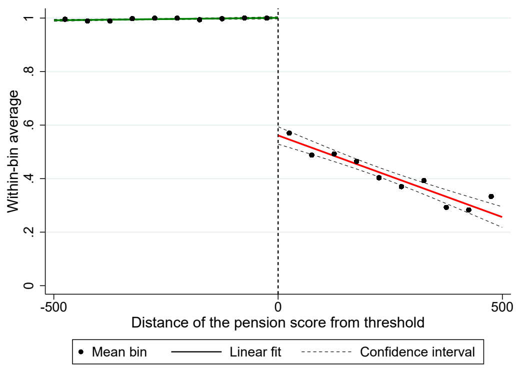
(B) Received a pension within 2 years



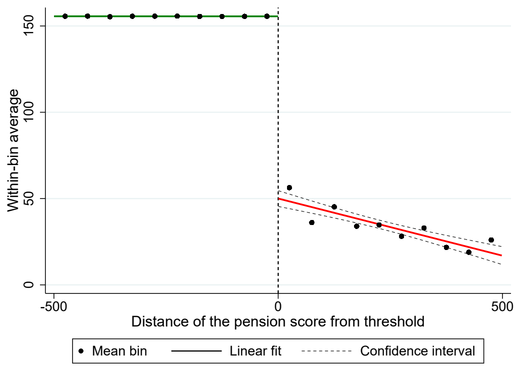
(C) Received a pension within 3 years



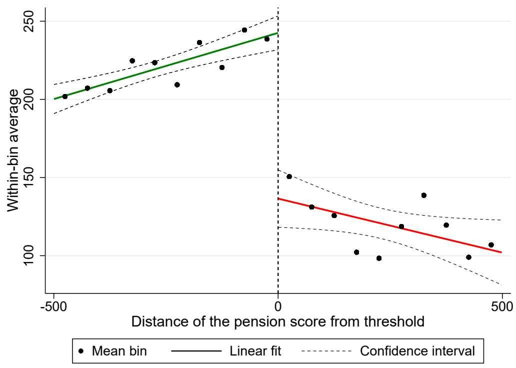
(D) Received a pension within 4 years



(E) Pension income



(F) Applicant's total income



Notes: These figures show the effect of the first-application pension score distance from the cut-off on the applicant's probability of receiving a basic pension within each year following their first application and the applicant's pension and total income. Income estimates are performed on applicants for 2012. The circles are averages across 50-point bins on either side of the threshold, while the solid and dashed lines represent the predicted values and confidence intervals, respectively.

TABLE 3.1: First stage on the probability of receiving a basic pension by year and on income

Variables	ITT Coef. (1)	S.E. (2)	t-stat (3)	P-value (4)	BW (5)	N (6)	Control (7)
Panel A: time of pension receipt							
Pension in the 1st year	0.785	(0.014)	56.486	0.000	500	8,499	0.203
Pension in the first 2 years	0.632	(0.017)	36.448	0.000	500	8,499	0.367
Pension in the first 3 years	0.483	(0.018)	26.847	0.000	500	8,499	0.517
Pension in the first 4 years	0.427	(0.021)	20.387	0.000	500	8,499	0.574
Years receiving payments	2.419	(0.051)	47.132	0.000	500	8,499	1.356
Panel B: income change (only for 2012 applicants)							
Pension income (2012 USD)	103.640	(3.302)	31.386	0.000	500	4,066	51.958
Total income (2012 USD)	102.148	(10.558)	9.675	0.000	500	4,066	141.062

Notes: This table reports results from OLS regressions of several dependent variables on a treatment dummy indicator and deviation of the pension score from the cut-off. In the first four rows, the dependent variable is a dummy indicator equal to 1 if the applicant received the basic pension within a particular year after their first application. In the fifth row, the dependent variable is the length of time in which the applicant received pension payments within four years from the first application. In the sixth and seventh rows the dependent variables are applicant's monthly average basic pension and total income within four years from the first application, respectively. Income estimates use only applicants in 2012, since we only have non-pension income for them and at the moment of aplication, and are expressed in 2012 US dollars. Column (1) and (2) report the treatment indicator coefficient and its standard error clustered at the province level, respectively. Column (3) and (4) report the t-statistic and p-value of the treatment dummy indicator coefficient, respectively. Column (5) and (6) report the range of pension score points from the cut-off and the number of observations in the regression sample, respectively. Column (7) reports the variable mean for control applicants at the cut-off.

3.4.2 Continuity of applicants' density and pre-determined covariates

Identification of the treatment effect requires that applicants do not manipulate their first-application pension score in order to receive the basic pension. For instance, this assumption would fail if more motivated applicants, who happen to be healthier, are able to adjust their pension score to fall below the cut-off. To formally confirm the absence of first-application score manipulations, we use the density of applicants in 10 score-point bins as the dependent variable in Equation (3.1) (McCrary, 2008). The test does not reject the null hypothesis of no discontinuity in the density of applicants with a t-statistic of -1.019 and p-value of 0.309 (see Appendix Figure C.10).

Identification of the treatment also requires comparable treatment and control groups in the RD design. Then, a series of pre-determined characteristics that could affect applicants' health should change smoothly at the cut-off (Lee and Lemieux, 2010). Appendix Figures C.11 and C.12 graphically shows that pre-determined covariates vary smoothly at the cut-off for applicants. Column (3) of Table 3.2 reports the results of the t-test performed on the coefficient β_t^{ITT} in Equation (3.1) (without controls), using as a dependent variable one of the 11 individual and household characteristics at the time of application.

TABLE 3.2: Balancing tests

Variables	ITT Coef. (1)	S.E. ITT (2)	t-stat (3)	P-value (4)	BW (5)	N (6)	Control (7)
Female	-0.016	(0.015)	-1.016	0.314	500	8,499	0.890
Age (years)	-0.372	(0.236)	-1.578	0.121	500	8,499	67.57
% days hospitalized	-0.096	(0.071)	-1.344	0.185	500	8,499	0.248
Influenza vaccination	-0.025	(0.020)	-1.281	0.206	500	8,499	0.357
Pneumonia vaccination	0.017	(0.008)	2.019	0.049	500	8,499	0.043
Household size	-0.008	(0.040)	-0.192	0.849	500	8,499	2.634
Social security score	64.69	(181.386)	0.357	0.723	500	8,499	9737
Elderly relative	0.016	(0.018)	0.872	0.387	500	8,499	0.693
Working-age relative	-0.004	(0.018)	-0.214	0.832	500	8,499	0.548
Child under 16	0.002	(0.004)	0.396	0.694	500	8,499	0.006
Municipal income	-2.465	(4.250)	-0.580	0.564	500	8,483	146.7

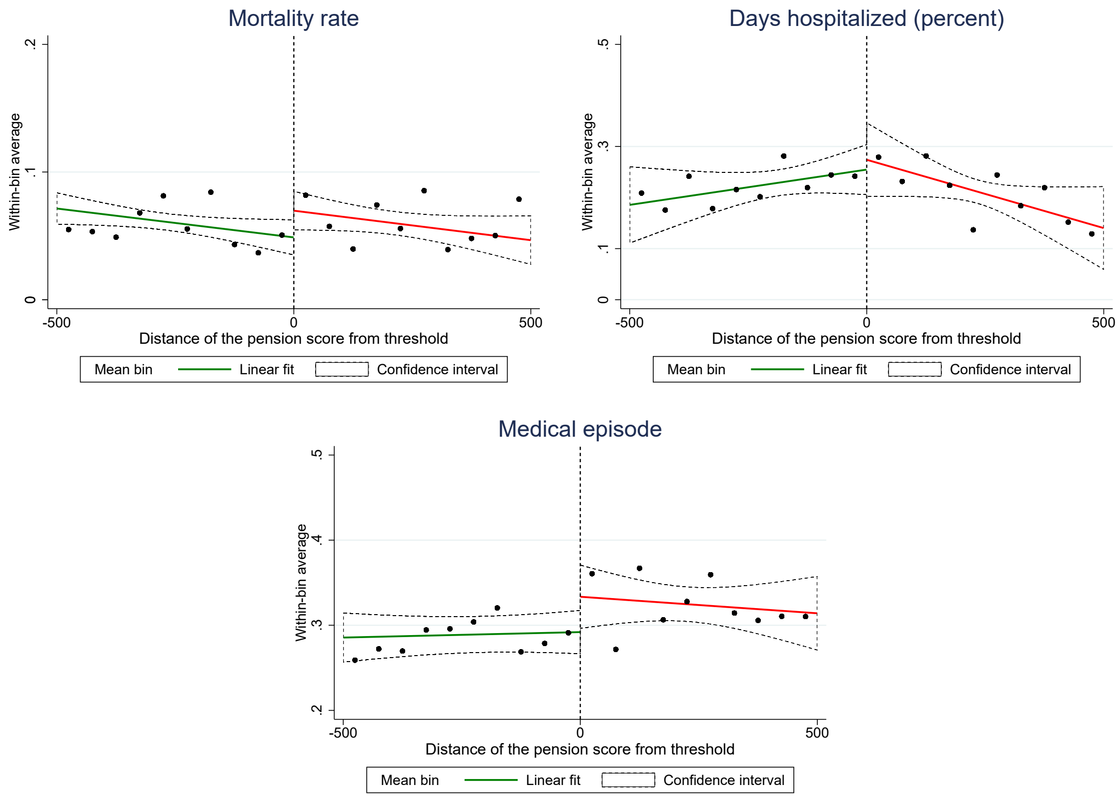
Notes: This table reports results from OLS regressions of pre-determined variables on a treatment dummy indicator and deviation of the pension score from the cut-off. Columns (1), (2), (3), and (4) report the treatment indicator coefficient, its standard error clustered at the province level, t-statistic, and p-value, respectively. Columns (5) and (6) report the range of pension score points from the cut-off and the number of observations in the regression, respectively. Column (7) reports the variable mean for control applicants at the cut-off. Health covariates are computed for the 6 months before applying.

This table confirms the results and shows that only 1 out of the 11 estimations (pneumonia vaccinations) is significant at conventional levels. We do not believe that this represents a systematic difference between treatment and control groups around the cut-off, however we do include this variable among the controls in the main specification. Performing these regressions as seemingly unrelated regressions, we cannot reject the hypothesis that the coefficients are all equal to zero. For the covariates used to calculate the pension score, Appendix Table C.4 shows that only 1 out of the 14 estimates (imputed income) is significant at the 10% level. The evidence presented above suggests that the basic pension is as good as (locally) randomly assigned around the cut-off, after conditioning on first-application pension score.

3.5 Results

3.5.1 The effect of receiving a pension on applicants' health

FIGURE 3.2: Effect of the basic pension on mortality, percentage of days hospitalized and medical episodes



Notes: Each graph shows the average value of the corresponding variable conditional on the distance of the pension score from the cut-off. The circles represent averages across 50-point bins on either side of the threshold, while the solid and dashed lines represent the predicted values and confidence interval, respectively.

The top left panel of Figure 3.2 shows the causal effect of receiving a basic pension from the first application on the probability of dying within four years after applying (henceforth *mortality*). This panel indicates that applicants in the treatment group were less likely to die within four years of applying than applicants in the control group. Column (1) of Table 3.3 confirms this result and shows that receiving a basic pension significantly decreases the probability of dying by 2.7 pp. The ITT effect of the pension is a 2.0 pp. reduction (p-value=0.045) in the probability of dying from a baseline mortality at the cut-off of 7.0 pp.

TABLE 3.3: Applicants' health outcomes over four years from application

Variables	TOT (1)	S.E. TOT (2)	ITT (3)	S.E. ITT (4)	P-value (5)	BW (6)	N (7)	Control (8)
Mortality rate	-0.027	(0.013)	-0.020	(0.010)	0.045	500	8,499	0.070
% days hospitalized	-0.042	(0.066)	-0.006	(0.051)	0.905	500	8,499	0.274
Medical episode	-0.060	(0.024)	-0.039	(0.017)	0.025	500	8,499	0.333

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (3.1). Column (1) reports the *treatment on the treated* coefficient as in Equation (3.4) and Column (2) reports its standard error computed using the delta method. Column (3) reports the *intent-to-treat* coefficient and Column (4) reports its standard error clustered at the province level. Column (5) reports the p-value of the ITT coefficient reported in Column (3). Column (6) reports the range of pension score points from the cut-off and Column (7) reports the number of observations in the regression. Column (8) reports the variable mean for control applicants at the cut-off.

Appendix Figure C.17 suggests that the mortality effect manifests itself approximately one year after the first payment and grows almost monotonically over time, reaching a maximum at the end of the studied period.[15] This can have relevant policy implications in the context of a middle-income country: increasing income can improve the health of the elderly, even at a late stage in life.

Since the basic pension affects the probability of dying, we cannot estimate its causal effect on the raw number of days of hospitalization. To partially account for the survival bias, we divide the number of days of hospitalization by the number of days alive, excluding the final six months observed.[16] We find that the basic pension reduces the percentage of days spent in hospital, but the reduction is small and insignificant.

We summarize treatment effects on health outcomes by using as an outcome variable a dummy indicator equal to 1 if the applicant has either been hospitalized or died in the four years after applying (hereafter 'medical episode'). Column (1) of Table 3.3 shows that treated applicants are 6.0 pp. less likely to experience a medical episode in these four years, a result that it is statistically significant and not affected by the survival bias.

Appendix Section C.5 shows that results on mortality and medical episodes remain significant when using different specifications and bandwidths. When including all available controls, the p-values are slightly higher, but the effects remain significant. Also,

[15]Appendix Figure C.19 confirms that the impact on mortality does not appear in the first year after the application, but rather becomes evident from the second year.

[16]The raw number of days of hospitalization for applicants on each side of the cut-off is not comparable, as those above the cut-off have fewer days available to be hospitalized due to their higher mortality rate. The survival bias would mechanically increase the point estimate (attenuation bias). Dividing the number of days in hospital by the number of days alive partially corrects the survival bias, as it compares shares rather than absolute numbers. Excluding the last six months observed prevents this variable from simply becoming an indicator of mortality.

these effects are well powered according to the approach by Gelman and Carlin (2014) and do not seem to appear in other parts of the pension score distribution.

3.5.2 Discussion on the mortality effect

Tables 3.1 and 3.3 show that the basic pension increases recipients' income by 72.4% (102/141) and reduces their mortality by 28% (0.020/0.07), respectively. Therefore, we estimate an ITT income-mortality elasticity of -0.386, which represents the percentage change in mortality over four years due to a 1 percent increase in income at the cut-off following a successful first application for the basic pension.[17]

Figure 3.3 shows that the confidence interval of our estimate encompasses all the negative income-mortality elasticity estimates obtained from previous papers. Our point estimate is slightly below the median negative elasticity estimated in the literature.[18] These include estimates for different countries and historical periods, such as Russia and Mexico in the late 1990s (Barham and Rowberry, 2013; Jensen and Richter, 2003), the United States in the 1900s (Salm, 2011) and women in the United States in the 1970s (Snyder and Evans, 2006). Although our analyzed time span is limited by data availability, it is similar to those used in other income-mortality elasticity estimates.[19]

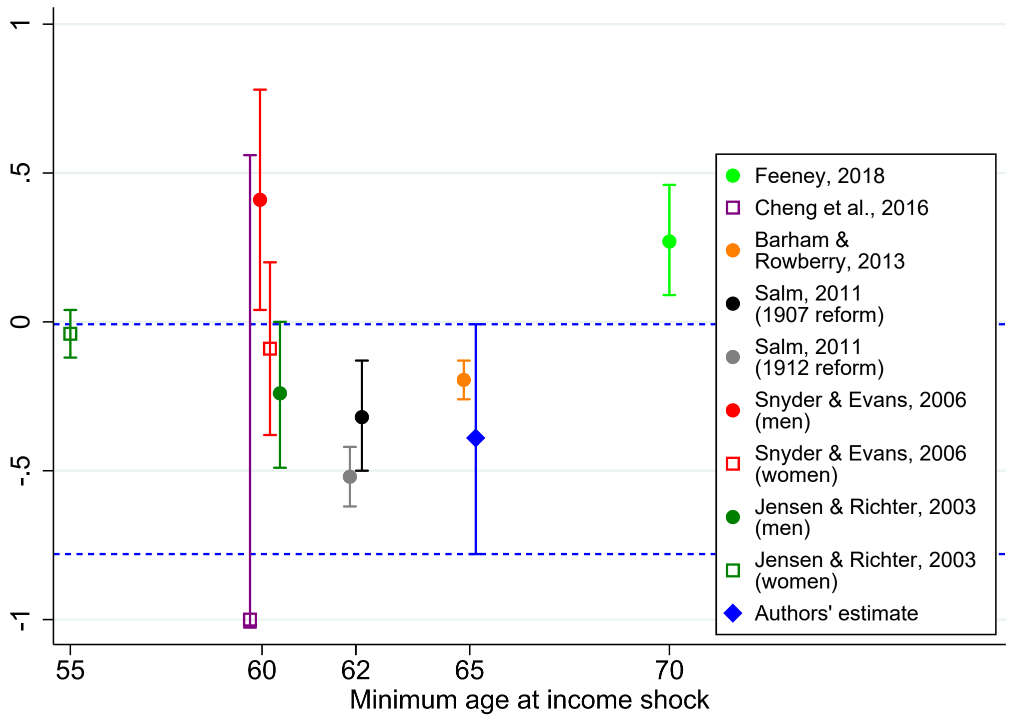
The positive estimates by Snyder and Evans (2006) for men and by Feeney (2018) are notable exceptions. Snyder and Evans (2006) estimate that a notch in US social security payments for the cohorts 1916-1917, which reduced the later cohort's income, significantly *reduced* men's mortality rates in comparison to the wealthier cohort. They justify this result by showing that the poorer cohort retired *later*, reducing their social isolation and improving their health outcomes. Feeney (2017, 2018) exploit the age eligibility cut-off and the staggered introduction of a Mexican non-contributory pension across small rural towns, finding that this pension increases recipients' transition to retirement and mortality rates.

[17] As mentioned earlier, the percentage change in income takes into account baseline non-pension income and the full trajectory of pension payments received by control and treatment group applicants in 2012. Ideally, we would compute the elasticity using the full trajectory of non-pension income as well. However, we have no information on how non-pension income changes after the application, and so we assumed that non-pension income remains stationary in real terms at its 2012 level.

[18] As the majority of estimates in the literature are based on an individual measure of income (Barham and Rowberry, 2013; Cheng et al., 2016; Salm, 2011; Snyder and Evans, 2006), we use the applicant's income to compute the income-mortality elasticity. We use the ITT estimate for consistency with the majority of the estimates in the literature.

[19] Snyder and Evans (2006), Feeney (2018) and Barham and Rowberry (2013) use comparable time spans, while Jensen and Richter (2003) and Cheng et al. (2016) use shorter periods. Salm (2011) is the only paper to analyze a period longer than four years.

FIGURE 3.3: Estimates of income-mortality elasticity of elderly



Notes: This graph plots point estimates and confidence intervals of income-mortality elasticity on the minimum age at which the income shock commenced. Empty squares indicate insignificant estimates. The dashed lines indicate the 95% confidence interval of our estimate. Elasticities in the other papers were computed using different measures of baseline income: Feeney (2018) household income; Cheng et al. (2016) average per capita net income among potential beneficiaries; Barham and Rowberry (2013) average beneficiary income in rural areas; Salm (2011) average monthly earnings for non-farm employees; Jensen and Richter (2003) household income; Snyder and Evans (2006) individual income. Where possible, estimates were separated by gender.

Differences in 'pre-pension' labor market participation levels can explain the opposite sign of our estimate. Basic pension applicants cannot have a history of formal employment (e.g. former stay-at-home mothers), and so arguably the pension induced very limited labor supply effects, as shown in Section 3.5.4 below. On the other hand, a high fraction of recipients in Feeney (2017), Feeney (2018), Snyder and Evans (2006) were workers induced to retire because of the income increase.[20] Fitzpatrick and Moore (2018) showed that the transition to retirement causes a significant jump in mortality due to the fall in labor supply, independently of whether income is affected. There is also

[20]Gelber, Isen, and Song (2016) studies the same pension notch as Snyder and Evans (2006) and also provides evidence of elderly labor supply responses to the pension increase.

evidence that transition to retirement is associated with changes in consumption patterns and lifestyles (Browning and Meghir, 1991; Fitzpatrick and Moore, 2018), along with social isolation (Snyder and Evans, 2006), and all of these factors are positively associated with mortality. Thus, our estimate can better isolate the negative mortality effect of the permanent income increase from the positive mortality effect of the increase in retirement.

3.5.3 The heterogeneous effects of receiving a pension on applicants

The pension may have different health effects depending on the recipient's characteristics. Appendix Table C.5 shows that the effects are significantly negative for female applicants and insignificantly positive for males. However, as males constitute a small fraction of our sample, the standard errors are too large to detect a statistically significant difference in the effects across gender.

Following the medical literature on aging and mortality, which stresses the importance of living arrangements (Garre-Olmo et al., 2013; Hawton et al., 2011), we explore another potential pattern of heterogeneity: the household structure of the applicants. Living with children can result in stronger financial assistance for the elderly (Shi, 1993) and affect their compliance with social and health norms (Manzoli et al., 2007; Rogers, 1996). Reciprocal support between children and parents can last throughout the entire lifespan.

TABLE 3.4: Applicant's health outcomes over four years from application by household structure

Variables	TOT (1)	S.E. TOT (2)	ITT (3)	S.E. ITT (4)	P-value (5)	BW (6)	N (7)	Control (8)
Panel A: applicants not living with a working-age household member								
Mortality rate	-0.055	(0.020)	-0.044	(0.015)	0.006	500	3,647	0.094
% days hospitalized	-0.157	(0.074)	-0.085	(0.042)	0.047	500	3,647	0.309
Medical episode	-0.128	(0.051)	-0.091	(0.039)	0.023	500	3,647	0.352
Panel B: applicants living with working-age household members								
Mortality rate	-0.007	(0.013)	-0.004	(0.010)	0.686	500	4,852	0.049
% days hospitalized	0.053	(0.111)	0.053	(0.081)	0.518	500	4,852	0.245
Medical episode	-0.007	(0.050)	-0.000	(0.035)	0.990	500	4,852	0.318

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (3.1). Column (1) reports the *treatment on the treated* coefficient as in Equation (3.4) and Column (2) reports its standard error computed using the delta method. Column (3) reports the *intent-to-treat* coefficient and Column (4) reports its standard error clustered at the province level. Column (5) reports the p-value of the ITT coefficient reported in Column (3). Column (6) reports the range of pension score points from the cut-off and Column (7) reports the number of observations in the regression. Column (8) reports the variable mean for control applicants at the cut-off.

Table 3.4 shows that treated applicants living without a working-age household member are strongly affected by the receipt of the basic pension, with a significant reduction in their mortality rate of 5.5 pp. The pension also significantly reduces their percentage of days spent in hospital by 0.157 pp., and the probability of a medical episode by 12.8 pp. Conversely, Panel B suggests that those living with at least one working-age household member remain unaffected by the receipt of the basic pension, with a small and insignificant reduction in their mortality rate and medical episodes. The difference between the coefficients in the two groups are statistically significant.[21] These heterogeneous results are in line with Cheng et al. (2016), who report significantly larger beneficial health effects for pensioners living alone or with a spouse than for pensioners co-residing with other adults.

The insignificant effects on applicants living with working-age relatives could be the result of working-age relatives reducing their net transfers of income to applicants after pension payments began, as has been suggested by previous papers (Cox and Jimenez, 1992; Juarez, 2009). This seems a plausible mechanism, considering that the fraction of Chilean elderly people approaching retirement age who expect transfers from their children to finance their retirement is twice as large for those who live with working-age household members than for those who do not (36% and 18%, respectively). [22]

Which diseases drive the effects?

Appendix Table C.9 shows that the effects appear to be driven mainly by a reduction in the probability of experiencing a medical episode caused by respiratory diseases or tumors.[23] Circulatory and digestive/nutritional medical episodes appear to play a less relevant role in health improvement for pension recipients, but we do not have sufficient power to find a significant difference with respect to the estimate for respiratory diseases in the sample of

[21] These subsamples appear to be locally comparable. First, test statistics for the McCrary test are -0.486 and -0.976 for applicants living with and without a working-age relative, respectively (Figure C.18). Second, applicants living with and without a working-age relative have a significant imbalance in 0 out of 10 and 1 out of the 10 pre-determined covariates, respectively (Table C.6). The mortality and medical episode results are robust to the use of different specifications (Appendix Tables C.7 and C.8), and remain significant at the 5% level when adjusting p-values by the number of hypotheses that we tested (Romano and Wolf, 2005b).

[22] The majority of people close to retirement age expect 'to finance their retirement with the help of the government': 50% of those who live with working-age household members, and 60% of those who do not. Transfers from children are the next most likely expected source of retirement income. These percentages are obtained using the 2004 and 2006 survey waves of the EPS survey (Ministerio Trabajo y Previsión Social, 2015), where we identify individuals who applied for the basic pension after 2008 and consider how they planned to finance their retirement.

[23] Applicants can have multiple causes for a medical episode. For instance, a person first hospitalized due to a respiratory disease and then due to a tumor would have both causes recorded for this analysis. The decrease in respiratory episodes does not appear to be driven by a significant increase in influenza or pneumonia vaccinations (Appendix Table C.10).

applicants living without working-age household members. As expected, the basic pension does not reduce the occurrence of medical episodes that are less directly connected to individual behavioral choices, such as transport accidents, although the probability of dying due to an accident is low in our sample.

3.5.4 Mechanisms behind the effects

Since our administrative data does not contain information on consumption, labor supply or health insurance coverage, we rely on survey evidence to shed light on the potential mechanisms underlying the estimated effects. We exploit the social benefits longitudinal survey (EPS) conducted by the Chilean Ministry of Labor (Ministerio Trabajo y Previsión Social, 2015), which is representative of the population aged 18 or older. We use the available EPS waves (years 2004, 2006, 2009, 2012 and 2015) which provide information on the respondent's age, health insurance affiliation, employment status, smoking and drinking habits, self-reported health, whether the respondent applied for and obtained the basic pension, and the number of times they had visited a health center in the last two years. We restrict the sample to the panel of 1,288 individuals who report to have applied for the basic pension between 2009 and 2015 and were sufficiently old to be pension recipients in 2015.[24] The EPS also consistently provides information about household income as well as monthly expenditures on food, clothes, utilities, transport, domestic services, medicine, and children's education. We then estimate the following fixed effect regression:

$$y_{i,s,a} = \alpha_0 + \beta \mathit{Pension}_{i,s,a} + \gamma_i + \gamma_s + \gamma_a + \varepsilon_{i,s,a}, \tag{3.5}$$

where $y_{i,s,a}$ is the outcome of interest for individual i in survey year s and at age a. $\mathit{Pension}_{i,s,a}$ is a dummy variable indicating whether individual i obtained the pension in survey year s and at age a. γ_i, γ_s and γ_a are individual, year-of-survey and age fixed effects, respectively. Standard errors are clustered at the individual level. β estimates the correlation of outcome y with receiving a basic pension after controlling for age, year of survey and unobserved time-invariant heterogeneity across applicants.

Panel A of Appendix Table C.11 shows that when applicants were aged 60-64 (henceforth future applicants) less than 2% had private health insurance. Furthermore, full basic pension payments were not sufficient to purchase the cheapest private health insurance

[24] This is an unbalanced panel, as some individuals have a missing value for some survey questions, or were not surveyed in some particular EPS waves.

plan available at that time.[25] The table also shows that less than 16% of future pension recipients spent at least one hour in informal work in the week prior to the survey.[26] Amongst future recipients, 77% had visited a health center in the last two years, but only 22% reported having bad health.

Panel A of Table 3.5 shows the results of the fixed effect regression analysis for variables measured at the individual level. The basic pension is not associated with a significant change in private health insurance coverage or employment status, which suggests that these factors play a minor role in the estimated mortality reduction. If anything, the basic pension income effect would be expected to incentivize retirement, and this should in turn *increase* mortality, according to the findings by Fitzpatrick and Moore (2018). On the other hand, the basic pension is associated with a significant increase in the probability of visiting a health center and in the actual number of health center visits during the preceding two years (by 6.62 pp. and 2.77 visits, respectively).[27] Since we estimate a negative insignificant impact on hospitalizations (Section 3.5.1), the increase in the number of visits to a health centre can be interpreted as an increase in outpatient care. The medical literature has shown that outpatient care is crucial in the prevention and treatment of most diseases, including respiratory diseases and tumors, and it is conducive to better health status and lower mortality (Rennard, 2004; Shi et al., 2005; Starfield, Shi, and Mackinko, 2005). Panel B also shows that monthly household expenditure on drugs increases by 26% with the receipt of the pension. Although this increase is not statistically significant, it could indicate that the basic pension enhanced adherence to medical treatment.

We also find an insignificant decrease in self-reported 'bad' health, although the high fraction of 'middle' responses for self-reported health (around 50%) provides little variation across survey waves and may be an indication of inaccurate reporting (Greene, Harris,

[25]According to the price comparison website 'Queplan' (https://queplan.cl/), in 2012 the cheapest private insurance plan in Chile had an average monthly cost of $175 for a 65-year-old and $218 for a 69-year-old. Moreover, public health insurance is free and available for all Chilean residents. Except for a few exceptions (e.g. members of the military force), every person without private health insurance is enrolled in the public system.

[26]The survey does not allow for distinguishing between formal and informal work (formal work being defined as a job eligible for mandatory social security payments). However, as a condition of eligibility for the pension, future pension recipients could not have been employed in formal work. We therefore interpret the fraction of future pension recipients doing at least one hour of work as the fraction of future pension recipients doing informal work.

[27]In waves 2004 to 2009, respondents are asked how many times they visited a health center in the past two years and to select from the reasons provided: general consultation, consultation with a specialist, consultation with a dentist, emergency, laboratory exam, X-ray examination, surgery, and hospitalization. In waves 2012 and 2015 there is only one general question asking how many times they had visited a health center in the last two years. We aggregate the 2004 and 2009 questions in a single variable and assume it is comparable to the generalized question in 2012 and 2015. Results are qualitatively unchanged if we use more restrictive definitions of visits to a health center for the 2004 and 2009 waves. The increase in medical visits is insignificant if we focus only on visits to a GP in the last two years.

and Hollingsworth, 2015).

Panel B of Table 3.5 shows that upon receiving the basic pension, both monthly household income and expenditure significantly increases by \$131 and \$115.6, respectively. The basic pension amount is slightly higher (\$166), but it remains within the 95% confidence interval of the estimated household income increases.[28]

[28]We find a marginal propensity to consume equal to 0.88 for recipients' households, which is on the higher end of the range of previous empirical estimates (0-0.9) and is in line with evidence that consumers with low liquid assets show stronger consumption responses to income shocks (Agarwal and Qian, 2014; Carroll et al., 2017).

TABLE 3.5: Fixed effect regressions for people who applied for the basic pension

Variables	Pension coefficient (1)	S.E. (2)	P-value (3)	Observations (4)
Panel A: individual level variables				
Private health insurance	-0.001	0.005	0.894	4124
Informal work	0.038	0.025	0.125	4166
Visited a GP	0.001	0.038	0.978	4217
Visited a health center	0.066	0.034	0.053	4199
Visits to health center	2.777	1.275	0.029	4199
Bad Health	-0.011	0.034	0.740	4217
Smoked, last month	-0.014	0.021	0.487	3509
Number of cigarettes, last month	5.303	5.233	0.311	3509
Drunk alcohol, last month	0.029	0.026	0.272	3509
Number of drinks, last month	0.193	0.176	0.272	3505
Panel B: household income and expenditure in 2012 US dollars				
Monthly income	130.501	44.561	0.003	4221
Total expenditures	115.568	51.787	0.026	4221
Food	25.805	10.966	0.019	4070
Clothes	7.280	3.350	0.030	4034
Utilities	64.486	49.446	0.192	4107
Transport	6.567	3.852	0.088	4037
Domestic services	0.448	0.930	0.630	4126
Drugs	7.077	4.491	0.115	3960
Children's education	6.314	3.039	0.038	4221

Notes: This table reports results from regressions of several dependent variables on a basic pension dummy indicator, as well as individual, survey wave and age fixed effects. Column (1) reports the basic pension dummy indicator coefficient. Columns (2) and (3) report the standard error, clustered at the individual level, and the p-value of the pension coefficient. Column (4) reports the number of observations used in the regression. 'Visited a health center' is a dummy variable equal to 1 if the individual had at least one appointment at a health center in the last two years. Income and expenditure variables are reported in 2012 US dollars. Total expenditures refers to the sum of the expenditures reported in the table. Data is from the panel survey conducted in 2004, 2006, 2009, 2012, and 2015 by the Ministry of Labor.

As in previous studies, the pension income increase is associated with a significant increase in household consumption of food (Duflo, 2000; Jensen and Richter, 2003; Salm, 2011), without a significant change in drinking or smoking habits (Cheng et al., 2016).[29]

[29]Data on drinking and smoking behaviors is not available for the 2012 wave. We also observe a large but imprecisely estimated increase in expenditures on utilities. The vast majority ($> 95\%$) of urban Chilean

Higher nutrient intake can improve the functioning of the immune and respiratory systems (Chandra, 1997; Hu and Cassano, 2000), and can also reduce the risk of developing tumors and help the elderly to sustain invasive tumor treatments, such as chemotherapy (Fiolet et al., 2018; Hurria et al., 2011). This is particularly relevant considering that low-income elderly adults in Chile show a high prevalence (40%) of food insecurity. Food insecurity is an index based on factors of insufficient food intake (e.g. going to bed hungry), insufficient food quality (e.g. low food variety), and anxiety and uncertainty about the food supply in the home (Atalah, Amigo, and Bustos, 2014).

Finally, we see that expenditure on children's education significantly increases with the beginning of pension payments. Appendix Section C.6 expands our RD analysis and provides additional evidence of spillover effects. The basic pension significantly increases the probability of having a child by 2.4 pp. for working-age household members and by 9.8 pp. for fertility-age women living with a pension recipient. On the one hand, the pension might have reduced the cost of raising children thanks to the help of more financially autonomous grandparents. On the other hand, since children can be considered as 'normal goods' (Becker, 1960), fertility ought to increase when higher income is available. Upon receiving the pension, recipients may have seen a reduction in transfers of income from their working-age relatives, as in Cox and Jimenez (1992) and Jensen (2003), or they may have transferred part of the pension amount to working-age household members, as in Duflo (2000).[30] In both cases, intra-household transfers of income between recipients and younger relatives could explain the presence of spillover effects on fertility and the absence of mortality effects on recipients living with working-age household members shown in Section 3.5.3.

3.6 Cost-benefit analysis

The estimated impact on mortality allows us to compute the basic pension cost that is necessary to increase the life expectancy of recipients and to compare it with the value of statistical life as estimated in the literature. For the basic pension program to pass a cost-benefit test in terms of life expectancy, the associated increase in the value of statistical life must exceed the monetary costs of the policy (Viscusi, 1994).

families already have access to electricity, potable water, and sewerage (División de Acceso y Desarrollo Social, 2019; Valenzuela and Jouravlev, 2007)). An increase in utilities may have been health conducive if, for instance, the basic pension was spent on heating during winter, but we are unable to test this hypothesis. Furthermore, less than 1% of households with a future applicant pay for a nurse to provide formal care, leaving little room for this as a potential mechanism.

[30] This last hypothesis would need to be reconciled with survey evidence showing that only 4% of pension recipients share more than one-fifth of their pension with others (Ministerio Trabajo y Previsión Social, 2017).

Table 3.6 shows that the basic pension increased recipients' life expectancy by around 4 months, and that it had an expected cost to government of $\$16,068$.[31] Assuming the life expectancy gain is linear in the government transfer, the cost to government for an additional year of life was $\$50,697$. To compare this with previous estimates of the value of statistical life, we multiply the cost by the average life expectancy for applicants close to the cut-off (20.09 years) and obtain a value of 1.01 million dollars. This is less than the value of statistical life at 62 estimated by Aldy and Viscusi (2008) for the US (5.02 million), and on the lower end of estimates for Chile, which range from 0.87 to 4.63 million dollars (Bowland and Beghin, 2001; Parada-Contzen, Riquelme-Won, and Vasquez-Lavin, 2013). Our analysis suggests that the basic pension was cost-effective in increasing the life expectancy of recipients close to the cut-off, as its cost was not higher than most estimates of the value of statistical life reported in the literature.

TABLE 3.6: Cost benefit analysis

Variables	ITT (1)	S.E. ITT (2)	t-stat (3)	P-value (4)	BW (5)	N (6)	Control (7)
Exp. lifetime income	16,068	(666)	24.12	0.000	500	8,499	15,070
Life expectancy	0.319	(0.146)	2.181	0.034	500	8,499	19.64

Notes: This table reports ITT effects on expected lifetime pension income (in 2012 US dollars) and expected life expectancy (including the observed four years since application date) on a treatment dummy indicator and deviation of the pension score from the cut-off. Columns (1), (2), (3), and (4) report the *intent-to-treat* coefficient, its standard error clustered at the province, t-statistic, and p-value, respectively. Columns (5) and (6) report the range of pension score points from the cut-off and the number of observations in the regression, respectively. Column (7) reports variable mean for control applicants at the cut-off.

3.7 Concluding remarks

Using a regression discontinuity design, this paper shows that permanently increasing the income of the elderly poor reduces their mortality rates within four years. In a longitudinal survey analysis, we find that the pension income increase is accompanied by an

[31]Life expectancy is measured by counting the observed years of life from the first application date until the observed date of death. If applicants are alive four years after the application date, we add the expected remaining years of life for their corresponding age-gender group in the Chilean population (Superintendencia de Pensiones, 2014). We assume that the expected years of life after the observed time span are the same for surviving pension recipients and for non-recipients, conditional on age and gender, and that the pension status remains unchanged. To measure expected cost, we multiply the pension amount received by ITT treatment and control applicants by the number of months that they receive the basic pension and are expected to live, discounted by an annual rate of 0.03. We cannot estimate the TOT effect, as we would need to estimate the probability of a successful application in each year after the first application, and data on successful applications after 2016 is not available.

increase in recipients' food consumption and visits to health centers. Both of these factors are relevant in improving health outcomes: higher nutrient intake can help to improve the functioning of the immune and respiratory systems, while also preventing tumor development and allowing people to better sustain invasive treatments; and visits to health centers, which could be interpreted as outpatient care, can improve overall health status and lead to decreases in mortality from several causes.

Consistent with previous papers, the beneficial effects of the pension are concentrated on pensioners living alone or with their spouse. The absence of working-age household members appears to be an important factor in financial fragility for the elderly, making the income shock particularly beneficial for this group of applicants. The insignificant impact on applicants living with working-age household members could be result of reductions in net transfers of income to pension recipients. Evidence of spillover effects on the fertility of working-age relatives further suggests the presence of intra-household transfers that could explain the heterogeneity of the results.

Our study provides evidence that health inequalities in the elderly population are driven in part by contemporaneous income inequalities. In a cost-benefit analysis, we also show that the basic pension is a cost-effective measure to increase life expectancy, as the costs to government are lower than the benefits in terms of value of statistical life. The key policy implication is that non-contributory pension programs, intended to improve the living standards of the elderly poor, can effectively improve their health, and this should be taken into account when similar policies are considered for implementation.

Chapter 4

The Equilibrium Effects of Taxing Property Investors: A Welfare Analysis

4.1 Introduction

Since the turn of the century housing prices have experienced a dramatic growth in almost all developed countries (Knoll, Schularick, and Steger, 2017). In the UK, housing prices increased by 240% relative to retail prices and by 220% relative to average earnings (Figure 4.1). At the same time, a wave of property investors flooded housing markets all over the world (Martin, Hulse, and Pawson, 2018). In the UK, buy-to-let mortgages rose from virtually none to almost 2 millions, the share of privately rented properties doubled from 10 to 20%, while home-ownership dropped.

The simultaneous increase in investors' entry and prices might be a spurious correlation and the direction of causality is unclear without a more careful analysis. Policymakers are concerned that investors might increase property prices, exacerbate housing cycles and crowd out owner-occupiers (Bank of England, 2016; De Nederlandsche Bank, 2018; HM Treasury, 2016; Reserve Bank of Australia, 2017; Reserve Bank of New Zealand, 2016). Yet, investors may also have a beneficial impact on welfare by increasing real-estate liquidity, stimulating supply and reducing rental prices (Bayer, Geissler, and Roberts, 2011; Gao, Sockin, and Xiong, 2020).

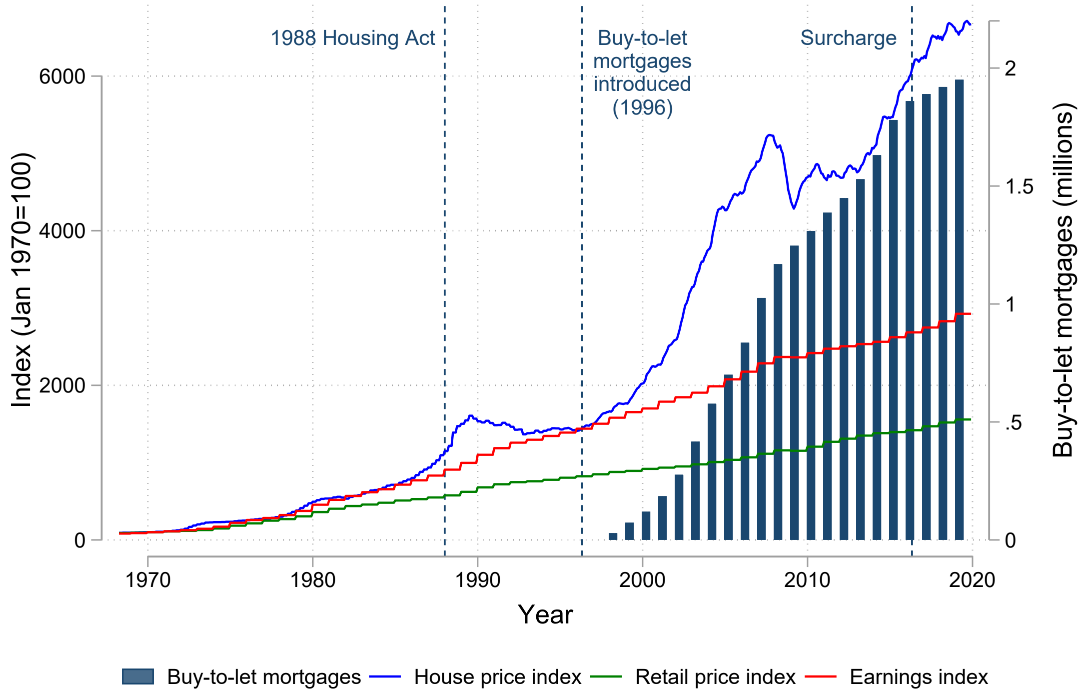
To shed light on the role of investors in the housing market, we need a large and exogenous demand shock, such as a tax, that directly affects property investors but not owner-occupiers. This paper asks whether a tax on property investors can increase total welfare by studying its spillover effects on owner-occupiers. I use an incremental difference-in-differences design and an equilibrium model with search frictions to evaluate a unique policy introduced in the UK in April 2016: a 3% transfer tax surcharge on 'buyers of additional properties'. Real-estate companies, investors that buy a property to let (buy-to-let) or to leave empty expecting its value will rise (buy-to-leave) are liable for this surcharge, whereas owner-occupiers (buy-to-live) are not.[1]

The surcharge was implemented nationally but there was large heterogeneity in the share of privately rented properties across local authorities. I exploit this pre-policy heterogeneity to identify the impact of the surcharge on housing market outcomes, using the incremental difference-in-differences estimator proposed by Card (1992). I use the local authority share of rented properties in 2015 as a measure of the 'dose' of the treatment, since the surcharge was paid by owners of rented properties (buy-to-let investors) but not by owner-occupiers. To conduct the empirical analysis, I build a dataset that contains the universe of property transactions in England and Wales from 2013 to 2019 by matching Land Registry sale records with Energy Performance Certificates and confidential data on properties listed on Zoopla, the second most popular UK property platform. The combination of these datasets is crucial for a comprehensive understanding of how the housing market reacts to the surcharge: it allows me to identify buy-to-let from buy-to-live transactions and it includes rich information on paid and listing price, as well as property characteristics and time to sell.

I find that the tax surcharge had large and significant effects that can hardly be reconciled with a frictionless and perfectly competitive housing market. The surcharge reduced pre-tax property prices for buy-to-let investors by 2.7% and for future owner-occupiers by 2.1%, even though the latter were generally not liable for the surcharge. The differential impact on pre-tax prices is prima facie evidence that buyers had some degree of market power. Sellers would not have accepted a lower price from buy-to-let investors, unless the search for a future owner-occupier (willing to pay a higher price) was costly. Moreover, I find evidence of overshifting: had all buyers been liable for the surcharge, the impact on prices would have been higher than the tax itself, with a tax elasticity of prices of -3.4. Overshifting is not compatible with a perfectly competitive and frictionless market (Fullerton and Metcalf, 2002), but can be explained by imperfect competition and the amplification mechanisms that search frictions generate in a property market. In line with this interpretation, data from Zoopla reveal that it takes seven months to sell a property for a median seller in England and Wales, and that the surcharge significantly increased time-to-sell by 4%. Longer time-to-sell can push sellers to accept lower prices or to opt out and induce buyers to wait for even lower prices, amplifying the initial effect of the surcharge. Consistent with this mechanism, the volume of transactions fell by

[1] A property purchased by a future owner-occupier is not subject to the surcharge, unless the buyer owns a second property and does not sell it within 18 months from the transaction. Second homes were only 1.2% of total homes in England in 2015.

FIGURE 4.1: The growth of housing prices and buy-to-let mortgages in the UK



Notes: The blue, red and green solid lines are the house price index, earnings index and retail price index from 1968 to 2019, normalized to 100 in year 1970. The histogram represents the number of outstanding buy-to-let mortgages (in millions) from 1998 to 2019.

10.2%, even though the surcharge did not reduce housing supply within four years from its introduction.

The magnitude of these results is large but comparable with previous estimates of transfer-tax elasticities on volumes and prices (Besley, Meads, and Surico, 2014; Best and Kleven, 2018; Han, Ngai, and Sheedy, 2021; Kopczuk and Munroe, 2015). Reassuringly, I do not find any evidence of significant effects in the eight quarters before the policy announcement which would invalidate the empirical strategy. Results are robust to the inclusion of a rich set of local authority controls and property characteristics and do not appear to be driven by other policies occurring in the same period (Section D.1), nor by the outcome of the Brexit referendum.

Guided by the empirical findings, I develop a search model of the housing market with buy-to-let investors, buy-to-live households and lenders to illustrate the mechanisms and quantify the impact of the surcharge on welfare. Households and investors compete in the same property market. Based on evidence of search frictions, the property market in the model is characterized by search costs and a matching function à la Pissarides (2000). Households and investors are heterogeneous in wealth and a fraction of them search in credit markets for buy-to-live and buy-to-let mortgages, respectively. Since access to credit plays a central role in the housing market, I introduce credit rationing by assuming credit markets for households and investors are subject to search frictions in a symmetric way with respect to the property market. It allows my framework to capture how credit market frictions contribute to the propagation of fiscal effects while maintaining model tractability. This is a common feature in search models that combine credit, labor and non-durable goods markets (Dell'Ariccia and Garibaldi, 2005; Haan, Ramey, and Watson, 2003; Petrosky-Nadeau and Wasmer, 2013, 2015; Wasmer and Weil, 2004) but was only recently introduced in housing market models by Gabrovski and Ortego-Marti (2021, 2023). Differently from Gabrovski and Ortego-Marti (2021, 2023) I also model the rental market and introduce investors as agents in the economy. To replicate the UK standard practice of reaching a mortgage 'agreement in principle' before bargaining for a property (Lloyds Bank plc, 2022), loan amounts and property prices are negotiated in a sequential Nash bargaining process that maximizes the total surplus of borrowers-lenders and buyers-sellers, respectively. Given that the observed median time-to-let is less than one tenth of the median time-to-sell, I assume that the rental price instantaneously clears a frictionless rental market.

The tractability of the model allows me to analytically identify three equilibrium effects through which the surcharge on investors can make housing more affordable for owner-occupiers, despite not being directly affected by the tax change. First, the surcharge reduces the number of buy-to-let investors and it makes it easier to find a property to buy. This favors buyers over sellers and reduces prices. Second, since households and investors find properties to buy more easily, rental demand decreases relative to supply. The resulting fall in rental prices generates further downward pressure on property prices. Third, in the long run construction cost decreases to adjust to a less tight housing market with lower prices and this has a negative feedback effect on prices themselves. Interestingly, if the tax on investors becomes too high, the effects are reversed. Not enough buy-to-let investors enter the market, rental prices increase and this induces too many households to search for a property to buy. Finding a property to buy becomes more difficult and property prices for owner-occupiers rise.

I estimate model parameters using pre-surcharge data and the 4% increase in days-to-sell after the surcharge. Qualitatively, the model is able to replicate all the empirical effects. Quantitatively, it captures the magnitude of the effects on transactions, housing supply and rental prices reasonably well. It overestimates the effect on prices in order to match the observed change in housing market tightness, in a similar manner labor search models require large changes in wages to generate the magnitude of observed fluctuations in employment (Shimer, 2005).[2] As prices are transfers between equally weighted risk-neutral individuals, price effects do not directly affect the welfare analysis. Using a utilitarian welfare function that weighs the utility of households, investors and lenders equally, the model shows that the transfer-tax surcharge on investors increased overall welfare by 2.3%. Investors do not internalize the negative externality they impose on households: their competition in the property market makes it longer and more expensive to find a property to buy, reducing the home-ownership rate. As I estimate that households have higher intrinsic home-owning utility than investors, the surcharge on investors increases welfare because it partially offsets this negative 'crowding-out' externality. Households are the beneficiaries of the welfare increase, as they are exempt from the tax surcharge, they pay lower rental prices and find more easily a property to own which gives them positive utility.

My paper contributes to the literature in several ways. While previous papers have analyzed the impact of transfer taxes that target all buyers unconditionally, little is known about the housing market response when the tax targets those who do not purchase properties for consumption, but only for investment.[3] Whereas previous papers find that unconditional transfer taxes lead to welfare losses due to lock-in effects and destruction of matches with positive surplus (Best and Kleven, 2018; Dachis, Duranton, and Turner, 2012; Eerola et al., 2021; Fritzsche and Vandrei, 2019; Han, Ngai, and Sheedy, 2021; Hilber and Lyytikäinen, 2017; Kopczuk and Munroe, 2015; Määttänen and Terviö, 2021), my paper shows that a moderate transfer tax can increase social welfare if it is imposed on property investors only.

Following the seminal work by Wheaton (1990), several papers have used search models to study frictions and amplification effects in the housing market (see Han and Strange (2015) for a review).[4] Few papers focused on the role of investors. Halket and Custoza (2015), Ioannides and Zabel (2019) and Bø (2021) build search models with property

[2] Different papers have advanced different solutions to this 'unemployment volatility puzzle'. Ljungqvist and Sargent (2017) show that all these ultimately diminish the fundamental surplus fraction, an upper bound on the fraction of a job's output allocated to the vacancy creation (e.g. the difference between productivity and worker's value of leisure).

[3] An exception is the empirical analysis by Fu, Qian, and Yeung (2016) on the withdrawal of a stamp duty deferral in the presale market in Singapore, which reduced speculative trading but raised price volatility. While they study short-term investment in a presale market, my paper analyzes the impact of taxing long- and short-term investors on the spot market.

[4] Recent examples are Albrecht, Gautier, and Vroman (2016), Anenberg and Bayer (2020), Díaz and Jerez (2013), Gabrovski and Ortego-Marti (2019), Genesove and Han (2012), Head, Lloyd-Ellis, and Sun (2014), Moen, Nenov, and Sniekers (2021), Ngai and Tenreyro (2014), Ngai and Sheedy (2020), Piazzesi, Schneider, and Stroebel (2020).

and rental markets, but to study different questions (e.g. the relationship between home-ownership and rent-to-price ratio). Lundborg and Skedinger (1999) analyze the effect of transfer taxes on search effort but they abstract from the rental market. Closer in spirit to my paper, Han, Ngai, and Sheedy (2021) find that a land transfer tax in Toronto induced a rise in buy-to-let transactions but a fall in owner-occupiers transactions, despite the tax applying to both. Contrary to my analysis in which owner-occupiers are exempt from the surcharge, this unconditional transfer tax increased the share of investors in the housing market, reduced home-ownership and caused large deadweight losses. My empirical and normative results have first-order relevance for policymakers because increasing home-ownership and decreasing property prices without discouraging housing supply is among the main objectives of current housing policies around the world.

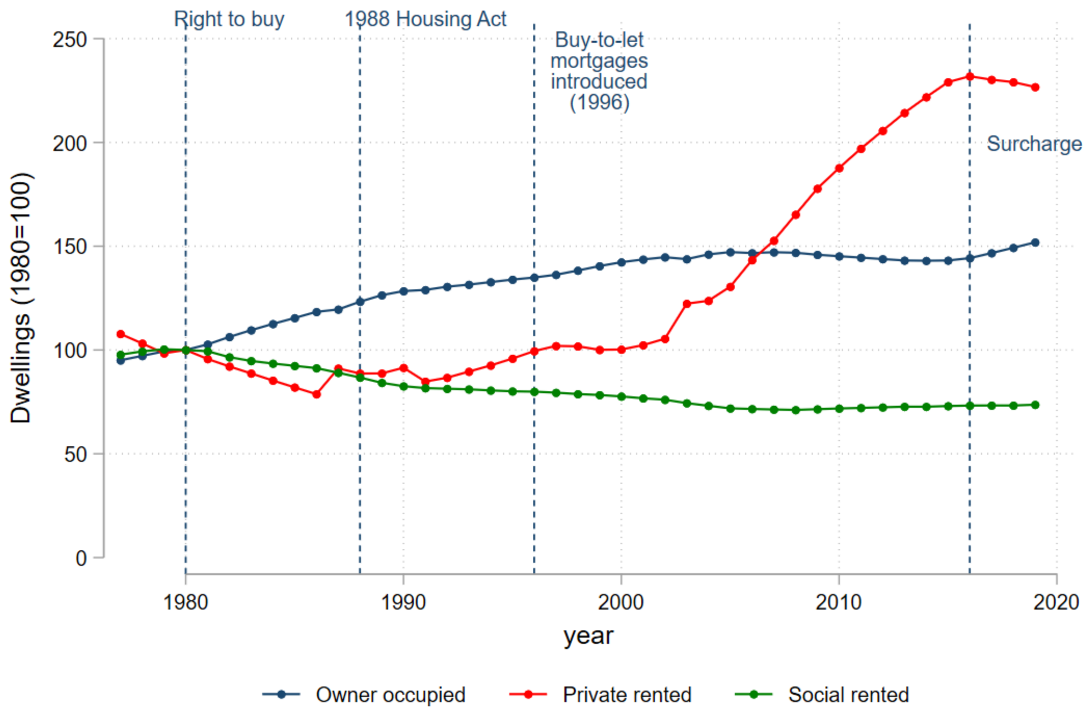
The paper is organized as follows. Section 4.2 presents the policy background of the surcharge and describes the data used to analyze its impact. Section 4.3 explains the empirical strategy and Section 4.4 discusses the empirical results. Section 4.5 illustrates the search model and analyzes the comparative statics of introducing a surcharge on investors. Section 4.6 describes the identification and estimation of model parameters. Section 4.7 validates the model and analyses the impact of the surcharge on social welfare. Section 4.8 concludes.

4.2 Policy background and data

A series of changes in the rental and credit markets triggered the explosion of buy-to-let investment in the UK. Until 1988, tenants could appeal to rent officers to obtain a 'fair rent' and the 1977 Rent Act ensured a long-term security of tenure and restricted landlords' powers of eviction (Kemp, 2015). The Housing Act 1988 liberalized the heavily regulated private rental sector. It allowed landlords to let properties at market rents, it reduced the minimum notice to evict to two months and the minimum tenancy tenure to six months (Housing Act, 1988).

Another push to the buy-to-let sector came from the credit market. Before 1996, loans for properties bought to let were mortgages based on the mortgagor's income with an additional risk premium of around 2% with respect to standard mortgage interest rates. As a consequence, buy-to-let mortgages were rather uncommon. In 1996, a panel of mortgage lenders in concert with the Association of Residential Letting Agents devised the 'buy-to-let mortgage': a new mortgage product based on expected rent with an interest rate close to the standard one for residential mortgages (Leyshon and French, 2009). As a result, the number of outstanding buy-to-let mortgages raised from virtually none to almost 2 millions (Figure 4.1) and the share of privately rented properties doubled from 10 to 20% in two decades (Figure 4.2).

FIGURE 4.2: Number of dwellings by type of tenure



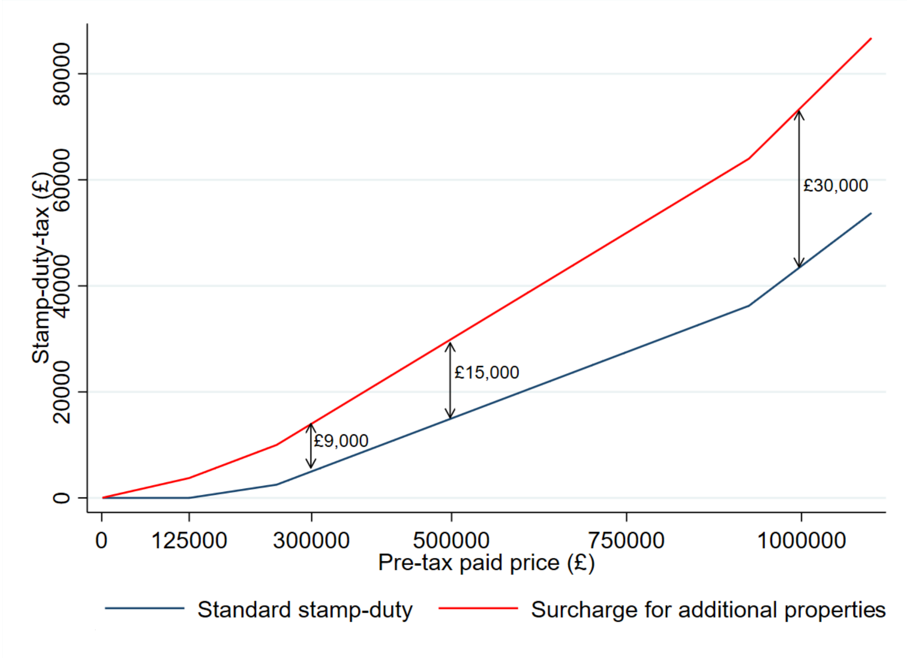
Notes: This Figure shows the number of dwellings that were owner-occupied, private rented and social rented in the UK from 1977 to 2019. Their number is normalized to 100 in 1980.

To contrast the fall in home-ownership, on November 25, 2015 Chancellor George Osborne announced a 3% surcharge for 'buyers of additional properties' on top of the standard Stamp-Duty Land Tax (SDLT), which is a transfer tax paid by every residential property buyer in England, Northern Ireland and Wales (Ministry of Housing, 2022b).[5] The surcharge was part of a Five-Point-Plan whose other points were: to deliver 400,000 affordable housing starts by 2020-21; to accelerate housing supply and get more homes built (e.g. by releasing public sector land); to prolong the already existing 'Help to Buy' Equity Loan scheme until 2021 and to create a London 'Help to Buy' scheme; to extend

[5]The SDLT became the 'Land and Buildings Transaction Tax' in Scotland from 1 April 2015 and the 'Land Transaction Tax' in Wales from 1 April 2018. In Wales, the tax schedule remained unchanged until December 2020.

the 'Right to Buy' scheme to Housing Association tenants (HM Treasury, 2015, 2016).[6] In Section D.1, I explain how I can isolate the effect of the surcharge from the other policies of the Five-Point-Plan and show that the main results stand when performing several robustness checks.

FIGURE 4.3: Stamp-duty schedule



Notes: This Figure shows the SDLT tax schedule in 2015 ('Standard stamp-duty') and how it increased for buyers of additional properties in 2016 ('Surcharge for additional properties').

As we can see in Figure 4.3, the SDLT schedule presents several kinks as the marginal rate increases in the transaction price, starting from 0% of the portion of the transaction price below $\pounds 125,000$ up to 12% of the portion of the transaction price above $\pounds 1.5m$ in 2016. The SDLT surcharge consists of an increase of 3 percentage points on the standard SDLT rates independently of the transaction price, but it applies only to buyers of additional properties. If the buyer owns more than one property after 18 months from the transaction, the surcharge applies. Accordingly, the SDLT surcharge applies to buy-to-let investors, buy-to-leave investors, real estate companies and second-home buyers, but does not apply to owner-occupiers. The diagram in Figure D.2 specifies the liability of the SDLT surcharge in more detail.

[6]The 'Help to Buy' Equity Loan is a government equity loan that covered from 5% to 20% of the property purchase price of a newly built home. The London 'Help to Buy' scheme covered up to 40% of the price if the property was in London. The 'Right to Buy' scheme allows tenants of public housing to buy their homes at a discount.

The SDLT surcharge helped the government raised a non-negligible amount of tax revenues: stamp-duty revenues increased from 9.3 billion pounds in 2015 to 14.8 in 2019 (Figure D.4). It was announced on November 25, 2015 but came into effect on April 1, 2016. As we can see in Figure D.3, the period between the announcement and the implementation of the reform saw an increase in the volume of property transactions. Some buyers of additional properties appear to have anticipated their planned property transactions in order to avoid the transfer tax increase. Section 4.4 explains how I account for these anticipation effects in the analysis of the housing market impact of the SDLT surcharge.

4.2.1 Data

To analyze the impact of the surcharge on the housing market, I have geocoded and merged three datasets: the HM Land Registry Price Paid data, the Energy Performance Certificates dataset and WhenFresh/Zoopla data provided by the Consumer Data Research Centre. The linking variable is the property address, which consists in the Primary Addressable Object Name (typically the house number or name), the Secondary Addressable Object Name (e.g. flat number), the street and the full postcode.

The Land Registry dataset contains the universe of residential property transactions occurred in the UK from 1995 to 2021. Each observation includes the property address, its coordinates, the transaction date, the price paid, and several property characteristics (e.g. whether the property is new/old, whether the property is a leasehold/freehold) (HM Land Registry, 2022b).

The Energy Performance Certificates dataset contains every energy performance certificate produced on sale or rent of a building in England and Wales from October 2008 to December 2021. Each certificate reports the property address, the certificate date, a richer set of property characteristics (e.g. floor area size, energy efficiency rate) and the type of tenure (private rented, public rented or owner-occupied) (DLUHC, 2022a).[7] To merge this dataset with the Land Registry dataset, I use the certificate that has the closest date *after* the property transaction date.[8] I identify transactions in which buyers are buy-to-let investors as properties that have an Energy Performance Certificate after the transaction

[7]The EPC register does not hold data for every residential building, but only for those buildings for which an energy performance certificate was required in the period 2008-2021. After September 2008, lodging the data became a mandatory requirement and a building must have a valid EPC when constructed, sold or let. An EPC is valid for 10 years.

[8]The algorithm for merging the Land Registry and EPC dataset was kindly shared by Hans Koster and Edward Pinchbeck. For details on this algorithm, I refer to their paper (Koster and Pinchbeck, 2022).

that classifies them as privately rented. I identify transactions in which buyers are future owner-occupiers as properties that have an Energy Performance Certificate after the transaction that classifies them as owner-occupied.[9]

The WhenFresh/Zoopla data includes information on all properties in England and Wales to sell and to rent listed on Zoopla in the period 2012-2019 and sold in the period 2014-2019. For each property, we can observe the listing dates for sales and lets, the listing price, the listing rental price, the transaction date, the starting tenancy date and additional property characteristics (e.g. listed number of bedrooms/batrooms)(Consumer Data Research Centre, 2020a,b).

