EUI DEPARTMENT OF ECONOMICS

Essays in Empirical Microeconomics

Miguel Alquézar Yus

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

Florence, 24 May 2023

European University Institute
Department of Economics

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Abstract

This thesis is composed of papers in applied microeconomics.

The first chapter studies how religious leaders influence their communities' religious and political attitudes. To do this, I build a novel dataset containing the universe of Catholic priests appointed to their parishes in rural Spain between 2000 and 2019. I leverage the quasi-natural experiment by which foreign priests are allocated to parishes and use a staggered difference-in-differences design to identify their influence on their communities. I show that foreign-born priests, whom I find more devoted to their cause, are effective at revitalizing local religiosity, measured by an increase in Catholic marriages and fertility. They also influence local political opinions towards Catholic-aligned positions. However, such extra influence prevents parishioners from challenging the old status quo, measured by the higher maintenance of dictatorial honors, limiting social progress. These findings highlight that religious leaders have a considerable impact on sociopolitical attitudes.

The second chapter, coauthored with Josep Amer-Mestre, studies the influence members of parliament with previous involvement in interest groups exert on their colleagues. To do so, we collect novel data containing the voting history, and résumés of all legislators present at the European Parliament between 2004 and 2019. Using the alphabetic allocation of seats, we find that seating beside reverse revolvers when the motion is relevant to their interest groups increases co-voting by 2.4%, attendance by 1.3%, and decreases abstention by 9%. These effects are driven by budget-related motions. Our results show that the revolving doors influence the political process even when working in reverse.

The third chapter explores how reviewing time affects physicians' medical decisions. I test this prediction using high-frequency data from a Spanish outpatient department and leverage on-the-day cancellations as exogenous time shocks. I find that longer visits lead to more valuable care, measured by the provision of more detailed diagnoses, to higher testing intensity, and to lower drug prescriptions. These effects are driven by junior physicians, who use this extra time to compensate for their more overloaded shifts.

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Chapter 1

The Reversal of the Mission: How do religious leaders influence sociopolitical attitudes?

Solo-authored[1]

Abstract This paper explores how religious leaders influence their communities' religious and political attitudes. To do this, I build a novel dataset containing the universe of Catholic priests appointed to their parishes in rural Spain between 2000 and 2019. I leverage the quasi-natural experiment by which foreign priests are allocated to parishes and use a staggered difference-in-differences design to identify their influence on their communities. I show that foreign-born priests, whom I find more devoted to their cause, are effective at revitalizing local religiosity, measured by an increase in Catholic marriages and fertility. They also influence local political opinions towards Catholic-aligned positions. However, such extra influence prevents parishioners from challenging the old status quo, measured by the higher maintenance of dictatorial honors, limiting social progress. These findings highlight that religious leaders have a considerable impact on sociopolitical attitudes.

[1]I wish to thank Sule Alan, Josep Amer-Mestre, Michèle Belot, Elisa Failache, Alejandro Martínez-Marquina, Andrea Mattozzi, Guadalupe Tuñón, together with seminar participants at AYEW Spring 2022, ASREC 2022, SMYE 2022, EPSA 2022, FES Conference 2022, 2022 ASREC Europe Conference, and EUI, for their helpful comments. A special thanks goes to the staff at the Spanish Episcopal Conference for their support throughout the process. I also thank Íñigo Ena Sanjuán, Julià Gómez Reig, and Ismael Moreno Martinez for excellent research assistance. Financial support from the Salvador Madariaga-EUI scholarship is gratefully acknowledged. All errors remain my own.

1.1 Introduction

Religion is an integral part of the lives of many, constituting one of the dominant identity structures. As of 2015, 78% of the world's population reported following a denomination (Brown and James, 2019).[2] The religious leader is the central figure in charge of transmitting their values. These individuals act as intermediaries between their institution and laypeople, providing their supporters with moral teachings, offering advice on practical and spiritual matters, and holding a venerated position within the local community (Richerson and Christiansen, 2013).[3] However, how effectively religious leaders convey their messages, especially in increasingly secular societies, is far from trivial.

Over the past decades, the Western World has challenged that religious predominance. These countries have undergone an extensive secularization process resulting in decreasing social support for religious practices, from 62% in the 1980s to 53% in 2018 (EVS, 2021), and lower enrollment in the priesthood. Consequently, Western Christian churches have resorted to attracting foreign-born religious leaders to overcome the lack of native leaders, which has been coined as "the reversal of the mission". The extent to which these foreign-born priests, characterized by their high devotion and outreach, can transmit their social values is central to understanding religious persistence.

In this paper, I investigate whether the arrival of foreign priests to rural Spanish parishes has shaped local sociopolitical attitudes. During the study period, the Spanish Catholic priesthood suffered a significant compositional change, from completely native in 2000 to up to 14% of foreign-born priests in 2019. I provide novel evidence that the arrival of these foreign-born priests, whom I find to be more devoted to their cause, has revived local religiosity, measured by an increase in Catholic marriages and fertility, and influenced political opinions toward conservative positions. That influence has favored a stronger in-group cohesion among the native population while not affecting the local immigrant population. These findings have important implications for policy-making as they highlight that the composition of the priesthood has a real impact on today's sociopolitical attitudes and demographics.

I conduct rigorous data collection to estimate the influence foreign religious leaders have on their local communities. First, I collect data on the universe of appointments of priests to parishes in rural Spain between 2000 and 2019. I extract this information from the diocesan periodical bulletins retrieved from the Spanish Episcopal Conference's internal archives. Second, I use the yearbook of each diocese to obtain detailed information on the priests' demographics, including their country of birth, order, age, tenure, and

[2]According to Pew Research Center (2015), the share of the world's population following a denomination is projected to grow by 2050 to the 86.7%.

[3]In fact, as of 2018, 62% of the world's population reported completely trusting religious leaders (GfK Verein, 2018).

education. I further complement these data with a phone survey, in which I ask priests for their political opinions and work habits. Finally, I merge the previous sets of information with the universe of Spanish parishes to identify those municipalities that had a change of religious leader, allowing me to trace the influence of foreign-born priests on natives' preferences. My final sample includes detailed information on the employment record of 2587 priests working in 4020 different municipalities for the 20 years of study.

I leverage the plausibly exogenous variation in the timing of the arrival of foreign-born priests to local Spanish parishes to identify their differential impact on their community. To do this, I use a staggered difference-in-differences approach. This strategy allows me to account for time-invariant municipality characteristics and seasonality confounders. Following the recent developments in the literature on staggered two-way fixed effects, I implement the stacking approach proposed by Cengiz et al. (2019) to ensure that the treatment effects are estimated using only clean comparison units. In this setting, I define a municipality as treated if it ever had a foreign-born priest in charge of its local parish and a municipality as control if it did not. In my sample, all municipalities have assigned at least one religious leader.

I first document, using the results from my survey, that foreign-born priests are younger and more missionary-oriented than their local counterparts. Despite that, foreign-born priests are comparable to local priests regarding their political leanings, such as their social conservatism and hierarchical preference, and work habits, such as their working hours and the type of church-related activities they participate in.

I find that foreign-born priests are more effective than their local counterparts in convincing people into two of the main pillars of the Catholic Church, namely marriage and family creation. Only one year after the arrival of a foreign priest, the local community experiences 0.1 more Catholic weddings, representing a 6% increase in the mean number of Catholic weddings hold in the sample. As time passes, such influence exacerbates. Six years after the arrival of a foreign priest, these municipalities have 21% more Catholic weddings than those with a local priest. That increase comes at the expense of civil-only weddings, which decrease by 13%. Similarly, I find that six years after the arrival of a foreign priest, the community experiences an average increase of 0.65 new births per year, representing a 10% increase in the average number of births. Overall, these results suggest foreign-born priests are regaining natives' religiosity back to Catholicism.

While foreign religious leaders might influence their parishioners back to the church, their leadership could also reach other religiously-relevant outcomes. For this, I look at local political preferences. I show that the arrival of a foreign priest to the community changes its voting behavior towards more traditionally conservative positions. Six years after the arrival, municipalities with a foreign priest cast 1.4% more votes in favor of conservative parties (i.e., Catholic-aligned parties), representing a 3.5% average increase

in the conservative spectrum. This increase comes at the expense of left-leaning parties, which favor the separation between the Spanish state and the Catholic Church, and radical right parties, which are openly antagonistic towards minorities. Overall, this evidence has important implications for policy-making as foreign religious leaders might act as a deterrent to the open support for xenophobic positions.

However, the extra influence exerted by foreign-born priests could also be helping the Spanish church push its own historical narrative. To answer that question, I focus on the legacy of Franco's regime, given that this dictatorship had a symbiotic relationship with the Spanish Church, shaping each other's ideologies. However, it is presumably orthogonal to foreign-born priests. More precisely, I look at the maintenance of street names honoring Franco's dictatorship. I use this variation in street naming as it proxies the social desire to bring back the old ways. As of 2019, 3.5% of all the municipalities in my sample still had at least a street honoring the old dictatorship.[4] I find that those municipalities with foreign-born priests keep honoring the old dictatorship more often than those with local priests. Overall, this result suggests that while foreign priests do not necessarily convey a pro-Francoist speech, their ability to mobilize people towards more conservative and religious positions makes it easier for people to long for an old status quo.

In terms of heterogeneity in impact, I show that the ethnic composition of the municipalities matters. I find that the influence exerted by foreign priests is driven by ethnically diverse municipalities, where they bring their parishioners back to Catholicism. That substantial influence comes from the influence exerted on the native population, while not on the immigrants. I find no effects on ethnically homogeneous municipalities. Overall, these results reinforce the idea that foreign-born religious leaders effectively promote in-group cohesion among the native population, irrespective of the cultural proximity to local immigrants.

Finally, I investigate which priests' characteristics help at explaining the results. I find suggestive evidence that priests' age, tenure, country of study, and origin, do not play a differential role vis-à-vis foreign-born religious leaders' influence. These results suggest that foreign-born priests' influential behavior comes from unobserved characteristics common to all foreign-born priests, such as their inherently higher motivation and devotion, rather than from cultural and language proximity to the local communities.

I perform three exercises to check the validity of my empirical strategy. First, I show visually that the parallel trends assumption is not violated for the outcomes studied in the analysis. Moreover, I test whether pre-treatment outcome realizations can predict the arrival of a foreign-born priest, finding no major evidence in that respect. In other words, the allocation of foreign-born priests to a new parish does not take into account its religious and political attitudes. Second, I check whether there are underlying differences

[4]In comparison, as of 2000, 20.2% of the municipalities analyzed were honoring the old dictatorship.

between those municipalities that never had a foreign priest and those that had at least one between 2000 and 2019, finding some significant differences across municipality types. Therefore, I replicate the main analysis using a subsample of matched municipalities based on their 2001 Census characteristics, obtaining comparable results. Third, I test whether the arrival of a foreign priest triggered a migration shock, which could change the local composition. I find no evidence in that respect. These patterns are consistent with the evidence presented in Section 2.2, according to which the replacement of priests is mainly determined by supply-side factors, such as the number of recently ordained priests and the death or retirement of a previous priest. Overall, my robustness checks confirm that my identification strategy is sound and effect estimates are internally valid.

In this paper, I provide novel evidence on the influence local religious leaders have on their communities. My contribution is twofold. First, I build a very rich dataset containing the universe of Catholic priests' appointments in rural Spain between 2000 and 2019 and complement it with novel archival and survey data on the priests' characteristics. Second, I leverage the quasi-natural experiment by which foreign-born religious leaders are allocated to Spanish parishes to identify their influence on their communities. I show that foreign-born religious leaders, whom I find to be more devoted to their cause, revive local religiosity and influence political opinions toward Catholic-aligned positions. They achieve that prevalence by promoting in-group cohesion among native population, while not mobilizing culturally and religiously similar immigrants. My results support the hypothesis that foreign leaders are influential traditional community builders.

This paper contributes to and builds on three different strands of the literature. First, this work contributes to the literature on leaders' influential behavior determinants, which harks back to Carlyle (1840). Previous research has focused on explaining how leaders' identity characteristics, such as gender (Broockman, 2014; Bhalotra et al., 2018; Ladam et al., 2018; Baskaran and Hessami, 2018), ethnicity (Burgess et al., 2015; Sakong, 2021), religiosity (Bhalotra et al., 2014, 2021), charisma (Assouad, 2020; Wang, 2021), social connections (Becker et al., 2020; Pulejo, 2022), and political views (Broockman and Butler, 2017; Butler and Hassell, 2018), shape their constituents' preferences and behavior. I focus on an often overlooked authority figure, local religious leaders. Religious leaders differ from those previously studied as they are appointed following a top-down process in which their communities have minimal involvement. Three notable exceptions are Engelberg et al. (2016), which shows that high-performing Methodist pastors are key in explaining church attendance; Tuñón (2017), which shows that left-leaning Brazilian bishops were able to mobilize their network towards economically progressive but socially conservative positions; and Bassi and Rasul (2017), which shows that papal visits influenced the timing of births via religious persuasion. I contribute to the literature by showing that the arrival of foreign priests, characterized by being more devoted than their local counterparts,

promotes Catholic-aligned values such as higher fertility and marriages and mobilizes voting behavior towards more conservative positions. Since religious leaders mediate in the transmission of values and the persistence of the local culture, this paper also relates to the literature on cultural transmission (Bisin et al., 2004; Alesina et al., 2013; Guiso et al., 2016).

Second, my paper relates to the broader literature on the societal impacts of religiosity, which goes back to Weber (1920). Some recent studies have provided compelling evidence that religious practices have long-lasting implications on individual outcomes and behaviors, such as economic outcomes (Campante and Yanagizawa-Drott, 2015; Montero and Yang, 2022; Heldring et al., 2021; Bryan et al., 2021; Drelichman et al., 2021), human capital (Becker and Woessmann, 2009; Calvi et al., 2020), and pro-social behaviors (Clingingsmith et al., 2009; Schulz et al., 2019; Gagliarducci and Tabellini, 2022).[5] This paper looks at religiosity from a yet unexplored angle, that one of its local religious leaders. I contribute to this literature by causally studying how changes in the current composition of the local priesthood can lead to religious awakenings and changes in the parishioner's sociopolitical views.

Finally, I contribute to the study of the consequences of secularization. Previous literature has found mixed evidence on the relationship between socioeconomic prosperity and a country's secularization (Barro and McCleary, 2003; Lozano, 2017; Cantoni et al., 2018; Andersen and Bentzen, 2022). I contribute to the literature by examining how the "reversal of the mission", a direct consequence of Europe's secularization, helps revive local religiosity, highlighting the important role of supply-side factors in explaining European secularization. Since most foreign religious leaders preaching in Europe share a missionary trait, this paper also relates to the literature on the long-lasting effects of exposure to missionaries (Bai and Kung, 2015; Waldinger, 2017; Valencia Caicedo, 2019; Calvi et al., 2020; Becker and Won, 2021).

The remainder of the paper is organized as follows: Section 2.2 explains the Spanish religious background and how dioceses manage their own resources. Section 2.3 presents and describes the data. Section 2.5 exposes the empirical strategy followed. Section 2.6 presents the main results, and Section 1.6 explores the mechanisms at play. Finally, Section 2.7 concludes.

[5]For more detailed literature reviews, see Iannaccone (1998); Iyer (2016); Becker et al. (2021).

1.2 Institutional Setting

1.2.1 Spanish Religious Context

In Spain, Catholicism has traditionally been considered a state religion.[6] Despite that, in recent years, a rapid process of secularization has challenged this scenario. Figure 1.1 reports the evolution of religiosity between 2000 and 2019. Figure 1.1a shows that the proportion of Spaniards who self-identify as Catholic has slowly decreased from 82% in 2000 to 63% in 2019. However, that share may include many culturally religious but non-practicing individuals. Figure 1.1b shows the evolution of church attendance between 2000 and 2019. It depicts that while a large majority of Spanish people self-identifies as Catholic, as of 2019, only 35% of them frequently attend church (i.e., 22% of the whole population). That drop in religiosity is largest among the younger generations. While in the 2000s, 25% of all young people reported attending church, in 2019, only 6% attend often.

The evolution of the Spanish priesthood has taken a similar route. At its peak in 1961, the Spanish Church was composed of more than 35,000 priests, which represented the 10% of all priests worldwide, and had around 900 new priests ordained yearly.[7] This environment favored the missionary outflow of more than 25,000 individuals, representing, at that time, the 25% of all Spanish clergy (Suárez Fernández et al., 1991). However, since then, religious calling has been steadily decreasing.[8] Figure 1.2 displays how the ordainment of priests has evolved during recent years, showing that Spanish seminaries have been continuously losing vocations throughout the study (from 227 priests ordained in 2001 to 124 in 2019). This limited religious calling has led to the need to attract foreign-born priests, resorting to what has been coined as the "reversal of the mission" (Ojo, 2007).[9] As of 2019, 20% of all new seminarians were foreign-born. This situation has created a unique scenario, especially in Spanish rural areas, characterized by their aged population, high religiosity, and high ethnic homogeneity.

[6]The creation of the Spanish nation-state was heavily influenced by the historical power the Catholic Church had in the Spanish territories. This influence has shaped the current understanding of Spanish society, culture, and politics (Suárez Cortina, 2014).

[7]These values have been calculated using the 1956-1965 Pontifical Yearbooks.

[8]Numerous reasons have been stressed for having promoted the decay in religious callings, such as the increasing secularization of the Spanish state, the rigid and antagonistic positioning of the Spanish bishops to church modernization following the II Vatican Council, and the decreasing fertility rates (Menéndez Pidal, 1996).

[9]The process of attraction of non-Western missionaries to the West has been recently documented in several countries, such as Germany (Währisch-Oblau, 2009), the Netherlands (Koning, 2011), the United Kingdom (Woodhead and Catto, 2012; Burgess, 2019), and the United States (Kim, 2015).

1.2.2 Diocese Management

The Spanish Catholic Church is characterized by a decentralized structure, with all decisions taken at the diocese level. A diocese is an administrative unit, similar in size to a civil province, that manages all the religious activities held in its territory, ranging from the appointment and ordainment of priests to how collected funds are allocated. Each diocese is guided by a bishop. As of 2019, there are 69 territorial dioceses, coordinated by the Spanish Episcopal Conference, and comprise around 23.000 parishes and 17.000 priests.[10]

Dioceses manage their resources following an autocratic approach. Every diocese has its own seminary where future priests study. The process to become a priest, homogeneous to all dioceses, takes around 8-9 years, and includes the study of Philosophy and Theology and the pastoral training. Upon finishing their studies, priests are ordained by their diocesan bishop, forming a perpetual relationship with the ordaining diocese.[11] These bonds help dioceses maintain a loyal number of vocations in the territory.

Bishops are the actors in charge of allocating priests to their corresponding parishes. The process works as follows. First, preceding religious leaders are moved out of their parish in the event of death, retirement (Code of Canon Law, 1983, § 537), or by forza maggiore, always after an extended stay in the parish.[12] When assigning a new priest, bishops consider the parish demographic and religious characteristics and the priest's pastoral and religious attributes.[13] Throughout the process, religious leaders have de facto no power to object to a bishop's decision (Code of Canon Law, 1983, § 1748).[14] Once at the parish, priests have complete discretion on their engagement with the local community (Code of Canon Law, 1983, § 530).

In the event of missing vocations, dioceses can opt for two different formulas. First, dioceses could choose to aggregate parishes, providing multiple parishes to a unique priest, called "administrator", or many parishes to several priests, called "in solidum". While the former strategy gets used as a temporary solution in finding a permanent priest (Code of Canon Law, 1983, § 539-540), the latter approach has become a prevalent strategy, especially in rural areas. Between 2000 and 2019, 16% of all municipalities in my sample

[10] For an updated figure, see https://www.conferenciaepiscopal.es/iglesia-en-espana.

[11] A popular saying among Spanish deacons exemplifies that commitment: "If in God you trust, there will be no regret" (in Spanish, *Si en Dios confías, no hay arrepentimiento).*

[12] Spanish dioceses are requested to promote the continuity of priests in their corresponding parishes for at least six years. See Art. 4 of the C.E.E Official Bulletin - July 1984, inspired in Code of Canon Law (1983), § 522.

[13] Anecdotal evidence from various talks with priests and bishops suggests that the main factors influencing priests' appointments are the parish's demographic characteristics. No noticeable attention is paid to other sociopolitical factors at the local level. In the analysis, I control for population and the number of previous priests appointed to a given parish to account for its desirability.

[14] Qualitative evidence, collected via a phone survey, shows that priests have little saying in their own appointment process.

have had at least a priest “in solidum". This approach is mainly used in parishes governed by foreign-born priests (23%) when compared to those led by local priests (10%). In my analysis, I control whether a given municipality has a priest “in solidum" to account for that difference.

Second, bishops could also work on attracting priests from other dioceses to attend their local seminaries and preach in their territory. These contacts between demanding dioceses and supplying dioceses are conceived as a long-standing relationship by the two dioceses, nurtured by the established presence of Spanish missionaries in the offering dioceses. Spanish dioceses leverage these connections to overcome their decreasing native religious calling. In practice, local bishops contact their foreign counterparts asking for vocations. Then, foreign bishops proceed by contacting those religious members in their dioceses interested in becoming missionaries. Finally, these members decide whether to accept the pastoral call and, if so, move to Spain. Upon arrival, they are treated as equals to nationals, having to attend the local seminary if they are not yet ordained and subsequently preaching under their new diocese. Additionally, foreign-born priests once in Spain hold no formal obligations towards their home diocese. According to Lara (2021), the main reason that motivates foreign-born priests arriving in Spain is to help under provisioned dioceses.

1.3 Data

This paper uses matched priest-municipality data that spans the period from January 1st, 2000, to December 31st, 2019. I complement this with information from a priest survey and administrative data on municipalities’ characteristics. This section describes sources and methods of data collection.

1.3.1 Priests’ Appointments

I use a novel collected dataset containing the universe of priests’ appointments to parishes in rural Spain between January 1st, 2000, and December 31st, 2019. My dataset contains parish-month level information on all 8533 priest appointments that occurred in 4020 municipalities for those 20 years. The final sample includes all those municipalities with a single population center and parish to avoid any within-municipality self-selection into parishes. This sample comprehends 72% of all parishes and 93% of all municipalities of the target dioceses.

This data was collected at the Spanish Episcopal Conference’s archive (hereafter, SEC). The data gathering worked as follows. First, I use the periodical bulletins published by each diocese (in Spanish, Boletín Oficial Eclesiástico) and regularly submitted

to the SEC. These bulletins, which aim at providing a complete screenshot of the diocese, include information ranging from the events in which the bishop participated to detailed parish-level information. I extract from each monthly bulletin information on the priests' appointment process, including the priests' names, the exact working positions, the date when those appointments occur, and the parishes they are assigned. Then, I scrape the list of parishes each diocese has from the SEC's website and aggregate it at the municipality level using the catalog of population entities available at the Spanish Statistical Office. Lastly, I merge the dataset containing all priests' appointments with the universe of Spanish parishes to identify which municipalities had a change of religious leader and when that change happened. Table 1.A1 shows the total number of parishes and municipalities each diocese has and those included in my final sample.

1.3.2 Priests' Characteristics

Priest Demographics

I collect detailed priests' demographic characteristics using multiple sources from the Spanish Episcopal Conference's archive. First, I collect information on the priests' country of origin and religious order. This data is scattered over different sources ranging from the dioceses' yearbooks to interviews conducted by the national newspaper, Ecclesia. I use the full name of each priest to accurately identify them from the multiple sources available. When compared to survey responses, I find a 1.4% and 5.4% error rate in nationality and religious order coding.

Table 1.1 provides descriptive statistics on the priests' nationality. While the average priest is of Spanish origin, 13.3% of the priests in my sample was not born in Spain. Among those other countries of birth, Colombia is the largest supplier, with 5.7% of all priests, followed by Mexico with 1.6%. However, their distribution in the territory is far from homogeneous. Figure 1.3 maps all municipalities used in the study, identifying those that ever had, and never had, a foreign priest. We can observe how the distribution of foreign priests is not homogeneous across the Spanish geography, being highly influenced by the dioceses' pool of native religious calling. Table 1.A2 puts into numbers such heterogeneity, showing that some dioceses never had a foreign priest, such as Osma-Soria, while others use this formula more intensively, such as Tarazona. Finally, Table 1.2 provides the summary statistics on the differences between those municipalities that received at least a foreign priest after January 2000 and those that never received one. For instance, ever-treated municipalities have, on average, fewer young people and singles, fewer temporal workers, and a higher share of immigrants than never treated municipalities.

Second, I collect information on the priests' education from each dioceses' periodical bulletins. Using these books, I extract the flow of religious celebrations that took place

in each diocesan seminary at any point in time between 2000 and 2019. I use the full name of each foreign priest to identify whether they studied in a Spanish seminary before preaching. I use this information to identify which foreign priests took part in local seminaries as a proxy for partial cultural assimilation.

Finally, I collect information on the priest's birthdate and ordainment. In particular, I use the ordainment date to construct a measure of working tenure, as religious members, once ordained, are considered priests in full responsibility, and most cases, entrusted with leading a new parish. This information was retrieved from the diocese's yearbooks and websites. Unfortunately, not all dioceses provided this information to the Spanish Episcopal Conference. Table 1.A3 shows that the dioceses providing data have fewer foreign priests, fewer members of religious orders, and more priests educated in a local seminary.

Priest Survey

I collect a survey on individual priests working at the dioceses under study. This survey aims at understanding which characteristics define the current Spanish priesthood. In particular, I am interested in learning which personal, sociopolitical, and work-related traits foreign and local priests differ.

The data gathering worked as follows.[15] Before the survey release, I manually collected the personal contact details of all priests present on each diocese's website and diocesan yearbooks. I use all information available on these sources as of April 2022. The sample of potential priests was 1288 priests. The survey release followed a two-stage process. First, phone numbers are contacted based on a random rank.[16] Individuals in the sample were contacted up to five times, at different times and with multiple phone numbers, both from mobile and landline devices. The phone survey was collected from May to June 2022. Then, once the phone survey was over, I sent an email survey with the same questionnaire to all those priests for whom I had their contact details but could not reach via phone call. This online survey took place from early July to mid-August 2022. Weekly reminders were sent throughout the process. A total of 257 individuals completed the survey, of which 87% did it by phone. Table 1.A4 shows for each diocese the distribution of all priests, potential priests, and surveyed priests, differentiating by their foreigner status. The survey had a 20% response rate. Table 1.A5 compares surveyed priests to all other priests, showing that the surveyed priests are less likely to belong to a religious order and more likely to have studied in a local seminary. Similarly, they are younger and less

[15]Each survey takes an average of 8.6 minutes to complete. Phone surveys were conducted by the principal investigator together with the support of three research assistants. The survey received ethical approval from the Ethics Committee at the European University Institute (available upon request).

[16]In the event of not finding the person indicated in the phone book but another person, I attempted to obtain updated contact details for the priest in my sample.

tenured than non-surveyed priests. I find no significant differences regarding the priests' country of birth.

The survey is composed of three blocks of questions. The first block includes questions on the priests' characteristics, such as age, ordainment year, nationality, reasons to come to Spain, and working experience abroad. The second block incorporates the political views of priests. I elicit individuals' political opinions by using a list of politically relevant and highly polarized statements and asking the participants to state how much they agree or disagree with them. Then, I summarize those scores into two broad categories measuring individuals' social and economic conservatism. I further identify those priests that prefer not to question the Pope's ruling. I do so, as in L.A. Times Archives (2002), by asking participants about their opinion on the moral views of both Pope Francis and Benedict XVI. In the last block, I ask priests about their working habits, including questions on church attendance, hours spend preaching, and the level of local involvement. At the end of the survey, I pose an open question to participants to understand which other challenges they face in their parishes. Qualitative evidence shows little to no local repudiation based on the priest's country of birth.[17] Section 1.C in the Appendix includes all the questions administered.

Table 1.3 describes all the characteristics collected for my sample of priests. In Panel A, we can observe that 13% of the overall sample of priests are foreign-born, 90% are diocesan, and 89% studied in a Spanish seminary. Panel B shows the information contained in each diocese's yearbooks. We can see that the average priest is 63 years old and has 36 years of working experience. Panel C presents the information collected in the survey. We can observe that 20% of the priests working in Spain, as of 2022, have had previous missionary experience, ranging from short stays to long-standing missionary expeditions. The average priest reports to be slightly more socially and economically conservative than average, often discussing political issues with his parishioners but not questioning the moral views of the Catholic Church. Furthermore, priests estimate that around 23% of their parishioners regularly attend the Sunday sermon, spending 10 hours a week officiating masses. Priests play a central role in their communities, participating in numerous projects, with special emphasis on social care and family planning activities, and maintaining a good relationship with the local governments.

Comparing foreign priests to their local counterparts, we can observe how foreign-born priests are 13 years younger and have a higher probability of being part of a religious order and of being missionaries. Furthermore, foreign-born priests are more optimistic about the current economic situation both when compared to Spanish-born priests and to the

[17] Only one of all foreign priests surveyed admitted having faced racism during the first year he spent in Spain. Even in that situation, his bishop did not change his working parish, reinforcing the idea that bishops do not entirely internalize local circumstances when deciding the allocation of priests.

general population.[18] No differences are found concerning their political ideology and working habits. These differences are consistent with the evidence presented in Section 2.2, according to which foreign-born priests' main trait is their missionary inclination.

1.3.3 Outcomes of Interest

Religious Attitudes

I measure (social) religiosity by using one of the couples' most important events, their wedding. More precisely, I use data from the Spanish Statistical Office containing the number of weddings celebrated by the residents of a given municipality and year. This source allows me to classify weddings by the rite followed, namely whether they are civil-only weddings, they follow the Catholic rite, or they follow a non-Catholic rite. Figure 1.4a displays their evolution over the period of study. It depicts the Spanish secularization process, with a decrease in religious encounters, from 80% of all weddings in 2000 to a mere 20% in 2019.

However, marrying by the Catholic rite might be partially influenced by the inherent traits of the officiant priest. To provide further evidence of foreign priests' role in bringing people closer to Christianity, I look at fertility. This measure is a relevant proxy for religiosity given the long-standing support of the Catholic Church to family-driven and natalist policies (McKeown, 2014). In fact, according to the 2018 Spanish Fertility Survey, practicing Catholics have an average of 1.33 children, non-practicing Catholics have 1.07 children, and non-believers have 0.68 children. More precisely, I use a database provided by the Spanish Statistical Office containing the number of births in a given municipality and year. Figure 1.4b displays the distribution of the average number of births per municipality over time. It clearly shows how during the period pre-2008 financial crisis, the number of births per municipality was smoothly increasing. However, since then, it has been steadily decreasing, reaching its lowest level at the end of the study period.

Political Behavior

I use voting data for all national and European elections held in Spain between 2000 and 2019. More specifically, my sample contains the national elections held in 2000, March 2004, 2008, 2011, 2015, 2016, and April and November 2019, and the European elections held in June 2004, 2009, 2014, and May 2019. This information was retrieved from the Spanish Interior Ministry database and has been aggregated at the municipality level.[19]

[18] Question 19 is also asked in the Spanish monthly political polls collected by Spanish Centre for Sociological Research (CIS). Individuals surveyed as of May 2022 ranked their economic situation to be 3.665, comparable to the valuation of the local priests.

[19] I focus only on national and European elections, while not on the regional and local administrations, to avoid cross-municipality differences in candidates.

I first classify all political parties that ever run for the national or European parliament into two broad categories: right-wing and left-wing parties. To do so, I use the information reported in their public register and online sources. I follow this bipartisan approach as it represents the predominant political culture in the study period. Additionally, such categorization exemplifies the support of the Catholic Church. On the one hand, right-wing parties advocate for preserving the status quo, with Catholicism having an active role in society. On the other hand, left-wing parties advocate the real separation between the Spanish state and the Catholic Church. Figure 1.4c illustrates the evolution of these categories over time. It highlights that the municipalities in my sample are consistently more right-wing leaning. Table 1.A6 in the Appendix exemplifies the main political parties considered in each category.

I then proceed by sub-classifying those parties in the right-wing sphere, differentiating those belonging to the conservative aisle from those on the radical right aisle. These two ideologies differ primarily in their approach toward minorities. While conservative parties take a status quo approach, embracing prevalent covert intolerance, radical right parties follow an antagonist approach, publicly displaying minorities as a threat to Spanish traditions (Olivas Osuna, 2021). Figure 1.4d illustrates the evolution of these subcategories over time, showing how radical right parties have gained political momentum in recent years at the expense of conservative parties. Table 1.A7 in the Appendix exemplifies the main political parties considered in each category.

Dictatorial Honors

I use data from the Electoral Census Street Map available at the Spanish Statistical Office to classify municipalities based on street names. This database contains cadastre information on a biannual frequency since 2001. Following Oto-Peralías (2018), I measure, for each municipality, the percentage of streets using a Francoist name. I interpret the fact of maintaining names from the dictatorship as a sign of social stagnation.

The relevance of street naming in the Spanish political agenda is paramount. Since the passing of the Historical Memory Law in December 2007, all public institutions had to eliminate any recognition of the Francoist dictatorship that was in power in Spain from 1936 to 1975.[20] While all municipalities had to eliminate any Francoist reminiscent when such a change was supposed to happen was not regulated. This scenario is suitable for studying social change at the local level. Figure 1.4e identifies the evolution of Francoist street naming in Spain. We can see how, progressively, municipalities have moved away from street names honoring public figures of the Spanish dictatorship and civil war. However, still, 0.2% of all streets evoke such a historical period. Table 1.A8 in the Appendix displays the keywords used to identify Francoist streets.

[20]See Law 52/2007 Article 15.

1.4 Empirical Strategy

In the first empirical exercise, I examine the extent to which the arrival of a foreign priest to a given parish influences his constituents' sociopolitical and religious attitudes using the following model:

$$Y_{it} = \alpha + \beta Foreign_{it} + \gamma_i + \omega_t + \Psi X_{it} + \epsilon_{it} \tag{1.1}$$

where Y identifies the outcome of interest, as described in Section 2.3, for a municipality i and a year-month t. The key independent variable *Foreign* identifies whether a given municipality ever had in the past a foreign-born religious leader. I control for municipality characteristics, X_{it}, such as population, population squared, the number of previous priest appointments, and whether the current priest is sharing his office with other priests (in solidum). As explained in Section 2.2, I use these controls as bishops look at them in deciding the priest-to-parish allocation process. I include municipality fixed effects, γ_i, which allows me to account for time-invariant variation across municipalities; and year-month fixed effects, ω_t, which mitigate that results are confounded by secularization (e.g., periods in which people have lower religiosity levels may also lead to lower native religious calling, thus a higher propensity of having a foreign religious leader). Standard errors are clustered at the municipality level.

Under conventional two-way fixed effects assumptions, β measures the average effect of having a foreign priest in a municipality in a given year-month compared to municipalities with a local priest. However, as recent developments in the estimation of staggered difference-in-differences designs have shown, the average treatment on the treated is a weighted sum of different ATTs, with potentially even negative weights (De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021). To overcome such limitation, I use the stacked difference-in-difference approach proposed by Cengiz et al. (2019).[21] This approach transforms my staggered setting into a two-group by two-period design, aligning observations relative to the time of the event. I estimate the following equation:

$$Y_{i,t}\mu = \sum_{\mu}^{\mathcal{P}} \beta_\mu Foreign_\mu + \gamma_i + \omega_t + \Psi X_{it} + \epsilon_{it}, \qquad \text{for } \mu \in -Q, \ldots, +P \tag{1.2}$$

where i refers to municipality, t to time, and μ to the relative time-to-event. Q and P refer to any arbitrary time window to and since the event, respectively. Outcomes are normalized to $Y_{i,t}(\mu)$ - $Y_{i,t}(-1)$, using the year prior to the arrival of foreign-born priests as comparison baseline.

[21] For completeness, Section 1.B in the Appendix compares the influence of foreign-born religious leaders depending on the estimation method used. Using alternative estimation methods, I obtain similar results to my main specification.

As in the conventional difference-in-differences framework, the main identification assumption is that both treated and control municipalities would have had a similar outcome in the absence of a foreign priest's arrival. The main concern is whether there exists self-selection into treatment, leading to differential pre-trends between treated and control municipalities. As shown in Section 2.2, the timing of the treatment should be uncorrelated to the evolution of outcomes over time in treated and untreated municipalities, given that bishops do not strategically select priests based on the municipality's sociopolitical leanings. Furthermore, the top-down nature of the decision prevents parishes from voicing their preferences, thus facilitating priest assignments to be uncorrelated with municipality characteristics.

I provide two crucial exercises to prove that in the event of no treatment, both control and treated municipalities would have behaved similarly. First, I perform a placebo test comparing the change in a given outcome variable from $\mu = -2$ to $\mu = -1$ in those treated municipalities, in my case, those receiving a foreign priest, and those municipalities not treated during that period. Table 1.4 shows that none of the main outcomes used in this paper, except the vote share of radical right parties, is significant, which indicates that in the absence of the arrival of a foreign priest, both treated and control municipalities would have followed similar trends. Second, I replicate the same analysis using a matching-on-observables approach, as this method allows me to account for the fact that treated and control municipalities may be structurally different at baseline. I use a propensity score matching algorithm with two neighbors and no replacement on a list of municipality characteristics available in the 2001 Spanish Census. Section 1.D in the Appendix includes a detailed description of the propensity score matching procedure, together with the replication of all baseline results only using the sample of matched municipalities. I find no significant differences between using the full sample and only matched municipalities.

A second issue relates to the stable unit treatment value assumption. This assumption implies that the arrival of a foreign priest to a municipality does not influence its control municipalities' potential outcomes. In this setting, I would violate this assumption if the arrival of the foreign-born priest would motivate individuals from other parishes to attend the foreign-born priest's masses. To tackle this concern, I use only municipalities with a single population center and parish to avoid any within-municipality self-selection into parishes. However, individuals could still commute between municipalities to attend a foreign priest's mass. This concern is minimized by the Spanish population density structure, which is defined as highly concentrated. That means that individuals are not widely spread over the territory but live only at core population centers built around a parish, making it improbable to commute to another municipality.[22] Similarly, I would

[22]In fact, if we were to divide Spanish geography into 1km^2 cells, only 12.5% of these cells would be inhabited, compared with 68% in France and 57% in Italy. These ratios are computed using the 2011 GEOSTAT 1km2 population grid dataset.

violate the SUTVA assumption if the arrival of a foreign priest would influence individuals' decision to migrate to the municipality. I test whether those municipalities with newly arrived foreign priests enjoyed a differential migration pattern. Figure 1.A1 shows that the arrival of a foreign priest to a municipality does not influence individuals' migration decisions.[23]

A third issue pertains to any other plausible interpretation of the main variable of interest, *Foreign*. While *Foreign* identifies whether a religious leader was foreign-born, it might also capture how important the arrival of a new priest is in the community. Table 1.5 tests that hypothesis, showing that only foreign-born new priests significantly influence their communities. In fact, the replacement of religious leaders by other local priests is detrimental to religious spread. That finding is in line with the characteristics defining local priesthood, as seen in Section 2.3. In this study, I focus on the arrival of foreign-born priests, which could be interpreted as a lower-bound effect, given that the change of religious leaders, per se, worsens religious influence.

1.5 Main Results

1.5.1 Influence on Religiosity

I start by looking at whether the arrival of a foreign priest to a given municipality changes its local religiosity. As explained in Section 2.3, I measure social religiosity by using the number of Catholic weddings and the number of births.

Whether and how foreign priests shape social religiosity is far from trivial. On the one hand, we could expect that the arrival of a foreign priest to a community might deter individuals from following the Church's directives. In fact, the decision to move into and out of a religious group might be more influenced by the need for belonging than believing itself (Stark and Bainbridge, 1985). In this case, this would mean that locals, when deciding their type of wedding, might factor in positively having as officiant priest the Spanish one they grew up with. That would result in fewer Catholic weddings in those municipalities led by a foreign priest. On the other hand, foreign priests might be inherently more effective than local priests in conveying Catholic teachings. In fact, foreign priests in my sample are two times more likely to be missionaries than their local counterparts. In this case, this would imply that parishioners would relate to their church more intensely, possibly overcoming the "loss" of their old-standing local priest and resulting in more Catholic celebrations.

Figure 1.5 displays the evolution of weddings at the local level following the arrival of a

[23]For completeness, Figure 1.A2 shows that foreign-born priests, when working in dioceses with a higher presence of foreign-born priests, do not influence their communities more than when working in dioceses with a lower presence of foreign-born priests.

foreign religious leader. Important to notice is that weddings, especially in municipalities with few residents, are regarded as a rare event given the demographic dynamics. On average, the municipalities used in the sample have 2.8 weddings per year, but more than 50% of the municipalities do not have any weddings in a given year. Subfigure 1.5a highlights that the arrival of a foreign-born priest to the municipality leads to an increase in the number of weddings following the Catholic rite. The change in nuptial celebrations after the arrival of a foreign-born priest is explained by the relative availability of churches in rural Spain, allowing for the observed short-term effect. Six years after a foreign priest's arrival, treated municipalities have significantly 0.35 more Catholic weddings than those with local priests. That represents a 21% increase in the average number of Catholic weddings. Such an increase comes at the expense of civil-only weddings. Subfigure 1.5b shows how, six years after the arrival of a foreign priest, the municipality holds 0.17 fewer civil-only weddings, representing a 12.75% decrease in the average number of civil-only weddings. Taking both wedding types together, subfigure 1.5c shows that those municipalities with a foreign-born priest experienced a weakly positive increase in the total number of weddings. Moreover, subfigure 1.5d shows that the arrival of foreign priests does not affect the celebration choice of individuals from other denominations.

However, marrying by the Catholic rite could still be entirely influenced by the inherent traits of the officiant priest and not so much by local religiosity. To provide further evidence on this matter, I look at local fertility. As explained in Section 2.3, fertility is a good proxy for social religiosity, given the Catholic Church's strong emphasis on traditional family creation. Figure 1.6 shows how the number of local births starts growing upon the arrival of a foreign priest to a municipality. Six years after the arrival of a foreign priest, the village experiences an average increase of 0.65 new births per year, representing a 10% increase in the average number of births. While small in magnitude, that figure has an important economic significance for the municipalities analyzed, as their aging population and economic decline characterize them.

Overall, these results suggest that foreign priests are more effective than their local counterparts in convincing people into two of the main pillars of the Catholic Church, namely marriage and family creation. However, whether and how foreign priests influence their parishioners on non-religious matters is still under question.

1.5.2 Influence on Electoral Outcomes

In this section, I study whether the arrival of a foreign priest changes the political behavior of its parishioners. As explained in Section 2.3, I first focus on the right-left dimension

as it divides parties into church supporters and detractors, respectively.[24]

Whether and how foreign priests shape local political preferences is far from trivial. On the one hand, one could expect that the arrival of a foreign-born priest would help the community to realize that foreigners can be part of the "local elite" and become a bridge between immigrants and natives in the municipality as theorized in Allport et al. (1954). In such a case, we would observe that those pro-immigration parties, identified more prominently in the sample as left-leaning (Volkens et al., 2021), would benefit from the arrival of a foreign priest. On the other hand, foreign priests might be inherently more religious and charismatic than their local counterparts, which could lead to a higher vote share for right-wing parties, given their Church-friendly positions. A third viable option is that religious leaders no longer have societal relevance, given the increasing secularization shown in Section 2.2. This would ultimately imply that their demographic attributes would no longer play an active role in shaping natives' political behavior.

Figure 1.7 displays the political evolution of left and right-leaning parties following the arrival of a foreign priest to the community. Subfigure 1.7a presents the vote share for left-wing parties and shows how the appointment of a foreign priest slowly changes individual political preferences away from the left. Six years after the arrival of the foreign priest,[25] treated municipalities are 1.4% less likely to vote for left-wing parties, representing a 3.5% decrease in the average voting share for the left. Such decrease is captured by right-wing parties, which see a significant increase in their voting share of 1% (See Figure 1.7b).

However, such influence towards traditionalist parties might hide a compositional effect in which minority integration could play a role. I subdivide the right aisle into conservative and radical right parties to explore that dimension. Figure 1.8 shows how parishioners slowly move towards conservative positions and away from radical right ones upon the arrival of a foreign priest. Six years after the arrival of a foreign priest, the village votes 1.5% more to conservative parties, representing a 3% increase in the average voting share for conservative parties. Contrarily, the arrival of a foreign priest to the parish mobilizes his parishioners away from extreme positions and decreases the voting share of these parties by 0.6%, representing a 23% decrease in the average voting share for radical rights parties.

A secondary channel explaining that electoral shift would be the change in the voters' composition. On the one hand, one could expect that foreign-born priests, given their stronger religious influence in their communities, might move their parishioners to rely more on the Church than the state. Thus, deciding not to participate in the elections.

[24]For completeness, Figures 1.A3, 1.A4, 1.A5, and 1.A6 in the Appendix show that foreign-born priests do not have a significantly effect on the spending preferences and economic prosperity of their communities.

[25]Given that the elections are held on average every 18 months, the six years window corresponds to four elections after arrival, including its contemporaneous one.

On the other hand, the presence of these priests could have quite the opposite effect, as their influence might increase their communities' cohesion and political positioning. Thus, increasing their voting participation. Figure 1.9 corroborates the latter argument. It shows that foreign-born religious leaders consistently mobilize their communities into electoral participation.

Overall, these findings are in line with the duality defining foreign priests, religiosity and nationality. They show that foreign priests successfully bring their parishioners closer to Church-friendly positions, represented politically in Spain by the conservative parties, and away from openly xenophobic positions, represented politically by radical right parties. They do so by promoting electoral participation among their parishioners.

1.5.3 Influence on Social Progress

In the previous sections, we saw that when a foreign priest is present in a municipality, its people move towards more conservative and religious positions. These religious leaders seem more effective than their local counterparts in guiding people, but is such guidance also helping the Spanish Church push its own historical narrative?

To answer this question, I look at a political regime that has heavily influenced the ideology of the Spanish Church but is presumably orthogonal to that of foreign-born priests, Franco's dictatorship. As explained in Section 2.3, I measure the level of Francoist support for each municipality, using the share of streets using dictatorial names. I interpret the fact of maintaining names from the dictatorship as a sign of social stagnation.

Whether and how foreign priests influence their parishioners to maintain old dictatorial honors is far from trivial. On the one hand, one could expect that the arrival of a foreign priest might be beneficial for removing these honors, as the previous local priest might have played the role of gatekeeper, preventing such changes. In that event, we would observe that upon arrival, the community would decide to remove all Francoist-sounding street names. On the other hand, given the considerable influence that foreign priests have on their parishioners, we could expect a second-order effect by which, given that individuals get closer to the Church, they start to pay less attention to the Francoist past. While foreign priests might not necessarily be convincing their parishioners on anything related to Franco's dictatorship, creating a closer community around Catholicism might trigger a higher conformism in the community, which would result in preserving dictatorial honors, among other measures. A third viable option is that religious leaders and the Spanish Church might no longer have a favorable opinion about that political regime. In such case, we would expect to find no effect on the arrival of a foreign priest, as parishioners would not associate in any way the Spanish Catholic Church with the dictatorship.

Figure 1.10 presents the evolution of Francoist street naming following the arrival of

a foreign priest to the community. One year after the arrival, treated municipalities have 0.07% more streets with a Francoist name, representing a 7.7% increase in the average number of Francoist streets. As time passes, such differences deepen. Six years after the arrival of a foreign priest, treated municipalities have 0.19% more Francoist streets than comparable villages with no foreign-born priest, representing an increase of 21.7% on the unconditional mean. As shown in Section 2.3, the percentage of streets honoring the Francoist dictatorship has steadily decreased throughout the study, going from 1.56% in 2001 to 0.2% in 2019. In turn, exposure to a foreign priest delays social change by up to 3 years.

Overall, this result highlights that while foreign priests do not necessarily convey a pro-Francoist speech, their ability to mobilize their communities towards more conservative and religious positions makes it easier for their communities to accept an old status quo.

1.6 What Is Driving the Effects?

1.6.1 Municipality Heterogeneous Effects

In this section, I explore how municipal characteristics act as modulators of the influence foreign-born religious leaders have on their parishioners.[26]

I begin by investigating whether local immigration plays a role in the influence of foreign priests. To do so, I use the 2001 Spanish Census to identify those municipalities with an above and below the median number of foreign-born individuals. This allows me to differentiate between those municipalities with frequent contact with immigrants from those with little to no contact. On the one hand, we could expect municipalities with a long tradition of immigration to be more receptive to foreign-born priests as other immigrants might ease the initial contact. Moreover, the immigrant population in these communities might act as the initial stepping stone into the parish, allowing the priest to reach the whole community. On the other hand, natives might not necessarily see foreigners positively, reject the arrival of a foreign priest, and move away from the church. This could happen in high-immigration municipalities, where natives might feel that their culture is challenged, and in low-immigration municipalities, where locals might hold stereotypical prejudices toward foreigners.

Figure 1.11 shows how the arrival of a foreign-born priest affects their parishioners

[26]For completeness, Figures 1.A7 and 1.A8 in the Appendix show that foreign priests do not differentially influence their parishioners along the presence of local religious grass-root movements, and local education. On the contrary, Figure 1.A9 shows that foreign-born priests are especially effective in bringing people to the church when placed in more demographically vibrant municipalities.

depending on the community's local immigration status.[27] We can see how foreign-born religious leaders are more effective in guiding more ethnically diverse municipalities, especially in bringing these parishioners back to the church. However, foreign priests do not differentially influence individuals' political ideology, especially regarding radical right parties. These results suggest that foreign-born priests bring new parishioners to the church in diverse municipalities, but they influence their communities' political views irrespective of the local composition. Thus, it does not support the contact hypothesis theory (Allport et al., 1954).

The previous results highlighted that foreign priests were more effective in more diverse communities. However, whether foreign priests could be an effective tool in helping minorities integrate into their communities is still under question. To provide evidence in that respect, I use the 2001 Spanish Census and identify those municipalities that had Latin American and Maghrebi communities in 2001. I focus on those two subgroups as they constitute the most significant subgroups of immigrants living in Spain, amounting to 0.39% and 0.25% of the Spanish population in 2001, respectively. Furthermore, while Latin American immigrants are culturally and religiously close to Spanish-born people, Maghrebi immigrants are not, having been historically portrayed as the "others" (Martin Corrales, 2002). Figure 1.12 displays the influence of foreign-born priests on those municipalities with long traditions with Latin American and Maghrebi populations. We can observe how foreign-born priests are equally effective at influencing those communities with a long tradition of Latin American and Maghrebi communities. These results suggest that foreign-born priests effectively mobilize their native followers towards closer communities around traditional values in ethnically diverse parishes but do not accommodate culturally and religiously similar immigrants.

I then examine whether foreign priests influence their parishioners differently depending on the local political background. To that end, I identify which municipalities have a historically conservative profile and which ones do not. I use voting data for all national and European elections between 1975 and 1999 and classify municipalities as conservative when they voted more for right-wing parties and non-conservatives when they voted more for left-wing parties. Figure 1.13 shows how the presence of a foreign priest in a community affects its parishioners along with the community's political background. We can see that local political ideology does not affect the religious influence of foreign priests. However, that is not the case when it comes to political matters. First, we can see that foreign priests guide towards more conservative positions those parishioners in municipalities that were historically liberal. Second, we can also observe that such political influence is reverted when it comes to the extreme right. Foreign-born priests

[27] Throughout all heterogeneous effects, I look at the influence six years after the arrival of a foreign priest, as the Spanish Church's policy is to maintain religious leaders in their parishes for at least that duration.

influence their parishioners away from radical-right positions, especially in those historically conservative municipalities. Lastly, while traditionally conservative parishes deviate from radical-right positions, with the arrival of foreign priests, they also stop questioning the old status quo, as shown by the higher maintenance of streets honoring Franco's dictatorship. Taking all results together, they suggest that foreign priests effectively gain votes for the conservative parties, mainly at the expense of left-leaning parties in liberal strongholds.

Finally, I examine whether the local economic situation influences how foreign-born priests interact with their communities. To do so, I use the 2001 Spanish Census to identify those municipalities with above and below-median unemployment rates. This classification is crucial as religion could be perceived as a good of last resort, especially appealing in municipalities with worse economic outcomes. Figure 1.14 shows that foreign-born priests are more effective than their local counterparts in bringing people to the church, especially in municipalities with more economic needs. These results suggest that foreign religious leaders are better at promoting the social support of the Catholic Church.

Overall, these results shed light on which parish characteristics are crucial in understanding the effective influence of foreign-born priests. In this section, I have shown that foreign-born religious leaders are particularly influential in ethnically diverse municipalities, promoting closer communities around religious values; promoting a conservative, and traditional mindset, away from liberal and extreme-right positions; and standing by those communities in more need.

1.6.2 Priest Heterogeneous Effects

In this section, I explore which of the priests' characteristics act as modulators of the influence exerted by foreign-born religious leaders.[28]

I first examine whether foreign-born religious leaders differentially influence their communities depending on whether they were ordained in a Spanish seminary. We could expect that studying in a Spanish seminary teaches priests specific theological approaches and the Spanish cultural idiosyncrasy, which could lead to some partial cultural assimilation. Whether such assimilation is favorable or not for the priest's endeavors is far from trivial. On the one hand, the know-how of the local culture could be beneficial to access the whole community, thus promoting Catholic teachings more effectively. On the other hand, a complete adaptation of the foreign priest to the local culture could also prove detrimental as it could ultimately lead to losing the religious drive that brought them to

[28]For completeness, Figures 1.A10 and 1.A11 in the Appendix show that the religious leader's tenure and age do not play a determinant role in explaining foreign priests' influence. On the contrary, Figure 1.A12 shows that foreign-born priests belonging to a religious order are more effective at revitalizing religiosity than comparable local order members. Similarly, Table 1.A9 shows that the appointment of Pope Francis reinforced the political influence of foreign-born priests.

Spain. Figure 1.15 shows whether the presence of a foreign priest in a community affects its parishioners depending on whether the priest studied in a Spanish seminary. I find no significant differences in any of the outcomes of interest, suggesting that studying in a Spanish seminary, as opposed to studying in a foreign seminary, is used more as a decoy to attract foreign priests but having no significant impact on their role as religious leaders.

I further look at whether the nationality of foreign priests matters. To that end, I differentiate foreign priests between those born in a Latin American country and those born elsewhere. I follow this dichotomous approach for a couple of reasons. While Latin Americans share with Spaniards their language and rich cultural, religious, and historical similarities, those priests born elsewhere will likely only share the religious aspect. This fact might help Latin American priests integrate more effectively into the local communities as opposed to those coming from other countries, which could have a deeper influence on the community. Figure 1.16 shows how priests born in Latin America and elsewhere influence Spanish parishioners, respectively, as opposed to having a local priest. We can see that foreign-born priests do not differentially influence their communities concerning their country of birth.[29]

Overall, I show that having similar cultural and language traits to the local population is not the key determinant explaining foreign-born priests' effective influence. Moreover, I find no evidence that priests' age and tenure are good predictors of foreign-born priests' influence. These results suggest that the influence foreign-born priests have on their communities comes from unobserved characteristics common to all foreign-born priests, such as their inherently higher motivation and devotion. These results are in line with recent sociological research, which describes the role of the Ghanaian Adventist Church in the Netherlands (Koning, 2011) and Korean protestants in the United States (Kim, 2015).

1.7 Conclusion

This paper provides novel evidence of the importance of religious leaders in shaping religious and sociopolitical attitudes. To do so, I follow a twofold approach. First, I collect a unique dataset containing the universe of Catholic priest appointments taking place in rural Spain at any point in time between 2000 and 2019 and including detailed information on the priests' inherent traits, such as country of birth, order, age, tenure, and education. I further complement such data with a novel phone survey covering the priests' political opinions and working habits. Second, I exploit plausibly exogenous variation in the

[29]For completeness, Figure 1.A13, and Figure 1.A14 show suggestive evidence that priests' ideology, and hierarchy-leanings, are important in explaining foreign-born religious leaders' influence, respectively. However, these estimates come from the priest's survey, which might suffer from small power considerations.

assignment of foreign-born priests to Spanish parishes to provide a causal interpretation of how these religious leaders shape the native sociopolitical attitudes. I document that 13.3% of all the priests in the sample were not born in Spain, arriving primarily from Latin American countries and with a missionary vocation.

I find that foreign-born priests are influential traditional community builders. I show that upon arrival, foreign-born religious leaders influence their communities towards higher religious practice, measured by the number of Catholic weddings and births. Similarly, foreign priests effectively bring their parishioners' political opinions closer to Catholic-aligned positions at the expense of left-leaning and radical right parties. However, such extra influence comes at a price, as the more decisive predominance of the Spanish Catholic church prevents parishioners from challenging an old status quo, such as the Francoist legacy, limiting social progress.

I further find evidence that foreign-born religious leaders significantly influence ethnically diverse municipalities, suggesting that a contact hypothesis story is not at play. That stronger influence comes from the native population, reinforcing the idea that foreign-born religious leaders are effective at creating strong in-group cohesion among the native population. Additional evidence highlights that cultural and linguistic distance from the local community are not relevant factors in explaining the stronger influence of foreign-born priests. This suggests that the extra influence exerted by foreign religious leaders is driven by unobserved characteristics common to all foreign-born priests, such as their inherently higher motivation and devotion.

This paper has important implications for policy-making as it highlights religious leaders' central role in shaping social values. I study this influence in a context in which religious leaders have traditionally been one of the leading community pillars, but that recently undergone a strong secularization process, ultimately challenging their social centrality. More specifically, this study focuses on an overlooked process, i.e., the "reversal of the mission", showing that the arrival of foreign-born priests, with their inherent missionary devotion, has led to a secularisation setback. Our results highlight the important role of supply-side factors in explaining European secularization.

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Tables and Figures

Table 1.1: Descriptives - Priests' country of birth

Country of Origin	Share	N
Spanish	0.867	2243
Colombian	0.057	150
Other Latin American	0.024	64
Other nationalities	0.022	58
Mexican	0.016	43
Polish	0.011	29
Total	1	2587

Note: The table provides descriptive evidence of the nationality of those priests working in my sample of analysis. Own elaboration based on the information collected at the Spanish Episcopal Conference.

Table 1.2: Summary Statistics (at baseline)

	Never Foreign-Led	Ever Foreign-Led	p-value
Population	762.422	641.897	0.042
Youth share	0.127	0.122	0.046
Retired share	0.325	0.334	0.041
Uneducated share	0.212	0.212	0.918
Technical education share	0.717	0.719	0.727
Singles share	0.378	0.365	0.000
Divorced share	0.004	0.004	0.123
Immigrants share	0.013	0.019	0.000
Labor participation (Male)	0.575	0.562	0.002
Labor participation (Female)	0.269	0.274	0.231
Unemployed share	0.106	0.100	0.059
Self-employed share	0.298	0.297	0.931
Temporal workers share	0.222	0.213	0.064
Farmers share	0.287	0.281	0.403
Share Right-wing	0.406	0.390	0.001
Grassroot Catholicism	0.074	0.058	0.119

Note: The table provides a comparison of the baseline characteristics between municipalities that had a foreign priest between 2000 and 2019 (ever foreign-led) and those that did not (never foreign-led). The information at the municipal level is extracted from the 2001 Census. The share of votes to right-leaning parties was calculated using all national and European elections held between 1975 and 2000. Grassroot Catholicism identifies whether there existed in 2001 any grassroot Catholic initiative in the municipality.

Table 1.3: Priests' Characteristics - Foreign vs. Local

		Foreign Priests		**Local Priests**		
	Baseline	Mean	SD	Mean	SD	p-value
Panel A: Full Sample Characteristics						
Foreign	0.134					
Religious Order	0.100	0.150	0.358	0.092	0.290	(0.004)
Spanish Educated	0.891	0.223	0.417	1.000	0.000	(0.000)
Observations	2587		346		2241	2587
Panel B: Individual Characteristics (from Annual Directories)						
Age (in years)	63.531	51.250	11.680	64.659	14.645	(0.000)
Tenure (in years)	36.431	20.647	11.757	37.884	15.887	(0.000)
Observations	808		68		740	808
Panel C: Individual Characteristics (from Survey)						
C.1) Missionary Status						
Missionary	0.292	0.667	0.480	0.248	0.433	(0.000)
Missionary (in years)	1.905	4.963	4.701	1.546	4.668	(0.001)
C.2) Political Ideology						
Social Conservative	6.056	6.467	1.854	6.006	1.794	(0.230)
Economic Liberal	5.593	4.720	3.127	5.701	2.306	(0.125)
Church is Right	0.665	0.520	0.510	0.683	0.467	(0.139)
Discuss Political Issues	2.210	2.074	1.072	2.226	0.916	(0.485)
Economic Situation	3.567	2.926	0.958	3.645	0.783	(0.001)
C.3) Working Habits						
Church Attendance (%)	23.706	21.462	21.967	23.975	19.212	(0.581)
Mass Hours (weekly)	9.769	9.704	5.485	9.777	5.167	(0.948)
Family Planning	0.537	0.500	0.508	0.542	0.499	(0.665)
Minorities Integration	0.337	0.375	0.492	0.332	0.472	(0.643)
Social Care	0.667	0.719	0.457	0.660	0.475	(0.499)
Village Participation	0.375	0.438	0.504	0.368	0.483	(0.462)
Num. Activities	2.868	3.074	1.466	2.843	1.815	(0.457)
Parish-Local Council Relation	2.400	2.259	1.347	2.417	1.186	(0.564)
Open Question (length)	133.809	136.519	114.324	133.491	109.194	(0.897)
Observations	257		27		230	257

Note: This table shows the distribution of priests' characteristics as follows: Baseline (Col. 1); Foreign priests (Col. 2-3); Local Priests (Col. 4-5). The p-value of the difference between Foreign and Local priests is reported in Column 6. Missionary is an indicator variable that identifies if a priest has ever worked/is working as missionary (See 6.2 and 7 in Section 1.C). Missionary (in years) measures the cumulative time that an individual has ever worked as missionary. Social Conservative is a weighted sum of Questions 11-16 in Section 1.C. Economic Liberal is a weighted sum of Questions 9 and 10, as exposed in Section 1.C in the Appendix. Both Social Conservative and Economic Liberal have a distribution that go from 0 to 10. Church is Right is an indicator variable that identifies if a priest does not question the Pope's political views (See Questions 17 and 18). Discuss Political Issues corresponds to Question 23 in the survey and measures how much a priest talks with his parishioners about political issues. Economic Situation corresponds to Question 19 and measures how bad the priesthood sees the current economic situation. Church attendance measures the percentage of the priest's parishioners that attend frequently the Sunday mass (See Question 20). Mass Hours is a variable that measures the hours a priest employs directly officiating masses (See Question 21). Family Planning, Minorities Integration, Social Care, and Village Participation are indicator variables that identify if a given priest participates in any related activity (See Question 24). Num Activities is a discrete variable that identifies in how many activities a given priest participates (See Question 24). Parish-Local Council Relation is a discrete variable that identifies the level of disagreement between local council and parish (See Question 25). Open Question is a continuous variable that measures the length of Open Question 26. All other information is self-explanatory.

Table 1.4: Placebo test - Influence of foreign priests on sociopolitical attitudes

	(1) Cath	(2) Civil	(3) Birth	(4) Right	(5) Left	(6) Cons	(7) Rad. Right	(8) Francoist
Foreign	0.0406	-0.00850	0.153	0.00450	-0.00330	0.00340	0.0010**	0.0293
	(0.0554)	(0.0611)	(0.1376)	(0.0028)	(0.0026)	(0.0029)	(0.0005)	(0.0374)
Observations	71079	71079	71039	44568	44568	44568	44568	109741

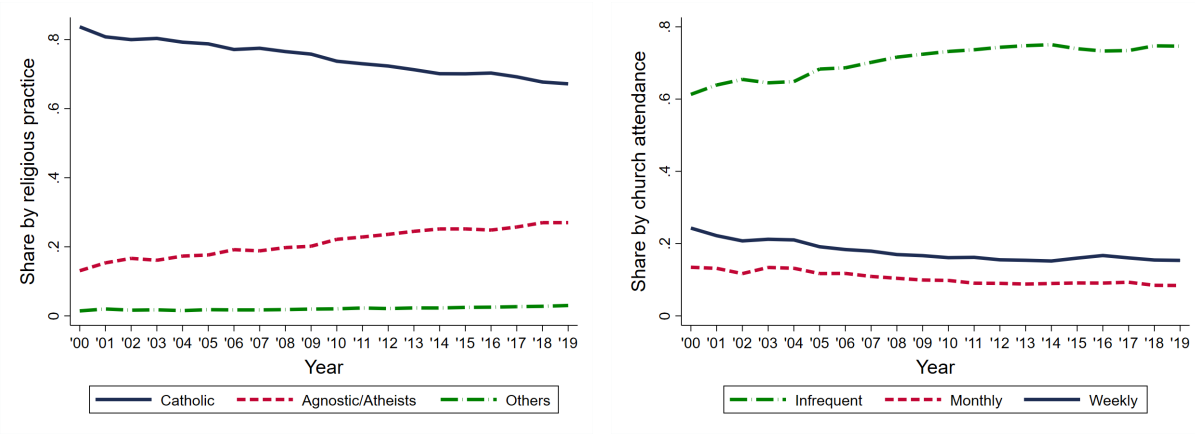
Note: This table tests whether the evolution of an outcome variable from $t-2$ to $t-1$, differs between municipalities treated at t from those not treated. Cath identifies the number of Catholic weddings performed in a municipality, and Civil the number of civil-only weddings. Birth measures the number of births taking place in the municipality. Right, Left, Cons, and Rad. Right are variables that measure the share of the votes in a municipality that were earned by Right, Left, Conservative, and Radical Right parties, respectively. Francoist is a variable measuring the share of streets in a municipality that are honoring Francoist regime. All regressions include municipality and time fixed effects, and control by row population, number of previous priest changes, and whether the current priest is in solidum. Standard error are clustered at the municipality level.

Table 1.5: Effect of a new priest's arrival vs. new foreign priest's arrival

	(1) Cath	(2) Civil	(3) Birth	(4) Right	(5) Left	(6) Cons	(7) Rad Right	(8) Francoist
New Priest	-0.2836***	0.2320***	-0.0848	0.00120	-0.00300	0.00200	-0.000900	0.1119***
	(0.0745)	(0.0331)	(0.1004)	(0.002)	(0.002)	(0.002)	(0.001)	(0.044)
New Foreign Priest	0.2973***	-0.1747***	-0.0679	-0.00270	0.00170	0.00270	-0.0054***	0.3777***
	(0.0458)	(0.0226)	(0.1053)	(0.0037)	(0.0035)	(0.0038)	(0.0019)	(0.0258)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	78651	78651	78596	46454	46454	46454	46454	121059
Joint p-value	0.856	0.112	0.193	0.709	0.742	0.233	0.002	0.000
Mean Dep. Var.	1.587	1.286	6.506	0.568	0.408	0.538	0.0298	0.868

Note: The table tests whether the arrival of a new foreign-born priest to a municipality affects a series of outcomes. Cath and Civil identifies the number of catholic and civil-only weddings, respectively. Right, Left, Cons, and Rad. Right identify the voting share to Right, Left, Conservative, and Radical Right parties, respectively. Francoist measures the percentage of the street in a given municipality that honor Franco's dictatorship. Joint p-value tests whether the sum of New Priest and New Foreign Priest is jointly equal to zero. All regressions include municipality and month-year fixed effects, and control by population, squared population, number of previous priest changes, and whether the priest shares his office (i.e., in solidum). Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Figure 1.1: Religiosity evolution (2000-2019)

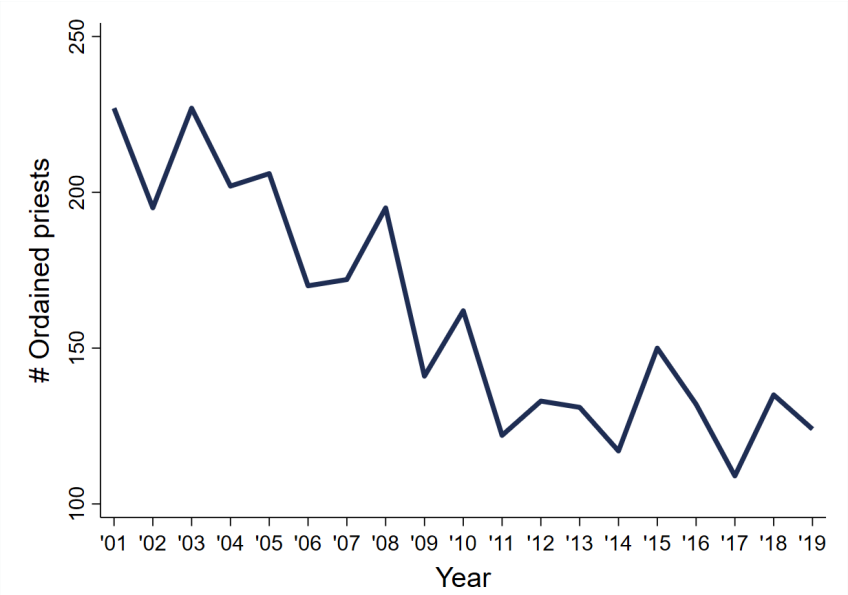


(a) By Religious practice

(b) By Church Attendance

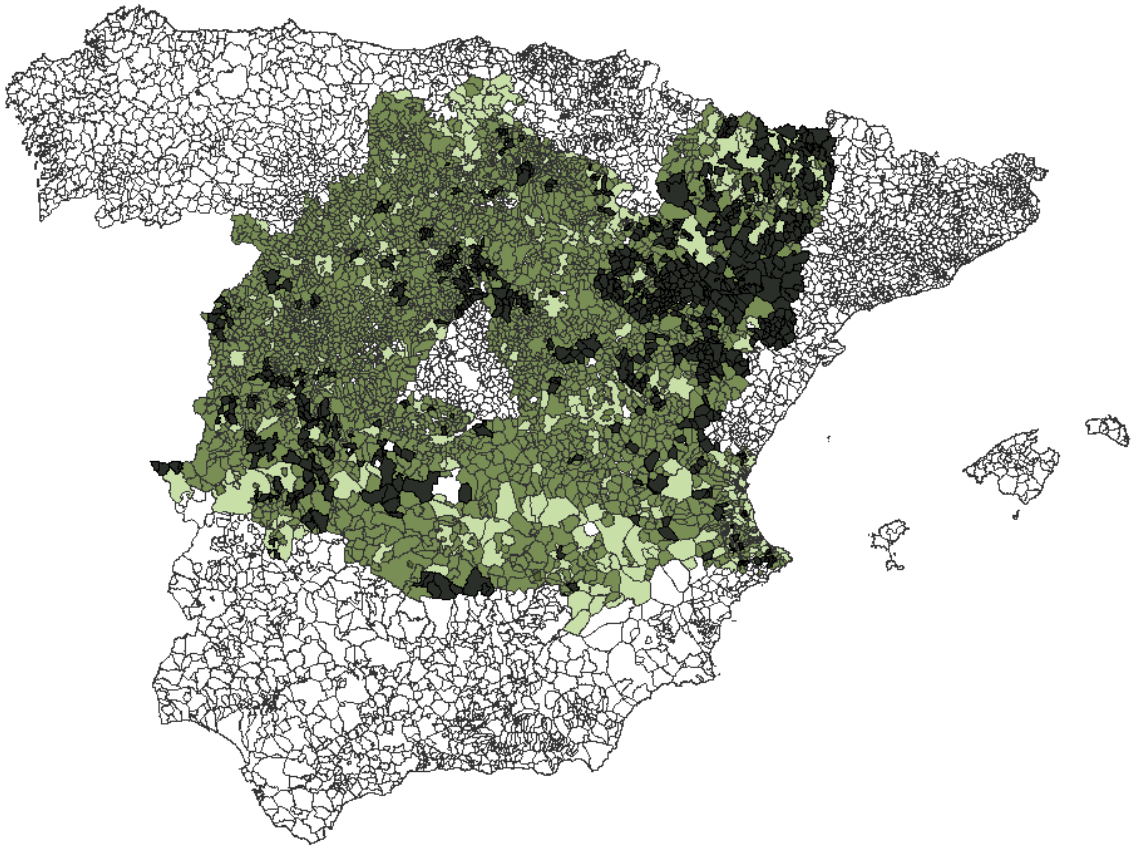
Note: The figure reports how self-reported religiosity has evolved over time. I use collapsed information at the yearly level from the monthly public opinion surveys conducted by the Spanish Center of Sociological Research (CIS). Subfigure 1.1a displays the overall evolution by religious practice, and Subfigure 1.1b shows the evolution by church attendance, for those individuals reporting following a denomination.

Figure 1.2: Ordainment evolution (2001-2019)



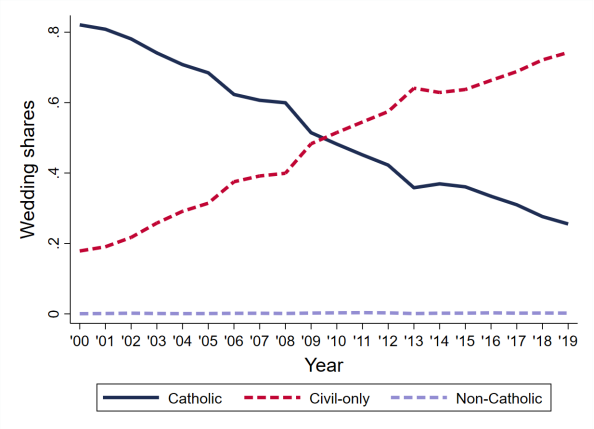
Note: The figure displays the number of ordained priests per year in Spanish seminaries. Own elaboration based on the information collected at the Spanish Episcopal Conference.

Figure 1.3: Distribution by municipalities

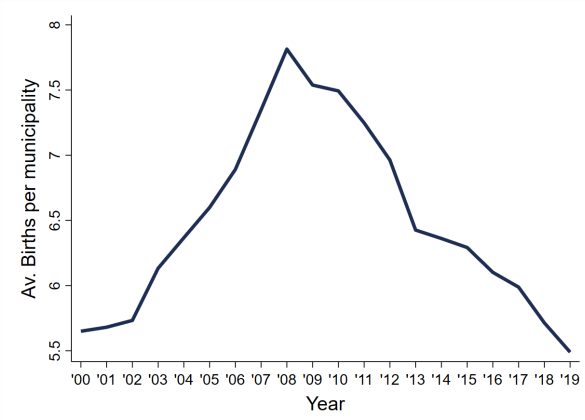


Note: The figure displays the relationship of municipalities used in the analysis. The municipalities in dark green represent those in which at any point in time between 2000 and 2019, a foreign priest was present in the municipality. The municipalities in olive green represent all those other municipalities used in the analysis, which never had a foreign priest. Municipalities in light green represent those municipalities not used in the study, as they are composed by several population centers and/or parishes. Own elaboration based on the information collected at the Spanish Episcopal Conference.

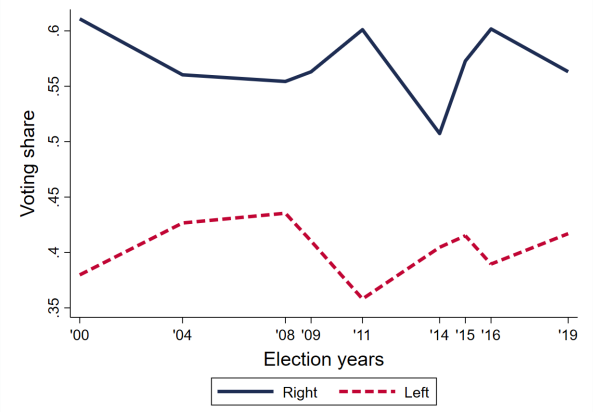
Figure 1.4: Outcome evolution (2000-2019)



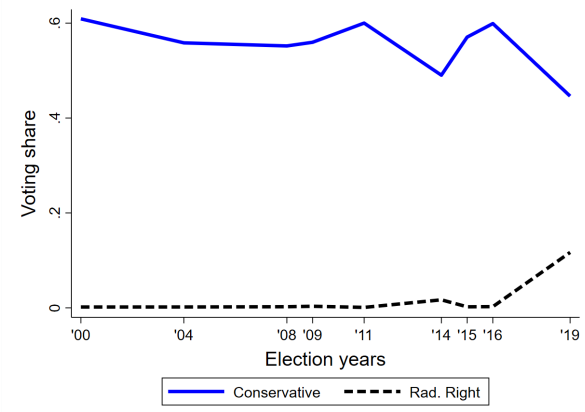
(a) Distribution by wedding type



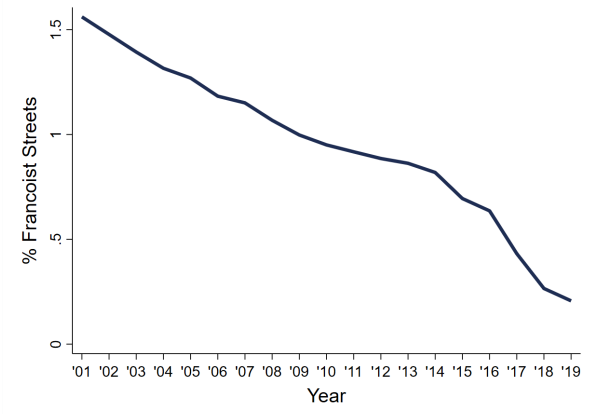
(b) Distribution of Births



(c) Right vs. Left



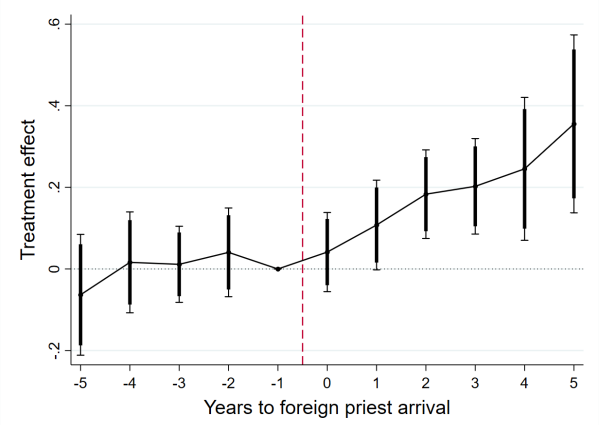
(d) Conservative vs. Rad. Right



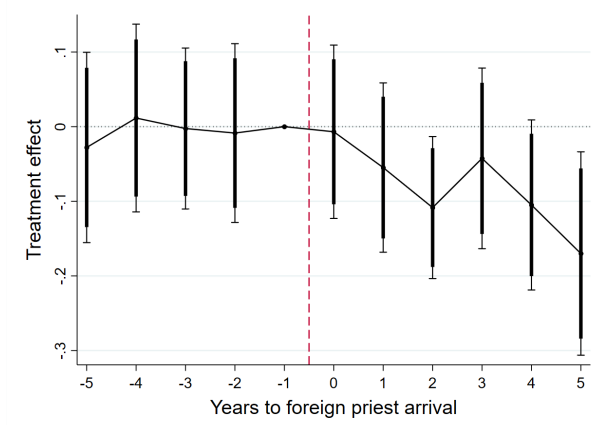
(e) Share of Francoist streets

Note: The figure reports how a series of outcomes have evolved over time. Subfigure 1.4a shows the evolution of the relative share of Catholic, non-Catholic, and civil weddings. 1.4b displays the average number of births per municipality in the sample of study. 1.4c shows the evolution of the voting share of right and left-leaning parties, while 1.4d displays the evolution of the voting share of conservative and radical right parties. 1.4e reports the percentage of streets in the sample of study having a Francoist name.

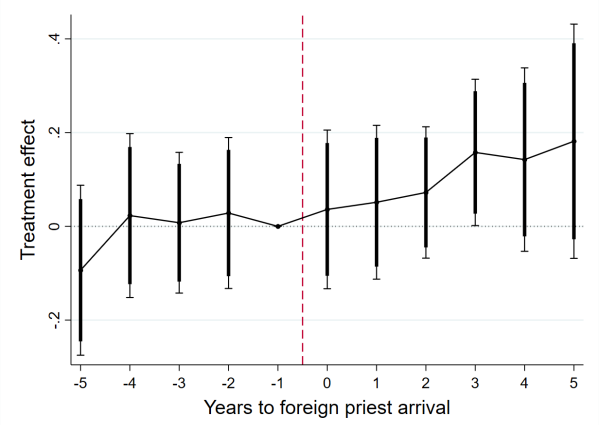
Figure 1.5: Effect of a foreign priest's arrival on religious outcomes



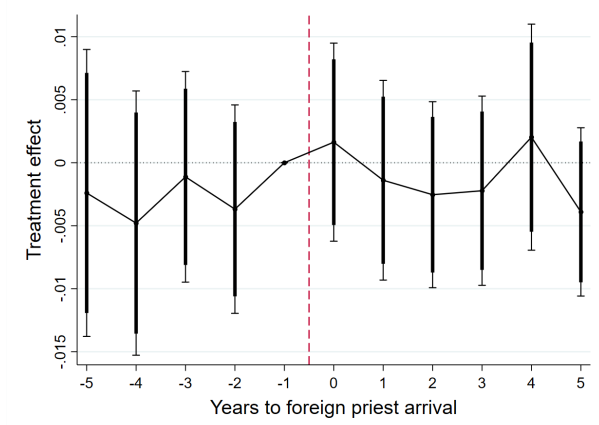
(a) # Catholic weddings



(b) # Civil-only weddings



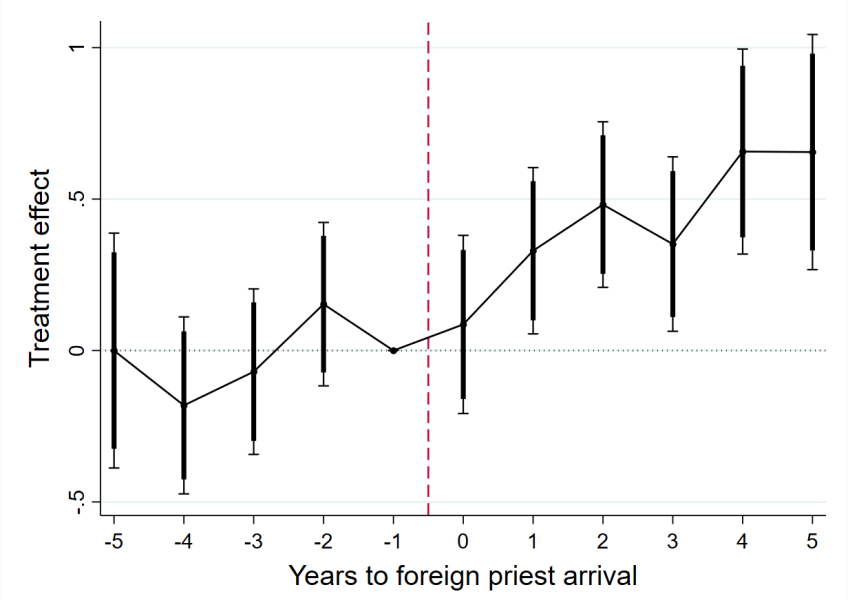
(c) # All weddings



(d) # Weddings (Other Denominations)

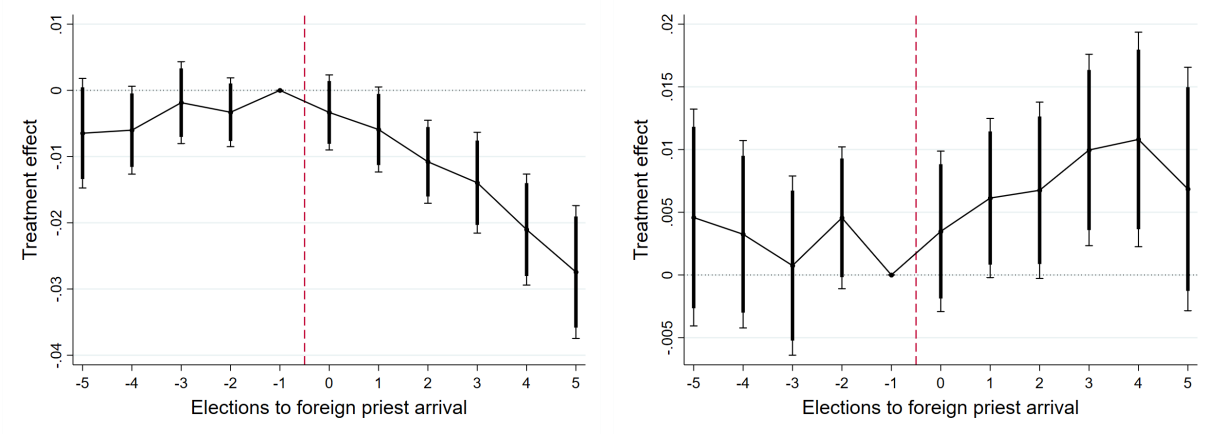
Note: This figure shows whether the arrival of a foreign priest to a municipality affects the probability of getting married, by wedding ritual. Subfigures 1.5a displays how it affects Catholic weddings, 1.5b studies how it affects civil-only weddings, 1.5c whether it affects the total number of weddings carried out in a given municipality, and 1.5d whether it affects the wedding probability in other denominations. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A10 displays numerically this figure.

Figure 1.6: Effect of a foreign priest's arrival on fertility



Note: This figure shows whether the arrival of a foreign priest to a municipality influences the number of births in the municipality. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A10 displays numerically this figure.

Figure 1.7: Effect of a foreign priest's arrival on political outcomes - Left vs. Right



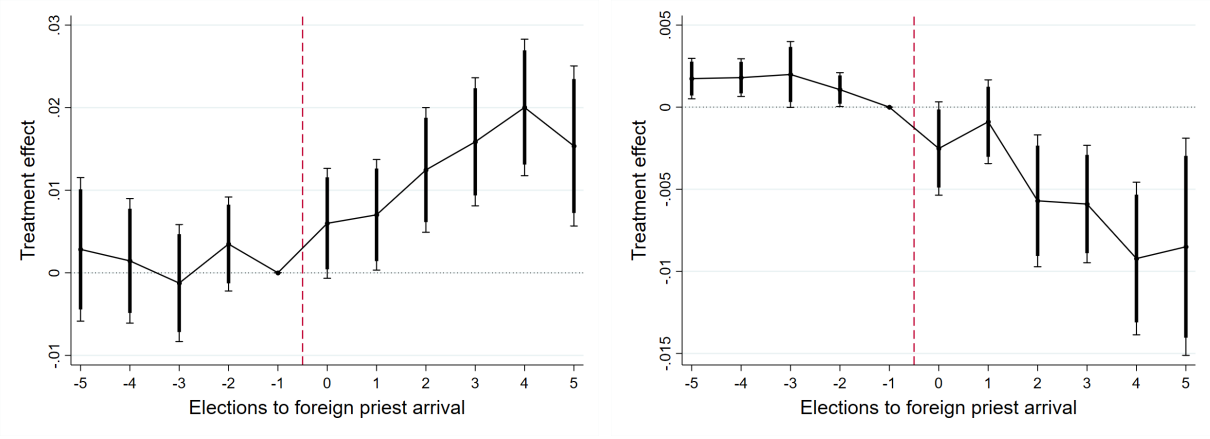
(a) Left-wing parties

(b) Right-wing parties

Note: This figure shows whether the arrival of a foreign priest to a municipality affects its voting behavior. Subfigures 1.7a shows how it affects the voting share of left-wing parties and Subfigure 1.7b displays how it affect the voting share of right-wing parties. The x-axis identifies the number of national and European elections since the arrival of a foreign priest. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A11 displays numerically this figure.

Figure 1.8: Effect of a foreign priest's arrival on political outcomes

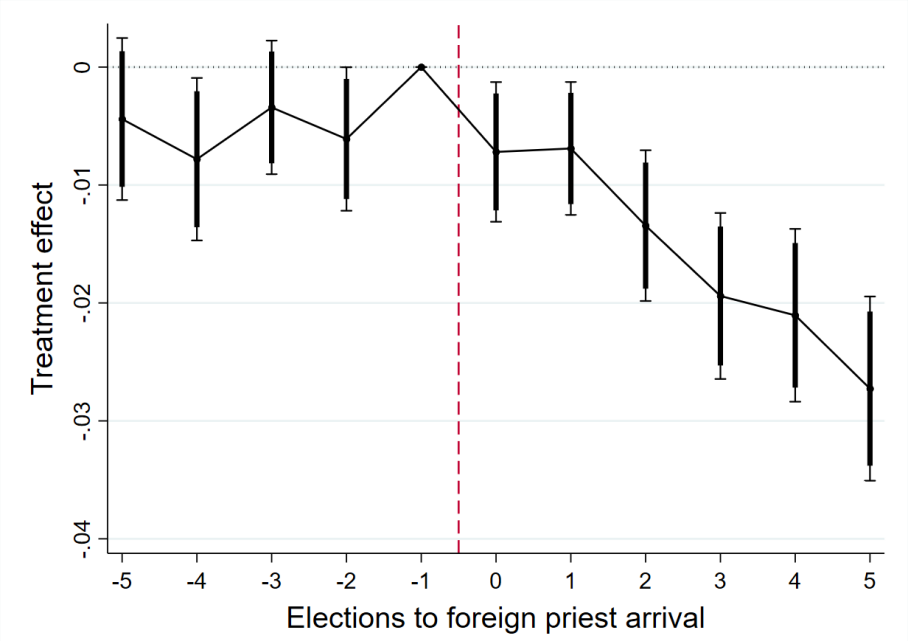
Conservative vs. Rad. Right



(a) Conservative parties (b) Rad. Right parties

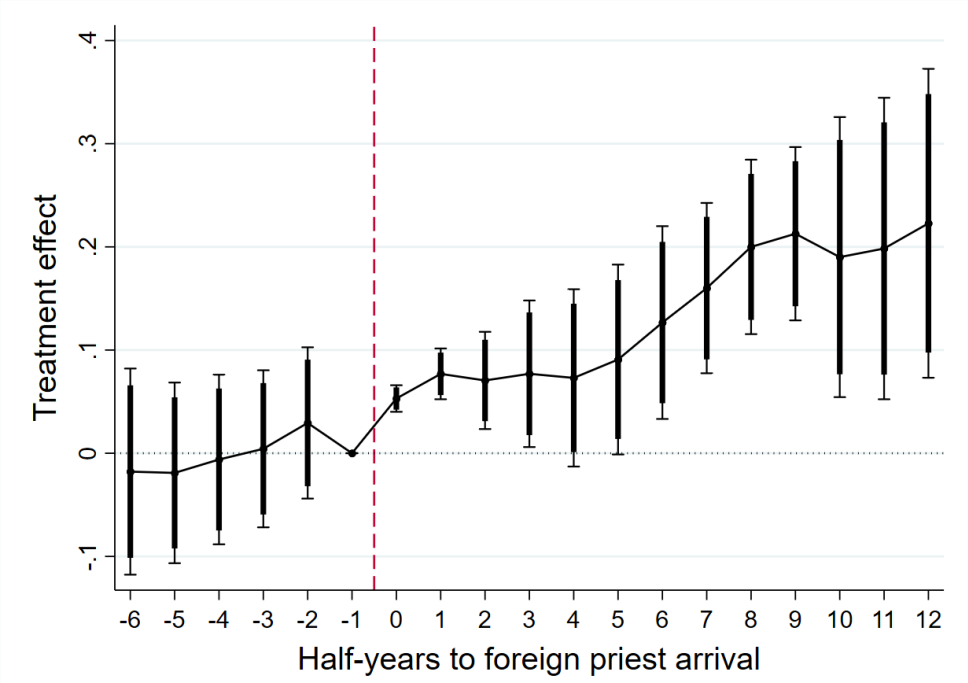
Note: This figure shows whether the arrival of a foreign priest to a municipality affects its voting behavior. Subfigures 1.8a displays how it affect the voting share of conservative parties and Subfigure 1.8b how it affects the voting share of radical right parties. The x-axis identifies the number of national and European elections since the arrival of a foreign priest. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A11 displays numerically this figure.