I use several other datasets for the regression covariates at local authority levels. For population and GDP per capita in each local authority, I use annual estimates provided by the ONS (2021a,c,d). To account for the outcome of the Brexit referendum, I also control for the interaction between an indicator for the post-policy period and the population share with EU nationality in each local authority in 2015, which is obtained from the Annual Population Survey (ONS, 2015), and the share of properties owned by EU companies in each local authority in October 2015 (HM Land Registry, 2022a). For council total and housing expenditures, I use data from the DLUHC (2022b) and the Welsh Government (2021a,b).

Finally, for model calibration I also use aggregate UK annual data in 2015, the year before the surcharge was introduced. This includes administrative data on the housing stock, the housing flows, the vacant stock, the number of outstanding residential and buy-to-let mortgages provided by the Ministry of Housing (2014, 2022a) and the Council of Mortgage Lenders (2022).

4.3 Empirical strategy

The surcharge amounted to 3% of the price paid by property investors and was introduced in the whole UK simultaneously. Yet, local authorities in England and Wales presented a high and longstanding geographic variation in buy-to-let investment. As we can see in Figure 4.4, the share of properties that were privately rented in 2008-2015 ranged from 9.5% in the Welsh county borough of Torfaen to 50.4% in the City of London district. This variation implies that the surcharge affected local authorities to different intensities. The larger the private rental sector in a local authority, the stronger the ‘dose’ of the treatment

[9] This approach is different from Bracke (2021) who identifies buy-to-let purchases as transactions where a Zoopla rental advertisement follows a sale on the same property during the following six months. Bracke (2021) cannot identify properties purchased by future owner-occupiers: a transaction that is not followed by a Zoopla rental advertisement might still be a buy-to-let transaction (e.g. if the property is not advertised, or it is advertised in other platforms).

in that local housing market because buy-to-let investors had to pay the surcharge, whereas owner-occupiers did not.

My empirical analysis is based on the incremental difference-in-differences estimator introduced by Card (1992) and exploits the heterogeneous degree to which local housing markets are affected by the surcharge.[10] I use the share of private residential properties that were privately rented according to Energy Performance Certificate data from 2008 to 2015 in local authority j as a measure of the dose of the treatment. Then, I apply the incremental difference-in-differences estimator to analyse the impact of the surcharge on a range of housing market outcomes within four years from its introduction (2016-2019).

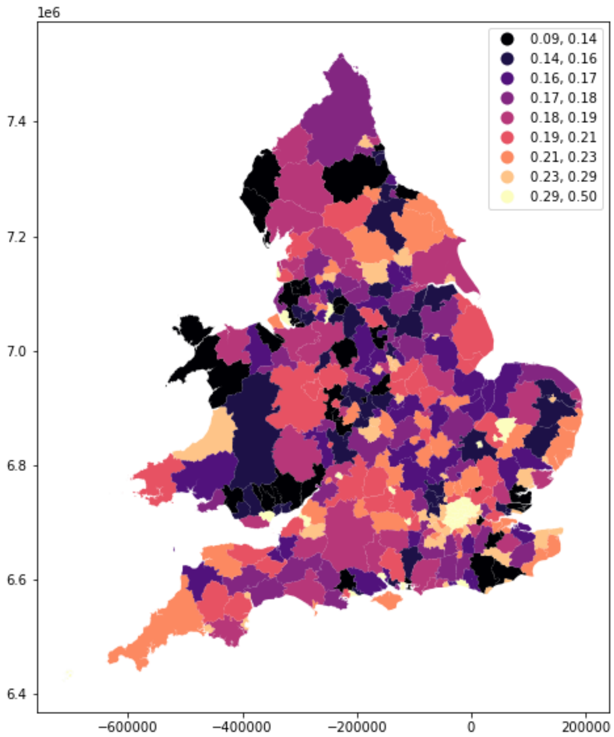
The choice of what constitutes a local housing market is open to discussion. Since local housing policies (e.g. council taxes) are determined at local authority level, the natural choice is to have local authorities as the geographical units. In 2011, there were 348 local authorities in England and Wales: 36 Metropolitan Districts, 201 Non-Metropolitan Districts, 31 London Boroughs and 54 Unitary Authorities in England, as well as 22 Unitary Authorities in Wales.[11]

I restrict the regression sample to properties *sold* in the period October 2013-December 2019, except when the dependent variable is the listing price, in which case I restrict the sample to all the properties *listed* on Zoopla in the same period. This is because the Land Registry includes buy-to-let mortgage transactions only from October 2013 and because stopping the analysis at the end of 2019 avoids potential confounding factors such as the withdrawal agreement from the EU formalized in January 2020 and the insurgence of the COVID-19 pandemic in February 2020. Since the Energy Performance Certificates and Zoopla datasets do not contain information on properties in Scotland and Northern Ireland, I also restrict the regression sample to England and Wales. I use standard errors clustered at local authority levels in all regressions. For regressions at property-level, which may be heavily affected by spatial correlation, I also allow for spatial correlation within $100km$ from the local authority using heteroskedasticity and autocorrelation-consistent standard errors (Conley, 1999).

[10] Callaway, Goodman-Bacon, and Sant'Anna (2021) call this estimator the 'dose-response' difference-in-differences estimator. See Dolton, Bondibene, and Wadsworth (2010) and Caliendo et al. (2018) for more recent applications.

[11] In 2019 some local authorities changed and new local authorities were created. Address geocoding allows to maintain the boundaries of the local authorities fixed at the 2011 boundaries throughout the entire sample period and to assign each property to a fixed spatial unit.

FIGURE 4.4: Share of private rented properties by local authorities in 2008-2015



Notes: This heat map of England and Wales shows the share of private rented properties by local authorities using Energy Performance Certificate data from 2008 to 2015. The legend reports the range of the share of private rented properties corresponding to each color.

For each property i, local authority j and quarter t, I estimate the following regression equation:

$$\begin{aligned} y_{ijt} = &\alpha_j + \eta_t + Post_t \cdot (\gamma Rented_{j,2015} + \xi Second_{j,2015} + \lambda London_j \\ &+ \iota EUcom_{j,2015} + \kappa EUpop_{j,2015}) + \omega X_{ijt} + \zeta Z_{jt-4} + \upsilon_{ijt} \end{aligned} \tag{4.1}$$

Equation (4.1) is estimated for housing market outcomes y_{ijt} at property transaction level. I use the same regression specification dropping the subscript i and property-level controls when the housing market outcome is at local authority level y_{jt}. In this regression, α_j are local authority fixed effects and η_t are quarter fixed effects. $Post_t$ is a binary variable equal to 1 for each quarter since the introduction of the surcharge and 0 otherwise, whereas $Rented_{j,2015}$ is the share of properties that were rented in local authority j in 2015. Note that the non-interacted variable $Post_t$ is captured by the quarter fixed effects η_t. γ is the parameter of interest that captures the outcome change for a local authority in which all properties were bought to let ($Rented_{j,2015} = 1$) with respect to a local authority in which all properties were owner-occupied ($Rented_{j,2015} = 0$). It can be interpreted as the change in housing market outcome y_{ijt} for a 3% surcharge on property investors if all properties in the local authority were bought to let.

To account for housing quality heterogeneity, I control for a rich set of property-level characteristics X_{ijt} which includes quadratics in latitude and longitude, type of property (detached, semi-detached, terraced or flat), an indicator for whether the property is new, leasehold/freehold, property size, number of rooms, energy performance, type of wall, the presence of a fireplace and property extensions. Z_{jt-4} is a vector of local-authority covariates lagged by one year, which includes population, GDP per capita, housing stock, council total expenditures, council housing expenditures and the (band-D) council tax amount. $Post_t \cdot London_j$ controls for the impact of the surcharge on London with respect to the rest of the country to account for the introduction of the London 'Help to Buy' scheme. $Second_{j,2015}$ is the share of second homes in local authority j in 2015 and $Post_t \cdot Second_{j,2015}$ controls for the impact of the surcharge on second-home buyers.[12] $EUpop_{j,2015}$ is the share of residents with a EU nationality and $EUcom_{j,2015}$ is the share of properties owned by EU companies in local authority j in 2015. $Post_t \cdot EUpop_{j,2015}$ and $Post_t \cdot EUcom_{j,2015}$ control for the potential impact that the result of the Brexit referendum on 23 June 2016 may have had on housing demand by EU citizens and companies. υ_{ijt} is an error term.

Recent papers have pointed out that two-way fixed effect specifications other than

[12]For Wales, data on second homes is absent. In its place, I use the local-authority share on homes without a usual resident from the 2011 census (Office for National Statistics, 2011)

the canonical two-groups difference-in-differences, such as fuzzy or staggered designs, estimate a weighted sum of the average treatment effects in each unit and period, with weights that may be negative (Borusyak, Jaravel, and Spiess, 2021; Chaisemartin and D'Haultfœuille, 2018, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). In my setting, the treatment is not staggered over time but it varies in intensity ('dose') across local authorities. Callaway, Goodman-Bacon, and Sant'Anna (2021) study this case. They show that, under a parallel trends assumption, the incremental difference-in-differences coefficient γ is equal to a weighted average of the average causal responses for different doses, where all the weights are guaranteed to be non-negative.[13] The parallel trend assumption is that for all doses d, the average change in outcomes over time across all units had they been assigned dose d is the same as the average change in outcomes over time for all units that actually experienced dose d.[14]

In the context of the UK surcharge, the required parallel trend assumption is that, for each share of rented properties, the average change in housing market outcomes over time across all local authorities had they been assigned that share is the same as the average change in housing market outcomes over time for all local authorities that actually had that share of rented properties. This would be violated if certain local housing markets would have reacted differently to the surcharge even if they had the same share of rented properties. I account for differences across local authorities that might induce heterogeneous treatment effects by including the controls Z_{jt-4} and the interactions between $Post_t$ and , $Second_{j,2015}$, $London_j$, $EUcom_{j,2015}$, $EUpop_{j,2015}$. Moreover, to indirectly assess the validity of the 'parallel trends assumption', I also check for the presence of pre-policy trends using the following regression equation:

$$\begin{aligned} y_{ijt} =& \alpha_j + \eta_t + \gamma_t Rented_{j,2015} + \xi_t Second_{j,2015} + \lambda_t London_j \\ & + \iota_t EUcom_{j,2015} + \kappa_t EUpop_{j,2015} + \omega X_{ijt} + \zeta Z_{jt-4} + \upsilon_{ijt} \end{aligned} \tag{4.2}$$

The vector of coefficients γ_t are the coefficients of interest, which capture the effect of the surcharge on buy-to-let investors in each quarter relative to the default period of

[13]The average causal response at dose d_j is $ACR(d_j) = E[Y_t(d_j) - Y_t(d_{j-1})]$ where $Y_t(d_j)$ is the potential outcome at time t that the local authority would have in the case it had a d_j share of rented properties. Under a parallel trend assumption, Callaway, Goodman-Bacon, and Sant'Anna (2021) show that $\gamma = \sum_{d_j} \omega(d_j) \frac{E[Y_t(d_j) - Y_t(d_{j-1})]}{d_j - d_{j-1}}$ with $\omega(d_j) \geq 0$ and $\sum_{d_j} \omega(d_j) = 1$. If average causal responses are constant over d (the treatment effect function is linear), γ is equal to the average treatment effect of the surcharge applied to all properties in the local authority: $\gamma = E[Y_t(1) - Y_t(0)]$.

[14]The parellel trend assumption in Callaway, Goodman-Bacon, and Sant'Anna (2021) is: $E[Y_t(d) - Y_{t-1}(0)] = E[Y_t(d) - Y_{t-1}(0)|D = d]$ where t is the post-surcharge period, $t-1$ the pre-surcharge period. This is likely to be stronger than the standard parallel trend assumption $E[Y_t(0) - Y_{t-1}(0)|D = d] = E[Y_t(0) - Y_{t-1}(0)|D = 0]$.

the sample (the quarter before the surcharge announcement). Since these coefficients are quarter-specific, we can check for pre-trends by testing the significance of coefficients γ_t in the quarters t before the surcharge announcement.

4.4 Reduced-form results

In this Section, I analyze how the surcharge affected multiple aspects of the housing market in England and Wales. As shown in Appendix Section D.1, the results do not appear to be driven by other housing policy changes in the same period, nor by the outcome of the Brexit referendum.

Table 4.1 reports the estimates of the impact of the surcharge on the log number of quarterly property transactions at local authority level. In column (1), I estimate the coefficient γ in (4.1) without local authority controls (Z_{jt-4}), which are added in column (2). In column (3), I control for anticipation effects by adding the interaction $Ant_t * Rented_{j,2015}$ in which Ant_t is a binary variable equal to 1 in the quarter between the announcement and the introduction of the surcharge and 0 otherwise. In order to account for the anticipation effects described in Section 4.2, in column (4) I estimate the coefficient γ in (4.1) using a donut hole approach: I test whether dropping all property transactions within 6 months before and after the introduction of the surcharge significantly changes the estimates. In column (5) I add the interactions $Post_t * EUcom_{j,2015}$ and $Post_t * EUpop_{j,2015}$ to account for potential effects of the outcome of the Brexit referendum.

TABLE 4.1: Effect of stamp-duty surcharge on log number of quarterly property transactions

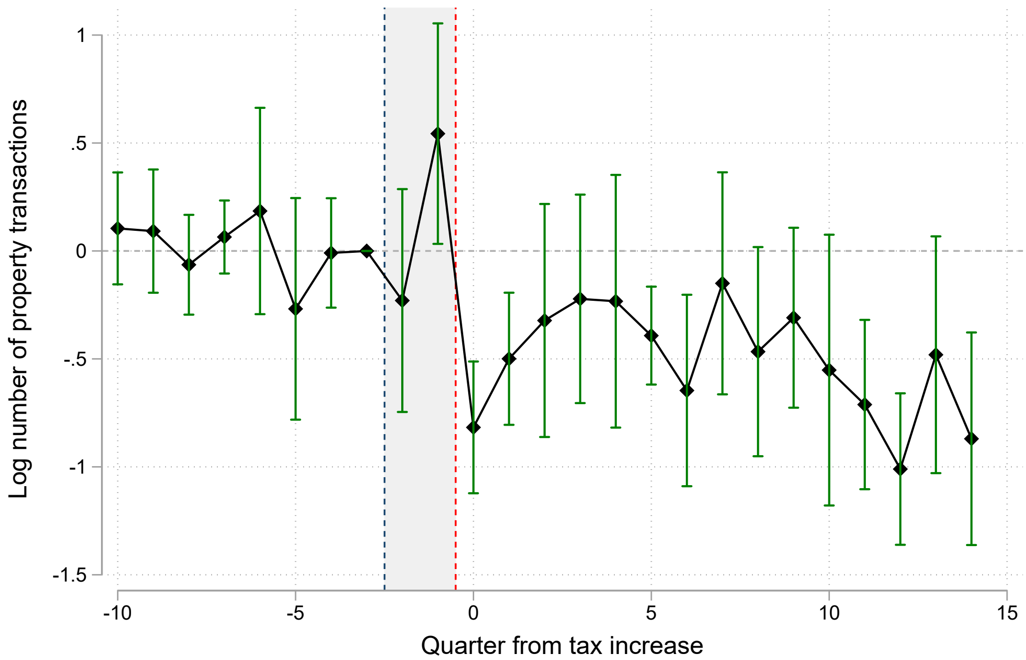
	(1)	(2)	(3)	(4)	(5)
Post*Share Rented	-0.597***	-0.655***	-0.489***	-0.591***	-0.502**
	(0.146)	(0.145)	(0.188)	(0.196)	(0.242)
Ant.*Share Rented			0.600***		
			(0.189)		
N	8,700	8,700	8,700	7,308	7,308
LA controls	NO	YES	YES	YES	YES
Donut hole	NO	NO	NO	YES	YES
Post*London	YES	YES	YES	YES	YES
Post*Second shares	YES	YES	YES	YES	YES
Post*EU shares	NO	NO	NO	NO	YES
S.E.	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA

Notes: This table reports results from OLS regressions of Equation (4.1) using the log of the number of days between the transaction date and the listing date as the dependent variable. Controls (LA level): lagged population, GDP per capita, housing stock, council total expenditures, council housing expenditures, council tax. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

The estimates for γ range from -0.489 to -0.655 and are robust to every specification: the surcharge significantly reduces the volume of transactions in local authorities in which the buy-to-let market is larger (p-value< 0.05). The significantly positive coefficient of $Ant_t * Rented_{j,2015}$ represents evidence that some buyers anticipated a property purchase to avoid the surcharge payment. However, the estimates for γ in columns (3), (4) and (5) are only slightly lower than the estimates in columns (1)-(2), showing that neither anticipation effects nor the Brexit referendum are the main drivers of the policy impact on transactions.

These estimates can be interpreted as the percentage change in transactions for a 3% stamp-duty surcharge increase if all properties were buy-to-let properties. Using column (5), the estimated elasticity of the number of transactions with respect to the tax is $-0.502/0.03 = -16.7$, which is large but in the ballpark of previous estimates. Best and Kleven (2018) analyze the impact of a UK stamp-duty holiday in 2008-9 on the number of transactions and estimate a short-run elasticity of -20.62 and a long-run elasticity of -14.3. To obtain the average impact on the housing market outcome, we need to multiply each coefficient by the average share of rented properties (assuming the surcharge effect is linear in the rented share). Considering that only 20.35% of the properties were rented, the average impact of the 3% surcharge on the English and Welsh housing market was a $-0.502 \cdot 0.2035 = 10.2\%$ decrease in the volume of property transactions over 2016-2019.

FIGURE 4.5: Quarterly effect on log-number of transactions



Notes: This figure reports point-estimates and 90% confidence intervals for γ_t from the OLS regression of Equation (4.2) using the log number of transactions as the dependent variable. The horizontal axis shows the number of quarters from the introduction of the 3% surcharge. The shaded area represents the period between the surcharge announcement and its introduction.

Figure 4.5 shows estimates of γ_t from regression (4.2) using the log number of transactions as dependent variable and can be regarded as a test for the parallel trend assumption. Reassuringly, there is no clear trend before the introduction of the surcharge: pre-policy effects are never significant except for the significantly positive one in the anticipation period. On the other hand, the quarterly estimates are negative in all quarters after the introduction of the surcharge and significantly negative in half of them.

The extensive margin response on transactions might cause a selection bias in the estimation of the impact of the surcharge on prices. The surcharge might have disproportionately changed transactions of properties of higher (lower) quality than average.[15] To account for this, I control for the rich set of property characteristics described in Section 4.3 with the addition of the *listing* price on Zoopla, which can be regarded as a measure of housing quality provided by the seller. Importantly, in Table D.1, we see that sellers did

[15]For instance, in Tables D.13 and D.14 we see that the surcharge significantly increased the average size of transacted properties, but not the energy performance.

not significantly change the property prices listed on Zoopla in response to the surcharge. Therefore, the listing price can be used as a control for housing quality in the regressions on paid prices.[16]

The results are shown in Table 4.2, separately for buy-to-let investors and buyers that will be owner-occupiers. Even though the surcharge had a stronger impact on the pre-tax price of buy-to-let transactions, the effect for owner-occupiers is significant and economically meaningful.[17] Housing has become more affordable for owner-occupiers, even if they were generally not liable for the surcharge. On average, the surcharge reduced property prices for future owner-occupiers by 0.9-2.1% and property prices for buy-to-let investors by 1.7-2.7%.[18] The difference in the price effects between buy-to-let and future owner-occupiers is significant (Table D.2) and it is prima facie evidence that the housing market is not perfectly competitive. Sellers were willing to accept a lower price from investors liable for the surcharge, because the search for another (owner-occupier) buyer was costly. As shown in Section 4.5, search frictions and price bargaining can intuitively explain why prices for all agents decrease but prices for investors decrease more. Only investors' transaction surplus is directly cut by the surcharge and this results in a lower price at the end of the price negotiation.

The estimates for γ are significantly negative in every specification. In column (6), the standard errors adjusted for spatial correlation are lower than the clustered ones originally calculated, which is common in longitudinal studies with fixed effects (Kelly, 2020).[19] The estimate becomes larger in magnitude when we control for local authority characteristics that are likely to affect house prices (e.g. the council tax) and they remain stable after accounting for anticipation effects and the shares of EU residents and companies (p-value<0.01).

Reassuringly, as we can see in Figures 4.6 and 4.7, the quarterly effects on paid prices are around 0 until the introduction of the surcharge, and they become gradually more negative and significant over time. The fact that the impact on prices accrues over time cannot be explained by a static tax incidence effect in a perfectly competitive market. Instead, they are consistent with the presence of amplifying equilibrium effects that take

[16] Estimates are qualitatively unchanged and of a similar magnitude if I do not include the listing price as a control.

[17] Since the surcharge was applied uniformly over the entire country and on all buy-to-let transactions at 3%, using post-tax prices as the dependent variable yields virtually identical results.

[18] Given the large degree of heterogeneity in the share of rented properties, the impact of the surcharge vary substantially across local authorities. Figure D.5 shows that in areas as London, where the buy-to-let market is stronger, the surcharge reduces the number of transactions by as much as 22% and prices by as much as 4%.

[19] Kelly (2020) argues that fixed effects already absorb a large degree of the spatio-temporal structure of the residuals and 'clustering is an aggressive solution to a problem that has substantially dissipated'.

some time to develop.

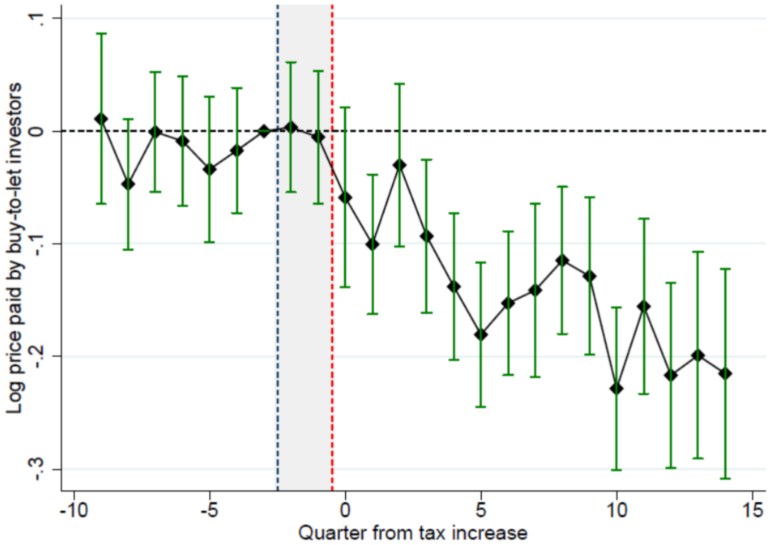
TABLE 4.2: Effect of stamp-duty surcharge on log paid price

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	**Log price paid by buy-to-let investors**					
Post*Share Rented	-0.085***	-0.121***	-0.117***	-0.122***	-0.135***	-0.135***
	(0.024)	(0.018)	(0.019)	(0.021)	(0.025)	(0.023)
Ant.*Share Rented			0.031**			
			(0.015)			
N	342,803	342,803	342,803	283,801	283,801	283,801
Dependent variable:	**Log price paid by future owner-occupiers**					
Post*Share Rented	-0.045**	-0.078***	-0.076***	-0.085***	-0.102***	-0.102***
	(0.021)	(0.016)	(0.016)	(0.019)	(0.022)	(0.019)
Ant.*Share Rented			0.022**			
			(0.010)			
N	1,226,749	1,226,749	1,226,749	978,144	978,144	978,144
LA controls	NO	YES	YES	YES	YES	YES
Donut hole	NO	NO	NO	YES	YES	YES
Post*London	YES	YES	YES	YES	YES	YES
Post*Second shares	YES	YES	YES	YES	YES	YES
Post*EU shares	NO	NO	NO	NO	YES	YES
S.E.	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Spatial HAC (100km)

Notes: This table reports results from OLS regressions of Equation (4.1) using the log price paid buy buy-to-let investors (top panel) and the log price paid by future owner-occupiers (bottom panel) as dependent variables. Controls (property level): log listing price, quadratics in latitude and longitude, size, number of rooms, energy performance, type of property, new, leasehold, fireplace, type of wall, extensions. Controls (LA level): lagged population, GDP per capita, housing stock, council total expenditures, council housing expenditures, council tax. In columns (1)-(5), s.e. are clustered at local authority level. In column (6), I allow spatial HAC s.e. to be serially correlated over the entire period. Spatial weighting kernels are assumed to decay linearly. Zero spatial correlation is assumed beyond 100km. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

If we pool together all transactions and assume all buyers are tax liable for comparability with previous estimates, the estimated elasticity of prices with respect to the tax is $-0.112/0.03 = -3.7$ (Column (5) in Table D.5). This large value is not far from estimates in previous studies that study similar tax variations. Kopczuk and Munroe (2015) analyze the impact of transfer taxes on property prices in New York and New Jersey and estimate a range of tax elasticities of prices between -2 and -3. Transfer taxes appear to be overshifted on property prices. Overshifting (a tax elasticity of prices larger than

FIGURE 4.6: Quarterly effect on prices paid by buy-to-let investors



Notes: This figure reports point-estimates and 90% confidence intervals for γ_t from the OLS regression of Equation (4.2) using the log price paid by buy-to-let investors as the dependent variable. The horizontal axis shows the number of quarters from the introduction of the 3% surcharge. The shaded area represents the period between the surcharge announcement and its introduction.

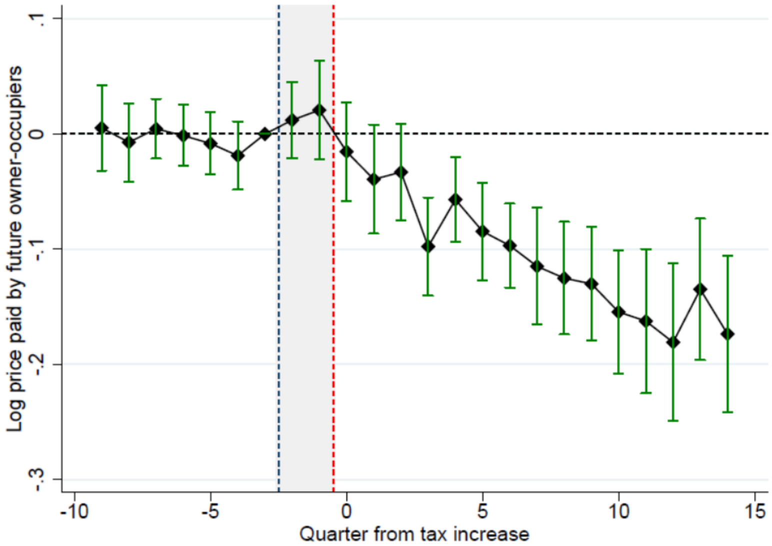
one in absolute value) is not possible in a perfectly competitive and frictionless market within a partial equilibrium framework (Fullerton and Metcalf, 2002). A potential explanation for this puzzling result is proposed by Kopczuk and Munroe (2015). They argue that overshifting and excessive market unravelling are consequences of search frictions in the housing market that amplify the initial price decrease: sellers may opt out or continue waiting for better offers, and buyers may continue searching in order to benefit from locally depressed prices.

However, the fall in transaction volumes and prices did not discourage housing supply in the medium term. I do not find any significant response in the construction of new private residential buildings or in the number of demolitions within four years from the introduction of the surcharge (Tables D.3-D.4 and Figure D.6). These insignificant results are consistent with previous estimates in the UK of a low (between 0 and 1) long-run price elasticity of supply of new residential constructions (Malpezzi and Maclennan, 2001).

4.4.1 Evidence of search frictions

The mere coexistence of an inventory of homes for sale and a stock of potential buyers indicates the presence of search frictions in the property market. These appear to be substantial in England and Wales. Figure 4.8a shows that it takes almost seven months to sell a property for a median seller and more than a year for the average seller, in the sample

FIGURE 4.7: Quarterly effect on prices paid by future owner-occupiers



Notes: This figure reports point-estimates and 90% confidence intervals for γ_t from the OLS regression of Equation (4.2) using the log price paid by future owner-occupiers as the dependent variable. The horizontal axis shows the number of quarters from the introduction of the 3% surcharge. The shaded area represents the period between the surcharge announcement and its introduction.

of properties listed on Zoopla in 2012-2019. Time-to-sell is calculated by subtracting the transaction date recorded in the Land Registry and the date the property was listed on Zoopla. These estimates are a lower bound of the actual median and average time-to-sell considering that: 1) some properties may have been for sale before being listed on Zoopla; 2) some properties listed on Zoopla were not sold.[20]

To test whether the surcharge affected the search process as suggested by Kopczuk and Munroe (2015), we can estimate whether it had a significant impact on this measure of time-to-sell. Table D.6 show estimates of γ in regression (4.1) using the log number of days between the listing date on Zoopla and the transaction date recorded in the Land Registry (henceforth *days to sell*). The surcharge increases days to sell in all specifications and is statistically significant after adding controls at local authority level. Considering that only 20.35% of the properties were rented, the average impact of the 3% surcharge on English and Welsh local housing markets was a 3.4-5.7% increase in days-to-sell. This result is confirmed by the analysis of quarterly effects in Figure D.7: time-to-sell gradually increases and becomes significantly higher one year after the introduction of the surcharge.

Search frictions can amplify the volatility of housing prices as the evidence of tax overshifting suggests, but can also generate equilibrium effects on the price paid by

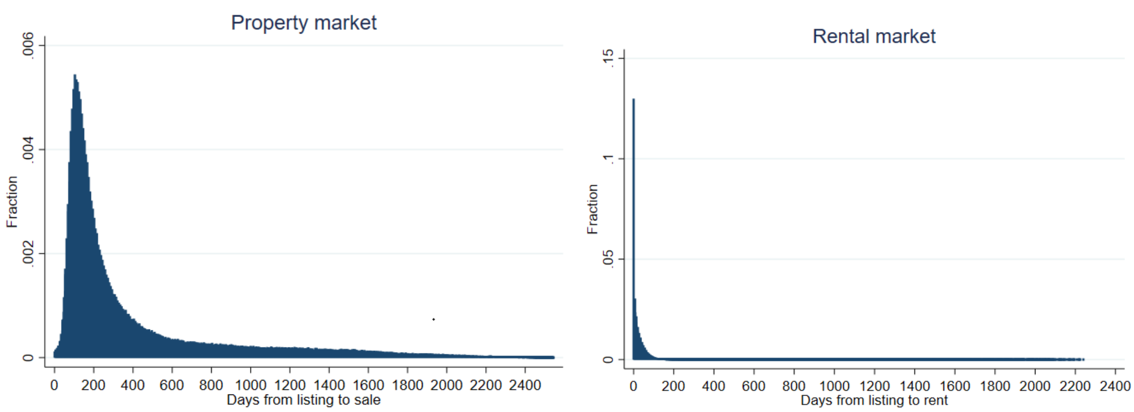
[20]The Zoopla dataset contains only properties that are listed *and* sold, so the variable time-to-sell is truncated.

owner-occupiers who are not liable for the surcharge. In a perfectly competitive and frictionless market, pre-tax prices should not be lower for investors than for owner-occupiers as sellers can always find a owner-occupier that buys the property without having to pay the surcharge. All the evidence gathered in this section highlights the importance of the interaction between investors and search frictions in the determination of prices in the housing market.

4.5 The model

This section presents a search model with property, rental and credit markets to illustrate the mechanisms behind the effects discussed in Section 4.4 and perform a welfare analysis of the surcharge on property investors. The property market is characterized by search frictions: it is costly and time-consuming to search for a house to buy and it is costly and time-consuming to sell a house. For simplicity, the rental market is assumed to be frictionless. This simplification is based on the empirical evidence that it takes a substantially lower amount of time to let than to sell a property. As we can see in Figure 4.8, for the sample of properties listed on Zoopla, the median number of days to sell a property is 208, whereas the median number of days to let a property is 20.[21]

FIGURE 4.8: Evidence of search frictions in property and rental markets.



(A) Days to sell: median=208; mean=420

(B) Days to let: median=20; mean=55

The introduction of buy-to-let mortgages has had a relevant impact in the British housing market (Figure 4.1) and can be interpreted as a reduction in credit market frictions for investors. To account for the role of credit rationing in the housing market, credit markets

[21] Time-to-sell is measured as the number of days between the listing date on Zoopla as a property for sale and the transfer date on the Land Registry. Time-to-let is measured as the number of days between the listing date on Zoopla as a property to let and the starting date of the tenancy period recorded on Zoopla.

for households and investors are also subject to frictions. I model credit rationing by introducing a credit search cost and a credit matching function symmetrically with respect to the property market.[22] This modelling choice is not new to models that combine credit, labor and non-durable goods markets (Dell'Ariccia and Garibaldi, 2005; Haan, Ramey, and Watson, 2003; Petrosky-Nadeau and Wasmer, 2013, 2015; Wasmer and Weil, 2004) but it was only recently introduced in models of the housing market (Gabrovski and Ortego-Marti, 2021, 2023), in which access to credit plays a central role.

Characterizing the credit market via an aggregate matching function maintains the model tractable and is in line with several empirical findings. Dell'Ariccia and Garibaldi (2005) document that credit contractions are more volatile than credit expansions and that gross flows are much larger than net flows in the credit market. This evidence is consistent with a matching model in which banks need time to identify new profitable clients after a positive aggregate shock, but can recall credit without time delay after a negative aggregate shock. An efficiency increase of the credit market matching function can also explain the observed increase in average geographical distance between lenders and borrowers that occurred during the IT revolution (Petersen and Rajan, 2002).

Agents in the economy are risk-neutral and of four different types: households (h), investors (i) and their respective lenders (l_h and l_i).[23] Time is continuous with an infinite horizon. All agents discount the future with factor $r > 0$. The population of households is exogenously given and denoted by $\mathcal{H}$. Investors can enter freely the housing market and lenders can enter freely in the credit market. Their respective total number in equilibrium is endogenously determined by the model.

Developers can build new houses if existing properties are not sufficient to satisfy households and investors' demand. Vacant homes depreciate at rate δ, whereas new homes are supplied at construction cost K. As in Gabrovski and Ortego-Marti (2019), I assume that new sellers can enter the housing market at this cost.[24] Given that all houses are

[22]The presence of credit rationing can be micro-founded in a model of asymmetric information between lenders and borrowers, as in Stiglitz and Weiss (1981). The reason of excess demand equilibria in credit markets is that the higher the interest rate set by the bank, the riskier the borrowers that are willing to get a loan (adverse selection) and/or the riskier the projects they will engage in (moral hazard). Non-monotonicity of profits in the interest rate can result in a profit-maximizing equilibrium interest rate that is lower than the market-clearing interest rate.

[23]The model focuses on long-term buy-to-let investors rather than on short-term speculators (flippers). Empirically, buy-to-let investors appear to engage in long-term operations: using data on repeated sales in the Land Registry from 1995 to 2019, I calculate an implied average duration of ownership of 22.2 years for buy-to-let investors.

[24]The only role developers play in the model is to supply new homes when the existing stock of properties for sale is insufficient to meet demand. Free entry of both buyers and sellers is a departure from standard search models of the labour market, in which the measure of sellers (the labor force) is exogenously given. This is necessary to obtain an *upward* sloping Beveridge curve consistent with the signs of empirically estimated elasticities in housing markets (Díaz and Jerez, 2013; Gabrovski and Ortego-Marti, 2019).

identical, the value of a house for sale is determined by the entry condition, regardless of whether it is a newly built or an old house. Construction cost is an increasing function of new residential constructions e due to capacity constraints: $K = K(e)$ and $K'(e) \geq 0$. This corresponds to assuming a positive cost elasticity of supply and is a generalization of Gabrovski and Ortego-Marti (2019) that assume an infinite elasticity of supply with constant K.

Houses are homogeneous, but buyers are heterogeneous in terms of wealth. A fraction σ_h (σ_i) of households (investors) who search for a property do not need a mortgage to purchase it. Households own at most one property so they pay a standard ad-valorem transfer tax τ_h, whereas investors are buyers of multiple properties, so they pay a higher ad-valorem transfer tax which includes the surcharge $\tau_i > \tau_h$. Households earn income y. When they rent a house, they pay rental price R to investor-owners which is endogenously determined by the model. When they become owners, they stop paying the rent and receive homogeneous home-owning utility ε_h. On the other hand, investors earn R when they own a house and receive homogeneous home-owning utility ε_i (e.g. maintenance costs). Rental price R instantaneously clears the rental market.

4.5.1 Timing and meeting probabilities

Buyers of type $j \in \{h, i\}$ can either buy a house outright or use a mortgage. In the latter case, loan amounts are negotiated before prices. This replicates the common practice of mortgage 'agreements in principle' in the UK. Buyers obtain information on the loan amount they can obtain from a bank before searching for a house, and sellers generally ask to see a mortgage agreement in principle before agreeing to a sale (Lloyds Bank plc, 2022). Agents face three stages:

- Stage 0: buyers and lenders randomly search for each other. When they meet, they negotiate over the loan amount a_j in exchange for a flow mortgage repayment ρ_j for any given price p_j^L.
- Stage 1: buyers and sellers randomly search for each other. When they meet, they negotiate the price p_j^L and buyers pay $p_j^L(1 + \tau_j) - a_j$,
- Stage 2: owners receive home-owning utility ε_j and pay lenders ρ_j until a moving shock, which occurs at rate π_j.[25]

[25]Using Land Registry data, I calculate an average home-ownership duration of 22.2 years for investors and 25.6 years for households (Section 4.6). This is quite similar to the median mortgage duration, which was 25 years in 2006 (FCA, 2019). Since agents have linear utility of income and they discount utility at the same rate of lenders, mortgage duration is not relevant for their decisions.

If they choose to buy a house outright, they face two stages:

- Stage 1: buyers and sellers randomly search. When they meet, they negotiate the price p_j and buyers pay $p_j(1+\tau_j)$.
- Stage 2: owners receive home-owning utility ε_j until a moving shock, which occurs at rate π_j.

To find a seller, household-buyers and investor-buyers compete in the same property market with tightness $\theta = \frac{h_1+h_1^L+i_1+i_1^L}{s_1}$, where s_1, h_1, i_1, h_1^L and i_1^L are the measures of sellers, household-buyers and investor-buyers with and without a mortgage agreement in stage 1, respectively. Meeting probabilities are determined by a standard constant returns to scale matching function $M(h_1+h_1^L+i_1+i_1^L, s_1)$ which is increasing and concave in its arguments (Pissarides, 2000). Accordingly, buyers find sellers with probability

$$\frac{M(h_1+h_1^L+i_1+i_1^L, s_1)}{h_1+h_1^L+i_1+i_1^L} = M(1, \theta^{-1}) \equiv m(\theta) \tag{4.3}$$

Since search is random, sellers find a type-j buyer with probability $\tilde{j}\theta m(\theta)$ where $\tilde{j} = \frac{j}{h_1+h_1^L+i_1+i_1^L}$ for $j \in \{h_1, h_1^L, i_1, i_1^L\}$. By properties of the matching function $m(\theta)$ and $\theta m(\theta)$ are respectively decreasing and increasing in θ.

If they choose to buy using a mortgage, household- and investor-buyers randomly search for a lender in different credit markets with tightness $\phi_h = \frac{h_0^L}{l_{h0}}$ and $\phi_i = \frac{i_0^L}{l_{i0}}$, where h_0^L, i_0^L, l_{h0} and l_{i0} are the measures of household-buyers, investor-buyers and their respective lenders in stage 0. Meeting probabilities are determined by standard matching functions à la Pissarides (2000) $M_h(h_0^L, l_{h^L0})$ and $M_i(i_0^L, l_{i0})$. Accordingly, buyers find a lender with probabilities

$$\frac{M_j(j_0^L, l_{j0})}{j_0} = M(1, \phi_j^{-1}) \equiv q_j(\phi_j), \quad j \in \{h, i\} \tag{4.4}$$

Lenders find a type-j buyer with probability $\phi_j q_j(\phi_j)$. By properties of the matching function $q_j(\phi_j)$ and $\phi_j q_j(\phi_j)$ are respectively decreasing and increasing in ϕ_j.

4.5.2 Agent values

Household-renters who are not sufficiently wealthy to buy a property outright choose whether to search for a lender. If they do, they pay search cost χ_h and find a lender at rate $q_h(\phi_h)$. They have value:

$$rH_0^L = y - R + \max\{-\chi_h + q_h(\phi_h)\max\{H_1^L - H_0^L, 0\}, 0\} \tag{4.5}$$

Once they have a mortgage agreement in principle, household-renters decide whether to search for a seller at cost c_h and they find one at rate $m(\theta)$. They have value:

$$rH_1^L = y - R + \max\{-c_h + m(\theta)\max\{H_2^L - [p_h^L(1+\tau_h) - a_h] - H_1^L, 0\}, 0\} \quad (4.6)$$

If they purchase a house they pay the after-tax price $p_h^L(1+\tau_h)$ net of the loan amount a_h. Once they own a house, households with a mortgage receive additional utility ε_h, pay mortgage repayment ρ_h until a moving shock which occurs at rate π_h. In that case they become renter-buyers and sellers at the same time with value $H_0^L + S_1$:

$$rH_2^L = y + \varepsilon_h - \rho_h + \pi_h[H_0^L + S_1 - H_2^L] \quad (4.7)$$

In equilibrium, households are indifferent between searching for a mortgage or not: $-\chi_h + q_h(\phi_h)\max\{H_1^L - H_0^L, 0\} = 0$

The values for investors who need a mortgage to buy a house are symmetric to the households, except for the fact that investors do not pay rent while searching for a house and they receive rental payment R when they own a property. They are:

$$rI_0^L = \max\{-\chi_i + q_i(\phi_i)\max\{I_1^L - I_0^L, 0\}, 0\} \quad (4.8)$$

$$rI_1^L = \max\{-c_i + m(\theta)\max\{I_2^L - [p_i^L(1+\tau_i) - a_i] - I_1^L, 0\}, 0\} \quad (4.9)$$

$$rI_2^L = R + \varepsilon_i - \rho_i + \pi_i[\max\{I_0^L, 0\} + S_1 - I_2^L] \quad (4.10)$$

In equilibrium, investors are indifferent between searching for a mortgage or not: $-\chi_i + q_i(\phi_i)\max\{I_1^L - I_0^L, 0\} = 0$

For household-renters that are sufficiently wealthy to buy a house outright, the choice is between searching for a seller, searching for a lender or not searching. Their value is:

$$\begin{aligned} rH_1 = y - R + \max\{ &-c_h + m(\theta)\max\{H_2 - p_h(1+\tau_h) - H_1, 0\}, \\ &-\chi_h + q_h(\phi_h)\max\{H_1^L - H_1, 0\}, 0\} \end{aligned} \quad (4.11)$$

The value for a household-owner without a mortgage is simply:

$$rH_2 = y + \varepsilon_h + \pi_h[\max\{H_1, H_0^L\} + S_1 - H_2] \quad (4.12)$$

For investors that are sufficiently wealthy to buy a house outright, the values are symmetric:

$$rI_1 = \max\{ - c_i + m(\theta)\max\{I_2 - p_i(1+\tau_i) - I_1, 0\},\\ - \chi_i + q_i(\phi_i)\max\{I_1^L - I_1, 0\}, 0\} \tag{4.13}$$

$$rI_2 = R + \varepsilon_i + \pi_h[\max\{I_1, 0\} + S_1 - I_2] \tag{4.14}$$

Notice that households and investors that are sufficiently wealthy to buy a house outright will always choose to do so in equilibrium (see Appendix Section D.3.4).

Lenders pay a screening cost χ_{Lj} until they find a type-*j* buyer, which occurs at rate $\phi_j q_j(\phi_j)$. Their value is:

$$rL_{j0} = -\chi_{Lj} + \phi_j q_j(\phi_j)\max\{L_{j1} - L_{j0}, 0\}, \quad j \in \{h, i\} \tag{4.15}$$

The value of a lender waiting for the type-*j* buyer to find a seller is:

$$rL_{j1} = m(\theta)[L_{j2} - a_j - L_{j1}], \quad j \in \{h, i\} \tag{4.16}$$

When the type-j buyer find a property to buy, the lender pays the loan amount a_j. A lender under a mortgage contract with a type-*j* buyer receive flow payments ρ_j until the moving shock:

$$rL_{j2} = +\rho_j + \pi_j[\max\{L_{j0}, 0\} - L_{j2}], \quad j \in \{h, i\} \tag{4.17}$$

Finally, household- and investor-sellers have an identical value. They pay search cost c_s, face depreciation δ and find a buyer with probability $\theta m(\theta)$. Since search is random, the probability to find a buyer of a particular type conditional on finding a buyer is equal to the type-share of buyers. Denote these type-share as $\tilde{j} = \frac{j}{h_1 + h_1^L + i_1 + i_1^L}$ for $j \in \{h_1, h_1^L, i_1, i_1^L\}$. They are derived in section D.3.3 as functions of housing market tightness θ and new constructions e. Then, the value of sellers is:

$$rS_1 = -c_s - \delta S_1 + \theta m(\theta)\Big[\tilde{h}_1^L(e,\theta)\max\{p_h^L - S_1, 0\} + \tilde{h}_1(e,\theta)\max\{p_h - S_1, 0\}\\ + \tilde{i}_1^L(e,\theta)\max\{p_i^L - S_1, 0\} + \tilde{i}_1(e,\theta)\max\{p_i - S_1, 0\}\Big] \tag{4.18}$$

In equilibrium, there is free entry of lenders ($L_{h1} = L_{i1} = 0$) and sellers enter at construction cost $K(e)$: $S_1 = K(e)$. To summarize, the entry conditions in the steady state equilibrium are:

$$\begin{aligned} S_1 = K(e), \quad H_1^L - H_0^L = \frac{\chi_h}{q_h(\phi_h)}, \quad I_1^L - I_0^L = \frac{\chi_i}{q_i(\phi_i)}, \\ L_{h1} - L_{h0} = \frac{\chi_{lh}}{\phi_i q_h(\phi_h)}, \quad L_{i1} - L_{i0} = \frac{\chi_{li}}{\phi_i q_i(\phi_i)} \end{aligned} \tag{4.19}$$

4.5.3 Prices and mortgage negotiations

There are two types of negotiations in the economy: the loan amount negotiated between buyers and lenders and the property transaction price bargained between buyers and sellers. For buyer-borrowers, these contracts are negotiated sequentially. Buyers and sellers take as given the loan amount which was agreed before they met. Accordingly, lenders and buyers know that the result of their negotiation will affect the bargaining over the property price.

To solve this sequential problem, we proceed by backwards induction. In stage 1, buyers and sellers bargain the price, given the loan amount negotiated in stage 0. They maximize the surplus of the property transaction according to the following Nash bargaining rules:[26]

$$\begin{aligned} &\max_{p_h^L} [p_h^L - S_1]^{\beta_h} [H_2 - p_h^L(1+\tau_h) + a_h - H_1]^{1-\beta_h}, \\ &\max_{p_i^L} [p_i^L - S_1]^{\beta_i} [I_2 - p_i^L(1+\tau_i) + a_i - I_1]^{1-\beta_i} \end{aligned} \tag{4.20}$$

where β_j is the seller's bargaining power when meeting a type-j buyer ($j \in \{h, i\}$). The loan amounts a_j are negotiated when buyers and lenders meet, taking into account the impact they will have on property prices negotiated in the future.[27] Lenders and buyers maximize the surplus of their relationship according to the following Nash bargaining rules:

$$\max_{a_h} [L_{h1} - L_{h0}]^{\psi_h} [H_1 - H_0]^{1-\psi_h}, \quad \max_{a_i} [L_{i1} - L_{i0}]^{\psi_i} [I_1 - I_0]^{1-\psi_i} \tag{4.21}$$

[26]As shown by Rubinstein (1982), sharing the surplus according to the agents' bargaining power is the subgame perfect equilibrium outcome of an infinite-horizon, alternating-offers bargaining game in which every agent has a fixed discount factor. A player's bargaining power monotonically increases with their discount factor.

[27]I assume buyers and lenders negotiate over loan amounts a_j taking mortgage repayment ρ_j as given. For instance, mortgage repayment could be a fixed fraction of the borrower's income: $\rho_j = \lambda y$ where $\lambda \in (0, 1)$. The model is isomorphic if buyers and lenders negotiate over mortgage repayments and take the loan amount as given.

where ψ_j is the seller's bargaining power when meeting a type-j buyer ($j \in \{h, i\}$).

Outright buyers simply negotiate the price with sellers to maximize the transaction surplus:

$$\max_{p_h}[p_h - S_1]^{\beta_h}[H_2 - p_h(1+\tau_h) - H_1]^{1-\beta_h}, \quad \max_{p_i}[p_i - S_1]^{\beta_i}[I_2 - p_i(1+\tau_i) - I_1]^{1-\beta_i} \tag{4.22}$$

Solving these surplus maximization problems and plugging the equilibrium conditions yield equilibrium equations for prices and loan amounts (see Appendix Section D.3.1).

The equilibrium values for loan amounts a_j satisfy:

$$\begin{aligned}\frac{\rho_j}{r+\pi_j} =& a_j + \psi_j\left(\frac{R+\varepsilon_j+\pi_j K(e)}{r+\pi_j} - (1+\tau_j)K(e) - \frac{c_j}{(1-\beta_j)m(\theta)} + \frac{\beta_j\chi_j}{(1-\beta_j)q_j(\phi_j)}\right),\\ & j \in \{h, i\}\end{aligned} \tag{4.23}$$

The present discounted value of mortgage repayments is equal to the loan amount plus the lender's share of the house transaction surplus net of buyer's search cost. In equilibrium, non-borrowers pay prices:

$$p_j = K(e) + \frac{\beta_j}{1+\tau_j}\left[\frac{\varepsilon_j + R - rK(e) - (r+\pi_j)\tau_j K(e) + c_j}{r+\pi_j+(1-\beta_j)m(\theta)}\right], \quad j \in \{h, i\} \tag{4.24}$$

In equilibrium, borrowers pay prices:

$$\begin{aligned}p_j^L =& K(e) + \frac{\beta_j}{(1+\tau_j)}\left[\frac{\varepsilon_j + R - rK(e) - (r+\pi_j)\tau_j K(e)}{r+\pi_j} - \frac{\chi_j}{q_j(\phi_j)} - \frac{\rho_j}{r+\pi_j} + a_j\right]\\ =& K(e) + \frac{\beta_j}{(1+\tau_j)}\left[(1-\psi_j)\left(\frac{\varepsilon_j + R - rK(e) - (r+\pi_j)\tau_j K(e)}{r+\pi_j}\right)\right.\\ & \left. - \frac{[1-\beta_j(1-\psi_j)]\chi_j}{(1-\beta_j)q_j(\phi_j)} + \frac{\psi_j c_j}{(1-\beta_j)m(\theta)}\right], \quad j \in \{h, i\}\end{aligned} \tag{4.25}$$

Prices are simply equal to construction cost plus the seller's share of the transaction surplus. Prices for buyers with a mortgage decrease with mortgage repayments (interest rates) and increase with the loan amount borrowers are able to obtain. Intuitively, the average price decreases with credit frictions $\frac{\chi_j}{q_j(\phi_j)}$ and increases with rental price R, construction cost $K(e)$ and housing market tightness θ ceteris paribus. However, we need to solve for the equilibrium values of the endogenous variables $(\phi_i, \phi_h, \theta, R, e)$ to account for general equilibrium effects.

4.5.4 A financial accelerator

Using the equilibrium expressions for the values of borrowers and lenders (see Appendix Section D.3.2), we obtain the following equations for buyers' entry and lenders' entry:

$$\begin{aligned}\frac{\chi_j}{q_j(\phi_j)} =& \frac{(1-\psi_j)}{r+m(\theta)[1-\beta_j(1-\psi_j)]}\Bigg\{-c_j\\ &+m(\theta)(1-\beta_j)\left[\frac{\varepsilon_j+R-rK(e)-(r+\pi_j)\tau_j K(e)}{r+\pi_j}\right]\Bigg\}\end{aligned} \tag{BE_j}$$

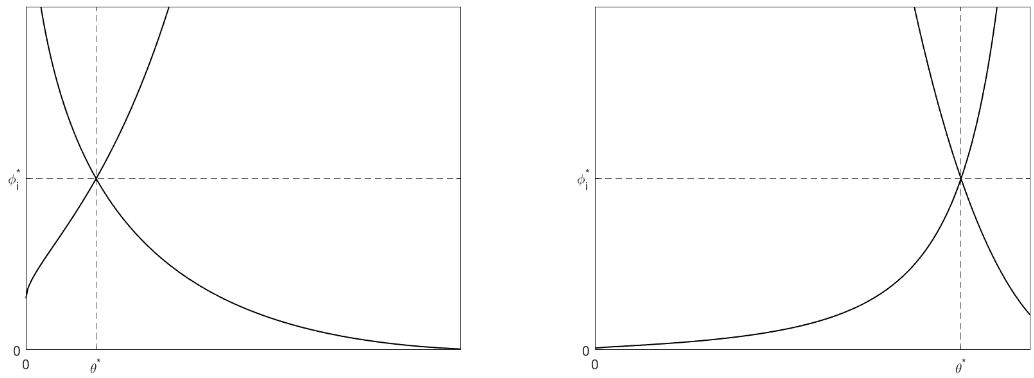
$$\begin{aligned}\frac{\chi_{lj}}{\phi_j q_j(\phi_j)} =& \frac{\psi_j/(1-\beta_j)}{r+m(\theta)[1-\beta_j(1-\psi_j)]}\Bigg\{-c_j\\ &+m(\theta)(1-\beta_j)\left[\frac{\varepsilon_j+R-rK(e)-(r+\pi_j)\tau_j K(e)}{r+\pi_j}\right]\Bigg\}\end{aligned} \tag{LE_j}$$

For given rental price R and construction cost $K(e)$, borrowers' entry equation (BE_j) defines a downward sloping iso-value curve and lenders' entry equation (LE_j) defines an upward-sloping iso-value curve in the (θ, ϕ_j) plane. If the expected cost of entry for a borrower is lower because the credit market is less tight, then the expected value of entering the property market can go to zero only if housing market tightness (i.e. expected duration of house search) is higher. If the expected cost of entry for a lender is higher because the credit market is less tight, then zero profits can only be achieved by having lower housing market tightness.

Borrowers and lenders' entry curves are represented in Figure 4.9. As we can see, an increase in credit market efficiency leaves credit market tightness unchanged but increases housing market tightness, for a given rental price R and construction cost $K(e)$. In the limit case in which credit frictions disappear ($q_j(\phi_j) \to \infty$), housing market tightness is maximized at value $\bar{\theta}$.

As Wasmer and Weil (2004) show for the labor market, credit market frictions can amplify and propagate shocks to the housing market, acting in the form of a *financial accelerator*. A reduction in credit frictions increases the number of lenders, which incentivizes borrowers' entry, further encouraging lenders' entry and so on. Shocks to the credit sector result in an amplified effect on housing market tightness, which can in turn have a strong impact on housing prices. Differently from the labor market, shocks to the credit sector will have an impact on the rental market as well (the rental price R) which must be taken into account when analyzing how credit market frictions affect housing market tightness.

FIGURE 4.9: The effect of a reduction in credit frictions on housing market tightness



(A) Low credit matching efficiency

(B) High credit matching efficiency

Notes: These figures represent credit market tightness for investors in the vertical axis and housing market tightness in the horizontal axes. In panel (a) credit market frictions are high and housing market tightness is low. In panel (b) credit market frictions are low and housing market tightness is high.

4.5.5 A recursive equilibrium

To solve the model, we need to find four endogenous variables: the credit market tightness for households ϕ_h, the credit market tightness for investors ϕ_i, the housing market tightness θ and the measure of new constructions e. Given these variables, we can find property prices using Equations (4.24)-(4.25) and the rental price using Equation (BE_j). In addition, we can find the equilibrium dwellings stock $D = \mathcal{H} + \frac{e}{\delta}$ and the stock all of agents in steady state using the stocks and flows equations presented in Appendix Section D.2 and D.3.3.

Solving the surplus maximization problems in (4.21) and using the equilibrium conditions (4.19), we obtain equilibrium credit market tightness ϕ_j in each market:

$$\phi_j = \frac{(1-\beta_j)(1-\psi_j)\chi_{lj}}{\psi_j \chi_j}, \quad j \in \{h, i\} \tag{4.26}$$

As in Wasmer and Weil (2004), credit market tightness is constant in equilibrium and depends only on bargaining powers and search costs. The higher the bargaining powers of buyers in the credit and in the property market and the lower their credit search costs, the higher credit market tightness. The lower the bargaining power of lenders and the higher their credit search costs, the higher credit market tightness.

To find θ and e given ϕ_h and ϕ_i, first we can equalize the rental price in (BE_h) and (BE_i):

$$\frac{r+\pi_i}{m(\theta)(1-\beta_i)}\left\{\frac{\chi_i\{r+m(\theta)[1-\beta_i(1-\psi_i)]\}}{q_i(\phi_i)(1-\psi_i)}+c_i\right\}-\varepsilon_i+rK(e)+(r+\pi_i)\tau_i K(e)=$$
$$R=$$
$$\frac{r+\pi_h}{m(\theta)(1-\beta_h)}\left\{\frac{\chi_h\{r+m(\theta)[1-\beta_h(1-\psi_h)]\}}{q_h(\phi_h)(1-\psi_h)}+c_h\right\}-\varepsilon_h+rK(e)+(r+\pi_h)\tau_h K(e) \tag{4.27}$$

This two equations represent rental market supply and demand, respectively, and the rental price R instantaneously clears the rental market. For a given construction cost, the rental price decreases in the probability a buyer finds a property to buy because rental demand decreases and rental supply increases.

Secondly, to close the model we use equilibrium sellers' entry from Equation (4.18):

$$\begin{aligned} rK(e) = -c_s - \delta K(e) + \theta m(\theta)\Big[&\tilde{h}_1^L[p_h^L - K(e)] + \tilde{h}_1[p_h - K(e)] \\ &+ \tilde{i}_1^L[p_i^L - K(e)] + \tilde{i}_1[p_i - K(e)]\Big] \end{aligned} \tag{4.28}$$

where prices $\{p_h, p_h^L, p_i, p_i^L\}$ and shares of buyers $\{\tilde{h_1}, \tilde{h_1^L}, \tilde{i_1}, \tilde{i_1^L}\}$ can be derived as functions of θ and e from Equations (4.24)-(4.25) and Equations (D.38)-(D.41), respectively.

The solution of the model depends on the functional form of $K(e)$. Under infinite elasticity of supply, the construction cost $K(e)$ is independent of the number of constructions e and the stationary equilibrium of this model is a recursive equilibrium, in which we can solve for θ using (4.27) and then we can plug it in (4.28) to solve for e. In general, the steady-state equilibrium is defined as follows.

Definition 1 *The steady-state equilibrium is a recursive equilibrium that consists of: (i) a credit market tightness for households and investors ϕ_j for $j \in \{h, i\}$ satisfying (4.26); (ii) housing market tightness and new constructions $\{\theta, e\}$ satisfying simultaneously (4.27)-(4.28) ; (iii) property prices $\{p_j, p_j^L\}$ for $j \in \{h, i\}$ satisfying price equations (4.24)-(4.25); (iv) rental price R that clears the rental market satisfying (4.27); (v) type-j shares of buyers $\tilde{j}$ for $\tilde{j} \in \{\tilde{h_1}, \tilde{h_1^L}, \tilde{i_1}, \tilde{i_1^L}\}$ consistent with the stocks and flows of agents in the steady state satisfying (D.38)-(D.41).*

4.5.6 Comparative statics: the short run

In this section, I use the model to show how equilibrium effects can rationalize all the empirical findings of Section 4.4: an increase in time-to-sell; a decrease in number of transactions; a decrease in prices paid by future owner-occupiers and a larger decrease in prices paid by buy-to-let investors. The effects analyzed in the four years of the post-surcharge period can be interpreted as short- and medium-term effects.[28] In the short run, we can assume the dwellings stock D is fixed. Accordingly, the construction cost K is also fixed.

First of all, applying the implicit function theorem to Equation (4.27), we see that the probability of finding a buyer increases with the introduction of a surcharge on investors ($\frac{dm(\theta)}{d\tau_i} > 0$) if

$$\begin{aligned} &-\varepsilon_i + (r+\pi_i)K\tau_i + \frac{(r+\pi_i)[1-\beta_i(1-\psi_i)]}{(1-\beta_i)(1-\psi_i)}\frac{\chi_i}{q_i(\phi_i)} \\ &< -\varepsilon_h + (r+\pi_h)K\tau_h + \frac{(r+\pi_h)[1-\beta_h(1-\psi_h)]}{(1-\beta_h)(1-\psi_h)}\frac{\chi_h}{q_i(\phi_h)} \end{aligned} \tag{4.29}$$

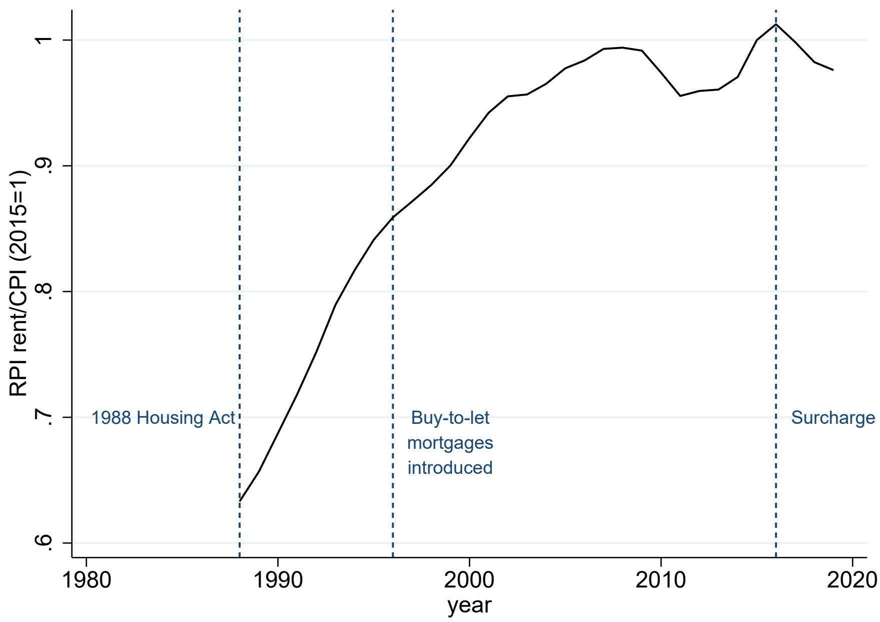
If credit search frictions for households are substantially larger than credit search frictions for investors at the moment of the tax change, we would expect the tax to increase buyer's probability to find a property to buy and to decrease seller's probability to find a buyer, i.e. to reduce housing market tightness. In Section 4.6, I estimate that the credit market frictions for households are substantially larger than the credit market frictions for investors in 2015 and condition (4.29) holds. Accordingly, we should observe an increase in time to sell $\frac{1}{\theta m(\theta)}$, as estimated in Section 4.4. Interestingly, notice that if the surcharge on investors becomes too high ($\tau_i \gg \tau_h$) housing market tightness will *increase* and buyers' probability to find a home will decrease. The intuition is that a very high surcharge increases rental prices at a level that induces most households to seek a property to buy, thus increasing the number of total buyers in the market.