Figure 1.9: Effect of a foreign priest's arrival on voting absenteeism



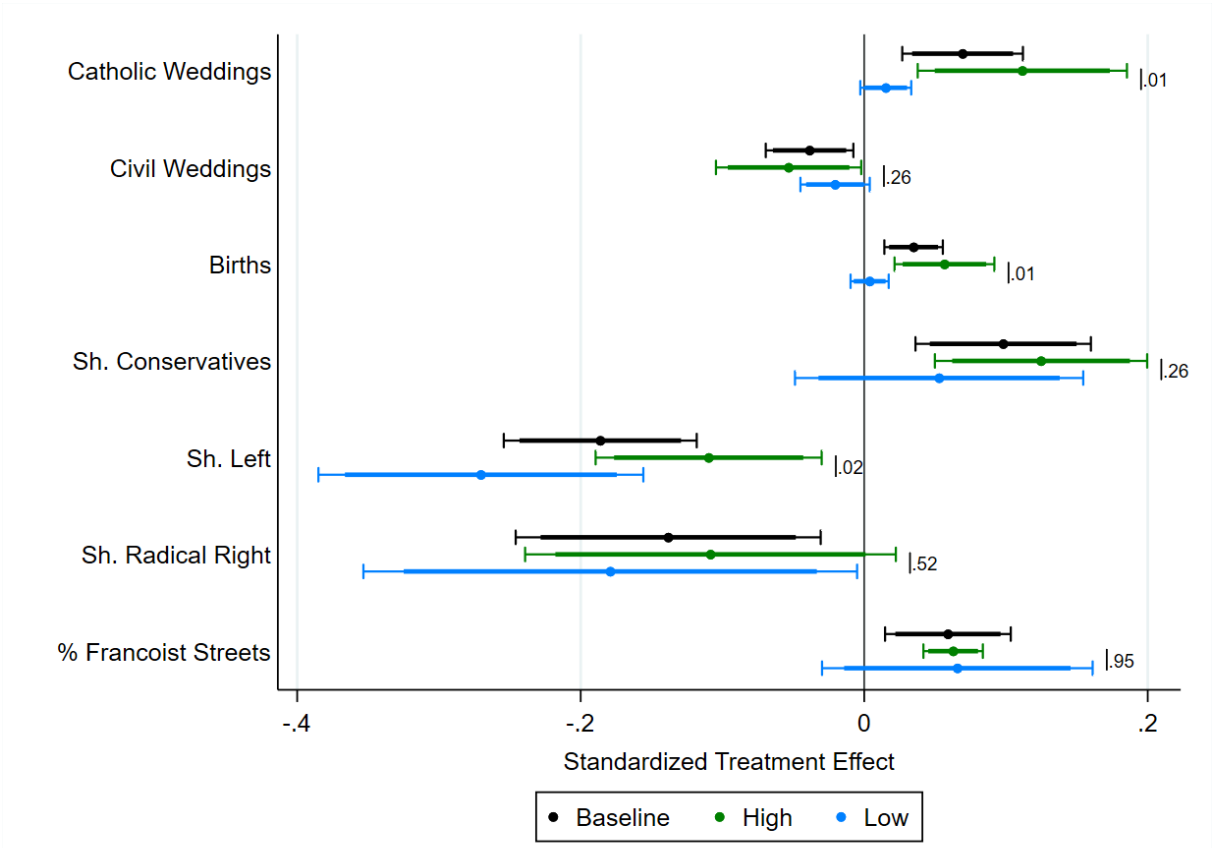
Note: This figure shows whether the arrival of a foreign priest to a municipality has an effect on its electoral participation. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A11 displays numerically this figure.

Figure 1.10: Effect of a foreign priest's arrival on Francoist street naming



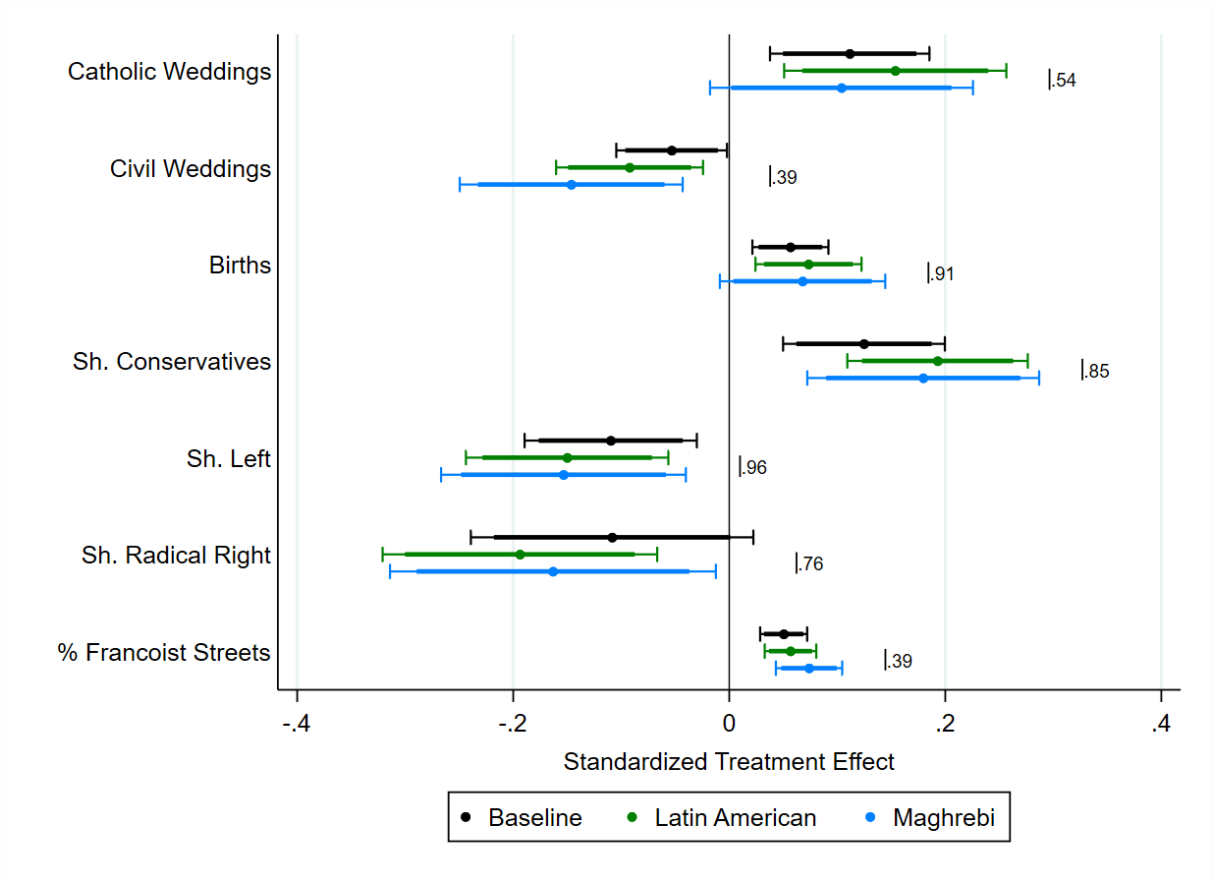
Note: This figure shows whether the arrival of a foreign priest to a municipality has an effect on the number of streets in the municipality with a Francoist name. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A12 displays numerically this figure.

Figure 1.11: Effect of a foreign priest's arrival by Local Immigration



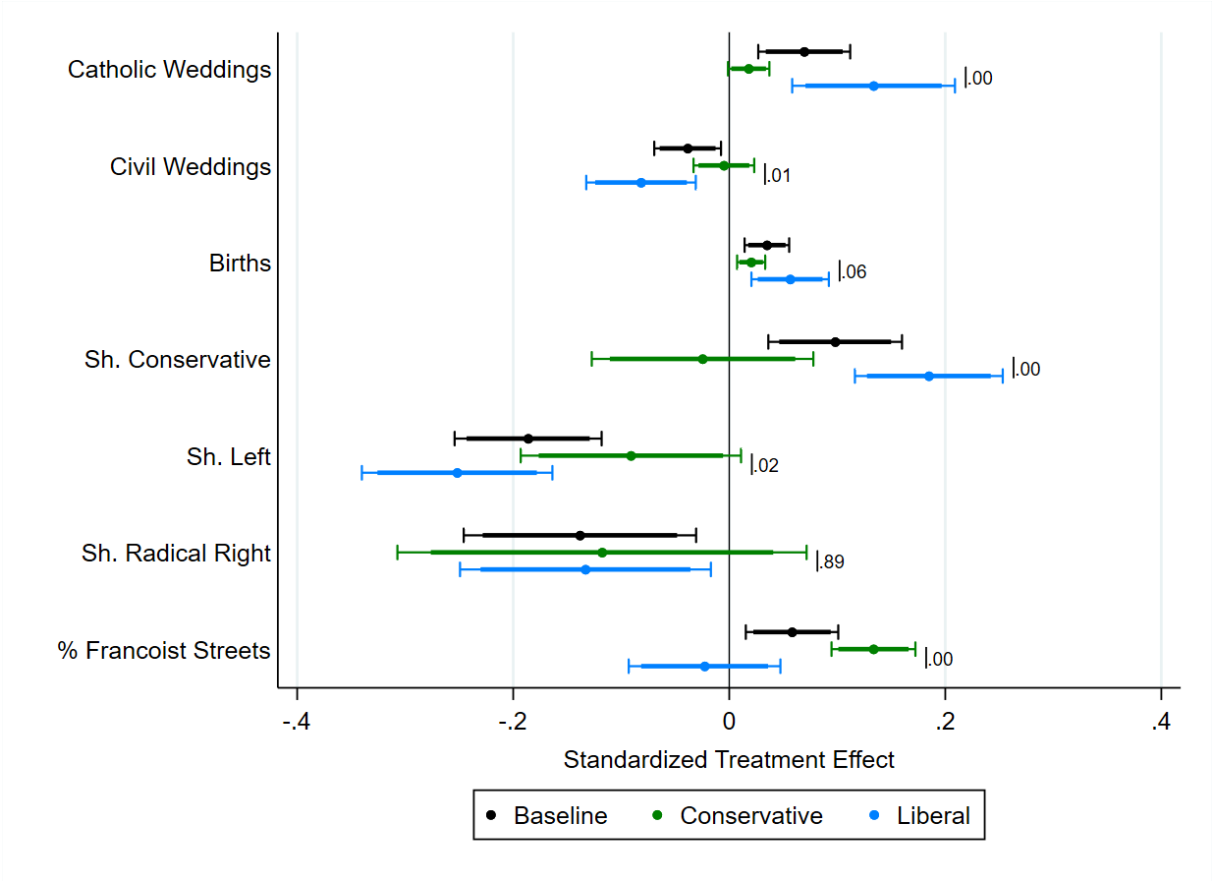
Note: This figure shows whether the arrival of a foreign priest to a municipality motivates the change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign priest. See Section 2.3 for further details. It differentiates between those municipalities with an above median number of foreign-born individuals, in green, and those with a below median number of foreign-born individuals, in blue. Municipalities are classified using data from the 2001 Spanish Census. All the results are reported in standardized units. p-values from Wald tests for the equality of two estimates are reported next to each solid vertical line between the two estimates. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A13 displays numerically this figure.

Figure 1.12: Effect of a foreign priest's arrival by Local Immigration
Latin American vs. Maghrebi



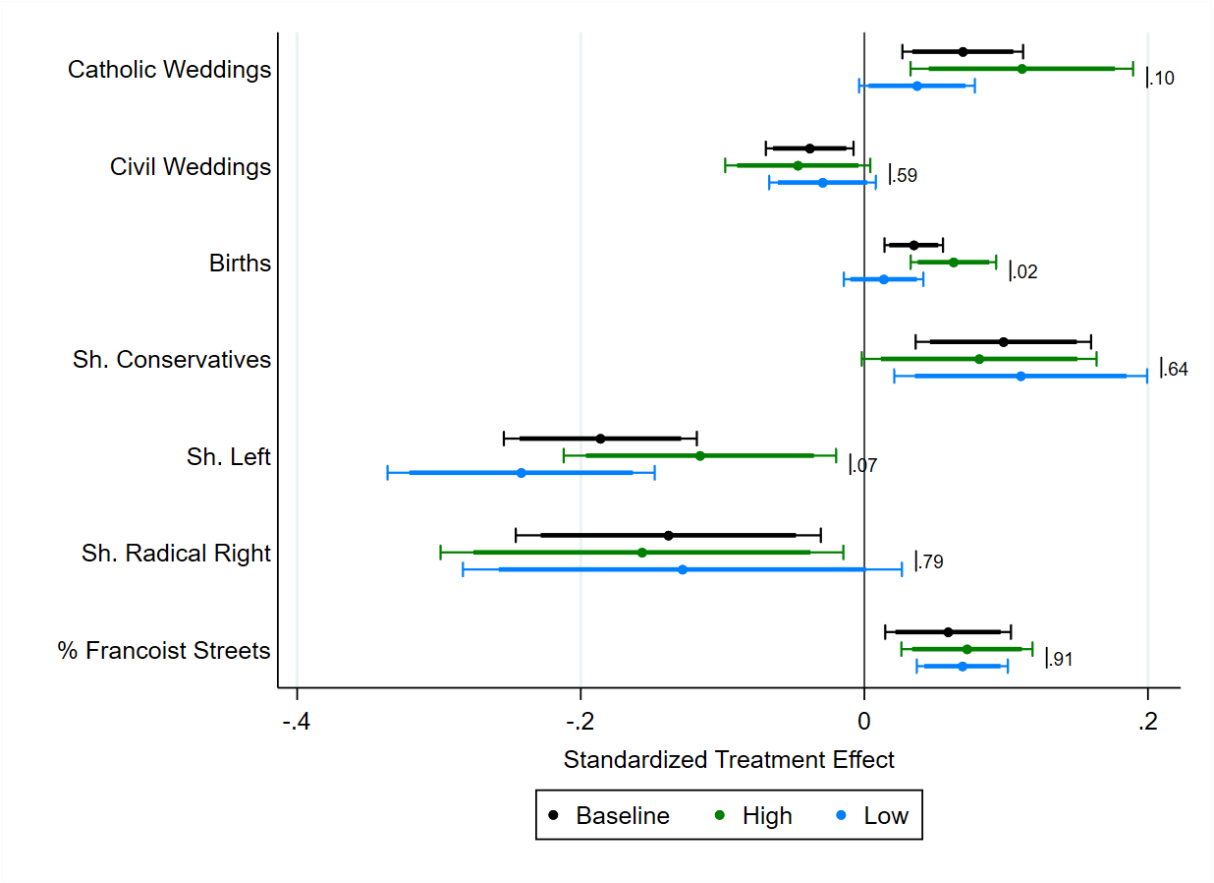
Note: This figure shows whether the arrival of a foreign priest to a municipality motivates the change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign priest. See Section 2.3 for further details. It differentiates between those municipalities with an above median number of foreign-born individuals, baseline, with an above median number of Latin American individuals, in green, and those with an above median number of Maghrebi individuals, in blue. Municipalities are classified using data from the 2001 Spanish Census. All the results are reported in standardized units. p-values from Wald tests for the equality of two estimates are reported next to each solid vertical line between the two estimates. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A14 displays numerically this figure.

Figure 1.13: Effect of a foreign priest's arrival by Local Conservatism



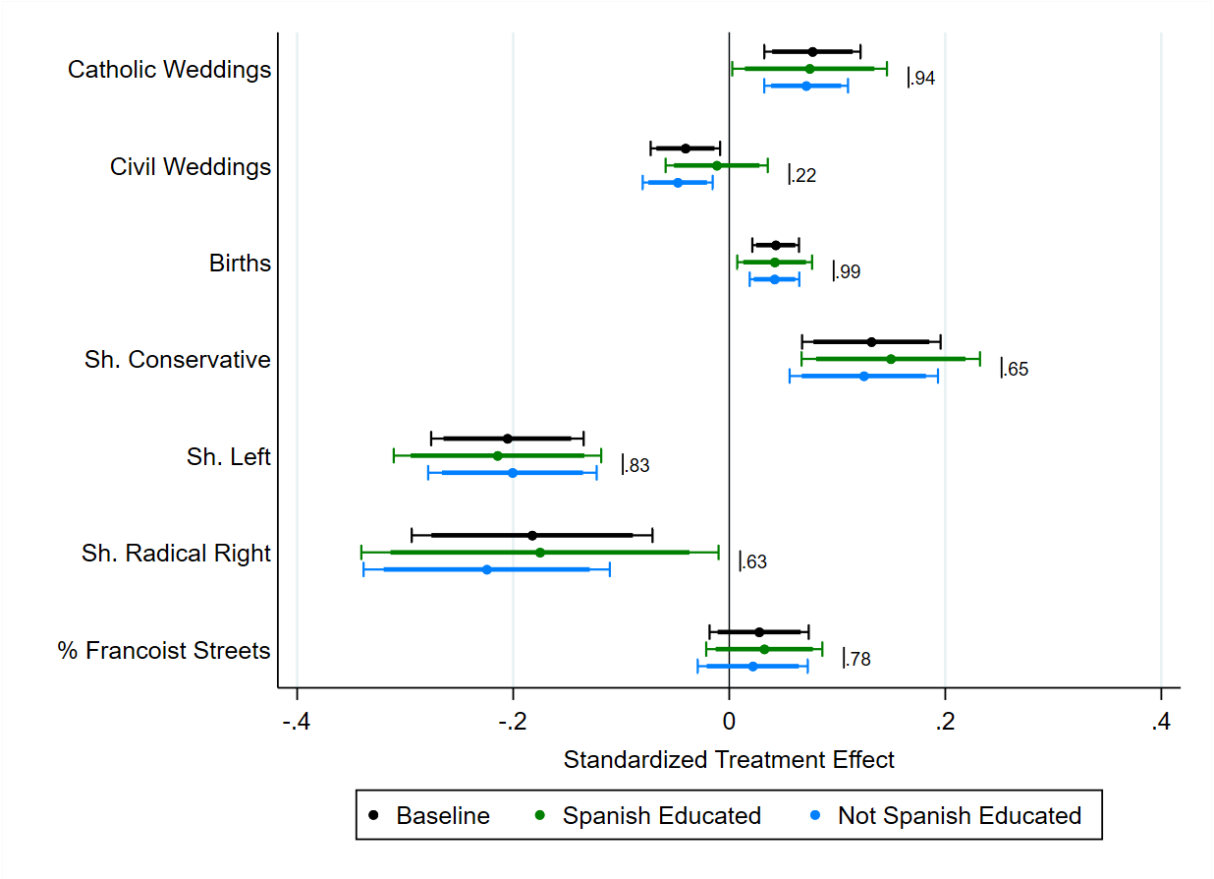
Note: This figure shows whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign priest. See Section 2.3 for further details. It differentiates between those municipalities with a historically conservative voting behavior (i.e., above median vote share for right-wing during the period 1975-1999), in green, and those with a historically liberal voting behavior (i.e., below median vote share for right-wing during the period 1975-1999), in blue. Municipalities are classified using data on all national and European elections that took place between 1975 and 1999, available at the Spanish Interior Ministry. All the results are reported in standardized units. p-values from Wald tests for the equality of two estimates are reported next to each solid vertical line between the two estimates. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A15 displays numerically this figure.

Figure 1.14: Effect of a foreign priest's arrival by Local Unemployment



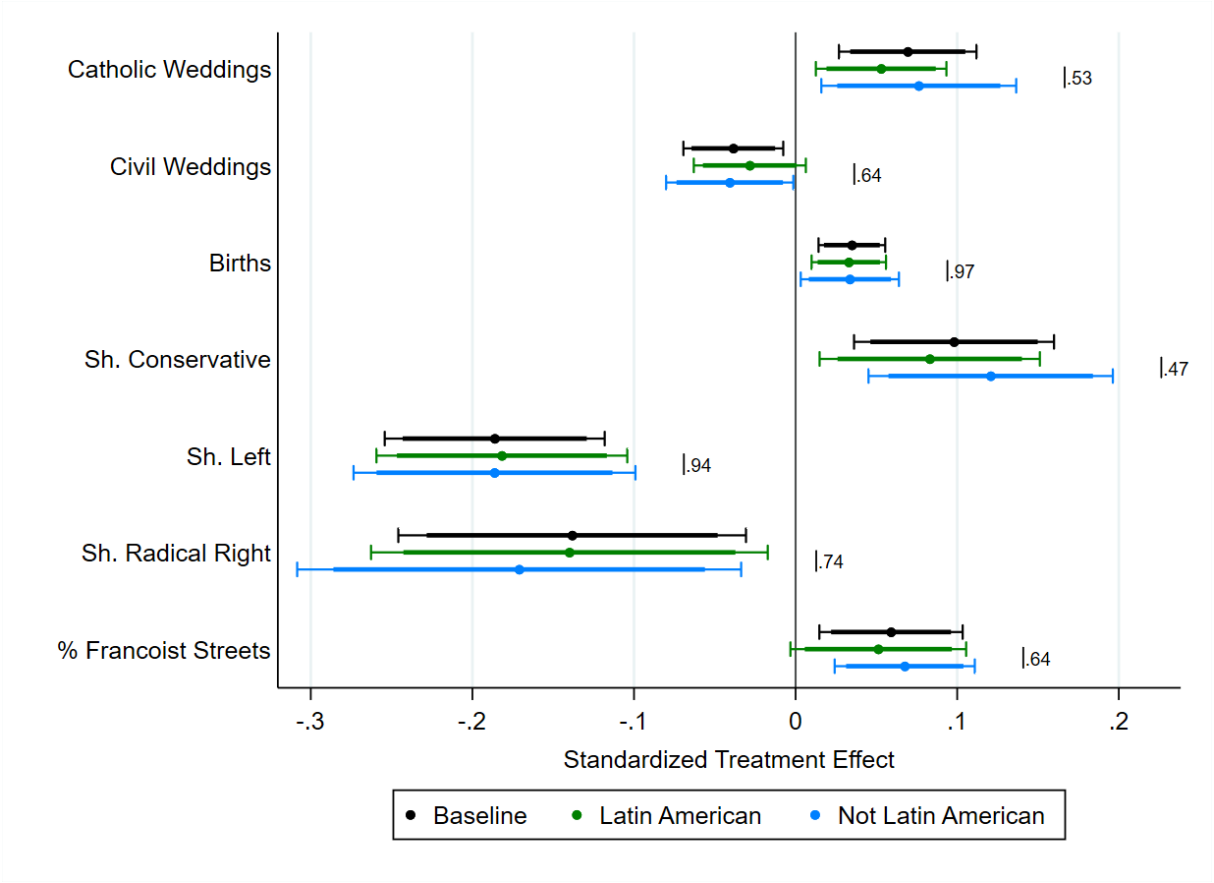
Note: This figure shows whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign priest. See Section 2.3 for further details. It differentiates between those municipalities with an above median unemployment rate, in green, and those with a below median unemployment rate, in blue. Municipalities are classified using data from the 2001 Spanish Census. All the results are reported in standardized units. p-values from Wald tests for the equality of two estimates are reported next to each solid vertical line between the two estimates. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A16 displays numerically this figure.

Figure 1.15: Effect of a foreign priest's arrival by Religious Leader's Country of Study



Note: This figure shows whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign priest. See Section 2.3 for further details. It differentiates between those priests who studied in a Spanish seminary, in green, and those who did not, in blue. All the results are reported in standardized units. *p*-values from Wald tests for the equality of two estimates are reported next to each solid vertical line between the two estimates. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A17 displays numerically this figure.

Figure 1.16: Effect of a foreign priest's arrival by Religious Leader's Country of Origin

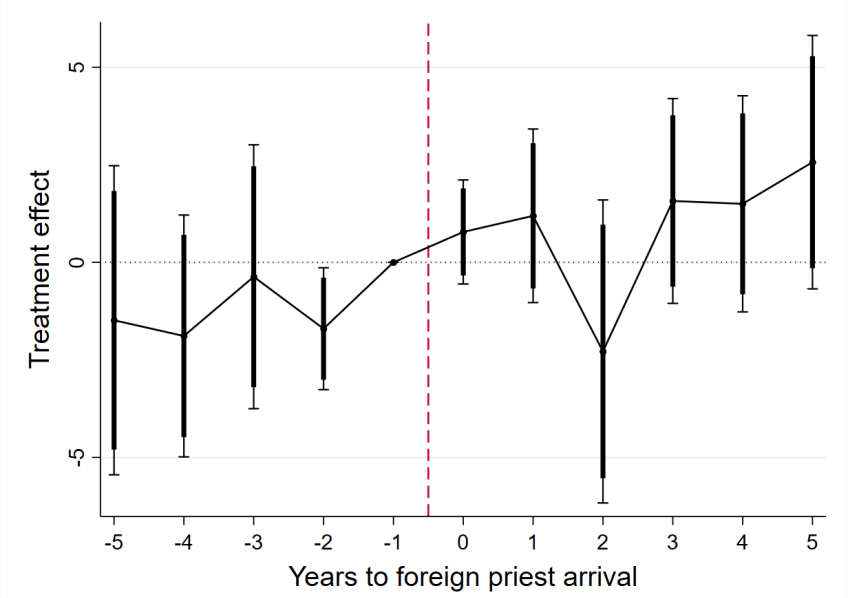


Note: This figure shows whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign priest. See Section 2.3 for further details. It differentiates foreign priests by those who were born in Latin America, in green, and those who were born anywhere else in the world, in blue. All the results are reported in standardized units. p-values from Wald tests for the equality of two estimates are reported next to each solid vertical line between the two estimates. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A18 displays numerically this figure.

Appendix

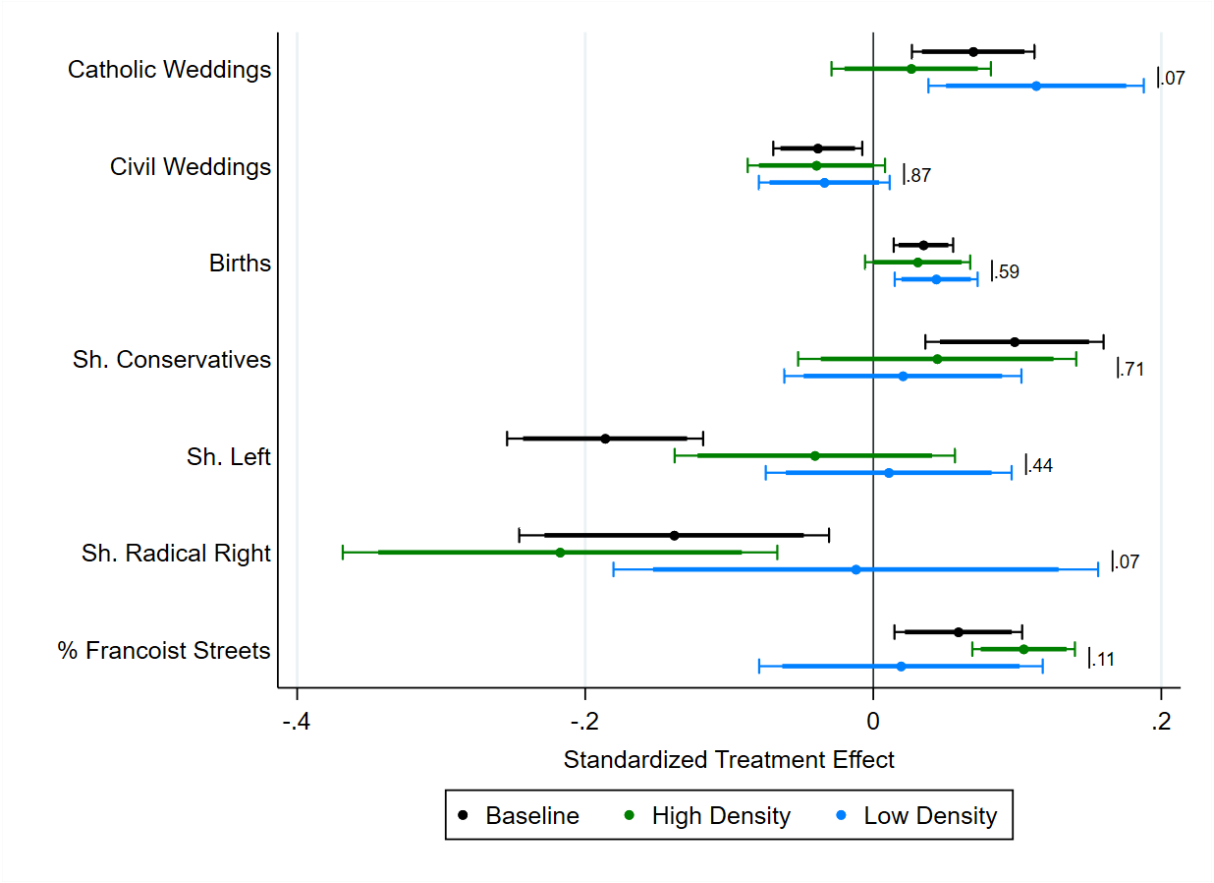
1.A Figures and Tables

Figure 1.A1: Effect of a foreign priest's arrival on migration



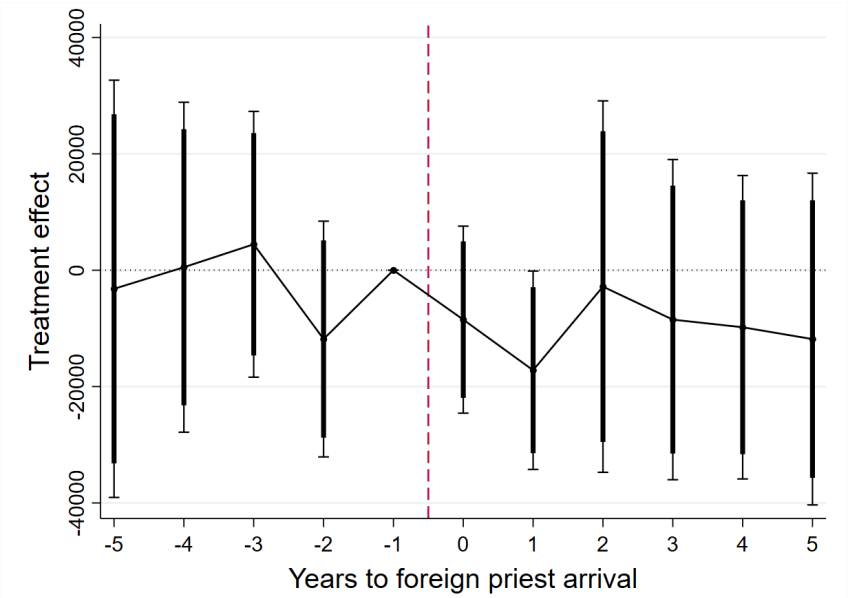
Note: This figure shows whether the arrival of a foreign priest to a municipality influences the arrival of new incoming population. The x-axis identifies the number of years since the arrival of a foreign priest. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A19 displays numerically this figure.

Figure 1.A2: Effect of a foreign priest's arrival by foreign-born priests density



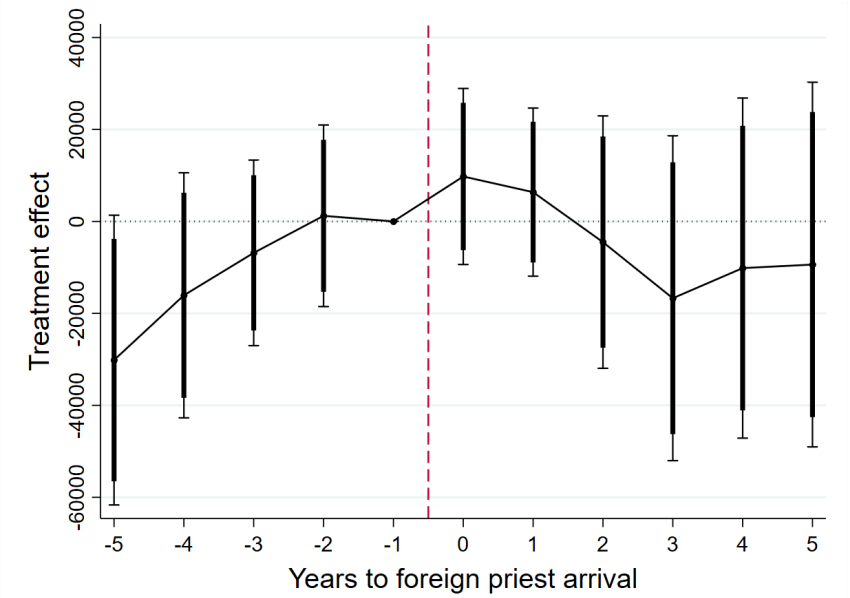
Note: This figure shows whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign-born priest. See Section 2.3 for further details. It differentiates between those dioceses that belong to the ecclesiastical province of Zaragoza (characterized by the extensive use of foreign-born priests), in green, and those located elsewhere, in blue. All the results are reported in standardized units. p-values from Wald tests for the equality of two estimates are reported next to each solid vertical line between the two estimates. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A20 displays numerically this figure.

Figure 1.A3: Effect of a foreign priest's arrival on social spending



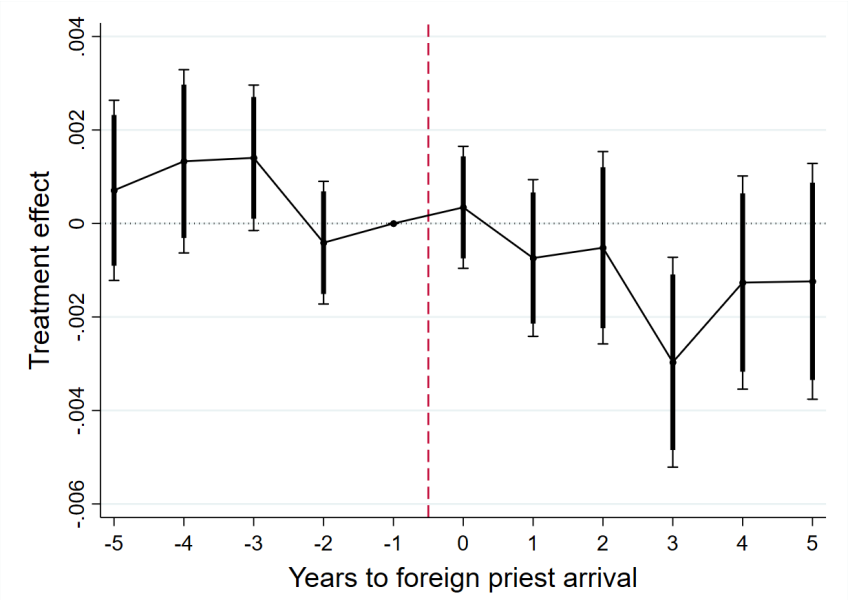
Note: This figure shows whether the arrival of a foreign priest to a municipality shapes preferences toward social projects. The x-axis identifies the number of years since the arrival of a foreign priest. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A21 displays numerically this figure.

Figure 1.A4: Effect of a foreign priest's arrival on business-related spending



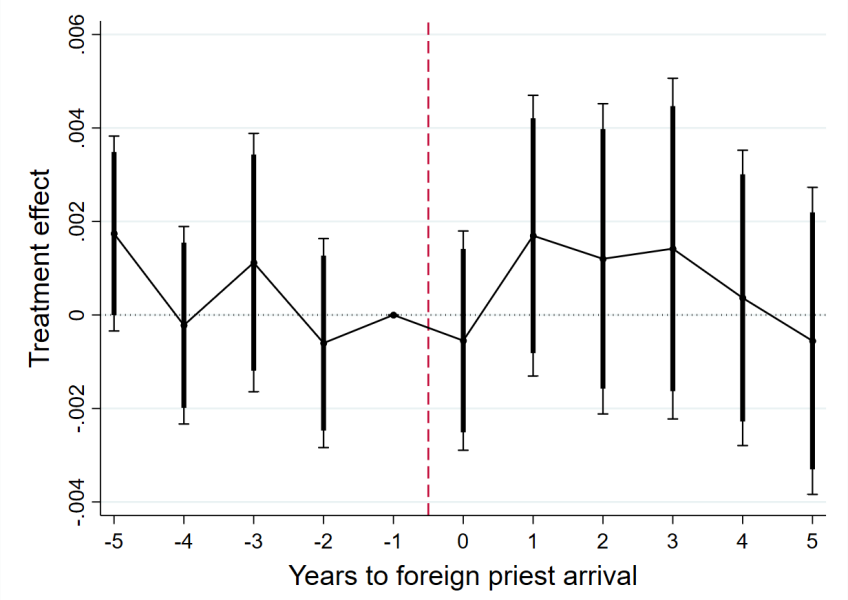
Note: This figure shows whether the arrival of a foreign priest to a municipality shapes preferences toward business-related projects. The x-axis identifies the number of years since the arrival of a foreign priest. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A21 displays numerically this figure.

Figure 1.A5: Effect of a foreign priest's arrival on unemployment rates



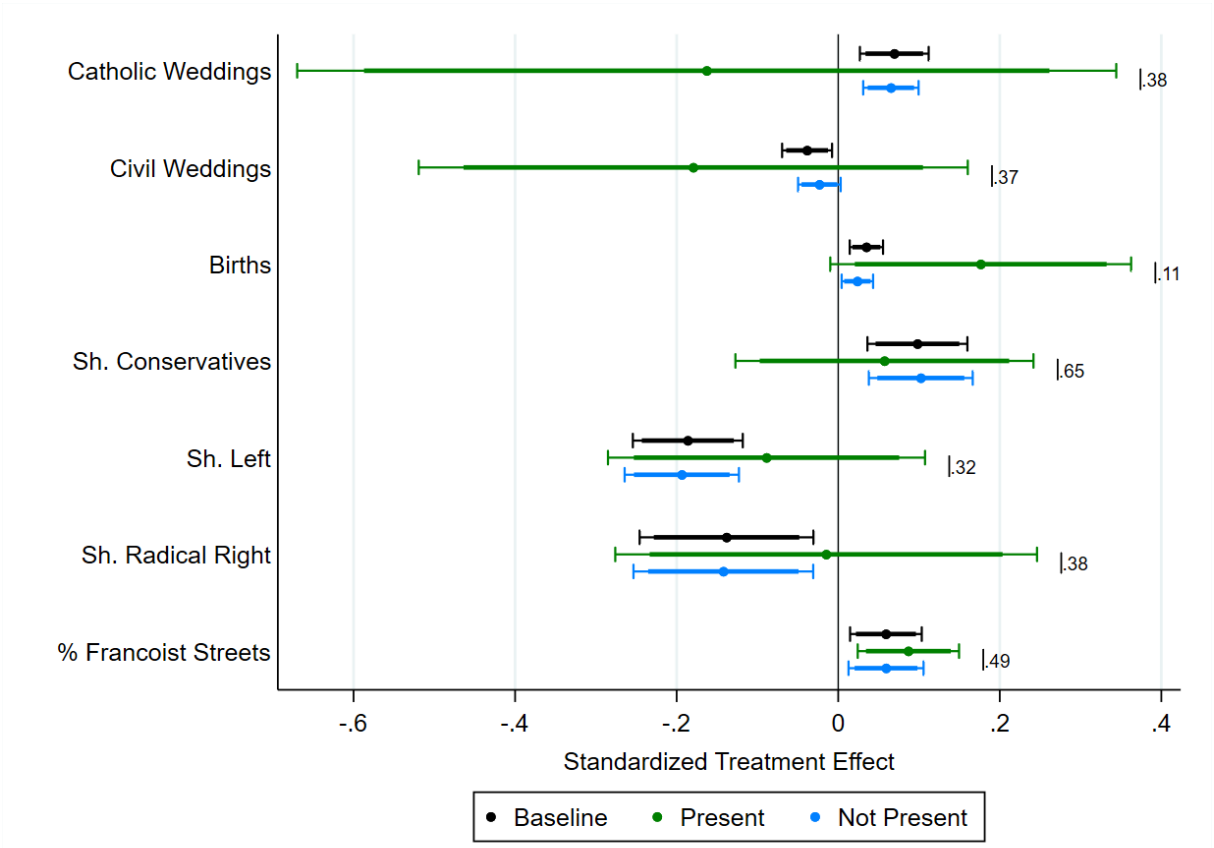
Note: This figure shows whether the arrival of a foreign priest to a municipality leads to changes in unemployment rates. The unemployment rates are measured per capita instead of per working age population due to missing data. The x-axis identifies the number of years since the arrival of a foreign priest. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A22 displays numerically this figure.

Figure 1.A6: Effect of a foreign priest's arrival on contracting rates



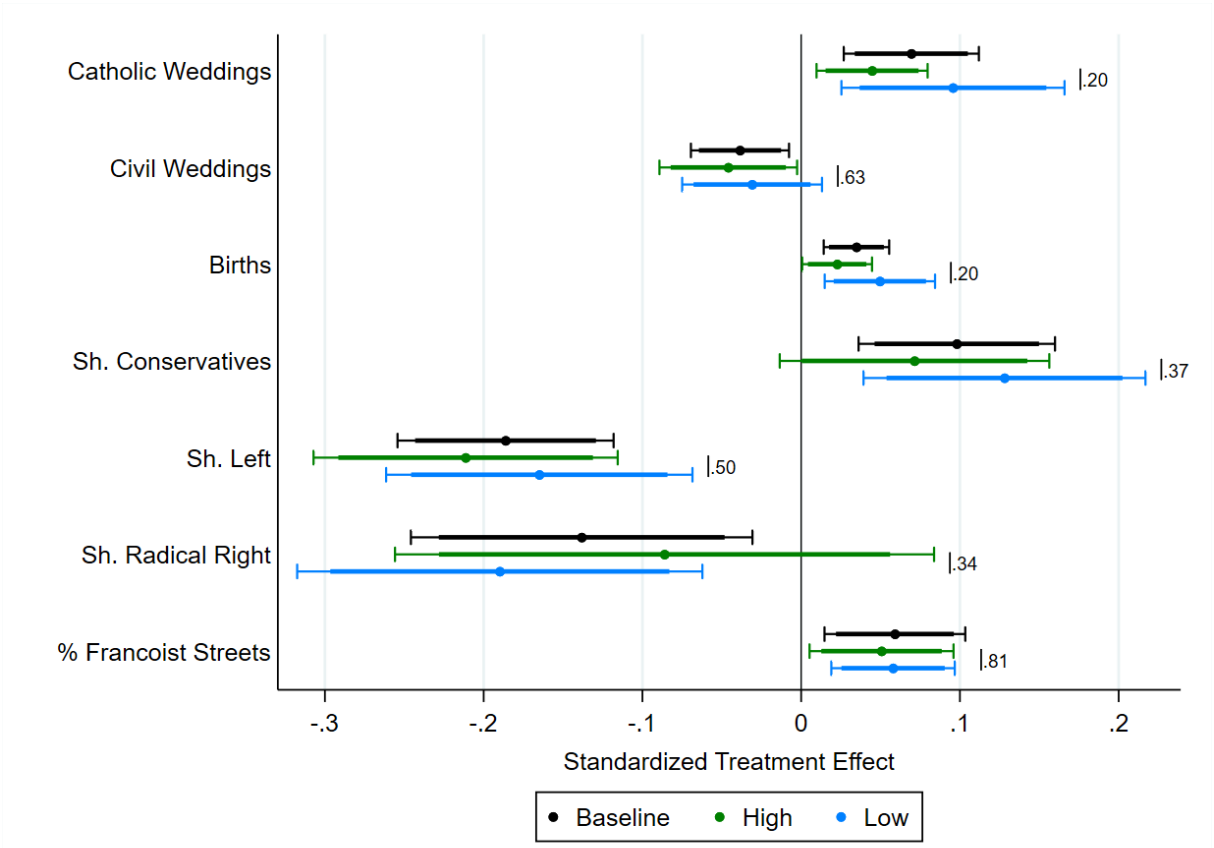
Note: This figure shows whether the arrival of a foreign priest to a municipality promotes the creation of new labor contracts. The contracting rates are measured per capita instead of per working age population due to missing data. The x-axis identifies the number of years since the arrival of a foreign priest. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A22 displays numerically this figure.

Figure 1.A7: Effect of a foreign priest's arrival by Local Catholic Movements



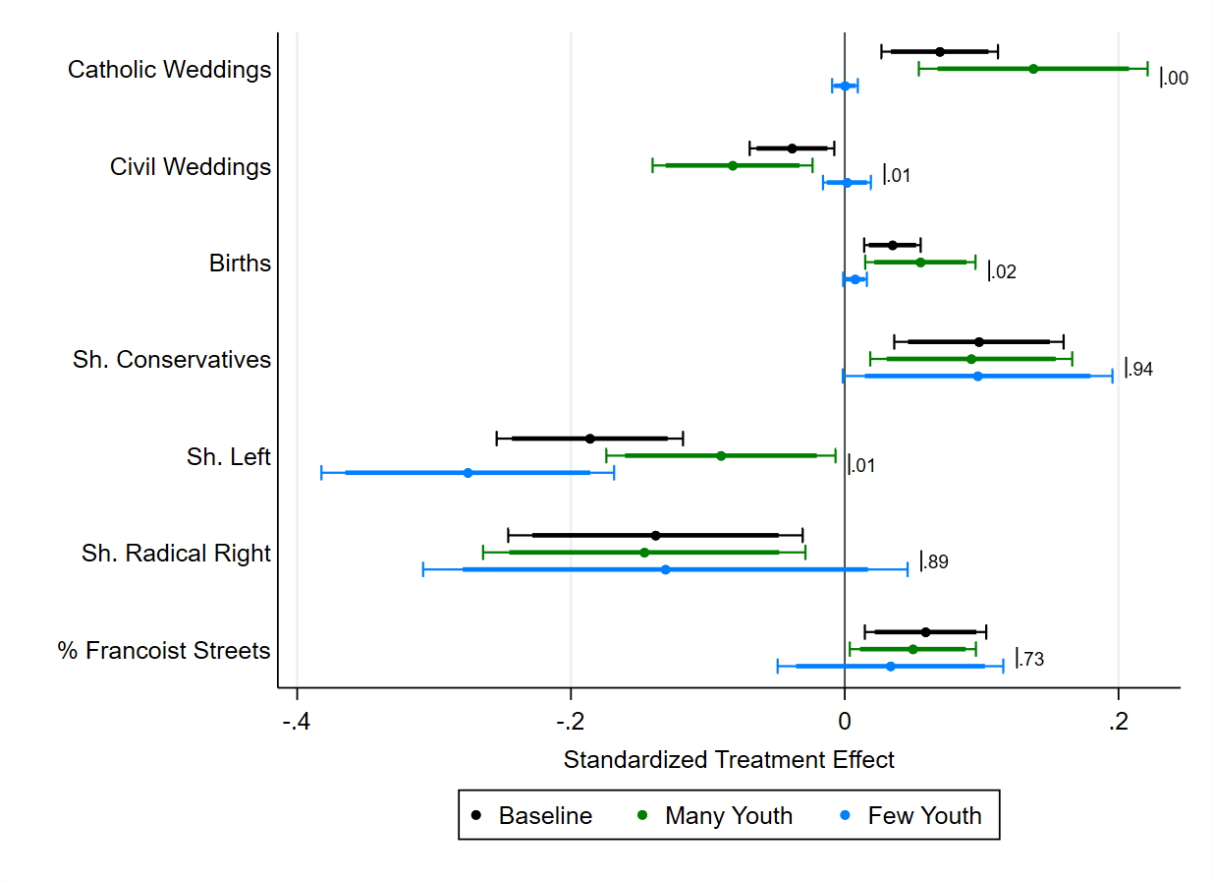
Note: This figure shows whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign priest. See Section 2.3 for further details. It differentiates between those municipalities who had registered as of the year 2000 at least a Catholic organization, in green, and those who did not have any organization registered, in blue. Municipalities are classified using data from Directory of Religious Entities, provided by the Spanish Ministry of Justice. All the results are reported in standardized units. p-values from Wald tests for the equality of two estimates are reported next to each solid vertical line between the two estimates. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A23 displays numerically this figure.

Figure 1.A8: Effect of a foreign priest's arrival by Local Uneducatedness



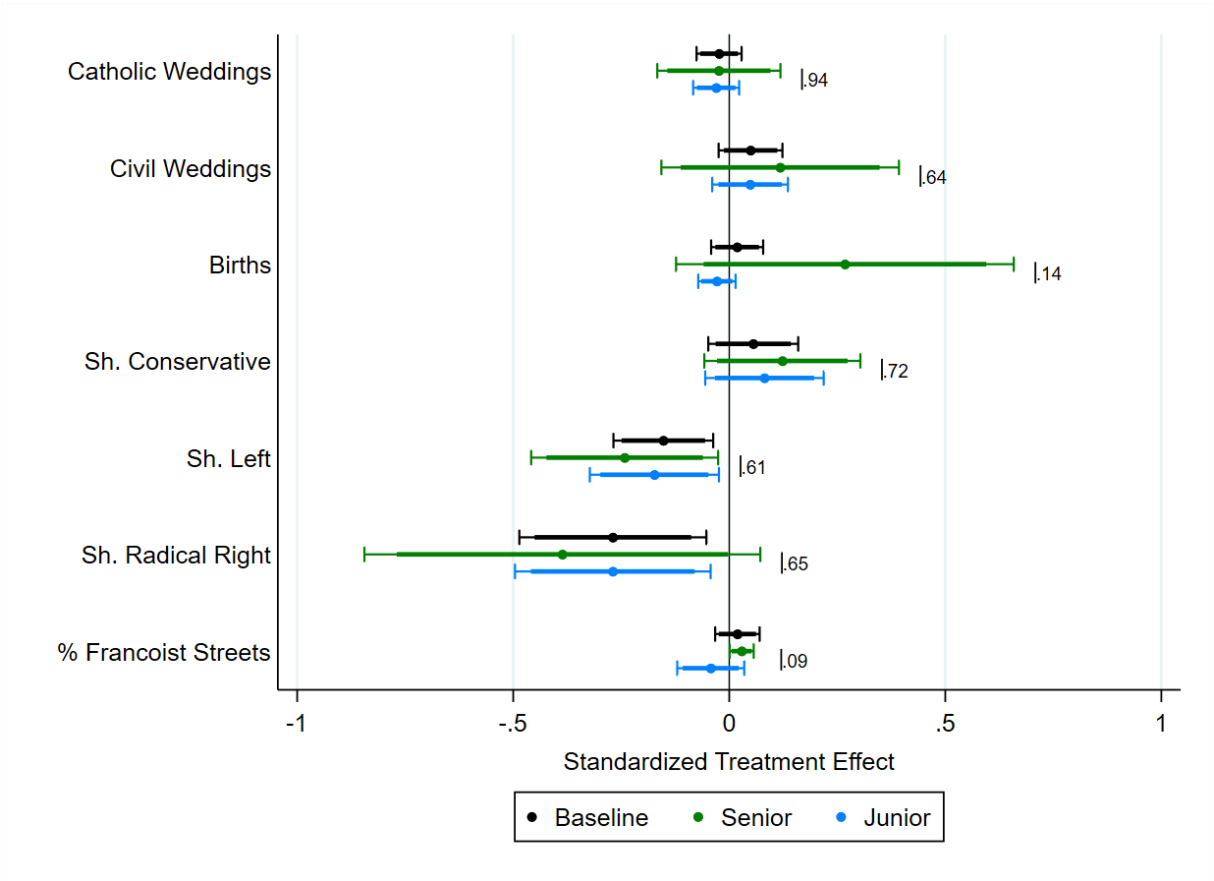
Note: This figure shows whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign priest. See Section 2.3 for further details. It differentiates between those municipalities with an above median number in individuals with no formal education, in green, and those with a below median number in individuals with no formal education, in blue. Municipalities are classified using data from the 2001 Spanish Census. All the results are reported in standardized units. p-values from Wald tests for the equality of two estimates are reported next to each solid vertical line between the two estimates. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A24 displays numerically this figure.

Figure 1.A9: Effect of a foreign priest's arrival by Local Demographic Structure



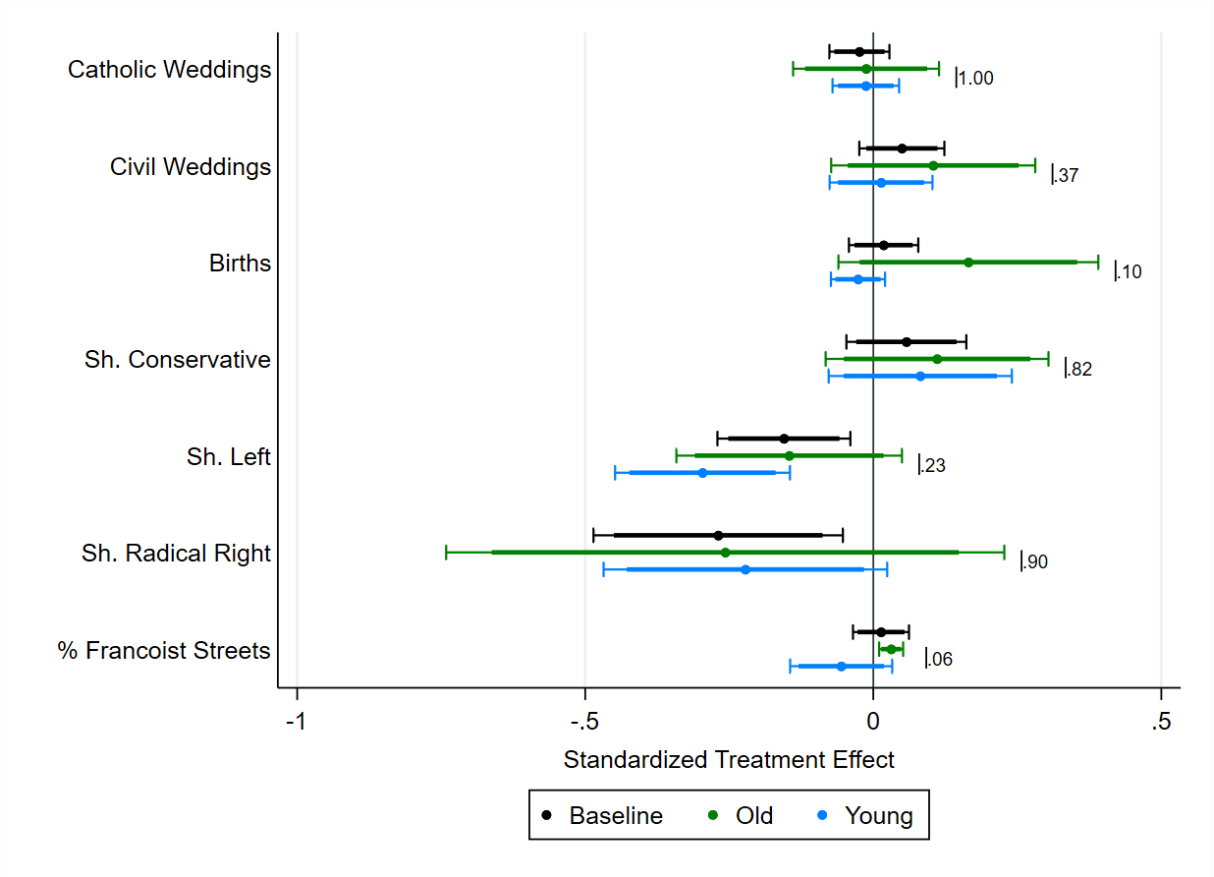
Note: This figure shows whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign priest. See Section 2.3 for further details. It differentiates between those municipalities with an above median number of young individuals, in green, and those with a below median number of young individuals, in blue. Municipalities are classified using data from the 2001 Spanish Census. All the results are reported in standardized units. p-values from Wald tests for the equality of two estimates are reported next to each solid vertical line between the two estimates. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A25 displays numerically this figure.

Figure 1.A10: Effect of a foreign priest's arrival by Religious Leader's Tenure



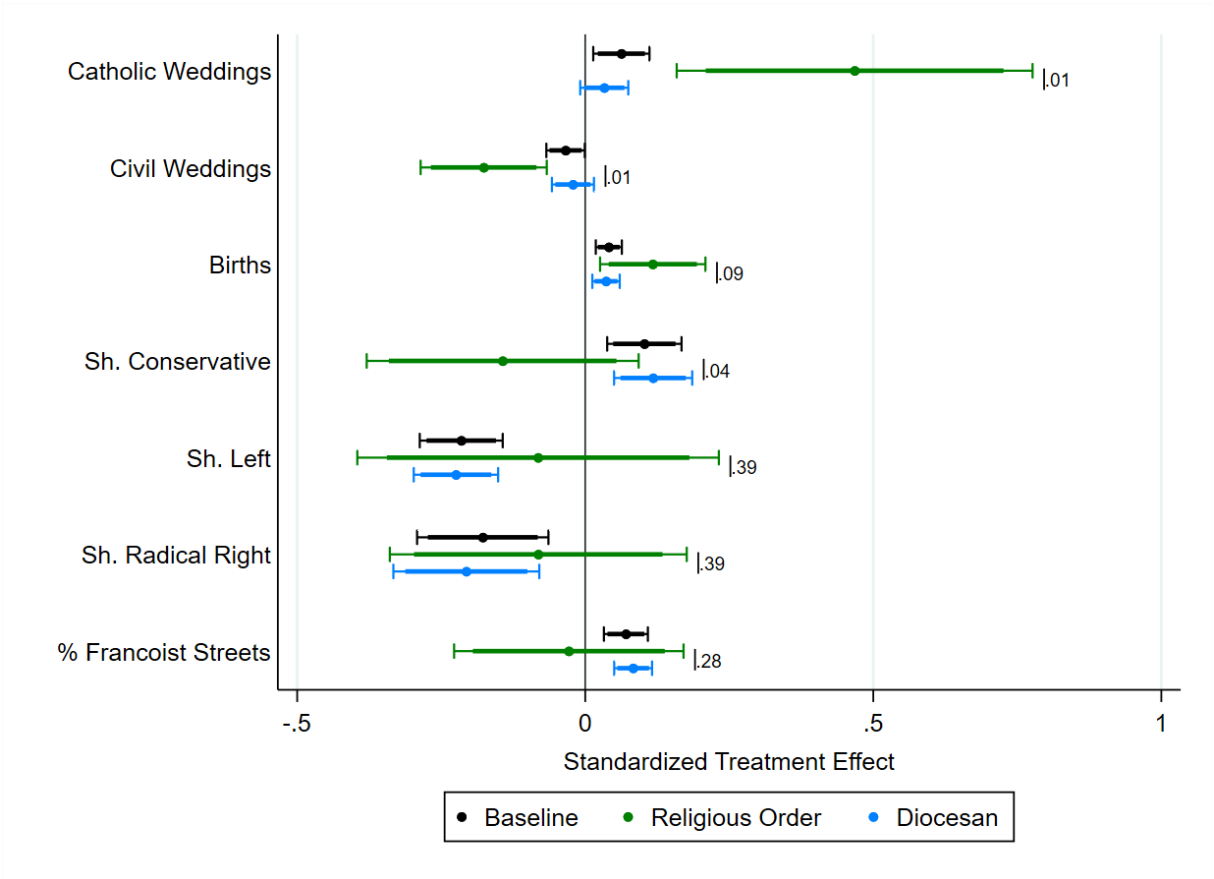
Note: This figure shows whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign priest. See Section 2.3 for further details. It differentiates between those priests whose tenure is above the median, in green, and those whose tenure is below the median, in blue. All the results are reported in standardized units. p-values from Wald tests for the equality of two estimates are reported next to each solid vertical line between the two estimates. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A26 displays numerically this figure.

Figure 1.A11: Effect of a foreign priest's arrival by Religious Leader's Age



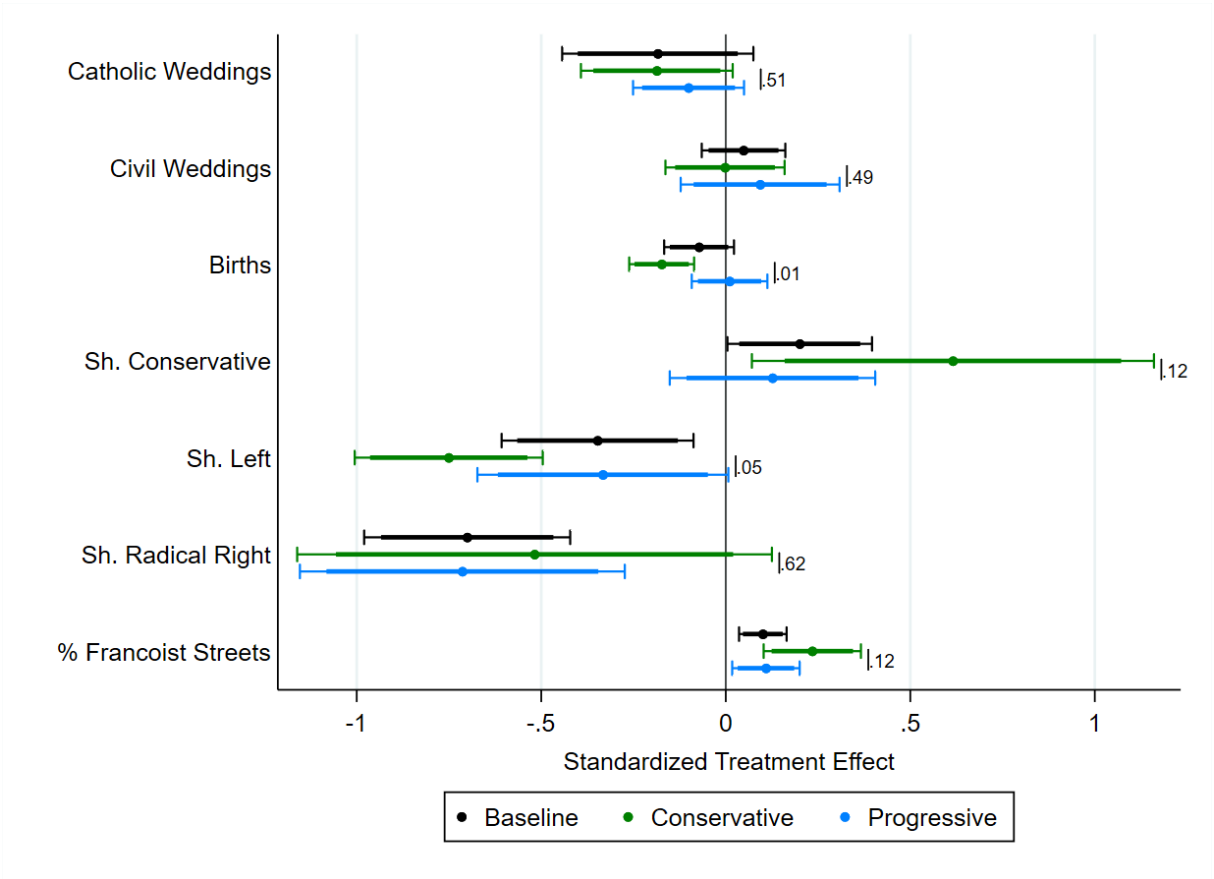
Note: This figure shows whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign priest. See Section 2.3 for further details. It differentiates between those priests whose age is above the median, in green, and those whose age is below the median, in blue. All the results are reported in standardized units. p-values from Wald tests for the equality of two estimates are reported next to each solid vertical line between the two estimates. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A27 displays numerically this figure.

Figure 1.A12: Effect of a foreign priest's arrival by Religious Leader's Order



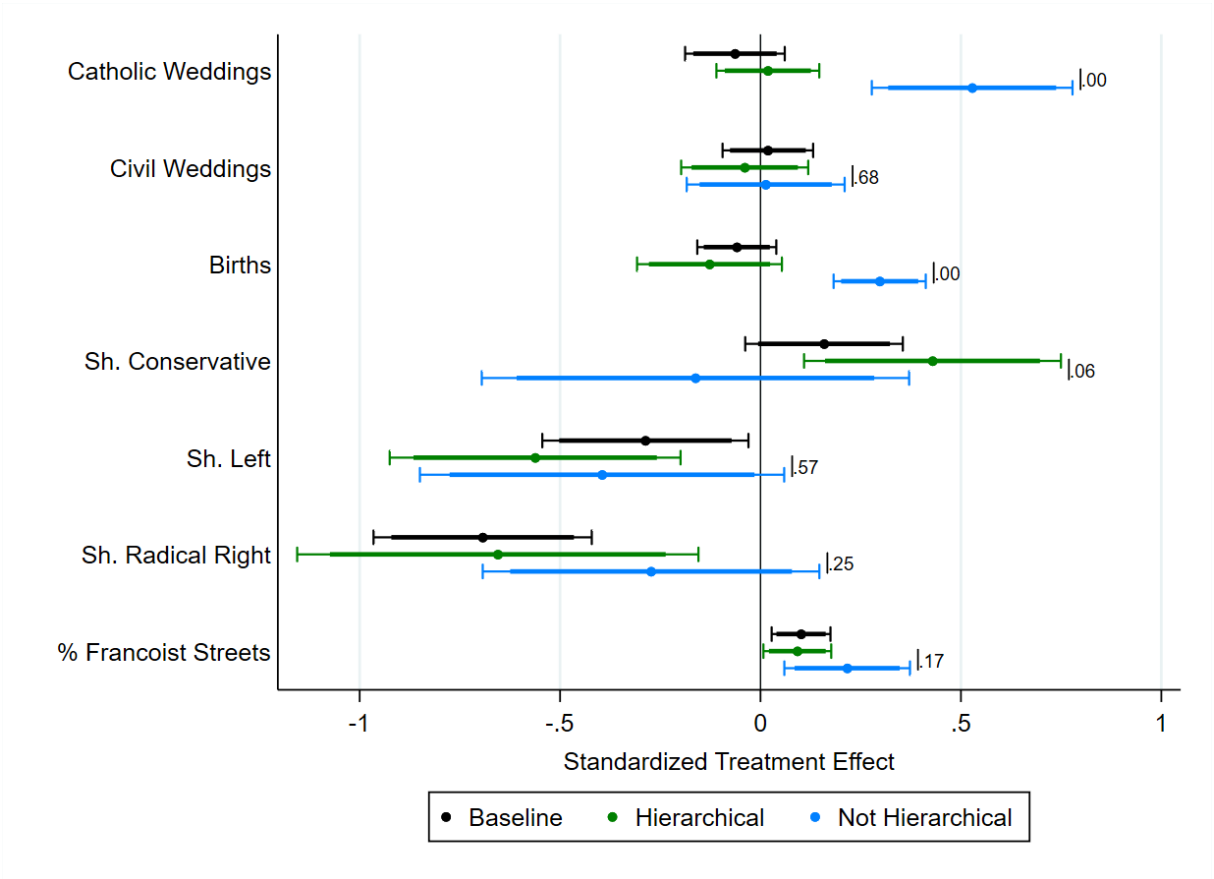
Note: This figure shows whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign priest. See Section 2.3 for further details. It differentiates between those priests who are part of a religious orders, in green, and those who are diocesan, in blue. All the results are reported in standardized units. p-values from Wald tests for the equality of two estimates are reported next to each solid vertical line between the two estimates. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A28 displays numerically this figure.

Figure 1.A13: Effect of a foreign priest's arrival by Religious Leader's Ideology



Note: This figure shows whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign priest. See Section 2.3 for further details. It differentiates between those priests holding more conservative views, in green, and those who do not, in blue. All the results are reported in standardized units. *p*-values from Wald tests for the equality of two estimates are reported next to each solid vertical line between the two estimates. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A29 displays numerically this figure.

Figure 1.A14: Effect of a foreign priest's arrival by Religious Leader's Hierarchy-Leaning



Note: This figure shows whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign priest. See Section 2.3 for further details. It differentiates between religious leaders that do not challenge the pope's rulings, in green, and those that challenge them, in blue. All the results are reported in standardized units. p-values from Wald tests for the equality of two estimates are reported next to each solid vertical line between the two estimates. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2. Table 1.A30 displays numerically this figure.

Table 1.A1: Distribution of municipalities and parishes per diocese

	Municipalities			**Parishes**		
Diocese	Used	Not Used	Total	Used	Not Used	Total
Albacete	61	25	86	61	115	176
Ávila	216	4	220	216	15	231
Barbastro-Monzón	91	10	101	91	34	125
Burgos	328	40	368	328	359	687
Calahorra y La Calzada - Logroño	166	8	174	166	49	215
Ciudad Real	89	13	102	89	48	137
Ciudad Rodrigo	79	3	82	79	11	90
Coria-Cáceres	102	6	108	102	25	127
Cuenca	223	14	237	223	55	278
Huesca	61	11	72	61	45	106
Jaca	54	6	60	54	20	74
Osma-Soria	172	8	180	172	33	205
Palencia	177	13	190	177	53	230
Plasencia	132	10	142	132	32	164
Salamanca	243	13	256	243	53	296
Segovia	206	3	209	206	21	227
Sigüenza-Guadalajara	281	6	287	281	31	312
Tarazona	119	2	121	119	10	129
Teruel y Albarracín	191	3	194	191	14	205
Toledo	210	11	221	210	47	257
Valencia	274	52	326	274	326	600
Valladolid	204	17	221	204	92	296
Zamora	163	6	169	163	33	196
Zaragoza	178	2	180	178	71	249
Total	4020	286	4306	4020	1592	5612

Note: The table displays in Columns 1-3 those municipalities used and not used in the analysis. Column 4-6 display the equivalence in terms of parishes used and not used. I use in the analysis all those municipalities with a single population center and parish (Columns 1 and 4). I do not use those municipalities with multiple population centers or multiple parishes.

Table 1.A2: Distribution of priest by country of birth and diocese

Diocese	Foreign Priests	Local Priests	Sh. Foreign	Total
Albacete	5	34	0.13	39
Ávila	2	74	0.03	76
Barbastro-Monzón	35	89	0.28	124
Burgos	6	177	0.03	183
Calahorra y La Calzada-Logroño	9	74	0.11	83
Ciudad Real	10	86	0.10	96
Ciudad Rodrigo	2	31	0.06	33
Coria-Cáceres	9	84	0.10	93
Cuenca	5	104	0.05	109
Huesca	12	40	0.23	52
Jaca	5	23	0.18	28
Osma-Soria	0	78	0	78
Palencia	1	92	0.01	93
Plasencia	34	71	0.32	105
Salamanca	3	73	0.04	76
Segovia	10	68	0.13	78
Sigüenza-Guadalajara	7	135	0.05	142
Tarazona	32	50	0.39	82
Teruel y Albarracín	32	65	0.33	97
Toledo	20	257	0.07	277
Valencia	16	264	0.06	280
Valladolid	4	109	0.04	113
Zamora	1	45	0.02	46
Zaragoza	86	118	0.42	204
Total	346	2241	0.13	2587

Note: The table displays in Column 1 the list of dioceses covered in the analysis. Column 2 displays the distribution of foreign-born priests per diocese. Column 3 shows the distribution of Spanish-born priest per diocese. Column 4 presents the share of priest in a given diocese that are foreign-born. Column 5 reports the total number of priests per diocese.

Table 1.A3: Sample Comparison: Birth Data vs. No Birth Data

	Birth Data		No Birth Data		
	Mean	SD	Mean	SD	p-value
Foreign	0.084	0.278	0.156	0.363	(0.000)
Order Member	0.047	0.212	0.124	0.330	(0.000)
Spanish Educated	0.948	0.222	0.861	0.346	(0.000)
In Solidum	0.256	0.387	0.199	0.362	(0.000)
Observations	808		1779		2587

Note: This table compares how the sample of priest for which birth data was found (Col. 1 and 2) compares with those for which no birth data was found (Col.3 and 4). The p-value of the difference between both groups is presented in Column 5. Foreign is a dummy variable identifying if the priest was not born in Spain. Order Member is a variable identifying whether a priest is part of a religious order or not. Spanish Educated is a dummy variable identifying whether a given priest studied in a Spanish seminary. In Solidum identifies the share of positions held by a given priest that are shared with other priests (in solidum).

Table 1.A4: Distribution of priest by country of birth and diocese - Survey Sample

Diocese	All Priests	Potential	Surveyed	Foreign	Local
Albacete	39	0	0	0	0
Ávila	76	47	10	0	10
Barbastro-Monzón	124	48	22	7	15
Burgos	183	38	5	0	5
Calahorra y La Calzada - Logroño	83	48	11	0	11
Ciudad Real	96	85	12	0	12
Ciudad Rodrigo	33	0	0	0	0
Coria-Cáceres	93	56	6	0	6
Cuenca	109	106	21	0	21
Huesca	52	28	11	2	9
Jaca	28	16	5	1	4
Osma-Soria	78	73	18	0	18
Palencia	93	0	0	0	0
Plasencia	105	48	3	0	3
Salamanca	76	57	18	0	18
Segovia	78	73	10	0	10
Sigüenza-Guadalajara	142	141	14	2	12
Tarazona	82	56	9	1	8
Teruel y Albarracín	97	41	14	4	10
Toledo	277	138	20	0	20
Valencia	280	0	0	0	0
Valladolid	113	51	14	0	14
Zamora	46	45	8	0	8
Zaragoza	204	93	26	10	16
Total	2587	1288	257	27	230

Note: The table displays in Column 1 the distribution of all priests per diocese. In Column 2, it shows the distribution of all priests per diocese for which the personal contact information was available. In Column 3, it shows the distribution per diocese of those priests that have been surveyed. In Column 4, it shows the distribution per diocese of foreign-born priests, and in Column 5, it shows the distribution of Spanish-born priests.

Table 1.A5: Sample Comparison: Survey vs. No Survey

	Survey Data		**No Survey Data**		
	Mean	SD	Mean	SD	p-value
Foreign	0.105	0.307	0.137	0.344	(0.120)
Order Member	0.047	0.211	0.106	0.308	(0.000)
Spanish Educated	0.949	0.220	0.884	0.320	(0.000)
In Solidum	0.305	0.402	0.207	0.366	(0.000)
Observations	257		2330		2587
Age	58.545	13.988	65.864	14.746	(0.000)
Tenure	31.358	15.269	38.802	16.232	(0.000)
Observations	257		550		807

Note: This table compares how the sample of priest for which survey data was collected (Col. 1 and 2) compares with those for which no survey data was collected (Col.3 and 4). The p-value of the difference between both groups is presented in Column 5. Foreign is a dummy variable identifying if the priest was not born in Spain. Order Member is a variable identifying whether a priest is part of a religious order or not. Spanish Educated is a dummy variable identifying whether a given priest studied in a Spanish seminary. In Solidum identifies the share of positions held by a given priest that are shared with other priests (in solidum). All other variables are self-explanatory.

Table 1.A6: Left-Right Political Spectrum

Panel A: Left-wing parties

AR, CEUS, CHA, EQUO, En Marea, ERC, ERPV, EUPV-EV, FRONT, IU, Los Verdes, Mas Pais, Més Compromís, PCPE, Podemos, PH, POSI, Primavera Europea, PR+, PSOE, PUM+J, Recortes Cero

Panel B: Right-wing parties

ADÑ, AES, AN, AUN, CDES, Cs, CVA, DN, España 2000, FA, FE, FEdelasJONS, FN, La Falange, MAS, MSR, PADE, PDN, PAR, PFyV, PLD, PP, PPSO, PRGU, UDCA, VOX

Note: The initials for the left-wing parties relate to the following parties. AR: Acción Republicana; CEUS: Coalición por una Europa Solidaria; CHA: Chunta Aragonesista; ERC: Esquerra Republicana de Catalunya; ERPV: Esquerra Republicana del País Valencià; EUPV-EV: Esquerra Unida del País Valencià; FRONT: Front per País Valencià; IU: Izquierda Unida<; PCPE: Partido Comunida del Pueblo Español; PH: Partido Humanista; POSI: Partido Obrero Socialista Internacionalista; PR+: Partido Riojano; PSOE: Partido Socialista Obrero Español; PUM+J: Por Un Mundo Más Justo. The initials for the right-wing parties relate to the following parties. ADÑ: ADÑ Identidad Española; AES: Alternativa Española; AN: Alianza Nacional; AUN: Alianza por la Unidad Nacional; CDES: Centro Democrático Español; Cs: Ciudadanos; CVA: Coalición Valenciana; DN: Democracia Nacional; FA: Falange Auténtica; FE: Frente Español; FEdelasJONS: Falange Española de las JONS; FN: Fuerza Nueva; MAS: Movimiento Social Aragonés; MSR: Moviment Social Republicà; PADE: Partido Demócrata Español; PDN: Partido Demócrata Nacional de España; PFyV: Familia y Vida; PLD: Partido Liberal de Derechas; PP: Partido Popular; PPSO: Plataforma del Pueblo Soriano; PRGU: Partido Regionalista de Guadalajara; UDCA: Unidad Castellana

Table 1.A7: Conservative - Far Right Political Spectrum

Panel A: Conservative Parties
CDES, Cs, PADE, PAR, PDN, PFyV, PLD, PP, PPSO, PRGU, UDCA
Panel B: Far-Right Parties
ADÑ, AES, AN, AUN, CVA, DN, España 2000, FA, FE, FEdelasJONS, FN, La Falange, MAS, MSR, VOX

Note: The initials for the conservative parties relate to the following parties. CDES: Centro Democrático Español; Cs: Ciudadanos; PADE: Partido Demócrata Español; PDN: Partido Demócrata Nacional de España; PFyV: Familia y Vida; PLD: Partido Liberal de Derechas; PP: Partido Popular; PPSO: Plataforma del Pueblo Soriano; PRGU: Partido Regionalista de Guadalajara; UDCA: Unidad Castellana. The initials for the far-right parties refer to the following parties. ADÑ: ADÑ Identidad Española; AES: Alternativa Española; AN: Alianza Nacional; AUN: Alianza por la Unidad Nacional; CVA: Coalición Valenciana; DN: Democracia Nacional; FA: Falange Auténtica; FE: Frente Español; FEdelasJONS: Falange Española de las JONS; FN: Fuerza Nueva; MAS: Movimiento Social Aragonés; MSR: Moviment Social Republicà

Table 1.A8: Francoist street names

Francoist keywords
18 de julio, 18 julio, alzamiento, caudillo, dieciocho de julio, dieciocho julio, division azul, emilio mola, francisco franco, general aranda, general franco, general mola, general moral, general moscardo, general sagardia, general sanjurjo, general valera, general varela, general yague, generalisimo, jose calvo sotelo, jose enrique varela, jose moscardo, jose sanjurjo, los caidos, millan astray, onesimo redondo, primo de rivera, queipo de llano, ramiro ledesma, ruiz de alda, salas pombo, xeneral aranda, xeneral franco, xeneral mola, xeneral moral, xeneral moscardo, xeneral sagardia, xeneral sanjurjo, xeneral valera, xeneral varela, xeneral yague, xeneralisimo

Note: This table presents the list of ASCII-style keywords used to identify Francoist street names, following Oto-Peralías (2018).

Table 1.A9: Influence of foreign priests' arrival on sociopolitical attitudes Before and After Pope Francis

	(1) Cath	(2) Civil	(3) Birth	(4) Right	(5) Left	(6) Cons	(7) Rad Right	(8) Francoist
Foreign	1.4758***	-0.3941***	1.0275***	-0.00120	0.00540	0.00130	-0.0026**	-0.00410
	(0.2588)	(0.1204)	(0.2583)	(0.0038)	(0.0037)	(0.0038)	(0.0012)	(0.0560)
Foreign x Francis	-1.4033***	0.3300**	-0.6234**	0.00290	-0.0120***	0.0070*	-0.0041***	0.2055***
	(0.3059)	(0.1427)	(0.2890)	(0.0037)	(0.0035)	(0.0037)	(0.0014)	(0.0381)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	78651	78651	78596	46454	46454	46454	46454	121059
Joint p-value	0.431	0.192	0.003	0.569	0.019	0.00380	0.000	0.000
Mean Dep. Var.	1.587	1.286	6.506	0.568	0.408	0.538	0.0298	0.868

Note: The table tests whether the arrival of a foreign-born priest to a municipality affects a series of outcomes, depending on whether Pope Francis is in power or not (from March 2013 onward). Cath and Civil identifies the number of catholic and civil-only weddings, respectively. Right, Left, Cons, and Rad. Right identify the voting share to Right, Left, Conservative, and Radical Right parties, respectively. Francoist measures the percentage of the street in a given municipality that honor Franco's dictatorship. Joint p-value tests whether the sum of Foreign and Foreign x Francis is jointly equal to zero. All regressions include municipality and month-year fixed effects, and control by population, squared population, number of previous priest changes, and whether the priest shares his office (i.e., in solidum). Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A10: Effect of a foreign priest's arrival on religious outcomes and fertility

	(1) Cath. W.	(2) Civil W.	(3) Total W.	(4) Other W.	(5) Births
Pre-treatment					
Years from treatment: -5	-0.0634	-0.0278	-0.0935	-0.00240	-0.0002
	(0.0754)	(0.0650)	(0.0925)	(0.0057)	(0.1977)
Years from treatment: -4	0.0162	0.0115	0.0230	-0.00480	-0.181
	(0.0631)	(0.0641)	(0.0891)	(0.0054)	(0.1491)
Years from treatment: -3	0.0114	-0.00240	0.00770	-0.00110	-0.0698
	(0.0476)	(0.0549)	(0.0766)	(0.0043)	(0.1394)
Years from treatment: -2	0.0406	-0.00860	0.0284	-0.00370	0.153
	(0.0555)	(0.0610)	(0.0821)	(0.0041)	(0.1376)
Post-treatment					
Years from treatment: 0	0.0412	-0.00680	0.0361	0.00160	0.0860
	(0.0494)	(0.0593)	(0.0864)	(0.0040)	(0.1500)
Years from treatment: 1	0.1076*	-0.0548	0.0513	-0.00140	0.3294***
	(0.0560)	(0.0579)	(0.0837)	(0.0040)	(0.1401)
Years from treatment: 2	0.1833***	-0.1084**	0.0723	-0.00240	0.4819***
	(0.0553)	(0.0485)	(0.0715)	(0.0038)	(0.1395)
Years from treatment: 3	0.2025***	-0.0425	0.1577**	-0.00220	0.3513***
	(0.0597)	(0.0617)	(0.0797)	(0.0038)	(0.1468)
Years from treatment: 4	0.2452***	-0.1049*	0.142	0.00200	0.6568***
	(0.0892)	(0.0581)	(0.0997)	(0.0046)	(0.1727)
Years from treatment: 5	0.3555***	-0.1700***	0.182	-0.00380	0.6553***
	(0.1111)	(0.0694)	(0.1274)	(0.0034)	(0.1981)
Pre-Trend Joint p-value	0.458	0.985	0.487	0.806	0.106
Pre-Trend Sum p-value	0.980	0.882	0.899	0.485	0.853
Mean Dep. Var.	1.612	1.328	2.946	0.007	6.640
Observations	71079	71079	71079	71079	71039

Note: The table tests whether the arrival of a foreign priest to a municipality affects its voting behavior. Column 1 uses as outcome the number of Catholic weddings carried out in a given municipality. Column 2 looks at the number of Civil-only weddings. Column 3 looks at the number of weddings following a non-Catholic denomination. Column 4 explores the total number of weddings performed. Column 5 focuses on the total number of birth in a given municipality. Pre-Trend Joint p-value tests whether all pre-treatment values are jointly equal to zero. Pre-Trend Sum p-value tests whether the sum of all pre-treatment values are different from zero. For further details on the data, see Section 2.3. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A11: Effect of a foreign priest's arrival on political outcomes

	(1) Right	(2) Left	(3) Cons	(4) Rad Right	(5) Abstention
Pre-treatment					
Elections from treatment: -5	0.00460	-0.00650	0.00270	0.0017***	-0.00440
	(0.0044)	(0.0041)	(0.0044)	(0.0006)	(0.0035)
Elections from treatment: -4	0.00310	-0.0060*	0.00140	0.0018***	-0.0077**
	(0.0038)	(0.0034)	(0.0038)	(0.0006)	(0.0035)
Elections from treatment: -3	0.000700	-0.00190	-0.00120	0.0020**	-0.00340
	(0.0035)	(0.0031)	(0.0035)	(0.001)	(0.0029)
Elections from treatment: -2	0.00460	-0.00330	0.00350	0.0011**	-0.0060**
	(0.0029)	(0.0026)	(0.0029)	(0.0005)	(0.0031)
Post-treatment					
Elections from treatment: 0	0.00350	-0.00330	0.0060*	-0.0024*	-0.0071***
	(0.0033)	(0.0029)	(0.0034)	(0.0014)	(0.003)
Elections from treatment: 1	0.0060*	-0.0059*	0.0070**	-0.000900	-0.0068***
	(0.0031)	(0.0033)	(0.0034)	(0.0013)	(0.0029)
Elections from treatment: 2	0.0068*	-0.0108***	0.0125***	-0.0057***	-0.0133***
	(0.0035)	(0.0031)	(0.0038)	(0.0020)	(0.0033)
Elections from treatment: 3	0.0099***	-0.0140***	0.0159***	-0.0059***	-0.0194***
	(0.0038)	(0.0038)	(0.0040)	(0.0018)	(0.0035)
Elections from treatment: 4	0.0108***	-0.0209***	0.0199***	-0.0092***	-0.0209***
	(0.0044)	(0.0043)	(0.0041)	(0.0024)	(0.0037)
Elections from treatment: 5	0.00680	-0.0274***	0.0153***	-0.0085***	-0.0273***
	(0.0049)	(0.0051)	(0.0049)	(0.0034)	(0.0040)
Pre-Trend Joint p-value	0.441	0.355	0.465	0.033	0.251
Pre-Trend Sum p-value	0.272	0.112	0.588	0.002	0.037
Mean Dep. Var.	0.569	0.407	0.539	0.0294	0.282
Observations	44568	44568	44568	44568	44568

Note: The table tests whether the arrival of a foreign priest to a municipality affects its voting behavior. Column 1 uses as outcome the share of votes to left-leaning parties and Column 2 the share of votes to right-leaning parties. Column 3 looks at the share of votes to conservative parties, and Column 4 focuses on the share of votes to radical right parties. Column 5 reports the abstention rates. Pre-Trend Joint p-value tests whether all pre-treatment values are jointly equal to zero. Pre-Trend Sum p-value tests whether the sum of all pre-treatment values are different from zero. For further details on the data, see Section 2.3. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A12: Effect of a foreign priest's arrival on Francoist street naming

	(1) % Francoist Streets
Pre-treatment	
Half-years from treatment: -6	-0.0177
	(0.0509)
Half-years from treatment: -5	-0.0190
	(0.0447)
Half-years from treatment: -4	-0.00600
	(0.0419)
Half-years from treatment: -3	0.00430
	(0.0388)
Half-years from treatment: -2	0.0294
	(0.0373)
Post-treatment	
Half-years from treatment: 0	0.0529***
	(0.0066)
Half-years from treatment: 1	0.0768***
	(0.0126)
Half-years from treatment: 2	0.0706***
	(0.0240)
Half-years from treatment: 3	0.0769**
	(0.0362)
Half-years from treatment: 4	0.0729*
	(0.0438)
Half-years from treatment: 5	0.0908*
	(0.0469)
Half-years from treatment: 6	0.1266***
	(0.0476)
Half-years from treatment: 7	0.1599***
	(0.0421)
Half-years from treatment: 8	0.2000***
	(0.0430)
Half-years from treatment: 9	0.2127***
	(0.0428)
Half-years from treatment: 10	0.1900***
	(0.0692)
Half-years from treatment: 11	0.1984***
	(0.0745)
Half-years from treatment: 12	0.2228***
	(0.0763)
Pre-Trend Joint p-value	0.0636
Pre-Trend Sum p-value	0.964
Mean Dep. Var.	0.891
Observations	109741

Note: The table tests whether the arrival of a foreign priest to a municipality has an effect on the number of streets in the municipality with a Francoist name. Pre-Trend Joint p-value tests whether all pre-treatment values are jointly equal to zero. Pre-Trend Sum p-value tests whether the sum of all pre-treatment values are different from zero. For further details on the data, see Section 2.3. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A13: Effect of a foreign priest's arrival by Local Immigration

	Standardized			Non-Standardized		
	(1) Baseline	(2) High	(3) Low	(4) Baseline	(5) High	(6) Low
Cath. Weddings	0.0693***	0.1115***	0.0152*	0.3619***	0.7792***	0.0255*
	(0.0217)	(0.0377)	(0.0092)	(0.1133)	(0.2630)	(0.0154)
Observations	74503	37290	37213	74503	37290	37213
Civil-Only Weddings	-0.0384***	-0.0533**	-0.0205*	-0.1717***	-0.3228**	-0.0204*
	(0.0157)	(0.0261)	(0.0125)	(0.0702)	(0.1581)	(0.0124)
Observations	74503	37290	37213	74503	37290	37213
Births	0.0348***	0.0566***	0.00380	0.6613***	1.4436***	0.0174
	(0.0104)	(0.0179)	(0.0068)	(0.1999)	(0.4580)	(0.0313)
Observations	74503	37290	37213	74503	37290	37213
Sh. Cons.	0.0979***	0.1247***	0.0527	0.0153***	0.0174***	0.00870
	(0.0315)	(0.0381)	(0.0518)	(0.0049)	(0.0052)	(0.0086)
Observations	44764	22400	22364	44764	22400	22364
Sh. Left	-0.1861***	-0.1097***	-0.2703***	-0.0274***	-0.0142***	-0.0427***
	(0.0346)	(0.0406)	(0.0584)	(0.0051)	(0.0052)	(0.0092)
Observations	44764	22400	22364	44764	22400	22364
Sh. Rad Right	-0.1383***	-0.108	-0.1791**	-0.0085***	-0.00660	-0.0109**
	(0.0549)	(0.0666)	(0.0887)	(0.0034)	(0.0041)	(0.0054)
Observations	44764	22400	22364	44764	22400	22364
% Francoist Streets	0.0590***	0.0627***	0.0657	0.2035***	0.1372***	0.284
	(0.0226)	(0.0107)	(0.0487)	(0.0780)	(0.0233)	(0.2107)
Observations	115501	57822	57679	115501	57822	57679

Note: The table tests whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign-born priest. It presents the effect six years after the arrival of a foreign-born priest. See Section 2.3 for further details. It differentiates between those municipalities with an above median number of foreign-born individuals, and those with a below median number of foreign-born individuals. Municipalities are classified using data from the 2001 Spanish Census. Columns 1-3 report the effects in Standard Deviations, and Columns 4-6 display them not standardized. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A14: Effect of a foreign priest's arrival by Local Immigration
Latin American vs. Maghrebi

	Standardized			Non-Standardized		
	(1) Baseline	(2) Latam	(3) Maghrebi	(4) Baseline	(5) Latam	(6) Maghrebi
Cath. Weddings	0.1115***	0.1536***	0.1039*	0.7792***	1.2137***	0.9674*
	(0.0377)	(0.0524)	(0.0621)	(0.2630)	(0.4144)	(0.5781)
Observations	37290	28379	17514	37290	28379	17514
Civil-Only Weddings	-0.0533**	-0.0922***	-0.1463***	-0.3228**	-0.6277***	-1.1829***
	(0.0261)	(0.0346)	(0.0526)	(0.1581)	(0.2361)	(0.4257)
Observations	37290	28379	17514	37290	28379	17514
Births	0.0566***	0.0733***	0.0679*	1.4436***	2.0871***	2.2815*
	(0.0179)	(0.0250)	(0.0390)	(0.4580)	(0.7124)	(1.3135)
Observations	37290	28379	17514	37290	28379	17514
Sh. Cons.	0.1247***	0.1928***	0.1796***	0.0174***	0.0244***	0.0218***
	(0.0381)	(0.0425)	(0.0548)	(0.0052)	(0.0054)	(0.0066)
Observations	22400	17166	10660	22400	17166	10660
Sh. Left	-0.1097***	-0.1500***	-0.1535***	-0.0142***	-0.0173***	-0.0170***
	(0.0406)	(0.0478)	(0.0577)	(0.0052)	(0.0055)	(0.0063)
Observations	22400	17166	10660	22400	17166	10660
Sh. Rad Right	-0.108	-0.1938***	-0.1632**	-0.00660	-0.0119***	-0.0103**
	(0.0666)	(0.0649)	(0.0769)	(0.0041)	(0.0040)	(0.0048)
Observations	22400	17166	10660	22400	17166	10660
% Francoist Streets	0.0504***	0.0566***	0.0738***	0.1102***	0.1058***	0.1222***
	(0.0110)	(0.0121)	(0.0156)	(0.0242)	(0.0227)	(0.0259)
Observations	57822	44138	27327	57822	44138	27327