As population $\mathcal{H}$ is fixed, the number of sellers $s_1 = D - \mathcal{H}$ is also fixed in the short run. Since housing market tightness decreases with the surcharge on investors and the number of sellers is fixed, in the short-run we expect a decrease in the number of buyers (in particular buy-to-let investors) and, accordingly, a decrease in the number of transactions: $\frac{dM(h_1+h_1^L+i_1+i_1^L,s_1)}{d\tau_i} < 0$. The tax reduces the number of buyers relative to the number of sellers: this makes it harder for sellers to sell their property and reduces the number of transactions.

[28] In England, the average construction period is 2.5 years for sites between 100 and 499 units and 5 years for sites over 1,000 units in England (Swan, 2016).

The increase in the probability of finding a property to buy results in a reduction in rental prices. From the second equality in (4.27), it is clear that $\frac{dR}{d\tau_i} = -\frac{(r+\pi_h)r\chi_h}{(1-\beta_h)q_h(\phi_h)(1-\psi_h)m(\theta)^2}\frac{dm(\theta)}{d\tau_i} < 0$ when $\frac{dm(\theta)}{d\tau_i} > 0$. This is due to a reduction in rental demand relative to supply as households find properties to buy more easily and investors find properties to buy and let more easily. Even though the identification strategy does not allow to analyze the impact on rental prices, we see in Figure 4.10 that the average deflated rental price in England and Wales peaks in 2016 and starts to decrease exactly after the surcharge on investors is introduced.

FIGURE 4.10: Rental price over time



Notes: This figure reports the Retail Price Index for Housing Rents deflated by the Consumer Price index in the UK from 1988 to 2019. The ratio is normalized to 1 in 2015.

When housing market tightness decreases, the short-run impact that a surcharge on investors has on property prices is unambiguously negative *for each type of buyer*. To see this, we can use the implicit function theorem on price equations (4.24)-(4.25) to obtain:

$$\frac{dp_h}{d\tau_i} = \frac{\beta_h \frac{dR}{d\tau_i} - [(p_h - K)(1-\beta_h)(1+\tau_h)]\frac{dm(\theta)}{\tau_i}}{[r + \pi_h + m(\theta)(1-\beta_h)](1+\tau_h)} < 0 \tag{4.30}$$

$$\frac{dp_h^L}{d\tau_i} = \frac{\frac{\beta_h(1-\psi_h)}{r+\pi_h}\frac{dR}{d\tau_i} - \frac{\beta_h\psi_h}{(1-\beta_h)}\left[\frac{1}{m(\theta)^2}\right]\frac{dm(\theta)}{\tau_i}}{(1+\tau_h)} < 0 \tag{4.31}$$

$$\begin{aligned}\frac{dp_i}{d\tau_i} =& \frac{\beta_i \frac{dR}{d\tau_i} - [(p_i - K)(1-\beta_i)(1+\tau_i)]\frac{dm(\theta)}{\tau_i}}{[r+\pi_i+m(\theta)(1-\beta_i)](1+\tau_i)} \\ &- \frac{\{p_i[r+\pi_i+m(\theta)(1-\beta_i)] - (1-\beta_i)K[r+\pi_i+m(\theta)]\}}{[r+\pi_i+m(\theta)(1-\beta_i)](1+\tau_i)} < 0\end{aligned} \tag{4.32}$$

$$\begin{aligned}\frac{dp_i^L}{d\tau_i} =& \frac{\frac{\beta_i(1-\psi_i)}{r+\pi_i}\frac{dR}{d\tau_i} - \frac{\beta_i\psi_i}{(1-\beta_i)}[\frac{1}{m(\theta)^2}]\frac{dm(\theta)}{\tau_i}}{(1+\tau_i)} \\ &- \frac{\{p_i^L - K[1-\beta_i(1-\psi_i)]\}}{(1+\tau_i)} < 0\end{aligned} \tag{4.33}$$

The surcharge for multiple-property investors has three negative effects on prices. A *direct tax incidence effect* that has an impact only on properties purchased by investors: the last term on the right-hand side of (4.32) and (4.33).[29] In addition, there are *two equilibrium effects* through which the surcharge on investors reduces prices for all buyers:

1. an increase in the probability to find a property to buy ($\frac{dm(\theta)}{d\tau_i}$) which favors buyers over sellers;
2. a decrease in rental price ($\frac{dR}{d\tau_i}$) which pushes down demand for buying properties by households (because renting a house is now cheaper) and by investors (because letting a house is now less profitable).

Thanks to these two equilibrium effects, there is a price decrease even for those transactions that are not directly affected by the surcharge (p_h and p_h^L), namely the transactions for households wanting to buy a home. The direct tax incidence effect on investors explains the stronger impact on buy-to-let transaction prices.

4.5.7 Comparative statics: the long run

In the long run, the dwellings stock adjusts to reach a new equilibrium. In particular, the equilibrium equation for sellers' entry (4.28) can be rearranged as:

$$K(e) = \frac{-c_s + \theta m(\theta)}{(r+\delta)+\theta m(\theta)}\bar{p} \tag{4.34}$$

where $\bar{p}$ is the average price across all types of buyers. Since the surcharge on investors reduces all prices ($\frac{d\bar{p}}{d\tau_i} < 0$) and market tightness ($\frac{d\theta m(\theta)}{d\tau_i} < 0$), the construction cost must

[29] This term is unambiguously negative as each price must be larger than the construction cost K in an equilibrium in which each type of buyer is active, otherwise their respective transaction surplus would be negative.

decrease for the housing market to reach a new equilibrium ($\frac{dK(e)}{d\tau_i} < 0$). As construction cost is increasing in constructions e, the equilibrium number of constructions and the total dwellings stock should decrease. Indeed, the number of new constructions appears to decrease due to the surcharge especially at the end of the analysed period (Figure D.6), even though the total effect is insignificant (Table D.3). The adjustment of the dwellings stock should be smaller, the lower the elasticity of supply. Malpezzi and Maclennan (2001) estimate a low long-run price elasticity of supply of new residential construction in the UK (between 0 and 1), explaining the lack of a strong supply response to the surcharge.

Ultimately, the decrease in construction cost will amplify the negative effect on prices but also have a feedback effect on housing market tightness, which will lead to a new equilibrium. In Section 4.6, I calibrate the model to the pre-surcharge period and simulate the new equilibrium after the surcharge on investors is introduced. The comparison between the pre- and post-surcharge equilibrium shows that the model can qualitatively replicate all the main effects estimated in Section 4.4 and allows to perform a welfare analysis of the surcharge.

4.6 Identification and calibration

Assume that the housing matching function is Cobb-Douglas ($\theta m(\theta) = \nu\theta^{1-\gamma}$) and that the construction cost has the functional form $K(e) = a - \frac{b}{e}$ with $a, b > 0$. This functional form satisfies the property $K'(e) > 0$ and simplifies the solution of the model as sellers' entry (4.28) becomes linear in constructions e. The parameters to calibrate in the model are:
$\beta_h, \beta_i, c_h, c_i, c_s, \chi_h, \chi_i, \delta, \varepsilon_h, \varepsilon_i, \gamma, \nu, \pi_h, \pi_i, \psi_h, \psi_i, r, a, b, \sigma_h, \sigma_i, \mathcal{H}, Y, \rho_h, \rho_i$.[30]

I directly match some parameters to analogue moments or quantities in the data and I use previous estimates to calibrate other parameters. I derive the rest of the parameters by plugging data analogues into the model equations at the steady state (Table 4.3). For the estimation, I target data for 2015, the year before the introduction of the surcharge. I fix the transfer tax rate to the one corresponding to the median price (£204,000) in 2015, which is $\tau_h = \tau_i = 0.0075$. To estimate the housing market matching function parameters, I also target the estimated 4% increase in the time-to-sell caused by the 3% surcharge.

[30] We can also assume Cobb-Douglas matching functions for the credit market: $M_h(h_1, l_{h1}) = \nu_h l_{h1}^{\gamma_h} h_1^{1-\gamma_h}$ and $M_i(i_1, l_{i1}) = \nu_i l_{i1}^{\gamma_i} i_1^{1-\gamma_i}$. The parameters $\chi_{Lh}, \chi_{Li}, \gamma_h, \gamma_i, \nu_h, \nu_i$ are not necessary to obtain the equilibrium variables of interest in the model. They can be used to estimate the measure of lenders and the credit market tightness.

The demolition rate δ is calculated by dividing the number of demolitions over the number of vacant houses in 2015 (Ministry of Housing, 2019). This yields a demolition rate of 0.019. To compute the moving rate for owner-occupiers and buy-to-let investors, I restrict to the period 2008-2019 in which the indicator to distinguish them is available and compute the hazard rate for properties that already existed in 2008. The total number of properties owned by investors (rented) and households (owner-occupied) that existed in 2008 is observed. Since the hazard rate is assumed to be constant, the hazard rate for a single property is simply equal to the number of events (number of transactions in 2008-2019) divided by the time lapse (12 years). As virtually all standard transactions are lodged in the Land Registry, I assume that properties that do not appear in this dataset had zero events in the period of interest. Then, taking the average of all individual hazard rates, I obtain an average hazard rate for households of $\pi_h = 0.04$ and an average hazard rate for investors $\pi_i = 0.05$.

From model equations, we can estimate the probability a seller finds a buyer $\theta m(\theta)$:

$$\theta m(\theta) = \frac{\mathcal{H}}{D - \mathcal{H}} \left[\pi_h \left(\frac{h_2 + h_2^L}{\mathcal{H}} \right) + \pi_i \left(\frac{i_2 + i_2^L}{\mathcal{H}} \right) \right] \tag{4.35}$$

where we can observe the number of vacant homes $D - \mathcal{H}$, the number of occupied homes $\mathcal{H}$, the share of owner-occupied properties $\frac{h_2+h_2^L}{\mathcal{H}}$, and the share of privately rented properties $\frac{i_2+i_2^L}{\mathcal{H}}$ in 2015 (EHCS, 2004; Ministry of Housing, 2019). Using this method, I obtain an estimate of $\theta m(\theta) = 1.62$ or, equivalently, an average time to sell of around seven months. This is reassuringly close to the median of 208 days between the listing date and the sale date observed for properties listed on Zoopla in the period 2012-2019 (Consumer Data Research Centre, 2020b). In addition, I target an average time-to-buy of one year (Zoopla, 2022) to obtain $m(\theta)$ and θ.

To identify σ_h and σ_i, I target the number of households' mortgages h_2^L, the number of buy-to-let mortgages i_2^L as well as the number of total properties occupied by households $h_2 + h_2^L$ and rented $i_2 + i_2^L$ (Council of Mortgage Lenders, 2022; Ministry of Housing, 2022a). Then, using model equations we can estimate:

$$\sigma_j = \frac{j_2[1 + \frac{m(\theta)}{\pi_j}]}{j_2^L[1 + \frac{m(\theta)}{\pi_j}\frac{m(\theta)}{q_j(\phi_j)}] + j_2[1 + \frac{m(\theta)}{\pi_j}]}, \quad j \in \{h, i\} \tag{4.36}$$

This yields an estimate for the share of households and investors that search for a property without a mortgage of $\sigma_h = 0.35$ and $\sigma_i = 0.62$, respectively.

I calibrate the ratio of the search cost for sellers to the average price to be 0.01 in order to account for a 1% maintenance cost (BCIS, 2022). In absence of data, the estimated search cost for investors c_i is assumed to be equal to the cost for sellers. The estimated search cost for households c_h is based on the opportunity cost of the time spent searching a property, following the approach by Ngai and Sheedy (2020). I assume one property viewing entails the loss of half a day of average annual income y in 2015 (ONS, 2022). The value of c_h and c_i is equal to the opportunity cost of making the expected number of viewings. According to leading estate and letting agents (LSL property services plc, 2022), the average number of viewings before buying is $V_b = 9$. To buy a property the average cost is $c_h = V_b \frac{y}{2 \cdot 365}$. As the time to buy a property is $1/m(\theta)$, the expected annual search cost for a buyer is set to $m(\theta) V_b \frac{y}{2 \cdot 365}$, which corresponds to 1.2% of annual income.

Construction cost for each year t is identified by using sellers' entry equation

$$K = \left(\frac{\theta m(\theta) - \frac{c_s}{\bar{p}}}{r + \delta + \theta m(\theta)} \right) \bar{p} \tag{4.37}$$

where $\bar{p}$ is the observed median price in 2015. Targeting an elasticity of supply of $\eta = 0.1$ (Malpezzi and Maclennan, 2001), I can estimates $a = K(1 + \frac{1}{\eta})$ and $b = \frac{\delta(D - \mathcal{H})K}{\eta}$
.

For lack of data, I assume a symmetric bargaining power both in the credit market $\psi_h = \psi_i = 0.5$, as in Petrosky-Nadeau and Wasmer (2013), and in the housing market $\beta_h = \beta_i = 0.5$, as in Ngai and Sheedy (2020). Since it takes between four and six weeks to obtain a mortgage (Barclays plc, 2023), I target an average mortgage-search duration of one month: $\frac{1}{q_h(\phi_h)} = \frac{1}{q_i(\phi_i)} = \frac{1}{12}$.

To estimate buyers' expected credit search costs, I use buyers' entry (BE_j) and price equations (4.24)-(4.25) which yield:

$$\frac{\chi_j}{q_j(\phi_j)} = \frac{(1 + \tau_j)(\bar{p}_j - K)m(\theta)\frac{(1-\beta_j)}{\beta_j} - c_j}{\left(\frac{j_1}{j^L + j_1}\right)\frac{(r+\pi_j)\{r + m(\theta)[1 - \beta_j(1-\psi_j)]\}}{[r + \pi_j + (1-\beta_j)m(\theta)](1-\psi_j)} + \left(1 - \frac{j_1^L}{j^L + j_1}\right)r}, \quad j \in \{h, i\} \tag{4.38}$$

where $\bar{p}_j$ is the observed median price paid by type-j buyer, and $\frac{j_1}{j^L + j_1}$ is the share of type-j buyer that buys properties without a mortgage.[31]

Rearranging (BE_j) we obtain the home-owning utility for households and investors:

$$\begin{aligned} \varepsilon_j = & - R + rK(e) + (r + \pi_j)\tau_j K(e) \\ & + \frac{r + \pi_j}{m(\theta)(1 - \beta_j)} \left(\frac{\frac{\chi_j}{q_j(\phi_j)}\{r + m(\theta)[1 - \beta_j(1 - \psi_j)]\}}{(1 - \psi_j)} + c_j \right), \quad j \in \{h, i\} \end{aligned} \tag{4.39}$$

[31] Note that using the observed values for h_2, h_2^L, i_2 and i_2^L we can estimate h_1, h_1^L, i_1 and i_1^L from Equations (D.10) and (D.11) as $j_1 = \frac{\pi_j j_2}{m(\theta)}$ and $j_1^L = \frac{\pi_j j_2^L}{m(\theta)}$ for $j \in \{h, i\}$.

I estimate that households receive an annual intrinsic utility of $£5,277$, whereas investors lose $£2,180$ per year from owning a property (e.g. maintenance costs, agency fees, income tax) which is more than compensated by the rental price of $£9,456$ they receive.

I match households' income Y with the average earnings in 2015 in the UK from the Annual survey of hours and earnings (ONS, 2022). For mortgage repayments ρ_h and ρ_i, I target the average loan-to-value ratios for residential and for buy-to-let mortgages using loan equation (4.23) (Bank of England, 2021; ONS, 2021b). Finally, I estimate the housing matching function parameters ν and γ by minimizing the sum of squares between the simulated change in equilibrium time-to-sell after the surcharge is introduced in the model and the 4% increase in time-to-sell estimated in the quasi-experimental analysis of Section 4.4.[32]

4.7 Quantitative results

4.7.1 Model validation

In Table 4.4, I compare housing market outcomes under three scenarios:

1. the outcome simulated before the surcharge and perfectly matched with data in 2015;
2. the outcome obtained by multiplying the pre-surcharge outcome with the estimated surcharge effect in the reduced-form estimates of Section 4.4;
3. the outcome simulated after introducing the surcharge in the model.

By construction, the model replicates exactly the 4% increase in the number of days to sell a property estimated in Section 4.4, which rises from 225 to 234. The model is also able to capture qualitatively all the other effects. In the post-surcharge equilibrium, simulated transactions fall by 4.6% compared to the estimated reduction of 10.2%. The dwellings stock shows a very small decrease both in the simulated post-surcharge equilibrium (-0.02%) and in the insignificant estimates (-0.06%). Simulated prices for both buy-to-let investors and owner-occupiers decrease and the simulated price decrease is larger for buy-to-let investors. However, the model largely amplifies the magnitude of the impact on prices relative to the reduced-form estimates. The puzzle that large changes in property prices are necessary to generate the observed fluctuations in housing market

[32] This is the average of the estimates across the different regression specifications in Table D.6 after adjusting for the average share of rented properties.

TABLE 4.3: Calibration

Parameter	Identification	Dataset/estimate	Value
δ	Target percentage of UK vacant stock that in 2015.	Author's estimates using Ministry of Housing (2022a)	$\delta = 0.019$
$\mathcal{H}$	Target non-vacant privately owned dwellings in 2015.	Ministry of Housing (2022a) and EHCS (2004)	$\mathcal{H} = 18.96m$
π_h, π_i	Average ratio between number of transactions and years observed across owner-occupiers' and investors' properties in 2008-2019.	Author's estimates using HM Land Registry (2019) and DLUHC (2022a)	$\pi_h = 0.04, \pi_i = 0.05$
c_s, c_i	Target maintenance cost as a percentage of price.	Author's estimates using BCIS (2022) and HM Land Registry (2019)	$c_s = c_i = 2049.5$
c_h	Target house visits before buying and median income.	Author's estimates using (LSL property services plc, 2022; ONS, 2022)	$c_h = 340.5$
a, b	Previous estimate of elasticity of supply	Author's estimates using Malpezzi and Maclennan (2001)	$a = 20.2m, b = 0.19m$
ψ_h, ψ_i	Previous calibration	Petrosky-Nadeau and Wasmer (2013)	$\psi_h = \psi_i = 0.5$
γ, ν	Model equations. Target vacant and occupied homes, shares of owner-occupied and rented properties in 2015. Target time to buy and post-surcharge 4% increase in time-to-sell.	Author's estimates using Zoopla (2022), Ministry of Housing (2022a)	$\gamma = 0.96, \nu = 1.59$
β_h, β_i	Previous calibration	Ngai and Sheedy (2020)	$\beta_h = \beta_i = 0.5$
σ_h, σ_i	Model equations. Target households' and investors' mortgages, owner-occupied and rented properties.	Author's estimates using Council of Mortgage Lenders (2022) and Ministry of Housing (2022a)	$\sigma_h = 0.35, \sigma_i = 0.62$
r	Target average 1-month Gilt repo interest rate in 2015	Bank of England (2018)	$r = 0.01$
$\frac{\chi_i}{q_i(\phi_i)}$	Model equations. Target median price paid by investors, rental price, number of investors' mortgages and rented properties in 2015.	Council of Mortgage Lenders (2022), HM Land Registry (2019), DLUHC (2022a)	$\frac{\chi_i}{q_i(\phi_i)} = 27,075$
$\frac{\chi_h}{q_h(\phi_h)}$	Model equations. Target median price paid by households, rental price, households' mortgages and properties in 2015.	Council of Mortgage Lenders (2022), HM Land Registry (2019), DLUHC (2022a)	$\frac{\chi_h}{q_h(\phi_h)} = 83,042$
ε_h	Model equations. Target median rental price in 2015.	Valuation Office Agency (2019)	$\varepsilon_h = 5,277$
ε_i	Model equations. Target median rental price in 2015.	Valuation Office Agency (2019)	$\varepsilon_i = -2,180$
Y	Average earnings in 2015.	Annual survey of hours and earnings (ONS, 2022)	$Y = 27,615$
ρ_i, ρ_h	Average loan-to-value ratios for residential and buy-to-let mortgages.	ONS (2021b), Bank of England (2021)	$\rho_h = 15,189, \rho_i = 11,429$

TABLE 4.4: Comparison between pre-surcharge data, post-surcharge reduced-form estimates and model simulation.

	Pre-surcharge	Estimates	Model
Transactions	804,978	722,870	767,963
Property price, investors (£)	204,950	199,416	44,386
Property price, households (£)	204,950	200,646	44,460
Dwellings stock (millions)	19.458	19.446	19.454
Rental price (£)	9,456	9,097	7,702
Homeownership rate	0.75	0.77	0.95
Welfare (million £)	48,521		49,654
Welfare, households per capita (£)	13,159		15,225
Welfare, investors per capita (£)	4,448		2,971

Notes: The column 'Pre-surcharge' reports the pre-surcharge outcome value simulated before the introduction of the surcharge and perfectly matched with data in 2015. The column 'Estimates' reports the post-surcharge outcome obtained by multiplying the pre-surcharge outcome with the surcharge effect estimated in Section 4.4. The column 'Model' reports the post-surcharge outcome obtained by simulating the model after introducing the surcharge. Reduced-form estimates are not available for the post-surcharge rental price and home-ownership rate, so I use aggregate UK data in 2019 for these outcomes.

tightness is analogue to the 'unemployment volatility puzzle' described by Shimer (2005) for classical labor search models of the business cycle.[33]

The model allows to predict outcomes that cannot be estimated using the empirical strategy described in Section 4.3. The model predicts a reduction in annual rental prices from $£9,456$ to $£7,702$, which is not far from the fall in average rents to $£9,097$ observed in aggregate data in the entire UK from 2016 to 2019. Intuitively, the surcharge has an unambiguously positive effect on home-ownership: owner-occupiers increases their share of private dwellings from 75% to 95% in the new model equilibrium at the expense of investors. In Figure D.8, indeed we see that the UK home-ownership rate declined steadily after the introduction of buy-to-let mortgages, but the surcharge inverted the trend and the share was higher in 2019 relative to 2016.

4.7.2 Welfare analysis

Assume the government equally redistributes all tax revenues from stamp-duty taxes τ_h and τ_i to the agents in the economy. Also assume that the social planner is utilitarian and weighs each agent's welfare equally. The total flow value of utility net of costs over all

[33] These models require very large wage changes to generate the magnitude of observed employment fluctuations. To increase responses of tightness to prices, recent models use different methods that ultimately diminish the fundamental surplus fraction, the proportional difference between productivity and workers' value of leisure (Ljungqvist and Sargent, 2017).

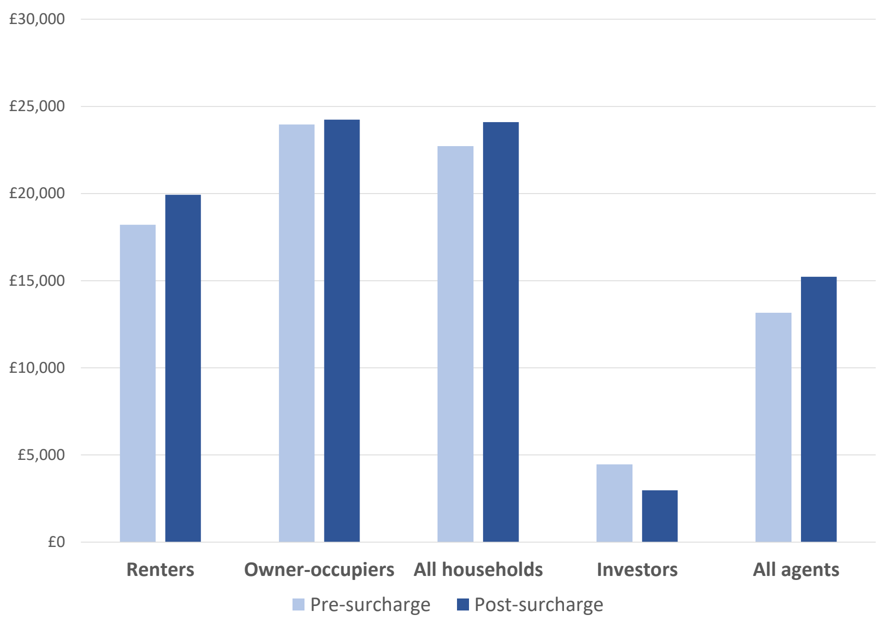
agents in the economy is:

$$
\begin{aligned}
rW =& \mathcal{H}y - h_0^L \chi_h - h_1^L c_h + h_2^L \varepsilon_h - h_1 c_h + h_2 \varepsilon_h - i_0^L \chi_i - i_1^L c_i + i_2^L \varepsilon_i - i_1 c_i + i_2 \varepsilon_i \\
& - l_{h0} \chi_{lh} - l_{i0} \chi_{li} - s c_s - eK
\end{aligned}
\tag{4.40}
$$

I compare the flow welfare rW before and after introducing the surcharge to understand whether the surcharge was welfare-improving. As we can see in Table 4.4, the surcharge on investors increased total welfare by 2.3%. The main reason behind the welfare increase is the rise in households' home-ownership at the expense of investors' ownership. The flow utility households receive from owning the property in which they live is positive ($\varepsilon_h = \pounds 5,277$), whereas the utility investors receive from letting the property they own is negative ($\varepsilon_i = -\pounds 2,180$). Investors are active in the property market because the rental price makes it a profitable investment ($R - \varepsilon_i > 0$) , but they do not take into account that their competition in the property market reduces households' utility by making it longer and more expensive to find a property to buy, thus reducing the number of owner-occupied properties. The surcharge on investors increased welfare because it partially offset the negative externality that investors imposed on households, by reducing their number.

Note that property and rental prices do not appear in the welfare function, as they are transfers between agents, who are equally weighted in the calculation of social welfare. If society values households more than investors, the estimated welfare increase would be a lower bound of the welfare change. This is because, thanks to the surcharge, households pay lower rental prices to investors, and investors have to pay a higher stamp-duty tax than households but revenues are equally distributed. In Section D.3.5, I compute the welfare per capita for households and for investors separately, taking into account the effect of the tax on rental and property prices. Figure 4.11 shows that the surcharge increases utility per capita for households by $\pounds 1,366$ (+6.0%), but it decreases utility per capita for investors by $\pounds 1,477$ (-33.2%), resulting in an overall welfare increase. Renters' utility per capita increases by $\pounds 1,717$ ($+9.4\%$) thanks to the decrease in rental prices. The utility of household buyers and owner-occupiers increases by $\pounds 277$ ($+1.2\%$). Their utility is higher than renters' utility and the surcharge increases the number of owner-occupiers relative to renters, having a positive effect on overall utility per capita that amounts to $\pounds 2,067$.

FIGURE 4.11: Welfare per capita (£ per year)



Notes: From lighter to darker color and from left to right, the histogram represents the pre-surcharge and the post-surcharge welfare per capita for renters, owner-occupiers, households, investors and all agents, obtained by simulating the model and plugging the outcomes into Equations (D.49) and (D.50) and (D.51).

4.8 Conclusions

This paper studies the effects of a transfer tax surcharge in the UK that targeted property investors, but not owner-occupiers. Using an incremental difference-in-differences estimator, I document equilibrium effects that can hardly be reconciled with a frictionless and perfectly competitive housing market. The surcharge reduced pre-tax property prices for all buyers, but more for buy-to-let investors than for future owner-occupiers. The decline in prices was larger than the tax itself. This overshifting can be explained by search frictions in the property market. The tax increased time-to-sell and reduced the volume of transactions which amplified the effect on prices, even if the housing supply was not affected in the medium run.

Using a tractable search model with ownership, rental and credit markets, I show that equilibrium effects can rationalize the empirical findings. The surcharge reduces housing market tightness, which in turn decreases rental prices and construction costs leading to a new equilibrium in which housing becomes more affordable for owner-occupiers and the home-ownership rate rises. The model offers an important caveat for policymakers: if the surcharge becomes excessively high, the effects are reversed. Not enough buy-to-let investors will enter the market, the rental price will increase and too many households will search for a property to buy, inducing property prices to rise.

Most previous papers find that unconditional transfer taxes lead to deadweight losses. My study shows that a moderate transfer tax targeting investors can increase social welfare by offsetting the crowding-out externality that investors impose on owner-occupiers while competing for the same properties. An interesting avenue for future research is to check whether the normative implications hold in a richer theoretical framework in which the moving decision is endogenous and housing quality is heterogeneous. The results of this analysis have first-order relevance for policymakers because increasing home-ownership and decreasing property prices without discouraging housing supply is among the main objectives of current housing policies in many countries.

Appendix A

Appendix: Parliamentary pensions, Government Stability and Party Control

A.1 Voting against party directives

Define 'voting against party directives' as voting confidence when elected in an opposition party and voting against confidence when elected in a majority party. Voting against party directives is negatively correlated with the probability of being re-elected. To show this, I restrict to the sample of MPs in their first term and regress:

$$Reelected_i = \alpha + \boldsymbol{\beta}\mathbf{x}_i + \delta Deviate_{it} + \eta_{pg} + \varepsilon_{ipgt} \tag{A.1}$$

where $Reelected_i$ is an indicator variable equal to 1 if the MP is ever re-elected for a second term in Parliament and 0 otherwise, and $Deviate_{it}$ is an indicator variable equal to 1 if the first-term MP voted against party directives at time t and 0 otherwise. η_{pg} are party-by-government fixed effects and $\mathbf{x}_i$ is a vector of individual characteristics: gender, high school diploma, university degree, born in South, born in Center, foreign, pre-parliament income. I repeat this regression with and without controls and fixed effects. As we can see in Table A.1, first-term MPs who vote against their party directives are 10-25$\%$ less likely to be re-elected, depending on the specification.[1] This significantly negative correlation corroborates the corresponding assumption of the model presented in Section 1.2.

[1]Results do not qualitatively change if I use Probit or Logit models instead of a linear probability model.

TABLE A.1: Correlation between voting against party directives and being re-elected in parliament.

	(1)	(2)	(3)
Vote against party directives	-0.242*** (0.005)	-0.252*** (0.008)	-0.097*** (0.009)
N	99,797	64,789	64,789
R-squared	0.01	0.01	0.37
Average outcome	0.65	0.65	0.65
Party-by-gov. FE	NO	NO	YES

Notes: The regression sample is restricted to confidence votes by MPs in the first term. Controls are: gender, high school diploma, university degree, born in South, born in Center, foreign, pre-parliament income. Standard errors are Eicker-Huber-White heteroskedasticity-robust. Average outcome is the average of the outcome variable after reaching the tenure threshold.

A.2 Additional Tables

TABLE A.2: Summary statistics, by party stance towards government

	Tenure below 4.5 years	Tenure above 4.5 years
Majority		
Abstains/no vote	.16	.13
Confidence if voting	.96	.95
Vote against party directives	.03	.05
Number of terms	1.44	2.05
Tenure (years)	3.99	4.79
Age (years)	51.37	53.01
Female	.33	.26
Pre-parliament income (€)	103723.1	118324.1
High school	.99	.99
University degree	.74	.73
Born in southern Italy	.38	.36
Born in central Italy	.23	.27
Born outside Italy	.02	.02
Observations	19928	7373
Opposition		
Abstains/no vote	.22	.3
Confidence if voting	.1	.08
Vote against party directives	.08	.08
Number of terms	1.71	2.17
Tenure (years)	4.03	4.86
Age (years)	50.2	52.08
Female	.24	.21
Pre-parliament income (€)	65914.07	85675.11
High school	.96	.96
University degree	.66	.67
Born in southern Italy	.32	.34
Born in central Italy	.22	.22
Born outside Italy	.02	.02
Observations	7239	5151

Notes: The sample is restricted to tenure between 3.5 and 5.5 years for votes of confidence between 2001 and 2022 (legislatures XIV-XVIII).

TABLE A.3: Summary statistics, by house

	Tenure below 4.5 years	Tenure above 4.5 years
Chamber		
Abstains/no vote	.17	.2
Confidence if voting	.74	.64
Vote against party directives	.04	.04
Number of terms	1.59	2.16
Tenure (years)	4.01	4.82
Age (years)	48.87	51.05
Female	.3	.24
Pre-parliament income (€)	91047.77	104683.7
High school	.98	.98
University degree	.71	.7
Born in southern Italy	.37	.36
Born in central Italy	.23	.24
Born outside Italy	.02	.02
Observations	18649	9251
Senate		
Abstains/no vote	.21	.24
Confidence if voting	.72	.64
Vote against party directives	.06	.1
Number of terms	1.35	1.92
Tenure (years)	3.98	4.82
Age (years)	55.71	56.7
Female	.31	.23
Pre-parliament income (€)	100973.2	102537.3
High school	0	0
University degree	0	0
Born in southern Italy	.38	.36
Born in central Italy	.21	.23
Born outside Italy	.02	.02
Observations	9435	3854

Notes: The sample is restricted to tenure between 3.5 and 5.5 years for votes of confidence between 2001 and 2022 (legislatures XIV-XVIII).

TABLE A.4: Diff-in-disc estimates of minimum tenure requirement on pre-determined variables.

	(1) Female	(2) High school	(3) Degree	(4) South	(5) Center	(6) North	(7) Foreign	(8) Income (€)
Post*Tenure under 4.5 years	-0.021 (0.017)	0.011 (0.010)	0.014 (0.026)	0.025 (0.024)	-0.024 (0.020)	-0.005 (0.024)	0.004 (0.006)	-6978.631 (8820.270)
N	33,330	22,055	22,055	33,330	33,330	33,330	33,330	33,245
R-squared	0.03	0.00	0.00	0.00	0.00	0.00	0.00	0.01
Average outcome	0.32	0.98	0.70	0.35	0.24	0.40	0.02	86090.61
MP FE	NO	NO	NO	NO	NO	NO	NO	NO
Party-by-gov. FE	NO	NO	NO	NO	NO	NO	NO	NO

Notes: The regression sample is restricted to votes of confidence in a bandwidth of 12 months on each side of the cutoff. Standard errors are Eicker-Huber-White heteroskedasticity-robust. High school and University degree refer to the highest education level achieved. South, Center, North and Foreign refer to the birthplace. Income refers to the private income in the year prior to entering Parliament. Data on education levels is not available for senators. Average outcome is the average of the outcome variable after reaching the tenure threshold.

TABLE A.5: Diff-in-disc estimates of minimum tenure requirement on confidence, by house.

	Chamber of Deputies			Senate		
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Tenure under 4.5 years	0.085*** (0.028)	0.031*** (0.010)	0.031*** (0.004)	0.029 (0.042)	-0.000 (0.019)	0.033*** (0.008)
N	22,931	22,931	22,931	10,399	10,399	10,399
R-squared	0.03	0.80	0.96	0.03	0.70	0.94
Average outcome	0.71	0.71	0.71	0.70	0.70	0.70
MP FE	NO	NO	YES	NO	NO	YES
Party-by-gov. FE	NO	YES	YES	NO	YES	YES

Notes: The regression sample is restricted to a bandwidth of 12 months on each side of the cutoff. Standard errors are Eicker-Huber-White heteroskedasticity-robust. Average outcome is the average of the outcome variable after reaching the tenure threshold.

TABLE A.6: Diff-in-disc estimates of minimum tenure requirement on confidence, all sample.

	All		
	(1)	(2)	(3)
Post*Tenure under 4.5 years	0.060***	0.019*	0.030***
	(0.020)	(0.010)	(0.007)
N	33,330	33,330	33,330
R-squared	0.02	0.76	0.95
Average outcome	0.71	0.71	0.71
MP FE	NO	NO	YES
Party-by-gov. FE	NO	YES	YES

Notes: The regression sample is restricted to a bandwidth of 12 months on each side of the cutoff. Standard errors are Eicker-Huber-White heteroskedasticity-robust. Average outcome is the average of the outcome variable after reaching the tenure threshold.

TABLE A.7: Diff-in-disc estimates of minimum tenure requirement on confidence, local quadratic regressions.

	All			Chamber of Deputies			Senate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post*Tenure under 4.5 years	-0.015	0.026**	0.039***	-0.006	0.035**	0.037***	0.109*	0.010	0.041***
	(0.032)	(0.013)	(0.006)	(0.039)	(0.014)	(0.006)	(0.062)	(0.028)	(0.014)
N	33,330	33,330	33,330	22,931	22,931	22,931	10,399	10,399	10,399
R-squared	0.03	0.76	0.95	0.03	0.80	0.96	0.03	0.70	0.94
Average outcome	0.72	0.72	0.72	0.73	0.73	0.73	0.70	0.70	0.70
MP FE	NO	NO	YES	NO	NO	YES	NO	NO	YES
Party-by-gov. FE	NO	YES	YES	NO	YES	YES	NO	YES	YES

Notes: The regression sample is restricted to a bandwidth of 12 months on each side of the cutoff. Standard errors are Eicker-Huber-White heteroskedasticity-robust. is the average of the outcome variable after reaching the tenure threshold.

TABLE A.8: Diff-in-disc estimates of minimum tenure requirement on abstension/no vote, all sample.

	All		
	(1)	(2)	(3)
Post*Tenure under 4.5 years	-0.032*	0.001	-0.015
	(0.017)	(0.017)	(0.016)
N	41,189	41,189	41,189
R-squared	0.01	0.10	0.31
Average outcome	0.19	0.19	0.19
MP FE	NO	NO	YES
Party-by-gov. FE	NO	YES	YES

Notes: The regression sample is restricted to a bandwidth of 12 months on each side of the cutoff. Standard errors are Eicker-Huber-White heteroskedasticity-robust. Average outcome is the average of the outcome variable after reaching the tenure threshold.

TABLE A.9: Diff-in-disc estimates of minimum tenure requirement on abstension/no vote, all sample.

	All		
	(1)	(2)	(3)
Post*Tenure under 4.5 years	-0.032*	0.001	-0.015
	(0.019)	(0.019)	(0.019)
N	41,189	41,189	41,189
R-squared	0.01	0.10	0.31
Average outcome	0.19	0.19	0.19
MP FE	NO	NO	YES
Party-by-gov. FE	NO	YES	YES

Notes: The regression sample is restricted to a bandwidth of 12 months on each side of the cutoff. Standard errors are Eicker-Huber-White heteroskedasticity-robust. Average outcome is the average of the outcome variable after reaching the tenure threshold.

TABLE A.10: Diff-in-disc estimates of minimum tenure requirement on confidence, by type of party.

	Female			Male		
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Tenure under 4.5 years	0.026	0.017	0.011*	0.071***	0.026***	0.037***
	(0.066)	(0.027)	(0.006)	(0.025)	(0.010)	(0.005)
N	9,419	9,419	9,419	23,911	23,911	23,911
R-squared	0.04	0.74	0.96	0.02	0.77	0.95
Average outcome	0.77	0.77	0.77	0.70	0.70	0.70
MP FE	NO	NO	YES	NO	NO	YES
Party-by-gov. FE	NO	YES	YES	NO	YES	YES

Notes: The regression sample is restricted to a bandwidth of 12 months on each side of the cutoff. Standard errors are Eicker-Huber-White heteroskedasticity-robust. Average outcome is the average of the outcome variable after reaching the tenure threshold.

TABLE A.11: Regression discontinuity estimates interacted with age.

	Majority party		Opposition party	
	(1)	(2)	(3)	(4)
Tenure under 4.5 years	-0.066*	-0.037	0.081	0.092
	(0.037)	(0.036)	(0.086)	(0.075)
Tenure under 4.5 years*Age (years)	0.002***	0.001**	-0.003*	-0.003**
	(0.001)	(0.001)	(0.002)	(0.001)
N	19,218	19,218	7,250	7,250
R-squared	0.01	0.06	0.02	0.20
MP FE	NO	NO	NO	NO
Party-by-gov. FE	NO	YES	NO	YES

Notes: The regression sample is restricted to a bandwidth of 12 months on each side of the cutoff. Standard errors are Eicker-Huber-White heteroskedasticity-robust. Average outcome is the average of the outcome variable after reaching the tenure threshold. Age is the age of the MP at the moment of the vote

TABLE A.12: Regression discontinuity estimates interacted with the majority margin.

	Majority party			Opposition party		
	(1)	(2)	(3)	(4)	(5)	(6)
Tenure under 4.5	0.077***	0.089***	0.072***	-0.122***	-0.091***	-0.001
years	(0.013)	(0.013)	(0.007)	(0.023)	(0.020)	(0.008)
Tenure under 4.5	-0.027***	-0.028***	-0.026***	0.055***	0.028**	0.005
years*Majority margin ('00)	(0.006)	(0.006)	(0.004)	(0.013)	(0.013)	(0.007)
N	19,218	19,218	19,218	7,250	7,250	7,250
R-squared	0.01	0.05	0.79	0.04	0.20	0.89
MP FE	NO	NO	YES	NO	NO	YES
Party FE	NO	YES	YES	NO	YES	YES

Notes: The regression sample is restricted to a bandwidth of 12 months on each side of the cutoff. Standard errors are Eicker-Huber-White heteroskedasticity-robust. Average outcome is the average of the outcome variable after reaching the tenure threshold. Majority margin indicates the majority margin in the house in which the confidence vote took place measured in hundred MPs.

TABLE A.13: Diff-in-disc estimates of minimum tenure requirement on pre-determined variables, by house.

	(1) Female	(2) High school	(3) Degree	(4) South	(5) Center	(6) North	(7) Foreign	(8) Income (€)
Chamber								
Post*Tenure under	-0.024	0.011	0.014	0.030	-0.039	0.005	0.003	-9042.003
4.5 years	(0.022)	(0.010)	(0.026)	(0.028)	(0.025)	(0.028)	(0.008)	(8718.947)
N	22,931	22,055	22,055	22,931	22,931	22,931	22,931	22,872
R-squared	0.03	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Average outcome	0.32	0.98	0.70	0.34	0.24	0.40	0.02	88749.14
Senate								
Post*Tenure under	-0.049*			0.024	-0.024	0.006	-0.006	22568.439
4.5 years	(0.029)			(0.047)	(0.038)	(0.047)	(0.006)	(23835.848)
N	10,399			10,399	10,399	10,399	10,399	10,373
R-squared	0.04			0.00	0.00	0.00	0.00	0.02
Average outcome	0.32			0.35	0.23	0.40	0.02	80624.78
MP FE	NO	NO	NO	NO	NO	NO	NO	NO
Party-by-gov. FE	NO	NO	NO	NO	NO	NO	NO	NO

Notes: The regression sample is restricted to votes of confidence in a bandwidth of 12 months on each side of the cutoff. Standard errors are Eicker-Huber-White heteroskedasticity-robust. High school and University degree refer to the highest education level achieved. South, Center, North and Foreign refer to the birthplace. Income refers to the private income in the year prior to entering Parliament. Data on education levels is not available for senators. Average outcome is the average of the outcome variable after reaching the tenure threshold.

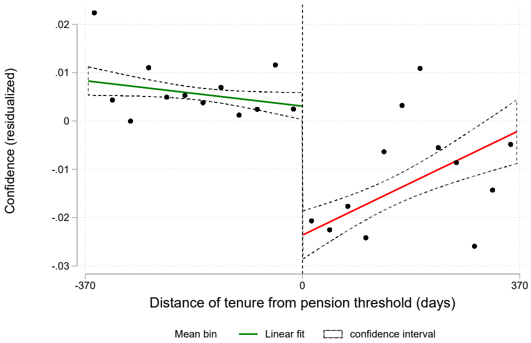
TABLE A.14: Diff-in-disc estimates of minimum tenure requirement on pre-determined variables, by party's stance with respect to the current government.

	(1) Female	(2) High school	(3) Degree	(4) South	(5) Center	(6) North	(7) Foreign	(8) Income (€)
				Majority party				
Post*Tenure under	-0.019	0.008*	-0.001	0.054*	-0.050**	-0.007	0.003	-13913.420
4.5 years	(0.020)	(0.005)	(0.031)	(0.030)	(0.023)	(0.030)	(0.008)	(12970.827)
N	23,064	15,386	15,386	23,064	23,064	23,064	23,064	23,007
R-squared	0.04	0.01	0.01	0.01	0.01	0.00	0.00	0.01
Average outcome	0.35	0.99	0.72	0.35	0.26	0.38	0.02	92779.88
				Opposition party				
Post*Tenure under	-0.003	-0.001	-0.014	-0.053	0.054	-0.000	-0.001	-7642.876
4.5 years	(0.034)	(0.025)	(0.050)	(0.040)	(0.041)	(0.043)	(0.012)	(6624.275)
N	9,226	5,987	5,987	9,226	9,226	9,226	9,226	9,198
R-squared	0.01	0.01	0.00	0.00	0.03	0.01	0.00	0.03
Average outcome	0.25	0.97	0.66	0.31	0.20	0.47	0.03	67602.94
MP FE	NO	NO	NO	NO	NO	NO	NO	NO
Party-by-gov. FE	NO	NO	NO	NO	NO	NO	NO	NO

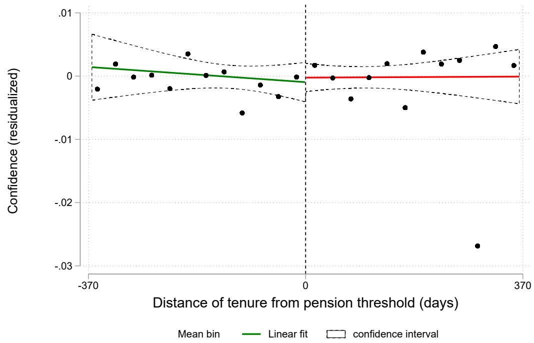
Notes: The regression sample is restricted to votes of confidence in a bandwidth of 12 months on each side of the cutoff. Standard errors are Eicker-Huber-White heteroskedasticity-robust. High school and University degree refer to the highest education level achieved. South, Center, North and Foreign refer to the birthplace. Income refers to the private income in the year prior to entering Parliament. Data on education levels is not available for senators. Average outcome is the average of the outcome variable after reaching the tenure threshold.

A.3 Additional Figures

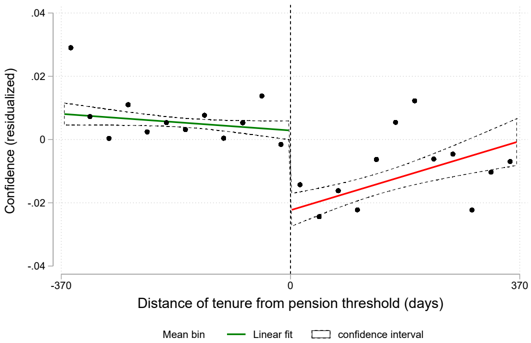
FIGURE A.1: Regression discontinuities by House, post-treatment and pre-treatment



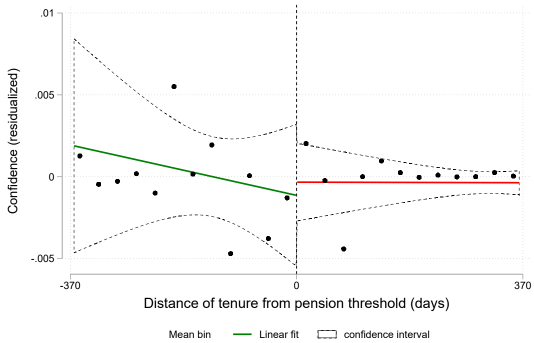
(A) Both Houses, post-treatment



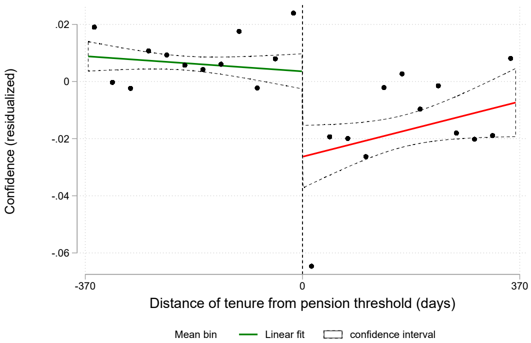
(B) Both Houses, pre-treatment



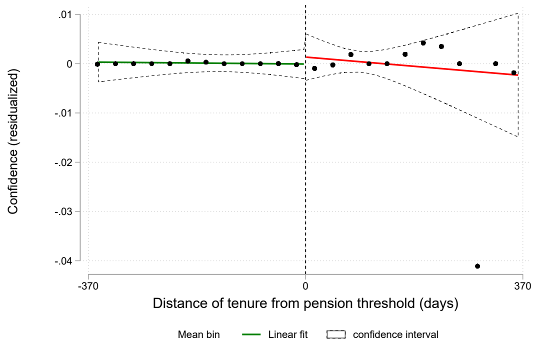
(C) Chamber of Deputies, post-treatment



(D) Chamber of Deputies, pre-treatment



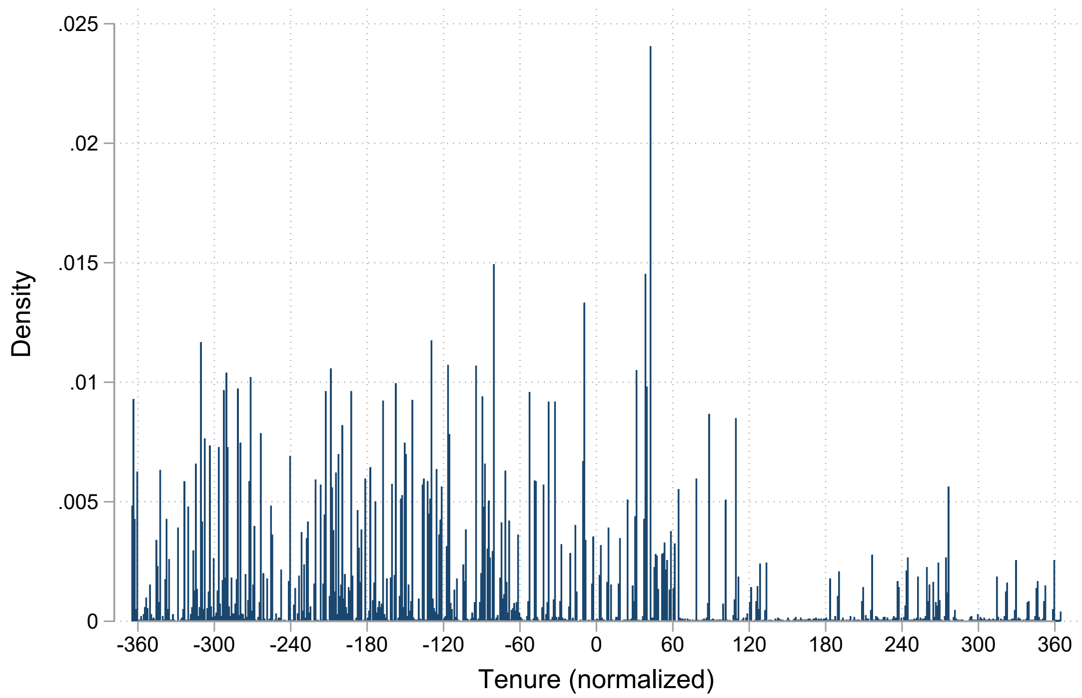
(E) Senate, post-treatment



(F) Senate, pre-treatment

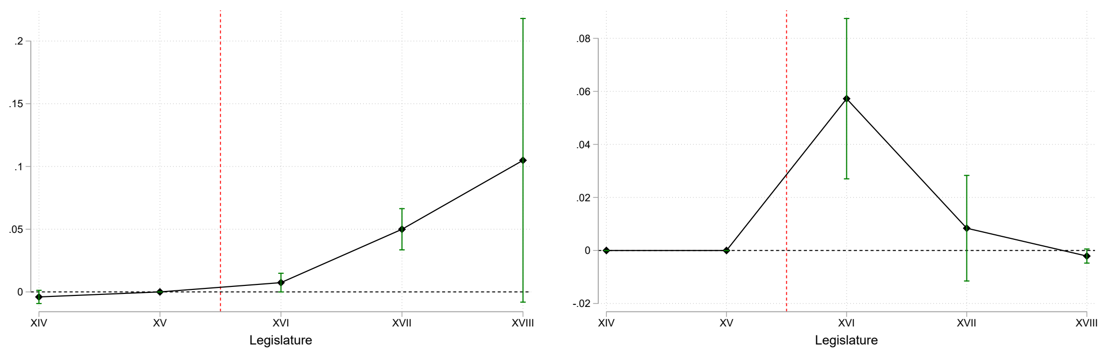
Notes: These figures show the effect of the parliamentary tenure distance from the 4.5 year-cutoff on the MP's probability of voting confidence in the government. The circles are averages across monthly bins on either side of the threshold, while the solid and dashed lines represent the predicted values and confidence intervals of a local linear regression of the outcome on (days of tenure, normalized) and the fixed effects, separately for each side of the cutoff. The bandwidth includes observations within one year from the 4.5 year-cutoff.

FIGURE A.2: Density of confidence votes



Notes: This figure plots the density of confidence votes in daily bins for both Houses over the number of days of tenure from the 4.5-year threshold.

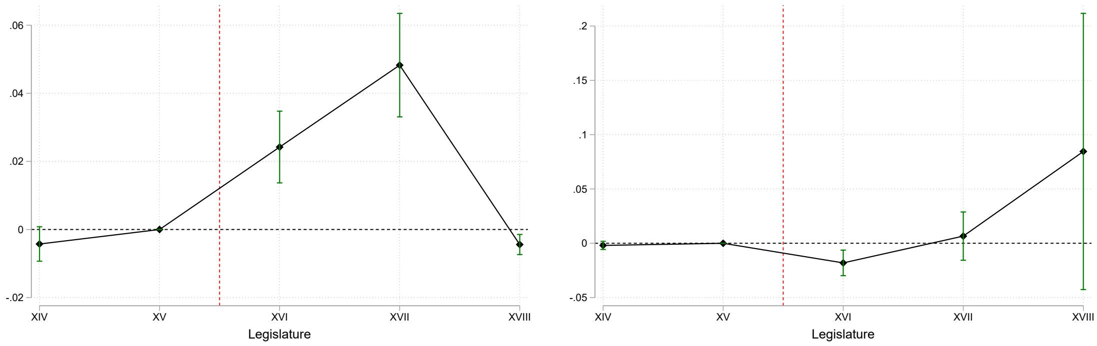
FIGURE A.3: RD coefficients, by legislature and House



(A) Chamber of Deputies (B) Senate

Notes: These figures show the RD coefficient and its 95% confidence interval estimating regression Equation (1.14), separately for each House and legislature (XIV-XVIII).

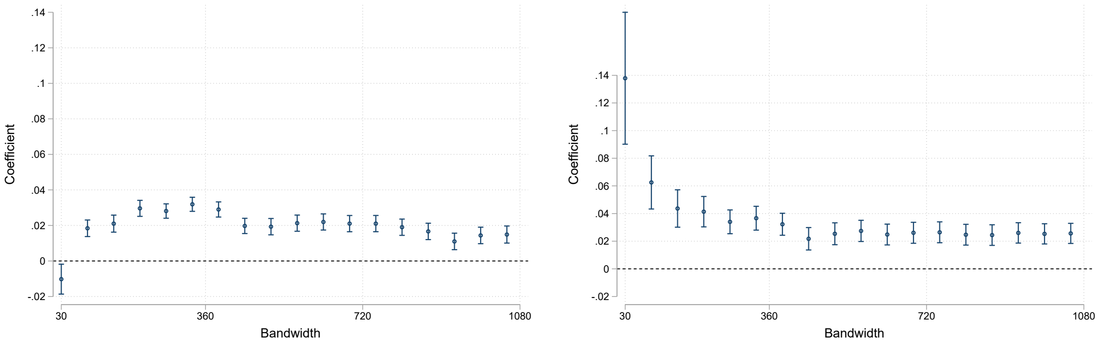
FIGURE A.4: RD coefficients, by party's stance with respect to the current government



(A) Elected in majority party (B) Elected in opposition party

Notes: These figures show the RD coefficient and its 95% confidence interval estimating regression Equation (1.14), separately for MPs elected in parties supporting the government and MPs elected in parties opposing the government.

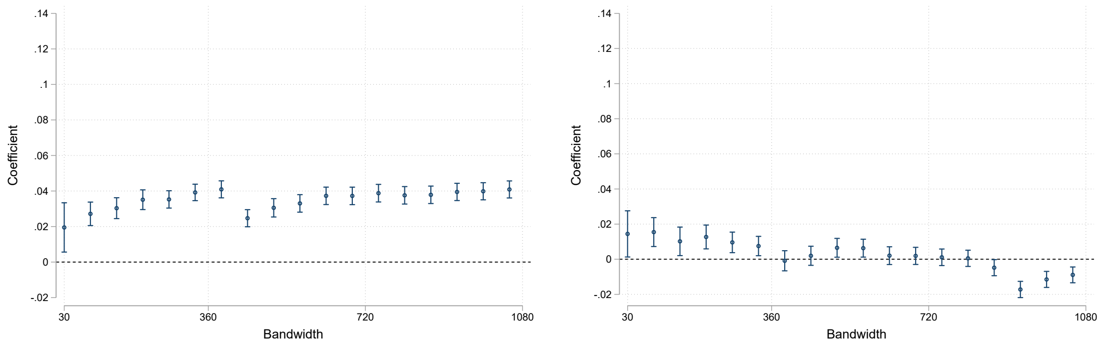
FIGURE A.5: Diff-in-disc coefficients: bandwidth sensitivity, by House



(A) Chamber of Deputies (B) Senate

Notes: The graph shows the point estimate and the 95% confidence interval of the Diff-in-Disc coefficients on the entire sample (separately for each House), estimating the regression specified in Equation (1.15) using different bandwidths. The horizontal axis shows the number of tenure days from the 4.5-year-cutoff in each side of the bandwidth.

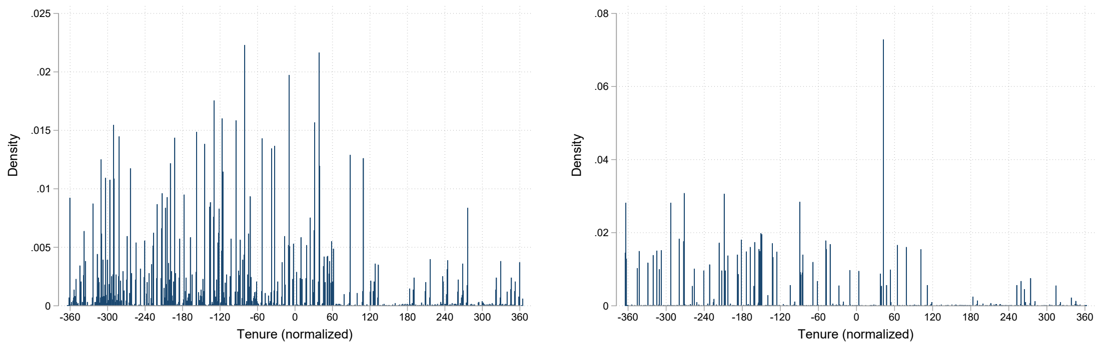
FIGURE A.6: Diff-in-disc coefficients: bandwidth sensitivity, by party's stance with respect to the current government.



(A) Elected in majority party (B) Elected in opposition party

Notes: The graph shows the point estimate and the 95% confidence interval of the Diff-in-Disc coefficients on the entire sample (separately for majority and opposition MPs), estimating the regression specified in Equation (1.15) using different bandwidths. The horizontal axis shows the number of tenure days from the 4.5-year-cutoff in each side of the bandwidth.

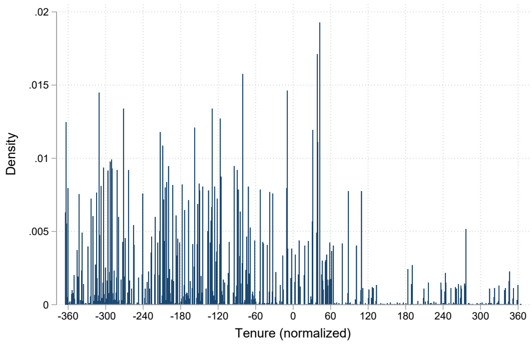
FIGURE A.7: Density of confidence votes, by House



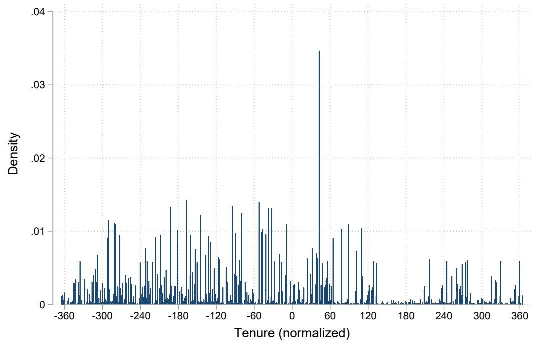
(A) Chamber of Deputies (B) Senate

Notes: This figure plots the density of confidence votes in daily bins over the number of days of tenure from the 4.5-year threshold, separately for each House.

FIGURE A.8: Density of confidence votes, by party's stnace with respect to the current government.



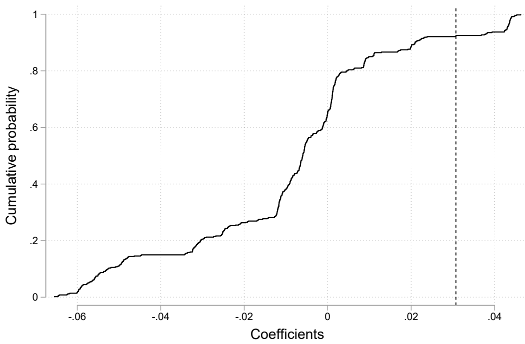
(A) Elected in majority party



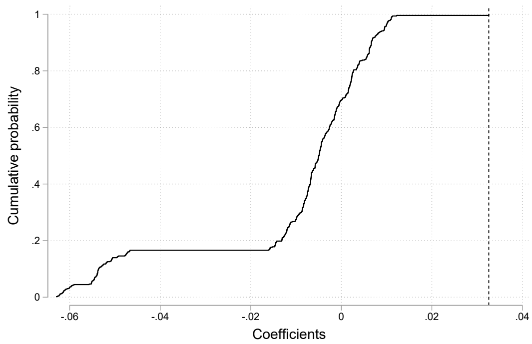
(B) Elected in opposition party

Notes: This figure plots the density of confidence votes in daily bins over the number of days of tenure from the 4.5-year threshold, separately for majority and opposition MPs.

FIGURE A.9: Placebo estimates, by House



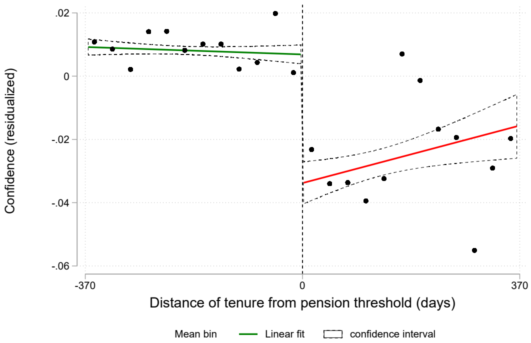
(A) Chamber of Deputies



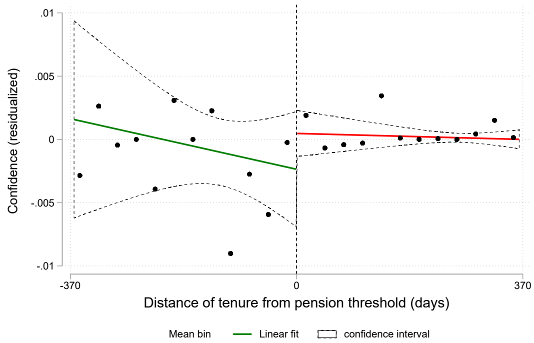
(B) Senate

Notes: This graph shows the cumulative distribution of Diff-in-Disc estimates on votes of confidence for each House separately, from placebo local linear regressions in which the cutoff is set in different parts of the tenure distribution. Estimates are computed using the regression in Equation (1.15) within a 1-year bandwidth. Cut-offs are located at every day from 3 years and 7 months to 4 years and 5 months and at every day from 4 years and 7 months to 5 years and 5 months, in order to stay sufficiently away from the policy thresholds of 3.5, 4.5 and 5.5 years at which the severance payment, the pension age and the pension payment change discontinuously. The vertical dashed line shows the coefficient estimated using the true 4.5-year tenure threshold.