Note: The table tests whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign-born priest. It presents the effect six years after the arrival of a foreign-born priest. See Section 2.3 for further details. It differentiates between those municipalities with an above median number of foreign-born individuals, baseline, with an above median number of Latin American individuals (Latam), and those with an above median number of Maghrebi individuals (Maghrebi). Municipalities are classified using data from the 2001 Spanish Census. Columns 1-3 report the effects in Standard Deviations, and Columns 4-6 display them not standardized. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A15: Effect of a foreign priest's arrival by Local Conservatism

	Standardized			Non-Standardized		
	(1) Baseline	(2) Conservative	(3) Liberal	(4) Baseline	(5) Conservative	(6) Liberal
Cath. Weddings	0.0693***	0.0179*	0.1335***	0.3619***	0.0452*	0.9056***
	(0.0217)	(0.0098)	(0.0384)	(0.1133)	(0.0247)	(0.2606)
Observations	74503	37285	37136	74503	37285	37136
Civil-Only Weddings	-0.0384***	-0.00490	-0.0816***	-0.1717***	-0.0114	-0.4715***
	(0.0157)	(0.0142)	(0.0259)	(0.0702)	(0.0326)	(0.1492)
Observations	74503	37285	37136	74503	37285	37136
Births	0.0348***	0.0203***	0.0562***	0.6613***	0.1888***	1.3811***
	(0.0104)	(0.0066)	(0.0183)	(0.1999)	(0.0617)	(0.4481)
Observations	74503	37285	37136	74503	37285	37136
Sh. Cons.	0.0979***	-0.0247	0.1847***	0.0153***	-0.00350	0.0219***
	(0.0315)	(0.0522)	(0.0348)	(0.0049)	(0.0077)	(0.0041)
Observations	44764	22352	22355	44764	22352	22355
Sh. Left	-0.1861***	-0.0910*	-0.2518***	-0.0274***	-0.0119*	-0.0280***
	(0.0346)	(0.0520)	(0.0450)	(0.0051)	(0.0068)	(0.0049)
Observations	44764	22352	22355	44764	22352	22355
Sh. Rad Right	-0.1383***	-0.118	-0.1331**	-0.0085***	-0.00770	-0.0076**
	(0.0549)	(0.0966)	(0.0592)	(0.0034)	(0.0063)	(0.0034)
Observations	44764	22352	22355	44764	22352	22355
% Francoist Streets	0.0581***	0.1334***	-0.0228	0.2002***	0.5491***	-0.0590
	(0.0218)	(0.0197)	(0.0357)	(0.0751)	(0.0815)	(0.0926)
Observations	115501	57746	57601	115501	57746	57601

Note: The table tests whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign-born priest. It presents the effect six years after the arrival of a foreign-born priest. See Section 2.3 for further details. It differentiates between those municipalities with a historically conservative voting behavior (i.e., above median vote share for right-wing during the period 1975-1999), and those with a historically liberal voting behavior (i.e., below median vote share for right-wing during the period 1975-1999). Municipalities are classified using data on all national and European elections that took place between 1975 and 1999, available at the Spanish Interior Ministry. Columns 1-3 report the effects in Standard Deviations, and Columns 4-6 display them not standardized. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A16: Effect of a foreign priest's arrival by Local Unemployment

	Standardized			Non-Standardized		
	(1) Baseline	(2) High	(3) Low	(4) Baseline	(5) High	(6) Low
Cath. Weddings	0.0693***	0.1111***	0.0370*	0.3619***	0.6226***	0.1776*
	(0.0217)	(0.0399)	(0.0208)	(0.1133)	(0.2243)	(0.0996)
Observations	74503	37202	37301	74503	37202	37301
Civil-Only Weddings	-0.0384***	-0.0469*	-0.0295	-0.1717***	-0.2358*	-0.112
	(0.0157)	(0.0261)	(0.0192)	(0.0702)	(0.1310)	(0.0728)
Observations	74503	37202	37301	74503	37202	37301
Births	0.0348***	0.0627***	0.0136	0.6613***	1.2941***	0.234
	(0.0104)	(0.0153)	(0.0142)	(0.1999)	(0.3167)	(0.2462)
Observations	74503	37202	37301	74503	37202	37301
Sh. Cons.	0.0979***	0.0810*	0.1102***	0.0153***	0.0121*	0.0176***
	(0.0315)	(0.0421)	(0.0454)	(0.0049)	(0.0063)	(0.0073)
Observations	44764	22340	22424	44764	22340	22424
Sh. Left	-0.1861***	-0.1159***	-0.2421***	-0.0274***	-0.0164***	-0.0362***
	(0.0346)	(0.0489)	(0.0480)	(0.0051)	(0.0070)	(0.0071)
Observations	44764	22340	22424	44764	22340	22424
Sh. Rad Right	-0.1383***	-0.1569**	-0.128	-0.0085***	-0.0094**	-0.00800
	(0.0549)	(0.0724)	(0.0790)	(0.0034)	(0.0044)	(0.0049)
Observations	44764	22340	22424	44764	22340	22424
% Francoist Streets	0.0590***	0.0724***	0.0691***	0.2035***	0.2770***	0.2081***
	(0.0226)	(0.0236)	(0.0164)	(0.0780)	(0.0903)	(0.0493)
Observations	115501	57765	57736	115501	57765	57736

Note: The table tests whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign-born priest. It presents the effect six years after the arrival of a foreign-born priest. See Section 2.3 for further details. It differentiates between municipalities with an above median unemployment rate, and those with a below median unemployment rate. Municipalities are classified using data from the 2001 Spanish Census. Columns 1-3 report the effects in Standard Deviations, and Columns 4-6 display them not standardized. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A17: Effect of a foreign priest's arrival by Religious Leader's Country of Study

	Standardized			Non-Standardized		
	(1) Baseline	(2) Spanish E.	(3) No Spanish E.	(4) Baseline	(5) Spanish E.	(6) No Spanish E.
Cath. Weddings	0.0769***	0.0744**	0.0710***	0.4018***	0.4061**	0.3856***
	(0.0227)	(0.0364)	(0.0197)	(0.1185)	(0.1994)	(0.1071)
Observations	74503	63838	64971	74503	63838	64971
Civil-Only Weddings	-0.0406***	-0.0115	-0.0476***	-0.1808***	-0.0540	-0.2199***
	(0.0164)	(0.0241)	(0.0164)	(0.0731)	(0.1123)	(0.0762)
Observations	74503	63838	64971	74503	63838	64971
Births	0.0430***	0.0419***	0.0419***	0.8158***	0.8361***	0.8253***
	(0.0109)	(0.0176)	(0.0116)	(0.2091)	(0.3517)	(0.2308)
Observations	74503	63838	64971	74503	63838	64971
Sh. Cons.	0.1315***	0.1494***	0.1246***	0.0206***	0.0232***	0.0194***
	(0.0326)	(0.0421)	(0.0350)	(0.0051)	(0.0066)	(0.0055)
Observations	44764	37946	38704	44764	37946	38704
Sh. Left	-0.2054***	-0.2145***	-0.2006***	-0.0302***	-0.0313***	-0.0293***
	(0.0359)	(0.0489)	(0.0397)	(0.0052)	(0.0071)	(0.0057)
Observations	44764	37946	38704	44764	37946	38704
Sh. Rad Right	-0.1825***	-0.1752**	-0.2245***	-0.0111***	-0.0109**	-0.0138***
	(0.0568)	(0.0843)	(0.0582)	(0.0035)	(0.0052)	(0.0035)
Observations	44764	37946	38704	44764	37946	38704
% Francoist Streets	0.0276	0.0324	0.0218	0.0952	0.109	0.0727
	(0.0233)	(0.0274)	(0.0260)	(0.0807)	(0.0923)	(0.0868)
Observations	115501	97681	100164	115501	97681	100164

Note: The table tests whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign-born priest. It presents the effect six years after the arrival of a foreign-born priest. See Section 2.3 for further details. It differentiates between those priests who studies in a Spanish seminary, and those who did not. Columns 1-3 report the effects in Standard Deviations, and Columns 4-6 display them not standardized. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A18: Effect of a foreign priest's arrival by Religious Leader's Country of Origin

	Standardized			Non-Standardized		
	(1) Baseline	(2) Latam	(3) Non Latam	(4) Baseline	(5) Latam	(6) Non Latam
Cath. Weddings	0.0693***	0.0529***	0.0762***	0.3619***	0.2773***	0.4018***
	(0.0217)	(0.0206)	(0.0307)	(0.1133)	(0.1080)	(0.1622)
Observations	74503	73601	72367	74503	73601	72367
Civil-Only Weddings	-0.0384***	-0.0283	-0.0408**	-0.1717***	-0.127	-0.1824**
	(0.0157)	(0.0176)	(0.0200)	(0.0702)	(0.0790)	(0.0898)
Observations	74503	73601	72367	74503	73601	72367
Births	0.0348***	0.0329***	0.0335**	0.6613***	0.6270***	0.6413**
	(0.0104)	(0.0116)	(0.0154)	(0.1999)	(0.2239)	(0.2960)
Observations	74503	73601	72367	74503	73601	72367
Sh. Cons.	0.0979***	0.0829***	0.1207***	0.0153***	0.0130***	0.0187***
	(0.0315)	(0.0348)	(0.0386)	(0.0049)	(0.0054)	(0.0060)
Observations	44764	43928	43024	44764	43928	43024
Sh. Left	-0.1861***	-0.1817***	-0.1862***	-0.0274***	-0.0266***	-0.0274***
	(0.0346)	(0.0395)	(0.0445)	(0.0051)	(0.0057)	(0.0065)
Observations	44764	43928	43024	44764	43928	43024
Sh. Rad Right	-0.1383***	-0.1400**	-0.1711***	-0.0085***	-0.0086**	-0.0104***
	(0.0549)	(0.0626)	(0.0701)	(0.0034)	(0.0038)	(0.0043)
Observations	44764	43928	43024	44764	43928	43024
% Francoist Streets	0.0590***	0.0511*	0.0675***	0.2035***	0.1761*	0.2360***
	(0.0226)	(0.0276)	(0.0220)	(0.0780)	(0.0954)	(0.0772)
Observations	115501	113477	110962	115501	113477	110962

Note: The table tests whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign-born priest. It presents the effect six years after the arrival of a foreign-born priest. See Section 2.3 for further details. It differentiates foreign priests by those who were born in Latin America, and those were born anywhere else in the world. Columns 1-3 report the effects in Standard Deviations, and Columns 4-6 display them not standardized. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A19: Effect of a foreign priest's arrival on migration

	(1) All Inflow Migrants
Pre-treatment	
Years from treatment: -5	-1.484 (2.0204)
Years from treatment: -4	-1.887 (1.5808)
Years from treatment: -3	-0.369 (1.7251)
Years from treatment: -2	-1.6996** (0.7968)
Post-treatment	
Years from treatment: 0	0.779 (0.6802)
Years from treatment: 1	1.192 (1.1347)
Years from treatment: 2	-2.283 (1.9808)
Years from treatment: 3	1.573 (1.3387)
Years from treatment: 4	1.500 (1.4128)
Years from treatment: 5	2.567 (1.6565)
Pre-Trend Joint p-value	0.071
Pre-Trend Sum p-value	0.306
Mean Dep. Var.	44.03
Observations	63379

Note: The table tests whether the arrival of a foreign priest to a municipality influences the arrival of new settlers. Pre-Trend Joint p-value tests whether all pre-treatment values are jointly equal to zero. Pre-Trend Sum p-value tests whether the sum of all pre-treatment values are different from zero. For further details on the data, see Section 2.3. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A20: Effect of a foreign priest's arrival by foreign-born priests density

	Standardized			Non-Standardized		
	(1) Baseline	(2) High Density	(3) Low Density	(4) Baseline	(5) High Density	(6) Low Density
Cath. Weddings	0.0693***	0.0263	0.1130***	0.3619***	0.133	0.5925***
	(0.0217)	(0.0282)	(0.0381)	(0.1133)	(0.1424)	(0.2000)
Observations	74503	10633	63870	74503	10633	63870
Civil-Only Weddings	-0.0384***	-0.0395	-0.0340	-0.1717***	-0.165	-0.153
	(0.0157)	(0.0242)	(0.0231)	(0.0702)	(0.1017)	(0.1041)
Observations	74503	10633	63870	74503	10633	63870
Births	0.0348***	0.0307*	0.0436***	0.6613***	0.6079*	0.8231***
	(0.0104)	(0.0186)	(0.0147)	(0.1999)	(0.3675)	(0.2770)
Observations	74503	10633	63870	74503	10633	63870
Sh. Cons.	0.0979***	0.0443	0.0206	0.0153***	0.00680	0.00310
	(0.0315)	(0.0493)	(0.0419)	(0.0049)	(0.0076)	(0.0065)
Observations	44764	6831	37933	44764	6831	37933
Sh. Left	-0.1861***	-0.0406	0.0107	-0.0274***	-0.00590	0.00160
	(0.0346)	(0.0496)	(0.0436)	(0.0051)	(0.0073)	(0.0063)
Observations	44764	6831	37933	44764	6831	37933
Sh. Rad Right	-0.1383***	-0.2175***	-0.0120	-0.0085***	-0.0131***	-0.000700
	(0.0549)	(0.0769)	(0.0857)	(0.0034)	(0.0046)	(0.0052)
Observations	44764	6831	37933	44764	6831	37933
% Francoist Streets	0.0590***	0.1044***	0.0192	0.2035***	0.1905***	0.0700
	(0.0226)	(0.0182)	(0.0502)	(0.0780)	(0.0331)	(0.1831)
Observations	115501	16955	98546	115501	16955	98546

Note: The table tests whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign-born priest. It presents the effect six years after the arrival of a foreign-born priest. See Section 2.3 for further details. It differentiates between those dioceses that belong to the ecclesiastical province of Zaragoza (characterized by a high density of extensive use of foreign-born priests), so-called High Density, and those located elsewhere, so-called Low Density. Columns 1-3 report the effects in Standard Deviations, and Columns 4-6 display them not standardized. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A21: Effect of a foreign priest's arrival on local budget allocations

	(1) Social Budget	(2) Business Budget
Pre-treatment		
Years from treatment: -5	-3196	-30157*
	(18294)	(16074)
Years from treatment: -4	514	-16071
	(14468)	(13598)
Years from treatment: -3	4457	-6842
	(11652)	(10292)
Years from treatment: -2	-11825	1219
	(10337)	(10076)
Post-treatment		
Years from treatment: 0	-8493	9768
	(8200)	(9768)
Years from treatment: 1	-17187**	6368
	(8698)	(9327)
Years from treatment: 2	-2814	-4500
	(16284)	(14005)
Years from treatment: 3	-8491	-16704
	(14039)	(18020)
Years from treatment: 4	-9802	-10149
	(13303)	(18867)
Years from treatment: 5	-11834	-9387
	(14544)	(20229)
Pre-Trend Joint p-value	0.554	0.403
Pre-Trend Sum p-value	0.823	0.209
Mean Dep. Var.	248924	90831
Observations	43008	43008

Note: The table tests whether the arrival of a foreign priest to a municipality affects how local budget is spent, differentiating between social (Column 1) and business-related item (Column 2). Pre-Trend Joint p-value tests whether all pre-treatment values are jointly equal to zero. Pre-Trend Sum p-value tests whether the sum of all pre-treatment values are different from zero. For further details on the data, see Section 2.3. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A22: Effect of a foreign priest's arrival on local economic conditions

	(1) Unemployment	(2) Contracts
Pre-treatment		
Years from treatment: -5	0.0007	0.0017
	(0.001)	(0.0011)
Years from treatment: -4	0.0013	-0.0002
	(0.001)	(0.0011)
Years from treatment: -3	0.0014*	0.0011
	(0.0008)	(0.0014)
Years from treatment: -2	-0.0004	-0.0006
	(0.0007)	(0.0011)
Post-treatment		
Years from treatment: 0	0.0003	-0.0005
	(0.0007)	(0.0012)
Years from treatment: 1	-0.0007	0.0017
	(0.0009)	(0.0015)
Years from treatment: 2	-0.0005	0.0012
	(0.001)	(0.0017)
Years from treatment: 3	-0.003***	0.0014
	(0.0011)	(0.0019)
Years from treatment: 4	-0.0013	0.0004
	(0.0012)	(0.0016)
Years from treatment: 5	-0.0012	-0.0006
	(0.0013)	(0.0017)
Pre-Trend Joint p-value	0.0778	0.1780
Pre-Trend Sum p-value	0.285	0.589
Mean Dep. Var.	0.0507	0.0155
Observations	54876	54876

Note: The table tests whether the arrival of a foreign priest to a municipality influence local economic outcomes. Column 1 looks at the unemployment per capita and Column 2 displays the effect on the number of contracts per capita. Pre-Trend Joint p-value tests whether all pre-treatment values are jointly equal to zero. Pre-Trend Sum p-value tests whether the sum of all pre-treatment values are different from zero. For further details on the data, see Section 2.3. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A23: Effect of a foreign priest's arrival by Local Catholic Movements

	Standardized			Non-Standardized		
	(1) Baseline	(2) Present	(3) Not Present	(4) Baseline	(5) Present	(6) Not Present
Cath. Weddings	0.0693***	-0.163	0.0652***	0.3619***	-2.094	0.2296***
	(0.0217)	(0.2587)	(0.0175)	(0.1133)	(3.3269)	(0.0615)
Observations	74503	5429	69074	74503	5429	69074
Civil-Only Weddings	-0.0384***	-0.179	-0.0232*	-0.1717***	-1.902	-0.0751*
	(0.0157)	(0.1733)	(0.0133)	(0.0702)	(1.8372)	(0.0432)
Observations	74503	5429	69074	74503	5429	69074
Births	0.0348***	0.1764*	0.0237***	0.6613***	7.9801*	0.3048***
	(0.0104)	(0.0949)	(0.0098)	(0.1999)	(4.2983)	(0.1277)
Observations	74503	5429	69074	74503	5429	69074
Sh. Cons.	0.0979***	0.0571	0.1022***	0.0153***	0.00680	0.0162***
	(0.0315)	(0.0940)	(0.0328)	(0.0049)	(0.0111)	(0.0052)
Observations	44764	3253	41511	44764	3253	41511
Sh. Left	-0.1861***	-0.0887	-0.1935***	-0.0274***	-0.00930	-0.0289***
	(0.0346)	(0.1001)	(0.0361)	(0.0051)	(0.0105)	(0.0054)
Observations	44764	3253	41511	44764	3253	41511
Sh. Rad Right	-0.1383***	-0.0149	-0.1421***	-0.0085***	-0.000900	-0.0088***
	(0.0549)	(0.1332)	(0.0568)	(0.0034)	(0.0080)	(0.0035)
Observations	44764	3253	41511	44764	3253	41511
% Francoist Streets	0.0590***	0.0868***	0.0592***	0.2035***	0.1370***	0.2101***
	(0.0226)	(0.0320)	(0.0237)	(0.0780)	(0.0505)	(0.0841)
Observations	115501	8416	107085	115501	8416	107085

Note: The table tests whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign-born priest. It presents the effect six years after the arrival of a foreign-born priest. See Section 2.3 for further details. It differentiates between those municipalities who had registered as of the year 2000 at least a Catholic organization, so-called Present, and those who did not have any organization registered (Not Present). Columns 1-3 report the effects in Standard Deviations, and Columns 4-6 display them not standardized. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A24: Effect of a foreign priest's arrival by Local Uneducatedness

	Standardized			Non-Standardized		
	(1) Baseline	(2) High	(3) Low	(4) Baseline	(5) High	(6) Low
Cath. Weddings	0.0693***	0.0447***	0.0956***	0.3619***	0.2232***	0.5188***
	(0.0217)	(0.0178)	(0.0357)	(0.1133)	(0.0892)	(0.1944)
Observations	74503	37273	37230	74503	37273	37230
Civil-Only Weddings	-0.0384***	-0.0458**	-0.0308	-0.1717***	-0.1594**	-0.162
	(0.0157)	(0.0220)	(0.0225)	(0.0702)	(0.0768)	(0.1177)
Observations	74503	37273	37230	74503	37273	37230
Births	0.0348***	0.0227**	0.0496***	0.6613***	0.3950**	1.0125***
	(0.0104)	(0.0111)	(0.0176)	(0.1999)	(0.1957)	(0.3621)
Observations	74503	37273	37230	74503	37273	37230
Sh. Cons.	0.0979***	0.0714*	0.1280***	0.0153***	0.0110*	0.0199***
	(0.0315)	(0.0432)	(0.0452)	(0.0049)	(0.0066)	(0.0071)
Observations	44764	22394	22370	44764	22394	22370
Sh. Left	-0.1861***	-0.2114***	-0.1650***	-0.0274***	-0.0311***	-0.0239***
	(0.0346)	(0.0489)	(0.0491)	(0.0051)	(0.0071)	(0.0071)
Observations	44764	22394	22370	44764	22394	22370
Sh. Rad Right	-0.1383***	-0.0860	-0.1897***	-0.0085***	-0.00550	-0.0113***
	(0.0549)	(0.0865)	(0.0650)	(0.0034)	(0.0055)	(0.0038)
Observations	44764	22394	22370	44764	22394	22370
% Francoist Streets	0.0590***	0.0505**	0.0579***	0.2035***	0.1744**	0.1996***
	(0.0226)	(0.0230)	(0.0197)	(0.0780)	(0.0797)	(0.0684)
Observations	115501	57797	57704	115501	57797	57704

Note: The table tests whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign-born priest. It presents the effect six years after the arrival of a foreign-born priest. See Section 2.3 for further details. It differentiates between municipalities with an above median number of individuals with no formal education, and those with a below median number of individuals with no formal education. Columns 1-3 report the effects in Standard Deviations, and Columns 4-6 display them not standardized. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A25: Effect of a foreign priest's arrival by Local Demographic Structure

	Standardized			Non-Standardized		
	(1) Baseline	(2) Many Youth	(3) Few Youth	(4) Baseline	(5) Many Youth	(6) Few Youth
Cath. Weddings	0.0693***	0.1377***	0.000200	0.3619***	0.9710***	0.000100
	(0.0217)	(0.0425)	(0.0048)	(0.1133)	(0.3007)	(0.0024)
Observations	74503	37275	37228	74503	37275	37228
Civil-Only Weddings	-0.0384***	-0.0819***	0.00160	-0.1717***	-0.4959***	0.000900
	(0.0157)	(0.0297)	(0.0088)	(0.0702)	(0.1805)	(0.0049)
Observations	74503	37275	37228	74503	37275	37228
Births	0.0348***	0.0553***	0.0074*	0.6613***	1.4048***	0.0118*
	(0.0104)	(0.0206)	(0.0044)	(0.1999)	(0.5225)	(0.0070)
Observations	74503	37275	37228	74503	37275	37228
Sh. Cons.	0.0979***	0.0923***	0.0970*	0.0153***	0.0125***	0.0162*
	(0.0315)	(0.0375)	(0.0502)	(0.0049)	(0.0051)	(0.0083)
Observations	44764	22406	22358	44764	22406	22358
Sh. Left	-0.1861***	-0.0904**	-0.2754***	-0.0274***	-0.0115**	-0.0432***
	(0.0346)	(0.0427)	(0.0544)	(0.0051)	(0.0054)	(0.0086)
Observations	44764	22406	22358	44764	22406	22358
Sh. Rad Right	-0.1383***	-0.1465***	-0.131	-0.0085***	-0.0089***	-0.00810
	(0.0549)	(0.0601)	(0.0903)	(0.0034)	(0.0037)	(0.0055)
Observations	44764	22406	22358	44764	22406	22358
% Francoist Streets	0.0590***	0.0498**	0.0333	0.2035***	0.1309**	0.137
	(0.0226)	(0.0234)	(0.0419)	(0.0780)	(0.0619)	(0.1720)
Observations	115501	57796	57705	115501	57796	57705

Note: The table tests whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign-born priest. It presents the effect six years after the arrival of a foreign-born priest. See Section 2.3 for further details. It differentiates between municipalities with an above median youth share, and those with a below median youth share. Municipalities are classified using data from the 2001 Spanish Census. Columns 1-3 report the effects in Standard Deviations, and Columns 4-6 display them not standardized. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A26: Effect of a foreign priest's arrival by Religious Leader's Tenure

	Standardized			Non-Standardized		
	(1) Baseline	(2) Senior	(3) Junior	(4) Baseline	(5) Senior	(6) Junior
Cath. Weddings	-0.0234	-0.0241	-0.0303	-0.0737	-0.0577	-0.113
	(0.0265)	(0.0727)	(0.0272)	(0.0833)	(0.1744)	(0.1008)
Observations	18696	9299	9397	18696	9299	9397
Civil-Only Weddings	0.0491	0.118	0.0482	0.167	0.360	0.178
	(0.0377)	(0.1403)	(0.0447)	(0.1277)	(0.4282)	(0.1650)
Observations	18696	9299	9397	18696	9299	9397
Births	0.0182	0.268	-0.0285	0.261	3.265	-0.464
	(0.0307)	(0.1993)	(0.0220)	(0.4415)	(2.4323)	(0.3580)
Observations	18696	9299	9397	18696	9299	9397
Sh. Cons.	0.0555	0.123	0.0813	0.00910	0.0205	0.0130
	(0.0531)	(0.0921)	(0.0700)	(0.0087)	(0.0153)	(0.0111)
Observations	11938	5879	6059	11938	5879	6059
Sh. Left	-0.1526***	-0.2421**	-0.1732**	-0.0234***	-0.0377**	-0.0262**
	(0.0588)	(0.1103)	(0.0762)	(0.0091)	(0.0172)	(0.0115)
Observations	11938	5879	6059	11938	5879	6059
Sh. Rad Right	-0.2694***	-0.3862*	-0.2694***	-0.0195***	-0.0285*	-0.0189**
	(0.1103)	(0.2337)	(0.1155)	(0.0080)	(0.0172)	(0.0082)
Observations	11938	5879	6059	11938	5879	6059
% Francoist Streets	0.0187	0.0287**	-0.0428	0.0546	0.0944**	-0.107
	(0.0263)	(0.0142)	(0.0395)	(0.0760)	(0.0463)	(0.0984)
Observations	33378	15937	17441	33378	15937	17441

Note: The table tests whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign-born priest. It presents the effect six years after the arrival of a foreign-born priest. See Section 2.3 for further details. It differentiates between those priests who have a tenure above the median tenure (Senior), and those who have a tenure below the median (Junior). Columns 1-3 report the effects in Standard Deviations, and Columns 4-6 display them not standardized. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A27: Effect of a foreign priest's arrival by Religious Leader's Age

	Standardized			Non-Standardized		
	(1) Baseline	(2) Old	(3) Young	(4) Baseline	(5) Old	(6) Young
Cath. Weddings	-0.0241	-0.0125	-0.0130	-0.0754	-0.0399	-0.0399
	(0.0265)	(0.0645)	(0.0295)	(0.0832)	(0.2055)	(0.0908)
Observations	18724	9233	9491	18724	9233	9491
Civil-Only Weddings	0.0496	0.104	0.0135	0.168	0.378	0.0421
	(0.0377)	(0.0904)	(0.0454)	(0.1275)	(0.3285)	(0.1424)
Observations	18724	9233	9491	18724	9233	9491
Births	0.0178	0.165	-0.0265	0.257	2.555	-0.351
	(0.0307)	(0.1150)	(0.0240)	(0.4411)	(1.7811)	(0.3161)
Observations	18724	9233	9491	18724	9233	9491
Sh. Cons.	0.0573	0.111	0.0816	0.00930	0.0177	0.0136
	(0.0531)	(0.0987)	(0.0811)	(0.0087)	(0.0157)	(0.0135)
Observations	11962	5743	6219	11962	5743	6219
Sh. Left	-0.1552***	-0.146	-0.2964***	-0.0239***	-0.0221	-0.0458***
	(0.0588)	(0.0998)	(0.0773)	(0.0091)	(0.0152)	(0.0120)
Observations	11962	5743	6219	11962	5743	6219
Sh. Rad Right	-0.2691***	-0.257	-0.2220*	-0.0194***	-0.0178	-0.0164*
	(0.1103)	(0.2470)	(0.1255)	(0.0080)	(0.0172)	(0.0093)
Observations	11962	5743	6219	11962	5743	6219
% Francoist Streets	0.0133	0.0309***	-0.0557	0.0386	0.0742***	-0.184
	(0.0248)	(0.0105)	(0.0452)	(0.0718)	(0.0254)	(0.1492)
Observations	33430	16441	16989	33430	16441	16989

Note: The table tests whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign-born priest. It presents the effect six years after the arrival of a foreign-born priest. See Section 2.3 for further details. It differentiates between those priests who are above the median age (Old), and those that are below the median age (Young). Columns 1-3 report the effects in Standard Deviations, and Columns 4-6 display them not standardized. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A28: Effect of a foreign priest's arrival by Religious Leader's Order

	Standardized			Non-Standardized		
	(1) Baseline	(2) Religious Order	(3) Diocesan	(4) Baseline	(5) Religious Order	(6) Diocesan
Cath. Weddings	0.0627***	0.4675***	0.0329	0.3021***	2.2204***	0.159
	(0.0249)	(0.1576)	(0.0212)	(0.1200)	(0.7481)	(0.1027)
Observations	40511	2776	37735	40511	2776	37735
Civil-Only Weddings	-0.0342**	-0.1763***	-0.0212	-0.1759**	-0.6812***	-0.112
	(0.0170)	(0.0559)	(0.0186)	(0.0877)	(0.2159)	(0.0974)
Observations	40511	2776	37735	40511	2776	37735
Births	0.0408***	0.1172***	0.0361***	0.8404***	2.0713***	0.7481***
	(0.0115)	(0.0467)	(0.0120)	(0.2371)	(0.8245)	(0.2511)
Observations	40511	2776	37735	40511	2776	37735
Sh. Cons.	0.1026***	-0.143	0.1178***	0.0164***	-0.0238	0.0187***
	(0.0329)	(0.1203)	(0.0346)	(0.0052)	(0.0199)	(0.0055)
Observations	25602	1798	23804	25602	1798	23804
Sh. Left	-0.2152***	-0.0817	-0.2243***	-0.0320***	-0.0129	-0.0333***
	(0.0366)	(0.1600)	(0.0373)	(0.0055)	(0.0252)	(0.0055)
Observations	25602	1798	23804	25602	1798	23804
Sh. Rad Right	-0.1779***	-0.0815	-0.2064***	-0.0122***	-0.00550	-0.0142***
	(0.0582)	(0.1315)	(0.0645)	(0.0040)	(0.0089)	(0.0044)
Observations	25602	1798	23804	25602	1798	23804
% Francoist Streets	0.0706***	-0.0285	0.0830***	0.2322***	-0.108	0.2703***
	(0.0195)	(0.1015)	(0.0167)	(0.0641)	(0.3873)	(0.0544)
Observations	71645	4945	66700	71645	4945	66700

Note: The table tests whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign-born priest. It presents the effect six years after the arrival of a foreign-born priest. See Section 2.3 for further details. It differentiates between those priests who are part of a religious orders, and those who are diocesan. Columns 1-3 report the effects in Standard Deviations, and Columns 4-6 display them not standardized. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A29: Effect of a foreign priest's arrival by Religious Leader's Ideology

	Standardized			Non-Standardized		
	(1) Baseline	(2) Conservative	(3) Progressive	(4) Baseline	(5) Conservative	(6) Progressive
Cath. Weddings	-0.184	-0.1869*	-0.101	-0.642	-0.6833*	-0.339
	(0.1322)	(0.1049)	(0.0767)	(0.4609)	(0.3837)	(0.2578)
Observations	6556	2717	3839	6556	2717	3839
Civil-Only Weddings	0.0480	-0.00200	0.0930	0.176	-0.00820	0.311
	(0.0577)	(0.0824)	(0.1098)	(0.2114)	(0.3352)	(0.3666)
Observations	6556	2717	3839	6556	2717	3839
Births	-0.0724	-0.1737***	0.0103	-1.173	-2.9230***	0.162
	(0.0483)	(0.0449)	(0.0522)	(0.7818)	(0.7551)	(0.8226)
Observations	6556	2717	3839	6556	2717	3839
Sh. Cons.	0.2003**	0.6154**	0.127	0.0331**	0.1027**	0.0208
	(0.0998)	(0.2779)	(0.1420)	(0.0164)	(0.0463)	(0.0232)
Observations	4289	1800	2489	4289	1800	2489
Sh. Left	-0.3474***	-0.7506***	-0.3330*	-0.0540***	-0.1180***	-0.0513*
	(0.1326)	(0.1299)	(0.1735)	(0.0206)	(0.0204)	(0.0267)
Observations	4289	1800	2489	4289	1800	2489
Sh. Rad Right	-0.7006***	-0.518	-0.7138***	-0.0537***	-0.0401	-0.0544***
	(0.1423)	(0.3282)	(0.2246)	(0.0109)	(0.0253)	(0.0171)
Observations	4289	1800	2489	4289	1800	2489
% Francoist Streets	0.1004***	0.2344***	0.1089***	0.3257***	0.6172***	0.3946***
	(0.0329)	(0.0671)	(0.0467)	(0.1066)	(0.1770)	(0.1691)
Observations	11576	4892	6684	11576	4892	6684

Note: The table tests whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign-born priest. It presents the effect six years after the arrival of a foreign-born priest. See Section 2.3 for further details. It differentiates between religious leaders with Conservative political views, and those with more progressive political views. Columns 1-3 report the effects in Standard Deviations, and Columns 4-6 display them not standardized. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.A30: Effect of a foreign priest's arrival by Religious Leader's Hierarchy-Leanings

	Standardized			Non-Standardized		
	(1) Baseline	(2) Hierarchical	(3) Not Hierarchical	(4) Baseline	(5) Hierarchical	(6) Not Hierarchical
Cath. Weddings	-0.0637	0.0183	0.5279***	-0.224	0.0786	0.8133***
	(0.0634)	(0.0654)	(0.1277)	(0.2224)	(0.2815)	(0.1967)
Observations	5955	3618	1959	5955	3618	1959
Civil-Only Weddings	0.0183	-0.0394	0.0129	0.0689	-0.178	0.0276
	(0.0575)	(0.0808)	(0.1005)	(0.2169)	(0.3650)	(0.2155)
Observations	5955	3618	1959	5955	3618	1959
Births	-0.0592	-0.127	0.2973***	-0.970	-2.479	2.9186***
	(0.0502)	(0.0922)	(0.0586)	(0.8241)	(1.7961)	(0.5756)
Observations	5955	3618	1959	5955	3618	1959
Sh. Cons.	0.159	0.4291***	-0.162	0.0260	0.0687***	-0.0274
	(0.1002)	(0.1634)	(0.2720)	(0.0164)	(0.0262)	(0.0460)
Observations	3889	2329	1319	3889	2329	1319
Sh. Left	-0.2872**	-0.5622***	-0.3953*	-0.0443**	-0.0848***	-0.0630*
	(0.1312)	(0.1851)	(0.2318)	(0.0201)	(0.0278)	(0.0368)
Observations	3889	2329	1319	3889	2329	1319
Sh. Rad Right	-0.6931***	-0.6553***	-0.273	-0.0524***	-0.0505***	-0.0206
	(0.1388)	(0.2554)	(0.2143)	(0.0104)	(0.0197)	(0.0162)
Observations	3889	2329	1319	3889	2329	1319
% Francoist Streets	0.1010***	0.0918**	0.2161***	0.3368***	0.1977**	1.0219***
	(0.0373)	(0.0432)	(0.0798)	(0.1247)	(0.0930)	(0.3779)
Observations	10475	6316	3470	10475	6316	3470

Note: The table tests whether the arrival of a foreign priest to a municipality motivates a change in a series of municipality characteristics. It presents the effect six years after the arrival of a foreign-born priest. It presents the effect six years after the arrival of a foreign-born priest. See Section 2.3 for further details. It differentiates between religious leaders that do not challenge the pope's decisions (i.e., Hierarchical), and those with their own opinion (i.e., Not Hierarchical). Columns 1-3 report the effects in Standard Deviations, and Columns 4-6 display them not standardized. Standard error are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

1.B Alternative Difference-in-Difference Estimators

In this section, I look at the influence that foreign religious leaders have on their communities, using alternative estimation methods. For each main outcome, I provide the results obtained using the approaches proposed by De Chaisemartin and d'Haultfoeuille (2020); Sun and Abraham (2021); Gardner (2021); Cengiz et al. (2019), together with the canonical two-way fixed effect model. All figures report the 95% confidence intervals.

Figure 1.B1: Effect of a foreign priest's arrival on Catholic weddings - Alt. Estimators

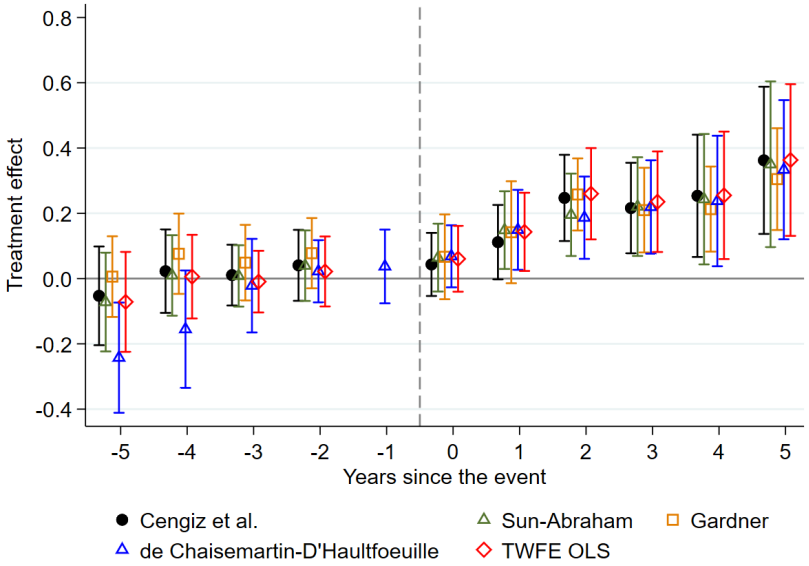


Figure 1.B2: Effect of a foreign priest's arrival on Civil-only weddings Alt. Estimators

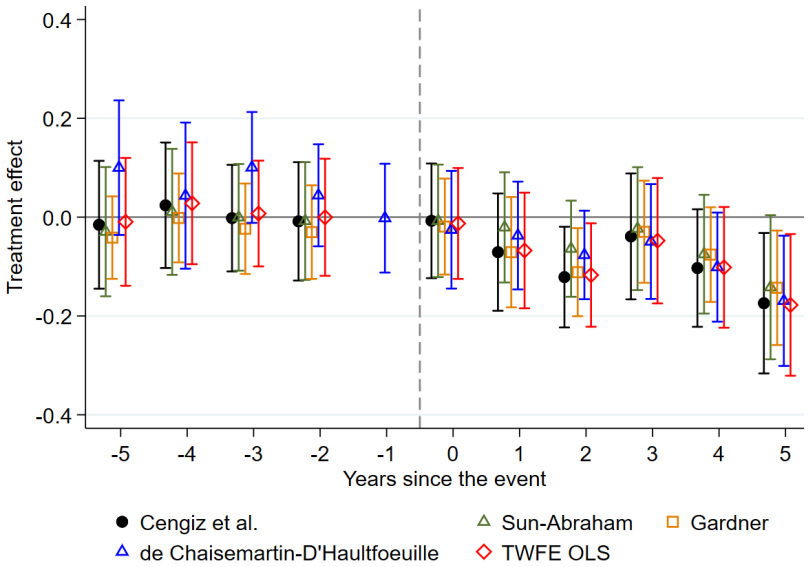


Figure 1.B3: Effect of a foreign priest's arrival on Fertility - Alt. Estimators

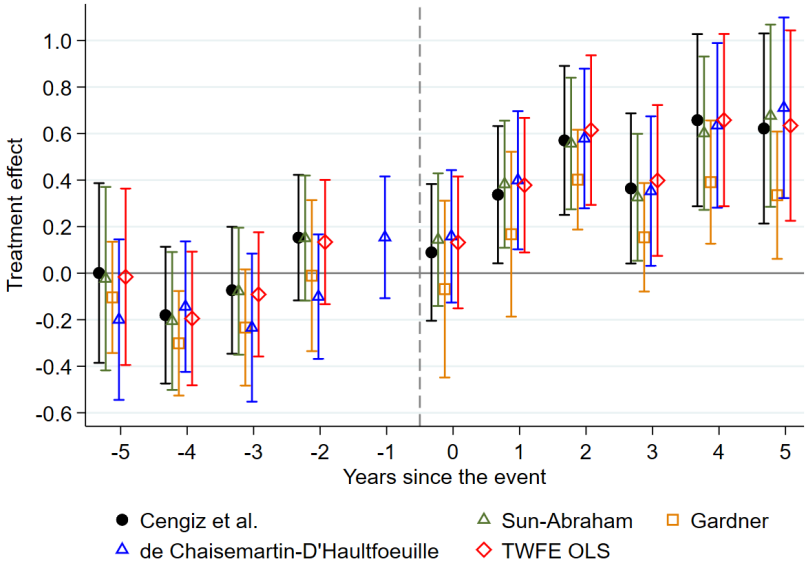


Figure 1.B4: Effect of a foreign priest's arrival on Left-leaning votes - Alt. Estimators

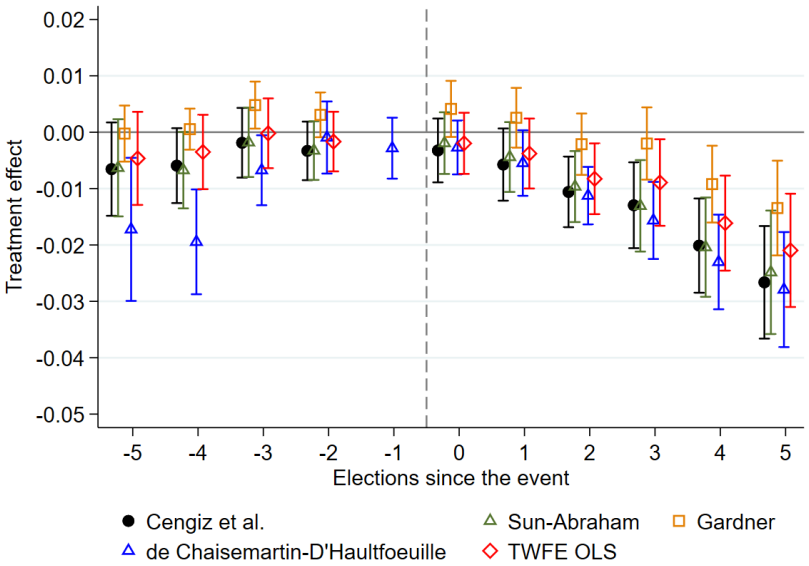


Figure 1.B5: Effect of a foreign priest's arrival on Right-leaning votes
Alt. Estimators

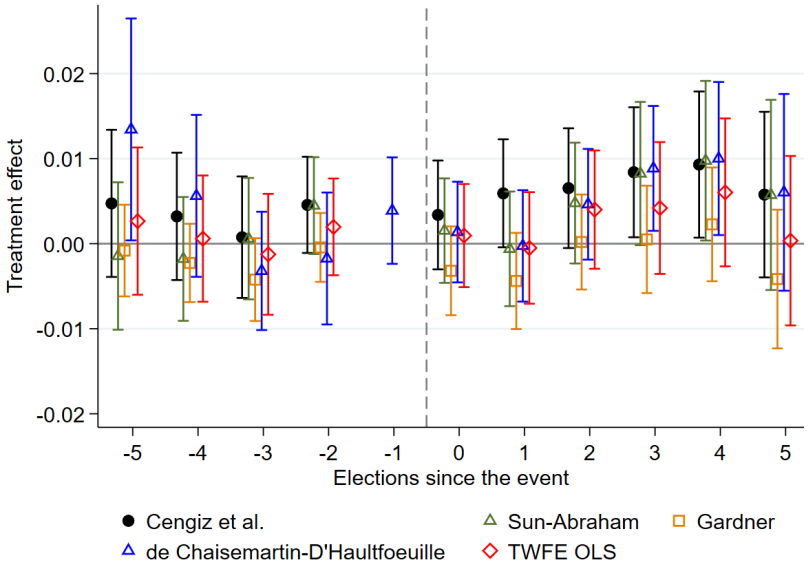


Figure 1.B6: Effect of a foreign priest's arrival on Conservative votes - Alt. Estimators

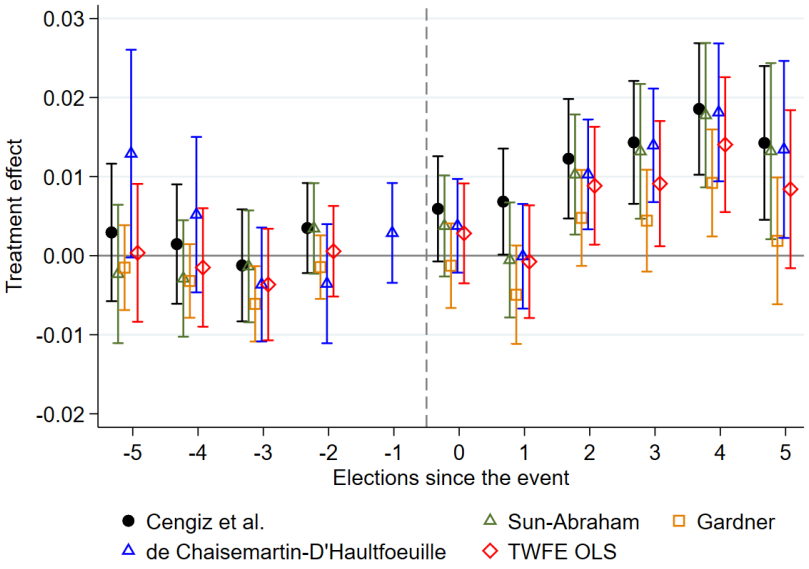


Figure 1.B7: Effect of a foreign priest's arrival on Rad. Right votes - Alt. Estimators

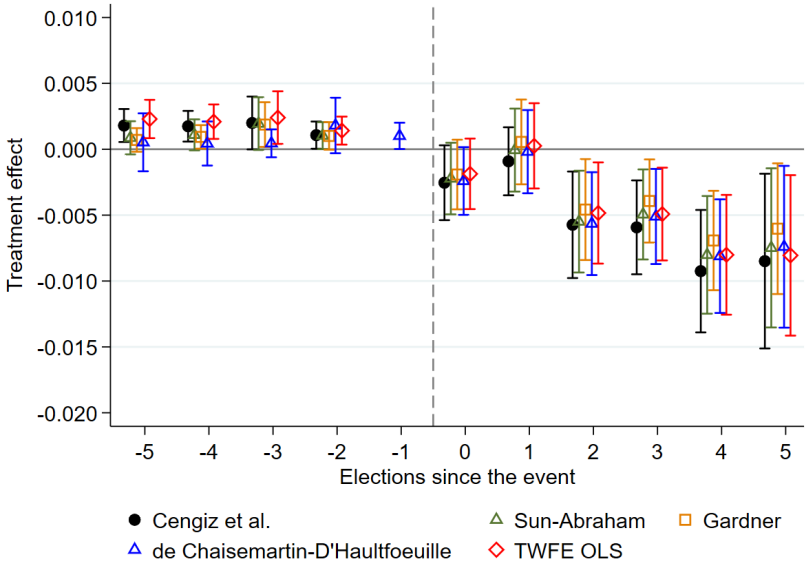


Figure 1.B8: Effect of a foreign priest's arrival on voting absenteeism - Alt. Estimators

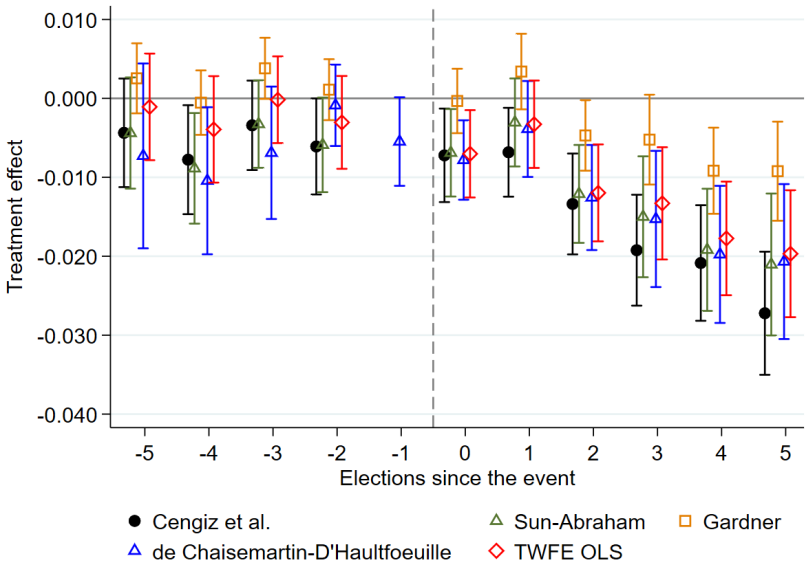
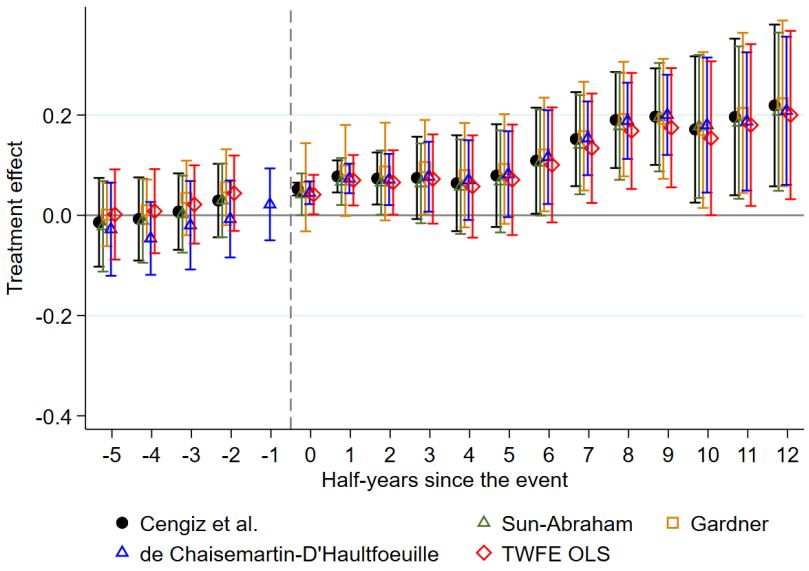


Figure 1.B9: Effect of a foreign priest's arrival on Francoist street naming Alt. Estimators



1.C Priests' Survey - List of Questions

In this section, I outline the questions used in the survey administered to those priests for which I could access their contact details. For each question, I detail the statement, together with the options provided to the interviewee. The same questions were used in both phone and email surveys. The questions were asked in Spanish (available upon request), and followed the ordering shown below:

1) **In which diocese are you currently living?**

2) **How old are you?**

3) **When were you ordained priest?**

4) **In which seminary did you study to become a priest?**

5) **Are you a diocesan priest or are you a member of a religious order?**

   - Diocesan priest
   - Member of a religious order (Please specify)

6) **In which country were you born?**

   6.1) **(If not born in Spain) How long have you been living in Spain?**

   6.2) **(If not born in Spain) Which one was your main motivation for coming to Spain?**

   - Work as missionary
   - Study
   - Express petition by a Spanish bishop
   - Other (Please specify)

7) **Have you ever worked as priest abroad? (Please specify each project and country).**

   7.1) **(If you worked abroad) How long have you worked as a priest abroad?**

8) **How do you identify yourself?**

   - White
   - Mestizo
   - Indigenous

- Black
- Other (Please specify)

Mark from 0 to 10, how much do you agree with the following statements (0=Nothing, 10=Completely):

9) **Nationalizing strategic industries is necessary to protect citizenry**

10) **Those who become rich, do so always by exploiting the work of the many**

11) **In Spain, there exist real equality between man and woman**

12) **We need to look at what unites us instead of dwelling on the past**

13) **Immigrants receive more social protection than nationals**

14) **Minorities should adapt their customs and traditions to the Spanish ones**

15) **The new lifestyles are contributing to today's social rupture**

16) **Spain would have far less problems if more emphasis would be set in traditional families**

For the next two questions, choose one of the following three options:

17) **In your opinion, how are Pope Francisco's views on moral issues?**

- Conservative
- About right
- Liberal

18) **In your opinion, how were Pope Benedict XVI's views on moral issues?**

- Conservative
- About right
- Liberal

19) **According to your own experience, how would you describe today's Spanish economic situation?**

- Very good
- Good

- Regular
- Bad
- Very bad

20) Which percentage of your parishioners attends frequently the Sunday sermon?

21) How many hours per week do you spend officiating masses?

22) How many hours per week do you spend in direct contact with your parishioners (but not in masses)?

23) How frequently do you discuss current sociopolitical events with your parishioners?

- Always
- Often
- Occasionally
- Seldom
- Never

24) In which other projects/activities, not directly linked to your priestly work, do you participate in?

- Helping with the integration of foreign families
- Helping in the local Caritas/Food Bank
- Participating in sport-related activities
- Preparing new events, such as talks
- Nothing (Only for the interviewer)
- Other (Please specify)

25) How frequently do you collaborate with the local administration in the promotion of religious activities? (e.g., the mass, catechism, religious pilgrimages, etc.)

- Always
- Often
- Occasionally
- Seldom

- Never

26) Have you found any new challenge at your parishes? Which ones? (Open question).

1.D Propensity Score Matching

In this section, I use a matching-on-observables approach, in the tradition of Heckman et al. (1997), to account for the fact that treated and control municipalities may be structurally different at baseline. Using this method, I implicitly assume that unobservable characteristics are time-invariant, being eliminated with the introduction of municipality fixed effects.

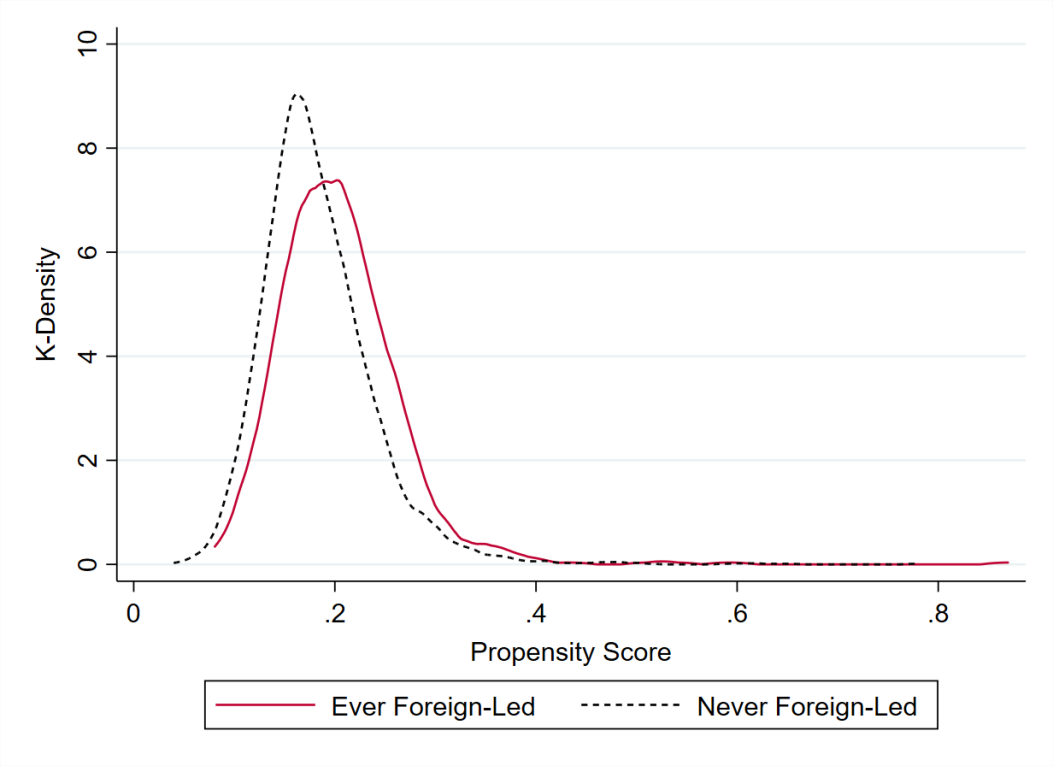
I use a propensity score matching algorithm (PSM) with two neighbors and no replacement on a list of municipality characteristics available at the 2001 Spanish Census. The variables used to matched those municipalities that ever had a foreign priest with their corresponding controls are: population, share of young people, share of retirees, share of people with no formal diploma, share of people with technical education, share of singles, share of divorcees, share of immigrants, unemployment rate, self-employment rate, share of temporal workers, share of farmers, and male and female labor force participation. Table 1.D1 shows the covariate means in control and treatment groups after matching, as well as the p-value of the mean difference between treatment and control groups. For reference, Table 1.2 shows the mean comparison when no propensity score matching is used.

Table 1.D1: Summary Statistics (at baseline) - Matched sample

	Never Foreign-Led	Ever Foreign-Led	p-value
Population	720.267	640.358	0.221
Youth share	0.124	0.122	0.367
Uneducated share	0.208	0.213	0.552
Technical education share	0.725	0.719	0.487
Divorced share	0.004	0.004	0.196
Immigrants share	0.015	0.018	0.043
Labor participation (Male)	0.566	0.561	0.390
Labor participation (Female)	0.275	0.274	0.869
Unemployed share	0.103	0.100	0.458
Temporal workers share	0.216	0.214	0.717
Farmers share	0.283	0.282	0.873
Share Right-wing parties	0.391	0.390	0.919
Grass-root Catholicism	0.055	0.057	0.854
% Francoist Streets	1.254	1.304	0.802

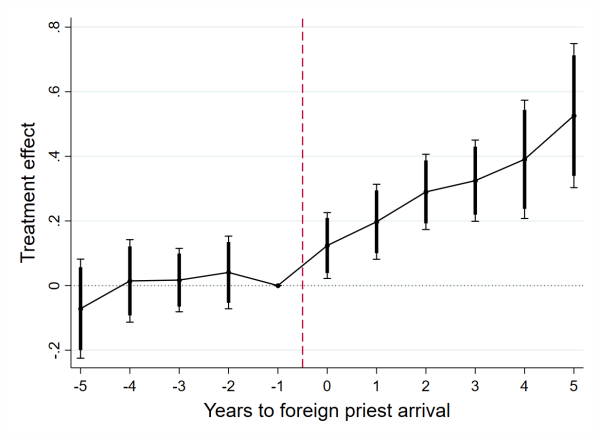
Note: The table provides a comparison of the baseline characteristics between municipalities that had a foreign priest between 2000 and 2019 (ever foreign-led) and those that did not (never foreign-led). The control group is composed by matched municipalities. I use a propensity score matching, with 2 neighbours, no replacement and a caliper of 0.1. The information at the municipal level is extracted from the 2001 Census. The share of votes to right-leaning parties was calculated using all national and European elections held between 1975 and 2000. Grassroot Catholicism identifies whether there existed in 2001 any grassroot Catholic initiative in the municipality. The percentage of Francoist streets is calculated using the 2001 Spanish Street Map Census.

Figure 1.D1: Kernel Densities - Common Support

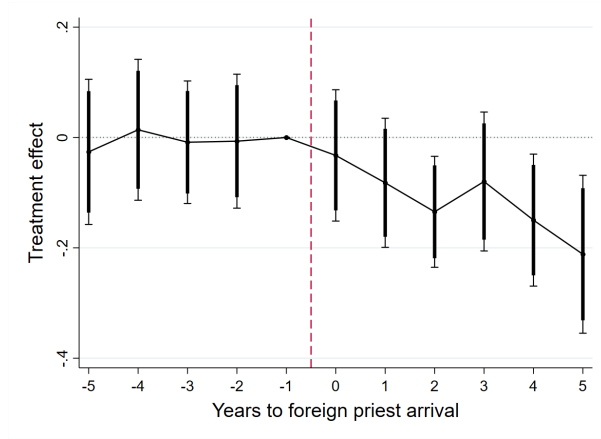


Using the previously explained matching method, Figure 1.D2 reproduces the main findings from Figure 1.5, Figure 1.D3 those from Figure 1.6, Figure 1.D4 those from Figure 1.7, and Figure 1.D5 those from Figure 1.8. I find no significant difference between using only matched municipalities when compared to using the full sample.

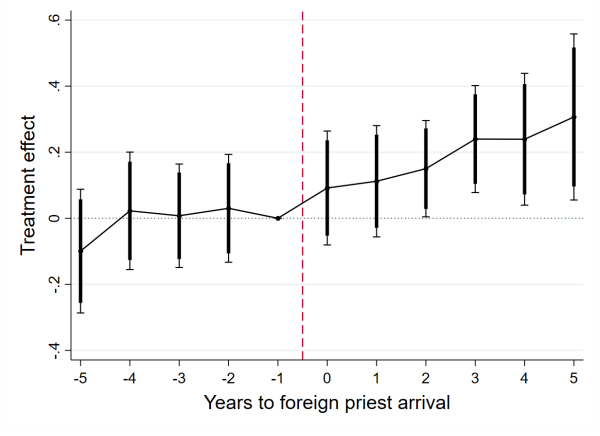
Figure 1.D2: Effect of a foreign priest's arrival on religious outcomes
Matched sample



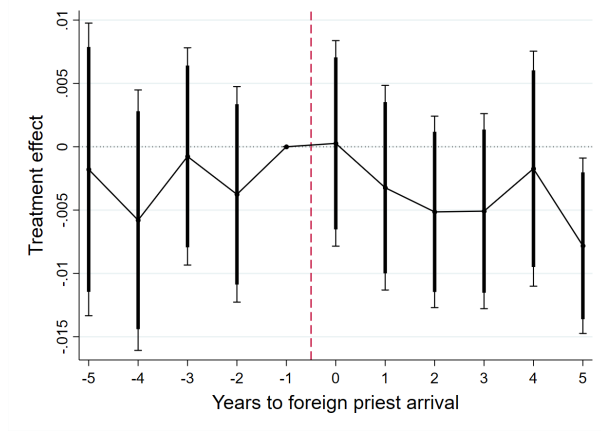
(a) # Catholic weddings



(b) # Civil-only weddings



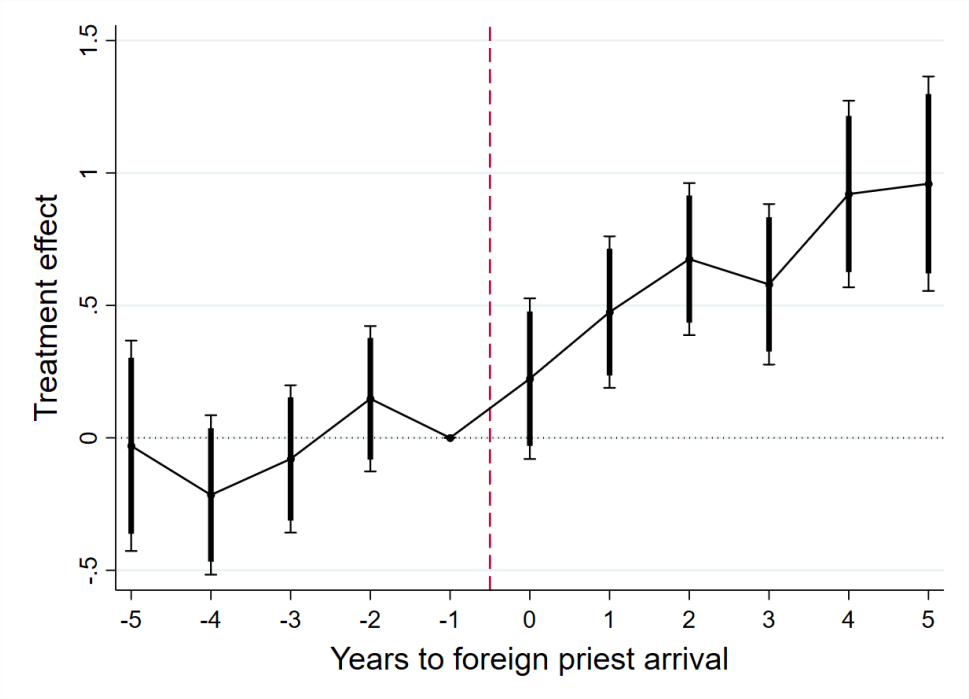
(c) # Weddings



(d) # Weddings (Other Denominations)

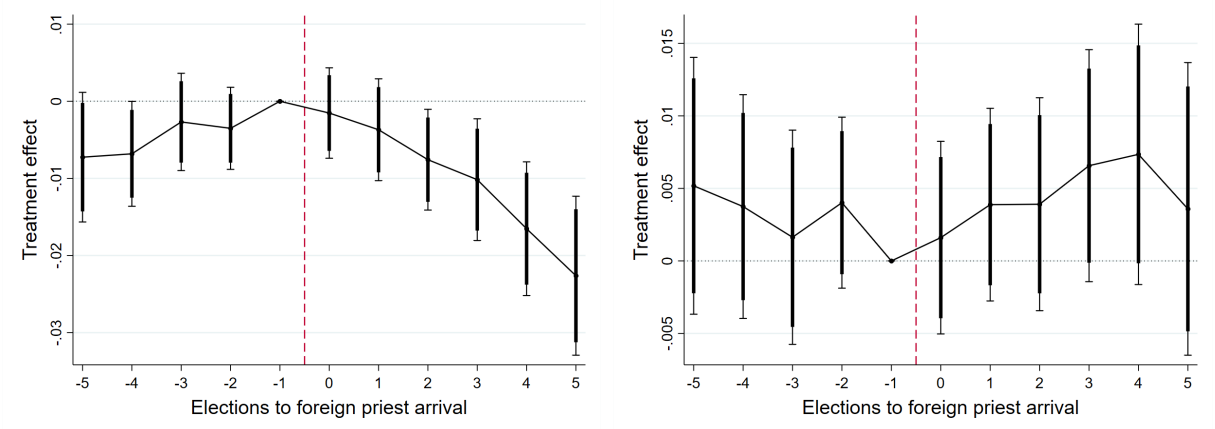
Note: This figure shows whether the arrival of a foreign priest to a municipality affects the probability of getting married, by wedding ritual. Subfigure 1.D2a displays how it affects Catholic weddings, subfigure 1.D2b shows how it affects civil-only weddings, subfigure 1.D2c whether it affects the total number of weddings carried out, and subfigure 1.D2d whether it affects the wedding probability in other denominations. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2.

Figure 1.D3: Effect of a foreign priest's arrival on fertility - Matched sample



Note: This figure shows whether the arrival of a foreign priest to a municipality influences the number of births in the municipality. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2.

Figure 1.D4: Effect of a foreign priest's arrival on political outcomes
Left vs. Right - Matched sample

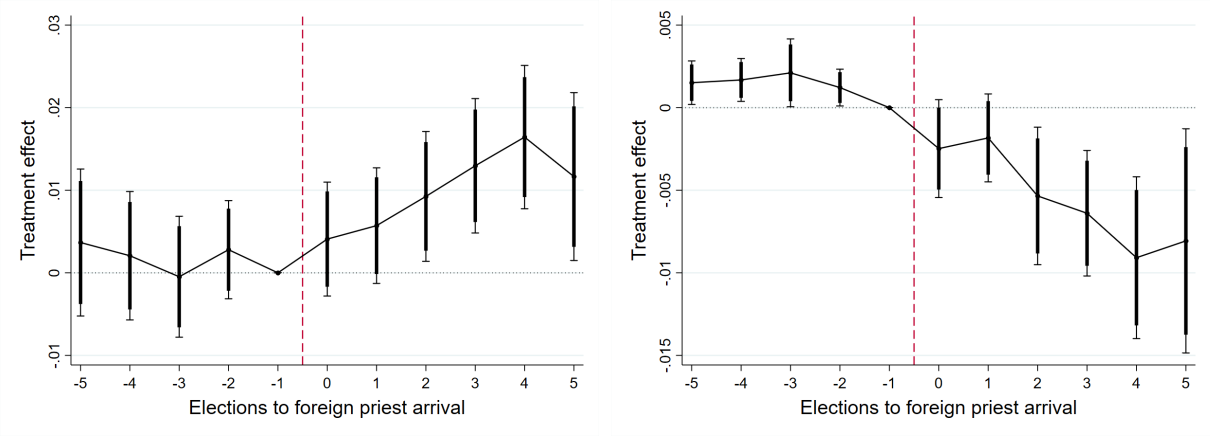


(a) Left-wing parties

(b) Right-wing parties

Note: This figure shows whether the arrival of a foreign priest to a municipality affects its voting behavior. Subfigure 1.D4a displays how it affect the voting share of right-wing parties and subfigure 1.D4b how it affects the voting share of left-wing parties. The x-axis identifies the number of national and European elections since the arrival of a foreign priest. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2.

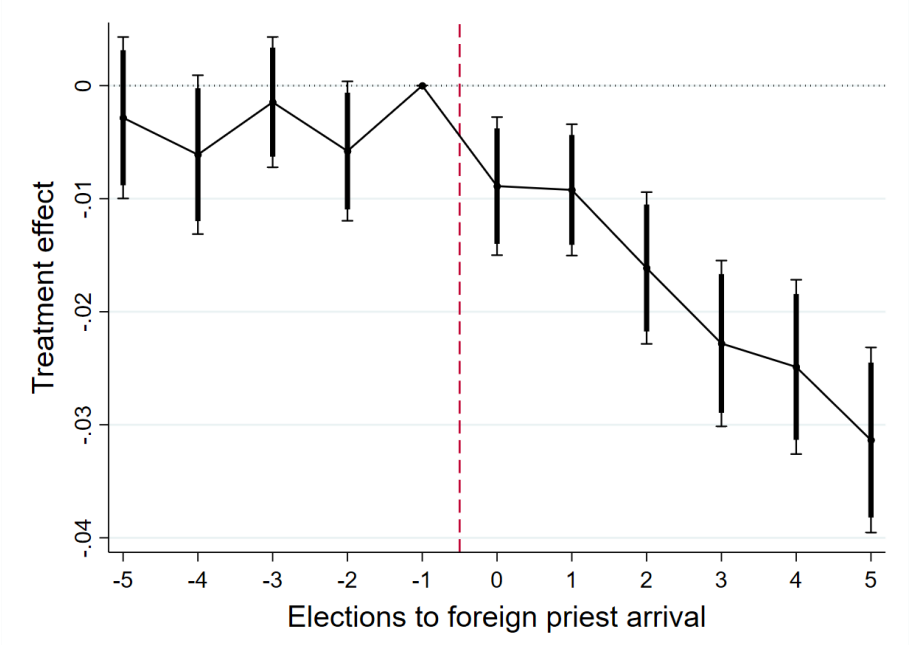
Figure 1.D5: Effect of a foreign priest's arrival on political outcomes
Conservative vs. Rad. Right - Matched sample



(a) Conservative parties (b) Rad. Right parties

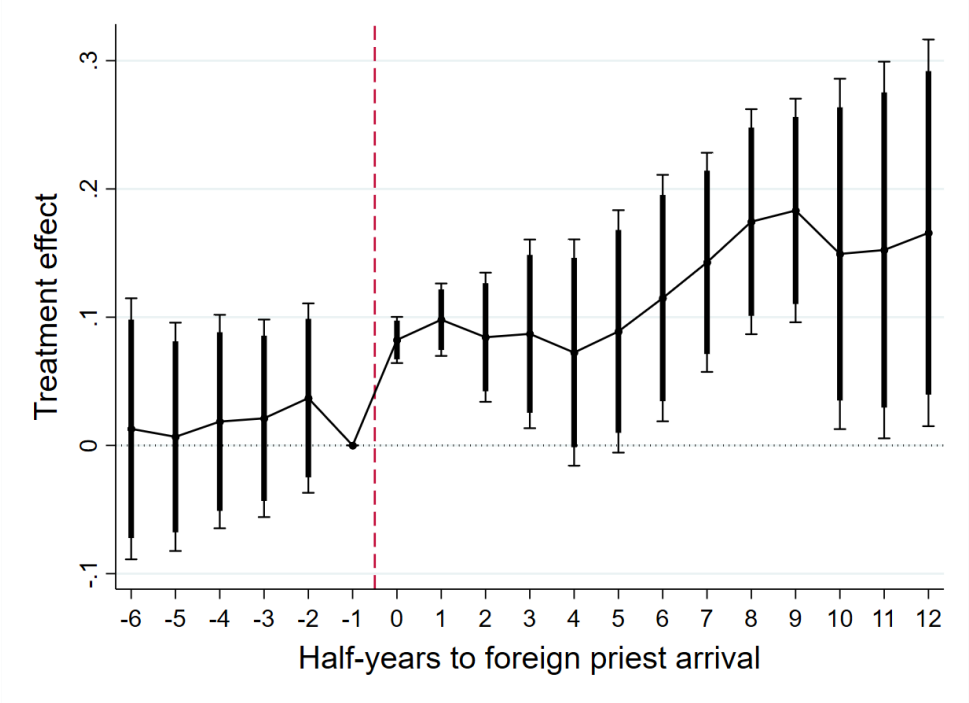
Note: This figure shows whether the arrival of a foreign priest to a municipality affects its voting behavior. Subfigure 1.D5a displays how it affect the voting share of conservative parties and subfigure 1.D5b how it affects the voting share of radical right parties. The x-axis identifies the number of national and European elections since the arrival of a foreign priest. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2.

Figure 1.D6: Effect of a foreign priest's arrival on voting absenteeism - Matched sample



Note: This figure shows whether the arrival of a foreign priest to a municipality has an effect on its electoral participation. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2.

Figure 1.D7: Effect of a foreign priest's arrival on Francoist street naming
Matched sample



Note: This figure shows whether the arrival of a foreign priest to a municipality leads to a change in the local street name composition away from Francoist street naming. All coefficients, 90% (shaded bar) and 95% (upper and lower spikes) confidence intervals are obtained from Equation 1.2.

Chapter 2

Reverse Revolving Doors: The Influence of Interest Groups on Legislative Voting

joint with Josep Amer Mestre[1]

Abstract This paper measures the influence members of parliament with previous involvement in interest groups exert on their colleagues. We collect novel data containing the voting history, and résumés of all legislators present at the European Parliament between 2004 and 2019. Using the alphabetic allocation of seats, we find that seating beside reverse revolvers when the motion is relevant to their interest groups increases co-voting by 2.4%, attendance by 1.3%, and decreases abstention by 9%. These effects are driven by budget-related motions. Our results show that the revolving doors influence the political process even when working in reverse.

2.1 Introduction

Modern democracies have long strived to regulate the activities of interest groups. In recent years, these efforts have been broadened owing to the growing intensity and public notoriety of interest groups. As of 2018, more than 12.000 organizations were openly inter-

[1]We are indebted to our advisors Michèle Belot, Andrea Ichino and Sule Alan for their continued encouragement and guidance. We wish to thank Thomas Crossley, Alessandro Ferrari, Simon Hix, David Levine, Andrea Mattozzi, Alessandro Saia, Stefan Thierse and Zheng Wang and seminar participants at the 2020 European Consortium for Political Research general conference, Bavarian Young Economists' Meeting 2021, Queen Mary University of London, Universitat Autònoma de Barcelona, 28th Meeting on Public Economics, SIOE 2021 conference, 15th CESifo Workshop on Political Economy, PhD-EVS 2021, and at the EUI Microeconometrics Working group for helpful comments. Financial support from the Salvador Madariaga-EUI scholarship is gratefully acknowledged by both authors. All errors remain our own.

ested in influencing European policy-making, spending €2.38 billion on lobbying-related activities (EU Transparency Register, 2018).[2] Lobbying directed at European institutions has mainly focused on influencing legislative powers. In particular, 89% of the Members of the European Parliament (hereafter, MEPs) report receiving voting instructions from interest groups. Similarly, legislators receive at least 21 weekly meeting requests from interest groups, with 59% of MEPs admitting attending at least one of those meetings (Hix et al., 2016).

Interest groups are also known for using a subtler practice, often overlooked by regulators: the *reverse revolving doors*. This practice refers to the flow of individuals from interest groups into active politics (hereafter, reverse revolvers). According to Hix et al. (2016), 22% of surveyed MEPs admitted having been encouraged by an interest group representative to stand in European elections.[3] Understanding whether the presence of reverse revolvers in public institutions affects decision-making is paramount for their healthy development. However, little is known in that respect.

In this paper, we investigate whether the European Parliament members' voting behavior is affected by their close contact with reverse revolvers. We document that 28% of all elected legislators between 2004 and 2019 had worked for an interest group before entering parliament. These engagements range from short work spells for regional NGOs to high-level consulting jobs in lobbying firms. Reverse revolvers are expected to hold policy preferences aligned to those of their former employers. As a result, whenever reverse revolvers influence their colleagues, they would do so in favor of their former employers' interests, even without an active connection.

Given the salience of reverse revolving doors, we set out to estimate the causal effect of legislators with a background in interest groups on the legislative process. The main challenge for our empirical strategy is to obtain a relevant metric of connection between legislators which is also exogenous to the characteristics predicting their voting behavior. We address this issue by using the seating adjacency of legislators in the European Parliament, in which non-leader members of the main political groups sit in alphabetic order. Two main reasons drive our choice of using this measure in the context of the European Parliament: First, lawmakers who sit next to each other during plenary sessions are more likely to interact, influencing each others' views (Masket, 2008; Saia, 2018; Harmon et al., 2019; Lowe and Jo, 2021). Second, the connections created by the alphabetic seating rule are as good as random after conditioning on specific observable characteristics (Harmon et al., 2019). This setting allows us to obtain causal estimates of reverse revolvers' influence

[2]The European Union lobbying industry is the second largest in the world, only after the US. According to OpenSecrets.org, in 2018, the US federal lobbying sector accounted for 11.600 organizations spending $3.42 billion.

[3]Reverse revolving doors are not unique to European institutions. According to OpenSecrets.org, in the US, as of 2017, 148 former lobbyists had been appointed to various executive federal agencies of the Trump administration.

on their colleagues' voting behavior.

A second obstacle that might hinder our causal estimates is the joint selection into lobbying and politics. For example, suppose charismatic individuals are more likely to undertake both career activities. In that case, we would not be able to distinguish the importance of charisma from having interest group working experience. To tackle that concern, we leverage variation in voted subjects by identifying which motions are relevant for the interest groups. Under our assumption that former interest group employees will lean towards opinions aligned with their former employers, our research hypothesis is that reverse revolvers will predominantly influence their seating neighbors' voting behavior when voting on relevant motions to their former employers.

We construct a novel dataset containing information on votes cast by MEPs and their work history. First, we collect all electronic ballots cast at the European Parliament between June 2004 and May 2019, characterizing each motion with the subjects they addressed. Second, we use the legislators' résumé to describe their work experience and education, and spot those who worked for an interest group before taking office. Third, we classify interest groups based on their topics of interest and match them with the subjects of each motion voted in Parliament to determine which votes are relevant for each reverse revolver. Finally, we merge all the previous data with the precise seating arrangement of every legislator in every plenary session, allowing us to study how seating adjacency to a reverse revolving door legislator influences voting behavior depending on the motion's relevance to their past employers.

We find that legislators seated next to reverse revolvers are 2.4% more likely to coincide in their ballots when the voting motions are related to the interest group's economic activity. The magnitude of the effect corresponds to 21% of the influence exerted by those legislators in charge of drafting the motions being voted – also known as *rapporteur* – and 43% of the magnitude of seating next to colleagues from the same national party. In contrast, we find no statistically significant effect of seating next to a former interest group employee when the vote is unrelated to the interest group's economic activity. These results show that reverse revolvers influence their peers when voting in motions relevant to their former interest group.

We shed light on how the legislators' ballots are influenced. First, we show that the influence exerted by reverse revolvers on their peers is twice as large when voting on relevant motions containing important public expenditure decisions. Second, we find that reverse revolvers mobilize their peers towards an active voting position, away from abstention and absenteeism. However, that influence is short-lived as legislators quickly avoid co-voting with their adjacent reverse revolvers.

To the best of our knowledge, this is the first study showing the influence reverse revolving doors have on the legislative process. Our contribution is twofold. First, we

build a unique dataset containing the universe of electronic ballots cast in the European Parliament between 2004 and 2019 and complement it with detailed information on the legislators' background. Second, we exploit the alphabetic seating rule followed at the European Parliament to construct an exogenous measure of network formation. We show that reverse revolvers influence their colleagues when voting on motions relevant to their former employer. These findings have important implications for policy-making as they shed light on a relatively overlooked feature of modern democracies: the presence of former interest group employees in democratically elected institutions. Our results support the hypothesis that revolving doors affect the political process, even when working in reverse.

This paper relates to two different strands of the literature. First, we contribute to the literature on lobbying in politics, which harks back to Logan and Fellow (1929). Some recent studies have provided compelling evidence in favor of the argument that lobbyists' main asset is their connection with policymakers: (de Figueiredo and Silverman, 2006; Blanes i Vidal et al., 2012; Bertrand et al., 2014, 2020; d'Este et al., 2020). While most of the literature focuses on how interest groups benefit from their political connections, our paper is the first one to causally study how those interest groups influence legislative voting in the chamber. We do so by focusing on a commonly overlooked practice: the placement of industry insiders in democratically elected institutions.

Second, this paper contributes to the literature on legislators' voting behavior determinants, which goes back to Rice (1927) and Routt (1938). However, existing evidence on how legislators affect each other's voting behavior is still limited. Recent research has focused on understanding the role of legislators' social ties (Cohen and Malloy, 2014; Battaglini et al., 2023) and in-parliament proximity (Masket, 2008; Saia, 2018; Harmon et al., 2019; Lowe and Jo, 2021) on their co-voting behavior. We build on and contribute to this literature by showing that those legislators who used to work for an interest group influence their seating peers' voting behavior, particularly in motions relevant to their former employer.

The remainder of the paper is organized as follows: Section 2.2 explains the institutional setting. Section 2.3 presents our data. Section 2.5 exposes the empirical strategy followed. Section 2.6 presents the main results, and Section 2.7 concludes.

2.2 Institutional Setting

2.2.1 Legislative Voting in the European Parliament

The European Parliament is the lower legislative branch of the European Union. Members of the European Parliament (MEPs) are chosen through elections held in each EU member state. Once elected, they join cross-national European Political Groups (EPGs) based on their national party's ideology. EPGs comprise legislators from different nationalities with close political affiliations. These groups perform actions similar to conventional political parties in national parliaments. Before every vote, each group discusses its position internally; however, crucially for our analysis, every MEP has the right to choose which ballot to cast in every single vote.

The European Parliament meets once or twice a month, during the so-called plenary sessions, in one of its two venues, Brussels and Strasbourg. These plenary sessions represent the final step of the legislative process, in which legislation is debated and voted on.[4] MEPs cast their ballot in three ways: by show of hands, secret ballot, or electronic vote.[5] In our analysis, we focus on electronic votes as they are the default practice at the European Parliament (i.e., 40% of all votes) and are the only voting method identifying each legislator's ballot. To cast a vote, legislators must first obtain recognition in the system by inserting their unique ID card into their voting device and subsequently pressing the button with their preferred choice. Casting a ballot for a colleague is strictly forbidden and penalized by the Parliament's norms.

2.2.2 Alphabetical Seating in the Chamber

The rules of the Conference of Presidents regulate the seating arrangement in the European Parliament's chambers. MEPs belonging to the different European political groups are clustered in the chamber, and groups are allocated from left to right according to their political orientation. Figure 2.1 shows the seat distribution, highlighting the block seating allocation by the European political groups. Within these groups, leaders sit in the front rows while the remaining seats are generally allocated alphabetically by surname. The five largest groups, S&D, Verts/ALE, ALDE, PPE, and ECR, adhere to this seating

[4]The average plenary session convenes legislators for 4 days. These voting dates start at 9 a.m. and last till 10 p.m. During that time, MEPs are expected to sit in their allocated seat, only being allowed to move around the hemicycle in between debates.

[5]Electronic voting substituted roll-call voting as the only voting procedure in which the MEPs' individual ballots are recorded. Electronic voting is the default practice at the European Parliament, as it encompasses all final legislative votes since 2009, those in which a qualified majority is required, those in which there is no clear visual majority, and those for which any EPG or any group of at least 40 legislators previously requested it.

rule.[6] In total, 55.7% of all MEPs sat alphabetically during our study period, amounting to 1,703 legislators.[7] Throughout the period of study, the European Parliament had an average of 755 legislators, varying with the access of new member states to the Union. The compliance rate with the alphabetic seating rule might vary across groups and time.[8] The explanation for the non-perfect adherence to the seating rule within the "alphabetical groups" is explained by the fact that the rule itself allows for members to occupy another seat for "technical or organizational proposes".

Figure 2.2 illustrates the predictive power of the alphabetical rank on the seating rank. It plots the within-EPG alphabetic rank and the within-EPG seating order for two groups, one that adheres to the seating rule (Panel A) and one that does not (Panel B). In addition, individuals with prior working experience in interest groups are identified. The sample used in our analysis is determined by the change in the seating pattern depicted in Panel A. The dots on the left-hand side of Panel A represent those MEPs in the front rows of their group who do not adhere to the alphabetic seating rule. We identify those as EPG leaders. The dots on the right-hand side represent those MEPs that do sit alphabetically within the seats designated for their EPG, the non-leader MEPs. Lastly, Panel B contains MEPs belonging to an EPG that does not adhere to the alphabetic seating rule. Our analysis is restricted to non-leader MEPs belonging to alphabetically seating EPGs. Moreover, the distribution of legislators with prior experience in an interest group is not spatially nor alphabetically clustered.

2.3 Data

2.3.1 Plenary Sessions

We collect the complete record of electronic votes at the European Parliament between June 2004 and May 2019, corresponding to the 6th, 7th, and 8th legislative terms, from each plenary session summary report. This dataset contains all electronically cast ballots for each MEP and information on the motions' characteristics, such as the subjects covered and the committees involved.[9] We combine this voting information with the MEP's

[6]The sample of non-alphabetically seated groups is composed by: EFD, EFDD, ENF, GUE/NGL, IND/DEM, ITS, UEN. The Greens (Verts/ALE) changed their seating organization to non-alphabetical at the beginning of Term 8.

[7]ALDE places part of its leaders in an alphabetic manner. We consider these alphabetically seated leaders as part of our sample of interest, pooling them with the rest of the alphabetically seated non-leader members. For simplicity, we refer to them also as non-leaders MEPs.

[8]The compliance rate is the correlation between the within-EPG alphabetical and seating rank. The average correlation across all voting dates is 0.92 in our sample of non-leaders from alphabetically organized EPGs.

[9]We restrict our analysis to those motions with an assigned rapporteur. Table 2.A1 in the Appendix displays how motions with and without rapporteur compare, showing the relative importance of the former ones.

corresponding plenary seating arrangement, published before each plenary session on the European Parliament's website.[10]

2.3.2 MEPs' Background

We obtain the legislators' biographical information of all those who took office at any point in time during our studied period from two different sources publicly provided by the European Parliament, namely the MEPs' profiles and their résumés. From the first source, we collect the legislators' characteristics, such as age, sex, nationality, and national party, and their roles in Parliament (e.g., working committees, EPG positions, and procedure rapporteurships). Second, we compile the biographical records of all the MEPs using their submitted résumés upon the start of their mandates.[11] The information in the résumés, initially collected by the European Parliament, was retrieved from the watchdog *Parltrack*. Using the information in these résumés, we classify legislators based on their educational and professional backgrounds.

We identify those MEPs who studied at a "Top 500" university, measured using the 2003 Academic Ranking of World Universities, as a proxy of education excellence as in Fisman et al. (2015). We further characterize MEPs using their professional experience. We use three main measures to classify our legislators: their labor profile, skill level, and topics of expertise. The first measure is obtained by classifying the legislators' working spells with the same categories used by the European Parliament: political, professional, or academic. We assign each parliamentarian to a category by selecting the one with the most repeated type of work spell after weighing them linearly by the duration of each spell. We use a supervised Random Forest algorithm to fill working spells that the European Parliament did not classify under any of these three categories.[12]

Regarding the legislator's skill level, we use a keyword-matching algorithm to capture those spells that reflect high levels of responsibility, such as CEO, secretary general, and director. We then define each parliamentarian as having or not having managerial skills, following the same methodology used to assign a labor profile. Lastly, we assign each legislator the topics in which they gained expertise before entering parliament to rule out any potential confounding effects through better knowledge of the voted subjects. We do this in two stages. First, using the educational and professional background of all legislators, we classify each legislator using the 14 different categories proposed in

[10] In the rare event that no seating plan was available for a particular plenary session, we take the preceding seating plan corresponding to the same venue as reference.

[11] Despite being voluntary, a vast majority of the MEPs (81%) submit their résumé. We hand-collect the biographical information of the remaining MEPs.

[12] We use as training dataset the résumés submitted during the terms 8th and 9th, as the European Parliament classified them under these three categories. The algorithm has a 5% error rate.

Yordanova (2009) and Daniel and Thierse (2018).[13] Next, using all 48 predefined subjects attached to each motion voted in parliament, we select those that best map into each of the 14 expertise categories. Table 2.A2 in the Appendix displays the mapping.

2.3.3 Interest Groups

The other fundamental source of information is provided by the EU Transparency Register. This voluntary register lists those organizations interested in influencing the EU decision-making process. Despite being voluntary, both the European Parliament and the European Commission require individuals to be listed in the register to access its facilities and to participate in a diverse range of activities that they promote, i.e., public consultations and expert groups or to contact high-level decision-makers.[14]

As of 2018, the register encompasses around 12.000 entities, with a total lobbying budget of €2.38 billion and almost 30.000 employees. We assemble a dataset including all the 17.000 entities registered on the European Transparency Registry at any point in time between 2016 and 2019, including information on each organization's lobbying budget, policy interests, and sectors of activity. We use this dataset to extract the list of all organizations that have expressed interest in EU policy-making and match them with the employers' names found in the MEPs' résumés. We employ a keyword-matching algorithm using a wide variety of patterns, such as stemmed words, the interest groups' websites, and different versions and translations of their registered names. The overall matching rate is 85%, computed using a hand-coded sample. A total of 28% of the MEPs in our sample worked for an interest group at some point before taking up office.

Lastly, and crucial for our analysis, we are interested in identifying those relevant motions for the economic activity of the interest groups identified in our sample. To do so, we rely on the 48-policy subject categories the European Parliament assigns to each motion, linking them to each interest group. The result of the hand-coded linkage between policy subjects and interest groups is the indicator variable *Relevant*, which allows us to distinguish which votes are relevant to each interest group. To construct this variable, we use information scattered over different sources, such as the revealed issues of interest reported in the EU Transparency Register, the topics covered during the meetings with high-level officials from the European Commission, and their activity description from their website, among others.[15]

[13] We thank the authors of both studies for kindly providing their data, covering the 6th and 8th parliamentary terms. Following their directions, we coded the same information for the 7th term.

[14] For further information, please refer to the Annual Report on the operations of the Transparency Register (2019) and Rule 11 in the Rules of Procedures of the European Parliament.