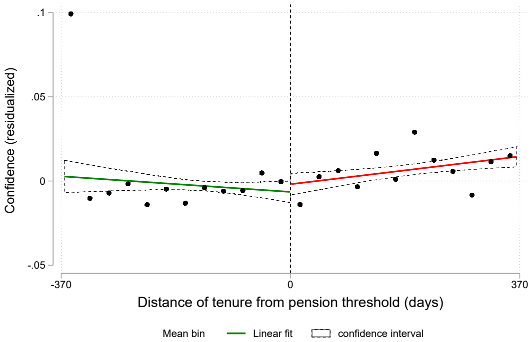
FIGURE A.10: Regression discontinuities, by party's stance with respect to the current government.



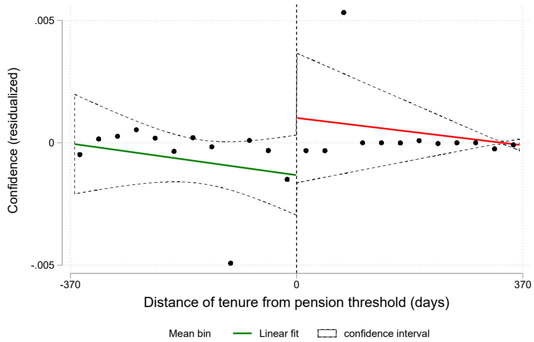
(A) Elected in majority party, post-treatment



(B) Elected in majority party, pre-treatment



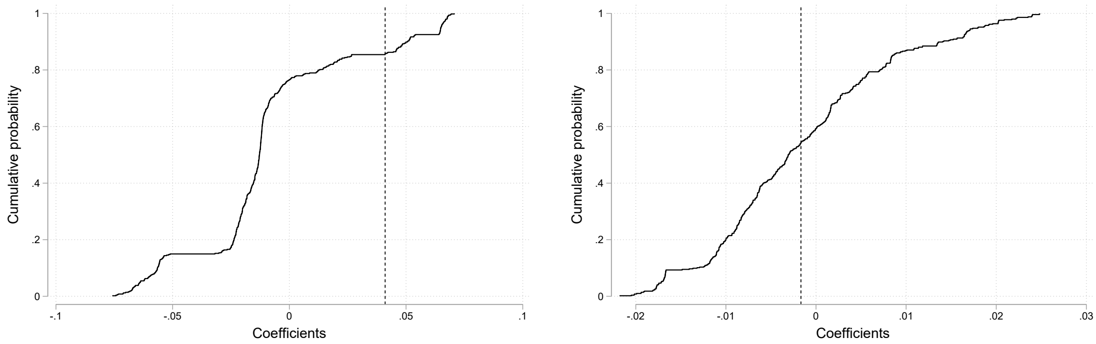
(C) Elected in opposition party, post-treatment



(D) Elected in opposition party, pre-treatment

Notes: These figures show the effect of the parliamentary tenure distance from the 4.5 year-cutoff on the MP's probability of voting confidence in the government. The circles are averages across monthly bins on either side of the threshold, while the solid and dashed lines represent the predicted values and confidence intervals of a local linear regression of the outcome on (days of tenure, normalized) and the fixed effects, separately for each side of the cutoff. The bandwidth includes observations within one year from the 4.5 year-cutoff.

FIGURE A.11: Placebo estimates, by party's stance with respect to the current government.

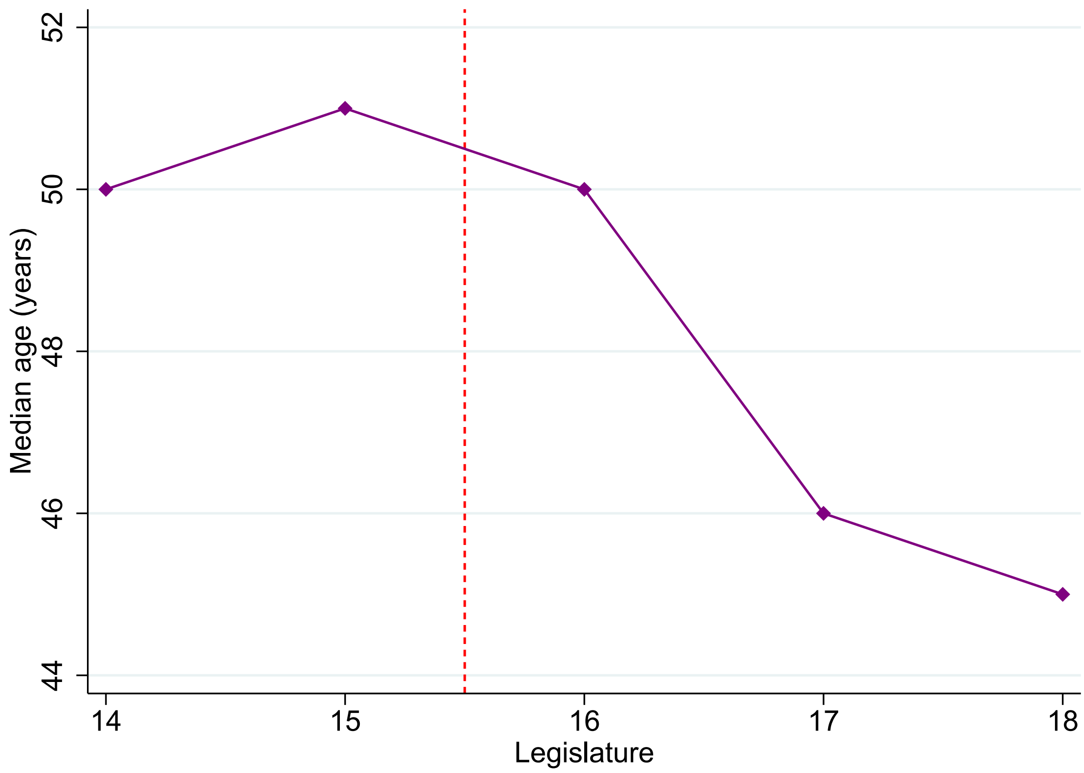


(A) Elected in majority party

(B) Elected in opposition party

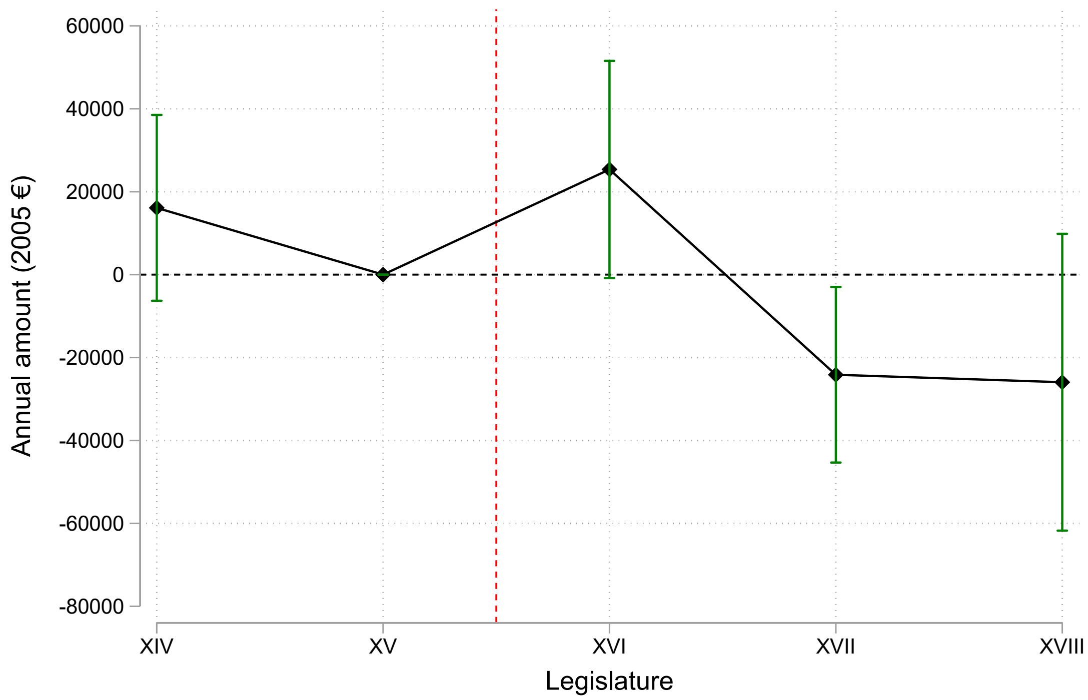
Notes: This graph shows the cumulative distribution of Diff-in-Disc estimates on votes of confidence for majority and opposition MPs separately, from placebo local linear regressions in which the cutoff is set in different parts of the tenure distribution. Estimates are computed using the regression in Equation (1.15) within a 1-year bandwidth. Cut-offs are located at every day from 3 years and 7 months to 4 years and 5 months and at every day from 4 years and 7 months to 5 years and 5 months, in order to stay sufficiently away from the policy thresholds of 3.5, 4.5 and 5.5 years at which the severance payment, the pension age and the pension payment change discontinuously. The vertical dashed line shows the coefficient estimated using the true 4.5-year tenure threshold.

FIGURE A.12: Median age, by legislature (both Houses)



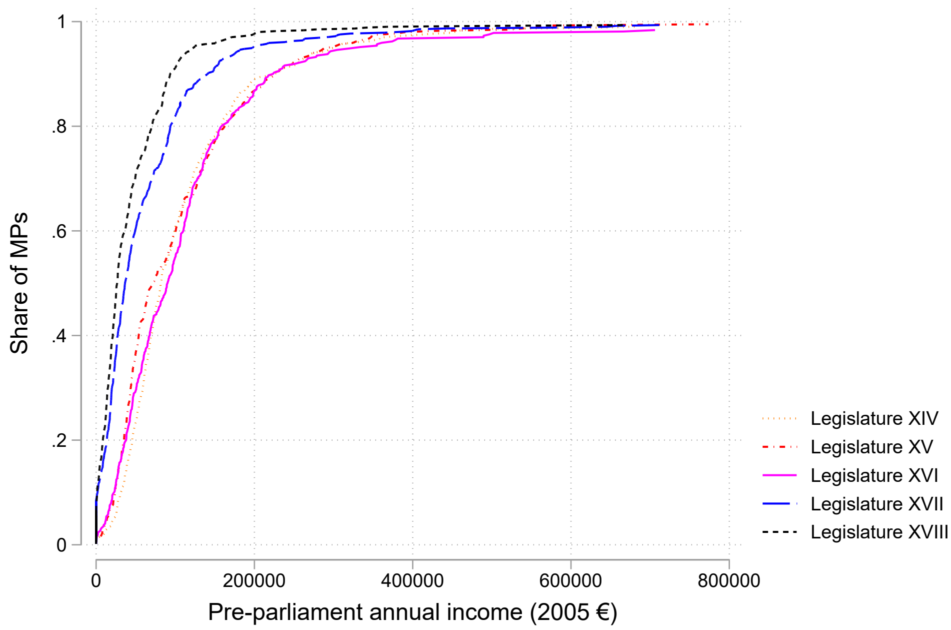
Notes: This figure shows the median age when entering parliament for newly-elected MPs in each legislature.

FIGURE A.13: Pre-parliament income variation across legislatures controlling for age



Notes: This figure shows the estimates for the coefficients λ_j and their 95% confidence interval in the regression $w_{ij} = \alpha_0 + \alpha_1 age_{ij} + \alpha_2 age_{ij}^2 + \sum_{j \neq XV} \lambda_j legislature_j + \nu_{ij}$.
w_{ij} is the pre-parliament wage of MP i elected for the first time in legislature j. age_{ij} is the age of MP i elected for the first time in legislature j when entering parliament. $legislature_j$ is a fixed effect for legislature j. ν_{ij} is the error term.

FIGURE A.14: Cdfs of annual income in the year before becoming MP, by legislature



Appendix B

Appendix: College Access When Preparedness Matters: New Evidence from Large Advantages in College Admissions

B.1 Fieldwork Information

All the sampled schools agreed to participate in our study, also thanks to the Ministry of Education, who encouraged school principals to participate. Our fieldworkers visited the schools several times and were able to survey all students who were present.

Students filled out paper questionnaires. Schools allowed us to administer our survey during class time. Our survey displaced one lecture. It took students approximately 50 minutes to fill out the questionnaire. At the start of the data collection, fieldworkers explained that they would take an achievement test for the first 20 minutes, and that they would be entered into a lottery to win an iPad, with the number of lottery tickets determined by the number of correct answers.[1] At the 20-minute mark, fieldworkers told students to stop working on the achievement test and to proceed to the survey part of the questionnaire. If a student completed the achievement test before the 20 minutes were up, she was allowed to proceed to the survey.

To limit the influence of the fieldworkers, the instructions were printed on the first page of the survey and the fieldworkers enunciated them. To further harmonize the data collection across fieldworkers, they had to submit checklists to their supervisors. During the first 20 minutes, the fieldworkers acted as invigilators. To further avoid cheating, we produced 6 versions of the achievement test. Versions differed in the question order. To

[1] The professional testing agencies Aptus Chile and Puntaje Nacional developed the test and we extensively piloted it.

ensure that all students faced questions of increasing difficulty, we assigned questions to three different difficulty categories (based on the difficulty index provided by the testing agencies and on extensive piloting on our target population), and we randomized the order of the questions within each category. Students were told, at the start of the test, that they would not all have identical tests.

The questionnaires did not show logos of any Ministry or public agency.

B.2 Additional Figures

FIGURE B.1: Timeline. Two-digit numbers refer to years (e.g. 13 means year 2013).

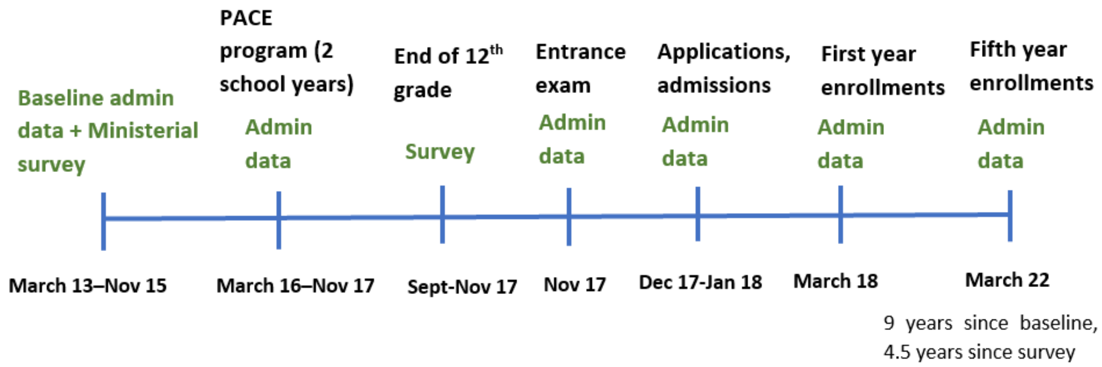


FIGURE B.2: Quality distribution of PACE and regular college seats.

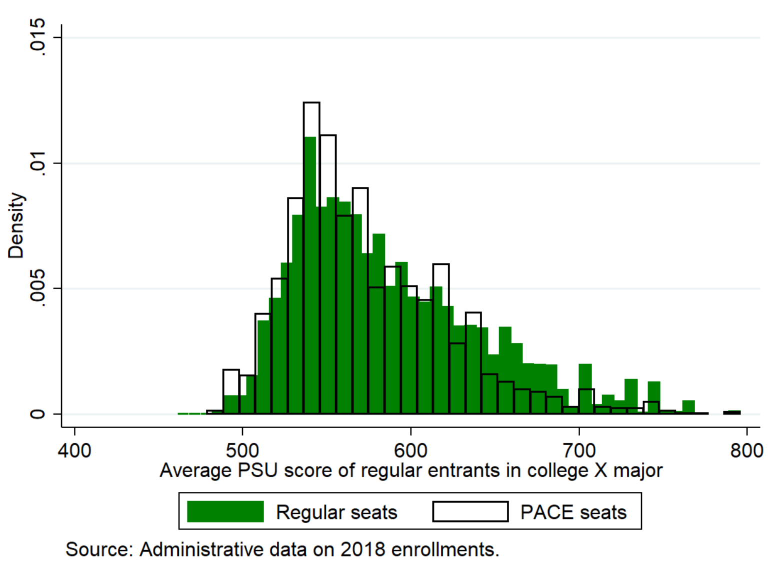


FIGURE B.3: Selected survey questions.

Belief over:	**Question:**	**Possible answers:**
Score on the PSU entry exam.	*Suppose that you will sit the PSU entry exam this year. What do you think your PSU score will be?*	• *700-850 (excellent)* • *600-700 (very good)* • *450-600 (good)* • *350-450 (modest)* • *250-350 (unsatisfactory)* • *150-250 (very unsatisfactory)* • *I don't know*
Own GPA.	*Thinking of yourself, what do you think your grade point average (GPA) will be at the end of high-school? (Introduce a number between 1.0 and 7.0)*	*Free format*
Percentiles of the GPA distribution in the school.	*Suppose that, in your school, there are 40 students in 12th grade. Think of the student with the **highest** grade point average (GPA) among the 40 students. (GPA is a number between 1.0 and 7.0).* *What do you think is the GPA that he/she has?* *Now think of the student with the 6th highest grade point average (GPA) among the 40 students. His/her GPA is in the **top 15%**.* *What do you think is the GPA that he/she has?* [This set of questions further elicits beliefs about the 12th student (top 30%) and the 30th student (bottom 25%)]	*Free format*

FIGURE B.4: Decision to take and prepare for PSU entrance exam and objective admission likelihood.

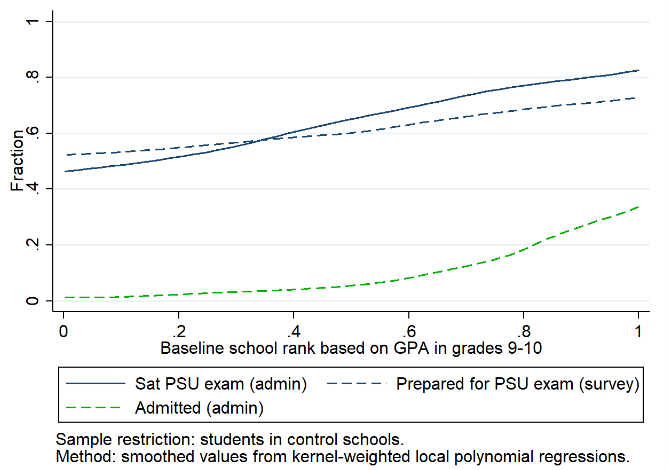
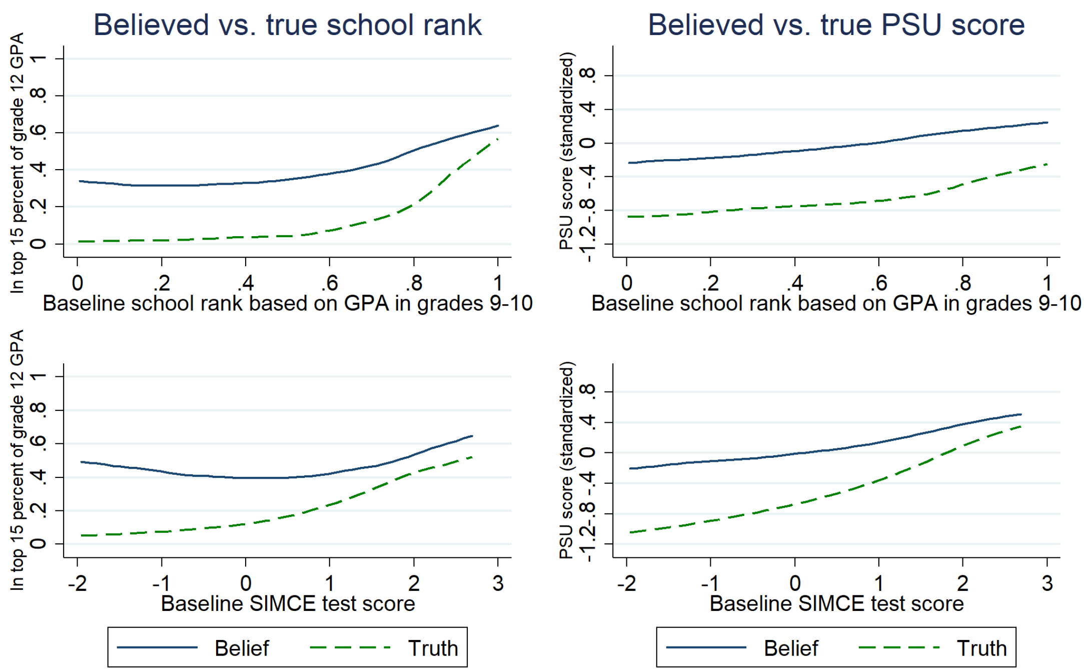


FIGURE B.5: Heterogeneity of subjective beliefs by baseline within-school rank and by baseline test scores.



Sample restriction: students in control schools.
Bottom panels trim students in top and bottom 1% of SIMCE distribution.
Method: smoothed values from kernel-weighted local polynomial regressions.

FIGURE B.6: Suggestive evidence of a response to perceived incentives.

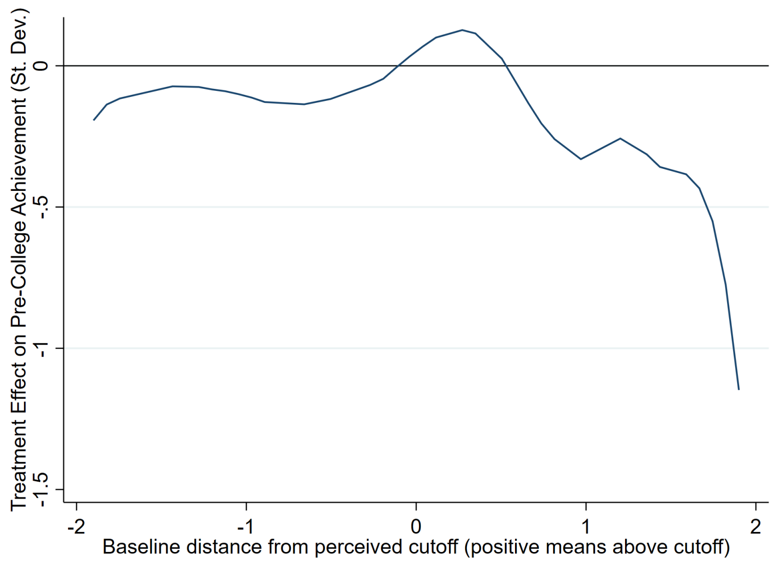


FIGURE B.7: Percentage of 18-19 year-old who are enrolled in college in Chile by family income quintile.

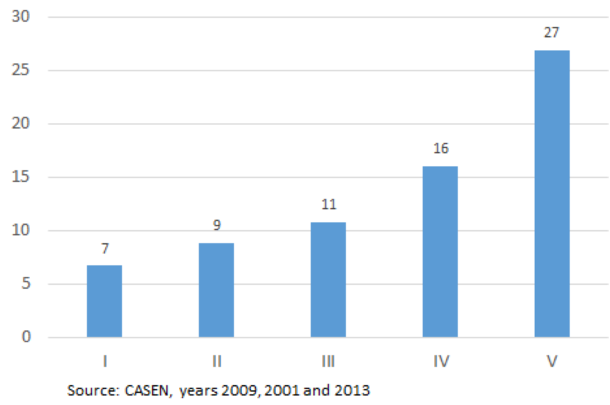


FIGURE B.8: Evidence of grade compression: Histogram of 12^{th} grade GPA.

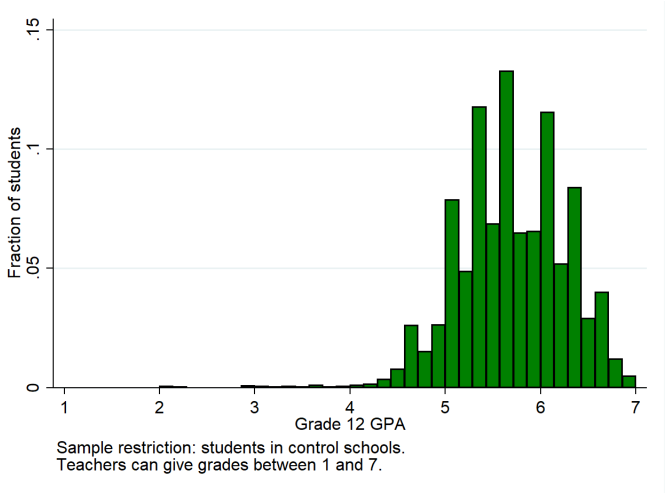
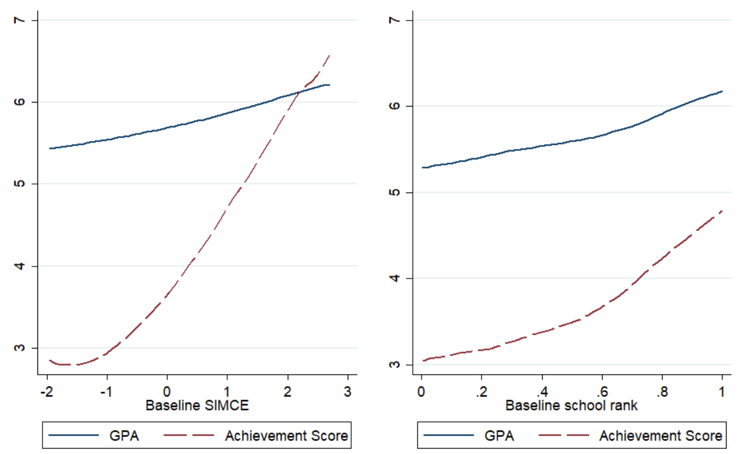


FIGURE B.9: Evidence of grade compression: GPA does not discriminate between students as well as the achievement score does.



Sample restriction: students in control schools.
Left panel trims students in top and bottom 1% of SIMCE distribution
Method: smoothed values from kernel-weighted local polynomial regressions.

B.3 Robustness analysis

Survey attrition. The response rate in our survey data is 69.4% percent in the control group, and it is not statistically significantly different in the treatment group, suggesting the absence of selective attrition. Table B.1 presents Lee, 2009 bounds for the treatment effects, confirming that the estimated treatment effects are not due to selective attrition.

TABLE B.1: LEE BOUNDS FOR AVERAGE TREATMENT EFFECTS

Treatment effect on	Lower bound	Upper bound
	(1)	(1)
Standardized achievement score (res)	-0.209	-0.024
Standardized study effort (res)	-0.285	-0.012
Standardized achievement score	-0.163	-0.013
Standardized study effort	-0.268	0.005

Notes: This table presents Lee (2009) bounds on the average treatment effect of being in a PACE school on pre-college achievement and effort. In the first and second rows we use residuals from a regression of the outcomes on baseline test scores as the dependent variable. In the third and fourth rows we use the raw outcome variables. In all rows we scale the outcomes as in Table 2.4, to keep our analysis of bounds analogous to the main average treatment effects.

B.4 Additional Tables

TABLE B.2: BASELINE CHARACTERISTICS OF ALL STUDENTS AND OF THOSE TARGETED BY THE PACE POLICY

	All students (1)	Targeted students (2)
Very low SES	0.40	0.61
Mother's education (years)	11.49	9.60
Father's education (years)	11.43	9.38
Family income (1,000 CLP)	600.10	291.66
SIMCE score (standardized)	0.00	-0.60
Rural resident	0.03	0.03
Santiago resident	0.30	0.17

Notes: Source: SIMCE and SEP administrative data on 10^{th} graders in 2015. Very low SES indicates a student that the government classified as socioeconomically vulnerable (*Alumno Pioritario*). SIMCE is a standardized achievement test taken in 10^{th} grade. Sample restriction in column (2): students in the 128 experimental schools. All characteristics were collected before the start of the intervention.

TABLE B.3: PRE-COLLEGE ACADEMIC PREPAREDNESS PREDICTS PERSISTENCE IN COLLEGE

	College persistence or graduation five years after high school graduation			
	(1)	(2)	(3)	(4)
GPA in 12^{th} grade (std)	0.104^{***}			
	(0.020)			
GPA in 12^{th} grade, subjects tested on PSU (std)		0.100^{***}		
		(0.020)		
GPA in 12^{th} grade, subjects not tested on PSU (std)		0.007		
		(0.026)		
PSU score (std)	0.031	0.071		
	(0.042)	(0.048)		
Study effort in last high school year (std)			0.062^{***}	
			(0.022)	
Hours of study per week in last high school year				0.015^{**}
				(0.006)
Baseline test score in 10^{th} grade (std)	0.023	-0.013	0.060^{**}	0.058^{**}
	(0.028)	(0.030)	(0.022)	(0.023)
Observations	1,015	741	737	750
R^2	0.061	0.064	0.048	0.048

Notes: Sample of students who enrolled in a selective college in the first year. The outcome variable is a dummy equal to one if five years later they are either still continuously enrolled or they have graduated, and zero otherwise. Results from OLS regressions. Inverse Probability Weights are used in columns (3) and (4). Standard set of control variables used: age, gender, very-low-SES, never failed a year, type of high school track (academic or vocational). The baseline test score is std in the population of students taking the SIMCE exam, the PSU is std in the population of exam takers. Standard errors in parentheses, clustered at school level. $*\,p < 0.10$, $**\,p < 0.05$, $***\,p < 0.01$.

TABLE B.4: EFFECTS OF PACE ON COLLEGE APPLICATIONS AND ADMISSIONS

	(1)	(2)	(3)	(4)	(5)	(6)
	All Sample		Bottom 85%		Top 15%	
	Applications	Admissions	Applications	Admissions	Applications	Admissions
Treatment	0.019	0.041***	0.000	0.011	0.147***	0.225***
	(0.019)	(0.012)	(0.018)	(0.009)	(0.037)	(0.030)
Control group mean	0.210	0.114	0.161	0.070	0.450	0.328
Observations	8,944	8,944	7,061	7,061	1,563	1,563
R^2	0.176	0.206	0.109	0.122	0.209	0.257

Notes: Columns (1) and (2) use the sample of all students in the experiment. Columns (3) and (4) use the sample of all students who at the end of 10^{th} grade, before the experiment started, were in the bottom 85% of their school according to GPA in the first two high school years. Columns (5) and (6) use the sample of all students who at the end of 10^{th} grade, before the experiment started, were in the top 15% of their school according to GPA in the first two high school years. The share of students in the top 15% at baseline is 18% because there are students with the same GPA average at baseline. "Control group mean" is the mean of the dependent variable in the control group (i.e., absent PACE). Results from OLS regressions. Treatment is a dummy equal to 1 if a school was randomly assigned to be in the PACE treatment, to 0 otherwise. All regressions use the standard set of controls (see notes under Figure 2.2). Standard errors clustered at the school level in parenthesis. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

TABLE B.8: AVERAGE TREATMENT EFFECT ON PRE-COLLEGE STUDY EFFORT - ITEMS

Panel A: At home	Study hours	Study days test	Assignm on time
Treatment	-0.081**	0.003	-0.086***
	(0.040)	(0.043)	(0.033)
R-W adjusted p	0.089	0.947	0.027

Panel B: In class	Take notes	Participate	Pay attention	Ask questions
Treatment	-0.089**	-0.008	-0.061	-0.018
	(0.039)	(0.013)	(0.037)	(0.042)
R-W adjusted p	0.083	0.864	0.269	0.864

Panel C: PSU entrance exam preparation	Prepare for PSU
Treatment	-0.042**
	(0.017)

NOTE.– Panels A and B report OLS estimates, panel C reports the average marginal effect from a probit model. Standard errors are clustered at the school level (for panel C, the delta method is used). We use the standard set of controls (see Figure 2.2), field-worker fixed effects and Inverse Probability Weights. *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program. The family of survey instruments in Panel A asked students the number of hours of study per week outside of class time, how many days before a test they start preparing, and how often they hand in homework on time. The family of survey instruments in Panel B asked students how often, when in class, they take notes, actively participate, pay attention, and ask questions. We report Romano-Wolf adjusted p-values calculated within family (as per the pre-analysis plan). The dependent variable in Panel C is a dummy indicating whether the student does at least one of the following PSU exam preparation activities: attending a PSU preparation course (*Preuniversitario*) for a fee, attending a free *Preuniversitario*, using an online *Preuniversitario* for a fee, using an online free *Preuniversitario*, preparing on his/her own. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

TABLE B.5: EFFECTS OF PACE ON CONTINUOUS ENROLLMENT OR GRADUATION OVER TIME, ALL SAMPLE

	Year 1	Year 2	Year 3	Year 4	Year 5
A. Continuous enrollment in or graduation from college					
Treatment	0.031***	0.023**	0.020**	0.017**	0.011
	(0.011)	(0.010)	(0.008)	(0.008)	(0.008)
Observations	8944	8944	8944	8944	8944
R^2	0.172	0.133	0.124	0.116	0.110
B. Continuous enrollment in or graduation from vocational HE institute					
Treatment	-0.024	-0.020	-0.016	-0.013	-0.017**
	(0.018)	(0.015)	(0.012)	(0.010)	(0.008)
Observations	8944	8944	8944	8944	8944
R^2	0.006	0.004	0.003	0.004	0.008
C. Continuous enrollment in or graduation from non-selective college					
Treatment	-0.014	-0.011	-0.008	-0.010	-0.010*
	(0.012)	(0.009)	(0.007)	(0.006)	(0.006)
Observations	8944	8944	8944	8944	8944
R^2	0.005	0.007	0.008	0.009	0.010
D. Continuous enrollment in or graduation from HE outside options					
Treatment	-0.038*	-0.031*	-0.024*	-0.022**	-0.027***
	(0.021)	(0.016)	(0.012)	(0.011)	(0.009)
Observations	8944	8944	8944	8944	8944
R^2	0.005	0.006	0.006	0.009	0.013
E. Continuous enrollment in or graduation from any HE institute					
Treatment	-0.007	-0.008	-0.004	-0.005	-0.016
	(0.022)	(0.017)	(0.013)	(0.012)	(0.011)
Observations	8944	8944	8944	8944	8944
R^2	0.060	0.061	0.066	0.071	0.074

Notes: Sample of all students in the experiment. Results from OLS regressions. Treatment is a dummy equal to 1 if a school was randomly assigned to be in the Treatment treatment, to 0 otherwise. All regressions use the standard set of controls (see notes under Figure 2.2). Standard errors clustered at the school level in parenthesis. HE stands for higher education. The HE outside options are vocational HE institutes and non-selective colleges. The construction of the outcome variable is explained in the notes under Figure 2.2. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

TABLE B.6: EFFECTS OF PACE ON CONTINUOUS ENROLLMENT OR GRADUATION OVER TIME, SAMPLE OF THOSE IN THE TOP 15% OF THEIR SCHOOL AT BASELINE

	Year 1	Year 2	Year 3	Year 4	Year 5
A. CONTINUOUS ENROLLMENT IN OR GRADUATION FROM COLLEGE					
Treatment	0.168***	0.132***	0.109***	0.094***	0.075***
	(0.029)	(0.026)	(0.025)	(0.025)	(0.023)
Observations	1563	1563	1563	1563	1563
R^2	0.210	0.169	0.161	0.154	0.155
B. CONTINUOUS ENROLLMENT IN OR GRADUATION FROM VOCATIONAL HE INSTITUTE					
Treatment	-0.042	-0.041	-0.024	-0.034	-0.032*
	(0.030)	(0.026)	(0.025)	(0.021)	(0.017)
Observations	1563	1563	1563	1563	1563
R^2	0.046	0.032	0.023	0.020	0.019
C. CONTINUOUS ENROLLMENT IN OR GRADUATION FROM NON-SELECTIVE COLLEGE					
Treatment	-0.059***	-0.048***	-0.039***	-0.036***	-0.035***
	(0.019)	(0.015)	(0.014)	(0.013)	(0.012)
Observations	1563	1563	1563	1563	1563
R^2	0.014	0.013	0.010	0.010	0.012
D. CONTINUOUS ENROLLMENT IN OR GRADUATION FROM HE OUTSIDE OPTIONS					
Treatment	-0.102***	-0.089***	-0.063**	-0.071***	-0.067***
	(0.032)	(0.027)	(0.026)	(0.022)	(0.018)
Observations	1563	1563	1563	1563	1563
R^2	0.046	0.031	0.018	0.019	0.021
E. CONTINUOUS ENROLLMENT IN OR GRADUATION FROM ANY HE INSTITUTE					
Treatment	0.067**	0.043	0.046*	0.023	0.008
	(0.030)	(0.027)	(0.027)	(0.026)	(0.026)
Observations	1563	1563	1563	1563	1563
R^2	0.070	0.054	0.064	0.077	0.088

Notes: Sample of all students who at the end of 10^{th} grade, before the experiment started, were in the top 15% of their school according to GPA in the first two high school years. Results from OLS regressions. Treatment is a dummy equal to 1 if a school was randomly assigned to be in the PACE treatment, to 0 otherwise. All regressions use the standard set of controls (see notes under Figure 2.2). Standard errors clustered at the school level in parenthesis. HE stands for higher education. The HE outside options are vocational HE institutes and non-selective colleges. The construction of the outcome variable is explained in the notes under Figure 2.2. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

TABLE B.7: EFFECTS OF PACE ON CONTINUOUS ENROLLMENT OR GRADUATION OVER TIME, SAMPLE OF THOSE IN THE BOTTOM 85% OF THEIR SCHOOL AT BASELINE

	Year 1	Year 2	Year 3	Year 4	Year 5
A. Continuous enrollment in or graduation from college					
Treatment	0.010	0.005	0.008	0.005	0.002
	(0.015)	(0.011)	(0.009)	(0.008)	(0.008)
	(0.009)	(0.007)	(0.006)	(0.006)	(0.006)
Observations	7061	7061	7061	7061	7061
R^2	0.097	0.070	0.062	0.054	0.049
B. Continuous enrollment in or graduation from vocational HE institute					
Treatment	-0.019	-0.017	-0.015	-0.008	-0.014
	(0.020)	(0.016)	(0.013)	(0.011)	(0.009)
Observations	7061	7061	7061	7061	7061
R^2	0.003	0.005	0.005	0.005	0.009
C. Continuous enrollment in or graduation from non-selective college					
Treatment	-0.004	-0.003	-0.002	-0.004	-0.005
	(0.012)	(0.008)	(0.007)	(0.006)	(0.005)
Observations	7061	7061	7061	7061	7061
R^2	0.004	0.007	0.007	0.009	0.009
D. Continuous enrollment in or graduation from HE outside options					
Treatment	-0.023	-0.020	-0.017	-0.012	-0.019*
	(0.022)	(0.017)	(0.013)	(0.011)	(0.010)
Observations	7061	7061	7061	7061	7061
R^2	0.004	0.008	0.009	0.010	0.015
E. Continuous enrollment in or graduation from any HE institute					
Treatment	-0.014	-0.014	-0.010	-0.006	-0.016
	(0.022)	(0.017)	(0.013)	(0.012)	(0.011)
Observations	7061	7061	7061	7061	7061
R^2	0.030	0.035	0.036	0.036	0.038

Notes: Sample of all students who at the end of 10^{th} grade, before the experiment started, were in the bottom 85% of their school according to GPA in the first two high school years. Results from OLS regressions. Treatment is a dummy equal to 1 if a school was randomly assigned to be in the PACE treatment, to 0 otherwise. All regressions use the standard set of controls (see notes under Figure 2.2). Standard errors clustered at the school level in parenthesis. HE stands for higher education. The HE outside options are vocational HE institutes and non-selective colleges. The construction of the outcome variable is explained in the notes under Figure 2.2. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

TABLE B.9: SOCIOECONOMIC CORRELATES OF BELIEF BIASES

	Rank belief bias	PSU belief bias
	(1)	(2)
Very low SES	0.014	-0.033
	(0.022)	(0.022)
Household log-income	-0.024	0.007
	(0.023)	(0.017)
Mother education (years)	0.003	0.018^***
	(0.005)	(0.005)
Father education (years)	-0.009^**	0.016^***
	(0.004)	(0.004)
Observations	4,570	3,769

Notes: Estimates stem from ordinary least square regressions. Very low SES is a dummy variable identifying students the government classified as particularly vulnerable based on socioeconomic status. Rank belief bias is the difference between actual and expected 85^{th} GPA percentile in the school, it is measured in GPA points (GPA ranges from 1 to 7). Positive values indicate overoptimism. PSU belief bias is the difference between expected and actual PSU entrance exam score, it is measured in standard deviations. Positive values indicate overoptimism. Standard errors in parenthesis clustered at the school level. Inverse Probability Weights used. $* \, p < 0.10$, $** \, p < 0.05$, $*** \, p < 0.01$.

TABLE B.10: VALIDATING ACHIEVEMENT AND EFFORT MEASURES

	Sit PSU	Apply	Admitted	Enroll year 1	Enroll year 2	Enroll year 3	Enroll year 4	Enroll year 5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				A. ACHIEVEMENT				
Achievement	0.060***	0.074***	0.057***	0.047***	0.039***	0.035***	0.032***	0.030***
	(0.009)	(0.008)	(0.008)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)
PSU score	No	No	No	No	No	No	No	No
Dep. var. mean	0.725	0.241	0.131	0.099	0.078	0.069	0.065	0.060
Observations	2,922	2,922	2,922	2,922	2,922	2,922	2,922	2,922
Pseudo-R^2	0.100	0.169	0.283	0.290	0.265	0.261	0.252	0.246
				B. ACHIEVEMENT, CONTROLLING FOR PSU SCORE				
Achievement		0.037**	0.017**	0.015**	0.014**	0.013*	0.010	0.010
		(0.012)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
PSU score		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. var. mean		0.333	0.183	0.136	0.107	0.095	0.089	0.083
Observations		2,122	2,122	2,122	2,122	2,122	2,122	2,122
Pseudo-R^2		0.238	0.556	0.504	0.425	0.401	0.394	0.380
				C. STUDY EFFORT				
Study Effort	0.056***	0.069***	0.045***	0.037***	0.031***	0.030***	0.030***	0.029***
	(0.010)	(0.009)	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)
PSU score	No	No	No	No	No	No	No	No
Dep. var. mean	0.731	0.244	0.136	0.101	0.080	0.071	0.066	0.062
Observations	2,746	2,746	2,746	2,746	2,746	2,746	2,746	2,746
Pseudo-R^2	0.096	0.163	0.255	0.262	0.238	0.240	0.237	0.233
				D. STUDY EFFORT, CONTROLLING FOR PSU SCORE				
Study Effort		0.055***	0.018**	0.017**	0.016**	0.018**	0.019**	0.020***
		(0.010)	(0.007)	(0.007)	(0.007)	(0.009)	(0.008)	(0.008)
PSU score		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. var. mean		0.334	0.186	0.138	0.109	0.097	0.091	0.085
Observations		2,010	2,010	2,010	2,010	2,010	2,010	2,010
Pseudo-R^2		0.241	0.550	0.501	0.425	0.401	0.397	0.384

NOTE.–: The outcome variables, listed at the top of the Table, are the same across Panels. The Panels differ in the measure (of achievement or of effort) used as an explanatory variable, high-lighted in the title of each Panel, and in some of the controls, high-lighted in the left-most column. All regressions use the standard set of controls (see notes under Figure 2.2) and Inverse Probability Weights. Sample restriction: students in control schools. Average marginal effects from probit models reported. Delta-method standard errors clustered at school level in parenthesis. The study effort score is the standardized score predicted from the principal component analysis of the eight survey instruments reported in Appendix Table B.8. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

TABLE B.11: VALIDATING BELIEF MEASURES

	Sit PSU (1)	Apply (2)	Enroll year 1 (3)	Enroll year 2 (4)	Enroll year 3 (5)	Enroll year 4 (6)	Enroll year 5 (7)
	A. BELIEVED PSU SCORE						
Believed PSU score	0.048***	0.086***	0.066***	0.049***	0.042***	0.042***	0.041***
	(0.010)	(0.010)	(0.008)	(0.007)	(0.007)	(0.007)	(0.006)
PSU score	No	No	No	No	No	No	No
Dep. var. mean	0.768	0.272	0.113	0.089	0.079	0.073	0.069
Observations	2,401	2,401	2,401	2,401	2,401	2,401	2,401
Pseudo-R^2	0.089	0.161	0.287	0.252	0.249	0.246	0.245
	B. BELIEVED PSU SCORE, CONTROLLING FOR PSU SCORE						
Believed PSU score		0.065***	0.036***	0.025***	0.020***	0.023***	0.025***
		(0.012)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)
PSU score		Yes	Yes	Yes	Yes	Yes	Yes
Dep. var. mean		0.354	0.147	0.116	0.102	0.095	0.089
Observations		1,848	1,848	1,848	1,848	1,848	1,848
Pseudo-R^2		0.239	0.505	0.421	0.396	0.393	0.381
	C. Δ BELIEVED (GPA-CUTOFF), CONTROL GROUP						
Δ Believed (GPA-cutoff)	0.000	−0.003	−0.004	0.002	0.002	0.002	0.004
	(0.011)	(0.011)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)
Believed PSU score	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. var. mean	0.781	0.286	0.121	0.095	0.084	0.079	0.074
Observations	2,170	2,170	2,170	2,170	2,170	2,170	2,170
Pseudo-R^2	0.085	0.159	0.283	0.250	0.247	0.246	0.246
	D. Δ BELIEVED (GPA-CUTOFF), TREATMENT GROUP						
Δ Believed (GPA-cutoff)	0.006	0.040***	0.059***	0.052***	0.047***	0.045***	0.038***
	(0.010)	(0.011)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)
Believed PSU score	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. var. mean	0.746	0.300	0.189	0.149	0.134	0.122	0.107
Observations	2,240	2,240	2,240	2,240	2,240	2,240	2,240
Pseudo-R^2	0.149	0.200	0.237	0.227	0.240	0.236	0.241

NOTE.–: The outcome variables, listed at the top of the Table, are the same across Panels. The Panels differ in the subjective belief used as an explanatory variable, high-lighted in the title of each Panel, and in some of the controls, high-lighted in the left-most column. All regressions use the standard set of controls (see notes under Figure 2.2) and Inverse Probability Weights. Sample restriction: students in control schools in Panels A-C, students in treatment schools in Panel D. Average marginal effects from probit models. Delta-method standard errors clustered at school level. The believed PSU score is standardized using the distribution of PSU scores among all exam-takers in the country. Δ Believed (GPA-cutoff) is the difference between the perceived own GPA and the perceived top 15% cutoff. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

TABLE B.12: TREATMENT EFFECTS ON TEACHERS' EFFORT AND FOCUS OF INSTRUCTION

	(1)	(2)	(3)	(4)	(5)	(6)
	Effort (Prep Hours)		Effort (Absences)		Focus of Instruction	
	Mathematics	Language	Mathematics	Language	Mathematics	Language
Treatment	0.024	0.247	0.308	0.152	0.033	0.021
	(1.253)	(0.449)	(1.371)	(1.000)	(0.033)	(0.028)
Observations	271	316	271	316	271	316

NOTE.– Results from OLS regressions. The unit of observations are classrooms (there are one Mathematics and one Language teacher per classroom). The construction of the focus of instruction variable is described in section B.5.1 below. It ranges from 0 to 1 and higher values indicate targeting higher-ability students. Absences from work are measured in days per year. Standard errors in parentheses. Treatment is a dummy equal to 1 if a school is randomly allocated to have PACE, and equal to 0 otherwise. $* < 0.10$; $** < 0.05$; $*** < 0.01$.

TABLE B.13: SURVEY ANSWERS TO HYPOTHETICAL EFFORT QUESTIONS

Survey question	Observations	Mean	Standard Deviation
Hours of study per week in the semester to obtain:			
at least 600 on the PSU	5,469	10.106	4.748
at least 450 on the PSU	5,442	7.668	4.390
at least 350 on the PSU	5,344	5.506	4.536
a GPA in the top 15% of the school	5,443	8.105	4.330
a GPA of at least 5.5	5,451	7.077	4.335

Notes: This table describes the answers to the survey questions used to build the perceived returns to effort in the production of a PSU score and of GPA. For the second-last row, the survey asked the student to think of how many hours they believe they needed to study to obtain a GPA above the cutoff that they perceived as the 85^{th} percentile according to a previous survey answer. In constructing perceived returns, we eliminated answers that delivered infinite or negative returns.

TABLE B.14: PARAMETERS ESTIMATED OUTSIDE OF THE MODEL

Symbol	Description	Estimate	Standard Error
γ_0	Constant, regular adm. prob.	-0.306***	0.061
γ_1	Coefficient of PSU, regular adm. prob.	2.481***	0.199
λ_0^R	Constant, regular selectivity	467.603***	1.334
λ_1^R	Coefficient of PSU, regular selectivity	43.861***	3.491
λ_0^P	Constant, PACE selectivity	295.740***	60.013
λ_1^P	Coefficient of GPA, PACE selectivity	32.295***	9.708

Notes: First two estimates from Probit regression, remaining estimates from OLS regressions. Standard errors clustered at school level. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

Table B.15: Parameter Estimates

Symbol	Description	Estimate	Standard Error
	A. Preferences		
ξ_1	Linear term, effort cost	-0.141^{***}	0.0045
ξ_2	Quadratic term, effort cost	-0.029^{***}	0.0054
ξ_3	Coefficient on treatment in effort cost for those w/ no intention to enroll	-0.020^{**}	0.0081
$\tilde{\alpha}$	Time preference	1.384^{***}	0.0079
c^S	Cost of taking PSU exam	0.467^{***}	0.0021
λ_{01}	Constant in utility from college enrollment, type 1	0.802^{***}	0.0065
λ_{02}	Constant in utility from college enrollment, type 2	0.607^{***}	0.0066
λ_1	Very-low-SES in utility from college enrollment	-0.500^{***}	0.0027
λ_2	Above median ability in utility from college enrollment	0.052^{***}	0.0041
λ_3	Program selectivity in utility from college enrollment	0.001	0.0007
δ^E	Stigma: disutility from PACE enrollment	0.999^{***}	0.0074
δ^A	Stigma: disutility from PACE admission	0.498^{***}	0.0067
	B. Technology		
α_{01}	Constant in achievement, type 1	-0.001	0.0089
α_{02}	Constant in achievement, type 2	-1.132^{***}	0.0045
α_{11}	Age in achievement	0.132^{***}	0.0026
α_{12}	Female in achievement	-0.238^{***}	0.0035
α_{13}	Very-low-SES in achievement	-0.093^{***}	0.0050
α_{14}	Never failed a year in achievement	-0.169^{***}	0.0068
α_{15}	Academic track in achievement	0.116^{***}	0.0038
α_2	Effort in achievement	0.281^{***}	0.0074
α_3	Lagged test score in achievement	0.619^{***}	0.0070
β_0^G	Constant in GPA	2.125^{***}	0.0020
β_1^G	Effort in GPA	0.037^{***}	0.0014
β_2^G	Lagged GPA in GPA	0.619^{***}	0.0052
β_0^P	Constant in PSU entrance exam score	-1.399^{***}	0.0038
β_1^P	Effort in PSU entrance exam score	0.161^{***}	0.0070
β_2^P	Lagged test score in PSU entrance exam score	0.602^{***}	0.0057
	C. Subjective Beliefs		
β_{01}^{Pb}	Constant in believed PSU entrance exam score, type 1	-1.393^{***}	0.0076
β_{02}^{Pb}	Constant in believed PSU entrance exam score, type 2	-1.696^{***}	0.0025
β_{11}^{Pb}	Effort in believed PSU entrance exam score, type 1	0.331^{***}	0.0047
β_{12}^{Pb}	Effort in believed PSU entrance exam score, type 2	0.262^{***}	0.0049
β_2^{Pb}	Lagged test score in believed PSU entrance exam score	0.952^{***}	0.0052
β_0^{Gb}	Constant in believed GPA	-2.201^{***}	0.0038
β_{11}^{Gb}	Effort in believed GPA, type 1	0.353^{***}	0.0026
β_{12}^{Gb}	Effort in believed GPA, type 2	0.148^{***}	0.0069
β_2^{Gb}	Lagged GPA in believed GPA	1.208^{***}	0.0047
γ_0^b	Constant in subj. prob. regular admission	0.408^{***}	0.0071
γ_1^b	Believed entrance exam score in subj. prob. regular admission	0.910^{***}	0.0054
ξ_0^b	Constant in subj. prob. PACE admission	1.064^{***}	0.0051
ξ_1^b	Perceived distance from cutoff in subj. prob. PACE admission	0.182^{***}	0.0054
	D. Unobserved Heterogeneity and Shocks		
ω_0	Constant in prob. type 1	-1.501^{***}	0.0011
ω_1	Missing survey in prob. type 1	-1.498^{***}	0.0004
ω_2	Lagged GPA in prob type 1	0.498^{***}	0.0039
$\sigma^{m.e.y.}$	St. dev. of measurement error on achievement	0.775^{***}	0.0034
$\sigma^{m.e.e.}$	St. dev. of measurement error on hours of study	2.720	0.0023
σ_G	St. dev. GPA shock	0.553^{***}	0.0030
σ_P	St. dev. PSU entrance exam shock	0.401^{***}	0.0060
ρ	Correlation coefficient of GPA and PSU shocks	0.873^{***}	0.0025

NOTE. – Standard Errors bootstrapped using 50 bootstrap samples. Lagged test score standardized in the estimation sample. $^*p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

TABLE B.16: MODEL FIT

	Sample	Data	Simulations	Targeted?
A. DESCRIPTIVE STATISTICS				
Took college entrance exam	Control	0.655	0.491	Yes
College entrance exam score $\mid$ took exam	Control	-0.601	-0.768	Yes
Admitted to college	Control	0.114	0.063	No
Enrolled in college	Control	0.085	0.048	No
B. TREATMENT EFFECTS				
Achievement Test Score	All	-0.121	-0.072	Yes
Study hours	All	-0.258	-0.264	Yes
Admissions	All	0.041	0.042	No
Enrollments	All	0.031	0.021	Yes
Entrance-exam taking	All	-0.041	-0.013	Yes
C. BELIEFS				
Believed minus actual entrance exam score $\mid$ took exam	Control	0.591	0.609	No
Believed minus actual 12^{th} grade GPA	Control	-0.075	-0.060	No
Believes is in top 15% of school	Control	0.431	0.376	No
Perceived returns to effort, GPA	All	0.177	0.123	Yes
Perceived returns to effort, entrance exam	All	0.299	0.188	Yes
D. DYNAMICS				
Correlation(take entrance exam, enroll in college)	All	0.265	0.270	No
Correlation(admitted to college, enroll in college)	All	0.849	0.820	No
Correlation(academic high school track, enroll in college)	All	0.193	0.101	No
Correlation(baseline test scores, enroll in college)	All	0.392	0.308	No
12^{th} grade GPA of college entrants	Control	6.24	6.23	No
12^{th} grade GPA of college entrants	Treatment	6.27	6.27	No
12^{th} grade GPA of college entrants	All	6.26	6.25	No

Notes: The last column identifies statistics that were directly targeted in estimation and statistics that were not. Perceived returns to effort are the expected change in outcome for an additional hour of study per week in the semester. Expected entrance exam score is measured in standard deviations of the exam scores; expected and actual GPA are measured on a scale from 1 to 7. Study hours are measured in reported study hours per week in the semester. The treatment effects are obtained from OLS regressions that do not use fieldworker fixed effects.

TABLE B.17: AVERAGE TREATMENT EFFECT ON PRE-COLLEGE ACHIEVEMENT SCORE USING IRT

	Standardized Achievement Score (IRT)	
Treatment	-0.084^{**}	-0.081^{**}
	(0.040)	(0.040)
Inverse Probability Weights	No	Yes
Observations	6,054	6,054
R^2	0.254	0.254

NOTE.– Coefficients are OLS estimates. Standard errors are clustered at the school level. Standard set of controls (see notes under Table 5) and with fieldworker fixed effects. *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program. Scores are scaled using Item Response Theory models, and standardized to have mean zero and variance one. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

TABLE B.18: ANALYSIS OF SCHOOL TRANSITIONS

	In-flow into treated schools	Out-flow from treated schools
SIMCE score in 10^{th} grade (std)	0.006	-0.007
	(0.012)	(0.012)
Constant	0.088***	0.115***
	(0.017)	(0.017)
Observations	3,925	4,073

NOTE.– Probability to transition into or out of a school which was randomly assigned to be treated in 2016, in the experimental cohort under study. Coefficients are OLS estimates. Standard errors (clustered at school level) are displayed in parentheses. In column (1) the sample consists of all students who were enrolled in a treated school in 2016, the dependent variable is a dummy equal to one if, in 2015, the student was not enrolled in a school that was randomized to be treated in 2016. In column (2) the sample consists of all students who, in 2015, were enrolled in a school which was randomized to be treated in 2016. The dependent variable is a dummy equal to one if the student was not enrolled in a treated school in 2016. Both samples exclude students who in 2015 or in 2016 were enrolled in schools which participated in the PACE program but not as part of the randomized experiment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE B.19: TEACHER GRADING

	12^{th} grade core GPA (standardized)	
	(1)	(2)
Achievement Score	0.335***	0.247***
	(0.025)	(0.025)
Achievement Score × Treatment	-0.031	-0.052
	(0.035)	(0.034)
Baseline SIMCE test score	No	Yes
Observations	6,046	6,046
R^2	0.216	0.262

Notes: Coefficients are OLS estimates. Standard errors are clustered at the school level. Standard set of controls except for baseline SIMCE test score (see notes under Table 5). Inverse Probability Weights used. *Core GPA* is the GPA in the core subjects, which are those tested on the PSU entrance exam. *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

TABLE B.20: SURVEY OF SCHOOL PRINCIPALS: GRADING METHODS AND SUPPORT CLASSES

	(1) Teachers discuss	(2) Teachers adjust	(3) Support (general)	(4) Support PSU	(5) Frequency support
Treatment	-0.019	-0.020	-0.056	0.042	-0.113
	(0.069)	(0.078)	(0.089)	(0.082)	(0.155)
Observations	127	127	127	127	64

Notes: Coefficients are OLS estimates. *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$. Outcome variables: dummy variables indicating whether teachers meet at the end of the year to discuss the grades of each student (column 1), whether teachers adjusts grades based on students' motivation, effort or other reason (column 2), whether the school offered support classes in any subject (column 3) and support classes for PSU entrance exam preparation (column 4) to the cohort of students under study. The outcome in the last column is the number of support classes per week.

TABLE B.21: SURVEY OF SCHOOL PRINCIPALS: ASSIGNMENT OF STUDENTS TO CLASSROOMS

	(1) Assignment Fixed	(2) Ability Tracking	(3) Random Assignment	(4) Alphabetical Assignment
Treatment	0.044	-0.049	-0.012	0.048
	(0.071)	(0.090)	(0.078)	(0.046)
Observations	127	93	127	127

Notes: Coefficients are OLS estimates. *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01. The outcome variables are dummy variables indicating whether: a student must stay in the same class throughout high school (column (1)), the school allocate students to classrooms based on ability (column (2)), the school allocates students to classrooms at random (column (3)), the student allocates students to classrooms alphabetically (column (4)).

B.5 Additional Details on Analysis of Mechanisms

B.5.1 Construction of Teacher Variables

This Section explains how we constructed the teacher variables that enter Table B.12 from the survey data that we collected among the Mathematics and Language teachers of the students in our sample.

Teacher effort. For each teacher we observe the hours the teacher spends to prepare his/her classes, and the number of days the teacher was absent from school.

Teacher's focus of instruction. This variable measures whether the teacher is targeting his/her teaching to a specific part of the student ability distribution.

For Mathematics and Language teachers separately we construct a variable indicating the difficulty level at which the teacher is teaching using survey questions about how much of various components of the curriculum the teacher covered during the term, coupled with the teacher's assessment of the difficulty level of each component. For example, for Mathematics we present the teacher with a list of the 4 subfields taken from the official national curriculum ("Algebra and Functions", "Geometry", "Statistics and Probability", "Trigonometry"), and for each subfield we present the teacher with a list of topics taken from the official national curriculum (for example, for "Algebra and Functions" two topics are "logarithmic and exponential function and analysis of their graphs" and "solution of second degree equations"). In all, we presented Mathematics teachers with 13 topics and Language teachers with 11 topics. For each topic, we first ask the teacher what percentage he/she was able to cover during the first semester (which was over when the data collection started). Second, we ask the teacher to think of the average student in his/her 12^{th} grade class, and tell us whether he/she thinks that this student would find the topic easy or difficult to understand. The answers to these questions were collected as 5-point Likert scales. Finally, we multiply the coverage and difficulty within each Mathematics (Language) topic and sum over all topics.

B.5.2 Beliefs over Returns to College Degree

Our survey included the survey instruments developed in Attanasio and Kaufmann, 2014 to elicit students' beliefs about returns to a college degree. We elicited beliefs about the distribution of wages at age 30 with and without a college degree. We find that students think that, on average, the return to a college degree is 200 percent. This is in line with observed differences in wages between Chileans with and without a college degrees, and in line with results from other surveys on different samples of Chilean high-school students (Hastings, Neilson, Ramirez, and Zimmerman, 2016).

We found that the treatment did not have any impact on students' beliefs about returns to education (no impacts on the mean nor on the variance of the returns), as reported in Table B.22.

TABLE B.22: TREATMENT EFFECT ON MEAN AND VARIANCE OF SUBJECTIVE EARNINGS DISTRIBUTIONS AT AGE 30, WITH AND WITHOUT A COLLEGE DEGREE.

	Expected Earnings (Elicited)		Expected Earnings (Estimated)		Variance of Earnings (Estimated)	
	Without	With	Without	With	Without	With
Treatment	-0.005	-0.004	-0.108	-0.102	-0.005	0.069
	(0.010)	(0.014)	(0.066)	(0.065)	(0.024)	(0.062)
Observations	3,339	2,048	4,219	2,674	4,219	2,674
R-squared	0.094	0.057	0.016	0.013	0.000	0.001

NOTE.– Standard errors clustered at school level. Inverse probability weights used. Expected earnings measured in million CLP. Variance measured in million CLP squared. Variance regressions are median regressions. "Without" means without a college degree. "With" means with a college degree. *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program. Standard set of controls (gender, age, *Prioritario* student, SIMCE, never failed a year, school track). Significance: *p < 0.10; **p < 0.05; ***p < 0.01.

Expected (mean) earnings were directly elicited, and we also estimated them, together with the variance of earnings, from elicited c.d.f. values. We report results on both measures of expected earnings, for comparison.

The survey questions asked "How much do you expect to earn per month with (without) a college degree on average?" and "How likely are you to earn at least X pesos per month with (without) a college degree?" where X=200.000, 800.000 without a degree and X=300.000, $1,000.000$ with a degree. To calculate the mean and variance of expected earnings using the answers to these questions, we fit the reported c.d.f. values using log-normal distributions for each respondent in the sample. In the estimation sample we kept only the students that answered at least two questions for each scenario (with and without a degree), because we needed at least two c.d.f. values to estimate the mean and variance of the Log-normal distribution. Finally, we used the Generalized Method of Moments to find the mean and variance of the log-normal distribution that minimize the distance of the simulated mean and simulated c.d.f. values from their data analogues.

For variance regressions we use median regressions because the variance is very vulnerable to outlier survey responses in which a student gives the same probability to the likelihood that his/her earnings at age 30 will be above two different values.

B.6 Technical Appendix

B.6.1 Structural model parameterizations

This section describes the functional form assumptions we make in estimating the structural model.