[15] Table 2.A3 in the Appendix shows the share of interest groups assigned to each subject and their share over the total number of votes cast.

2.4 Descriptive Statistics

Table 3.1 gives some descriptive evidence of how legislators in the sample used for our analysis, i.e., non-leaders affiliated to alphabetic seating groups, compare in a set of observable characteristics to their party leaders and members of non-alphabetic groups. In our main sample, we identify 5 large groups, namely EPP, S&D, Greens, ECR, and ALDE, with 1,703 MEPs in their ranks. These MEPs cast 55.36% of all ballots at the European Parliament during the 6th, 7th, and 8th legislatures.

Panel A displays information on legislators' individual characteristics. Compared to their leaders, our sample of MEPs is characterized by a higher share of women (37% of the votes cast), younger cohorts, and a lower proportion of members having studied in a top-ranked education institution. Note that no large differences in these measures appear between MEPs in our sample and those affiliated to non-alphabetic seating groups.

Panel B presents the roles held in parliament for each subsample. MEPS who seat alphabetically go marginally less often to vote compared to their party leaders, but do so more frequently than non-alphabetic members. They also hold fewer rapporteurships and positions in working committees than their leaders. This comes as a result of their novel status, with 57% of the votes cast by first-term members. Alternatively, we can observe how our sample of members is more actively involved in the parliament than those legislators from non-alphabetic groups.

Panel C reports information on the legislators' previous working experience. The predominant career profile among European Parliament legislators in our sample of interest is a political one rather than a professional or academic profile (69%, 27%, and 3%, respectively), with similar shares in each of those categories in the other two samples. Legislators in our sample are further defined by having a median working profile, both in terms of experience and managerial status, when compared to their leaders and to members of non-alphabetic groups. Similarly, their average number of prior employment spells, 12.2, represents a mid-ground between their party leaders and those legislators in non-alphabetic groups. Key to our study is that MEPs' résumés are exhaustive, something that can be visually verified by comparing the legislators' mean age and years worked.

Panel D details the information about the legislators' prior interest group experience. We can notice how legislators with that experience are not equally distributed across the three samples. In our main sample, 28% of the legislators have working experience in at least one interest group. Those MEPs are more prevalent among the party leaders of alphabetic seating groups, with 31%, and less among non-alphabetic EPGs, with 19% of their members. Nevertheless, the share of votes considered to be relevant to the economic activity of the interest groups that employed those legislators is similar across the three subsamples (5-6%).

Table 2.2 provides some descriptive evidence on the type of interest groups represented in our sample of non-leaders in alphabetical seating groups. The average interest group is a Belgium-based NGO, with on average 15 employees, 2 of which can access the European facilities, and with an average lobbying budget of 500.000€. Furthermore, the sample used contains a wide variety of interest groups, ranging from small to very large interest groups, as highlighted by the large budget and employees' standard deviations.

2.5 Empirical Strategy

We are first interested in examining the extent to which MEPs' voting behavior is influenced by being placed adjacent to a colleague with working experience in an interest group using the following model:

$$Agree_{iv} = \alpha + \beta_1 Peers\ IG_{iv} + \eta_{iv} \tag{2.1}$$

where $Agree_{iv}$ is a variable capturing the fraction of legislators sitting to the left and right of the focal legislator i casting the same ballot in vote v . $Peers\ IG_{iv}$ is the fraction of adjacent legislators to the focal legislator i during vote v who used to work for an interest group before joining parliament.

To interpret β_1 as the causal effect of sitting beside a colleague with an interest group background, we need legislators not to be able to choose where to sit; otherwise, some of their unobserved characteristics might correlate both with their voting behavior and their previous professional experience, biasing our estimation of β_1. We address this concern by restricting our attention to those members who sit in alphabetical order. Despite the high compliance rate with the alphabetic seating rule, as shown in Section 2.2, we estimate both the intention-to-treat (ITT) and the average treatment effect of the compliers (LATE) instrumenting the group of individuals that sit adjacently to the focal MEP using the individuals whose surname is adjacent in the group's alphabetic rank. Hence, $Name\ Peers\ IG_{iv}$ is the fraction of legislators who previously worked at an interest group whose surnames are adjacent to the focal MEP i in her EPG's alphabetic list in vote v.

A concern when using surname contiguity as an instrument for seat adjacency is that the former might be confounding other unobserved heterogeneous characteristics that cause legislators to vote similarly, such as having similar backgrounds. Using a dyadic approach, Harmon et al. (2019) assesses this concern by showing that, after conditioning for party affiliation and surname similarity controls, surname adjacency between two MEPs does not predict their shared characteristics, such as shared nationality, similar education, freshman status, or gender. Following their results, we control for surname

similarity by using the fraction of adjacent legislators sharing the same surname as the focal MEP and the absolute alphabetic rank across EPGs and terms. These two controls help us mitigate unobservable characteristics shared by the focal and peer legislators.

In addition to the name similarity controls, we further include a comprehensive set of controls to capture any other type of characteristic of the focal legislator and her group of peers that might affect their voting agreement, together with fixed effects by EPG-Term, plenary sessions since the term started, procedure type and vote subject. Section 2.C in the Appendix includes the list of all the controls introduced in our specifications, and their descriptive statistics are reported in Table 2.C1.

Next, we analyze whether the effect captured by β_1 depends on whether the subject of the voted motion is related to the adjacent legislators' former interest groups. To that end, we introduce a new variable that identifies whether any of the subjects of the voted proposal are related to the interest group in which the adjacent colleagues used to work, *Relevant*. Importantly, we code this variable only for the interest groups identified in our sample. Thus, this variable only takes value 1 if the motion voted on is relevant for the economic activity of any of the adjacent reverse revolvers; it takes value 0 when no adjacent legislator has experience in an interest group or when the voting subject is unrelated to their interest group's sector of activity. Thus, we estimate the following fully saturated model:

$$Agree_{iv} = \alpha + \gamma_1 Peers\ IG_{iv} + \gamma_2 Peers\ IG_{iv} \times Relevant_{iv} + \epsilon_{iv} \tag{2.2}$$

as in Equation 2.1, we instrument Equation 2.2 using *Name Peers* IG_{iv} and *Name Peers* $IG_{iv} \times$ *Relevant*, in a twin first stage regression setting. We cluster all standard errors at the legislator level.

2.6 Results

We present our first set of results in Table 2.3. Columns 1 to 5 display the ITT estimates from Equation 2.1, instrumenting *Peers IG* with *Name Peers IG* and progressively including different fixed effects and individual and peer controls. Our first coefficient of interest, present in Column 1, is estimated using a specification that does not include any fixed effect or control variables. It displays a statistically significant increase of 3.5 percentage points in the probability of MEPs casting the same ballot as their adjacent alphabetic peers when they all have professional experience in an interest group. By including EPG-by-Term and plenary session fixed effects and name similarity controls, we then account for the possibility that the estimated effect might come from a specific EPG at a given legislative term, from some temporal trend, or name similarity conditions. The

effect on the agreement probability is still statistically significant while attenuated to an increase of 2.07 percentage points. In Column 3, we further control by vote characteristics, namely by the procedure type and the vote subject, and estimate a similar effect of 2.06 percentage points.

In Column 4, we introduce focal legislators' characteristics, reducing the average probability of casting the same ballot as those surname-adjacent MEPs with an interest group background to 1.27 percentage points. Introducing peer-related controls in Column 5 produces a considerable drop in the probability of co-voting to 0.66 percentage points, and the coefficient becomes statistically insignificant.

Column 6 introduces our main regressor of interest, *Name Peers IG* × *Relevant*. It captures the additional effect of voting on a motion deemed relevant to the former employer of alphabetically adjacent MEPs on their probability of co-voting. It can be interpreted as the additional effect of being adjacent in the alphabetic list to a legislator who used to work for an interest group when the subject of the motion is related to that group's economic activity. When the subject is not relevant to the peers' former employers, the agreement rate is smaller and not precisely estimated. However, when the voting subject is relevant to the peers' former interest group, the probability of vote coincidence increases by 0.7 percentage points.

The mean agreement rate is 70%, implying that the estimated effect of surname adjacency to legislators with interest group background when the vote is relevant to their interest groups represents an increase in the probability of casting the same ballot of 1.9 percent on the mean. The magnitude of this effect is 16% and 44% of the influence of being name adjacent to the rapporteur and shadow rapporteur of the motion, respectively. Similarly, the estimated effect explains 34% of the variation in co-voting with a colleague from the same national party.[16] Given that the primary task of a (shadow)rapporteur is to convince other legislators to vote like them on the motion they represent, we argue that former interest group members have a sizable influence on their adjacent colleagues.

Finally, Column 7 estimates the LATE using both regressors of interest.[17] Compared to Column 6, both *Peers IG* and *Peers IG* × *Relevant* are similar in magnitude to their surname counterparts due to the strong first stages. We find an increase in the average probability of casting the same ballot as the adjacent MEPs when voting on subjects deemed of relevance to their interest groups by 1.7 percentage points, or 2.4%, compared to those legislators with no adjacent former interest group member. This effect corresponds to 21% and 57% of the influence exerted by an adjacent rapporteur or shadow rapporteurs, respectively. Similarly, it corresponds to a 43% of the co-voting behavior

[16]Table 2.A4 displays Table 2.3 together with the coefficients for both focal and peer rapporteur and shadow rapporteurs, and for whether both focal and peer MEPs are from the same national party.

[17]Table 2.A5 in the Appendix reports the first stage results corresponding to Column 7.

with a colleague from the same national party.[18] It is worth noticing that seating adjacency already increases the probability of vote coincidence by 0.6 percentage points, as shown in Harmon et al. (2019). Hence our results are interpreted as the additional influence on top of average seating adjacency effects.

We are now interested in understanding the potential mechanisms at play when former interest group employees turned politicians to persuade their colleagues to vote like them. To that end, we shed light on the channels through which these legislators affect voting behavior, such as voting mobilization, the emphasis on high stake votes, and the importance of the connection persistence over time.[19]

2.6.1 Voting Mobilization

We turn now to analyze how the legislators' ballots are influenced. Under the implicit assumption that legislators who previously worked for an interest group have a clear stance on motions with a subject related to their previous employers, their objective is to mobilize their network to vote in favor or against specific motions along their previous employer's economic activity. Using the specification in Equation (2.2), we estimate whether seating adjacent to a legislator with prior experience in an interest group affects the probability of abstaining from relevant votes.

We use an indicator variable taking value 1 if the focal legislator i casts an abstention ballot in vote v and 0 otherwise. Columns 1-3 in Table 2.4 display the results from that estimation. Seating adjacent to reverse revolvers does not affect voting abstention on average. In contrast, it does when the motion is relevant for the interest group in which the neighboring legislator used to work. In our preferred specification, although small in absolute magnitude, the effect predicts that legislators seating adjacent to reverse revolvers when the vote is of interest for their interest groups are on average 0.3 percentage points or 9% less likely to abstain. These results point towards reverse revolvers influencing their peers out of abstention when the motion voted on is relevant for their former employer. This influence is possible because the limited party line enforcement at the European Parliament reduces the individual cost of casting a vote instead of actively abstaining.

[18] We show in Table 2.A6 how reverse revolvers do affect not only their closest peers but also those at higher distances, with a decaying influence as distance increases. In the same line, Table 2.A7 shows that using row-aggregated information produces consistent results with our main specification. In Table 2.A8, we provide evidence that our benchmark results are not sensitive to different clustering choices, and in Table 2.A9, that they are comparable when assigning each interest group with up to 3 relevant subjects. Finally, Table 2.A10 shows that influence is absent in cross-party neighbors.

[19] Figure 2.B1 in the Appendix shows that reverse revolvers do not have a differential impact along the focal legislators' gender, tenure, expertise, and roles in parliament. Figure 2.B2 shows that the influence exerted by reverse revolvers does not depend on their interest group's economic nature or headquarter location. Figure 2.B3 shows suggestive evidence that reverse revolvers are more efficient at influencing their peers shortly after finishing their employment contracts, however their overall experience in interest group doesn't play a major role.

In the same direction, we could expect reverse revolvers to mobilize their network to participate in the voting process to increase the support for a specific motion. Columns 4-6 in Table 2.4 display the analogous analysis using as the dependent MEPs' absenteeism instead. We estimate Equation (2.2) with the dependent variable being an indicator variable taking value 1 when the focal legislator i was absent during vote v, and 0 otherwise. In our preferred specification, sitting next to reverse revolvers decreases the focal legislator's probability of not attending the vote by 1.15 percentage points. Since MEPs in our sample are, on average absent for 13% of the votes, the effect implies an 8.7% decrease in the mean absenteeism or, conversely, a 1.3% increase in the mean attendance.

Overall, these results show that reverse revolvers mobilize their peers towards an active voting position, away from abstention and absenteeism.

2.6.2 High-Stake Votes

We want to understand whether the influence of reverse revolvers is stronger in high-stakes situations. To that end, we rely on different vote characteristics to identify these types of situations.

First, in order to infer a motion's intrinsic importance, we turn our attention to whether it concerns the budget of the Union or not. We consider this to be a good proxy for high-stakes situations as these are the motions that determine how the annual EU budget is to be spent. Indeed, more than 16% of ballots in our sample refer to votes about the budget. Table 2.5 presents the results depending on whether the motion being voted on concerns the budget of the Union or not. We can observe that legislators are influenced when seating in close proximity to a former interest group member, both when voting on budget and non-budget-related motions. For instance, having all seating neighbors with an interest group background when the subject is relevant for any of their prior employers increases the probability of casting the same ballot by 1.6 percentage points in the case of non-budgetary votes and by 3.4 percentage points on budget votes. Both effects are statistically significant at the 5% level and, when compared to their corresponding average agreement rates, the probability of voting like the seating peers increases by 2.2% for non-budget votes and by 5% for budget-related motions.

Second, to infer the motion's relative voting importance, we look at those that passed by a narrow margin. We consider these to be a good ex-post measure capturing the legislators' voting pivotality in a given motion. In our sample, 2, 9.5, and 18% of the votes refer to motions passed by less than a 1, 5, and 10% margin of victory, respectively. Table 2.6 presents the results depending on the victory margin the motion being voted on concerns the budget of the Union or not. We can observe that seating next to reverse revolvers does not affect the probability of co-voting along the three winning margins

considered, namely winning by 1, 5, or 10 percent. While interest groups might put more resources into winning highly contested motions, legislators are also subject to higher scrutiny from their own party in those votes, making it more costly to deviate from other party peers. As a result, the reverse revolving door practice does not seem to play a significant role during highly contested votes, influencing only uncontested voting motions. Our results provide suggestive evidence that reverse revolvers, rather than fighting for individual voting motions, put more effort into creating majorities.

Overall, all these results suggest that legislators with an interest group background invest significant effort in persuading their colleagues in close proximity during budget-related votes, but are not found to do so during highly contested votes.

2.6.3 Connection Persistence

In the previous section, we showed that sitting adjacent to a reverse revolver increases the likelihood of casting the same ballot, especially in relevant motions for the interest group. In this section, we study how long-lasting that influence is.

On the one hand, sitting next to the same colleagues for long periods could facilitate the exchange of ideas and the negotiation process, thus potentially increasing the agreement rate between those members. In our case, this would allow reverse revolvers to draw adjacent legislators closer to their views. On the other hand, the opposite effect could also play a role; legislators might learn about each other's preferences and, as a result, avoid co-voting with them. In our case, this would imply that the influence of reverse revolvers would decrease over time as their peers learned about each other's inclinations.

Figure 2.3 shows the results of estimating Equation 2.2 looking at the cumulative time legislators have spent with their seating colleagues in a given legislature. As shown in the results of our baseline analysis, reverse revolvers only influence their peers' voting behavior in those motions classified as relevant to their previous employer. Figure 2.3 shows that this effect diminishes as the legislators spend time together. This result suggests that legislators learn from their peers' inclinations, limiting the initial influence exerted by reverse revolvers. It is worth mentioning that all the regressions include time fixed effects, ruling out confounding effects with the parliamentarian learning process. More concretely, we show that reverse revolvers' influence is short-lived, leading to an increase in co-voting by 3 percentage points only during the first year spent together. No significant effects are found at higher time horizons.[20]

[20]Table 2.A11 and Table 2.A12 present the fully interacted version of Figure 2.3, using the number of voting days and sessions together, respectively, showing quantitatively similar results.

2.7 Conclusion

This paper provides novel evidence of interest groups' influence on the legislative process through reverse revolving doors. To do so, we follow a twofold approach. First, we collect a unique dataset containing the universe of electronic votes that took place at the European Parliament between 2004 and 2019 and complement it with detailed information on the legislators' characteristics. In particular, we use the legislators' résumés to pinpoint those with prior experience in an interest group and identify the motions in which their former employers are more interested. We document that 28% of the legislators had work experience on interest groups before entering European politics. Second, we exploit the alphabetic seating rule followed at the European Parliament to construct an exogenous measure of network formation. This setting allows us to estimate the causal effect of sitting next to a former interest group member when voting on motions crucial to their former employer's business activity.

We show that reverse revolvers influence their adjacent colleagues when voting on a motion relevant to their former employer, implying a 2.4% increase in the co-voting probability. Meanwhile, no influence is exerted in non-relevant motions. When voting on relevant motions containing important public expenditure decisions, these results are twice as large. We further show that reverse revolvers influence their seating peers by decreasing their abstention ballots by 9% and increasing their voting attendance by 1.3%. However, legislators quickly learn from their peers' inclinations and avoid co-voting with their adjacent reverse revolvers.

To the best of our knowledge, this is the first study providing causal evidence of the influence of reverse revolving doors on the legislative process. These findings have important implications for policy-making as they shed light on a relatively overlooked lobbying practice used by interest groups, consisting of having insiders sitting in democratically elected institutions. Our results support the hypothesis that revolving doors affect the political process even when working in reverse.

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Tables and Figures

Table 2.1: European Parliament Sample Comparison

	Non-leaders alphabetic EPGs		Leaders alphabetic EPGs		No alphabetic EPGs	
	Votes cast	MEPs	Votes cast	MEPs	Votes cast	MEPs
Panel A: Legislators' characteristics						
Women	0.37	0.36	0.33	0.33	0.31	0.28
Age	53.41	53.22	56.33	55.58	53.14	53.62
Top ranked education	0.30	0.31	0.39	0.37	0.30	0.28
Panel B: Roles in Parliament						
First-term elected	0.57	0.58	0.26	0.34	0.66	0.67
Tenure at the EP	3.21	3.09	6.05	5.41	2.22	2.20
Absence	0.13	–	0.12	–	0.15	–
Rapporteur	0.001	–	0.002	–	0.000	–
Shadow rapporteur	0.003	–	0.003	–	0.01	–
Committee membership	4.96	–	5.37	–	4.65	–
Panel C: Legislators' prior experience						
Work spells	12.19	11.90	14.32	13.33	7.94	8.04
Work experience (years)	24.68	24.39	26.69	26.29	22.68	22.86
Managerial profile	0.27	0.26	0.30	0.28	0.23	0.23
Political	0.69	0.70	0.78	0.78	0.56	0.57
Professional	0.27	0.25	0.17	0.18	0.37	0.37
University	0.03	0.04	0.03	0.03	0.07	0.06
Panel D: Legislators' prior interest group experience						
Worked in interest group	0.28	0.28	0.31	0.31	0.21	0.19
Work experience in interest group (years)	9.40	9.05	9.19	8.86	9.14	8.90
Relevant subject	0.05	–	0.06	–	0.05	–
Total	6,770,336	1,703	3,056,927	828	2,400,508	527

Notes: The table shows counts and shares in three different subsamples representing all the members of the European Parliament. Every member is coded as part of one of these samples or blocks. Columns 1, 3, and 5 represent shares computed using all the votes cast, while Columns 2, 4, and 6, show those same shares computed using individual legislators. The sample selection criterion used to construct each of these three blocks is the same applied to obtain the sample used in the baseline analysis: we use only votes with an assigned rapporteur and containing at least one subject. In Columns 1 and 2, we look at non-leader legislators in an alphabetic seating group. In Columns 3 and 4, we look at those legislators who are leaders in an alphabetic seating group. Finally, in Columns 5 and 6, we look at all other legislators who are affiliated to non-alphabetic seating groups . Moreover, for all three categories, we use only members who sit beside at least one other legislator belonging to the same category.

Table 2.2: Interest Groups' Characteristics

	Mean	SD	Min	Max	N
Panel A: Business Type					
NGOs	0.23	0.42	0	1	513
Academic institutions	0.19	0.39	0	1	513
Companies & Groups	0.18	0.39	0	1	513
Trade Unions	0.10	0.30	0	1	513
Other institutions	0.09	0.29	0	1	513
Trade and Business associations	0.06	0.24	0	1	513
Think Tanks	0.06	0.23	0	1	513
Transnational associations	0.04	0.19	0	1	513
Consultancies	0.03	0.17	0	1	513
Regional structures	0.03	0.17	0	1	513
Panel B: Headquarter's Location					
Belgium	0.23	0.42	0	1	513
Germany	0.12	0.32	0	1	513
United Kingdom	0.11	0.32	0	1	513
Italy	0.07	0.26	0	1	513
France	0.07	0.25	0	1	513
Poland	0.04	0.21	0	1	513
Finland	0.04	0.20	0	1	513
Netherlands	0.04	0.20	0	1	513
Spain	0.04	0.20	0	1	513
Denmark	0.03	0.17	0	1	513
Rest of Europe	0.15	0.36	0	1	513
Rest of the World	0.05	0.22	0	1	513
Panel C: Other Characteristics					
Num. Employees	14.81	209.82	0	4750	513
Num. EP Accreditations	1.78	3.86	0	53	513
Lobbying Budget	512,445	1,131,297	0	10,000,000	513

Notes: The table displays the mean, standard deviation, minimum, and maximum values for a set of interest group's characteristics. The interest groups used correspond to those identified in the résumés of non-leader MEPs affiliated with an alphabetic seating group.

Table 2.3: Reverse Revolving Doors Connection and Vote Coincidence

	(1) OLS Agree	(2) OLS Agree	(3) OLS Agree	(4) OLS Agree	(5) OLS Agree	(6) OLS Agree	(7) 2SLS Agree
Name Peers IG	0.0350***	0.0207***	0.0206***	0.0126**	0.0066	0.0059	
	(0.0076)	(0.0067)	(0.0067)	(0.0053)	(0.0049)	(0.0050)	
Name Peers (IG × Relevant)						0.0074*	
						(0.0039)	
Peers IG							0.0080
							(0.0066)
Peers (IG × Relevant)							0.0092*
							(0.0049)
EPG × Term FEs	No	Yes	Yes	Yes	Yes	Yes	Yes
Sessions since term started FEs	No	Yes	Yes	Yes	Yes	Yes	Yes
Procedure type FEs	No	No	Yes	Yes	Yes	Yes	Yes
Vote subject FEs	No	No	Yes	Yes	Yes	Yes	Yes
Name controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Focal MEP controls	No	No	No	Yes	Yes	Yes	Yes
Peers controls	No	No	No	No	Yes	Yes	Yes
Observations	6,770,336	6,770,336	6,770,336	6,770,336	6,770,336	6,770,336	6,770,336
Mean Agree	0.707	0.707	0.707	0.707	0.707	0.707	0.707
Joint p-value						0.0236	0.0254
F-stat 1							1056
F-stat 2							1308

Notes: Results of estimating Equation (2.2). Joint p-value tests the joint significance of being adjacency to reverse revolvers and when the topic is relevant for any of their interest groups. A comprehensive set of controls of the focal and peer legislators is used. See Appendix 2.C for further information on the controls included. Standard errors, in parenthesis, are clustered at the legislator level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.4: Reverse Revolving Doors Connections and Voting Abstention and Absenteeism

	(1) OLS Abstain	(2) OLS Abstain	(3) 2SLS Abstain	(4) OLS Absent	(5) OLS Absent	(6) 2SLS Absent
Name Peers IG	-0.0010	-0.0009		-0.0086*	-0.0086*	
	(0.0016)	(0.0016)		(0.0047)	(0.0047)	
Name Peers (IG × Relevant)		-0.0017**			-0.0000	
		(0.0008)			(0.0038)	
Peers IG			-0.0012			-0.0115*
			(0.0021)			(0.0062)
Peers (IG × Relevant)			-0.0020**			-0.0000
			(0.0010)			(0.0047)
EPG × Term FEs	Yes	Yes	Yes	Yes	Yes	Yes
Sessions since term started FEs	Yes	Yes	Yes	Yes	Yes	Yes
Procedure type FEs	Yes	Yes	Yes	Yes	Yes	Yes
Vote subject FEs	Yes	Yes	Yes	Yes	Yes	Yes
Name controls	Yes	Yes	Yes	Yes	Yes	Yes
Focal MEP controls	Yes	Yes	Yes	Yes	Yes	Yes
Peers controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,881,658	5,881,658	5,881,658	6,770,336	6,770,336	6,770,336
Mean dep. variable	0.0229	0.0229	0.0229	0.131	0.131	0.131
Joint p-value		0.131	0.139		0.141	0.134
F-stat 1			1020			1056
F-stat 2			1236			1308

Notes: Results of estimating Equation (2.2) using as the dependent variable whether the legislator cast an abstention ballot (Columns 1-3) or was absent during the vote (Columns 4-6). Joint p-value tests the joint significance of being adjacency to reverse revolvers and when the topic is relevant for any of their interest groups. A comprehensive set of controls of the focal and peer legislators is used. See Appendix 2.C for further information on the controls included. Standard errors, in parenthesis, are clustered at the legislator level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.5: Reverse Revolving Doors Connections and Vote Coincidence by Vote Type

	Non-budget vote			Budget vote		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	2SLS	OLS	OLS	2SLS
	Agree	Agree	Agree	Agree	Agree	Agree
Name Peers IG	0.0062	0.0054		0.0073	0.0069	
	(0.0049)	(0.0049)		(0.0076)	(0.0076)	
Name Peers (IG × Relevant)		0.0070*			0.0222**	
		(0.0040)			(0.0102)	
Peers IG			0.0073			0.0092
			(0.0065)			(0.0101)
Peers (IG × Relevant)			0.0087*			0.0274**
			(0.0050)			(0.0124)
EPG × Term FEs	Yes	Yes	Yes	Yes	Yes	Yes
Sessions since term started FEs	Yes	Yes	Yes	Yes	Yes	Yes
Procedure type FEs	Yes	Yes	Yes	Yes	Yes	Yes
Vote subject FEs	Yes	Yes	Yes	Yes	Yes	Yes
Name controls	Yes	Yes	Yes	Yes	Yes	Yes
Focal MEP controls	Yes	Yes	Yes	Yes	Yes	Yes
Peers controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,651,802	5,651,802	5,651,802	1,118,534	1,118,534	1,118,534
Mean Agree	0.703	0.703	0.703	0.732	0.732	0.732
Joint p-value		0.0354	0.0376		0.0119	0.0119
F-stat 1			1055			977.1
F-stat 2			1290			598.1

Notes: Results of estimating Equation (2.2) using only votes related to the Union's budget and those not. Joint p-value tests the joint significance of being adjacency to reverse revolvers and when the topic is relevant for any of their interest groups. A comprehensive set of controls of the focal and peer legislators is used. See Appendix 2.C for further information on the controls included. Standard errors, in parenthesis, are clustered at the legislator level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.6: Average effect of reverse revolving doors connections on vote coincidence by margin of victory

	(1) OLS Agree	(2) OLS Agree	(3) 2SLS Agree	(4) OLS Agree	(5) OLS Agree	(6) 2SLS Agree	(7) OLS Agree	(8) OLS Agree	(9) 2SLS Agree
Name Peers IG	-0.0013	-0.0008		0.0029	0.0031		0.0042	0.0041	
	(0.0082)	(0.0082)		(0.0073)	(0.0072)		(0.0067)	(0.0067)	
Name Peers (IG * Relevant)		-0.0042			-0.0015			0.0009	
		(0.0090)			(0.0071)			(0.0065)	
Peers IG			-0.0012			0.0040			0.0054
			(0.0109)			(0.0096)			(0.0088)
Peers (IG * Relevant)			-0.0053			-0.0018			0.0011
			(0.0112)			(0.0089)			(0.0081)
EPG x Term FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sessions since term started FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Procedure type FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Vote subject FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Name controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Focal MEP controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Peers controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	135,195	135,195	135,195	643,715	643,715	643,715	1,226,807	1,226,807	1,226,807
Mean Agree	0.693	0.693	0.693	0.686	0.686	0.686	0.683	0.683	0.683
Joint p-value		0.675	0.679		0.877	0.869		0.586	0.582
F-stat 1			963.6			980.7			1075
F-stat 2			787.3			904.2			1045

Notes: This table shows the results of estimating Equation (2.2) with different sample of votes depending on the margin of victory. The sample is divided into votes that were passed by a margin of 1% (Columns 1, 2, 3), 5% (Columns 4, 5, 6) and 10% (Columns 7, 8, 9). We denote as joint p-value the test on the joint significance of the adjacency to a legislator with previous experience in an interest group, and when the topic is relevant for such interest group (both at the surname and seating level). A comprehensive set of controls at the focal and peer legislators is used in the analysis. See Appendix 2.C for further information on the controls included. Standard errors are clustered at legislator level. *** p<0.01, ** p<0.05, * p<0.1.

Figure 2.1: Strasbourg Seating Plan during the Plenary Session
Held on February 4th, 2013

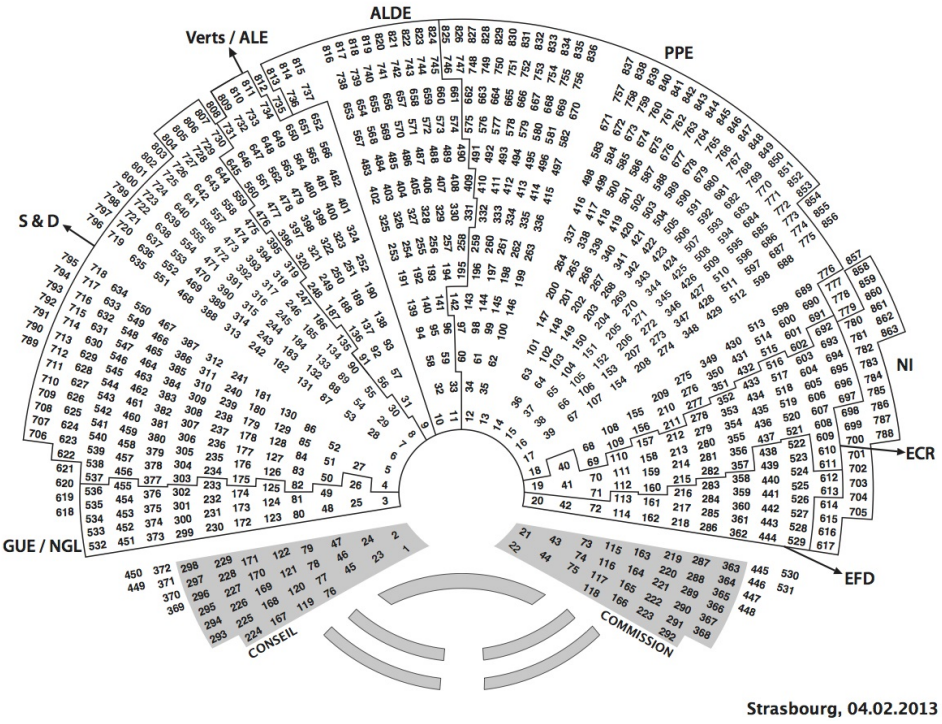
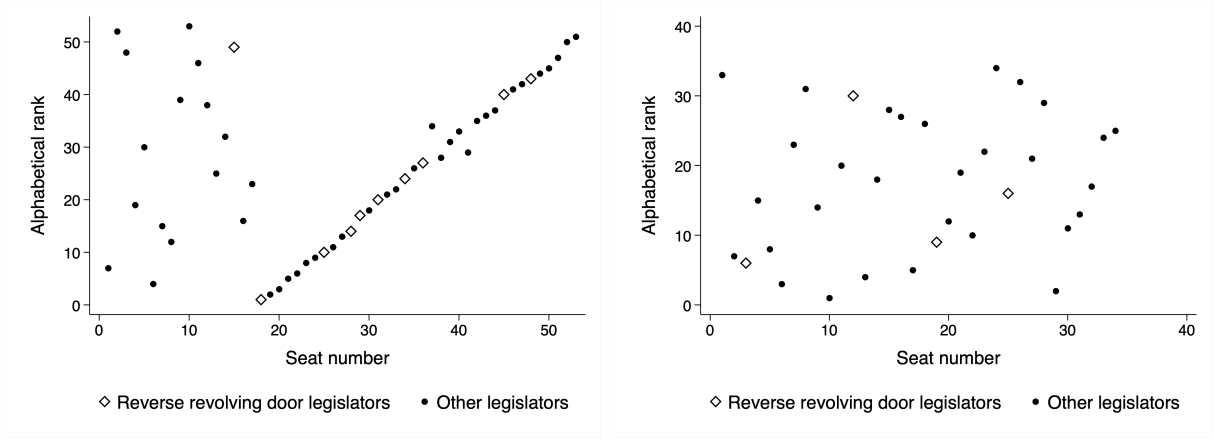


Figure 2.2: Seating and Alphabetical Rank

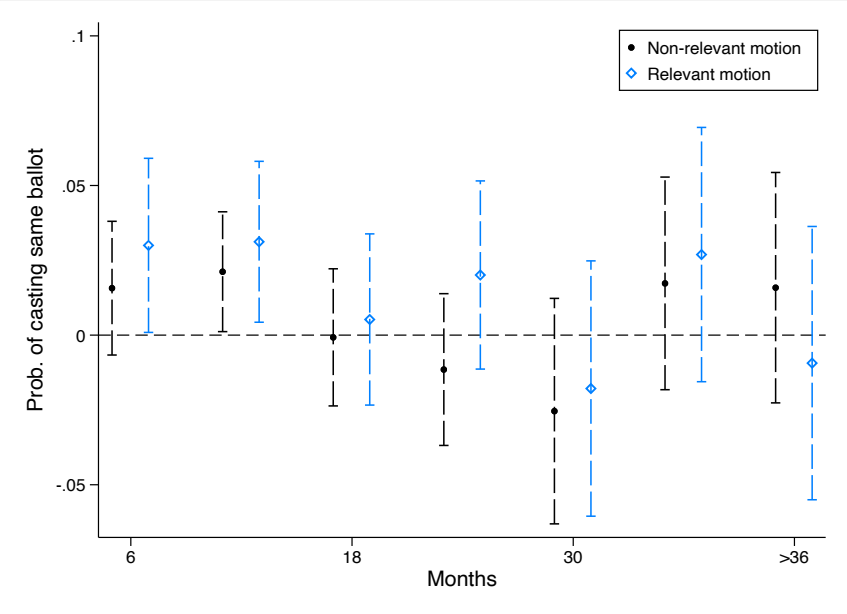


(a) Alphabetically Seating Group

(b) Non-alphabetically Seating Group

Notes: This figure shows the correlation between within-EPG alphabetic rank and within-EPG seating rank. Subfigure 2.2a displays the correlation for the ECR group, which adheres to the alphabetic seating rule. Subfigure 2.2b looks at the GUE/NGL group, which does not adhere to the alphabetic seating rule. The data plotted corresponds to the plenary seating held on February 5, 2013.

Figure 2.3: Revolving Doors and Vote Coincidence Over Time



Notes: Results of estimating Equation (2.2) at different cumulative times each group of legislators have been seated adjacently in a given legislature. The results shown correspond to the effect of sitting adjacently to reverse revolvers when the subject of the motion is not relevant for any of their former employer, *Non-Relevant motion*, and when it is, *Relevant motion*. A comprehensive set of controls of the focal and peer legislators is used. See Appendix 2.C for further information on the controls included. Standard errors are clustered at the legislator level. Dashed vertical lines represent the 95% confidence level.

Appendix

2.A Tables

Table 2.A1: Summary of Samples by Rapporteur Presence

	With Rapporteur	Without Rapporteur
Panel A: Voting distribution		
Electronic ballots	13,365,545	4,067,500
In favour	51.78	42.52
Abstained	3.49	3.84
Against	31.37	34.62
Absence	13.36	19.03
Panel B: Vote characteristics		
Position on voting order	40.10	35.52
Budget of the Union	13.12	0.09
Legislative & Non-legislative	38.32	2.13
Resolutions and initiatives	48.56	97.78

Notes: Counts and shares by whether a vote had a rapporteur assigned to or not. It displays the absolute frequency of electronic ballots cast with and without rapporteur during the terms 6, 7 and 8. The distributions by vote outcome and by vote characteristics are expressed in percentages. The three type of procedure categories shown in Panel B are based on the procedure description present at the European Parliament website.

Table 2.A2: Mapping of Expertise and Vote Subjects

Variable as in Yordanova (2009)	Vote subjects
Business/Industry	Common commercial policy in general; Competition; Enterprise policy, inter-company cooperation; Free movement of goods; Free movement of services, freedom to provide; Industrial policy; Taxation
Economics/Finance	Common commercial policy in general; Competition; Economic union; Enterprise policy, inter-company cooperation; European statistical legislation; Free movement of capital; Monetary union; Taxation
Education	Common cultural area, cultural diversity; Education, vocational training and youth; Research and technological development and space
Farming	Agricultural policy and economies; Fisheries policy
Green ties	Agricultural policy and economies; Environmental policy; Fisheries policy
International relations	Common foreign and security policy; Development cooperation; Emergency, food, humanitarian aid, aid to refugees, Emergency Aid Reserve; Enlargement of the Union; Relations with third countries
Legal	Citizen's rights; Consumers' protection in general; EU law; Free movement and integration of third-country nationals; Fundamental rights in the EU, Charter; Institutions of the Union; Judicial cooperation; Justice and home affairs; Police, judicial and customs cooperation in general; Revision of the Treaties, intergovernmental conferences; Treaties in general
Local government	Common cultural area, cultural diversity; Regional policy; Tourism
Media	Information and communications in general
Medicine	Public health
Science/Engineering	Energy policy; Environmental policy; Information and communications in general; Research and technological development and space
Social group	Citizen's rights; Free movement and integration of third-country nationals; Fundamental rights in the EU, Charter; Social policy, social charter and protocol
Trade Union	Employment policy, action to combat unemployment; Free movement of workers; Social policy, social charter and protocol
Transport/Telecommunications	Transport policy in general

Notes: The table displays how the expertise topics, as in Yordanova (2009), map into the vote subjects at the European Parliament.

Table 2.A3: Vote and Interest Groups Share by Procedure Subject

Vote Subjects	Share votes	Share IGs	Num. MEPs	Extra subjects
Budget of the Union	16.52	0	0	2.068
Environmental policy	12.08	3.824	15	2.558
Social policy, social charter and protocol	10.24	4.706	17	2.032
Employment policy, action to combat unemployment	8.815	10.29	35	2.366
Agricultural policy and economies	8.577	3.529	12	2.361
Industrial policy	7.753	3.235	11	2.767
Institutions of the Union	6.804	0.588	3	2
Consumers' protection in general	6.757	1.765	7	2.673
Common commercial policy in general	6.728	0.882	4	2.433
Transport policy in general	6.221	3.824	14	2.359
Common foreign and security policy	5.296	3.824	16	1.886
Energy policy	5.218	3.235	11	2.638
Police, judicial and customs cooperation in general	4.871	0.294	1	2.253
Relations with third countries	4.812	0	0	2.123
Research and technological development and space	4.120	5.588	20	2.394
Enterprise policy, inter-company cooperation	3.697	3.529	14	2.468
Fisheries policy	3.672	0.588	2	2.195
Public health	3.596	4.706	19	2.426
Free movement and integration of third-country nationals	3.498	1.471	5	1.821
Regional policy	3.346	8.529	30	2.311
Economic union	3.187	0	0	2.125
Free movement of capital	3.080	8.529	31	2.133
Free movement of services, freedom to provide	3.050	0.294	1	2.561
Information and communications in general	2.993	16.18	55	2.292
Free movement of goods	2.836	0	0	2.781
Development cooperation	2.719	1.176	5	2
Economic growth	2.660	0	0	2.417
Citizen's rights	2.657	0.588	3	2.441
Monetary union	2.300	0.294	1	1.833
Taxation	2.203	0.588	2	2.122
Judicial cooperation	1.917	0	0	2
Fundamental rights in the EU, Charter	1.867	1.471	6	2.148
Competition	1.661	0	0	2.308
Cooperation between administrations	1.489	0.294	1	2.532
Enlargement of the Union	1.409	0.294	2	1.375
Education, vocational training and youth	1.406	27.35	95	1.933
Revision of the Treaties, intergovernmental conferences	1.249	0	0	1.400
EU law	1.130	0	0	2.163
Common cultural area, cultural diversity	0.814	1.176	4	2.222
Global economy and globalisation	0.766	0.294	2	1.789
Treaties in general	0.672	0.294	2	1.222
Free movement of persons	0.338	0	0	2
Emergency, food, humanitarian aid, aid to refugees, Emergency Aid Reserve	0.281	1.471	5	1.786
Tourism	0.231	0.294	1	1.143
European statistical legislation	0.223	0	0	1.429
Free movement of workers	0.126	0	0	2.857
Justice and home affairs	0.0851	0	0	2
Civil protection	0.0774	0.294	1	1.250

Notes: Share of votes by procedure subject in Column 1. Column 2 shows the share of legislators who previously worked for an interest group, and for which the subject is considered to be relevant, and Column 3 shows the total number of them. Column 4 displays the average number of subjects each procedure classified with a particular subject is accompanied by. The sample used is the same as in the main analysis, namely only votes with a rapporteur and cast by legislators identified as non leader in alphabetically organized groups with peers satisfying the same requirements.