The production functions of the PSU score and of GPA are as follows:

$$PSU_i = \beta_0^P + \beta_1^P e_i + \beta_2^P y_{i,t-1}^{(1)} + \epsilon_i^P, \tag{B.1}$$

$$GPA_i = \beta_0^G + \beta_1^g e_i + \beta_2^G y_{i,t-1}^{(2)} + \epsilon_i^G, \tag{B.2}$$

where $y_{i,t-1}^{(1)}$ is a baseline standardized test score and $y_{i,t-1}^{(2)}$ is the baseline GPA (we restrict GPA_i to be between 1 and 7). We assume that the technology shocks $\epsilon_i = [\epsilon_i^P, \epsilon_i^G]$ are distributed as bivariate normal: $\epsilon_{it} \sim N(0, \Sigma)$, with $\Sigma = \begin{bmatrix} \sigma_P^2 & \rho\sigma_P\sigma_G \\ \rho\sigma_P\sigma_G & \sigma_G^2 \end{bmatrix}$. Given a PSU score, the probability of a regular admission is

$$Pr(A_i^R = 1 | PSU_i, S_i = 1; \gamma) = \Phi(\gamma_0 + \gamma_1 PSU_i). \tag{B.3}$$

The subjective production functions of the PSU score and of GPA are as follows:

$$PSU_{it}^b = \beta_{0k_i}^{Pb} + \beta_{1k_i}^{Pb} e_{it} + \beta_2^{Pb} y_{it-1}^{(1)} + \epsilon_{it}^{PSU^b}, \quad \epsilon_{it}^{PSU^b} \sim N(0, \sigma_{PSU^b}^2) \tag{B.4}$$

$$GPA_{it}^b = \beta_0^{Gb} + \beta_{1k_i}^{Gb} e_{it} + \beta_2^{Gb} y_{it-1}^{(2)} + \epsilon_{it}^{GPA^b}, \quad \epsilon_{it}^{GPA^b} \sim N(0, \sigma_{GPA^b}^2) \tag{B.5}$$

where the shocks $(\epsilon_{it}^{PSU^b}, \epsilon_{it}^{GPA^b})$ are i.i.d. normal and capture belief uncertainty. Observationally identical students hold heterogeneous beliefs about the production function: parameters $\beta_{0k_i}^{Pb}, \beta_{1k_i}^{Pb}, \beta_{1k_i}^{Gb}$ vary with the student's unobserved type. The believed outcomes vary also with baseline characteristics and effort.

The subjective probability of a regular admission, conditional on taking the PSU entrance exam ($S_i = 1$), is equal to the subjective probability that a student's believed score will be above the believed admission cutoff. Students form a subjective probability distribution for the admission cutoff: $c_i^{Rb} \sim N(\bar{c}^{Rb}, \sigma^2_{cRb})$. Letting $\overline{PSU}^b_{it} = \beta^{Pb}_{0k_i} + \beta^{Pb}_{1k_i} e_{it} + \beta^{Pb}_2 y^{(1)}_{it-1}$ denote the expected PSU score, ϵ_i^{cRb} the mean-zero additive belief shock around the expected cutoff, and A_i^R a dummy for a regular admission, the subjective probability of a regular admission is:

$$\begin{aligned} Pr^b(A_i^R = 1|e_{it}, y^{(1)}_{it-1}, k_i, S_i = 1) &= Pr\left(\overline{PSU}^b_{it} + \epsilon_i^{PSU^b} \geq \bar{c}^{Rb} + \epsilon_i^{cRb}\right) \\ &= \Phi\left(\frac{\overline{PSU}^b_{it} - \bar{c}^{Rb}}{\sqrt{\sigma^2_{PSU^b} + \sigma^2_{cRb}}}\right) \\ &= \Phi\left(\gamma_0^b + \gamma_1^b \overline{PSU}^b_{it}\right), \end{aligned} \tag{B.6}$$

where $\gamma_0^b = \frac{-\bar{c}^{Rb}}{\sqrt{\sigma^2_{PSU^b} + \sigma^2_{cRb}}}$ and $\gamma_1^b = \frac{1}{\sqrt{\sigma^2_{PSU^b} + \sigma^2_{cRb}}}$ and $\Phi(\cdot)$ is the standard Normal cumulative distribution function. Given an expected PSU score, uncertainty is generated by uncertainty around own score ($\sigma^2_{PSU^b}$) and around the admission cutoff (σ^2_{cRb}), which are absorbed by the parameters γ_0^b and γ_1^b. As it is standard to impose functional form restrictions on subjective probabilities (e.g. Delavande and Zafar, 2019; Kapor, Neilson, and Zimmerman, 2020), we impose normality.

Letting $\overline{GPA}^b_{it} = \beta^{Gb}_0 + \beta^{Gb}_{1k_i} e_{it} + \beta^{Gb}_2 y^{(2)}_{it-1}$ denote the expected GPA, ϵ_i^{c15b} the mean-zero belief shock around the expected school cutoff[2], and A_i^P a dummy for a preferential admission, the subjective probability of a preferential admission, conditional on taking the entrance exam ($S_i = 1$), for students in treated schools is:

$$\begin{aligned} Pr^b(A_i^P = 1|e_{it}, y^{(2)}_{it-1}, k_i, S_i = 1) &= Pr\left(\overline{GPA}^b_{it} + \epsilon_i^{GPA^b} \geq c_0 + c\bar{1}5^b_i + \epsilon_i^{c15b}\right) \\ &= \Phi\left(\frac{\overline{GPA}^b_{it} - c_0 - c\bar{1}5^b_i}{\sqrt{\sigma^2_{GPA^b} + \sigma^2_{c15^b}}}\right) \\ &= \Phi\left(\xi_0^b + \xi_1^b(\overline{GPA}^b_{it} - c\bar{1}5^b_i)\right), \end{aligned} \tag{B.7}$$

[2]Students form a subjective probability distribution for the cutoff in their school: $c15_i^b \sim N(c\bar{1}5^b_i, \sigma^2_{c15^b})$, characterized by a heterogeneous expected cutoff, $c\bar{1}5^b_i$, with uncertainty around it, $\sigma^2_{c15^b}$. We assume our survey instrument measured the expected cutoff $c\bar{1}5^b_i$ for each student i. The elicited $c\bar{1}5^b_i$ is missing for less than 20% of students. We assume these students correctly predict the cutoff; thus, results provide a lower bound to the role that biased rank beliefs play in policy response.

where $\xi_0^b = \frac{-c_0}{\sqrt{\sigma^2_{GPA^b}+\sigma^2_{c15^b}}}$ and $\xi_1^b = \frac{1}{\sqrt{\sigma^2_{GPA^b}+\sigma^2_{c15^b}}}$.[3] Given an expected GPA and an expected cutoff, uncertainty is generated by the uncertainty around own GPA ($\sigma^2_{GPA^b}$) and around the school cutoff ($\sigma^2_{c15^b}$), which are absorbed by the parameters ξ_0^b and ξ_1^b. As before, we assume normality.

In the first period, the per-period utility from effort depends on how effort affects achievement. We assume achievement is produced as follows: $y_i = \alpha_{0k_i} + \alpha_1 x_i + \alpha_2 e_{it} + \alpha_3$. We assume that our survey measures study effort with additive noise: $e_i^o = e_i + \epsilon_i^{m.e.e.}$, where $\epsilon^{m.e.e.} \sim N(0, \sigma^2_{m.e.e.})$ is a classical measurement error. We assume that our standardized test score measures achievement with additive noise: $y_i^o = y_i + \epsilon_i^{m.e.y.}$, with $\epsilon_i^{m.e.y.} \sim N(0, \sigma^2_{m.e.y.})$.

As in the real-world admission system, the selectivity of an admission depends on a student's PSU (for regular admissions) and GPA (for preferential admissions). We assume the following functional forms:

$$q^R(PSU_i) = \lambda_0^R + \lambda_1^R PSU_i + \epsilon_i^{qR} \tag{B.8}$$

$$q^P(GPA_i) = \lambda_0^P + \lambda_1^P GPA_i + \epsilon_i^{qP}. \tag{B.9}$$

B.6.2 Additional identification details

First, we discuss the identification of unobserved heterogeneity. Unobserved types affect parameters of the perceived production functions, the utility from enrolling in college, and achievement. We discuss these sets of parameters separately.

Type-dependent heterogeneity in beliefs. Unobserved heterogeneity and measurement error on the survey answers used to elicit returns to effort generate variation across observationally identical students in perceived PSU scores, GPA, and returns to effort. We assume that the measurement error on the survey answers regarding hours of study under alternative hypothetical outcome scenarios, used to construct beliefs, is identically distributed to the measurement error on the reported actual hours of study. Therefore, variation in reported actual hours of study that is not explained by observed baseline characteristics identifies the variance of the measurement error. Having identified this parameter separately, we can use variation in beliefs between observationally identical students to pin down the unobserved heterogeneity in beliefs.

[3] Parameter c_0 is a net adjustment to the GPA and the cutoff to capture the fact that the top 15% rule is based on adjusted GPA.

Type-dependent heterogeneity in the utility from enrolling in college. Observationally identical students who face identical admission sets can make different enrollment decisions because of idiosyncratic preference shocks and because of permanent unobserved heterogeneity. To separately identify them we exploit the longitudinal aspect of our data. We observe student's preference-revealing choices at both the exam-taking decision stage and the enrollment stage. Unlike temporary preference shocks, permanent unobserved heterogeneity induces correlations in behavior over time, which allow us to pin down unobserved heterogeneity in the preference for college.

Type-dependent heterogeneity in achievement. Observationally identical students can obtain different scores on the achievement test because of different type-dependent unobserved ability and different realizations of the measurement error. To separately identify them, first, we assume that the type is discrete and the measurement error is continuous. Therefore, the observed modes in the part of the achievement score not explained by observed characteristics are informative about type-specific ability. Second, we exploit the longitudinal aspect of our data. Students of different types obtain different achievement scores, exert different levels of effort, and make different educational choices. Unlike measurement error, permanent unobserved heterogeneity induces correlations between achievement, effort and later outcomes that are not explained by baseline characteristics and, therefore, are informative about unobserved heterogeneity.

Second, we discuss how we mitigate potential endogeneity of the arguments of the subjective probability functions. For the subjective probability of a preferential admission, we use variation that comes from the experiment. The treatment makes this subjective probability salient: differences in choices across treatment groups are informative about the parameters of this subjective probability, because it governs pre-college behavior in the treatment group but not in the control group. For the subjective probability of a regular admission, we assume that there is a continuous characteristics (lagged achievement test score) that affects the expected entrance exam score but not the type distribution. Therefore, conditional on the variables that enter the type distribution (which include lagged GPA), variation in this lagged achievement score is exogenous. The intuition is that this variation captures idyosincratic, test-day shocks that are uncorrelated with a student's true ability or preferences.

B.6.3 Auxiliary Regressions and Moments

In this section we list the parameters of the auxiliary models and the additional moments we match in estimation. The standard set of controls in the regressions is: age, gender, very-low-SES index (*alumno prioritario*), dummy for whether the student ever failed a grade, school-track type, baseline SIMCE score.

1. Treatment Effect Regressions:

    - All parameters, including the constant, of a regression of achievement on treatment, the standard controls, and average GPA in 9^{th} and 10^{th} grade (9).
    - Coefficient on treatment of a regression of hours of study on treatment and the standard controls (1).
    - Coefficient on treatment of a regression of hours of study on treatment and the standard controls for the sample of students who report, at baseline, no intention to attend college (1).
    - Coefficient on treatment of a regression of college enrollment on treatment and the standard controls (1).
    - Coefficient on treatment of a regression of taking the entrance exam on treatment and the standard controls (1).

2. Descriptive Regressions:

    - Constant and coefficient of regression of hours of study on dummy for whether student has no intention to stay in school beyond high school (2).
    - Coefficient on 10^{th} grade GPA of regression of 12^{th} grade GPA on 10^{th} grade GPA (1).
    - Coefficient on baseline SIMCE score of regression of entrance exam score on baseline SIMCE score (1).
    - Coefficients on whether the student participated in the survey and on the average between 9^{th} and 10^{th} grade GPA in a regression of whether a student takes the entrance exam on these variables and on the standard controls (2).
    - Coefficient on the average between 9^{th} and 10^{th} grade GPA in a regression of study hours on this variable and on the standard controls (1).

3. Descriptive Statistics:

    - Mean and variance of hours of study (2).
    - Fraction of students admitted to college by treatment group and baseline achievement, i.e., above or below median SIMCE score (4).

- Correlation between regular admissions and PACE admissions for treated students (1).
- Fraction taking entrance exam by treatment group (2).
- Mean and variance of entrance exam score by treatment group (4).
- Fraction of students who enroll in college by treatment group and baseline achievement, i.e., above or below median SIMCE score (4).
- Fraction of students enrolled in college by very-low-SES status, i.e., *alumno prioritario* categorization (2).
- Mean and variance of GPA in the control group (2).
- All pairwise correlations between the expected score on the PSU, enrollment, and the actual score on the PSU (3).
- Mean and variance of perceived returns to effort in GPA production and in PSU production (4).
- Correlation between taking the entrance exam and enrollment in the control group (1).
- Correlation between study hours and enrollment in the control group (1).
- Correlation between study hours and admissions in the control group (1).
- Correlation between taking the entrance exam and perceived distance from the within-school cutoff in the treatment group (1).
- Correlation between taking the entrance exam and expected PSU score in the control group (1).
- Unexplained variation in achievement and GPA after controlling for all initial conditions in the model affecting these outcomes. Specifically, variance of the residuals from regressions of achievement and of GPA on treatment, GPA in 9^{th} grade and average GPA between 9^{th} and 10^{th} grade, a dummy for whether a student reported at baseline to not being interested in attending college, perceived within-school cutoff, and the standard controls (2).
- Fractions enrolling through the regular and through the PACE channel for those admitted through both channels (2).
- Selectivity of the regular and of the PACE admissions for those admitted through both channels (2).
- Mean and variance of expected GPA and PSU score (4).

B.6.4 Equilibrium of the Tournament Game in the Counterfactual

In the counterfactual that debiases students' beliefs, we must solve for the Bayesian Nash equilibrium of the tournament game that awards preferential seats. We start by defining the Bayesian Nash Equilibrium (BNE) of the simultaneous effort game in each treated school in the first time period, under the assumption that students have rational expectations. When making effort decisions in time period 1, students observe their type k_i, private information. The joint distribution of types in the school, $F(k_1, k_2, ..., k_n)$, is common knowledge. There are no other shocks privately observed by students in the first time period. The distribution of all other model shocks, which are realized in later periods, is common knowledge. Model shocks include preference $(\eta_{it}, \eta_{it}^R, \eta_{it}^P)$ and technological shocks $(\epsilon_{it}^P, \epsilon_{it}^G)$. Objective production functions are common knowledge. Types make this a game of incomplete information.

$e_i(\cdot)$ is a function mapping $\{1, 2, ..., K\}$ into $\{0, 1, 2, ..., E\}$, the set of effort choices. This is the strategy for student i. Given a profile of pure strategies for all students in the school, $(e_1(k_1), e_2(k_2), ..., e_n(k_n))$, the expected payoff of student i is

$$\tilde{u}_i(e_i(k_i), k_i, e_{-i}(\cdot)) = E_{k_{-i}}[u_i(e_1(k_1), e_2(k_2), ..., e_n(k_n), k_i)],$$

where u_i is the sum of the first period utility and the expected value functions calculated using objective admission likelihoods. Let I denote the set of students in the school and E_i denote the pure strategy set of student i.

Definition 2 Rational Expectations Equilibrium. *A (pure strategy) Bayesian Nash equilibrium for the Bayesian game* $[I, \{E_i\}, \{\tilde{u}_i(\cdot)\}]$ *is a profile of decision rules* $(e_1^*(k_1), e_2^*(k_2), ..., e_n^*(k_n))$ *that are such that, for every* $i = 1, 2, ..., n$ *and for every realization of the type* k_i*,*

$$\tilde{u}_i(e_i^*(\cdot), k_i, e_{-i}^*(\cdot)) \geq \tilde{u}_i(e_i^{'}(\cdot), k_i, e_{-i}^*(\cdot))$$

for all $e_i^{'} \in \{0, 1, 2, ..., E\}$.

Intuition for approximation. Solving for the rational expectations equilibrium requires solving for a multi-dimensional fixed point in the vector of decision rules in each school. To reduce the dimensionality of the problem, we find an approximation to the rational expectations equilibrium.[4] Given an equilibrium profile of strategies for students $-i$, $e_{-i}^*(\cdot)$,

[4] We thank Nikita Roketskiy for suggesting this approximation. All errors are our own.

each effort choice of student i maps into the expected probability of a preferential admission for student i: $P_i^{15}(e_i, e_{-i}^*(\cdot))$, where the expectation is taken with respect to others' types. It is only through this probability that the strategies of others enter own payoffs. We posit a parametric approximation to this probability, $\check{P}^{15}(e_i, \gamma)$, where γ captures the strategy profiles of students $-i$. Let $\check{u}_i(e_i(\cdot), k_i, \check{P}^{15}(e_i, \gamma))$ denote i's approximated expected payoff.

Definition 3 Approximated Rational Expectations Equilibrium. *An approximation to the (pure strategy) Bayesian Nash equilibrium for the Bayesian game* $[I, \{E_i\}, \{\tilde{u}_i(\cdot)\}]$ *is a* γ^* *that is such that:*

- *given* γ^*, *each* i *and* k_i *chooses a decision rule* $\check{e}_i(k_i)$ *that maximizes his/her approximated expected payoff:*

  $$\check{u}_i(\check{e}_i(k_i), k_i, \check{P}^{15}(\check{e}_i, \gamma^*)) \geq \check{u}_i(e_i^{'}(\cdot), k_i, \check{P}^{15}(e_i^{'}, \gamma^*))$$

  for every $i = 1, 2, ..., n$, $k_i = 1, 2, ...K$ *and for all* $e_i^{'} \in \{0, 1, 2, ..., E\}$.

- *given the profile of decision rules* $(\check{e}_1(k_1), \check{e}_2(k_2), ..., \check{e}_n(k_n))$, *the approximated admission probability is close to the true admission probability for all* i*:*
  $P_i^{15}(\check{e}_i, \check{e}_{-i}(\cdot)) \approx P^{15}(\check{e}_i, \gamma^*)\ \forall i = 1, ..., n.$

Algorithm. Solving for the approximated rational expectations equilibrium requires solving for a fixed point problem of the dimension of γ^*. We use a linear probability approximation: $\check{P}^{15}(e_i, \gamma) = \gamma_0 + \gamma_1 GPA_{it}(e_i; \epsilon_{it}^G) + \gamma_2 X_i + \gamma_3 Z_j$, where GPA_{it} is own GPA, X_i are baseline student characteristics and Z_j are baseline school characteristics, and use the following algorithm:

1. Draw types and shocks for all students and fix these draws across iterations.

2. From the data, estimate a linear probability model of the likelihood of a preferential admission as a function of own GPA and of baseline characteristics of the student (X_i) and of the school (Z_j) selected through LASSO:

   $$Prob_i(Adm^P = 1 | GPA_{it}, X_i, Z_j) = \gamma_0 + \gamma_1 GPA_{it} + \gamma_2 X_i + \gamma_3 Z_j + \epsilon_{ij}$$

   Let the estimates $\hat{\gamma}_0, \hat{\gamma}_2, \hat{\gamma}_3$ be fixed across iterations, let the estimate $\hat{\gamma}_1$ be our first guess in all schools: $\gamma_{1j}^{(s=0)}$. The goal is to find a fixed point in γ_{1j}.

3. At the current iteration s, let students believe that

$$\begin{aligned} P_i^{15(s)}(e_i, \check{e}_{-i}(\cdot)) &= P_i^{(s)} = \\ &= \hat{\gamma}_0 + \gamma_{1j}^{(s)} GPA_{it}(e_i; \epsilon_{it}^G) + \hat{\gamma}_2 X_i + \hat{\gamma}_3 Z_j. \end{aligned}$$

4. Given these beliefs, find the best reply of each student. Let $e_{it}^{(s)}$ be the utility maximizing effort that each student exerts.

5. Calculate $GPA_{it}^{(s)} = GPA(e_{it}^{(s)}; \epsilon_{it}^G)$. Assign PACE slots to those with a GPA in the top 15 percent of their school and who took the entrance exam.

6. From the simulated data on PACE slot allocations and $GPA(e_{it}^{(s)}; \epsilon_{it}^G)$, compute $\gamma_{1j}^{(s+1)}$ by OLS.

7. If $\gamma_{1j}^{(s+1)}$ is sufficiently different from $\gamma_{1j}^{(s)}$, go back to point 3, otherwise stop.

We checked for uniqueness by plotting the $\gamma_{1j}^{(s+1)}$ against the $\gamma_{1j}^{(s)}$ and found that there is a unique fixed point in all schools.

B.7 PISA score re-scaling

Figure 1 plots the histogram of tenth grade SIMCE test scores, and draws a line corresponding to the OECD mean for reference (at 0.49). Since the SIMCE tests are administered only nationally, we draw on data from PISA in Chile and in OECD countries to predict the SIMCE mean in OECD countries. This is the reasoning and procedure we follow:

- In 2015 the mean PISA scores of Chile were 447 in Science, 459 in Reading, 423 in Math.

- In 2015 the mean PISA scores of OECD were 493 in Science, 493 in Reading, 490 in Math.

- There is theoretically no minimum or maximum score in PISA; rather, the results are scaled to fit approximately normal distributions, with means around 500 score points and standard deviations around 100 score points.

- Therefore, OECD countries had a:
  - mean Science score of $\frac{493-447}{100} = 0.46$ standard deviations above the Chilean one;

  - mean Reading score of $\frac{493-459}{100} = 0.34$ standard deviations above the Chilean one;
  - mean Mathematics score of $\frac{490-423}{100} = 0.67$ standard deviations above the Chilean one;

- On average, OECD countries had mean PISA scores that were higher than the Chilean mean PISA score by $(0.46 + 0.34 + 0.67)/3 = 0.49$ standard deviations.
- Sources: Link 1, Link 2

Appendix C

Appendix: Health Effects of Increasing Income for the Elderly: Evidence from a Chilean Pension Program

C.1 The pension score

The pension score was created solely to determine basic pension recipients and has no further use for other public agencies. This score is calculated as follows:

$$\textit{Pension score}_g = \frac{\sum_i^{n_g}\{Y_{i,g} + \textit{YP}_{i,g}\}}{\textit{IN}_g} \times F \tag{C.1}$$

Where:

- $Y_{i,g}$ is the labor income for person i in household group g.
  - For elderly household members, the National Revenue Service provides this information. In cases where Revenue Service records do not show any income from a particular person, the Pension Institute uses the self-reported measure collected from the social security score.
  - For working-age household members, labor income is imputed using a variation of the Mincer equation (also referred to by its Spanish name, "capacidad de generar ingreso" or CGI), which includes gender, level of education, town of residence, among other variables. This number is estimated by the Ministry of Planning and the equation is not known to the public. In this way, the government avoids score manipulations by working-age household members not reporting their full income or leaving their employment.

- $YP_{i,g}$ is income from other pensions, government transfers, financial assets and any other income source not considered in $Y_{i,g}$ for person i in household group g. The National Revenue Service, the Ministry of Planing, banks and the private companies administering the pension funds provide this information. If these institutions do not show any record for a person, the Pension Institute uses the self-reported measure collected from the social security score.

- IN_g is the household size of household g, adjusted by the level of disability of each household member. This index is computed as the sum of people in the household, with household members above the age of 65 and those in the national register of disabled persons adding an extra 0.4 and 1.3 points to this index, respectively.

- n_g is the number of people in the household group g.

- F is a transformation factor used to convert the results to the scale of the pension score. This factor is not publicly available and is not available to us.

For 2012 applicants, labor income from household members and income from assets represent on average 40% and 60% of the numerator of the pension score, respectively. This shows that wealth in the form of other pensions or financial assets seems to be the most relevant factor in the pension score for the average applicant, with labor income being relatively less important.

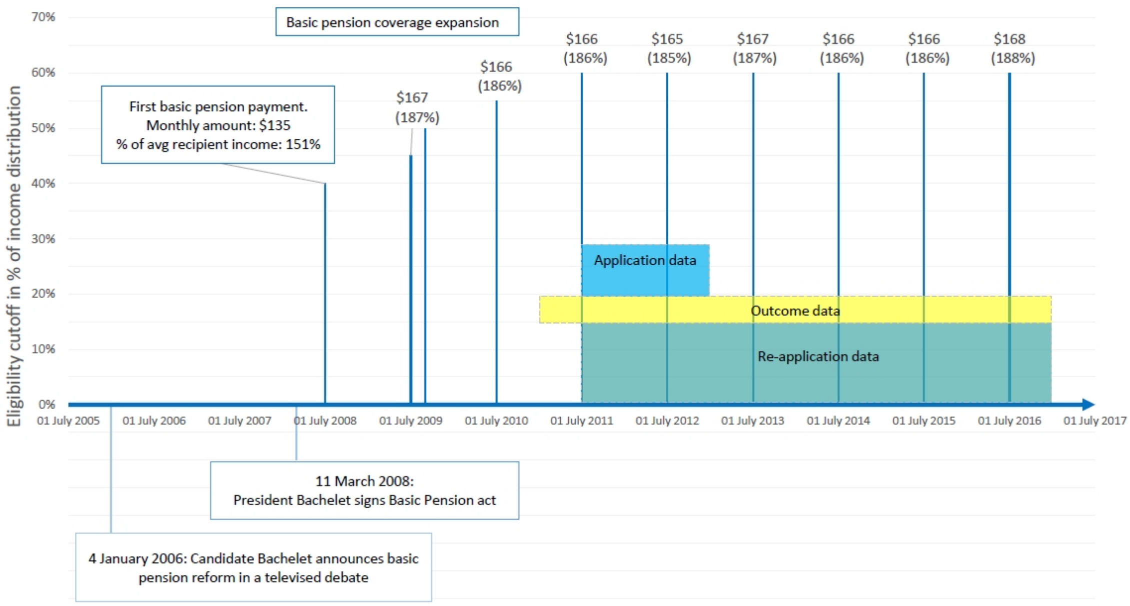
For applicants who submitted an application in 2011 or 2012, the pension score runs between 0 and 43,103 score points. To determine the 60$^{\text{th}}$ percentile for the Chilean population in 2011, the Pension Institute used data from the national household survey and estimated a pension score for each household in the survey. The cut-off then corresponds to the 60$^{\text{th}}$ percentile of the estimated pension score for the sample of households in the survey. There have been no updates to the pension score cut-off since July 2011, when the 60$^{\text{th}}$ percentile was estimated at 1,206 pension score points.

Overall, the majority of the elderly population who did not receive a contributory pension applied to receive a basic pension. In 2011, 64.3% of retirees without a contributory pension received a basic pension (Ministerio de Desarrollo Social, 2011) and an extra eight percent of those without a contributory pension submitted an unsuccessful application according to our records. Appendix Table C.12 shows the characteristics of the elderly population without contributory pensions in 2011.

Pension payments

Monthly income from the basic pension has been adjusted yearly at a level that is around the inflation rate, except in 2009, when the increase was well above the inflation rate. Appendix Figure C.1 shows the evolution of the cut-off and pension payments, along with their dates of changes. This figure also shows the years for which we have data.

FIGURE C.1: Timeline of the basic pension reform



Notes: This figure shows the evolution of the basic pension reform, the expansion of its coverage and monthly payment amounts from 2008 onwards. Dates, eligibility cut-off points, and payment amounts are reported by the Chilean Pension Institute. Payments are in 2012 US dollars. To obtain payments in 2012 US dollar, we transformed the nominal value of the payments into 2012 Chilean pesos using the consumer price index and converted this amount into US dollars using the 2012 exchange rate. In parentheses, we report payments as percentages of the average recipient's income at the cut-off in 2012. The 'outcome data' horizontal bar represents the timeframe for which we have outcome data (January 2011 to December 2016). The 'application data' horizontal bar represents the timeframe in which we analyze the first applications of the applicants (July 2011 to December 2012). The 're-application data' horizontal bar represents the timeframe for which we have data on applications for the applicants that re-applied after a first application (July 2011 to December 2016).

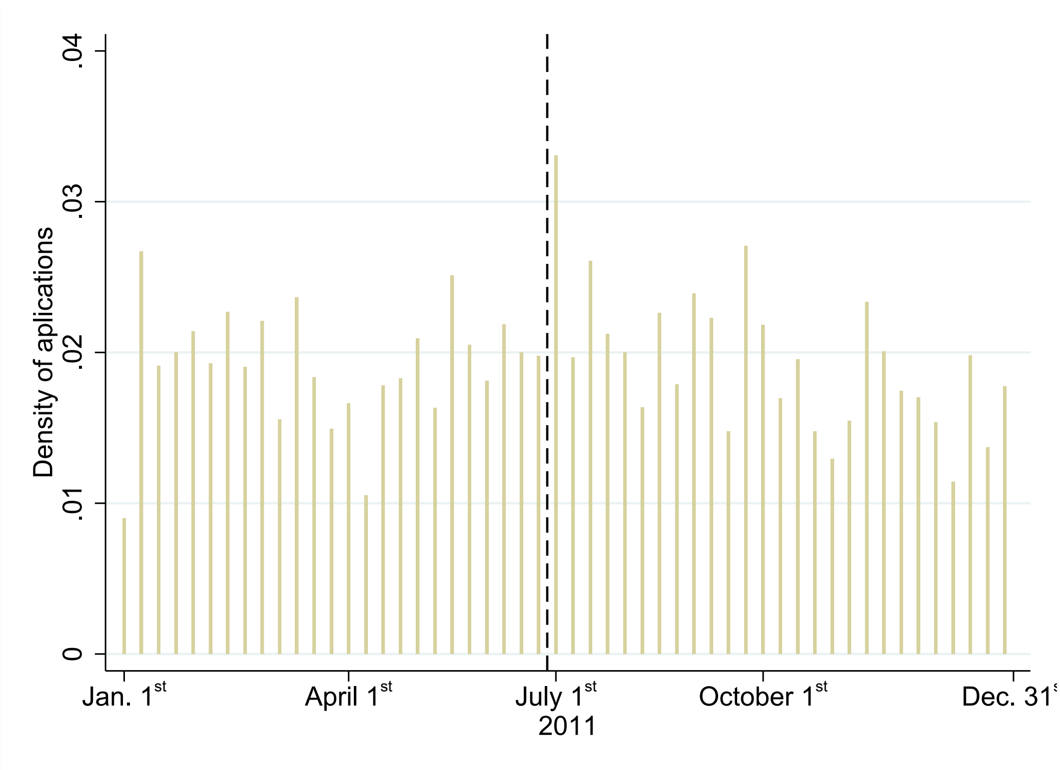
Basic pension payments can be received by bank transfer or collected in person with an ID card. In our sample, 96% of recipients collect their pension in person. This indicates that the pension payments are effectively being received by applicants.

Basic pension payments cease if the recipient spends more than 90 days abroad in a single calendar year. The person can apply again, but they will need to prove 270 days of continuous residency in Chile in the year before applying. Payments also cease if the recipient does not collect any pension money within six months. In this case, recipients of the basic pension have another six months to request that the Pension Institute restore their payments. If this is not done, the basic pension expires and people in this category can apply again for a basic pension without any restriction. Finally, payments immediately cease when the pension recipient dies.

Less than 0.05% of recipients who obtained the basic pension between 2008 and 2015 stopped receiving it at some point (Subsecretaría de Previsión Social, 2015). All of these were for reasons unrelated to the pension score (e.g. emigration).

C.2 Anticipating behavior

FIGURE C.2: Weekly density of applications over 2011



Notes: This figure shows the weekly density of applicants (both recipients and non-recipients) in 2011. The dashed vertical line represents the change in the pension score cut-off on July 1st, 2011.

The cut-off changes from covering 55% to covering 60% of the pension score distribution on July 1st, 2011 (Appendix Figure C.1). This may have incentivized people to wait until this date to apply, in order to increase their probability of receiving a pension. Appendix Figure C.2 shows an increase in the density of applications in the week beginning on July 1st, 2011, which is statistically significant according to the density test by Cattaneo, Jansson, and Ma (2019). However, this increase appears to be transitory and disappears immediately after the first week of July. The absence of a strong anticipating behaviour can be rationalized by considering that the cut-off increase was not large, the monetary cost of applying is zero and individuals can apply multiple times without a penalty. Thus the increase in the number of applicants in the week beginning on July 1st is arguably due to people stalling their application for only a short time or re-applying, and does not appear to affect the external validity of the main results. Our point estimates remain significant and of similar magnitude when we exclude applicants that applied in the first week of July 2011 (results are available upon request).

C.3 Serial applicants

Figure 3.1 shows that few applicants below the cut-off did not receive the basic pension. This is explained by reasons unrelated to the pension score (e.g. not redeeming the pension in time). This figure also shows that a relevant number of applicants above the cut-off obtained a basic pension within four years. This is fully explained by non-recipients who submitted a subsequent application (henceforth referred to as serial applicants) that was successful.

To analyze the characteristics of serial applicants, we regress an indicator for whether the person is a serial applicant against baseline covariates. Column (1) of Appendix Table C.1 presents a series of bivariate regressions in which each baseline characteristic is entered separately, while columns (2), (3), and (4) show estimations that regress on multiple covariates simultaneously. This table shows that applicants above the cut-off who are older and have a higher social security score are less likely to be serial applicants, while those in a larger household are more likely to apply more than once. This could be because: 1) older applicants might perceive a lower present value of the basic pension income (they expect to live for a shorter time); and, 2) wealthier people believe they are less likely to obtain the pension. In contrast, people in larger families might be more likely to see changes in their household composition or income. They may believe that these changes will affect their pension score which encourages them to reapply.

TABLE C.1: The effect of baseline covariates on the probability of applying multiple times

	(1)	(2)	(3)	(4)
Female	-0.076	-0.001	0.001	0.004
	(0.020)	(0.021)	(0.021)	(0.020)
Age (years)	-0.023	-0.019	-0.018	-0.016
	(0.001)	(0.001)	(0.001)	(0.001)
Social security score	-0.031	-0.026	-0.027	-0.025
	(0.001)	(0.001)	(0.002)	(0.002)
Days hospitalised	0.000	0.003	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Received influenza vaccination	0.017	0.034	0.037	0.014
	(0.013)	(0.013)	(0.013)	(0.014)
Received pneumonia vaccination	0.067	-0.001	-0.005	0.024
	(0.029)	(0.030)	(0.030)	(0.030)
Household size	0.022		0.021	0.023
	(0.006)		(0.010)	(0.010)
Elderly cohabitant	-0.116		-0.032	-0.030
	(0.014)		(0.017)	(0.017)
Working-age cohabitant	0.089		0.023	0.021
	(0.012)		(0.019)	(0.019)
Live with child under 16	0.106		0.009	-0.017
	(0.063)		(0.060)	(0.062)
Fertility age women	0.073		-0.027	-0.027
	(0.016)		(0.019)	(0.019)
FIXED EFFECTS	NO	NO	NO	YES
N	6,423	6,423	6,423	6,423

Notes: Using the sample of all applicants above the cut-off, this table reports results from OLS regressions of a binary indicator equal to 1 if the individual submitted at least another application within 4 years from the first application (and 0 otherwise) on several covariates. Column (1) reports coefficients of bivariate regressions. Columns (2), (3) and (4) report coefficients of multivariate regressions on the specified variables. Fixed effects are at the month-of-application and the health-district level. Standard errors are clustered at the province level. For ease of interpretation, the social security score is rescaled (divided by 1,000).

C.4 Set of controls used in the robustness estimations

We test the robustness of our results by replicating them on several specifications. For the specification in which we use a polynomial of order 1 in score and other controls, we perform the regressions using the following control variables:

- Individual and household covariates: month-year of the first application fixed effect, age of application fixed effect, gender, social security score, and number of applicants in the household. We also use the following household characteristics prior to applying: dummy for whether the applicant lives with an elderly household member, dummy for whether the applicant lives with a working-age relative, dummy for whether the applicant lives with a person below 16 years of age, and household-size fixed effects.

- Health covariates six months before applying: percentage of days of hospitalization, dummy indicator for whether the applicant had been given a pneumonia vaccination, and dummy indicator for whether the applicant had been given an influenza vaccination.

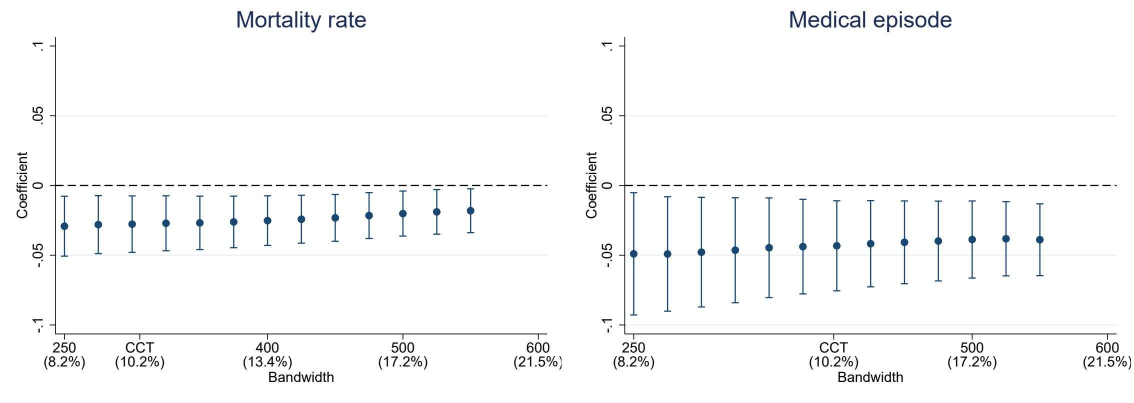
- Geographical covariates: health service fixed effects, the number of health facilities per square kilometer, municipal income per capita, whether the town is rural or urban, and whether there is a hospital in the town.

C.5 Sensitivity and placebo checks on the direct health effects

Appendix Table C.13 shows that the causal effect of the basic pension on mortality and medical episodes remains qualitatively unchanged whether we use logistic regressions, non-parametric estimations, different sets of controls, or polynomials of order two in $Score_h$. When we include all controls, the p-values are slightly higher but remain small. Figure C.3 also shows that the results do not change when we use different bandwidths around the cut-off, suggesting also that our results are not driven by observations far away from the cut-off.

Additionally, we implement the randomization inference method proposed by Cattaneo, Frandsen, and Titiunik (2015) on the mortality estimate. This method randomly varies which observations are assigned to treatment and control in a window around the threshold where treatment status is as good as randomly assigned. After running this permutation test based on difference in means, we reject the null hypothesis of no mortality effect with a p-value < 0.001. We also set placebo thresholds along the score distribution at intervals of 25 score-points and perform reduced form estimates at every placebo threshold. Figure C.4 compares these estimates and shows that the probability of obtaining a mortality estimate smaller than ours is as small as 0.0384. This result suggests that our estimated effect is not a random discontinuity that is likely to be observed in other parts of the score distribution.

FIGURE C.3: Robustness of results for mortality and medical episodes using different bandwidths

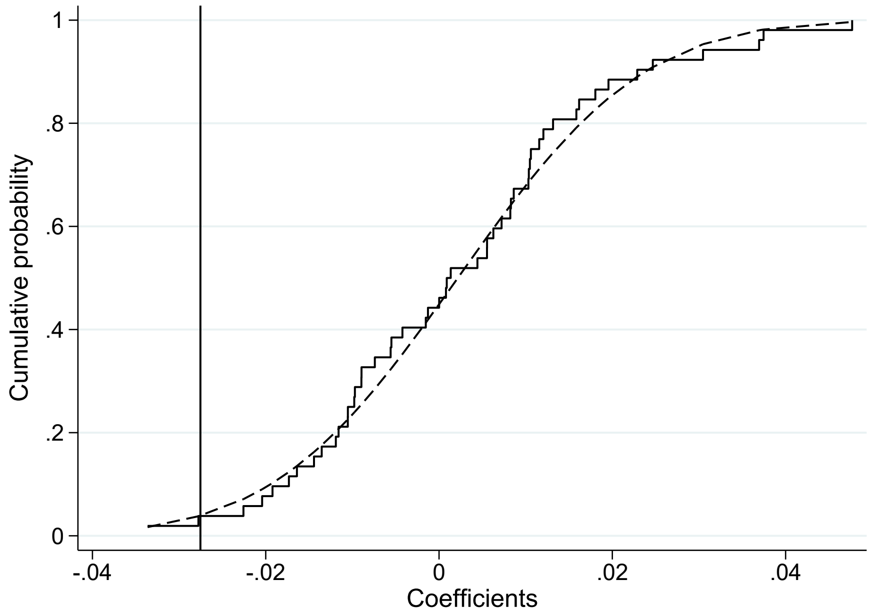


Notes: Each graph shows the point estimate and the standard error of the ITT effect of the basic pension on applicants' mortality and medical episodes, using different bandwidths and all controls specified in regression Equation (3.1). The x-axis labels report the number of score points in each side of the bandwidth and, in parentheses, the percentage of total applicants that fall in the bandwidth. CCT is the optimal bandwidth using the approach proposed by Calonico, Cattaneo, and Titiunik (2014).

Finally, according to the power calculation method suggested by Gelman and Carlin (2014), our mortality estimate appears to be well powered. Previous estimates in the literature find that the median income effect size on elderly mortality is 2.2 pp. and the average effect size is 2.7 pp. Barham and Rowberry (2013), Cheng et al. (2016), Feeney (2018), Jensen and Richter (2003), Salm (2011), and Snyder and Evans (2006).[1] In our power estimations, we use our standard error for the mortality effect (0.97 pp.) and a statistical significance threshold of 0.05 (Gelman and Carlin, 2014). Using these numbers, we obtain a power of 0.62 for the median average effect size (0.8 for the mean effect size). This is reassuring considering that problems with the exaggeration ratio (expectation of the absolute value of the estimate divided by the effect size) 'start to arise when power is less than 0.5, and problems with the Type S error rate [probability that the estimate has an incorrect sign if significant] start to arise when power is less than 0.1' (Gelman and Carlin (2014), p.643).

[1]The literature finds these mortality effect sizes using different income shocks, in different populations and historical periods. Keeping this caveat in mind, we prefer to use the face value of these estimates rather than adjusting them using an arbitrary criterion.

FIGURE C.4: Reduced-form effect of being below the cut-off on mortality: placebo estimates



Notes: This graph shows the cumulative distribution of reduced-form estimates on mortality, from placebo regressions in which the cut-off is set in different parts of the pension score distribution. Estimates are computed using the regression in Equation (3.1). Cut-offs are located every 25 points, starting from 306 (Calonico, Cattaneo and Titiunik's (2014) optimal bandwidth) up to 1606 score points, to make sure that we have observations in all points of the bandwidth. The cut-off is set at 1206 pension score points and the lowest pension score is zero. Therefore, placebo cut-offs are set between -900 and 400 pension score points from the cut-off. The solid line displays the empirical cumulative distribution of estimates and the dashed line displays fitted values of the cumulative distribution. The vertical line shows the coefficient estimated with our optimal bandwidth baseline specification.

C.6 Spillover effects on applicants' household members

C.6.1 Spillover results

This section provides causal evidence that a permanent income increase for the elderly poor can have spillover effects on the fertility of working-age household members. We are not aware of previous papers testing this directly, using administrative data and in a regression discontinuity design.

In Chile, the minimum legal age to claim contributory pension benefits is 65 for men and 60 for women, and the minimum legal working age is 15. Therefore, to analyze spillover effects, we define three exclusive groups of household members based on household members' age: 1) men above 64 and women above 59 years of age (elderly); 2) men aged 16-64 and women aged 16-59 years (working-age); and, 3) individuals below 16 years of age (school-age children). Given the small number of observations in this last group of household members (931), we focus the analysis on the first two groups.

TABLE C.2: Health outcomes over four years from application: household members by age

Variables	TOT (1)	S.E. TOT (2)	ITT (3)	S.E. ITT (4)	P-value (5)	BW (6)	N (7)	Control (8)
Panel A: working-age household members								
% days hospitalized	0.012	(0.035)	0.012	(0.021)	0.575	500	8,047	0.100
Newborn child	0.024	(0.010)	0.017	(0.008)	0.035	500	8,047	0.033
Panel B: female household members of fertility age (16-40)								
% days hospitalized	0.007	(0.043)	-0.005	(0.033)	0.872	500	2,058	0.116
Newborn child	0.098	(0.036)	0.067	(0.028)	0.023	500	2,058	0.130
Panel C: elderly household members								
Mortality rate	0.012	(0.016)	0.011	(0.013)	0.397	500	5,722	0.125
% days hospitalized	0.060	(0.084)	0.026	(0.055)	0.635	500	5,722	0.274
Medical episode	0.061	(0.038)	0.045	(0.032)	0.164	500	5,722	0.376

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (3.1). Column (1) reports the *treatment on the treated* coefficient as in Equation (3.4) and Column (2) reports its standard error computed using the delta method. Column (3) reports the *intent-to-treat* coefficient and Column (4) reports its standard error clustered at the province level. Column (5) reports the p-value of the ITT coefficient reported in Column (3). Column (6) reports the range of pension score points from the cut-off and Column (7) reports the number of observations in the regression. Column (8) reports the constant of the ITT regression, showing the variable mean for control applicants at the cut-off.

Panel A of Appendix Table C.2 shows that working-age relatives of basic pension recipients do not see a change in the percentage of days spent in hospital. This is not surprising, considering that working-age relatives are young (40 years old on average) and are rarely hospitalized.[2] Panel C of this table shows that elderly household members

[2]Covariates seem to change smoothly at the cut-off for working-age and elderly household members. Panel A of Table C.14 shows that 1 out of the 11 available covariates is significant for working-age household members. Panel B of Table C.14 shows that 2 out of the 10 available covariates are statistically significant among elderly household members. Appendix Table C.15 shows that adding covariates as controls does not change the results. Appendix Figure C.20 also shows no discontinuity in the density of applicants' working-age household members (t-statistic of -0.013 and p-value of 0.999) or elderly household members (t-statistic of -1.576 and p-value of 0.115) at the cut-off.

were more likely to die than applicants (their average mortality rate, in column (7), is 12.5 percent), but this seems to be unaffected by having a relative who receives the basic pension.

Section 3.5.3 shows that the household structure is a relevant determinant of the effect of the basic pension on recipients. One of the potential reasons is that families with a working-age household member pool income to different extents. To provide further evidence on the presence of intra-household transfers of income, we explore whether the fertility of working relatives living with recipients increases when pension payments begin. Becker (1960) suggests that children are normal goods, so their 'consumption' should increase when more income is available to parents. Panel A of Table C.2 reveals that working-age relatives are 2.4pp. more likely to have a newborn child nine months after the pension application or later. As our data only identifies mothers and not fathers of newborn children, Panel B repeats the analysis focusing on fertility-age women (16-40 years of age) and estimate that they are 9.8 pp. more likely to have a newborn nine months after the application or later.[3] The ITT effect of the pension is a 6.7pp. increase (p-value=0.023) on the probability of having a newborn from a baseline probability of 13.0pp. Appendix Section C.6.2 shows that fertility results remain statistically significant to a variety of robustness checks and are also in line with previous estimates in the literature.[4]

Our fertility results complement previous findings on the spillover benefits of non-contributory pensions on children's height, weight, school enrolment and attendance (Duflo, 2000, 2003; Edmonds, 2006); and on working-age relatives' self-reported nutrition, sanitation, and employment (Ardington, Case, and Hosegood, 2009; Case, 2004; Case and Menendez, 2007). The presence of spillover effects suggests that the benefits of pension policies could extend beyond the welfare of direct recipients and affect the life choices of younger generations.

The significant *spillover* effect on the fertility rate of working-age household members, combined with the insignificant *direct* effect on recipients living with them, could

[3] Appendix Figure C.21 shows no discontinuity in the density of applicants' fertility-age female household members (t-statistic of -1.131 and p-value of 0.258). Appendix Table C.16 shows that there is no imbalance out of 9 available covariates for female household members of fertility age.

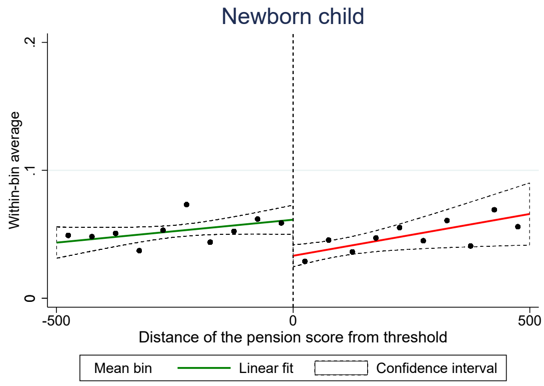
[4] According to our data, 49.9% of days spent in hospital by women of fertility age are due to pregnancy, childbirth and the puerperium. Hospitalizations for these reasons observe a significant increase if a family member receives a basic pension, in accordance with the positive effect on childbirth numbers. However, if we include days of hospitalization due to other causes, the estimation becomes less precise and we do not detect any significant effect. Results are available upon request.

be the result of intra-household transfers of income. As mentioned above, fertility is expected to increase when more income is available to parents (Becker, 1960).[5] On the one hand, working-age household members may have reduced their net transfers of income to applicants (current or expected future ones) after applicants started receiving the pension, and thus retained the necessary resources to raise a child. This would be consistent with previous evidence finding that social security benefits ‘crowd out’ 20%-30% of private transfers from younger generations to the elderly (Cox and Jimenez, 1992; Jensen, 2003), and the fact that a large fraction of recipients living with working-age relatives expect to finance their retirement with transfers from their children (see Section 3.5.3). On the other hand, recipients may transfer part of the pension to working-age household members, as documented in previous studies (Ardington, Case, and Hosegood, 2009; Duflo, 2000, 2003). This hypothesis would need to be reconciled with survey evidence showing that 82% of pension recipients do not share any money with their relatives or friends, and only 4% share more than one-fifth of their pension with others (Ministerio Trabajo y Previsión Social, 2017).

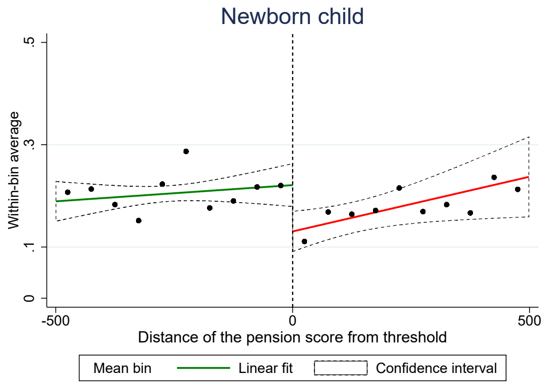
[5] Alternatively, we could have considered working-age household relatives’ consumption of other goods, such as food. Our administrative data does not contain consumption of these kinds of goods, and the EPS survey only contains household consumption without separating by household members.

FIGURE C.5: Effect of the basic pension on mortality and fertility of household members

(A) Working-age household members



(B) Female household members of fertility age



(C) Elderly household members

Notes: Each graph shows the average value of the corresponding variable conditional on the distance of the score from the cut-off. The circles are averages across 50-point bins on either side of the threshold, while the solid and dashed lines represent the predicted values and associated confidence interval, respectively.

Alternatively, receipt of the pension could reduce the cost of raising a child (for example, financially autonomous healthy grandmothers may be more able to accompany children to and from school) and increase fertility, as highlighted in the previous literature (D'Addio and d'Ercole, 2006; Kalwij, 2010; Liu et al., 2018). Even though we cannot separate the causes of our fertility results – an increase in income versus a decrease in the costs of child-raising – the latter does seem less relevant in our context, given that most pension recipients do not have any job to quit that might grant them more free time to provide support for their grandchildren (arguably the main cause of the reduction in child-raising costs).

C.6.2 Robustness of fertility results

This section explores the robustness and timing of the spillover effects on fertility and situates them in the context of the literature. Tables C.14 and C.16 show no imbalance in the probability of having a newborn before applying between the treatment and control groups. If we extend the analysis of the outcome up to 9 months after the application, we still find no evidence of imbalance between working-age (or women of fertility age) household members above and below the cut-off.

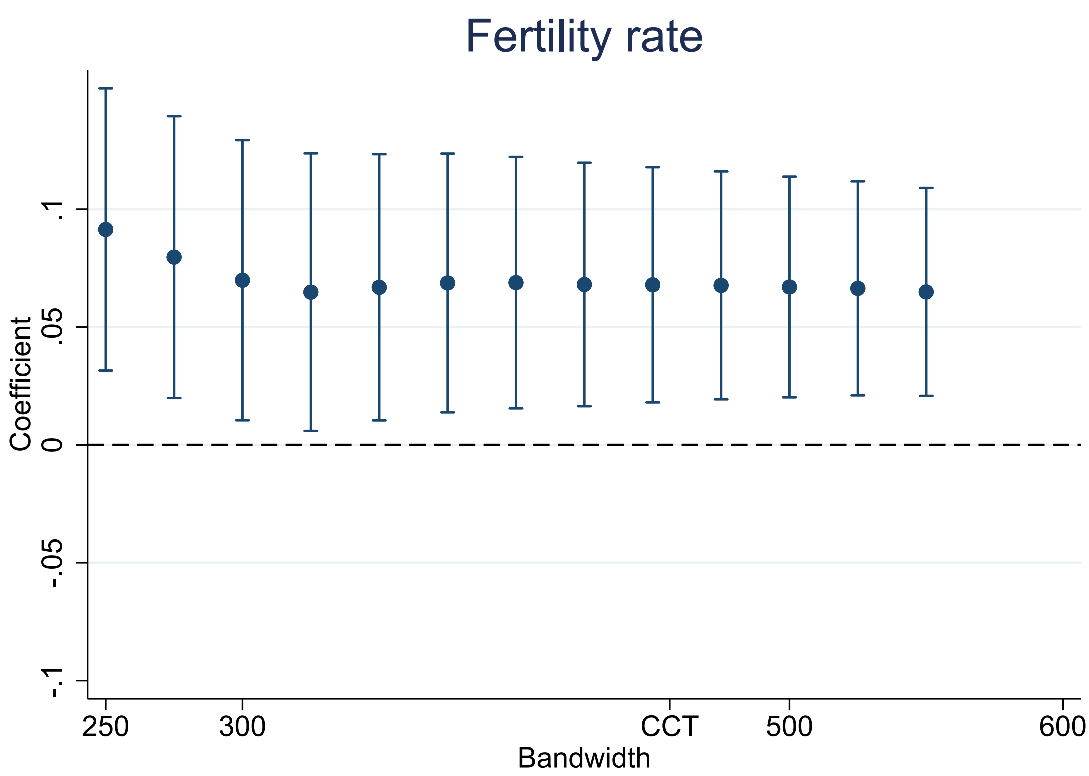
Appendix Tables C.15 and C.17 show that the results for working-age, female fertility-age, and elderly household members do not change when we use logistic regressions, non-parametric estimations, the optimal bandwidth approach proposed by Calonico, Cattaneo, and Titiunik (2014), or different sets of controls, nor when we control for a polynomial of order 2 in $Score_h$. This also ensures that the null effect on elderly household members is not driven by the slight imbalance in this group.

Figure C.6, shows that the fertility result remains positive and significant when using different bandwidths. Additionally, we implement the randomization inference method proposed by Cattaneo, Frandsen, and Titiunik (2015) on the fertility estimate and reject the null hypothesis of no fertility effect with a p-value < 0.001. We also set placebo thresholds along the score distribution, at intervals of 25 score-points, and perform reduced form estimates. Figure C.7 compares our estimate with the distribution of placebo estimates and shows that no estimate is higher than ours. This suggests that our estimated effect on fertility is not a random discontinuity that is likely to be observed in other parts of the score distribution. Finally, fertility estimates remain significant when adjusting our p-values for multiple hypothesis testing, with an adjusted p-value $= 0.03$ (Romano and Wolf, 2005a,b).

Figure C.8 shows the timing of childbirths for women of fertility age, between six months before and four years after the first application. Treated and control women in fertility age have a similar fraction of newborn children until 9 months after the application, with a slightly higher fertility rate for control group women. 1.2 years after the application, the two lines intersect and the treatment effect on fertility starts accumulating over time.[6] The fraction of women of fertility age who have a newborn is not small in this time span: almost a quarter of treated women and a fifth of control women had a child four years after applications are submitted.

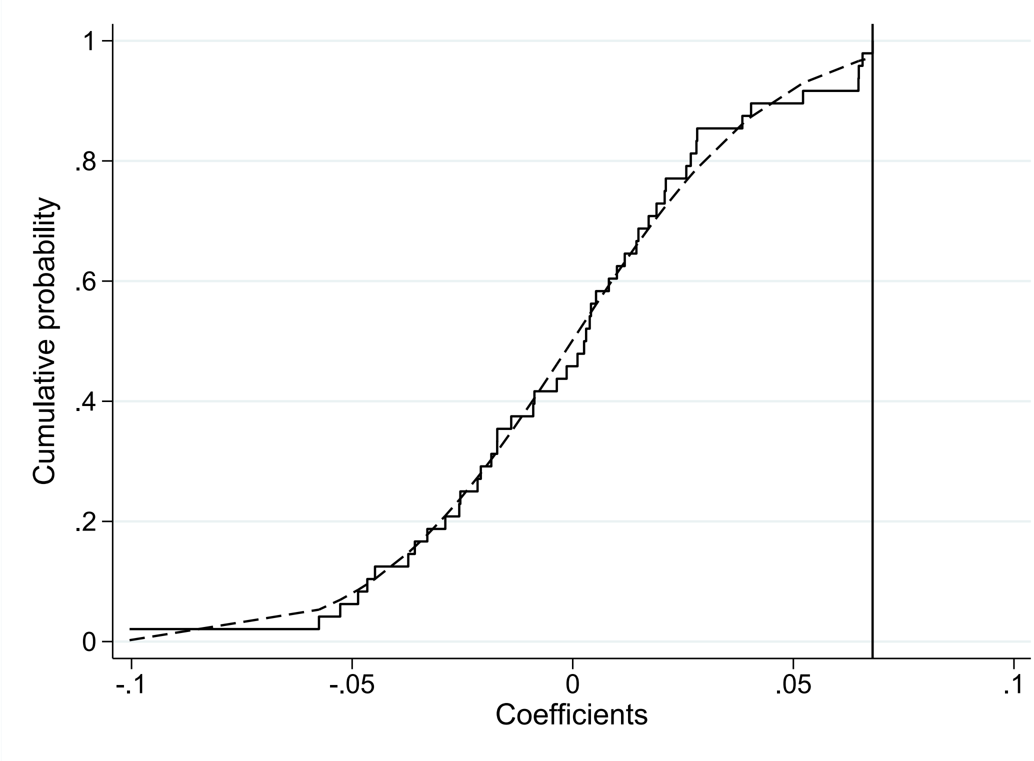
[6] In Appendix Figure C.22 we can see that the impact on fertility is not significant in the first year after the application, but it becomes evident since the second year after the application.

FIGURE C.6: Robustness of results for fertility using different bandwidths



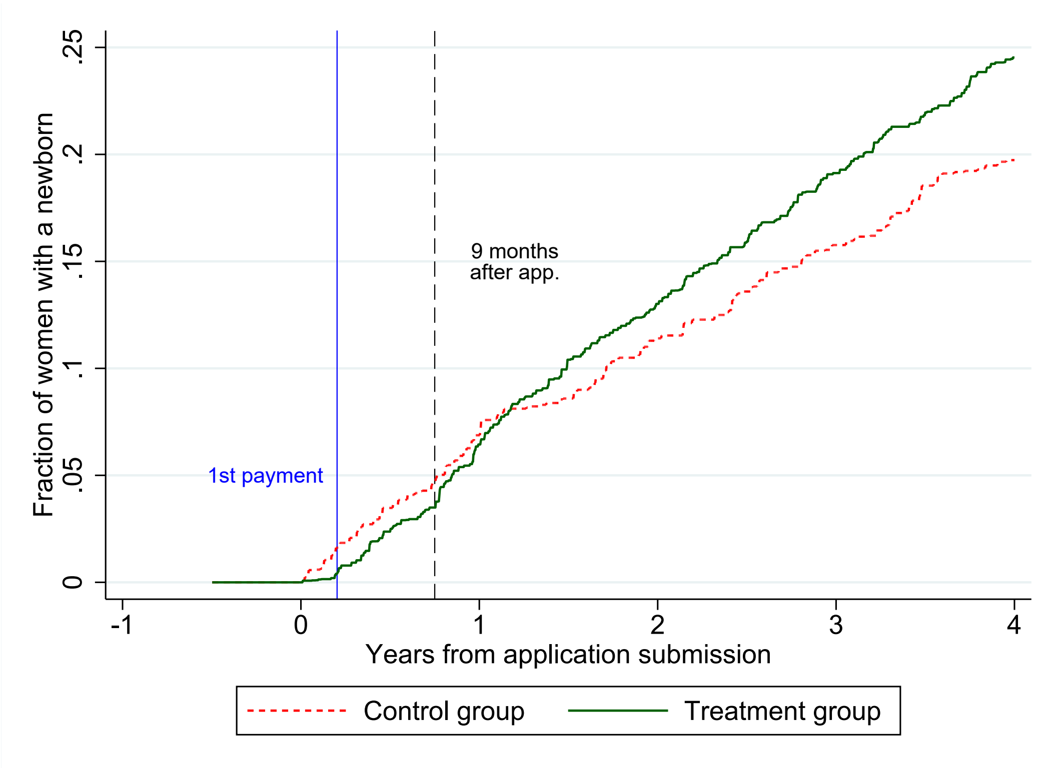
Notes: This graph shows the point estimate and the standard error of the ITT effect of the basic pension on having a newborn child in the period from 9 months to 4 years after application for applicants' female household members of fertility age, using different bandwidths and all controls specified in regression Equation (3.1). The x-axis labels report the number of score points on each side of the bandwidth. CCT is the optimal bandwidth using the approach proposed by Calonico, Cattaneo, and Titiunik (2014).

FIGURE C.7: Reduced-form effect of being below the cut-off on fertility: placebo estimates



Notes: This graph shows the cumulative distribution of reduced-form estimates on fertility, from placebo regressions in which the cut-off is set in different parts of the pension score distribution. Estimates are computed using regression Equation (3.1). Cut-offs are located every 25 score points, ranging from 456 (Calonico, Cattaneo and Titiunik's (2014) optimal bandwidth on fertility) to 1606, to ensure that we have observations in all points of the bandwidth. The lowest pension score is zero and the cut-off is set at 1206 pension score points. Then, placebo cut-offs are set between -750 and 400 pension score points from the cut-off. The solid line displays the empirical cumulative distribution of estimates, while the dashed line displays fitted values of the cumulative distribution. The vertical line shows the coefficient estimated with our optimal bandwidth baseline specification.

FIGURE C.8: Share of women of fertility age having a newborn between six months before applying and four years from date of application, adjusted by the deviation of the pension score from the cut-off.



Notes: This figure presents the share of women of fertility age that have a newborn in the treatment and control groups at each point in time following the first application. Shares are equal to $1-\hat{S}(t)$, with $\hat{S}(t)$ being the $k_0(t)$ term in the Cox proportional hazard model: $k(t) = k_0(t)\exp(\beta_1 Score_h)$, with *t* being the time following the first application. Shares are estimated separately for the treatment and control groups in the 500 score-point bandwidth and using triangular weights.

C.6.3 Discussion on the spillover effect on fertility

Following most of the literature, we estimate the income-fertility elasticity by dividing the ITT percentage change in newborns for women of fertility age by the ITT percentage income change for the recipients of income. In our case, the recipients of income are the applicants, and this calculation yields an income-fertility elasticity of 0.7. Alternatively, if we use the mother's income rather than recipient's income, the income-fertility elasticity is 0.76.[7] Figure C.9 shows that previous causal estimates of income-fertility elasticity are

[7]The probability of having a newborn increases by 51% (0.067/0.130) for women of fertility age living with a pension recipient at the cut-off. As the basic pension increases recipients' income by 72.4 percent, the recipient's income-fertility elasticity is 0.7. For the estimate of mothers' income-fertility elasticity, we assumed perfect income pooling. In households with a woman of fertility age, the pension increases average

also positive, which is in line with the predictions of Becker's (1960) neoclassical model of fertility.[8] Our estimate is roughly in the middle of the range, but there is a considerable dispersion of fertility-income elasticities across studies.

---

monthly income per-capita by USD 26 over the four years following the first application, from an average monthly income of USD 34 for control group applicants. This leads to a mother income-fertility elasticity of 0.76. As before, these estimates take into account the full trajectory of income and are done using only first applicants from 2012.

[8]Children are generally considered 'normal goods' and their 'consumption' should increase with income. Our results, along with other recent empirical studies presented in Figure C.9, help to explain the long-term puzzle of the negative cross-sectional correlation between income and fertility that is present in many parts of the world (see Jones and Tertilt (2008)).

FIGURE C.9: Estimated income-fertility elasticity across different empirical studies

Notes: This graph plots point estimates and confidence intervals of income-fertility elasticity in different empirical studies. Empty squares indicate insignificant estimates. The dashed lines indicate the 95% confidence intervals of our estimates. The elasticities in the other papers are computed using income shocks on different household members: (Black et al., 2013) and (Lindo, 2010) estimate income-fertility elasticity using husband's income; (Kearney and Wilson, 2018) and (Huttunen and Kellokumpu, 2016) estimate mother's income-fertility elasticity and husband's income-fertility elasticity; and (Lovenheim and Mumford, 2013) estimate a fertility elasticity with respect to the house price. In several studies, it is not possible to calculate the income-fertility elasticity, because either baseline fertility or income are not reported. The confidence interval for (Black et al., 2013) is unavailable as the standard errors are not reported.

One explanation for the diverse pattern of estimates is that the nature of the income shock is very diverse across studies: mother's or father's job displacements in Lindo (2010) and Huttunen and Kellokumpu (2016); boosts in house prices in Lovenheim and Mumford (2013); economic booms in Black et al. (2013) and Kearney and Wilson (2018); and the basic pension for elderly relatives in our case. Different shocks may also induce different impacts on household dynamics. For instance, job displacements might affect the probability of divorce and change women's career choices, while house price increases might be perceived as transitory income shocks with weaker effects on couples' decision to have a child, which is a permanent decision. Additionally, these studies are conducted in different countries, with different public provision of childcare, which could affect the relative 'price' of childbearing. For instance, Huttunen and Kellokumpu (2016) focuses on Finland which has a relatively generous welfare state compared to Chile and the US, the countries studied in our paper and the papers by Lindo (2010), Black et al. (2013), Lovenheim and Mumford (2013), and Kearney and Wilson (2018).

C.7 Additional tables

TABLE C.3: Characteristics of applicants, and their household members, at the moment of application and within 500 score points around the threshold

	Applicants (1)	Working-age household members (2)	Elderly household members (3)
Female	0.871	0.363	0.12
Age (years)	66.851	40.364	71.074
Social security score	9385.748	9576.395	9835.929
Household size	2.643	3.685	2.749
Working-age household member	0.571	1	0.434
Elderly household member	0.661	0.47	1
Child under 16	0.009	0.018	0.009
Days hospitalized	0.461	0.247	0.466
Influenza vaccination	0.32	0.089	0.347
Pneumonia vaccination	0.061	0.002	0.028
Urban town	0.762	0.737	0.77
Metropolitan region	0.373	0.348	0.368
Received a basic pension	0.799		
Observations	8,499	8,047	5,722

Notes: This table reports the mean of several covariates for applicants whose application score is within 500 score points from the cut-off and their household members. Column (1) reports means for applicants, Column (2) reports means for working-age household members, and Column (3) reports means for elderly household members. *Health covariates* are computed for the 6 months before applicants submit their first application.

TABLE C.4: Balancing tests on other covariates (2012 only)

Variables	ITT Coef. (1)	S.E. (2)	t stat (3)	P-value (4)	BW (5)	N (6)	Control (7)
Panel A: household measures							
Total household income	0.833	(10.163)	0.082	0.935	500	4,066	649.7
Imputed income	-25.000	(12.083)	-2.069	0.044	500	4,066	93.40
Labor income	27.940	(36.573)	0.764	0.449	500	4,066	246.5
All incomes from assets	-27.11	(36.282)	-0.747	0.459	500	4,066	403.1
Labor income factor	-0.013	(0.024)	-0.562	0.577	500	4,066	1.939
Needs index (IN)	-0.032	(0.021)	-1.539	0.130	500	4,066	2.021
Net working salary	-4.596	(19.870)	-0.231	0.818	500	4,066	187.8
Other labor income	36.160	(30.979)	1.167	0.249	500	4,066	20.10
Net pension income	5.339	(18.848)	0.283	0.778	500	4,066	357.0
Avg. no. of students	-0.021	(0.016)	-1.258	0.214	500	4,066	0.070
Panel B: income of household members							
Applicants' income	-1.464	(11.615)	-0.126	0.900	500	4,066	89.37
Elderly relatives' inc.	-17.44	(21.819)	-0.799	0.428	500	2,769	525.2
Work.-age relatives' inc.	-4.775	(31.926)	-0.150	0.882	500	2,309	290.0
Fert. age woman's inc.	0.956	(12.432)	0.0770	0.939	500	828	20.90

Notes: This table reports results from OLS regressions of pre-determined variables on a treatment dummy indicator and deviation of the pension score from the cut-off. All estimations are computed using averages at household level due to data limitations. Columns (1), (2), (3), and (4) report the treatment indicator coefficient, its standard error clustered at the province, t-statistic, and p-value, respectively. Columns (5) and (6) report the range of pension score points from the cut-off and the number of observations in the regression, respectively. Column (7) reports the variable mean for control applicants at the cut-off. All income variables are expressed in 2012 US dollars.

TABLE C.5: Applicant's health outcomes over four years from application by gender

Variables	TOT (1)	S.E. TOT (2)	ITT (3)	S.E. ITT (4)	P-value (5)	BW (6)	N (7)	Control (8)
Panel A: female applicants								
Mortality rate	-0.028	(0.011)	-0.022	(0.008)	0.013	500	7,403	0.063
% days hospitalized	-0.034	(0.062)	-0.005	(0.048)	0.908	500	7,403	0.263
Medical episode	-0.068	(0.030)	-0.047	(0.021)	0.026	500	7,403	0.328
Panel B: male applicants								
Mortality rate	0.010	(0.052)	0.014	(0.037)	0.710	500	1,096	0.129
% days hospitalized	-0.144	(0.258)	-0.019	(0.138)	0.890	500	1,096	0.363
Medical episode	0.005	(0.117)	0.034	(0.079)	0.669	500	1,096	0.382

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (3.1). Column (1) reports the *treatment on the treated* coefficient as in Equation (3.4) and Column (2) reports its standard error computed using the delta method. Column (3) reports the *intent-to-treat* coefficient and Column (4) reports its standard error clustered at the province level. Column (5) reports the p-value of the ITT coefficient reported in Column (3). Column (6) reports the range of pension score points from the cut-off and Column (7) reports the number of observations in the regression. Column (8) reports the constant of the ITT regression, showing the variable mean for control applicants at the cut-off.

TABLE C.6: Balancing tests by household structure

Variables	ITT Coef. (1)	S.E. (2)	t stat (3)	P-value (4)	BW (5)	N (6)	Control (7)
Panel A: applicants not living with a working-age relatives							
Female	-0.014	(0.020)	-0.693	0.491	500	3,647	0.871
Age (years)	-0.680	(0.457)	-1.488	0.143	500	3,647	69.00
% days hospitalized	-0.270	(0.116)	-2.339	0.023	500	3,647	0.336
Influenza vaccination	-0.011	(0.028)	-0.387	0.701	500	3,647	0.360
Pneumonia vaccination	0.025	(0.016)	1.513	0.137	500	3,647	0.033
Household size	-0.016	(0.020)	-0.840	0.405	500	3,647	1.915
Social security score	-48.82	(207.017)	-0.236	0.815	500	3,647	9640.
Elderly relative	-0.022	(0.019)	-1.180	0.244	500	3,647	0.892
Child under 16	-0.004	(0.004)	-1.036	0.305	500	3,647	0.004
Municipal income	5.761	(5.048)	1.141	0.259	500	3,640	141.8
Panel B: applicants living with working-age relatives							
Female	-0.017	(0.021)	-0.780	0.439	500	4,852	0.906
Age (years)	-0.116	(0.314)	-0.369	0.713	500	4,852	66.38
% days hospitalized	0.048	(0.099)	0.488	0.628	500	4,852	0.174
Influenza vaccination	-0.036	(0.027)	-1.342	0.186	500	4,852	0.355
Pneumonia vaccination	0.010	(0.014)	0.681	0.499	500	4,852	0.052
Household size	0.008	(0.060)	0.136	0.892	500	4,852	3.227
Social security score	167.3	(255.827)	0.654	0.516	500	4,852	9823.
Elderly relative	0.043	(0.026)	1.646	0.106	500	4,852	0.528
Child under 16	0.007	(0.006)	1.045	0.301	500	4,852	0.007
Municipal income	-9.301	(5.746)	-1.619	0.112	500	4,843	151.0

Notes: This table reports results from OLS regressions of pre-determined variables on a treatment dummy indicator and deviation of the pension score from the cut-off. Columns (1), (2), (3), and (4) report the treatment indicator coefficient, its standard error clustered at the province level, t-statistic, and p-value, respectively. Columns (5) and (6) report the range of pension score points from the cut-off and the number of observations in the regression, respectively. Column (7) reports the variable mean for control applicants at the cut-off. Health covariates are computed for the 6 months before applying.

TABLE C.7: Health outcomes, over four years from application, for applicants not living with working-age household members using logit, non-parametric estimations, optimal bandwidth, controls, and quadratic functional form in $Score_h$

Variables	Regression (1)	ITT Coef. (2)	S.E. ITT (3)	P-value (4)	BW (5)	N (6)
Mortality rate	No controls	-0.045	(0.016)	0.008	500	3,647
Mortality rate	Controls	-0.040	(0.015)	0.010	500	3,647
Mortality rate	Logit	-0.047	(0.015)	0.001	500	3,647
Mortality rate	Non-parametric	-0.045	(0.019)	0.021	500	3,647
Mortality rate	Optimal bandwidth	-0.050	(0.019)	0.010	374	2,704
Mortality rate	Quadratic	-0.065	(0.025)	0.013	500	3,647
Medical episode	No controls	-0.093	(0.036)	0.012	500	3,647
Medical episode	Controls	-0.086	(0.040)	0.036	500	3,647
Medical episode	Logit	-0.090	(0.037)	0.017	500	3,647
Medical episode	Non-parametric	-0.093	(0.034)	0.007	500	3,647
Medical episode	Optimal bandwidth	-0.116	(0.058)	0.053	294	2,124
Medical episode	Quadratic	-0.128	(0.066)	0.058	500	3,647

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (3.1). Column (1) indicates the specification used. *No controls* reports estimates of a regression of the outcome variable on the treatment dummy indicator and deviation of the pension score from the cut-off, without further controls. *Controls* employs our preferred specification, polynomial of order 1 in $Score_h$, with the addition of the 17 other controls listed in Appendix Section C.4. *Logit* reports estimations using a logistic regression. *Non-parametric* reports non-parametric estimations using kernel local linear regressions. *Optimal bandwidth* estimates treatment effects using the optimal bandwidth proposed by Calonico, Cattaneo, and Titiunik (2014). *Quadratic* uses polynomial of order 2 in $Score_h$. Column (2) reports the treatment indicator coefficient and Column (3) reports the standard error clustered at the province level. Column (4) reports the p-value of the treatment coefficient. Column (5) indicates the range of pension score points from the cut-off and Column (6) reports the number of observations in the regression.

TABLE C.8: Applicants' health outcomes, over four years from application, for applicants living with working-age household members using logit, non-parametric estimations, optimal bandwidth, controls, and quadratic functional form in $Score_h$

Variables	Regression (1)	ITT Coef. (2)	S.E. ITT (3)	P-value (4)	BW (5)	N (6)
Mortality rate	No controls	-0.001	(0.014)	0.954	500	4,852
Mortality rate	Controls	-0.004	(0.010)	0.679	500	4,852
Mortality rate	Logit	-0.002	(0.010)	0.810	500	4,852
Mortality rate	Non-parametric	-0.001	(0.013)	0.949	500	4,852
Mortality rate	Optimal bandwidth	-0.012	(0.012)	0.317	364	3,382
Mortality rate	Quadratic	-0.017	(0.017)	0.312	500	4,852
Medical episode	No controls	-0.000	(0.032)	0.998	500	4,852
Medical episode	Controls	0.001	(0.038)	0.985	500	4,852
Medical episode	Logit	0.000	(0.036)	0.994	500	4,852
Medical episode	Non-parametric	0.000	(0.035)	0.990	500	4,852
Medical episode	Optimal bandwidth	-0.000	(0.035)	0.997	506	4,924
Medical episode	Quadratic	0.008	(0.053)	0.874	500	4,852

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (3.1). Column (1) indicates the specification used. *No controls* reports estimates of a regression of the outcome variable on the treatment dummy indicator and deviation of the pension score from the cut-off, without further controls. *Controls* employs our preferred specification, polynomial of order 1 in $Score_h$, with the addition of the 17 other controls listed in Appendix Section C.4. *Logit* reports estimations using a logistic regression. *Non-parametric* reports non-parametric estimations using kernel local linear regressions. *Optimal bandwidth* estimates treatment effects using the optimal bandwidth proposed by Calonico, Cattaneo, and Titiunik (2014). *Quadratic* uses polynomial of order 2 in $Score_h$. Column (2) reports the treatment indicator coefficient and Column (3) reports the standard error clustered at the province level. Column (4) reports the p-value of the treatment coefficient. Column (5) indicates the range of pension score points from the cut-off and Column (6) reports the number of observations in the regression.

TABLE C.9: Medical episodes by cause over four years from application

Variables	TOT (1)	S.E. TOT (2)	ITT (3)	S.E. (4)	P-value (5)	BW (6)	N (7)	Control (8)
Panel A: applicants								
Circulatory	0.013	(0.016)	0.011	(0.012)	0.376	500	8,499	0.076
Respiratory	-0.030	(0.011)	-0.019	(0.008)	0.019	500	8,499	0.044
Tumour	-0.028	(0.015)	-0.021	(0.011)	0.067	500	8,499	0.054
Digestive or nutritional	-0.025	(0.016)	-0.020	(0.012)	0.097	500	8,499	0.098
Accidents	-0.002	(0.003)	-0.001	(0.002)	0.548	500	8,499	0.002
Panel B: applicants not living with a working-age household member								
Circulatory	-0.017	(0.026)	-0.011	(0.019)	0.544	500	3,647	0.099
Respiratory	-0.045	(0.012)	-0.031	(0.009)	0.001	500	3,647	0.050
Tumour	-0.048	(0.018)	-0.036	(0.014)	0.014	500	3,647	0.058
Digestive or nutritional	-0.009	(0.033)	-0.008	(0.026)	0.756	500	3,647	0.091
Accidents	0.002	(0.004)	0.001	(0.003)	0.600	500	3,647	0.001

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (3.1). Column (1) reports the *treatment on the treated* coefficient as in Equation (3.4) and Column (2) reports its standard error computed using the delta method. Column (3) reports the *intent-to-treat* coefficient and Column (4) reports its standard error clustered at the province level. Column (5) reports the p-value of the ITT coefficient reported in Column (3). Column (6) reports the range of pension score points from the cut-off and Column (7) reports the number of observations in the regression. Column (8) reports the constant of the ITT regression, showing the variable mean for control applicants at the cut-off.

TABLE C.10: Vaccinations received in the four years after applying for applicants and applicants by household structure

Variables	TOT (1)	S.E. TOT (2)	ITT (3)	S.E. ITT (4)	P-value (5)	BW (6)	N (7)	Control (8)
Panel A: applicants								
Influenza vaccine	0.010	(0.037)	-0.001	(0.025)	0.960	500	8,499	0.679
Pneumonia vaccine	0.027	(0.034)	0.009	(0.024)	0.721	500	8,499	0.306
Panel B: applicants not living with working-age household members								
Influenza vaccine	-0.001	(0.043)	-0.005	(0.031)	0.870	500	3,647	0.687
Pneumonia vaccine	0.008	(0.034)	-0.005	(0.025)	0.848	500	3,647	0.301
Panel C: applicants living with a working-age household members								
Influenza vaccine	0.012	(0.040)	-0.003	(0.026)	0.909	500	4,852	0.673
Pneumonia vaccine	0.040	(0.043)	0.019	(0.029)	0.510	500	4,852	0.311

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (3.1). Column (1) reports the *treatment on the treated* coefficient as in Equation (3.4) and Column (2) reports its standard error computed using the delta method. Column (3) reports the *intent-to-treat* coefficient and Column (4) reports its standard error clustered at the province level. Column (5) reports the p-value of the ITT coefficient reported in Column (3). Column (6) reports the range of pension score points from the cut-off and Column (7) reports the number of observations in the regression. Column (8) reports the constant of the ITT regression, showing the variable mean for control applicants at the cut-off.

TABLE C.11: Characteristics of basic pension applicants when aged between 60 and 64

Variables	Recipients (1)	Non-recipients (2)	Difference (3)	P-value (4)
Panel A: individual level variables				
Private health insurance	0.017	0.018	-0.001	0.991
Informal work	0.156	0.228	-0.072	0.252
Visited a GP	0.589	0.655	-0.066	0.331
Visited a health center	0.769	0.793	-0.024	0.695
Visits to health center	11.097	8.862	2.235	0.174
Bad Health	0.220	0.276	-0.056	0.432
Smoked, last month	0.163	0.163	0.000	0.998
Number of cigarettes, last month	32.413	54.102	-21.689	0.437
Drunk alcohol, last month	0.106	0.265	-0.159	0.026
Number of drinks, last month	0.884	1.673	-0.790	0.077
Panel B: household income and expenditure in 2012 US dollars				
Monthly income	475.663	552.012	-76.349	0.380
Total expenditure	356.933	446.101	-89.168	0.075
Food	192.412	227.491	-35.079	0.212
Clothes	17.713	19.192	-1.479	0.742
Utilities	90.335	128.805	-38.47	0.086
Transport	30.082	40.699	-10.617	0.226
Domestic services	0.686	2.182	-1.496	0.354
Drugs	26.804	23.549	3.255	0.643
Children's education	10.445	4.995	5.451	0.119

Notes: This table reports the mean of the listed covariates for basic pension applicants at age 60-64. Column (1) reports means for applicants who eventually obtained the pension. Column (2) reports means for applicants who did not obtain the pension. Column (3) reports the difference between columns (1) and (2). Column (4) reports the p-value of a test of means differences between column (1) and (2). 'Visited a health center' is a dummy variable for whether the individual had at least one appointment at a health center in the last two years. Income and expenditure variables are reported in 2012 US dollars. 'Total expenditure' refers to the sum of the expenditures reported in the table. Data is from the panel survey conducted in 2004, 2006, 2009, 2012, and 2015 by the Ministry of Labor.

TABLE C.12: Characteristics of Chileans who are aged 65 or over and do not have a contributory pension

	All (1)	Basic pension recipients (2)	Basic pension non-recipients (3)
Female	0.720	0.721	0.718
	(0.449)	(0.448)	(0.450)
Age	73.55	73.94	72.83
	(6.706)	(6.614)	(6.811)
Household size	2.358	2.345	2.383
	(1.099)	(1.114)	(1.070)
Elderly household member	0.579	0.580	0.579
	(0.494)	(0.494)	(0.494)
Working-age household member	0.461	0.436	0.507
	(0.499)	(0.496)	(0.500)
Child household member	0.0755	0.0772	0.0723
	(0.264)	(0.267)	(0.259)
Metropolitan area	0.307	0.295	0.327
	(0.461)	(0.456)	(0.469)
Urban town	0.770	0.722	0.855
	(0.421)	(0.448)	(0.352)
Employed	0.0263	0.0156	0.0457
	(0.160)	(0.124)	(0.209)
Food from health service	0.380	0.434	0.285
	(0.486)	(0.496)	(0.451)
Public health insurance	0.946	0.977	0.892
	(0.225)	(0.151)	(0.311)
Received a basic pension	0.643	1	0
	(0.479)	(0)	(0)

Notes: Using data from the 2011 Chilean household survey (Ministerio de Desarrollo Social, 2011), this table reports the means and standard deviations (in parentheses) of several covariates for the Chilean population without a contributory pension in 2011. Column (1) reports statistics for the whole population, Column (2) reports statistics for elderly people with a basic pension and Column (3) reports statistics for elderly people without a basic pension.

TABLE C.13: Applicants' health outcomes in four years from the first application using logit, non-parametric estimations, optimal bandwidth, controls, and quadratic functional form in $Score_h$

Variables	Regression (1)	ITT Coef. (2)	S.E. ITT (3)	P-value (4)	BW (5)	N (6)
Mortality rate	No controls	-0.021	(0.010)	0.034	500	8,499
Mortality rate	Controls	-0.019	(0.010)	0.058	500	8,499
Mortality rate	Logit	-0.018	(0.009)	0.055	500	8,499
Mortality rate	Non-parametric	-0.021	(0.010)	0.045	500	8,499
Mortality rate	Optimal bandwidth	-0.028	(0.012)	0.029	306	5,048
Mortality rate	Quadratic	-0.035	(0.015)	0.021	500	8,499
Medical episode	No controls	-0.042	(0.018)	0.024	500	8,499
Medical episode	Controls	-0.037	(0.016)	0.029	500	8,499
Medical episode	Logit	-0.038	(0.016)	0.020	500	8,499
Medical episode	Non-parametric	-0.042	(0.023)	0.071	500	8,499
Medical episode	Optimal bandwidth	-0.043	(0.020)	0.033	398	6,605
Medical episode	Quadratic	-0.050	(0.027)	0.077	500	8,499

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (3.1). Column (1) indicates the specification used. *No controls* reports estimates of a regression of the outcome variable on the treatment dummy indicator and deviation of the pension score from the cut-off, without further controls. *Controls* employs our preferred specification, polynomial of order 1 in $Score_h$, with the addition of the 17 other controls listed in Appendix Section C.4. *Logit* reports estimations using a logistic regression. *Non-parametric* reports non-parametric estimations using kernel local linear regressions. *Optimal bandwidth* estimates treatment effects using the optimal bandwidth proposed by Calonico, Cattaneo, and Titiunik (2014). *Quadratic* uses polynomial of order 2 in $Score_h$. Column (2) reports the treatment indicator coefficient and Column (3) reports the standard error clustered at the province level. Column (4) reports the p-value of the treatment coefficient. Column (5) indicates the range of pension score points from the cut-off and Column (6) reports the number of observations in the regression.

TABLE C.14: Balancing tests for working-age and elderly relatives

Variables	ITT Coef. (1)	S.E. (2)	t-stat (3)	P-value (4)	BW (5)	N (6)	Control (7)
Panel A: working-age relatives							
Female	0.030	(0.024)	1.240	0.221	500	8,047	0.358
Age (years)	-1.090	(0.656)	-1.661	0.103	500	8,047	40.96
% days hospitalized	-0.026	(0.033)	-0.794	0.431	500	8,047	0.094
Influenza vaccination	-0.015	(0.012)	-1.204	0.235	500	8,047	0.094
Pneumonia vaccination	-0.001	(0.003)	-0.271	0.788	500	8,047	0.004
Newborn child	0.007	(0.005)	1.514	0.137	500	8,047	0.006
Household size	0.007	(0.060)	0.121	0.904	500	4,836	3.228
Social security score	147.319	(261.230)	0.564	0.575	500	4,836	9857
Elderly relative	0.047	(0.026)	1.767	0.084	500	4,836	0.525
Child under 16	0.007	(0.006)	1.054	0.297	500	4,836	0.007
Municipal income	-8.321	(5.181)	-1.606	0.115	500	4,828	150.1
Panel B: elderly relatives							
Female	0.032	(0.016)	2.016	0.049	500	5,722	0.097
Age (years)	-0.608	(0.358)	-1.702	0.095	500	5,722	71.82
% days hospitalized	-0.022	(0.048)	-0.454	0.652	500	5,722	0.171
Influenza vaccination	-0.026	(0.029)	-0.899	0.373	500	5,722	0.364
Pneumonia vaccination	0.001	(0.006)	0.083	0.934	500	5,722	0.019
Household size	0.050	(0.050)	1.003	0.321	500	5,566	2.679
Social security score	96.419	(199.801)	0.483	0.632	500	5,566	1.0e+
Working-age relative	0.027	(0.024)	1.147	0.257	500	5,566	0.412
Child under 16	-0.000	(0.006)	-0.044	0.965	500	5,566	0.009
Municipal income	-2.603	(5.244)	-0.496	0.622	500	5,558	147.4

Notes: This table reports results from OLS regressions of pre-determined variables on a treatment dummy indicator and deviation of the pension score from the cut-off. Columns (1), (2), (3), and (4) report the treatment indicator coefficient, its standard error clustered at the province level, t-statistic, and p-value, respectively. Columns (5) and (6) report the range of pension score points from the cut-off and the number of observations in the regression, respectively. Column (7) reports the variable mean for control applicants at the cut-off. Health covariates are computed for the 6 months before applying.

TABLE C.15: Health outcomes of family members, by age, over four years from application using logit, non-parametric estimations, optimal bandwidth, controls, and quadratic functional form in $Score_h$

Variables	Regression (1)	ITT Coef. (2)	S.E. ITT (3)	P-value (4)	BW (5)	N (6)
Panel A: working-age household members						
% days hospitalized	No controls	0.009	(0.023)	0.685	500	8,047
% days hospitalized	Controls	0.032	(0.030)	0.291	500	8,047
% days hospitalized	Logit	0.005	(0.019)	0.788	500	8,047
% days hospitalized	Non-parametric	0.009	(0.033)	0.781	500	8,047
% days hospitalized	Optimal bandwidth	0.014	(0.030)	0.649	260	3,889
% days hospitalized	Quadratic	0.028	(0.044)	0.528	500	8,047
Newborn child	No controls	0.028	(0.007)	0.000	500	8,047
Newborn child	Controls	0.016	(0.008)	0.050	500	8,047
Newborn child	Logit	0.057	(0.026)	0.034	500	8,047
Newborn child	Controls	0.016	(0.008)	0.050	500	8,047
Newborn child	Non-parametric	0.028	(0.007)	0.000	500	8,047
Newborn child	Optimal bandwidth	0.017	(0.008)	0.043	452	7,185
Newborn child	Quadratic	0.019	(0.010)	0.059	500	8,047
Panel B: elderly household members						
Mortality rate	No controls	0.000	(0.013)	0.979	500	5,722
Mortality rate	Controls	0.012	(0.013)	0.379	500	5,722
Mortality rate	Logit	0.011	(0.012)	0.371	500	5,722
Mortality rate	Non-parametric	0.000	(0.015)	0.981	500	5,722
Mortality rate	Optimal bandwidth	0.009	(0.015)	0.547	402	4,596
Mortality rate	Quadratic	0.008	(0.020)	0.672	500	5,722
Medical episode	No controls	0.034	(0.030)	0.256	500	5,722
Medical episode	Controls	0.047	(0.033)	0.158	500	5,722
Medical episode	Logit	0.045	(0.032)	0.155	500	5,722
Medical episode	Non-parametric	0.034	(0.027)	0.208	500	5,722
Medical episode	Optimal bandwidth	0.047	(0.042)	0.268	407	4,657
Medical episode	Quadratic	0.062	(0.062)	0.320	500	5,722

Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (3.1). Column (1) indicates the specification used. *No controls* reports estimates of a regression of the outcome variable on the treatment dummy indicator and deviation of the pension score from the cut-off, without further controls. *Controls* employs our preferred specification, polynomial of order 1 in $Score_h$, with the addition of the 17 other controls listed in Appendix Section C.4. *Logit* reports estimations using a logistic regression. *Non-parametric* reports non-parametric estimations using kernel local linear regressions. *Optimal bandwidth* estimates treatment effects using the optimal bandwidth proposed by Calonico, Cattaneo, and Titiunik (2014). *Quadratic* uses polynomial of order 2 in $Score_h$. Column (2) reports the treatment indicator coefficient and Column (3) reports the standard error clustered at the province level. Column (4) reports the p-value of the treatment coefficient. Column (5) indicates the range of pension score points from the cut-off and Column (6) reports the number of observations in the regression.

TABLE C.16: Balancing tests for fertility-age female relatives

Variables	ITT Coef. (1)	S.E. (2)	t-stat (3)	P-value (4)	BW (5)	N (6)	Control (7)
Age (years)	-0.446	(0.466)	-0.958	0.343	500	2,058	29.58
% days hospitalized	0.000	(0.051)	0.006	0.995	500	2,058	0.103
Influenza vaccination	-0.013	(0.025)	-0.507	0.615	500	2,058	0.101
Newborn child	0.018	(0.018)	1.017	0.315	500	2,058	0.026
Household size	0.103	(0.175)	0.588	0.560	500	2,058	3.883
Social security score	396.901	(257.480)	1.541	0.130	500	2,058	9272.
Elderly relative	0.004	(0.057)	0.073	0.942	500	2,058	0.661
Child under 16	0.011	(0.016)	0.719	0.476	500	2,058	0.015
Municipal income	-17.838	(11.340)	-1.573	0.123	500	2,057	154.4

Notes: This table reports results from OLS regressions of pre-determined variables on a treatment dummy indicator and deviation of the pension score from the cut-off. Columns (1), (2), (3), and (4) report the treatment indicator coefficient, its standard error clustered at the province level, t-statistic, and p-value, respectively. Columns (5) and (6) report the range of pension score points from the cut-off and the number of observations in the regression, respectively. Column (7) reports the variable mean for control applicants at the cut-off. Health covariates are computed for the 6 months before applying.

TABLE C.17: Fertility rate of fertility-age female family members 9 months or later after application using non-parametric estimations, different controls, optimal bandwidth and quadratic functional form in $Score_h$

Variables	Regression (1)	ITT Coef. (2)	S.E. ITT (3)	P-value (4)	BW (5)	N (6)
Newborn child	No controls	0.091	(0.028)	0.002	500	2,058
Newborn child	Controls	0.052	(0.027)	0.062	500	2,058
Newborn child	Logit	0.068	(0.029)	0.020	500	2,058
Newborn child	Non-parametric	0.091	(0.029)	0.002	500	2,058
Newborn child	Optimal bandwidth	0.068	(0.030)	0.029	456	1,869
Newborn child	Quadratic	0.080	(0.034)	0.025	500	2,058

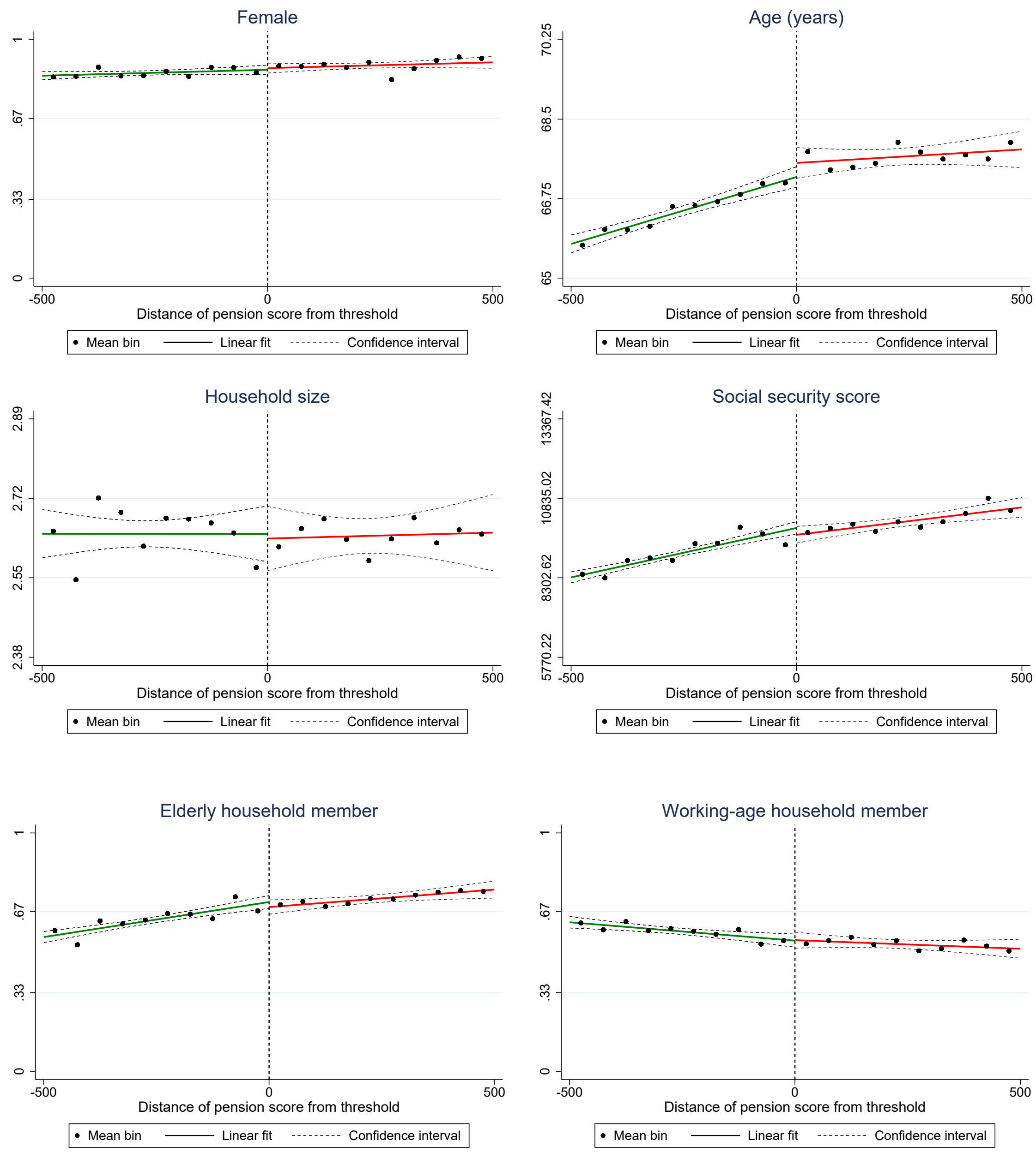
Notes: This table reports results, within four years from the date of the first application, from regressions of several dependent variables on a treatment dummy indicator, deviation of the pension score from the cut-off, and the control variables specified in Equation (3.1). Column (1) indicates the specification used. *No controls* reports estimates of a regression of the outcome variable on the treatment dummy indicator and deviation of the pension score from the cut-off, without further controls. *Controls* employs our preferred specification, polynomial of order 1 in $Score_h$, with the addition of the 17 other controls listed in Appendix Section C.4. *Logit* reports estimations using a logistic regression. *Non-parametric* reports non-parametric estimations using kernel local linear regressions. *Optimal bandwidth* estimates treatment effects using the optimal bandwidth proposed by Calonico, Cattaneo, and Titiunik (2014). *Quadratic* uses polynomial of order 2 in $Score_h$. Column (2) reports the treatment indicator coefficient and Column (3) reports the standard error clustered at the province level. Column (4) reports the p-value of the treatment coefficient. Column (5) indicates the range of pension score points from the cut-off and Column (6) reports the number of observations in the regression.

C.8 Additional figures

FIGURE C.10: McCrary test of applicants

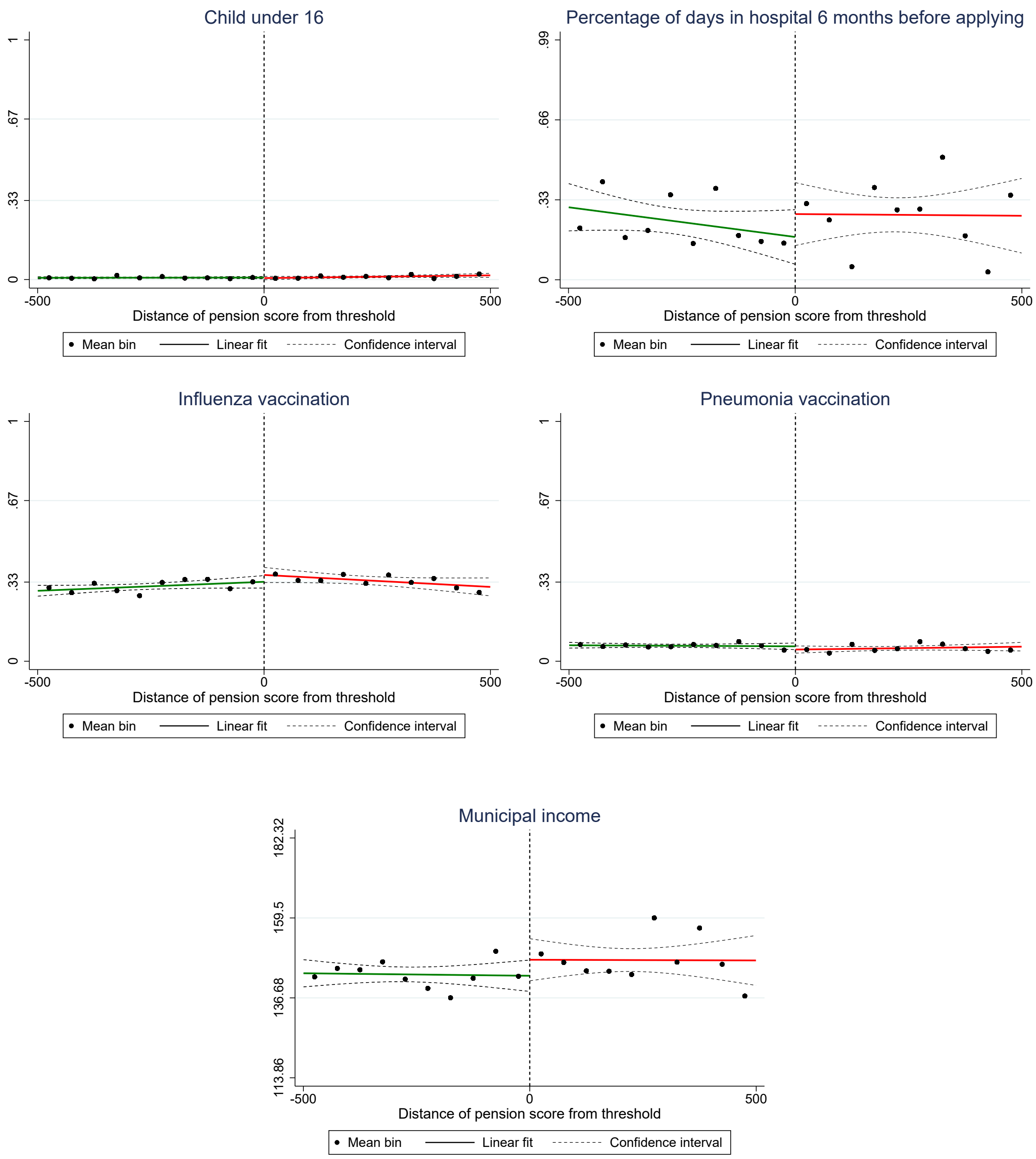
Notes: This figure shows the density of applicants in 10 score-point bins. The solid line plots fitted values from a local linear regression of density on pension score deviations from the cut-off, separately estimated on both sides of the cut-off. The thin lines represent the 95% confidence interval.

FIGURE C.11: Pre-determined covariates. RD plots, applicants



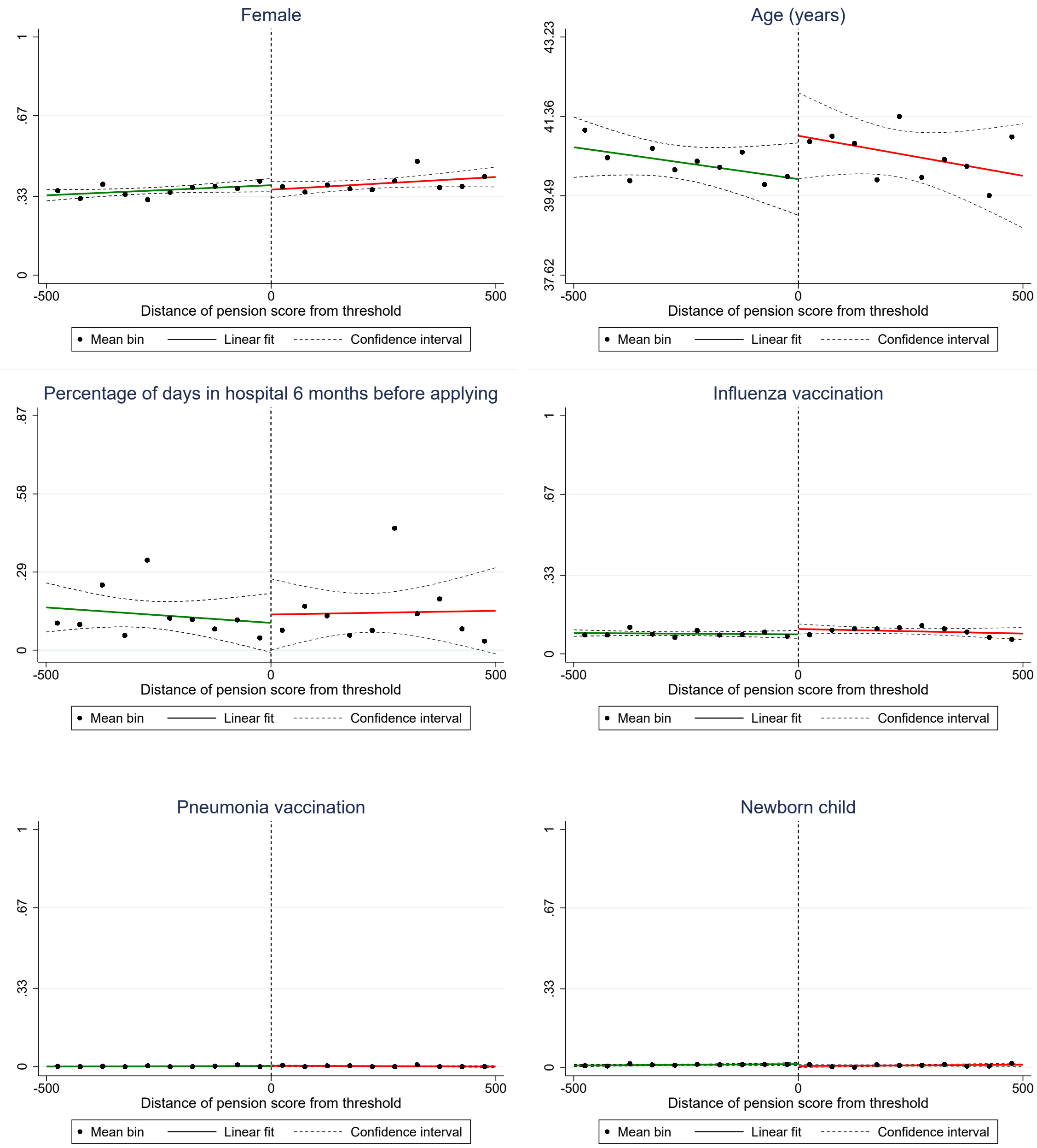
Notes: Each graph shows the average value of the corresponding covariate conditional on the distance of the score from the cut-off. The circles are averages across 50-point bins on either side of the threshold, while the solid and dashed lines represent the predicted values and associated confidence interval, respectively.

FIGURE C.12: Pre-determined covariates. RD plots, applicants



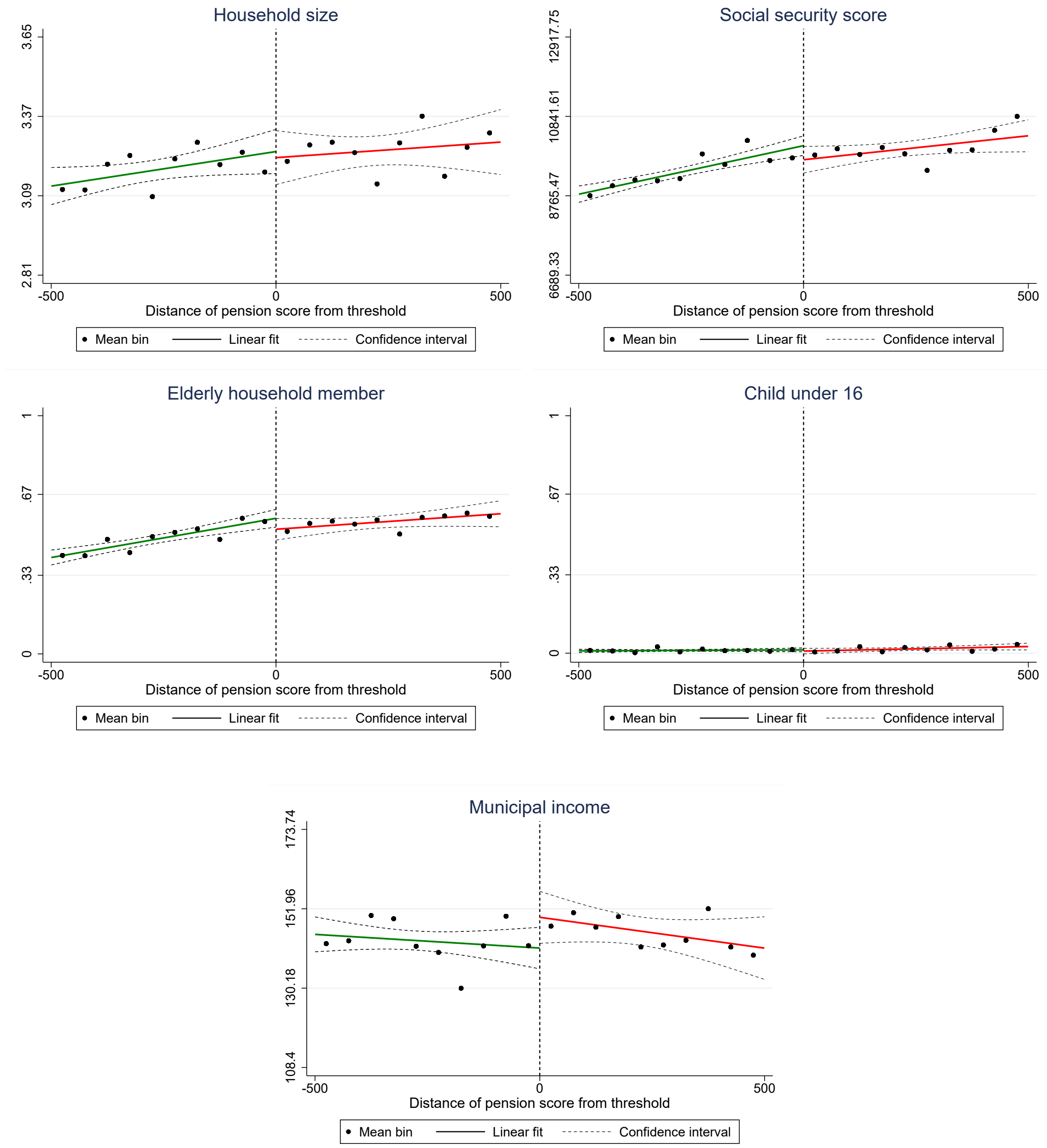
Notes: Each graph shows the average value of the corresponding covariate conditional on the distance of the score from the cut-off. The circles are averages across 50-point bins on either side of the threshold, while the solid and dashed lines represent the predicted values and associated confidence interval, respectively.

FIGURE C.13: Pre-determined covariates. RD plots, working-age household members



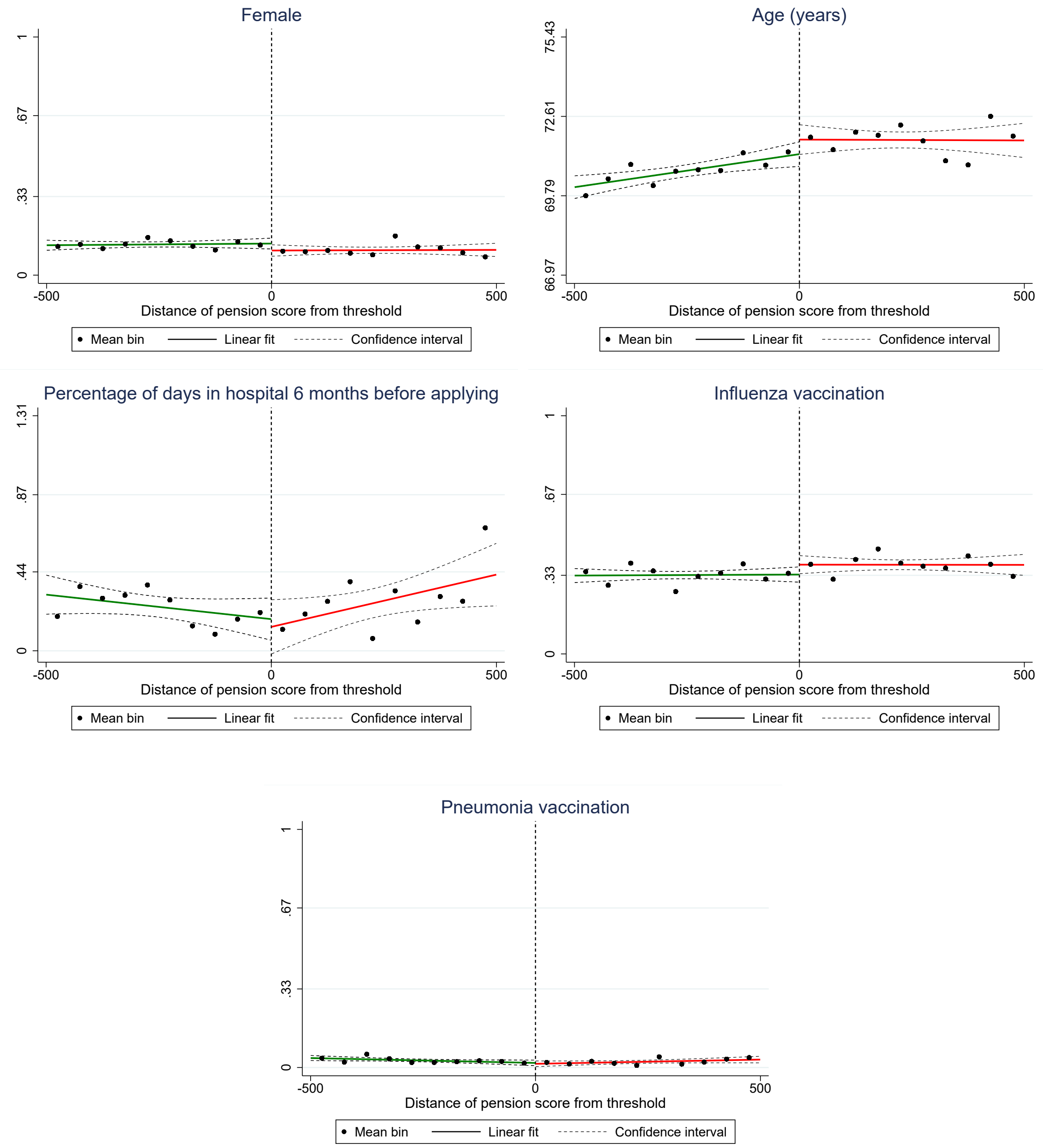
Notes: Each graph shows the average value of the corresponding covariate conditional on the distance of the score from the cut-off. The circles are averages across 50-point bins on either side of the threshold, while the solid and dashed lines represent the predicted values and confidence interval, respectively.

FIGURE C.14: Pre-determined covariates. RD plots, working-age household members



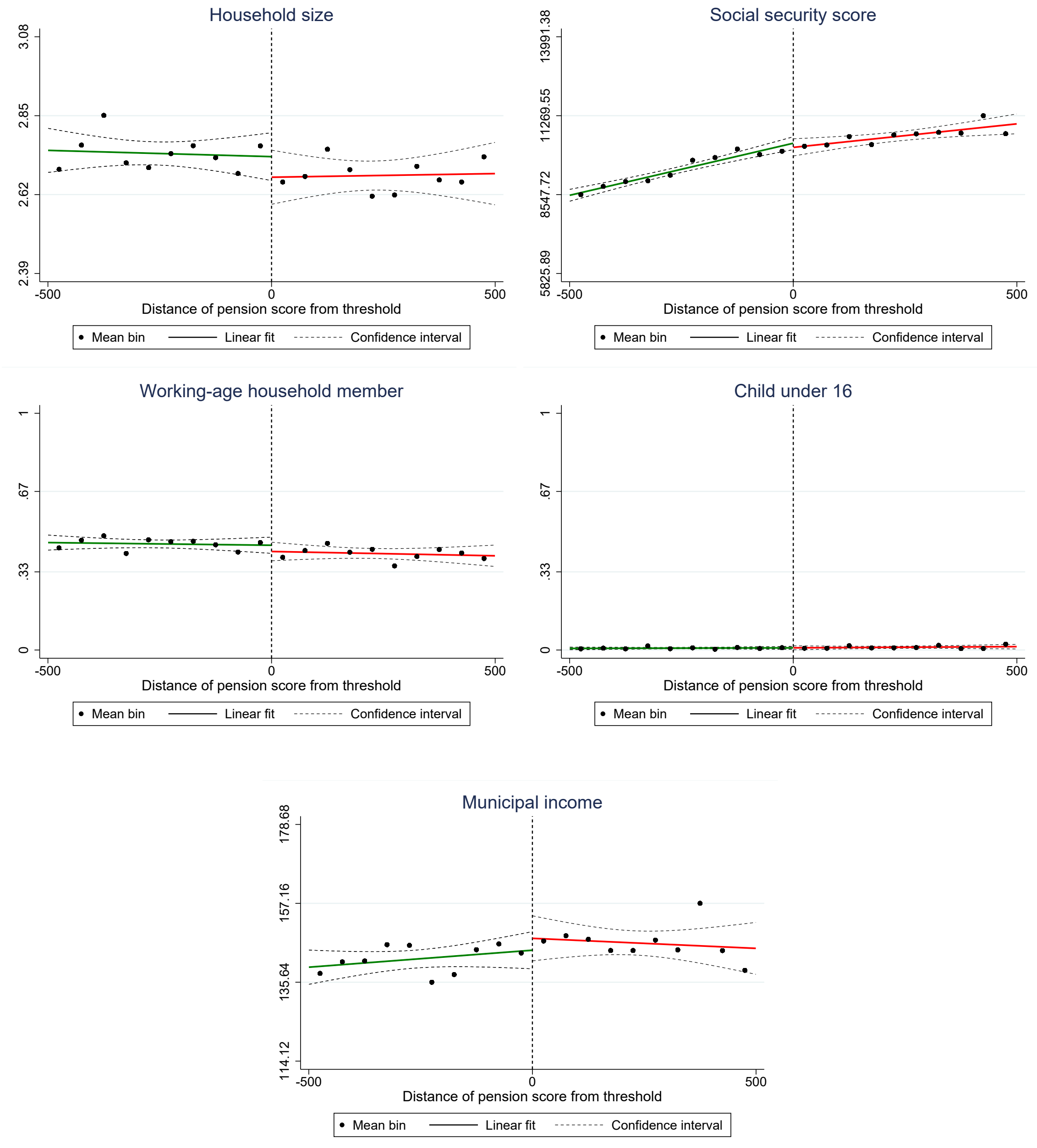
Notes: Each graph shows the average value of the corresponding covariate conditional on the distance of the score from the cut-off. The circles are averages across 50-point bins on either side of the threshold, while the solid and dashed lines represent the predicted values and confidence interval, respectively.

FIGURE C.15: Pre-determined covariates. RD plots, elderly household members



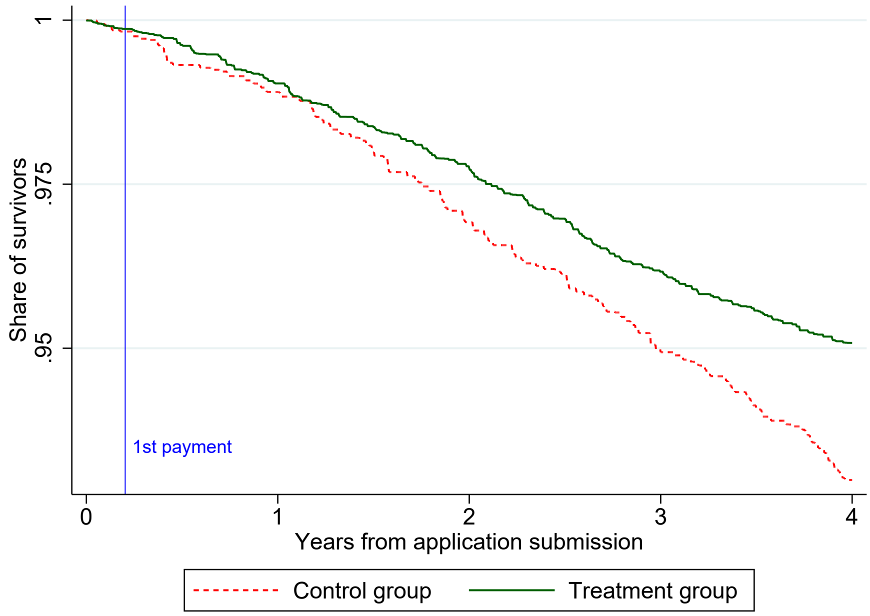
Notes: Each graph shows the average value of the corresponding covariate conditional on the distance of the score from the cut-off. The circles are averages across 50-point bins on either side of the threshold, while the solid and dashed lines represent the predicted values and associated confidence interval, respectively.

FIGURE C.16: Pre-determined covariates. RD plots, elderly household members



Notes: Each graph shows the average value of the corresponding covariate conditional on the distance of the score from the cut-off. The circles are averages across 50-point bins on either side of the threshold, while the solid and dashed lines represent the predicted values and associated confidence interval, respectively.

FIGURE C.17: Share of surviving applicants over 4 years from date of application, adjusted by the deviation of pension score from the cut-off.

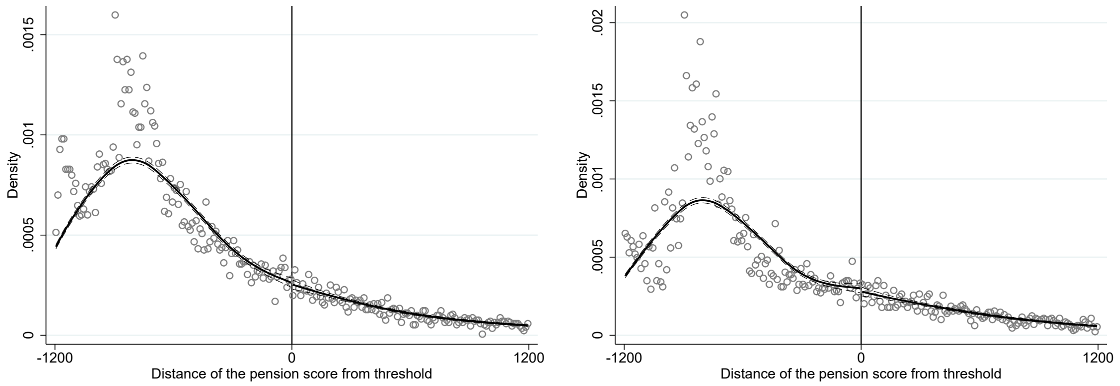


Notes: This figure presents the share of survivors in the treatment and control groups at each point in time following the first application. Survival rates are equal to $1 - \hat{S}(t)$, with $\hat{S}(t)$ being the $k_0(t)$ term in the Cox proportional hazard model: $k(t) = k_0(t)\exp(\beta_1 Score_h)$, with t being the time elapsed after the first application. Survival rates are estimated separately for the treatment and control groups in the 500 score-point bandwidth and using triangular weights.

FIGURE C.18: McCrary tests by household structure

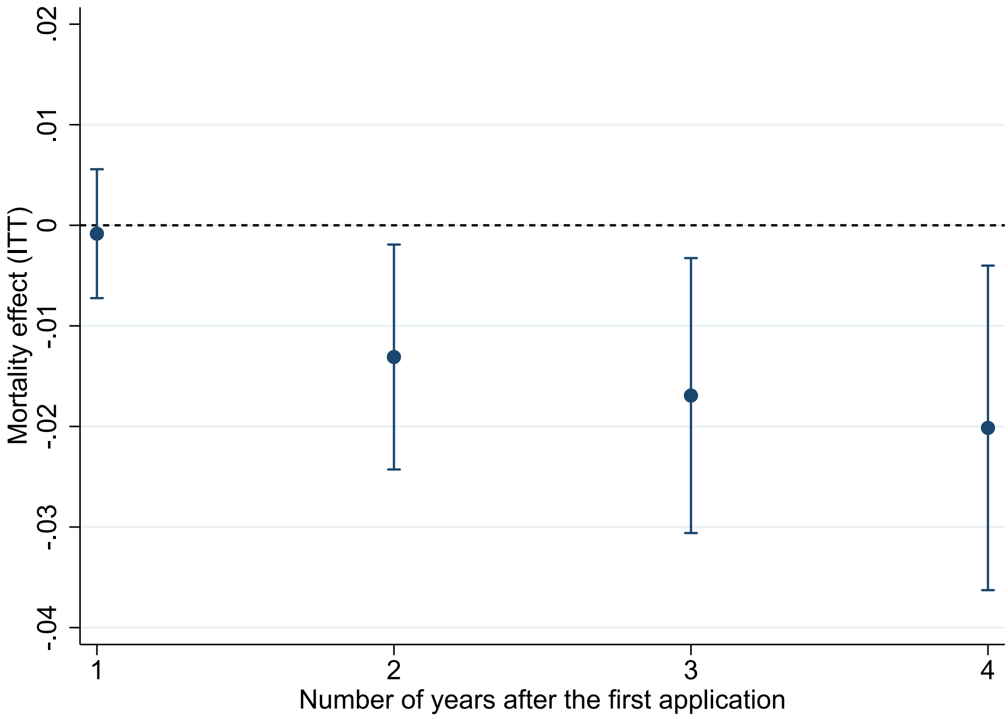
(A) Applicants living with working-age household members

(B) Applicants not living with a working-age household member



Notes: These figures show the density of individuals in 10 score-point bins. The solid line plots fitted values from local linear regressions of density on pension score deviations from the cut-off, separately estimated on both sides of the cut-off. The thin lines represent the 95% confidence intervals.

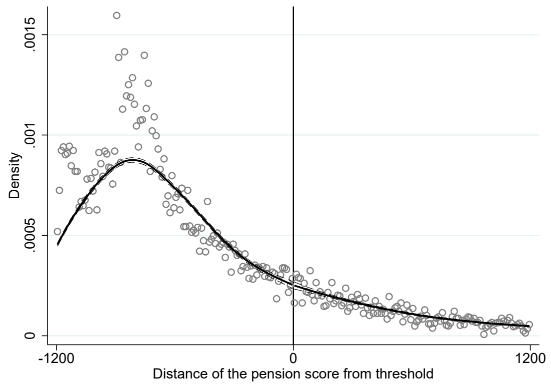
FIGURE C.19: Mortality by year



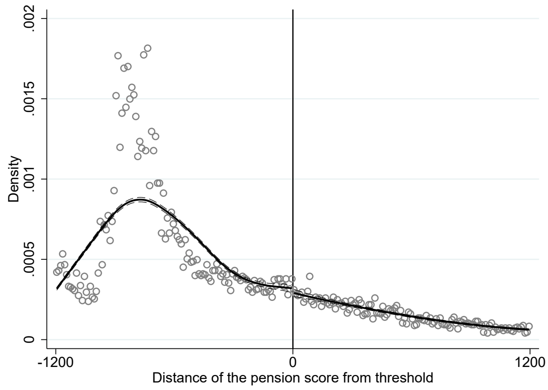
Notes: This graph represents the point estimate and 90% confidence intervals of the ITT effect of the basic pension on applicants' mortality in each of the four years observed after the first application.

FIGURE C.20: McCrary tests of working-age and elderly household members

(A) Working-age household members

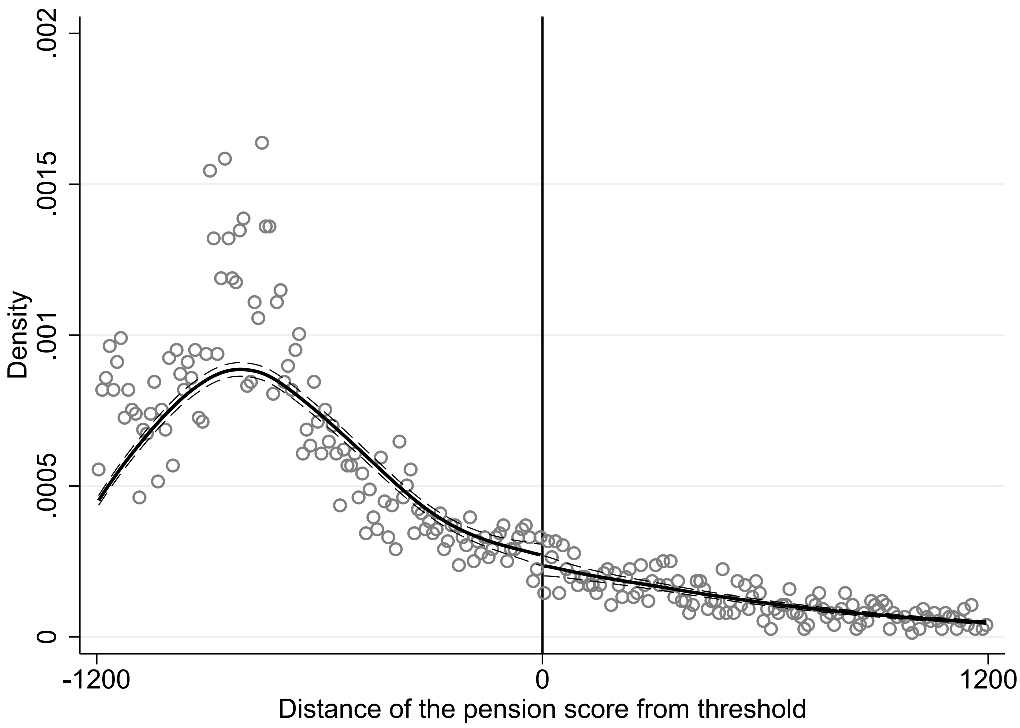


(B) Elderly household members



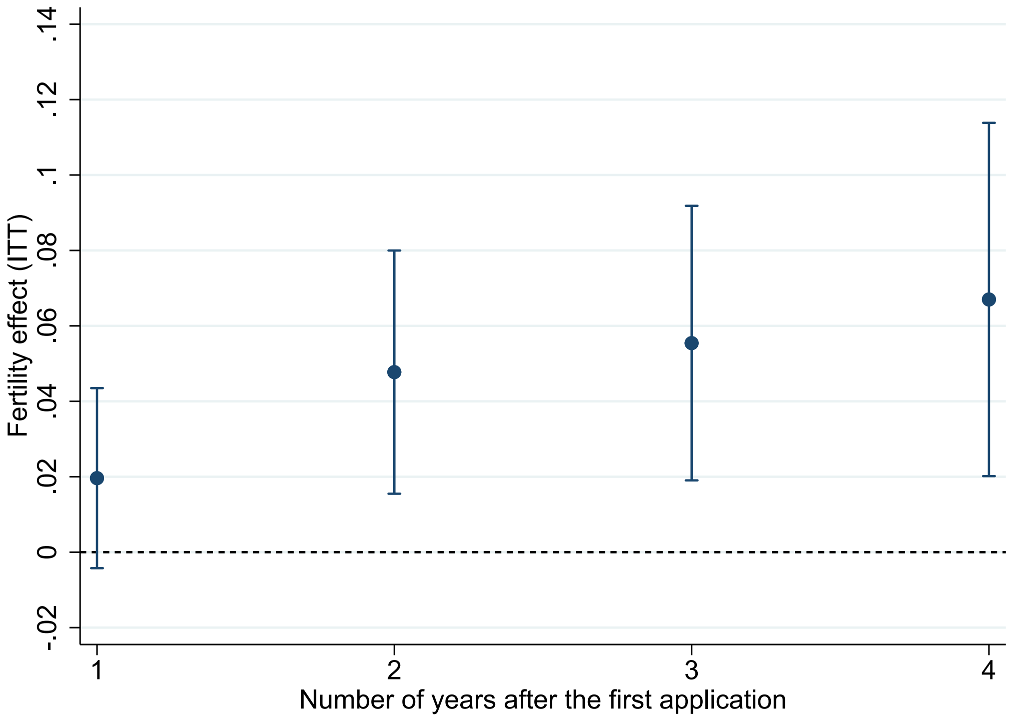
Notes: These figures show the density of individuals in 10 score-point bins. The solid line plots fitted values from a local linear regressions of density on pension score deviations from the cut-off, estimated separately on both sides of the cut-off. The thin lines represent the 95% confidence intervals.

FIGURE C.21: McCrary test on female fertility-age household members



Notes: This figure shows the density of applicants in 10 score-point bins. The solid line plots fitted values from a local linear regression of density on pension score deviations from the cut-off, separately estimated on both sides of the cut-off. The thin lines represent the 95% confidence interval.

FIGURE C.22: Fertility by year



Notes: This graph represents the point estimate and 90% confidence intervals of the ITT effect of the basic pension on the probability of having a child for a female fertility-age family member of an applicant in each of the four years observed after the first application.

Appendix D

Appendix: The Equilibrium Effects of Taxing Property Investors: A Welfare Analysis

D.1 Robustness checks

The 3% Stamp-Duty Land Tax (SDLT) surcharge on purchases of additional properties was part of a Five-Point-Plan to support home ownership in the UK (HM Treasury, 2015, 2016). The other points of the plan were: to deliver 400,000 affordable housing starts by 2020-21; to accelerate housing supply and get more homes built (e.g. by releasing public sector land); to prolong the already existing 'Help to Buy' Equity Loan scheme until 2021 and to create a London 'Help to Buy' scheme; to extend the 'Right to Buy' scheme to Housing Association tenants.

These factors would bias the estimates of the impact of the surcharge if their effects were correlated with the share of private rented properties. To account for this, I run several robustness checks. Regarding the first two points, I do not observe any differential impact in terms of housing supply, using changes in the construction of new private buildings as a dependent variable in the main regressions (Table D.3 and Figure D.6). The creation of the London 'Help to Buy' scheme is controlled in the main regressions by including the interaction of the region London with an indicator for the post-policy period. The extension of the 'Right to Buy' scheme to Housing Association tenants was never rolled-out at a national level, but only through a pilot in the Midlands in August 2018. Tables D.7-D.12 show that controlling for the interaction of the region Midlands with an indicator of the period after the introduction of the 'Right to Buy' scheme does not qualitatively affect the main results.

An additional policy that could affect the estimates is the change in deductions from rental income that occurred in April 2017. Before April 2017, landlords could deduct finance costs (mainly mortgage interest payments) from rental income before their income was taxed. After that date, deductions from rental income were restricted to:

- 75% for 2017 to 2018 with the remaining 25% taken as a basic rate tax reduction
- 50% for 2018 to 2019 with the remaining 50 % taken as a basic rate tax reduction
- 25% for 2019 to 2020 with the remaining 25% taken as a basic rate tax reduction
- 0% for 2020 to 2021 with the remaining 100% taken as a basic rate tax reduction.

The basic rate tax reduction consists in a tax credit equal to 20% times their finance costs.

To account for this policy change, I add to the main regressions the additional control $Post_t \cdot$ *BTL mortgages*$_{j,2015}$, where *BTL mortgages*$_{j,2015}$ is the share of total transactions under a buy-to-let mortgage in local authority j in 2015. If the change in deductions was responsible for the estimated effects in Section 4.4, the effects should disappear once we account for buy-to-let mortgages because outright buy-to-let transactions were not affected by the policy change. Tables D.7-D.12 show that the sign of all the estimated effects remain qualitatively unchanged and the magnitude of the effects is very similar.

D.2 Stocks and flows

Let j_n be the number of type $j \in \{h, i\}$ agents at stage $n \in \{1, 2\}$ without need of a mortgage to buy a property. Let h_0 be households who rent, but do not search for a mortgage or for a property to buy. Let j_n^L be the number of type $j \in \{h, i\}$ agents at stage $n \in \{0, 1, 2\}$ with need of a mortgage to buy a property. Let l_{jn} be the number of lenders to type $j \in \{h, i\}$ agents at stage $n \in \{0, 1, 2\}$.

Housing market tightness is:

$$\theta = \frac{h_1 + h_1^L + i_1 + i_1^L}{s_1} \tag{D.1}$$

Credit market tightnesses for households and investors are:

$$\phi_h = \frac{h_0^L}{l_{h0}}, \quad \phi_i = \frac{i_0^L}{l_{i0}} \tag{D.2}$$

The existing dwellings stock in steady state is:

$$D = s_1 + h_2^L + h_2 + i_2^L + i_2 \tag{D.3}$$

The measures of households is fixed at:

$$\mathcal{H} = h_0 + h_1 + h_2 + h_0^L + h_1^L + h_2^L \tag{D.4}$$

A fraction σ_h (σ_i) of households (investors) that search does not need a mortgage:

$$h_1 + h_2 = \sigma_h(h_1 + h_2 + h_0^L + h_1^L + h_2^L), \quad i_1 + i_2 = \sigma_i(i_1 + i_2 + i_0^L + i_1^L + i_2^L) \tag{D.5}$$

The rental market clears instantaneously. Therefore, the measure of renters must be equal to measure of investor-owners:

$$h_1 + h_0^L + h_1^L = i_2 + i_2^L \tag{D.6}$$

Lenders must be equal to the number of buyers with a mortgage in each stage:

$$l_{h1} = h_1^L, \quad l_{i1} = i_1^L, \quad l_{h2} = h_2^L, \quad l_{i2} = i_2^L \tag{D.7}$$

The laws of motion are:

$$\dot{s_1} = e + \pi_h(h_2^L + h_2) + \pi_i(i_2^L + i_2) - \delta s_1 - (h_1^L + h_1)m(\theta) - (i_1^L + i_1)m(\theta) \quad \text{(D.8)}$$

where e is the measure of new sellers (i.e. newly built houses).

$$\dot{h_1^L} = h_0^L q_h(\phi_h) - h_1^L m(\theta), \quad \dot{i_1^L} = i_0^L q_i(\phi_i) - i_1^L m(\theta) \quad \text{(D.9)}$$

$$\dot{h_2^L} = h_1^L m(\theta) - \pi_h h_2^L, \quad \dot{h_2} = h_1 m(\theta) - \pi_h h_2 \quad \text{(D.10)}$$

$$\dot{i_2^L} = i_1^L m(\theta) - \pi_i i_2^L, \quad \dot{i_2} = i_1 m(\theta) - \pi_i i_2 \quad \text{(D.11)}$$

In steady state, the 19 Equations in (D.1)-(D.11) pin down the 19 variables $(e, h_0^L, h_1^L, h_2^L, h_0, h_1, h_2, i_0^L, i_1^L, i_2^L, i_1, i_2, l_{h0}, l_{h1}, l_{h2}, l_{i0}, l_{i1}, l_{i2}, s_1)$ as functions of $(D, \theta, \phi_h, \phi_i)$ which are determined by the equilibrium equations in Section 4.5.5.

D.3 Derivations

D.3.1 Prices and loans

Borrower-buyers and lenders solve the following maximization problems, taking as given loan amounts a_h and a_i:

$$\max_{p_h^L} [p_h^L - S_1]^{\beta_h} [H_2 - (p_h^L(1+\tau_h) - a_h) - H_1]^{1-\beta_h},$$
$$\max_{p_i^L} [p_i^L - S_1]^{\beta_i} [I_2 - (p_i^L(1+\tau_i) - a_i) - I_1]^{1-\beta_i}$$

The first-order conditions are:

$$\begin{aligned} &\beta_h[H_2^L - H_1^L - p_h^L(1+\tau_h) + a_h] = (1+\tau_h)(1-\beta_h)[p_h^L - S_1], \\ &\beta_i[I_2^L - I_1^L - p_i^L(1+\tau_i) + a_i] = (1+\tau_i)(1-\beta_i)[p_i^L - S_1] \end{aligned} \quad \text{(D.12)}$$

Using the equilibrium conditions, we can rewrite the first-order-conditions as:

$$\begin{aligned} &\beta_h[H_2^L - H_0^L - \frac{\chi_h}{q_h(\phi_h)} - p_h^L(1+\tau_h) + a_h] = (1+\tau_h)(1-\beta_h)[p_h^L - S_1], \\ &\beta_i[I_2^L - I_0^L - \frac{\chi_i}{q_i(\phi_i)} - p_i^L(1+\tau_i) + a_i] = (1+\tau_i)(1-\beta_i)[p_i^L - S_1] \end{aligned} \quad \text{(D.13)}$$

Subtracting Equation (4.5) from (4.7) and plugging the equilibrium conditions (4.19), we obtain

$$H_2^L - H_0^L = \frac{R - rK + \varepsilon_h - \rho_h}{r + \pi_h} \tag{D.14}$$

Subtracting Equation (4.8) from (4.10) and plugging the equilibrium conditions (4.19), we obtain

$$I_2^L - I_0^L = \frac{R - rK + \varepsilon_i - \rho_i}{r + \pi_i} \tag{D.15}$$

Plugging (4.19), (D.14) and (D.15) into (D.13) and rearranging, we obtain the first equality in (4.25):

$$p_j^L = K + \frac{\beta_j}{(1+\tau_j)}\left[\frac{\varepsilon_j + R + \pi_j K}{r + \pi_j} - (1+\tau_j)K - \frac{\chi_j}{q_j(\phi_j)} - \frac{\rho_j}{r+\pi_j} + a_j\right], \quad j \in \{h, i\}$$

To obtain expressions for mortgage repayments, solve the surplus maximization problems taking into account the effect that a_j has on p_j^L according to equation (4.25):

$$\max_{a_h}[L_{h1} - L_{h0}]^{\psi_h}[H_1 - H_0]^{1-\psi_h}, \quad \max_{a_i}[L_{i1} - L_{i0}]^{\psi_i}[I_1 - I_0]^{1-\psi_i}$$

The first-order conditions are:

$$\begin{aligned} \psi_h[H_1^L - H_0^L] &= (1-\psi_h)(1-\beta_h)[L_{h1} - L_{h0}], \\ \psi_i[I_1^L - I_0^L] &= (1-\psi_i)(1-\beta_i)[L_{i1} - L_{i0}] \end{aligned} \tag{D.16}$$

Plugging the equilibrium conditions (4.19), we obtain equilibrium credit market tightness in each market:

$$\phi_j^* = \frac{(1-\beta_j)(1-\psi_j)\chi_{lj}}{\psi_j \chi_j}, \quad j \in \{h, i\} \tag{D.17}$$

Subtracting Equation (4.5) from (4.6), plugging the equilibrium conditions (4.19) and rearranging, we obtain

$$H_1^L - H_0^L = -\frac{c_h}{r + m(\theta)} + \frac{m(\theta)}{r + m(\theta)}\left[\frac{R + \pi_h K + \varepsilon_h - \rho_h}{r + \pi_h} - p_h^L(1+\tau_h) + a_h\right] \tag{D.18}$$

Plugging the price equation (4.25):

$$H_1^L - H_0^L = -\frac{c_h}{r+m(\theta)} + \frac{(1-\beta_h)m(\theta)}{r+m(\theta)}\left[\frac{R+\pi_h K+\varepsilon_h-\rho_h}{r+\pi_h} - (1+\tau_h)K + a_h + \frac{\beta_h\chi_h}{(1-\beta_h)q_h(\phi_h)}\right] \tag{D.19}$$

Subtracting Equation (4.15) from (4.16), plugging the equilibrium conditions (4.19) and rearranging, we obtain

$$L_{h1} - L_{h0} = \frac{m(\theta)}{r+m(\theta)}\left[\frac{\rho_h}{r+\pi_h} - a_h\right] \tag{D.20}$$

Then can substitute out $H_1^L - H_0^L$ and $L_{h1} - L_{h0}$ in the first-order condition (D.16) using (D.19) and (D.20) to obtain:

$$\frac{\rho_h}{r+\pi_h} = a_h + \psi_h\left(\frac{R+\varepsilon_h+\pi_h K}{r+\pi_h} - (1+\tau_h)K - \frac{c_h}{(1-\beta_h)m(\theta)} + \frac{\beta_h\chi_h}{(1-\beta_h)q_h(\phi_h)}\right) \tag{D.21}$$

which is the equation for loan amounts a_h in (4.23).

By a similar reasoning, we have:

$$I_1^L - I_0^L = -\frac{c_i}{r+m(\theta)} + \frac{(1-\beta_i)m(\theta)}{r+m(\theta)}\left[\frac{R+\pi_i K+\varepsilon_i-\rho_i}{r+\pi_i} - (1+\tau_i)K + a_i + \frac{\beta_i\chi_i}{(1-\beta_i)q_i(\phi_i)}\right] \tag{D.22}$$

$$L_{i1} - L_{i0} = \frac{m(\theta)}{r+m(\theta)}\left[\frac{\rho_i}{r+\pi_i} - a_i\right] \tag{D.23}$$

and the loan amount a_i satisfies:

$$\frac{\rho_i}{r+\pi_i} = a_i + \psi_i\left(\frac{R+\varepsilon_i+\pi_i K}{r+\pi_i} - (1+\tau_i)K - \frac{c_i}{(1-\beta_i)m(\theta)} + \frac{\beta_i\chi_i}{(1-\beta_i)q_i(\phi_i)}\right) \tag{D.24}$$

which is the equation for a_i in (4.23). If we substitute out the loan amounts in (4.25) using D.21 and D.24, we obtain the second equality in (4.25).

Buyers without a mortgage agreement and lenders solve the following maximization problems:

$$\begin{aligned} &\max_{p_h}[p_h - S_1]^{\beta_h}[H_2 - p_h(1+\tau_h) - H_1]^{1-\beta_h}, \\ &\max_{p_i}[p_i - S_1]^{\beta_i}[I_2 - p_i(1+\tau_i) - I_1]^{1-\beta_i} \end{aligned} \tag{D.25}$$

The first-order conditions are:

$$\begin{aligned} &\beta_h[H_2 - H_1 - p_h(1+\tau_h)] = (1+\tau_h)(1-\beta_h)[p_h - S_1], \\ &\beta_i[I_2 - I_1 - p_i(1+\tau_i)] = (1+\tau_i)(1-\beta_i)[p_i - S_1] \end{aligned} \tag{D.26}$$

Plugging the values for $H_2 - H_1$ and $I_2 - I_1$ we obtain (4.24).

D.3.2 The financial accelerator

Equalize $H_1^L - H_0^L$ and $I_1^L - I_0^L$ in the equilibrium conditions (4.19) to the forward expressions in equations (D.19) and (D.22) to obtain:

$$\begin{aligned} \frac{\chi_j}{q_j(\phi_j)} = &-\frac{c_j}{r+m(\theta)} \\ &+ \frac{(1-\beta_j)m(\theta)}{r+m(\theta)}\left[\frac{R+\pi_j K+\varepsilon_j-\rho_j}{r+\pi_j} - (1+\tau_j)K + a_j + \frac{\beta_j\chi_j}{(1-\beta_j)}\right] \end{aligned}$$

$$\frac{\chi_{lj}}{\phi_j q_j(\phi_j)} = \frac{m(\theta)}{r+m(\theta)}\left[\frac{\rho_j}{r+\pi_j} - a_j\right]$$

Plug Equation (4.23) into the two equations above to obtain borrowers and lenders' entry equations (BE_j) and (LE_j).

To obtain the graph in Figure 4.9, note that (BE_j) and (LE_j) represent a negative and a positive relationship between ϕ_j and θ for given R and K, respectively. When $\theta \to 0$, (BE_j) and (LE_j) yield level of credit market tightness ϕ_j^B and ϕ_j^L such that:[1]

$$\frac{\chi_j}{q_j(\phi_j^B)} = \frac{(1-\psi_j)(1-\beta_j)\left[\frac{\varepsilon_j+R+\pi_j K(e)}{(r+\pi_j)} - (1+\tau_j)K(e)\right]}{[1-\beta_j(1-\psi_j)]} \tag{D.27}$$

[1]For the existence of an equilibrium, assume that the parameter values are such that $\phi_j^B > \phi_j^L$.

and

$$\frac{\chi_{lj}}{\phi_j q_j(\phi_j^L)} = \frac{\psi_j \left[\frac{\varepsilon_j + R + \pi_j K(e)}{(r+\pi_j)} - (1+\tau_j)K(e)\right]}{[1-\beta_j(1-\psi_j)]} \tag{D.28}$$

When $\phi_j \to 0$ in (BE_j) and when $\phi_j \to \infty$ in (LE_j) market tightness is $\theta = \bar{\theta}$ such that:

$$c_j = m(\bar{\theta})(1-\beta_j)\left[\frac{\varepsilon_j + R + \pi_j K(e)}{r+\pi_j} - (1+\tau_j)K(e)\right] \tag{D.29}$$

Note that minimizing credit frictions ($q_j(\phi_j) \to \infty$ at any ϕ_j) yields the supremum of housing market tightness $\bar{\theta}$.

D.3.3 Shares of buyers' types

If we equalize Equations (D.4) and (D.6), we obtain:

$$\mathcal{H} = h_2 + h_2^L + i_2 + i_2^L \tag{D.30}$$

Since there are no homeless people in the model, the number of households must be equal to the number of non-empty houses (owner-occupied or rented). In steady state, plugging Equations (D.10)-(D.11) into (D.8) yields

$$e = \delta s \tag{D.31}$$

The number of new houses equals the number of demolished houses in steady state. From Equations (D.3), (D.30) and (D.31), we can find:

$$s_1 = \frac{e}{\delta} = D - \mathcal{H}, \tag{D.32}$$

Then, using the definition of housing market tightness:

$$h_1 + h_1^L + i_1 + i_1^L = \theta(D - \mathcal{H}) \tag{D.33}$$

Now note from Equations (D.5), (D.10) and (D.11)

$$h_1 + h_1^L = \frac{\pi_h}{\pi_i - \pi_h}\left[\frac{\pi_i \mathcal{H}}{m(\theta)} - \theta(D-H)\right] \tag{D.34}$$

$$i_1 + i_1^L = \frac{\pi_i}{\pi_i - \pi_h}\left[\theta(D-H) - \frac{\pi_h \mathcal{H}}{m(\theta)}\right] \tag{D.35}$$

Also,

$$h_1 = \frac{\sigma_h\left[1+\frac{m(\theta)}{\pi_h}+\frac{m(\theta)}{q_h(\phi_h)}\right]}{(1-\sigma_h)\left[1+\frac{m(\theta)}{\pi_h}\right]} h_1^L \tag{D.36}$$

$$i_1 = \frac{\sigma_i\left[1+\frac{m(\theta)}{\pi_i}+\frac{m(\theta)}{q_i(\phi_i)}\right]}{(1-\sigma_i)\left[1+\frac{m(\theta)}{\pi_i}\right]} i_1^L \tag{D.37}$$

Finally, using Equations (D.34)-(D.37) and (D.32) we can find the shares of buyers:

$$\frac{h_1}{h_1^L+h_1+i_1^L+i_1} = \frac{\sigma_h\left[1+\frac{m(\theta)}{\pi_h}+\frac{m(\theta)}{q_h(\phi_h)}\right]}{\left[1+\frac{m(\theta)}{\pi_h}+\frac{\sigma_h m(\theta)}{q_h(\phi_h)}\right]} \frac{\pi_h}{(\pi_i-\pi_h)}\left[\frac{\pi_i \mathcal{H}\delta}{\theta m(\theta)e}-1\right] \tag{D.38}$$

$$\frac{i_1}{h_1^L+h_1+i_1^L+i_1} = \frac{\sigma_i\left[1+\frac{m(\theta)}{\pi_i}+\frac{m(\theta)}{q_i(\phi_i)}\right]}{\left[1+\frac{m(\theta)}{\pi_i}+\frac{\sigma_i m(\theta)}{q_i(\phi_i)}\right]} \frac{\pi_i}{(\pi_i-\pi_h)}\left[1-\frac{\pi_h \mathcal{H}\delta}{\theta m(\theta)e}\right] \tag{D.39}$$

$$\frac{h_1^L}{h_1^L+h_1+i_1^L+i_1} = \frac{(1-\sigma_h)\left[1+\frac{m(\theta)}{\pi_h}\right]}{\left[1+\frac{m(\theta)}{\pi_h}+\frac{\sigma_h m(\theta)}{q_h(\phi_h)}\right]} \frac{\pi_h}{(\pi_i-\pi_h)}\left[\frac{\pi_i \mathcal{H}\delta}{\theta m(\theta)e}-1\right] \tag{D.40}$$

$$\frac{i_1^L}{h_1^L+h_1+i_1^L+i_1} = \frac{(1-\sigma_i)\left[1+\frac{m(\theta)}{\pi_i}\right]}{\left[1+\frac{m(\theta)}{\pi_i}+\frac{\sigma_i m(\theta)}{q_i(\phi_i)}\right]} \frac{\pi_i}{(\pi_i-\pi_h)}\left[1-\frac{\pi_h \mathcal{H}\delta}{\theta m(\theta)e}\right] \tag{D.41}$$

D.3.4 Wealthy households' and investors' mortgage choice

In this section, we compare the equilibrium values of household buyers searching for a seller with and without a mortgage agreement. Denote H_1^M the value of a household buyer. First, note that in equilibrium:

$$H_2 = \frac{y+\varepsilon_h+\pi_h K+\pi_h H_1}{r+\pi_h} \tag{D.42}$$

$$H_2^L + a_h = \frac{y+\varepsilon_h-\rho_h+\pi_h K+\pi_h H_1^L}{r+\pi_h} + a_h \leq \frac{y+\varepsilon_h+\pi_h K+\pi_h H_1^L}{r+\pi_h} \tag{D.43}$$

where the inequality stems from the fact that the present discounted value of a loan for a bank has to be positive $\frac{\rho_h}{r+\pi_h} - a_h > 0$. This is a necessary condition for the existence of an equilibrium in which lenders participate. Then, from the first-order-condition in the Nash bargaining and (D.42)

$$\begin{aligned} rH_1 &= y - R - c_h + m(\theta)[H_2 - H_1 - p_h] \\ &= y - R - c_h + m(\theta)(1-\beta_h)[H_2 - H_1 - K] \\ &= y - R - c_h + m(\theta)(1-\beta_h)\left[\frac{y + \varepsilon_h + \pi_h K - rH_1}{r+\pi_h} - (1+\tau_h)K\right] \end{aligned} \tag{D.44}$$

Rearranging:

$$H_1 = \frac{y - R - c_h + m(\theta)(1-\beta_h)\left[\frac{y+\varepsilon_h+\pi_h K}{r+\pi_h} - (1+\tau_h)K\right]}{r + \frac{m(\theta)(1-\beta_h)r}{r+\pi_h}} \tag{D.45}$$

Likewise, from the first-order-condition in the Nash bargaining and (D.43):

$$\begin{aligned} rH_1^L &= y - R - c_h + m(\theta)[H_2^L - H_1^L - p_h + a_h] \\ &= y - R - c_h + m(\theta)(1-\beta_h)[H_2^L + a_h - H_1^L - K] \\ &\leq y - R - c_h + m(\theta)(1-\beta_h)\left[\frac{y + \varepsilon_h + \pi_h K - rH_1^L}{r+\pi_h} - (1+\tau_h)K\right] \end{aligned} \tag{D.46}$$

Rearranging:

$$H_1^L \leq \frac{y - R - c_h + m(\theta)(1-\beta_h)\left[\frac{y+\varepsilon_h+\pi_h K}{r+\pi_h} - (1+\tau_h)K\right]}{r + \frac{m(\theta)(1-\beta_h)r}{r+\pi_h}} = H_1 \tag{D.47}$$

A wealthy buyer will always prefer to search and buy without a mortgage, as long as the search value is positive and the lender's bargaining power is not 0. The intuition is that the buyer prefers not share any of the transaction surplus with the lender and therefore will never ask for a mortgage in case she has sufficient wealth to purchase a property outright.

D.3.5 Welfare analysis for households and investors

Assume depreciation affects investors and households at the same rate. In the steady state, the number of entry sellers must be equal to the number of demolished homes for each type: $e_j = \delta s_j,\ j \in \{i,h\}$. As investor- and household-sellers have the same probability to find a buyer, their number depends only on the type-specific rate at which owners become sellers and the number of owners of each type. Therefore, the steady state number of household-sellers is $s_h = \frac{\pi_h(h_2^L+h_2)}{\pi_h(h_2^L+h_2)+\pi_i(i_2^L+i_2)} s$ and the number of investor-sellers is $s_i = \frac{\pi_i(i_2^L+i_2)}{\pi_h(h_2^L+h_2)+\pi_i(i_2^L+i_2)} s$. Finally, let G denote per-capita tax revenues which are equally redistributed across households and investors. They are equal to:

$$G = \frac{m(\theta)[(h_1p_h + h_1^L p_h^L)\tau_h + (i_1p_i + i_1^L p_i^L)\tau_i]}{\mathcal{H} + i_0^L + i_1^L + i_2^L + i_1 + i_2} \tag{D.48}$$

Per capita net flow utility for households is:[2]

$$\begin{aligned} rW_h = G + \{ & - h_0^L\chi_h - h_1^L[c_h + m(\theta) * (p_h^L(1+\tau_h) - a_h)] \\ & + h_2^L(\varepsilon_h + R - \rho_h) - h_1[c_h + m(\theta)p_h(1+\tau_h)] \\ & + h_2(\varepsilon_h + R) + s_h[-c_s + \theta m(\theta)\overline{p}] - e_hK\}/\mathcal{H} \end{aligned} \tag{D.49}$$

Per capita net flow utility for investors is:

$$\begin{aligned} rW_i = G + \{ & - i_0^L\chi_i - i_1^L[c_i + m(\theta) * (p_i^L(1+\tau_i) - a_i)] \\ & + i_2^L(\varepsilon_i + R - \rho_i) - i_1[c_i + m(\theta)p_i(1+\tau_i)] \\ & + i_2(\varepsilon_i + R) + s_i[-c_s + \theta m(\theta)\overline{p}] - e_iK\}/\{i_0^L + i_1^L + i_2^L + i_1 + i_2\} \end{aligned} \tag{D.50}$$

Finally, per capita net flow utility for all agents is:

$$\begin{aligned} rW = & \{\mathcal{H}y - h_0^L\chi_h - h_1^Lc_h + h_2^L\varepsilon_h - h_1c_h + h_2\varepsilon_h - i_0^L\chi_i - i_1^Lc_i \\ & + i_2^L\varepsilon_i - i_1c_i + i_2\varepsilon_i - l_{h0}\chi_{lh} - l_{i0}\chi_{li} - sc_s - eK\}/ \\ & \{\mathcal{H} + i_0^L + i_1^L + i_2^L + i_1 + i_2 + l_{h0} + l_{i0} + l_{h1} + l_{i1} + l_{h2} + l_{i2}\} \end{aligned} \tag{D.51}$$

[2] Sellers are not included in the denominator to avoid double-counting, as sellers are also simultaneously buyers or owners.

D.4 Additional tables

TABLE D.1: Effect of stamp-duty surcharge on log listing price

	(1)	(2)	(3)	(4)	(5)	(6)
Post*Share Rented	0.061	0.039	0.038	0.058	-0.101	-0.101
	(0.073)	(0.062)	(0.063)	(0.074)	(0.086)	(0.074)
Ant.*Share Rented			-0.005			
			(0.018)			
N	1,959,855	1,959,855	1,959,855	1,598,444	1,598,444	1,598,444
LA controls	NO	YES	YES	YES	YES	YES
Donut hole	NO	NO	NO	YES	YES	YES
Post*London	YES	YES	YES	YES	YES	YES
Post*Second shares	YES	YES	YES	YES	YES	YES
Post*EU shares	NO	NO	NO	NO	YES	YES
S.E.	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Spatial HAC (100km)

Notes: This table reports results from OLS regressions of Equation (4.1) using the log listing price as the dependent variable. Controls (property level): quadratics in latitude and longitude, size, number of rooms, energy performance, type of property, new, leasehold, fireplace, type of wall, extensions. Controls (LA level): lagged population, GDP per capita, housing stock, council total expenditures, council housing expenditures, council tax. In columns (1)-(5), s.e. are clustered at local authority level. In column (6), I allow spatial HAC s.e. to be serially correlated over the entire period. Spatial weighting kernels are assumed to decay linearly. Zero spatial correlation is assumed beyond 100km. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D.2: Differential effect on paid prices by type of buyer

	(1)	(2)	(3)	(4)	(5)	(6)
Buy-to-let	0.001*	0.001*	0.001*	0.001	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Post*Share Rented	-0.053***	-0.087***	-0.084***	-0.092***	-0.108***	-0.108***
	(0.022)	(0.016)	(0.016)	(0.019)	(0.021)	(0.019)
Buy-to-let*	-0.008***	-0.008***	-0.008***	-0.007**	-0.007**	-0.007**
Post*Share Rented	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
N	1,569,552	1,569,552	1,569,552	1,261,945	1,261,945	1,261,945
LA controls	NO	YES	YES	YES	YES	YES
Donut hole	NO	NO	NO	YES	YES	YES
Post*London	YES	YES	YES	YES	YES	YES
Post*Second shares	YES	YES	YES	YES	YES	YES
Post*EU shares	NO	NO	NO	NO	YES	YES
S.E.	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Spatial HAC (100km)

Notes: This table reports results from OLS regressions of Equation (4.1) using the log paid price as the dependent variable. An indicator for a buy-to-let transaction and the interaction with $Post * Rented$ is added to the right-hand side. Controls (property level): log listing price, quadratics in latitude and longitude, size, number of rooms, energy performance, type of property, new, leasehold, fireplace, type of wall, extensions. Controls (LA level): lagged population, GDP per capita, housing stock, council total expenditures, council housing expenditures, council tax. In columns (1)-(5), s.e. are clustered at local authority level. In column (6), I allow spatial HAC s.e. to be serially correlated over the entire period. Spatial weighting kernels are assumed to decay linearly. Zero spatial correlation is assumed beyond 100km. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D.3: Effect of stamp-duty surcharge on number of quarterly constructions of private buildings

	(1)	(2)	(3)	(4)	(5)
Post*Share	25.660	-16.705	-27.203	-25.026	-27.152
Rented	(70.549)	(55.080)	(67.707)	(61.221)	(70.672)
Ant.*Share			-59.983		
Rented			(95.508)		
N	8,108	8,108	8,108	6,804	6,804
LA controls	NO	YES	YES	YES	YES
Donut hole	NO	NO	NO	YES	YES
Post*London	YES	YES	YES	YES	YES
Post*Second shares	YES	YES	YES	YES	YES
Post*EU shares	NO	NO	NO	NO	YES
S.E.	Clustered	Clustered	Clustered	Clustered	Clustered
	at LA	at LA	at LA	at LA	at LA

Notes: This table reports results from OLS regressions of Equation (4.1) using the log of the number of days between the transaction date and the listing date as the dependent variable. Controls (LA level): lagged population, GDP per capita, housing stock, council total expenditures, council housing expenditures, council tax. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D.4: Effect of stamp-duty surcharge on number of demolitions

	(1)	(2)	(3)	(4)	(5)
Post*Share	10.797	16.832	-6.425	-1.230	-29.874
Rented	(60.546)	(55.562)	(64.844)	(58.650)	(69.736)
Ant.*Share			23.368		
Rented			(61.507)		
N	8,094	8,094	8,094	6,790	6,790
London*Quarter FE					
LA controls	NO	YES	YES	YES	YES
Donut hole	NO	NO	NO	YES	YES
Post*EU shares	NO	NO	NO	NO	YES
S.E.	Clustered	Clustered	Clustered	Clustered	Clustered
S.E.	at LA	at LA	at LA	at LA	at LA

Notes: This table reports results from OLS regressions of Equation (4.1) using number of demolitions in each local authority as the dependent variable. Controls (LA level): lagged population, GDP per capita housing stock, council total expenditures, council housing expenditures, council tax. All regressions include local authority and quarter fixed effects. ***< 0.01, **< 0.05, *< 0.1.

TABLE D.5: Effect of stamp-duty surcharge on log paid price

	(1)	(2)	(3)	(4)	(5)	(6)
Post*Share Rented	-0.060***	-0.094***	-0.091***	-0.097***	-0.112***	-0.112***
	(0.022)	(0.017)	(0.017)	(0.019)	(0.022)	(0.020)
Ant.*Share Rented			0.025***			
			(0.009)			
N	1,950,769	1,950,769	1,950,769	1,590,874	1,590,874	1,590,874
LA controls	NO	YES	YES	YES	YES	YES
Donut hole	NO	NO	NO	YES	YES	YES
Post*London	YES	YES	YES	YES	YES	YES
Post*Second shares	YES	YES	YES	YES	YES	YES
Post*EU shares	NO	NO	NO	NO	YES	YES
S.E.	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Spatial HAC (100km)

Notes: This table reports results from OLS regressions of Equation (4.1) using the log price paid buy all buyers as dependent variables. Controls (property level): log listing price, quadratics in latitude and longitude, size, number of rooms, energy performance, type of property, new, leasehold, fireplace, type of wall, extensions. Controls (LA level): lagged population, GDP per capita, housing stock, council total expenditures, council housing expenditures, council tax. In columns (1)-(5), s.e. are clustered at local authority level. In column (6), I allow spatial HAC s.e. to be serially correlated over the entire period. Spatial weighting kernels are assumed to decay linearly. Zero spatial correlation is assumed beyond 100km. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE D.6: Effect of stamp-duty surcharge on log days to sell

	(1)	(2)	(3)	(4)	(5)	(6)
Post*Share Rented	0.166	0.258**	0.269**	0.315***	0.284*	0.284**
	(0.112)	(0.103)	(0.105)	(0.121)	(0.147)	(0.127)
Ant.*Share Rented			0.080*			
			(0.041)			
N	1,994,783	1,994,783	1,994,783	1,628,019	1,628,019	1,628,019
LA controls	NO	YES	YES	YES	YES	YES
Donut hole	NO	NO	NO	YES	YES	YES
Post*London	YES	YES	YES	YES	YES	YES
Post*Second shares	YES	YES	YES	YES	YES	YES
Post*EU shares	NO	NO	NO	NO	YES	YES
S.E.	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Spatial HAC (100km)

Notes: This table reports results from OLS regressions of Equation (4.1) using the log of the number of days between the transaction date and the listing date as the dependent variable. Controls (property level): quadratics in latitude and longitude, size, number of rooms, energy performance, type of property, new, leasehold, fireplace, type of wall, extensions. Controls (LA level): lagged population, GDP per capita, housing stock, council total expenditures, council housing expenditures, council tax. In columns (1)-(5), s.e. are clustered at local authority level. In column (6), I allow spatial HAC s.e. to be serially correlated over the entire period. Spatial weighting kernels are assumed to decay linearly. Zero spatial correlation is assumed beyond 100km. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE D.7: Effect of stamp-duty surcharge on number of log quarterly transactions, controlling for *Post August 2018* $*$ $Midlands$ and *Post* $*$ *Buy-to-let mortgage share*

	(1)	(2)	(3)	(4)	(5)
Post*Share Rented	-0.853***	-0.868***	-0.639***	-0.825***	-0.667***
	(0.128)	(0.130)	(0.164)	(0.182)	(0.217)
Ant.*Share Rented			0.637***		
			(0.187)		
N	8,700	8,700	8,700	7,308	7,308
LA controls	NO	YES	YES	YES	YES
Donut hole	NO	NO	NO	YES	YES
Post*London	YES	YES	YES	YES	YES
Post*Second shares	YES	YES	YES	YES	YES
Post*EU shares	NO	NO	NO	NO	YES
S.E.	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA

Notes: This table reports results from OLS regressions of Equation (4.1) using the log of the number of days between the transaction date and the listing date as the dependent variable. Controls (LA level): lagged population, GDP per capita, housing stock, council total expenditures, council housing expenditures, council tax. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE D.8: Effect of stamp-duty surcharge on number of quarterly constructions of private buildings, controlling for *Post August 2018* $*$ $Midlands$ and *Post* $*$ *Buy-to-let mortgage share*

	(1)	(2)	(3)	(4)	(5)
Post*Share	26.625	-16.590	-27.250	-24.809	-27.048
Rented	(70.458)	(54.664)	(67.047)	(60.650)	(69.842)
Ant.*Share			-59.994		
Rented			(95.421)		
N	8,108	8,108	8,108	6,804	6,804
LA controls	NO	YES	YES	YES	YES
Donut hole	NO	NO	NO	YES	YES
Post*London	YES	YES	YES	YES	YES
Post*Second shares	YES	YES	YES	YES	YES
Post*EU shares	NO	NO	NO	NO	YES
S.E.	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA

Notes: This table reports results from OLS regressions of Equation (4.1) using the number of quarterly constructions of private buildings as the dependent variable. Controls (LA level): lagged population, GDP per capita, housing stock, council total expenditures, council housing expenditures, council tax. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D.9: Effect of stamp-duty surcharge on number of demolitions, controlling for *Post August 2018* $*$ $Midlands$ and *Post* $*$ *Buy-to-let mortgage share*

	(1)	(2)	(3)	(4)	(5)
Post*Share	49.834	50.959	21.182	43.770	6.827
Rented	(61.559)	(57.126)	(64.658)	(60.677)	(69.243)
Ant.*Share			19.875		
Rented			(61.603)		
N	8,094	8,094	8,094	6,790	6,790
London*Quarter FE					
LA controls	NO	YES	YES	YES	YES
Donut hole	NO	NO	NO	YES	YES
Post*EU shares	NO	NO	NO	NO	YES
S.E.	Clustered	Clustered	Clustered	Clustered	Clustered
S.E.	at LA	at LA	at LA	at LA	at LA

Notes: This table reports results from OLS regressions of Equation (4.1) using annual number of demolitions in each local authority as the dependent variable. Controls (LA level): lagged population, GDP per capita housing stock, council total expenditures, council housing expenditures, council tax. All regressions include local authority, quarter fixed effects, the interaction between Midlands and indicator for the period after the implementation of the Right-to-buy Scheme, the interaction between pre-policy buy-to-let shares and Post.

TABLE D.10: Effect of stamp-duty surcharge on log days to sell, controlling for *Post August 2018* $*$ $Midlands$ and *Post* $*$ *Buy-to-let mortgage share*

	(1)	(2)	(3)	(4)	(5)	(6)
Post*Share Rented	0.432	0.487**	0.496**	0.564***	0.464*	0.464**
	(0.100)	(0.098)	(0.099)	(0.118)	(0.126)	(0.109)
Ant.*Share Rented			0.68*			
			(0.040)			
N	1,994,783	1,994,783	1,994,783	1,628,019	1,628,019	1,628,019
LA controls	NO	YES	YES	YES	YES	YES
Donut hole	NO	NO	NO	YES	YES	YES
Post*London	YES	YES	YES	YES	YES	YES
Post*Second shares	YES	YES	YES	YES	YES	YES
Post*EU shares	NO	NO	NO	NO	YES	YES
S.E.	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Spatial HAC (100km)

Notes: This table reports results from OLS regressions of Equation (4.1) using the log of the number of days between the transaction date and the listing date as the dependent variable. Controls (property level): quadratics in latitude and longitude, size, number of rooms, energy performance, type of property, new, leasehold, fireplace, type of wall, extensions. Controls (LA level): lagged population, GDP per capita, housing stock, council total expenditures, council housing expenditures, council tax. In columns (1)-(5), s.e. are clustered at local authority level. In column (6), I allow spatial HAC s.e. to be serially correlated over the entire period. Spatial weighting kernels are assumed to decay linearly. Zero spatial correlation is assumed beyond 100km. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE D.11: Effect of stamp-duty surcharge on number of log price paid by future owner-occupiers, controlling for *Post August 2018* $* Midlands$ and *Post* $*$ *Buy-to-let mortgage share*

	(1)	(2)	(3)	(4)	(5)	(6)
Post*Share Rented	-0.036*	-0.066***	-0.063***	-0.069***	-0.086***	-0.086***
	(0.021)	(0.017)	(0.017)	(0.020)	(0.023)	(0.023)
Ant.*Share Rented			0.022**			
			(0.010)			
N	1,226,749	1,226,749	1,226,749	978,144	978,144	978,144
LA controls	NO	YES	YES	YES	YES	YES
Donut hole	NO	NO	NO	YES	YES	YES
Post*London	YES	YES	YES	YES	YES	YES
Post*Second shares	YES	YES	YES	YES	YES	YES
Post*EU shares	NO	NO	NO	NO	YES	YES
S.E.	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Spatial HAC (100km)

Notes: This table reports results from OLS regressions of Equation (4.1) using the log price paid by future owner-occupiers as the dependent variable. Controls (property level): log listing price, quadratics in latitude and longitude, size, number of rooms, energy performance, type of property, new, leasehold, fireplace, type of wall, extensions. Controls (LA level): lagged population, GDP per capita, housing stock, council total expenditures, council housing expenditures, council tax. In columns (1)-(5), s.e. are clustered at local authority level. In column (6), I allow spatial HAC s.e. to be serially correlated over the entire period. Spatial weighting kernels are assumed to decay linearly. Zero spatial correlation is assumed beyond 100km. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D.12: Effect of stamp-duty surcharge on log price paid by buy-to-let investors, controlling for *Post August 2018* $*$ $Midlands$ and *Post* $*$ *Buy-to-let mortgage share*

	(1)	(2)	(3)	(4)	(5)	(6)
Post*Share Rented	-0.082***	-0.112***	-0.107***	-0.108***	-0.120***	-0.120***
	(0.025)	(0.019)	(0.020)	(0.023)	(0.026)	(0.025)
Ant.*Share Rented			0.032**			
			(0.015)			
N	342,803	342,803	342,803	283,801	283,801	283,801
LA controls	NO	YES	YES	YES	YES	YES
Donut hole	NO	NO	NO	YES	YES	YES
Post*London	YES	YES	YES	YES	YES	YES
Post*Second shares	YES	YES	YES	YES	YES	YES
Post*EU shares	NO	NO	NO	NO	YES	YES
S.E.	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Spatial HAC (100km)

Notes: This table reports results from OLS regressions of Equation (4.1) using the log price paid by buy-to-let investors as the dependent variable. Controls (property level): log listing price, quadratics in latitude and longitude, size, number of rooms, energy performance, type of property, new, leasehold, fireplace, type of wall, extensions. Controls (LA level): lagged population, GDP per capita, housing stock, council total expenditures, council housing expenditures, council tax. In columns (1)-(5), s.e. are clustered at local authority level. In column (6), I allow spatial HAC s.e. to be serially correlated over the entire period. Spatial weighting kernels are assumed to decay linearly. Zero spatial correlation is assumed beyond 100km. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

TABLE D.13: Effect of stamp-duty surcharge on size of transacted properties

	(1)	(2)	(3)	(4)	(5)	(6)
Post*Share Rented	1.008	1.307	1.601	1.743	2.628**	2.628**
	(0.994)	(1.049)	(1.041)	(1.118)	(1.272)	(1.263)
Ant.*Share Rented			2.088*			
			(1.142)			
N	3,696,699	3,696,699	3,696,699	2,967,141	2,967,141	2,967,141
LA controls	NO	YES	YES	YES	YES	YES
Donut hole	NO	NO	NO	YES	YES	YES
Post*London	YES	YES	YES	YES	YES	YES
Post*Second shares	YES	YES	YES	YES	YES	YES
Post*EU shares	NO	NO	NO	NO	YES	YES
S.E.	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Spatial HAC (100km)

Notes: This table reports results from OLS regressions of Equation (4.1) using the size of transacted property (in square meters) as the dependent variable. I do not include controls at property level in this regression. Controls (LA level): lagged population, GDP per capita, housing stock, council total expenditures, council housing expenditures, council tax. In columns (1)-(5), s.e. are clustered at local authority level. In column (6), I allow spatial HAC s.e. to be serially correlated over the entire period. Spatial weighting kernels are assumed to decay linearly. Zero spatial correlation is assumed beyond 100km. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

TABLE D.14: Effect of stamp-duty surcharge on energy cost (£ per square meter per year)

	(1)	(2)	(3)	(4)	(5)	(6)
Post*Share Rented	0.403	0.368	0.282	0.282	0.274	0.274
	(0.266)	(0.261)	(0.264)	(0.295)	(0.353)	(0.302)
Ant.*Share Rented			-0.613			
			(0.296)			
N	3,696,699	3,696,699	3,696,699	2,967,141	2,967,141	2,967,141
LA controls	NO	YES	YES	YES	YES	YES
Donut hole	NO	NO	NO	YES	YES	YES
Post*London	YES	YES	YES	YES	YES	YES
Post*Second shares	YES	YES	YES	YES	YES	YES
Post*EU shares	NO	NO	NO	NO	YES	YES
S.E.	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Clustered at LA	Spatial HAC (100km)

Notes: This table reports results from OLS regressions of Equation (4.1) using the energy cost (£) required for lighting, space and water heating per square meter per year of the transacted property as the dependent variable. I do not include controls at property level in this regression. Controls (LA level): lagged population, GDP per capita, housing stock, council total expenditures, council housing expenditures, council tax. In columns (1)-(5), s.e. are clustered at local authority level. In column (6), I allow spatial HAC s.e. to be serially correlated over the entire period. Spatial weighting kernels are assumed to decay linearly. Zero spatial correlation is assumed beyond 100km. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

D.5 Additional figures

FIGURE D.1: Real housing price growth in OECD countries

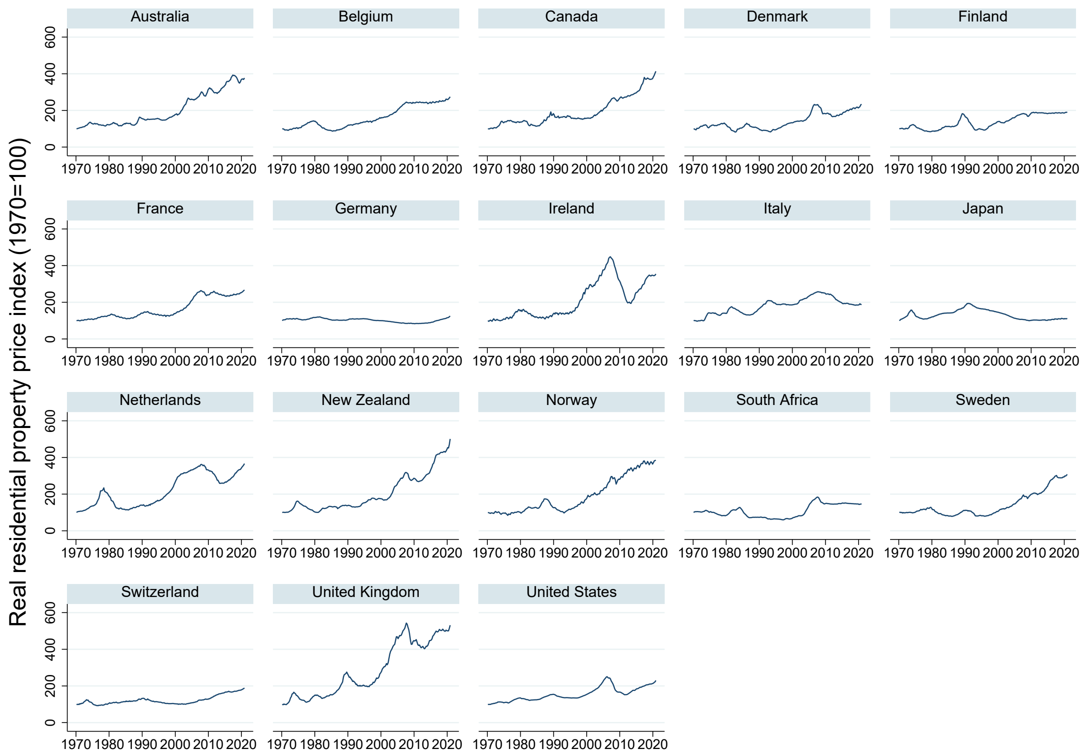


FIGURE D.2: Stamp-duty surcharge liability

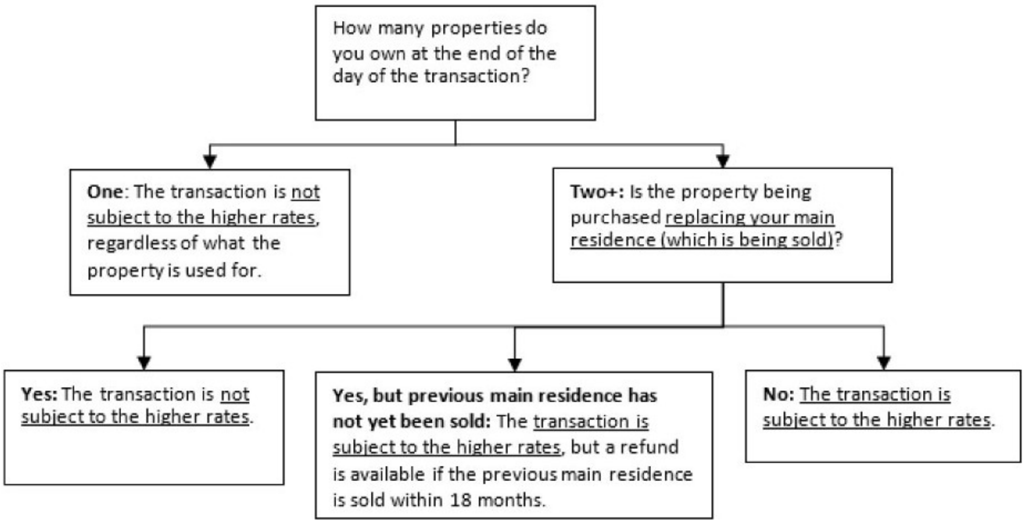


FIGURE D.3: Stamp duty anticipation effects

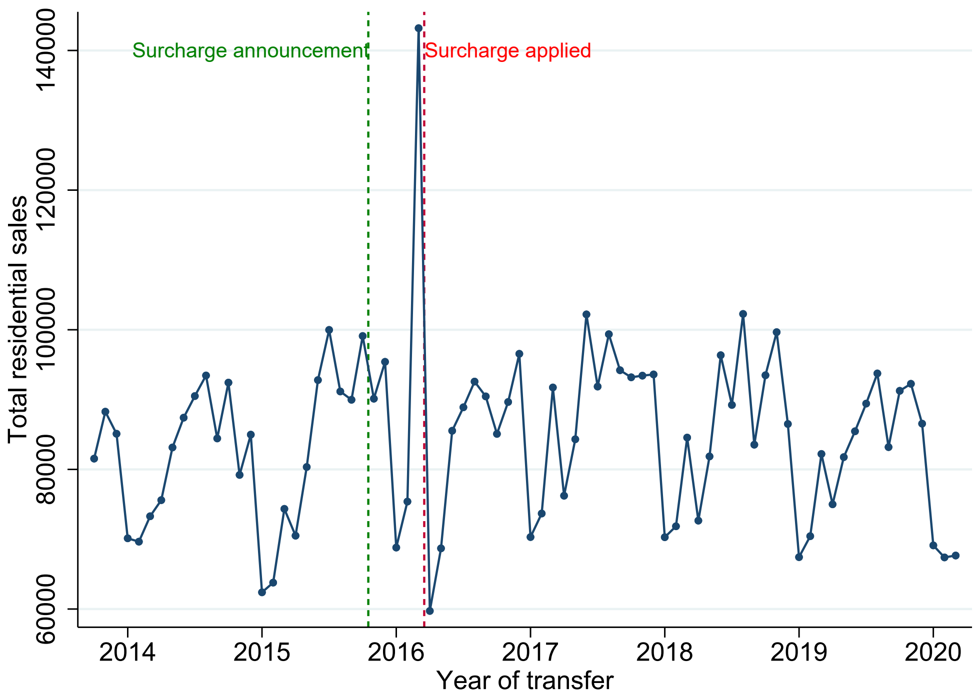
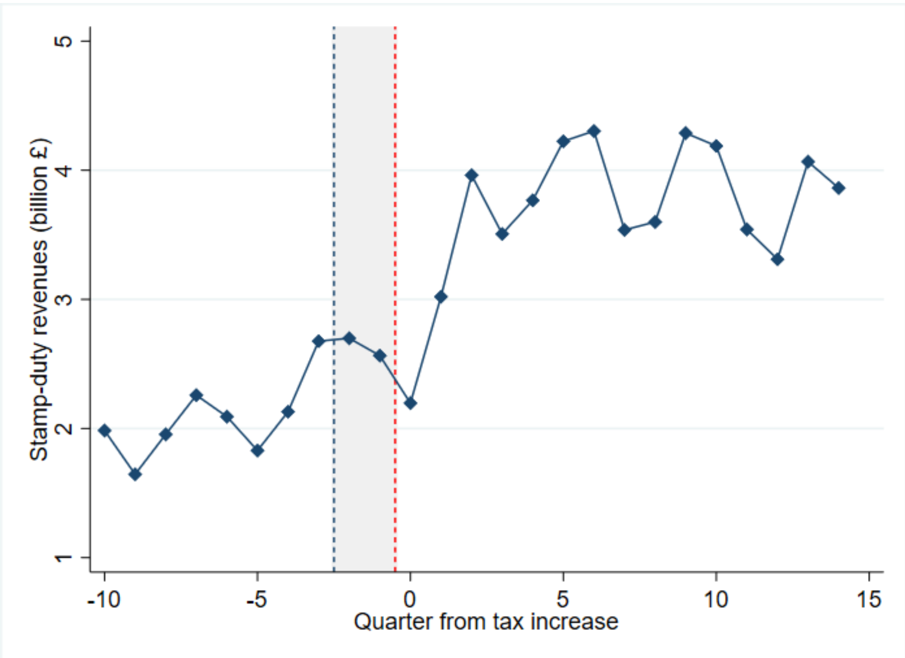


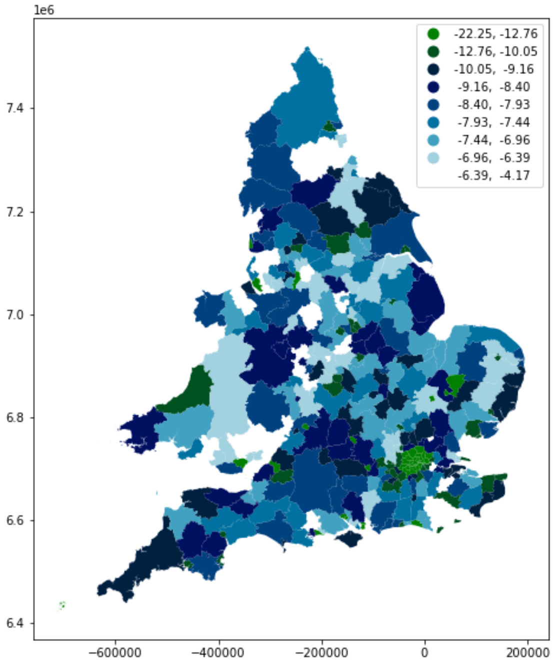
FIGURE D.4: Stamp-duty revenues



Notes: This Figure shows quarterly tax revenues from the SDLT from October 2013 to December 2019.

FIGURE D.5: Heterogeneous effects of the SDLT surcharge

(A) Heterogeneous effects on transactions



(B) Heterogeneous effects on paid prices

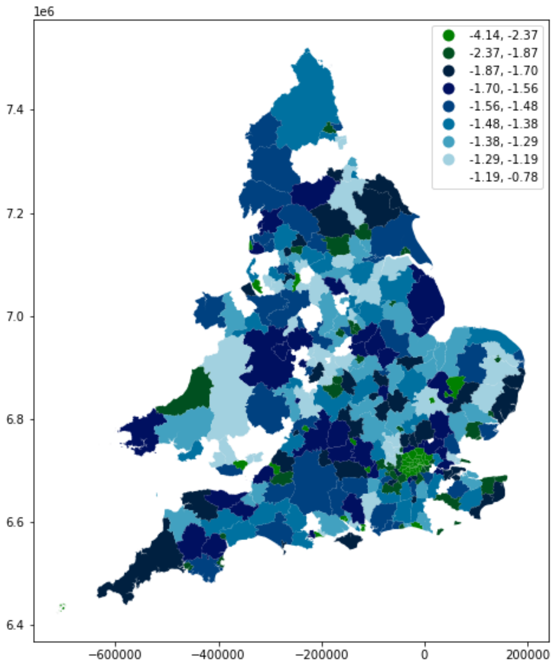
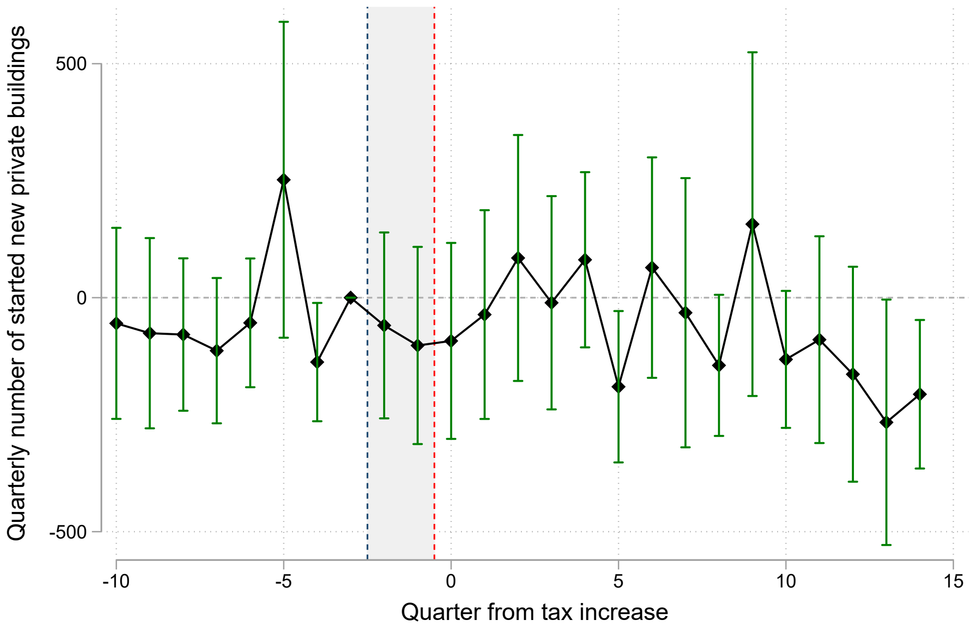
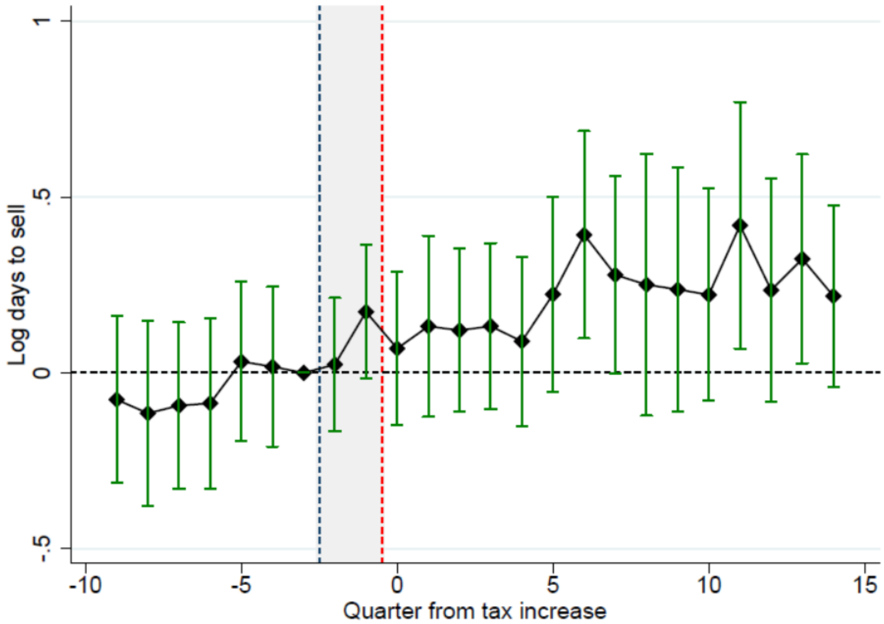


FIGURE D.6: Quarterly effect on new private buildings



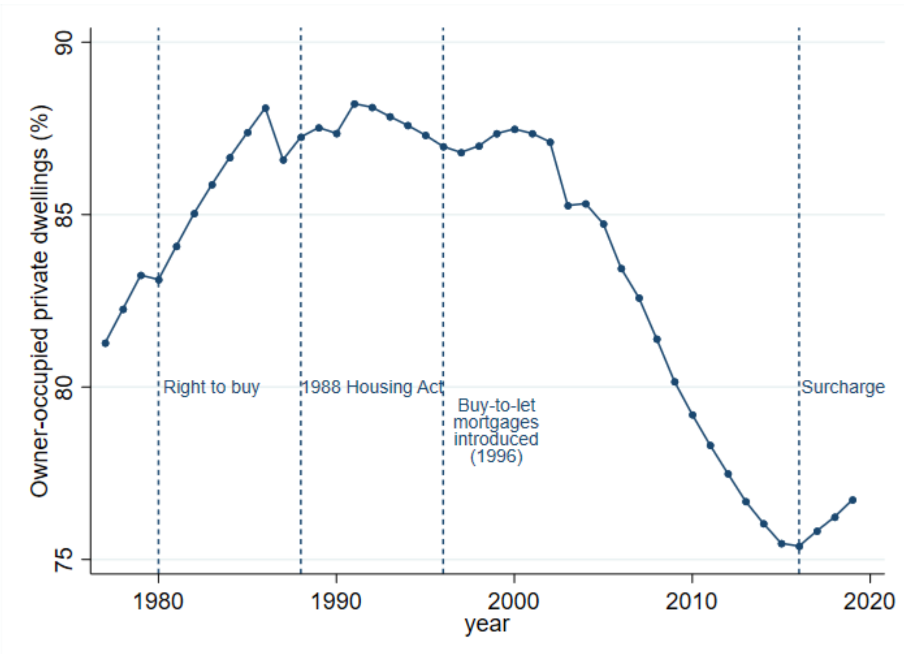
Notes: This figure reports point-estimates and 90% confidence intervals for θ_t from the OLS regression of Equation (4.2) using the quarterly number of new private residential buildings as the dependent variable. The horizontal axis shows the number of quarters from the introduction of the 3% surcharge. The shaded area represents the period between the surcharge announcement and its introduction.

FIGURE D.7: Effect on log days to sell



Notes: This figure reports point-estimates and 90% confidence intervals for θ_t from the OLS regression of Equation (4.2) using the log days to sell as the dependent variable. The horizontal axis shows the number of quarters from the introduction of the 3% surcharge. The shaded area represents the period between the surcharge announcement and its introduction.

FIGURE D.8: Home-ownership rate



Notes: This figure reports the share of private residential properties that were owner-occupied in the UK from 1977 to 2019.

Bibliography

Acconcia, Antonio and Carla Ronza (May 2022). *Women's Representation in Politics and Government Stability*. CSEF Working Papers 611. Centre for Studies in Economics and Finance (CSEF), University of Naples, Italy.

Agarwal, Sumit and Wenlan Qian (2014). "Consumption and Debt Response to Unanticipated Income Shocks: Evidence from a Natural Experiment in Singapore". eng. In: *The American economic review* 104.12, pp. 4205–4230. ISSN: 0002-8282.

Akhtari, Mitra, Natalie Bau, and Jean-William Laliberte (2022). "Affirmative Action and Pre-College Human Capital". conditionally accepted, AEJ: Applied Economics.

Albrecht, James, Pieter A. Gautier, and Susan Vroman (2016). "Directed search in the housing market". eng. In: *Review of economic dynamics* 19.January, pp. 218–231. ISSN: 1094-2025.

Aldy, Joseph E. and W. Kip Viscusi (Aug. 2008). "Adjusting the Value of a Statistical Life for Age and Cohort Effects". In: *The Review of Economics and Statistics* 90.3, pp. 573–581.

Alesina, Alberto et al. (1996). "Political Instability and Economic Growth". eng. In: *Journal of economic growth (Boston, Mass.)* 1.2, pp. 189–211. ISSN: 1381-4338.

Altonji, Joseph G, Anthony A Smith Jr, and Ivan Vidangos (2013). "Modeling earnings dynamics". In: *Econometrica* 81.4, pp. 1395–1454.

Anenberg, Elliot and Patrick Bayer (2020). "Endogenous Sources of Volatility in Housing Markets: The Joint Buyer-Seller Problem". eng. In: *International economic review (Philadelphia)* 61.3, pp. 1195–1228. ISSN: 0020-6598.

Arcidiacono, Peter (2005). "Affirmative action in higher education: How do admission and financial aid rules affect future earnings?" In: *Econometrica* 73.5, pp. 1477–1524.

Arcidiacono, Peter and Michael Lovenheim (2016). "Affirmative action and the quality-fit trade-off". In: *Journal of Economic Literature* 54.1, pp. 3–51.

Arcidiacono, Peter, Michael Lovenheim, and Maria Zhu (2015). "Affirmative action in undergraduate education". In: *Annual Review of Economics* 7.1, pp. 487–518.

Arcidiacono, Peter et al. (2011). "Does affirmative action lead to mismatch? A new test and evidence". In: *Quantitative Economics* 2.3, pp. 303–333.

Arcidiacono, Peter et al. (2014). "Affirmative action and university fit: Evidence from Proposition 209". In: *IZA Journal of Labor Economics* 3, pp. 1–29.

Arcidiacono, Peter et al. (2016). "College attrition and the dynamics of information revelation". In: *NBER Working Paper* 22325.

Arcidiacono, Peter et al. (2020). "Ex ante returns and occupational choice". In: *Journal of Political Economy* 128.12, pp. 4475–4522.

Ardington, Cally, Anne Case, and Victoria Hosegood (2009). "Labor supply responses to large social transfers: Longitudinal evidence from South Africa". In: *American economic journal: Applied economics* 1.1, pp. 22–48.

Atalah, Eduardo, Hugo Amigo, and Patricia Bustos (2014). "Does Chile's nutritional situation constitute a double burden?" eng. In: *The American journal of clinical nutrition* 100.6, 1623S–1627S. ISSN: 0002-9165.

Attanasio, Orazio P and Katja M Kaufmann (2014). "Education choices and returns to schooling: Mothers' and youths' subjective expectations and their role by gender". In: *Journal of Development Economics* 109, pp. 203–216.

Azmat, G et al. (2019). "What you don't know... Can't hurt you? A natural field experiment on relative performance feedback in higher education". In: *Management Science* 65.8, pp. 3714–3736.

Bagde, Surendrakumar, Dennis Epple, and Lowell Taylor (2016). "Does affirmative action work? Caste, gender, college quality, and academic success in India". In: *American Economic Review* 106.6, pp. 1495–1521.

Baker, Scott R., Nicholas Bloom, and Steven J. Davis (July 2016). "Measuring Economic Policy Uncertainty*". In: *The Quarterly Journal of Economics* 131.4, pp. 1593–1636. ISSN: 0033-5533. DOI: 10.1093/qje/qjw024.

Bank of England (2016). *Financial Policy Committee statement from its policy meeting, 23 March 2016*. URL: https://www.bankofengland.co.uk/statement/fpc/2016/financial-policy-committee-statement-march-2016.

— (2018). *Gilt repo (general collateral) rates*. URL: https://www.bankofengland.co.uk/boeapps/database/.

— (2021). *Bank of England core indicators: housing tools*. URL: https://www.bankofengland.co.uk/-/media/boe/files/core-indicators/housing-tools.xlsx.

Barclays plc (2023). *How to get a mortgage for your first home*. URL: https://www.barclays.co.uk/mortgages/first-time-buyers/guides/how-to-get-a-mortgage/.

Barham and Rowberry (2013). "Living longer: The effect of the Mexican conditional cash transfer program on elderly mortality". In: *Journal of Development Economics* 105.C, pp. 226–236.

Barro, Robert J. (1973). "The Control of Politicians: An Economic Model". eng. In: *Public choice* 14.1, pp. 19–42. ISSN: 0048-5829.

Bayer, Patrick, Christopher Geissler, and James Roberts (2011). "Speculators and Middlemen: The Role of Flippers in the Housing Market." In: *mimeo*.

BCIS (2022). *The value of building maintenance and repair costs*. URL: https://bcis-qa.383apps.com/news/building-maintenance-costs-contribution-to-uk-economy.

Becker, Gary S. (1960). "Demographic and Economic Change in Developed Countries". In: Princeton University Press. Chap. An Economic Analysis of Fertility, 209–240.

Behrman, Jere R et al. (2015). "Aligning learning incentives of students and teachers: Results from a social experiment in Mexican high schools". In: *Journal of Political Economy* 123.2, pp. 325–364.

Behrman, Jere R et al. (2016). "Teacher quality in public and private schools under a voucher system: The case of Chile". In: *Journal of Labor Economics* 34.2, pp. 319–362.

Besley, Timothy (2004). "PAYING POLITICIANS: THEORY AND EVIDENCE". eng. In: *Journal of the European Economic Association* 2.2-3, pp. 193–215. ISSN: 1542-4766.

— (2007). *Principled Agents?: The Political Economy of Good Government*. eng. The Lindahl Lectures. Oxford: Oxford University Press, pp. 1–288. ISBN: 9780199283910.

Besley, Timothy and Anne Case (1995). "Does Electoral Accountability Affect Economic Policy Choices? Evidence from Gubernatorial Term Limits". eng. In: *The Quarterly journal of economics* 110.3, pp. 769–798. ISSN: 0033-5533.

Besley, Timothy and Stephen Coate (1998). "Sources of Inefficiency in a Representative Democracy: A Dynamic Analysis". eng. In: *The American economic review* 88.1, pp. 139–156. ISSN: 0002-8282.

Besley, Timothy, Neil Meads, and Paolo Surico (2014). "The incidence of transaction taxes: Evidence from a stamp duty holiday". eng. In: *Journal of public economics* 119, pp. 61–70. ISSN: 0047-2727.

Best, Michael Carlos and Henrik Jacobsen Kleven (2018). "Housing Market Responses to Transaction Taxes: Evidence From Notches and Stimulus in the U.K". eng. In: *The Review of economic studies* 85.1 (302), pp. 157–193. ISSN: 0034-6527.

Black, Dan A et al. (2013). "Are children "normal"?" In: *The review of economics and statistics* 95.1, pp. 21–33.

Black, Sandra E, Jeffrey T Denning, and Jesse Rothstein (2023). "Winners and losers? The effect of gaining and losing access to selective colleges on education and labor market outcomes". In: *American Economic Journal: Applied Economics* 15.1, pp. 26–67.

Bleemer, Zachary (2021). "Top percent policies and the return to postsecondary selectivity". In: *Research & Occasional Paper Series: CSHE* 1.

— (2022). "Affirmative action, mismatch, and economic mobility after California's Proposition 209". In: *The Quarterly Journal of Economics* 137.1, pp. 115–160.

Bloom, Nicholas (May 2014). "Fluctuations in Uncertainty". In: *Journal of Economic Perspectives* 28.2, pp. 153–76. DOI: 10.1257/jep.28.2.153.

Bloom, Nick, Stephen Bond, and John Van Reenen (2007). "Uncertainty and Investment Dynamics". In: *The Review of Economic Studies* 74.2, pp. 391–415. ISSN: 0034-6527. DOI: 10.1111/j.1467-937X.2007.00426.x. eprint: https://academic.oup.com/restud/article-pdf/74/2/391/18342850/74-2-391.pdf.

Bobba, Matteo and Veronica Frisancho (2019). "Perceived Ability and School Choices". In: *TSE Working Paper* 16-660.

Bodoh-Creed, Aaron L and Brent R Hickman (2018). "College assignment as a large contest". In: *Journal of Economic Theory* 175, pp. 88–126.

— (2019). "Identifying the sources of returns to college education using affirmative action". Mimeo, Queen's University.

Boneva, Teodora and Christopher Rauh (2020). "Socio-economic Gaps in University Enrollment: The Role of Perceived Pecuniary and Non-Pecuniary Returns". In: *HCEO Working Paper 2017-080*.

Bonica, Adam (2014). "Mapping the Ideological Marketplace". eng. In: *American journal of political science* 58.2, pp. 367–386. ISSN: 0092-5853.

Bordo, Michael D., John V. Duca, and Christoffer Koch (2016). "Economic policy uncertainty and the credit channel: Aggregate and bank level U.S. evidence over several decades". In: *Journal of Financial Stability* 26, pp. 90–106. ISSN: 1572-3089. DOI: https://doi.org/10.1016/j.jfs.2016.07.002.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess (2021). "Revisiting Event Study Designs: Robust and Efficient Estimation". eng. In: *Working paper*.

Bowland, Brad J. and John C. Beghin (2001). "Robust Estimates Of Value Of A Statistical Life For Developing Economies: An Application To Pollution And Mortality In Santiago". In: *Journal of Policy Modeling* 23, pp. 385–396.

Bracke, Philippe (2021). "How Much Do Investors Pay for Houses?" eng. In: *Real estate economics* 49.S1, pp. 41–73. ISSN: 1080-8620.

Braveman, Paula A. et al. (2010). “Socioeconomic disparities in health in the United States: what the patterns tell us”. In: *American journal of public health* 100.S1, S186–S196.

Brønnum-Hansen, Henrik and Mikkel Baadsgaard (2012). “Widening social inequality in life expectancy in Denmark. A register-based study on social composition and mortality trends for the Danish population”. In: *BMC Public Health* 12.1, p. 994.

Browning, Martin and Costas Meghir (1991). “The Effects of Male and Female Labor Supply on Commodity Demands”. In: *Econometrica* 59.4, pp. 925–951.

Bruins, Marianne et al. (2018). “Generalized indirect inference for discrete choice models”. In: *Journal of Econometrics* 205.1, pp. 177–203.

Byrd, Richard H, Jorge Nocedal, and Richard A Waltz (2006). “Knitro: An integrated package for nonlinear optimization”. In: *Large-Scale Nonlinear Optimization*. Springer, pp. 35–59.

Bø, Erlend Eide (2021). “Buy to let: The role of rental markets in housing booms”. In: *Housing lab working paper series*.

Caliendo, Marco et al. (2018). “The short-run employment effects of the German minimum wage reform”. eng. In: *Labour economics* 53, pp. 46–62. ISSN: 0927-5371.

Callaway, Brantly, Andrew Goodman-Bacon, and Pedro H. C Sant'Anna (2021). “Difference-in-Differences with a Continuous Treatment”. eng. In: *Working paper*.

Calonico, Sebastian, Matia D. Cattaneo, and Rocio Titiunik (2014). “Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs”. In: *Econometrica* 82.6, pp. 2295–2326.

Camera dei Deputati (1997). *Regolamento della Camera. Parte I. Capo III. Dei Gruppi parlamentari*. http://leg14.camera.it/cost_reg_funz/663/912/928/listaArticoli.asp. [Online; accessed 29-May-2022].

— (2001). *XIV legislatura. Deputati. Trattamento economico*. https://leg14.camera.it/deputatism/4385/documentotesto.asp. [Online; accessed 29-May-2022].

— (2007). *XV legislatura. Deputati. Trattamento economico*. http://leg15.camera.it/deputatism/4385/documentotesto.asp. [Online; accessed 29-May-2022].

— (2013). *XVI legislatura. Deputati. Trattamento economico*. https://leg16.camera.it/383?conoscerelacamera=4. [Online; accessed 29-May-2022].

— (2022). *Parliamentary confidence in the government*. https://en.camera.it/4?scheda_informazioni=27. [Online; accessed 29-May-2022].

Camerer, Colin F, Teck-Hua Ho, and Juin-Kuan Chong (2004). “A cognitive hierarchy model of games”. In: *Quarterly Journal of Economics* 119.3, pp. 861–898.

Canen, Nathan, Chad Kendall, and Francesco Trebbi (2020a). "Unbundling Polarization". eng. In: *Econometrica* 88.3, pp. 1197–1233. ISSN: 0012-9682.

Canen, Nathan J, Chad Kendall, and Francesco Trebbi (Dec. 2020b). *Political Parties as Drivers of U.S. Polarization: 1927-2018*. Working Paper 28296. National Bureau of Economic Research. DOI: 10.3386/w28296. URL: http://www.nber.org/papers/w28296.

Card, David (1992). "USING REGIONAL VARIATION IN WAGES TO MEASURE THE EFFECTS OF THE FEDERAL MINIMUM-WAGE". eng. In: *Industrial and labor relations review* 46.1, pp. 22–37. ISSN: 0019-7939.

Carozzi, Felipe, Davide Cipullo, and Luca Repetto (2022). "Political Fragmentation and Government Stability: Evidence from Local Governments in Spain". eng. In: *American economic journal. Applied economics* 14.2, pp. 23–. ISSN: 1945-7790.

Carroll, Christopher et al. (2017). "The distribution of wealth and the marginal propensity to consume". eng. In: *Quantitative economics* 8.3, pp. 977–1020. ISSN: 1759-7323.

Case, Anne (2004). "Does Money Protect Health Status? Evidence from South African Pensions". In: University of Chicago Press. Chap. 7, pp. 287–312.

Case, Anne and Alicia Menendez (2007). "Does money empower the elderly? Evidence from the Agincourt demographic surveillance site, South Africa". In: *Scandinavian Journal of Public Health, 2007* 35.(Suppl 69), 157–164.

Caselli, Francesco and Massimo Morelli (2004). "Bad Politicians". In: *Journal of Public Economics* 88, 759–782.

Cattaneo, Matias D, Brigham R Frandsen, and Rocio Titiunik (2015). "Randomization inference in the regression discontinuity design: An application to party advantages in the US Senate". In: *Journal of Causal Inference* 3.1, pp. 1–24.

Cattaneo, Matias D, Michael Jansson, and Xinwei Ma (2019). "Simple local polynomial density estimators". In: *Journal of the American Statistical Association*, pp. 1–7.

Cellini, Stephanie Riegg, Fernando Ferreira, and Jesse Rothstein (2010). "The Value of School Facility Investments: Evidence from a Dynamic Regression Discontinuity Design". In: *The Quarterly Journal of Economics* 125.1, pp. 215–261. ISSN: 00335533, 15314650. URL: http://www.jstor.org/stable/40506281.

Cesarini, David et al. (2016). "Wealth, health, and child development: Evidence from administrative data on Swedish lottery players". In: *The Quarterly Journal of Economics* 131.2, pp. 687–738.

Chaisemartin, Clément de and Xavier D'Haultfœuille (2018). "Fuzzy Differences-in-Differences". eng. In: *The Review of economic studies* 85.2 (303), pp. 999–1028. ISSN: 0034-6527.

Chaisemartin, Clément de and Xavier D'Haultfœuille (2020). "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects". eng. In: *The American economic review* 110.9, pp. 2964–2996. ISSN: 0002-8282.

Chandra, Ranjit Kumar (1997). "Nutrition and the immune system: an introduction". In: *The American journal of clinical nutrition* 66.2, 460S–463S.

Cheng, Lingguo et al. (2016). "The health implications of social pensions: Evidence from China's new rural pension scheme". In: *Journal of Comparative Economics* 46.1, 1–25.

Chetty, Raj et al. (2016). "The association between income and life expectancy in the United States, 2001-2014". In: *Jama* 315.16, pp. 1750–1766.

Conley, T.G (1999). "GMM estimation with cross sectional dependence". eng. In: *Journal of econometrics*. Journal of Econometrics 92.1, pp. 1–45. ISSN: 0304-4076.

Consumer Data Research Centre (2020a). *Data Profile - WhenFresh/Zoopla Rentals*. https://data.cdrc.a

— (2020b). *Data Profile - WhenFresh/Zoopla Transaction*. https://data.cdrc.ac.uk/node/1631.

Cooper, Ryan, Antonia Sanhueza, and Michela Tincani (2020). "Evaluación Experimental Efecto del Programa PACE en Matrícula Universitaria El 2019". Report of the Chilean Budget Office, Ministry of Finance.

Cooper, Ryan et al. (2019). "Evaluación de Impacto del Programa PACE". Report of the Chilean Ministry of Education.

Cooper, Ryan et al. (2022). "The impacts of preferential college admissions for the disadvantaged: experimental evidence from the PACE programme in Chile". Institute for Fiscal Studies Working Paper W22/19.

Costa-Gomes, Miguel A and Vincent P Crawford (2006). "Cognition and behavior in two-person guessing games: An experimental study". In: *American economic review* 96.5, pp. 1737–1768.

Costa-Gomes, Miguel A and Klaus G Zauner (2003). "Learning, non-equilibrium beliefs, and non-pecuniary payoffs in an experimental game". In: *Economic Theory* 22.2, pp. 263–288.

Cotton, Christopher S, Brent R Hickman, and Joseph P Price (2020). "Affirmative Action and Human Capital Investment: Evidence from a Randomized Field Experiment". In: *Journal of Labor Economics, forthcoming*.

Council of Mortgage Lenders (2022). *Arrears and possessions (in Archives)*. URL: https://www.ukfinance.org.uk/data-and-research/data/mortgages/arrears-and-possessions.

Cox, Donald and Emmanuel Jimenez (Jan. 1992). "Social Security and Private Transfers in Developing Countries: The Case of Peru". In: *The World Bank Economic Review* 6.1, 155–169.

Crawford, Vincent P and Nagore Iriberri (2007). "Level-k auctions: Can a nonequilibrium model of strategic thinking explain the winner's curse and overbidding in private-value auctions?" In: *Econometrica* 75.6, pp. 1721–1770.

Dachis, Ben, Gilles Duranton, and Matthew A Turner (2012). "The effects of land transfer taxes on real estate markets: evidence from a natural experiment in Toronto". eng. In: *Journal of economic geography* 12.2, pp. 327–354. ISSN: 1468-2702.

Daconto, Claudia (2022). *Pensioni e vitalizi dei parlamentari: come funzionano*. Panorama.

Daugherty, Lindsay, Paco Martorell, and Isaac McFarlin (2014). "Percent plans, automatic admissions, and college outcomes". In: *IZA Journal of Labor Economics* 3, pp. 1–29.

De Nederlandsche Bank (2018). *Financial stability report: Spring 2018*.

De Santis, Valeria (2020). *Indennità e vitalizi: per uno studio dell'art. 69 della Costituzione*. Studi di Diritto Pubblico. Franco Angeli.

Delavande, Adeline and Basit Zafar (2019). "University choice: the role of expected earnings, nonpecuniary outcomes, and financial constraints". In: *Journal of Political Economy* 127.5, pp. 2343–2393.

Dell'Ariccia, Giovanni and Pietro Garibaldi (2005). "Gross Credit Flows". eng. In: *The Review of economic studies* 72.3, pp. 665–685. ISSN: 0034-6527.

Department for Levelling Up, Housing and Communities (2022a). *Energy Performance of Buildings Data: England and Wales*. URL: https://epc.opendatacommunities.org/login.

— (2022b). *Local authority revenue expenditure and financing*. URL: https://www.gov.uk/government/collections.

d'Haultfoeuille, Xavier, Christophe Gaillac, and Arnaud Maurel (2021). "Rationalizing rational expectations: Characterizations and tests". In: *Quantitative Economics* 12.3, pp. 817–842.

Diermeier, Daniel, Michael Keane, and Antonio Merlo (2005). "A Political Economy Model of Congressional Careers". eng. In: *The American economic review* 95.1, pp. 347–373. ISSN: 0002-8282.

Diermeier, Daniel and Antonio Merlo (2000). "Government Turnover in Parliamentary Democracies". eng. In: *Journal of economic theory*. Journal of Economic Theory 94.1, pp. 46–79. ISSN: 0022-0531.

Dipartimento per le Riforme Istituzionali (2022). *La riduzione del numero dei parlamentari*. https://www.riformeistituzionali.gov.it/it/la-riduzione-del-numero-dei-parlamentari. [Online; accessed 29-May-2022].

División de Acceso y Desarrollo Social (2019). *Mapa de Vulnerabilidad Energética*. Gobierno de Chile.

Dolton, Peter, Chiara Rosazza Bondibene, and Jonathan Wadsworth (2010). "The UK National Minimum Wage in Retrospect". eng. In: *Fiscal studies* 31.4, pp. 509–534. ISSN: 0143-5671.

Duflo, Esther (2000). "Child health and household resources in South Africa: evidence from the old age pension program". In: *American Economic Review* 90.2, pp. 393–398.

— (2003). "Grandmothers and Granddaughters: Old-Age Pensions and Intrahousehold Allocation in South Africa". In: *The World Bank Economic Review. Oxford University Press* 17.1, pp. 1–25.

Díaz, Antonia and Belén Jerez (2013). "House Prices, Sales, And Time on the Market: A Search-Theoretic Framework". eng. In: *International economic review* 54.3, pp. 837–872. ISSN: 0020-6598.

D'Addio, Anna Cristina and Marco Mira d'Ercole (2006). "Policies, institutions and fertility rates: a panel data analysis for OECD countries". In: *OECD Economic Studies* 2005.2, pp. 7–45.

Edmonds, Eric V. (2006). "Child labor and schooling responses to anticipated income in South Africa". In: *Journal of Development Economics* 81, pp. 286–414.

Eerola, Essi et al. (2021). "Revisiting the effects of housing transfer taxes". eng. In: *Journal of urban economics* 124, pp. 103367–. ISSN: 0094-1190.

EHCS (2004). *English House Condition Survey (1996-2004)*. URL: https://beta.ukdataservice.ac.uk/datacatalogue/studies/study?id=6104.

Eisenhauer, Philipp, James J Heckman, and Stefano Mosso (2015). "Estimation of dynamic discrete choice models by maximum likelihood and the simulated method of moments". In: *International Economic Review* 56.2, pp. 331–357.

FCA (2019). *The mortgage market today – analysis of an evolving sector*. URL: https://www.fca.org.uk/insight/mortgage-market-today-fca-analysis-evolving-sector.

Feeney, Kevin (2017). *Cash Transfers and Adult Mortality: Evidence from Pension Policies*. UC Berkeley PhD Thesis.

— (2018). *Dying for More: Cash Transfers and Health*. Mimeo.

Ferejohn, John (1986). "Incumbent Performance and Electoral Control". eng. In: *Public choice* 50.1/3, pp. 5–25. ISSN: 0048-5829.

Finan, Frederico and Claudio Ferraz (2009). *Motivating Politicians: The Impacts of Monetary Incentives on Quality and Performance*. eng. National Bureau of Economic Research.

Fiolet, Thibault et al. (2018). "Consumption of ultra-processed foods and cancer risk: results from NutriNet-Santé prospective cohort". In: *bmj* 360.

Fisman, Raymond et al. (2015). “LABOR SUPPLY OF POLITICIANS”. eng. In: *Journal of the European Economic Association* 13.5, pp. 871–905. ISSN: 1542-4766.

Fitzpatrick, Maria D and Timothy J Moore (2018). “The mortality effects of retirement: Evidence from Social Security eligibility at age 62”. In: *Journal of Public Economics* 157, pp. 121–137.

Fondazione Openpolis (2017). *Patrimoni trasparenti. Open data.* http://patrimoni.openpolis.it/. [Online; accessed 29-May-2022].

Fondazione Rodolfo Debenedetti (2009). *Membri del Parlamento Italiano. Dataset su Deputati italiani (1948-2008).* https://www.frdb.org/dati/membri-del-parlamento-italiano/. [Online; accessed 29-May-2022].

Frandsen, Brigham R (2017). “Party Bias in Union Representation Elections: Testing for Manipulation in the Regression Discontinuity Design when the Running Variable is Discrete”. eng. In: *Advances in econometrics*. Vol. 38. Emerald Publishing Limited, pp. 281–315. ISBN: 9781787143906.

Fritzsche, Carolin and Lars Vandrei (2019). “The German real estate transfer tax: Evidence for single-family home transactions”. eng. In: *Regional science and urban economics* 74, pp. 131–143. ISSN: 0166-0462.

Fu, Yuming, Wenlan Qian, and Bernard Yeung (2016). “Speculative Investors and Transactions Tax: Evidence from the Housing Market”. eng. In: *Management science* 62.11, pp. 3254–3270. ISSN: 0025-1909.

Fullerton, Don and Gilbert E Metcalf (2002). “Chapter 26 Tax incidence”. eng. In: *Handbook of Public Economics*. Vol. 4. Elsevier B.V, pp. 1787–1872. ISBN: 9780444823151.

Fusani, Claudia (2007). *Stretta su vitalizi e viaggi. Via ai tagli di Camera e Senato*. La Repubblica.

Gabrovski, Miroslav and Victor Ortego-Marti (2019). “The cyclical behavior of the Beveridge Curve in the housing market”. eng. In: *Journal of economic theory* 181, pp. 361–381. ISSN: 0022-0531.

— (2021). “Search and credit frictions in the housing market”. eng. In: *European economic review* 134, p. 103699. ISSN: 0014-2921.

— (2023). “Home Construction Financing and Search Frictions in the Housing Market”. In: *Mimeo*.

Gagliarducci, Stefano and Tommaso Nannicini (2013). “Do Better Paid Politicians Perform Better? Disentangling Incentives from Selection”. eng. In: *Journal of the European Economic Association* 11.2, pp. 369–398. ISSN: 1542-4766.

Gagliarducci, Stefano and M. Daniele Paserman (2012). “Gender Interactions within Hierarchies: Evidence from the Political Arena”. eng. In: *The Review of economic studies* 79.3, pp. 1021–1052. ISSN: 0034-6527.

Gao, Zhenyu, Michael Sockin, and Wei Xiong (2020). "Economic consequences of housing speculation". eng. In: *The Review of financial studies* 33.11, pp. 5248–5287. ISSN: 0893-9454.

Garre-Olmo, Josep et al. (2013). "Prevalence of frailty phenotypes and risk of mortality in a community-dwelling elderly cohort". In: *Age and Aging* 42.1, pp. 46–51.

Gelber, Alexander M., Adam Isen, and Jae Song (2016). *The Effect of Pension Income on Elderly Earnings: Evidence from Social Security and Full Population Data*. Unpublished.

Gelman, Andrew and John Carlin (2014). "Beyond power calculations: Assessing type S (sign) and type M (magnitude) errors". In: *Perspectives on Psychological Science* 9.6, pp. 641–651.

Gelman, Andrew and Guido Imbens (2019). "Why High-Order Polynomials Should Not Be Used in Regression Discontinuity Designs". eng. In: *Journal of business and economic statistics* 37.3, pp. 447–456. ISSN: 0735-0015.

Genesove, David and Lu Han (2012). "Search and matching in the housing market". eng. In: *Journal of urban economics* 72.1, pp. 31–45. ISSN: 0094-1190.

Gentzkow, Matthew, Jesse M. Shapiro, and Matt Taddy (2019). "Measuring Group Differences in High-Dimensional Choices: Method and Application to Congressional Speech". eng. In: *Econometrica* 87.4, pp. 1307–1340. ISSN: 0012-9682.

Giustinelli, Pamela (2016). "Group decision making with uncertain outcomes: Unpacking child–parent choice of the high school track". In: *International Economic Review* 57.2, pp. 573–602.

Golightly, Eleanor (2019). "Does College Access Increase High School Effort? Evaluating the Impact of the Texas Top 10% Rule". In: *Mimeo, U of Texas Austin*.

Goodman, Joshua, Michael Hurwitz, and Jonathan Smith (2017). "Access to 4-year public colleges and degree completion". In: *Journal of Labor Economics* 35.3, pp. 829–867.

Goodman-Bacon, Andrew (2021). "Difference-in-differences with variation in treatment timing". eng. In: *Journal of econometrics* 225.2, pp. 254–277. ISSN: 0304-4076.

Greene, William H., Mark N. Harris, and Bruce Hollingsworth (2015). "Inflated Responses in Measures of Self-Assessed Health". In: *American Journal of Health Economics* 1.4, 461–493.

Grembi, Veronica, Tommaso Nannicini, and Ugo Troiano (2016). "Do Fiscal Rules Matter?" eng. In: *American economic journal. Applied economics* 8.3, pp. 1–30. ISSN: 1945-7782.

Groseclose, T. and K. Krehbiel (1994). "Golden Parachutes, Rubber Checks, and Strategic Retirements from the 102d House". eng. In: *American journal of political science* 38.1, pp. 75–99. ISSN: 0092-5853.

Haan, Wouter J den, Garey Ramey, and Joel Watson (2003). "Liquidity flows and fragility of business enterprises". eng. In: *Journal of monetary economics*. Journal of Monetary Economics 50.6, pp. 1215–1241. ISSN: 0304-3932.

Hakimov, Rustamdjan, Renke Schmacker, and Camille Terrier (2022). *Confidence and college applications: Evidence from a randomized intervention*. Tech. rep. WZB Discussion Paper.

Halket, Jonathan and Matteo Pignatti Morano di Custoza (2015). "Homeownership and the scarcity of rentals". eng. In: *Journal of monetary economics* 76, pp. 107–123. ISSN: 0304-3932.

Hall, Richard L. and Robert P. Van Houweling (1995). "Avarice and Ambition in Congress: Representatives' Decisions to Run or Retire from the U.S. House". eng. In: *The American political science review* 89.1, pp. 121–136. ISSN: 0003-0554.

Han, Lu, Rachel Ngai, and Kevin Sheedy (2021). "To Own or to Rent? The Effects of Transaction Taxes on Housing Markets". In: *mimeo*.

Han, Lu and William C. Strange (2015). "Handbook of Regional and Urban Economics, vol. 5". In: Elsevier. Chap. Ch.13 The Microstructure of Housing Markets: Search, Bargaining, and Brokerage, pp. 813–886.

Hastings, Justine, Christopher A Neilson, and Seth D Zimmerman (2015). *The effects of earnings disclosure on college enrollment decisions*. Tech. rep. National Bureau of Economic Research.

Hastings, Justine S et al. (2016). "(Un)informed college and major choice: Evidence from linked survey and administrative data". In: *Economics of Education Review* 51, pp. 136–151.

Hawton, Annie et al. (2011). "The impact of social isolation on the health statusand health-related quality of life of older people". In: *Qualitative Life Research* 20, pp. 57–67.

Head, Allen, Huw Lloyd-Ellis, and Hongfei Sun (2014). "Search, Liquidity, and the Dynamics of House Prices and Construction." In: *The American Economic Review* 104.4, pp. 1172–1210.

Heckman, James and Burton Singer (1984). "A method for minimizing the impact of distributional assumptions in econometric models for duration data". In: *Econometrica*, pp. 271–320.

Hetherington, Marc J. (2015). *Why Washington won't work : polarization, political trust, and the governing crisis / Marc J. Hetherington, Thomas J. Rudolph.* eng. Chicago studies in American politics. Chicago: The University of Chicago Press. ISBN: 9780226299181.

Hilber, Christian A.L. and Teemu Lyytikäinen (2017). "Transfer taxes and household mobility: Distortion on the housing or labor market?" eng. In: *Journal of urban economics* 101, pp. 57–73. ISSN: 0094-1190.

Hinrichs, Peter (2014). "Affirmative action bans and college graduation rates". In: *Economics of Education Review* 42, pp. 43–52.

HM Land Registry (2019). *Price Paid Data*. Available at: https://www.gov.uk/guidance/about-the-price-paid-data.

— (2022a). *Overseas companies that own property in England and Wales*. URL: https://use-land-property-data.service.gov.uk/datasets/ocod.

— (2022b). *Price Paid Data*. URL: https://www.gov.uk/government/statistical-data-sets/price-paid-data-downloads.

HM Treasury (2015). *Spending Review and Autumn Statement 2015: documents*. URL: https://www.gov.uk/government/publications/spending-review-and-autumn-statement-2015-documents.

— (2016). *Higher rates of Stamp Duty Land Tax (SDLT) on purchases of additional residential properties*. URL: https://www.gov.uk/government/consultations/consultation-on-higher-rates-of-stamp-duty-land-tax-sdlt-on-purchases-of-additional-residential-properties/higher-rates-of-stamp-duty-land-tax-sdlt-on-purchases-of-additional-residential-properties.

Hoffman, Rasmus (2008). *Socioeconomic Differences in Old Age Mortality*. Vol. 25. Springer Science & Business Media.

Hopkins, Ed and Tatiana Kornienko (2004). "Running to keep in the same place: Consumer choice as a game of status". In: *American Economic Review* 94.4, pp. 1085–1107.

Horn, Catherine L. and Stella M. Flores (2003). "Percent Plans in College Admissions: A Comparative Analysis of Three States' Experiences". The Civil Rights Project, Harvard University.

— (2015). "Texas Top Ten Percent Plan: How It Works, What Are Its Limits, and Recommendations to Consider". Educational Testing Service.

Housing Act (1988). URL: https://www.legislation.gov.uk/ukpga/1988/50/section/21.

Hu, Guizhou and Patricia A Cassano (2000). "Antioxidant nutrients and pulmonary function: the third national health and nutrition examination survey (NHANES III)". In: *American journal of epidemiology* 151.10, pp. 975–981.

Hurria, Arti et al. (2011). "Predicting chemotherapy toxicity in older adults with cancer: a prospective multicenter study". In: *Journal of Clinical Oncology* 29.25, p. 3457.

Huttunen, Kristiina and Jenni Kellokumpu (2016). "The effect of job displacement on couples' fertility decisions". In: *Journal of Labor Economics* 34.2, pp. 403–442.

Ichino, Andrea, Aldo Rustichini, and Giulio Zanella (2022). *College education, intelligence, and disadvantage: policy lessons from the UK in 1960-2004*. Tech. rep. CEPR Discussion Paper 17284.

Iizuka, Toshiaki et al. (2021). "False alarm? Estimating the marginal value of health signals". eng. In: *Journal of public economics* 195, pp. 104368–. ISSN: 0047-2727.

Instituto de Previsión Social (2017). *Postulantes a la Pensión Básica Solidaria 2011-2012*. Accessed July 25, 2017.

Ioannides, Yannis M. and Jeffrey E. Zabel (2019). *Recent Developments in the Economics of Housing*. Edward Elgar.

ISTAT (2013). *Trattamenti pensionistici e beneficiari*. https://www.istat.it/it/archivio/87850. [Online; accessed 29-May-2022].

Iyengar, Shanto and Sean J. Westwood (2015). "Fear and Loathing across Party Lines: New Evidence on Group Polarization: FEAR AND LOATHING ACROSS PARTY LINES". eng. In: *American journal of political science* 59.3, pp. 690–707. ISSN: 0092-5853.

Jensen, Robert T. (2003). "Do private transfers 'displace' the benefits of public transfers? Evidence from South Africa". In: *Journal of Public Economics* 88, pp. 89–112.

Jensen, Robert T. and Kaspar Richter (2003). "The health implications of social security failure: evidence from the Russian pension crisis". In: *Journal of Public Economics* 88, 209– 236.

Jones, L. E. and M. Tertilt (2008). "Frontiers of Family Economics". In: Emerald Press. Chap. 5 An Economic History of Fertility in the United States: 1826–1960, 165–230.

Juarez, Laura (2009). "Crowding out of private support to the elderly: Evidence from a demogrant in Mexico". In: *Journal of Public Economics* 93.3-4, pp. 454–463.

Kalwij, Adriaan (2010). "The impact of family policy expenditure on fertility in western Europe". In: *Demography* 47.2, pp. 503–519.

Kapor, Adam (2020). "Distributional effects of race-blind affirmative action". mimeo Princeton University.

Kapor, Adam, Mohit Karnani, and Christopher Neilson (2022). "Aftermarket frictions and the cost of off-platform options in centralized assignment mechanisms". In: *Industrial Relations Working Paper Series (645)*.

Kapor, Adam J, Christopher A Neilson, and Seth D Zimmerman (2020). "Heterogeneous beliefs and school choice mechanisms". In: *American Economic Review* 110.5, pp. 1274–1315.

Keane, Michael P. and Antonio Merlo (2010). “Money, Political Ambition, and the Career Decisions of Politicians”. eng. In: *American economic journal. Microeconomics* 2.3, pp. 186–215. ISSN: 1945-7669.

Keane, Michael P and Kenneth I Wolpin (1994). “The solution and estimation of discrete choice dynamic programming models by simulation and interpolation: Monte Carlo evidence”. In: *Review of Economics and Statistics* 76.4, pp. 648–672.

— (1997). “The career decisions of young men”. In: *Journal of Political Economy* 105.3, pp. 473–522.

Kearney, Melissa S and Riley Wilson (2018). “Male earnings, marriageable men, and nonmarital fertility: Evidence from the fracking boom”. In: *Review of Economics and Statistics* 100.4, pp. 678–690.

Kelly, Morgan (2020). “Direct Standard Errors for Regressions with Spatially Autocorrelated Residuals”. In: *mimeo*.

Kemp, Peter A (2015). “Private Renting After the Global Financial Crisis”. eng. In: *Housing studies* 30.4, pp. 601–620. ISSN: 0267-3037.

Klaauw, Wilbert Van der (2012). “On the use of expectations data in estimating structural dynamic choice models”. In: *Journal of Labor Economics* 30.3, pp. 521–554.

Knoll, Katharina, Moritz Schularick, and Thomas Steger (2017). “No Price Like Home: Global House Prices, 1870-2012”. eng. In: *The American economic review* 107.2, pp. 331–353. ISSN: 0002-8282.

Kolesár, Michal and Christoph Rothe (2018). “Inference in Regression Discontinuity Designs with a Discrete Running Variable”. eng. In: *The American economic review* 108.8, pp. 2277–2304. ISSN: 0002-8282.

Kopczuk, Wojciech and David Munroe (2015). “Mansion Tax: The Effect of Transfer Taxes on the Residential Real Estate Market”. eng. In: *American economic journal. Economic policy* 7.2, pp. 214–257. ISSN: 1945-7731.

Koster, Hans R.A. and Edward W. Pinchbeck (Feb. 2022). “How Do Households Value the Future? Evidence from Property Taxes”. In: *American Economic Journal: Economic Policy* 14.1, pp. 207–39. DOI: 10.1257/pol.20200443. URL: https://www.aeaweb.org/articles?id=10.1257/pol.20200443.

Kotakorpi, Kaisa and Panu Poutvaara (2011). “Pay for politicians and candidate selection: An empirical analysis”. eng. In: *Journal of public economics*. Journal of Public Economics 95.7, pp. 877–885. ISSN: 0047-2727.

Lee, David S (2009). “Training, wages, and sample selection: Estimating sharp bounds on treatment effects”. In: *Review of Economic Studies* 76.3, pp. 1071–1102.

Lee, David S and Thomas Lemieux (2010). “Regression discontinuity designs in economics”. In: *Journal of economic literature* 48.2, pp. 281–355.

Leyshon, Andrew and Shaun French (2009). “We All Live in a Robbie Fowler House: The Geographies of the Buy to Let Market in the UK”. eng. In: *British journal of politics and international relations* 11.3, pp. 438–460. ISSN: 1369-1481.

Lindahl, Mikael (2005). “Estimating the Effect of Income on Health and Mortality Using Lottery Prizes as an Exogenous Source of Variation in Income”. In: *The Journal of Human Resources* XL.1, pp. 144–168.

Lindo, Jason M (2010). “Are children really inferior goods? Evidence from displacement-driven income shocks”. In: *Journal of Human Resources* 45.2, pp. 301–327.

Liu, Antung A et al. (2018). “Vehicle ownership restrictions and fertility in Beijing”. In: *Journal of Development Economics* 135, pp. 85–96.

Ljungqvist, Lars and Thomas J. Sargent (2017). “The Fundamental Surplus”. eng. In: *The American economic review* 107.9, pp. 2630–2665. ISSN: 0002-8282.

Lloyds Bank plc (2022). *Agreement in Principle*. URL: https://www.lloydsbank.com/mortgages/agreement-in-principle.html.

Long, Mark C, Victor Saenz, and Marta Tienda (2010). “Policy transparency and college enrollment: did the Texas top ten percent law broaden access to the public flagships?” In: *The ANNALS of the American Academy of Political and Social Science* 627.1, pp. 82–105.

Lovenheim, Michael F and Kevin J Mumford (2013). “Do family wealth shocks affect fertility choices? Evidence from the housing market”. In: *Review of Economics and Statistics* 95.2, pp. 464–475.

LSL property services plc (2022). Accessed: 22 December 2020). URL: https://www.lauristons.com/buy/guides/buyer-guides-the-buying-process.

Lundborg, Per and Per Skedinger (1999). “Transaction Taxes in a Search Model of the Housing Market”. eng. In: *Journal of urban economics*. Journal of Urban Economics 45.2, pp. 385–399. ISSN: 0094-1190.

Machado, Cecilia, Germán J Reyes, and Evan Riehl (2023). “The efficacy of large-scale affirmative action at elite universities”. In: *Documentos de Trabajo del CEDLAS*.

Malpezzi, Stephen and Duncan Maclennan (2001). “The Long-Run Price Elasticity of Supply of New Residential Construction in the United States and the United Kingdom”. eng. In: *Journal of housing economics*. Journal of Housing Economics 10.3, pp. 278–306. ISSN: 1051-1377.

Manasse, Paolo, Giulio Trigilia, and Luca Zavalloni (2013). *Professor Monti and the bubble*. https://voxeu.org/article/professor-monti-and-bubble. [Online; accessed 29-May-2022].

Manski, Charles F (2004). "Measuring expectations". In: *Econometrica* 72.5, pp. 1329–1376.

Manzoli, Lamberto et al. (2007). "Marital status and mortality in the elderly: a systematic review and meta-analysis". In: *Social science & medicine* 64.1, pp. 77–94.

Marmot, Michael (2005). "Social determinants of health inequalities". In: *The Lancet* 365.9464, pp. 1099–1104.

Martin, Chris, Kath Hulse, and Hal Pawson (2018). *The Changing Institutions of Private Rental Housing: An International Review*. Melbourne. URL: https://ssrn.com/abstract=3108291.

Mattozzi, Andrea and Antonio Merlo (2008). "Political careers or career politicians?" eng. In: *Journal of public economics*. Journal of Public Economics 92.3, pp. 597–608. ISSN: 0047-2727.

McCarty, Nolan M, Keith T Poole, and Howard Rosenthal (2008). *Polarized America: The Dance of Ideology and Unequal Riches*. eng. 1st ed. Vol. 1. MIT Press Books. Cambridge: The MIT Press, pp. xii–xii. ISBN: 9780262633611.

McCrary, Justin (2008). "Manipulation of the running variable in the regression discontinuity design: A density test". In: *Journal of Econometrics* 142.2, pp. 698–714.

Merlo, Antonio M. et al. (2010). "Part 1. The Labour Market of Italian Politicians". In: *The Ruling Class: Management and Politics in Modern Italy*. Ed. by Tito Boeri, Antonio Merlo, and Andrea Prat. Oxford University Press.

Messner, Matthias and Mattias K Polborn (2004). "Paying politicians". eng. In: *Journal of public economics*. Journal of Public Economics 88.12, pp. 2423–2445. ISSN: 0047-2727.

Mian, Atif, Amir Sufi, and Francesco Trebbi (2014). "Resolving Debt Overhang: Political Constraints in the Aftermath of Financial Crises". eng. In: *American economic journal. Macroeconomics* 6.2, pp. 1–28. ISSN: 1945-7707.

Miglino, Enrico et al. (2017). *Money can buy me life? The effect of a basic pension on mortality and fertility: a Regression Discontinuity Design*. UCL MRes Dissertation.

MinEduc (2015). "Estudio de seguimeniento a la implementacion del programa de acompanamiento y acceso efectivo (PACE)". Report of the Chilean Ministry of Education.

— (2017). "Levantamiento de informacion para el seguimiento a la implementacion del PACE". Report of the Chilean Ministry of Education.

— (2018). "Proceso de Admisión 2018. Nómina Oficial de Carreras PACE". Report of the Chilean Ministry of Education.

Ministerio de Desarrollo Social (2011). *Encuesta de Caraterización Socioeconómica Nacional*. http://observatorio.ministeriodesarrollosocial.gob.cl/encuesta-casen.

Ministerio Trabajo y Previsión Social (2015). *Encuesta EPS*. https://www.previsionsocial.gob.cl/sps/biblioteca/encuesta-de-proteccion-social/bases-de-datos-eps/.

— (2017). *Encuesta EPS*. https://www.previsionsocial.gob.cl/sps/biblioteca/encuesta-de-proteccion-social/bases-de-datos-eps/.

Ministry of Housing (2014). *Live tables on repossession activity (Table 1300)*. URL: https://www.gov.uk/government/statistical-data-sets/live-tables-on-repossession-activity.

— (Sept. 2019). *Homelessness data: notes and definitions*. Available at: https://www.gov.uk/guidance data-notes-and-definitions.

— (2022a). *Live tables on dwelling stock (including vacants) (Tables 104, 118 and 615)*. URL: https://www.gov.uk/government/statistical-data-sets/live-tables-on-dwelling-stock-including-vacants.

— (2022b). *Stamp Duty Land Tax*. URL: https://www.gov.uk/stamp-duty-land-tax/residential-property-rates.

Moen, Espen, Plamen Nenov, and Florian Sniekers (2021). "Buying first or selling first in housing markets". eng. In: *Journal of the European Economic Association* 19.1, pp. 38–81. ISSN: 1542-4774.

Moskowitz, Daniel J., Jon C. Rogowski, and Jr. James M. Snyder (2022). *Parsing Party Polarization in Congress.* Working Paper.

Määttänen, Niku and Marko Terviö (Oct. 2021). "Welfare Effects of Housing Transaction Taxes: A Quantitative Analysis with an Assignment Model". In: *The Economic Journal* 132.644, pp. 1566–1599. ISSN: 0013-0133. DOI: 10.1093/ej/ueab080. eprint: https://academic.oup.com/ej/article-pdf/132/644/1566/43617094/ueab080.pdf. URL: https://doi.org/10.1093/ej/ueab080.

Müller, Sebastian and Gunther Schnabl (2021). "A Database and Index for Political Polarization in the EU". In: *Economics Bulletin* 41.4, pp. 2232–2248.

Ngai, L Rachel and Kevin D Sheedy (2020). "The Decision to Move House and Aggregate Housing-Market Dynamics". eng. In: *Journal of the European Economic Association*. ISSN: 1542-4766.

Ngai, Rachel and Silvana Tenreyro (2014). "Hot and Cold Seasons in the Housing Market". In: *American Economic Review* 104.12, 3991–4026.

Niu, Sunny Xinchun and Marta Tienda (2010). "The impact of the Texas top ten percent law on college enrollment: A regression discontinuity approach". In: *Journal of Policy Analysis and Management* 29.1, pp. 84–110.

OECD (2018). *OECD Pensions Outlook 2018*. OECD Publishing, p. 256.

Office for National Statistics (2011). *Census 2011*. URL: https://www.nomisweb.co.uk/census/2011/qs417ew.

ONS (2015). *Population of the UK by country of birth and nationality - Annual Population Survey 2015*. URL: https://www.ons.gov.uk/peoplepopulationandcommunity/populationandmigration/internationalmigration/datasets/population

ONS (2021a). *Estimates of the population for the UK, England and Wales, Scotland and Northern Ireland*. URL: https://www.ons.gov.uk/peoplepopulationandcommuni populationandmigration/populationestimates/datasets/populationest

ONS (2021b). *House Price simple averages*.

ONS (2021c). *Population estimates by local authority and year by Welsh local authorities*. URL: https://statswales.gov.wales/Catalogue/Population-and-Migration/Population/Estimates/Local-Authority/populationesti by-localauthority-year.

— (2021d). *Regional gross domestic product: local authorities*. URL: https://www.ons.gov.uk/economy/grossdomesticproductgdp/datasets/regionalgross

ONS (2022). *Annual Survey of Hours and Earnings time series of selected estimates*. Accessed: 22 December 2020). URL: https://www.ons.gov.uk/employmentandlabour peopleinwork/earningsandworkinghours/methodologies/averageweeklye

Osservatorio sui Conti Pubblici Italiani (2021). *Il sistema pensionistico dei parlamentari: possibili implicazioni per la durata della legislatura*. https://osservatoriocpi.unicatt.it/ocpi-pubblicazioni-il-sistema-pensionistico-dei-parlamentari-possibili-implicazioni-per-la-durata-della. [Online; accessed 29-May-2022].

Otero, Sebastián, Nano Barahona, and Cauê Dobbin (2021). "Affirmative action in centralized college admission systems: Evidence from Brazil". In: *Unpublished manuscript*.

Parada-Contzen, Marcela, Andrés Riquelme-Won, and Felipe Vasquez-Lavin (2013). "The value of a statistical life in Chile". In: *Empirical Economics* 45.3, pp. 1073–1087.

Persson, Torsten and Guido Tabellini (2000). *Political economics : explaining economic policy*. eng. Zeuthen lecture book series. Cambridge, Mass.: MIT Press. ISBN: 0262161958.

Petersen, Mitchell A and Raghuram G Rajan (2002). "Does Distance Still Matter? The Information Revolution in Small Business Lending". eng. In: *The Journal of finance (New York)* 57.6, pp. 2533–2570. ISSN: 0022-1082.

Petrosky-Nadeau, Nicolas and Etienne Wasmer (2013). "The Cyclical Volatility of Labor Markets under Frictional Financial Markets". eng. In: *American economic journal. Macroeconomics* 5.1, pp. 193–221. ISSN: 1945-7707.

Petrosky-Nadeau, Nicolas and Etienne Wasmer (2015). “Macroeconomic dynamics in a model of goods, labor, and credit market frictions”. eng. In: *Journal of monetary economics* 72, pp. 97–113. ISSN: 0304-3932.

Piazzesi, Monika, Martin Schneider, and Johannes Stroebel (2020). “Segmented housing search”. eng. In: *The American economic review* 110.3, pp. 720–759. ISSN: 0002-8282.

Pissarides, Cristopher A. (2000). *Equilibrium Unemployment Theory*. The MIT Press.

Preece, Dianna C et al. (2004). “Agency theory and the House bank affair”. eng. In: *Review of financial economics*. Review of Financial Economics 13.3, pp. 259–267. ISSN: 1058-3300.

Rennard, Stephen (2004). “Treatment of stable chronic obstructive pulmonary disease”. eng. In: *The Lancet (British edition)* 364.9436, pp. 791–802. ISSN: 0140-6736.

Reserve Bank of Australia (2017). *Financial Stability Review: April 2017*.

Reserve Bank of New Zealand (2016). *Adjustments to restrictions on high-LVR residential mortgage lending*.

Rizzo, Sergio (2018). *Vitalizi d’oro: il vero privilegio sono le doppie pensionio*. La Repubblica.

Rogers, Richard G (1996). “The effects of family composition, health, and social support linkages on mortality”. In: *Journal of Health and Social Behavior*, pp. 326–338.

Romano, Joseph P and Michael Wolf (2005a). “Exact and approximate stepdown methods for multiple hypothesis testing”. In: *Journal of the American Statistical Association* 100.469, pp. 94–108.

— (2005b). “Stepwise multiple testing as formalized data snooping”. In: *Econometrica* 73.4, pp. 1237–1282.

Rubabshi-Shitrit, Ayelet and Sharon Hasson (2022). “The effect of the constructive vote of no-confidence on government termination and government durability”. In: *West European Politics* 45.3, pp. 576–590. DOI: 10.1080/01402382.2021.1914421.

Rubinstein, Ariel (1982). “Perfect Equilibrium in a Bargaining Model”. eng. In: *Econometrica* 50.1, pp. 97–109. ISSN: 0012-9682.

Salm, Martin (2011). “The effect of pensions on longevity: Evidence from union army veterans”. In: *The Economic Journal* 121.552, pp. 595–619.

Sander, Richard H (2004). “A systemic analysis of affirmative action in American law schools”. In: *Stan. L. Rev.* 57, p. 367.

Schwandt, Hannes (Oct. 2018). “Wealth Shocks and Health Outcomes: Evidence from Stock Market Fluctuations”. In: *American Economic Journal: Applied Economics* 10.4, pp. 349–77. DOI: 10.1257/app.20140499. URL: http://www.aeaweb.org/articles?id=10.1257/app.20140499.

Senato della Repubblica (2017). *Il Regolamento del Senato.Capo IV. Dei Gruppi parlamentari*. https://www.senato.it/istituzione/il-regolamento-del-senato/capo-iv/articolo-14-1. [Online; accessed 29-May-2022].

— (2022a). *La Costituzione. Articolo 94*. https://www.senato.it/istituzione/la-costituzione/parte-ii/titolo-iii/sezione-i/articolo-94. [Online; accessed 29-May-2022].

— (2022b). *XVIII legislatura. Senatori. Trattamento economico*. https://www.senato.it/leg18/1075?voce_sommario=61. [Online; accessed 29-May-2022].

Sesto, Mariolina (2021). *Pensioni degli onorevoli, perché il 24 settembre 2022 è il D day della legislatura*. Il Sole 24 ore.

Shi, L et al. (2005). "Primary care, social inequalities and all-cause, heart disease and cancer mortality in US counties: a comparison between urban and non-urban areas". eng. In: *Public health* 119.8, pp. 699–710. ISSN: 0033-3506.

Shi, Leiyu (1993). "Family financial and household support exchange between generations: A survey of Chinese rural elderly". In: *The Gerontologist* 33.4, pp. 468–480.

Shimer, Robert (2005). "The Cyclical Behavior of Equilibrium Unemployment and Vacancies". eng. In: *The American economic review* 95.1, pp. 25–49. ISSN: 0002-8282.

Snyder, Stephen E and William N Evans (2006). "The effect of income on mortality: evidence from the social security notch". In: *The review of economics and statistics* 88.3, pp. 482–495.

Stahl, Dale O and Paul W Wilson (1995). "On players' models of other players: Theory and experimental evidence". In: *Games and Economic Behavior* 10.1, pp. 218–254.

Starfield, Barbara, Leiyu Shi, and James Mackinko (2005). "Contribution of Primary Care to Health Systems and Health". eng. In: *The Milbank quarterly* 83.3, pp. 457–502. ISSN: 0887-378X.

Stiglitz, Joseph E and Andrew Weiss (1981). "Credit Rationing in Markets with Imperfect Information". eng. In: *The American economic review* 71.3, pp. 393–410. ISSN: 0002-8282.

Stinebrickner, Ralph and Todd R Stinebrickner (2014). "A major in science? Initial beliefs and final outcomes for college major and dropout". In: *Review of Economic Studies* 81.1, pp. 426–472.

Subsecretaría de Previsión Social (2015). *Sistema de Pensiones Solidarias (SPS) a 7 Años de su Implementación*. Gobierno de Chile.

Sun, Liyang and Sarah Abraham (2021). "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects". eng. In: *Journal of econometrics* 225.2, pp. 175–199. ISSN: 0304-4076.

Superintendencia de Pensiones (2014). *Tablas de Mortalidad*. https://www.spensiones.cl/portal/institucional/594/w3-propertyname-621.html.

Swan, Grant (2016). *Start to Finish: how quickly do large-scale housing sites deliver?* URL: https://lichfields.uk/blog/2016/november/8/start-to-finish-how-quickly-do-large-scale-housing-sites-deliver/.

Tarkiainen, Lasse et al. (2012). "Trends in life expectancy by income from 1988 to 2007: decomposition by age and cause of death". In: *J Epidemiol Community Health* 66.7, pp. 573–578.

Taylor, Eric (2014). "Spending more of the school day in math class: Evidence from a regression discontinuity in middle school". In: *Journal of Public Economics* 117, pp. 162 –181. ISSN: 0047-2727. DOI: https://doi.org/10.1016/j.jpubeco.2014.06.002. URL: http://www.sciencedirect.com/science/article/pii/S004727271400142X.

Todd, Petra and Kenneth I Wolpin (2018). "Accounting for Mathematics Performance of High School Students in Mexico: Estimating a Coordination Game in the Classroom". In: *Journal of Political Economy* 126.6, pp. 2608–2650.

Valenzuela, Soledad and Andrei Jouravlev (2007). *Servicios urbanos de agua potable y alcantarillado en Chile: factores determinantes del desempeño*. Cepal.

Valuation Office Agency (2019). *Private rental market statistics*.

Viscusi, W. Kip (1994). "Mortality Effects of Regulatory Costs and Policy Evaluation Criteria". eng. In: *The Rand journal of economics*. RAND Journal of Economics 25.1, pp. 94–109. ISSN: 0741-6261.

Waldron, Hilary (2013). "Mortality differentials by lifetime earnings decile: Implications for evaluations of proposed Social Security law changes". In: *Soc. Sec. Bull.* 73, p. 1.

Wasmer, Etienne and Philippe Weil (2004). "The macroeconomics of labor and credit market imperfections". eng. In: *The American economic review* 94.4, p. 942. ISSN: 0002-8282.

Weber, Max (1965). *Politics as a vocation*. Social ethics series. Philadelphia: Fortress Press.

Welsh Government (2021a). *Revenue outturn expenditure, by authority*. URL: https://statswales.gov.wales/Catalogue/Local-Government/Finance/Revenue/Outturn/revenueoutturnexpenditure-by-authority.

— (2021b). *Revenue outturn expenditure summary, by authority (£ thousand)*. URL: https://statswales.gov.wales/Catalogue/Local-Government/Finance/Revenue/Outturn/revenueoutturnexpendituresummary-by-authority.

Wheaton, William (1990). "Vacancy, Search, and Prices in a Housing Market Matching Model". eng. In: *The Journal of political economy* 98.6, pp. 1270–1292. ISSN: 0022-3808.

Wiswall, Matthew and Basit Zafar (2015). "Determinants of college major choice: Identification using an information experiment". In: *Review of Economic Studies* 82.2, pp. 791–824.

Wooldridge, Jeffrey M (2005). "Simple solutions to the initial conditions problem in dynamic, nonlinear panel data models with unobserved heterogeneity". In: *Journal of Applied Econometrics* 20.1, pp. 39–54.

Zimmerman, Seth D (2014). "The returns to college admission for academically marginal students". In: *Journal of Labor Economics* 32.4, pp. 711–754.

Zoopla (2022). *The timeline of buying a home: how long is too long?* URL: https://www.zoopla.co.uk/discover/buying/the-timeline-of-buying-a-home-how-long-is-too-long/.