Table 2.A4: Reverse Revolving Doors Connections and Vote Coincidence - Rapporteurs' and National Party's Influence

	(1) OLS Agree	(2) OLS Agree	(3) 2SLS Agree
Name Peers IG	0.0066	0.0059	
	(0.0049)	(0.0050)	
Name Peers (IG * Relevant)		0.0073*	
		(0.0039)	
Peers IG			0.0080
			(0.0066)
Peers (IG * Relevant)			0.0091*
			(0.0049)
Rapporteur	0.0766***	0.0765***	0.0765***
	(0.0132)	(0.0132)	(0.0132)
Shadow Rapporteur	0.0305***	0.0305***	0.0307***
	(0.0085)	(0.0085)	(0.0085)
Peer Rapporteur	0.0832***	0.0830***	0.0830***
	(0.0184)	(0.0184)	(0.0184)
Peer Shadow Rapporteur	0.0304**	0.0301**	0.0301**
	(0.0123)	(0.0123)	(0.0123)
Same National party	0.0392*	0.0392*	0.0395*
	(0.0210)	(0.0210)	(0.0207)
EPG x Term FEs	Yes	Yes	Yes
Sessions since term started FEs	Yes	Yes	Yes
Procedure type FEs	Yes	Yes	Yes
Vote subject FEs	Yes	Yes	Yes
Name controls	Yes	Yes	Yes
Focal MEP controls	Yes	Yes	Yes
Peers controls	Yes	Yes	Yes
Observations	6,770,336	6,770,336	6,770,336
Mean Agree	0.707	0.707	0.707
Joint p-value		0.0239	0.0257
F-stat 1			1056
F-stat 2			1308

Notes: Results of estimating Equation (2.2). It is analogous to the Columns 5, 6, and 7, in Table 2.3, respectively. Joint p-value of a test on the joint significance of the adjacency to a legislator with background in an interest group, and when the topic is relevant for such interest group. A comprehensive set of controls of the focal and peer legislators is used, see Appendix 2.C for further information on the controls included. Standard errors, in parenthesis, are clustered at the legislator level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A5: Stage Estimates of Name Adjacency on Seating Adjacency

	(1) OLS Peers IG	(2) OLS Peers (IG × Relevant)
Name Peers IG	0.7507***	-0.0083***
	(0.0164)	(0.0020)
Name Peers (IG × Relevant)	0.0020	0.8007***
	(0.0051)	(0.0157)
EPG × Term FEs	Yes	Yes
Sessions since term started FEs	Yes	Yes
Procedure type FEs	Yes	Yes
Vote subject FEs	Yes	Yes
Name controls	Yes	Yes
Focal MEP controls	Yes	Yes
Peers controls	Yes	Yes
Observations	6,770,336	6,770,336

Notes: Estimates for the baseline first stage regressions. A comprehensive set of controls of the focal and peer legislators is used, see Appendix 2.C for further information on the controls included. Standard errors, in parenthesis, are clustered at the legislator level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A6: Reverse Revolving Doors Connections and Vote Coincidence by Name Distance

	(1) OLS Agree	(2) OLS Agree	(3) OLS Agree	(4) OLS Agree	(5) OLS Agree
Name Peers IG dist. 1	0.0058	0.0051	0.0049	0.0041	0.0039
	(0.0049)	(0.0048)	(0.0048)	(0.0047)	(0.0047)
Name Peers IG × Relevant dist. 1	0.0071*	0.0071*	0.0071*	0.0073*	0.0073*
	(0.0039)	(0.0039)	(0.0039)	(0.0039)	(0.0039)
Name Peers IG dist. 2	0.0027	0.0025	0.0013	0.0005	-0.0001
	(0.0047)	(0.0046)	(0.0047)	(0.0046)	(0.0046)
Name Peers IG × Relevant dist. 2	0.0078**	0.0073*	0.0072*	0.0072*	0.0076**
	(0.0039)	(0.0039)	(0.0039)	(0.0039)	(0.0038)
Name Peers IG dist. 3		0.0050	0.0055	0.0041	0.0033
		(0.0042)	(0.0042)	(0.0042)	(0.0042)
Name Peers IG × Relevant dist. 3		0.0076**	0.0068*	0.0065*	0.0067*
		(0.0036)	(0.0036)	(0.0036)	(0.0036)
Name Peers IG dist. 4			-0.0001	-0.0005	-0.0011
			(0.0050)	(0.0050)	(0.0050)
Name Peers IG × Relevant dist. 4			0.0073*	0.0077*	0.0078*
			(0.0042)	(0.0042)	(0.0041)
Name Peers IG dist. 5				0.0019	0.0014
				(0.0040)	(0.0040)
Name Peers IG × Relevant dist. 5				0.0017	0.0014
				(0.0037)	(0.0037)
Name Peers IG dist. 6					0.0002
					(0.0038)
Name Peers IG × Relevant dist. 6					0.0037
					(0.0038)
EPG × Term FEs	Yes	Yes	Yes	Yes	Yes
Sessions since term started FEs	Yes	Yes	Yes	Yes	Yes
Procedure type FEs	Yes	Yes	Yes	Yes	Yes
Vote subject FEs	Yes	Yes	Yes	Yes	Yes
Observations	6,767,838	6,742,171	6,718,746	6,704,043	6,724,801
Mean Agree	0.707	0.707	0.706	0.706	0.705
p-value, all coef. = zero	0.0202	0.0108	0.00671	0.0116	0.0129
p-value, coef. dist. 1 = dist. 2	0.764	0.770	0.663	0.642	0.641
p-value, coef. dist. 1 = dist. 3	-	0.957	0.980	0.909	0.867
p-value, coef. dist. 1 = dist. 4	-	-	0.603	0.645	0.620
p-value, coef. dist. 1 = dist. 5	-	-	-	0.302	0.261
p-value, coef. dist. 1 = dist. 6	-	-	-	-	0.317

Notes: Results of estimating how name adjacency to legislators with interest group background affect their probability of voting alike at different distance levels. Joint p-value of a test on the joint significance of the adjacency to a legislator with background in an interest group, and when the topic is relevant for such interest group. A comprehensive set of controls of the focal and peer legislators is used, see Appendix 2.C for further information on the controls included. Standard errors, in parenthesis, are clustered at the legislator level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A7: Reverse Revolving Doors Connections and Vote Coincidence - Row-Level Analysis

	(1) OLS Agree	(2) OLS Agree	(3) OLS Agree	(4) OLS Agree
Num. IG members	0.0835**	0.0509**	0.0511**	0.0396
	(0.0339)	(0.0225)	(0.0227)	(0.0243)
Num. IG members × Relevant				0.0737***
				(0.0209)
EPG × Term FEs	No	Yes	Yes	Yes
Sessions since term started FEs	No	Yes	Yes	Yes
Procedure type FEs	No	No	Yes	Yes
Vote subject FEs	No	No	Yes	Yes
MEP controls	No	No	No	Yes
Observations	638,461	638,455	638,455	638,455
Mean Agree	0.704	0.704	0.704	0.704
Joint p-value				0.000249

Notes: Results of estimating Equation (2.2) collapsed at the row by aisle level. It tests whether the presence of more legislators with interest group background in a given chamber row affects the row voting agreement. Joint p-value of a test on the joint significance of the adjacency to a legislator with background in an interest group, and when the topic is relevant for such interest group. A comprehensive set of controls of the focal and peer legislators collapsed at the row level is used, see Appendix 2.C for further information on the controls included. Standard errors, in parenthesis, are clustered at the plenary session times the row-by-aisle level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A8: Average Effect of Reverse Revolving Doors Connections on Vote Coincidence using Different Clustering Levels

	(1) OLS Agree	(2) OLS Agree	(3) OLS Agree	(4) OLS Agree
Name Peers IG	0.0059	0.0059	0.0059	0.0059*
	(0.0050)	(0.0050)	(0.0047)	(0.0034)
Name Peers (IG * Relevant)	0.0073*	0.0074*	0.0073*	0.0073**
	(0.0039)	(0.0042)	(0.0041)	(0.0035)
EPG x Term FEs	Yes	Yes	Yes	Yes
Sessions since term started FEs	Yes	Yes	Yes	Yes
Procedure type FEs	Yes	Yes	Yes	Yes
Vote subject FEs	Yes	Yes	Yes	Yes
Name controls	Yes	Yes	Yes	Yes
Focal MEP controls	Yes	Yes	Yes	Yes
Peers controls	Yes	Yes	Yes	Yes
Observations	6,770,336	6,770,336	6,770,336	6,770,336
Mean of Dependent Var.	0.707	0.707	0.707	0.707
Joint p-value	0.0239	0.0453	0.0360	0.00602

Notes: This table shows the results of estimating Equation (2.2) using different clustering levels. All columns mimic Column 6 in Table 2.3, with differences in the clustering level, i) Column 1 clusters at the legislator level, ii) Column 2 clusters at the legislator and plenary session levels, iii) Column 3 clusters at the row and plenary session level, and iv) Column 4 clusters at the EPG and plenary session level. We denote as Joint p-value the test on the joint significance of the name adjacency to a legislator with previous interest group, and when the topic is relevant for such interest group. A comprehensive set of controls at the focal and peer legislators is used in the analysis. See Appendix 2.C for further information on the controls included. Standard errors are clustered at legislator level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A9: Reverse Revolving Doors Connections and Vote Coincidence with Multiple Topics of Interest

	(1) OLS Agree	(2) OLS Agree	(3) OLS Agree	(4) OLS Agree	(5) OLS Agree	(6) OLS Agree	(7) 2SLS Agree
Name Peers IG	0.0350***	0.0207***	0.0206***	0.0126**	0.0066	0.0056	
	(0.0076)	(0.0067)	(0.0067)	(0.0053)	(0.0049)	(0.0050)	
Name Peers (IG × Relevant)						0.0049*	
						(0.0029)	
Peers IG							0.0076
							(0.0066)
Peers (IG × Relevant)							0.0061*
							(0.0036)
EPG × Term FEs	No	Yes	Yes	Yes	Yes	Yes	Yes
Sessions since term started FEs	No	Yes	Yes	Yes	Yes	Yes	Yes
Procedure type FEs	No	No	Yes	Yes	Yes	Yes	Yes
Vote subject FEs	No	No	Yes	Yes	Yes	Yes	Yes
Name controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Focal MEP controls	No	No	No	Yes	Yes	Yes	Yes
Peers controls	No	No	No	No	Yes	Yes	Yes
Observations	6,770,336	6,770,336	6,770,336	6,770,336	6,770,336	6,770,336	6,770,336
Mean Agree	0.707	0.707	0.707	0.707	0.707	0.707	0.707
Joint p-value						0.0504	0.0540
F-stat 1							1052
F-stat 2							2023

Notes: Results of estimating Equation (2.2). Joint p-value of a test on the joint significance of the adjacency to a legislator with background in an interest group, and when the topic is relevant for such interest group. A comprehensive set of controls of the focal and peer legislators is used, see Appendix 2.C for further information on the controls included. Standard errors, in parenthesis, are clustered at the legislator level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A10: Reverse Revolving Doors Connections and Vote Coincidence in Cross-EPG

	(1) OLS Agree	(2) OLS Agree	(3) OLS Agree	(4) OLS Agree	(5) OLS Agree	(6) OLS Agree
Peer IG	-0.0005	-0.0021	-0.0013	0.0022	0.0006	0.0005
	(0.0118)	(0.0088)	(0.0088)	(0.0075)	(0.0077)	(0.0077)
Peer (IG × Relevant)						0.0010
						(0.0130)
EPG x Term FEs	No	Yes	Yes	Yes	Yes	Yes
Sessions since term started FEs	No	Yes	Yes	Yes	Yes	Yes
Procedure type FEs	No	No	Yes	Yes	Yes	Yes
Vote subject FEs	No	No	Yes	Yes	Yes	Yes
Name controls	No	Yes	Yes	Yes	Yes	Yes
Focal MEP controls	No	No	No	Yes	Yes	Yes
Peers controls	No	No	No	No	Yes	Yes
Observations	582,833	582,833	582,833	582,833	582,833	582,833
Mean Agree	0.654	0.654	0.654	0.654	0.654	0.654
Joint p-value						0.916

Notes: Results of estimating Equation (2.2) using only those legislators with adjacent colleagues from a different European group. *Peer IG* takes a value of 1 if the peer who was part of an interest group is from a different party, and a value of 0 if no peer was part of an interest group. Joint p-value of a test on the joint significance of the adjacency to a legislator with background in an interest group, and when the topic is relevant for such interest group. A comprehensive set of controls of the focal and peer legislators is used, see Appendix 2.C for further information on the controls included. Standard errors, in parenthesis, are clustered at the legislator level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A11: Average Effect of Reverse Revolving Doors Connections on Vote Coincidence Persistence by Voting Days

	(1) OLS Agree	(2) OLS Agree	(3) OLS Agree	(4) 2SLS Agree
Name Peers IG	0.0060	0.0046	0.0037	
	(0.0050)	(0.0068)	(0.0068)	
Name Peers (IG * Relevant)	0.0073*	0.0073*	0.0164**	
	(0.0039)	(0.0039)	(0.0065)	
Vote days name adjacent	-0.0000	-0.0000	-0.0000	
	(0.0000)	(0.0000)	(0.0000)	
Name Peers IG * Vote days name adjacent		0.0000	0.0000	
		(0.0001)	(0.0001)	
Name Peers (IG * Relevant) * Vote days name adjacent			-0.0001	
			(0.0001)	
Peers IG				0.0052
				(0.0093)
Peers (IG * Relevant)				0.0225**
				(0.0089)
Vote days seat adjacent				-0.0000
				(0.0001)
Peers IG * Vote days seat adjacent				0.0001
				(0.0001)
Peers (IG * Relevant) * Vote days seat adjacent				-0.0002*
				(0.0001)
EPG x Term FEs	Yes	Yes	Yes	Yes
Sessions since term started FEs	Yes	Yes	Yes	Yes
Procedure type FEs	Yes	Yes	Yes	Yes
Vote subject FEs	Yes	Yes	Yes	Yes
Name controls	Yes	Yes	Yes	Yes
Focal MEP controls	Yes	Yes	Yes	Yes
Peers controls	Yes	Yes	Yes	Yes
Observations	6,770,336	6,770,336	6,770,336	6,770,336
Mean Agree	0.707	0.707	0.707	0.707
Joint p-value		0.125	0.0308	0.0306
F-stat (KP)				172

Notes: This table shows the results of estimating Equation (2.2) adding as regressors the number of previous voting days in which each legislator has been assigned to sit adjacent to the same two other legislators, as well as the interactions with *Peers IG* and *Peers IG* $*$ *Relevant*, and their correspondent instruments. We denote as joint p-value the test on the joint significance of all the variables displayed in the table (both at the surname and seating level). A comprehensive set of controls at the focal and peer legislators is used in the analysis. See Appendix 2.C for further information on the controls included. The reported F Statistics has been calculated following Kleibergen and Paap (2006). Standard errors are clustered at legislator level. *** p<0.01, ** p<0.05, * p<0.1.

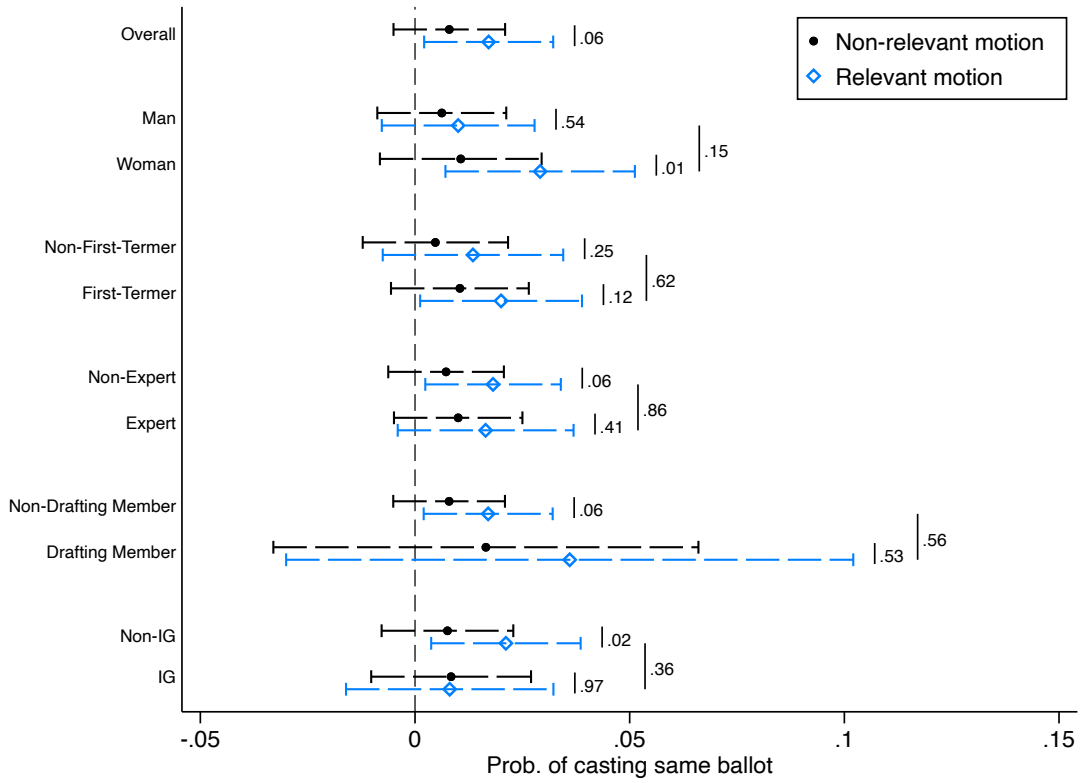
Table 2.A12: Average Effect of Reverse Revolving doors Connections on Vote Coincidence Persistence by Plenary Sessions

	(1) OLS Agree	(2) OLS Agree	(3) OLS Agree	(4) 2SLS Agree
Name Peers IG	0.0059	0.0043	0.0034	
	(0.0050)	(0.0069)	(0.0068)	
Name Peers (IG * Relevant)	0.0073*	0.0073*	0.0164**	
	(0.0039)	(0.0039)	(0.0064)	
Sessions name adjacent	-0.0000	-0.0000	-0.0000	
	(0.0001)	(0.0001)	(0.0001)	
Name Peers IG * Sessions name adjacent		0.0001	0.0001	
		(0.0002)	(0.0002)	
Name Peers (IG * Relevant) * Sessions name adjacent			-0.0003	
			(0.0002)	
Peers IG				0.0048
				(0.0094)
Peers (IG * Relevant)				0.0225**
				(0.0089)
Sessions seat adjacent				-0.0001
				(0.0002)
Peers IG * Sessions seat adjacent				0.0002
				(0.0004)
Peers (IG * Relevant) * Sessions seat adjacent				-0.0006*
				(0.0004)
EPG x Term FEs	Yes	Yes	Yes	Yes
Sessions since term started FEs	Yes	Yes	Yes	Yes
Procedure type FEs	Yes	Yes	Yes	Yes
Vote subject FEs	Yes	Yes	Yes	Yes
Name controls	Yes	Yes	Yes	Yes
Focal MEP controls	Yes	Yes	Yes	Yes
Peers controls	Yes	Yes	Yes	Yes
Observations	6,770,336	6,770,336	6,770,336	6,770,336
Mean Agree	0.707	0.707	0.707	0.707
Joint p-value		0.131	0.0322	0.0322
F-stat (KP)				188

Notes: This table shows the results of estimating Equation (2.2) adding as regressors the number of previous plenary sessions in which each legislator has been assigned to sit adjacent to the same two other legislators, as well as the interactions with *Peers IG* and *Peers IG* $*$ *Relevant*, and their correspondent instruments. We denote as Joint p-value the test on the joint significance of all the variables displayed in the table (both at the surname and seating level). A comprehensive set of controls at the focal and peer legislators is used in the analysis. See Appendix 2.C for further information on the controls included. The reported F Statistics has been calculated following Kleibergen and Paap (2006). Standard errors are clustered at legislator level. *** p<0.01, ** p<0.05, * p<0.1.

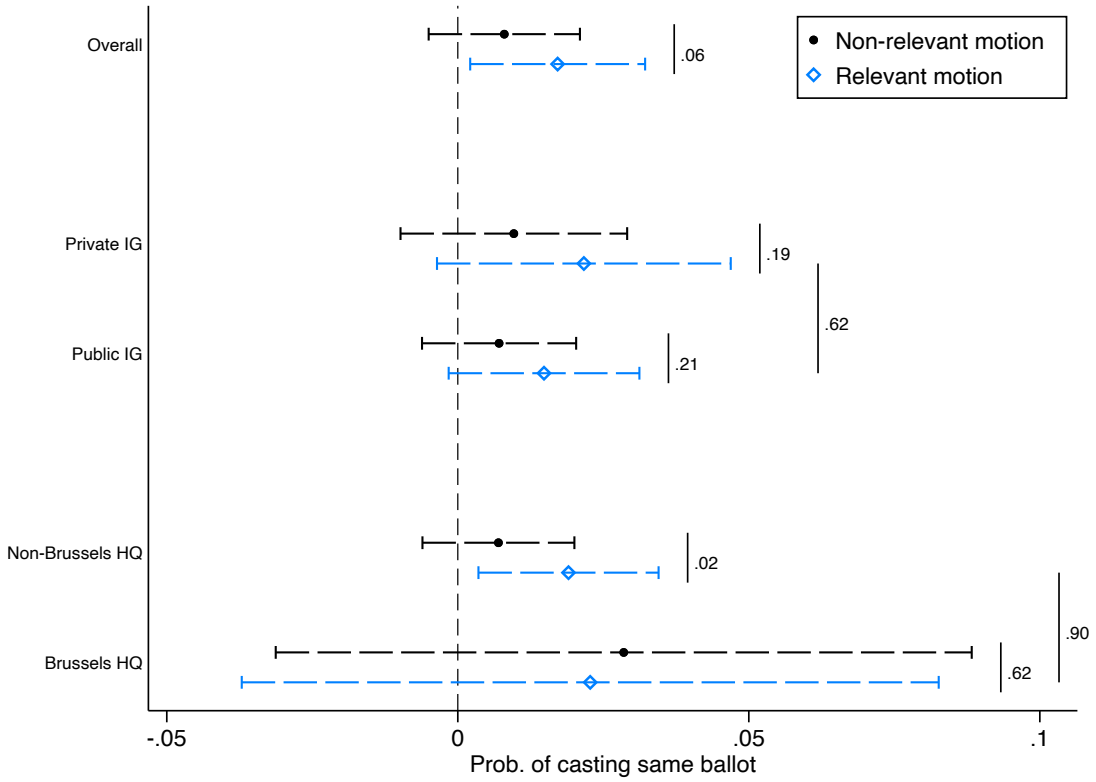
2.B Figures

Figure 2.B1: Reverse Revolving Doors and Vote Coincidence by Personal Characteristics



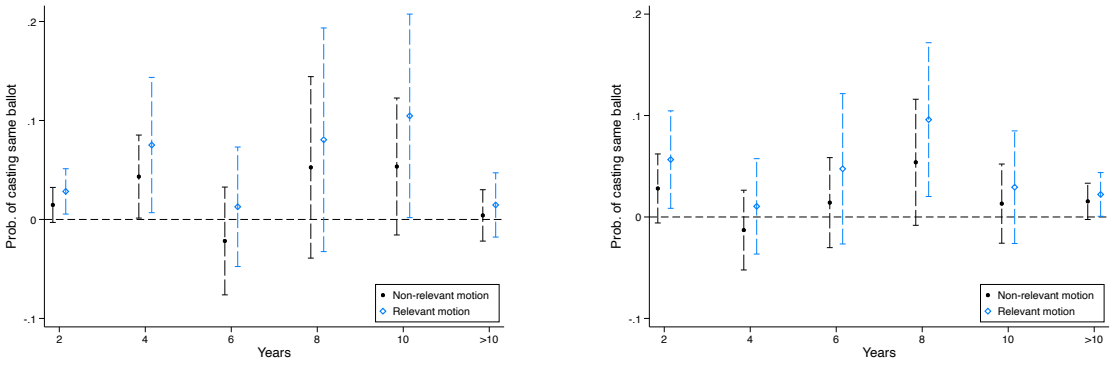
Notes: This figure shows the results of estimating Equation (2.2), interacted with the legislators' personal characteristics. The results shown correspond to the effect of seating adjacently to a legislator who previously worked for an interest group, when the subject of the motion is not relevant for its former employer, *Non-Relevant motion*, and when it is, *Relevant motion*. A comprehensive set of controls for the focal and peer legislators is used in the analysis. See Appendix 2.C for further information on the included controls. Standard errors are clustered at the legislator level. Confidence intervals represent the 95% confidence level. p-values from Wald tests for the equality of two estimates are reported next to each solid vertical line between the two estimates.

Figure 2.B2: Reverse Revolving Doors and Vote Coincidence by Interest Groups' Characteristics



Notes: This figure shows the results of estimating Equation (2.2), interacted with the Interest Group's characteristics. The results shown correspond to the effect of seating adjacently to a legislator who previously worked for an interest group, when the topic is not relevant for its former employer, *Non-Relevant motion*, and when the topic is relevant for its former employer, *Relevant motion*. A comprehensive set of controls at the focal and peer legislators is used in the analysis. See Appendix 2.C for further information on the controls included. Standard errors are clustered at the legislator level. Confidence intervals represent the 95% confidence level. p-values from Wald tests for the equality of two estimates are reported next to each solid vertical line between the two estimates.

Figure 2.B3: Temporal Distribution of Reverse Revolving Doors and Vote Coincidence



(a) Last employed by an interest group

(b) Experience in an interest group

Notes: This figure shows the results of estimating Equation (2.2) showing the results depending on the years since the employment of the legislators with an interest group background ended and their years of experience. Subfigure 2.B3a studies how this influence evolves vis-à-vis their adjacent peers' years since they last worked for an interest group. Subfigure 2.B3b focuses on how the effect depends on the years of experience adjacent legislators had in interest groups. The results shown correspond to the effect of seating adjacently to a legislator who previously worked for an interest group, when the topic is not relevant for its former employer, *Non-Relevant motion*, and when the topic is relevant for its former employer, *Relevant motion*. A comprehensive set of controls at the focal and peer legislators is used in the analysis. See Appendix 2.C for further information on the controls included. Standard errors are clustered at the legislator level. Confidence intervals represent the 95% confidence level.

2.C Description of Controls Used for Focal and Peer Legislators

This section presents the variables used as control in our main analysis, both for focal and peer legislators. We classify them into *Name controls*, *Focal MEP controls* and *Peers controls*.

i) *Name controls*: Owing to the possibility that surnames may represent the individuals, observable and unobservable, characteristics, such as socioeconomic background or family ties, in the spirit of Harmon et al. (2019), we control by the fraction of focal and individuals in the same group of peers sharing the same surname, and by the absolute alphabetic rank across EPGs and terms.

ii) *Focal MEP controls*: We characterize legislators using a wide set of controls. As for the legislators' personal characteristics, we control for their age, gender, national party, country of origin and whether they attended a top 500 university. As for the legislators' professional characteristics, we control for their years of professional experience before entering parliament, the total number of working positions, whether they have a managerial profile, whether their professional experience was conducted in the public, private, or academic sector, and their number of professional spells. We also control their topics of expertise, measured using Yordanova (2009)'s classification, and the number of those topics, as well as whether they previously worked for an interest group and if the topic is relevant for their previous employers. Regarding their previous interest groups' characteristics, we control by whether they have their headquarters in Brussels, and by their average reported EU lobbying budget. As for the legislator's in parliament characteristics, we control for their freshman status, their share of previous dates absent, their role at their EPG, whether they are part of the alphabetically seated leader sector in ALDE, whether they are the rapporteur or shadow rapporteur in the specific procedure voted, whether their EPG had one of these figures, whether the procedure refers to their own country, and whether they were at the responsible and opinion committees of the procedure voted on. We further control by whether the motion voted upon was a final vote or an amendment.

iii) *Peers controls*: We characterize connections, i.e., adjacent (left and right) siting peers, by expanding the above mentioned variables. We include as controls the fraction of the adjacent peers in the same EPG as the focal, the fraction in the same national party as the focal, the fraction from the same country as the focal, the fraction with the same EPG role as the focal, the fraction with the same profession profile as the focal, the fraction with the same managerial profile as the focal, the

fraction with the same freshman status as the focal, the fraction with the same gender as the focal, the fraction having the same “Top 500" education as the focal, and the fraction of the peers in the same committee as the focal. We also use peer controls that are irrespective of the focal characteristics such as the fraction of peers with freshman status, the fraction of female peers, the fraction of peers with a Top 500 education, the fraction of peers with a managerial profile, the fraction of rapporteur and shadow rapporteur peers, the fraction of peers in the committee responsible or committee of opinion for the procedure voted on, the fraction of peers with expertise in the topics voted on, the fraction of the peers for which the procedure voted on is of national relevance, the number of peers (from 1 to 2), the average absenteeism rate of the peers, the average number of topics of expertise of the peers, as well as, the fraction of peers with an interest group based in Brussels, and the average EU lobbying budget of these interest groups. Additionally, using information from peers and focal legislators, we control for the standard deviation in their age, professional experience, number of positions at the European Parliament, number of working positions, number of topics of expertise, and absenteeism rate.

Table 2.C1: Summary Statistics

	Mean	SD	Min	Max	N
Agree	0.71	0.38	0	1	6770336
Absention	0.02	0.14	0	1	6770336
Lobbyist Legislator	0.28	0.45	0	1	6770336
Ratio Relevant Topic (not political) (main)	0.01	0.07	0	1	6770336
Peers IG	0.28	0.33	0	1	6770336
Peers (IG * Relevant)	0.03	0.16	0	1	6770336
Name Peers IG	0.28	0.33	0	1	6770336
Name Peers (IG * Relevant)	0.03	0.17	0	1	6770336
Final vote	0.23	0.42	0	1	6770336
Expertise	0.28	0.45	0	1	6770336
Age	53.42	10.68	26	86	6770336
Rapporteur	0.00	0.04	0	1	6770336
Shadow Rapporteur	0.00	0.06	0	1	6770336
Part of the responsible committee	0.01	0.08	0	1	6770336
Part of the opinion committee	0.00	0.07	0	1	6770336
National law	0.00	0.01	0	1	6770336
National party	241.45	129.08	2	453	6770336
Country	16.07	7.85	1	28	6770336
EPG Role	4.87	0.50	2	5	6770336
Female	0.37	0.48	0	1	6770336
Part of the ALDE leader section	0.05	0.22	0	1	6770336
Freshman status	0.58	0.49	0	1	6770336
Number of professional positions	4.95	1.24	0	12	6770336
Rapporteur in the EPG	0.70	0.46	0	1	6770336
Top 500 education	0.31	0.46	0	1	6770336
Previous sector of activity	1.34	0.54	1	3	6770336
Professional experience	24.68	10.97	1	56	6770336
Managerial profile	0.27	0.45	0	1	6770336
Number of working spells	12.19	9.84	1	87	6770336
Share previous days absent	0.13	0.11	0	1	6770336
IG - Brussels HQ	0.05	0.20	0	1	6770336
IG - EU Lobbying budget	127203.57	447452.89	0	5002500	6770336
Number of expertise topics	11.01	5.95	0	31	6770336
National law (peers)	0.00	0.01	0	1	6770336
Freshman (peers)	0.58	0.37	0	1	6770336
Female (peers)	0.37	0.36	0	1	6770336
Managerial profile (peers)	0.27	0.33	0	1	6770336
Top 500 education (peers)	0.31	0.34	0	1	6770336
Rapporteur (peers)	0.00	0.03	0	1	6770336
Shadow Rapporteur (peers)	0.00	0.04	0	1	6770336
Part of the responsible committee (peers)	0.01	0.06	0	1	6770336
Part of the opinion committee (peers)	0.00	0.05	0	1	6770336
Number of peers	1.91	0.29	1	2	6770336
Expertise (peers)	0.28	0.36	0	1	6770336
Share previous days absent (peers)	0.13	0.08	0	1	6770336
IG - Brussels HQ (peers)	0.04	0.14	0	1	6770336
IG - EU Lobbying budget (peers)	129014.55	335746.82	0	5002500	6770336
Number of expertise topics (peers)	11.03	4.42	0	31	6770336
Same gender (peers)	0.53	0.38	0	1	6770336
Same EPG (peers)	0.96	0.14	0	1	6770336
Same national party (peers)	0.08	0.21	0	1	6770336
Same country (peers)	0.10	0.23	0	1	6770336
Same EPG role (peers)	0.93	0.21	0	1	6770336
Same freshman status (peers)	0.51	0.38	0	1	6770336
Same previous sector of activity (peers)	0.57	0.40	0	1	6770336
Same managerial profile (peers)	0.61	0.38	0	1	6770336
Same Top 500 education (peers)	0.57	0.39	0	1	6770336
Same position at the same committee (peers)	0.20	0.30	0	1	6770336
Age SD (peers)	9.43	4.98	0	34	6770336
Professional experience SD (peers)	9.73	5.14	0	33	6770336
Number of professional positions SD (peers)	1.03	0.65	0	6	6770336
Share previous days absent SD (peers)	0.08	0.06	0	1	6770336
Number of working spells SD (peers)	7.39	6.42	0	60	6770336
Number of Expertise Topics SD (peers)	5.29	2.81	0	20	6770336

Notes: Mean, standard deviation, minimum and maximum value for every variable used in the baseline regression. For further information, see Appendix 2.C.

Chapter 3

Time Constraints and the Quality of Physician Care

Solo-authored[1]

Abstract This paper explores how reviewing time affects physicians' medical decisions. Insufficient examination time may hamper physicians' care and diagnostic provision, leaving physicians more inclined to over-prescribe medication. I test this prediction using high-frequency data from a Spanish outpatient department and leverage on-the-day cancellations as exogenous time shocks. I find that longer visits lead to more valuable care, measured by the provision of more detailed diagnoses, to higher testing intensity, and to lower drug prescriptions. These effects are driven by junior physicians, who use this extra time to compensate for their more overloaded shifts.

3.1 Introduction

Working under time pressure has become a hallmark of today's economy. According to a survey conducted by Eurofound (2017), 36% of the workers in the European Union work under tight deadlines, while 10% report needing more time to complete their tasks.[2] Time pressure is most critical for the healthcare industry, where accurate and timely decision-making might prevent long-lasting social costs. However, 14% of healthcare workers report

[1]I wish to thank Sule Alan, Josep Amer-Mestre, Michèle Belot, Elisa Failache, Christian Fons-Rosen, Ezra Golberstein, Annika Herr, Andrea Ichino, Maria Ptashkina, Ity Shurtz, and Ana Tur-Prats, together with seminar participants at EuHEA PhD conference 2021, WIPE 2022, COPE 2022, SOLE 2022, XV RIDGE Forum, SMYE 2022, ESPE 2022, EALE 2022, Universitat Pompeu Fabra, and EUI, for their helpful comments. A special thanks to the outpatient department for their support throughout the process. Previously circulated as "Time Constraints and Productivity in Health Care". Financial support from the Salvador Madariaga-EUI scholarship is gratefully acknowledged. All errors remain my own.

[2]By comparison, in 1991, only 23% of the workers in the European Union worked under tight deadlines (Eurofound, 1993).

not having sufficient time to do their job correctly while also being the sector most affected by high emotional demands. In this context, learning how to manage existing resources, such as human capital, could have sizable welfare-improving implications.

In this paper, I investigate how the time physicians spend reviewing patients causally affects the quality of the physician care and the treatment provided, using detailed high-frequency information from a Spanish outpatient department. I focus on the provision of a detailed diagnosis as a proxy for a visit's successful completion, given that outpatient physicians' job is to provide clear-cut advice to those patients referred from Primary Care Centers. I also examine several other dimensions of physicians' productivity, including the number of tests ordered and their corresponding cost, the number of drugs prescribed, whether patients have subsequent visits, and the likelihood that either patients or physicians cancel those visits.

The main empirical challenge to estimating the causal effect of visit length on physicians' decisions is to obtain a relevant source of time that is also exogenous to the patient's characteristics. That is essential as physicians have a complete picture of their shift in real-time, allowing them to adjust to sudden changes and provide more extended visits based on patients' characteristics. On the one hand, physicians may decide to spend more time with those patients with more complicated conditions, allowing them to assess them better. On the other hand, physicians may provide patients with diagnostic inputs as a substitute for the extra needed time. I address that challenge by leveraging on-the-day cancellations as random time shocks to the physicians' schedules. When a cancellation occurs, physicians generally spend more time with all the visits for the remainder of the shift but also provide the very next scheduled visit with an unexpected extra visit length. I focus on such *bonus* time to extract conclusions on how physicians' diagnostic behavior responds to an unexpected increase in consultation time. On-the-day cancellations represent 15% of all visits.

A second obstacle that might hinder our causal estimation is the physicians' prioritization of patients with specific characteristics once a cancellation occurs. While physicians have to follow their daily schedule by law, in practice, they might select which patients to treat when a slot is freed. To tackle that concern, I focus only on first visits to the outpatient department, as new patients have no prior contact with their treating physician, minimizing such selection. Moreover, the Spanish outpatient system prevents patients from strategically responding to physician's cancellations by forbidding in-office dropouts and on-the-day appointments.

I build a unique dataset containing the universe of visits to a Spanish outpatient department between 2016 and 2018 and complement it with high-frequency information on the physician's schedules and the treatments and diagnoses provided. The main specification uses an IV approach, instrumenting the time allocated to review each patient with

whether the prior scheduled visit got canceled. I include physician fixed effects to control for inherent physicians' characteristics, and by construction, by those of their specializations; month-by-year fixed effects to control for seasonality confounders; hour fixed effects to account for different hour-patient compositions; and a comprehensive set of controls for patients' characteristics.

I find that longer visits increase the likelihood of providing a diagnosis, which is the main objective of outpatient departments. For every extra reviewing minute, the likelihood of providing a diagnosis increases by 4%. This effect is driven by uncommon diagnoses, while no effect is found on the most common diagnoses, suggesting that longer visits allow physicians to review patients in more detail and provide them with a higher-value service. Longer first visits also increase diagnostic input utilization in the form of procedures and laboratory tests, and decrease drug prescriptions. For every extra minute used to review patients, physicians provide 3% more tests, increasing the overall testing cost by 6%, and reduce the drug doses prescribed by 20%. These results suggest that test ordering, especially more expensive tests, complements longer visits, while drug prescription is used as a substitute for insufficient reviewing time. Overall, physicians use the extra visiting time to assess the patient's health problems in more detail, and in the event of indecision, to request further diagnostic inputs, ultimately improving the service provided.

I then look at how physicians' contracts influence diagnostic provision. These contracts, based on seniority, provide senior physicians with less overloaded shifts at the expense of their junior colleagues. I find that longer visits only lead to changes in the input composition and the provision of a diagnosis when such extra time is provided to junior physicians. In contrast, the extra time does not affect senior physicians. While junior and senior physicians react to cancellations by providing more time to their subsequent patients, only junior physicians use such *bonus* time to promote a service of higher quality. With these results in mind, I provide a back-of-the-envelope calculation for the direct labor cost of increasing diagnosis rates. Policymakers could attempt to improve diagnostic rates by increasing every physician's visiting times across the board. However, doing so might prove inefficient, as it does not internalize that senior physicians' practices are unaffected by longer visit lengths. A tailored approach targeting only junior physicians might help improve health provision while minimizing expenditure.

Understanding the trade-off between time and employee productivity is essential from a policy perspective. On the one hand, the provision of longer visits comes at the expense of fewer visits per shift and, in equilibrium, of long waiting lists to access the outpatient department. On the other hand, longer visits lead to higher visit quality, improving patients' health outcomes and reducing their need for readmission. To the best of my knowledge, this paper is the first to provide causal evidence that longer reviewing time

improves visit quality and show that correcting the distortionary incentives created by seniority-based contracts may be welfare-improving.

This paper contributes to different strands of the literature. First, it complements the growing literature on the determinants of physicians' labor supply. Recent literature has looked at the role of financial incentives (Powell-Jackson et al., 2015; Gupta, 2021), co-working (Chan, 2016), peer pressure (Silver, 2021), and scheduling (Chan, 2018).

More specifically, this paper complements the recent literature studying the workload-quality trade-off in the healthcare sector.[3] Mixed evidence has been found on how workload affects physicians' decisions. Shurtz et al. (2022) evaluates how physicians' decisions depend on their daily workload and finds that physicians provide higher diagnostic inputs and lower drug prescriptions on high-workload days. Neprash (2016) finds that when physicians fall behind schedule, they spend less time with their subsequent visits, order fewer procedures, and provide fewer diagnoses. Freedman et al. (2021) investigates how primary care providers react to moments of high time pressure induced by cancellations and add-ins, finding such pressure pushes physicians to provide fewer diagnostic inputs, more follow-up care, and lower referral rates. This paper is the first one to causally study how physicians' direct response to longer reviewing time, as opposed to indirect measures of workload or time pressure, affect the quality of their care and the treatments provided.

Second, this work relates to the literature on the impact of time pressure on output quality, which harks back to Tversky and Kahneman (1974). Some recent experiments have provided compelling evidence in favor of the argument that greater time pressure increases risk-taking behaviors (Kirchler et al., 2017; Essl and Jaussi, 2017; El Haji et al., 2019), and leads to more misjudgements (Suri and Monroe, 2003; Cao et al., 2022), especially among female participants (De Paola and Gioia, 2016). Frakes and Wasserman (2017, 2020) estimate the causal relationship between the time allocated to review patents and the examiners' effort, showing that lower examination time leads to reductions in examination scrutiny and to granting patents of weaker-than-average quality. This paper contributes to this literature by causally estimating the relationship between reviewing time and physicians' performance in a setting in which physicians work as single units in a single-stage process. Furthermore, this paper looks at the incentives at play, highlighting that seniority-based contracts might lead to inefficient time-to-input utilization.

The remainder of the paper is organized as follows: Section 3.2 explains the institutional setting. In Section 3.3, I present and describe the data used. Section 3.4 exposes the empirical strategy followed. Section 3.5 presents the main results, and Section 3.6 provides a quantification exercise. Finally, Section 3.7 concludes.

[3]This issue has also been studied in the service industry (Tan and Netessine, 2014; Bruggen, 2015), banking industry (Xu et al., 2022), and justice system (Coviello et al., 2015), among others.

3.2 Institutional Setting

3.2.1 Spanish Healthcare System

In Spain, healthcare is universal and free of charge. Its provision is structured around two main actors, Primary Care Centers and Specialized Care Centers, which together form Basic Health Zones (hereafter, BHZ). A BHZ is an administrative unit containing several Primary Care Centers mapped into a Specialized Care Center. Individuals are sorted into different BHZs based on their place of residence. Specialized Care Centers cover multiple services, such as the intensive care unit, the emergency room, and the outpatient department, which are usually located in hospitals.

This study focuses on the outpatient department. Initial access to this department is solely decided by the patients' treating primary care center, which allocates them to outpatient physicians based on their availability upon analyzing the patients' health conditions. The referral notification from the primary care center to the outpatient department is provided to patients some days after a patient visits her general practitioner, including information on the appointment time and date and the physician's name. This implies that the individuals' place of residence fully determines their outpatient department of reference, disallowing walk-in visits and blocking patients from choosing among clinics. Moreover, the region in which the department of my sample operates, Catalonia, does not allow patients to choose their outpatient physician, minimizing any possible relationship between physicians and their patients before a first visit.

3.2.2 Hospital Management Flow

The hospital manages patients following a production-line approach. Upon arrival, patients register at the main counter, where the administration secretary gives them directions to their waiting room and electronically notifies the physician in charge of the patient's arrival. From the waiting room, the physician calls patients following the appointment schedule, keeping track of who is in the waiting room. After the visit is completed, if a follow-up visit is ordered, patients return to the main counter, selecting the date and time slot of the follow-up visit within the physician's date recommendations. Throughout this process, physicians have full access to real-time information on all patients' availability status and health conditions.

Figure 3.1 presents the type of agenda displayed to physicians. At any given time, a physician knows precisely those patients who have not yet shown up, those who have canceled their visits, and those who are already in the waiting room. Physicians are also presented with patient characteristics, such as the patient's name and residence. In our example, a given physician is looking at her schedule at 10:00 a.m. The physician has

already seen six patients, while one did not attend his 9:00 a.m. appointment. She has four more visits until the end of the shift, one of which was already canceled. Moreover, the physician is working ahead of time, as she has already completed the appointment scheduled to start at 10.00 a.m. Due to such a comprehensive information system, physicians have a complete picture of their shift, allowing them to react on the spot to changes such as cancellations.

Over the course of a shift, physicians are mandated to provide care to any patient with an appointment and update patients' medical records. When physicians experience a cancellation or finish a visit faster than expected, they use that extra time to catch up on their schedule and fulfill their updating obligations. Additionally, physicians are provided with non-scheduled time for breaks, including a lunch break. Furthermore, following their pre-booked appointment order, physicians must finish all their visits on their corresponding appointment date.

3.3 Data

3.3.1 Hospital Data

I use data from one Spanish medium-sized, contracted hospital covering a wide range of specializations in the metropolitan area of Barcelona. My dataset contains all the 67,530 first visits to outpatient physicians from January 1, 2016, through June 30, 2018, assigned to 86 physicians covering 19 different specializations. These physicians always operate within their specialization, giving clear-cut advice to patients referred from Primary Care Centers.

Physicians are also involved in other minor tasks not included in the present study, such as night shifts, surgery visits, and rehabilitations. This dataset consists of high-frequency visit times and medical treatment information. It includes information on the patient's time of arrival in the hospital, the visit appointment time, the referral date, and the visit starting and ending times. Visit length is measured according to the time that the patient's profile was opened and closed on the physician's terminal. These times are automatically recorded by the terminals used rather than being self-reported by physicians. Referral and appointment dates are also crucial for reconstructing the entire outpatient process, from the first visit to all subsequent follow-up visits. Outpatient processes are used to test whether and how longer first visits affect the overall outpatient process. The dataset also covers the treatments provided in each visit, such as imaging and laboratory tests, drugs prescribed, and the testing cost.[4]

[4]We cannot retrieve the drug costs as prescriptions are issued based on the drugs' active components. See Law 29/2006.

Table 3.1 provides descriptive statistics for the variables used in the analysis. For instance, the average patient is a middle-aged Spanish woman living in an area adjacent to the hospital and with public coverage. The average first visit takes 12 minutes, with an average waiting list of 30 days and an 8% likelihood of receiving a diagnosis.[5]

3.3.2 Shift Distortions - Cancellations

A standard work shift is from 9 a.m. to 1.30 p.m., with a structure and composition decided ex-ante on a yearly basis, being specific to the physician's specialization. Shifts are characterized by being fully booked (the average waiting time for a first visit is 30 days) and compulsory for the physician (i.e., the physician cannot prioritize or decline visits). The whole dataset available contains 347,277 scheduled visits, divided into first visits (24.43%), follow-up visits (50.10%), and external consultations (23.06%). Figure 3.2a shows how the encounters spread over the shift by visit type. The period from 9 a.m. to 1.30 p.m. is the busiest, with 86.54% of the visits being concentrated in that period. While the outpatient department uses the hospital facilities in the morning, they are used later in the day for rehabilitations and surgeries, which are not included in the study. First visits spread homogeneously along the shift.

Cancellations represent the main perturbations on the physicians' schedules, providing unexpected free time. I use only those that occur on the visit date, as those happening on a prior date are easily re-booked. In absolute terms, the whole dataset contains 54,057 cancellations, comprising visits withdrawn before their appointed slot (18%) and no-shows for a visit (82%). No patient walk-outs are found in the data. Figure 3.2b shows the share of cancellations over the shift using all the visits' appointment times at the 30-minute bin level. Visually, there is no clear pattern indicating clustered cancellation periods.

I focus on how prior cancellations affect subsequent first visits during a shift. Using the benchmark sample as in Table 3.1, Figure 3.2c shows that the number of cancellations before a given visit accumulates over the schedule, with higher variation in the evening shift due to the combination of newly arrived physicians with those who are continuing from the preceding morning shift. Figure 3.2d shows the evolution of the probability of having the previous visit canceled, exhibiting a higher incidence at the beginning of the morning and evening shifts. Hour fixed effects are used in the study to account for such variation in the propensity of receiving a shock.

Figure 3.3 exposes the distribution of first visits with respect to prior cancellations.

[5]The hospital managing our outpatient department is considered a high-performing center within the Catalan health system. To give a few examples, as of 2017, i) the reported patient satisfaction was 8.2 out of 10 in my outpatient department (7.5 in the region); ii) the probability of readmission within 30 days was 9.22% (9.81% in the region); and iii) the average waiting time to access a first visit was 41 days (121 days in the region). For those reasons, I consider the results presented in this study as a lower bound when compared to other outpatient departments.

Subfigure 3.3a presents the fraction of visits with no prior cancellation and the fraction with a prior cancellation at higher horizons. In total, 62% of all the first visits had at least one preceding visit canceled, and 16% had the prior visit dropped. Subfigure 3.3b presents how the average actual and expected visit lengths evolve with respect to the distance from a prior cancellation.[6] We can appreciate that i) the hospital structurally assigns more time-consuming visits to earlier slots, where there is a lower probability of having any prior cancellation; ii) when shocked with a cancellation, physicians spend more time on their next visit; and iii) for all distances, expected visit length is generally greater than the actual visit length, which shows that the outpatient department provides visits with insufficient time to compensate for overbooking and other administrative duties.

3.4 Empirical Strategy

In the first empirical exercise, I examine the extent to which medical treatments are influenced by the time spent on a visit using the following model:

$$Y_{i,j,s} = \beta_0 + \beta_1 Length_{i,j,s} + \theta T_s + \delta_j + \Psi X_{i,s} + \epsilon_{i,j,s} \tag{3.1}$$

where Y identifies a given visit outcome, as described in Section 3.4.1, for a patient i, a physician j, and a slot s. The key independent variable, *Length*, identifies how many minutes a physician j spends with patient i in a visit slot s. I control for patient characteristics, $X_{i,s}$, such as gender, age, nationality, insurance coverage, and district distance to the hospital. All regressions include i) physician fixed effects, δ_j, which allows us to account for time-invariant variation across physicians, and by construction, across specializations; ii) month-year fixed effects, T_s, which mitigate the fact that results are confounded by seasonality (e.g., periods in which patients are more prone to suffer from diseases, such as with the seasonal flu, may also lead them to miss their hospital visits more frequently); and iii) hour fixed effects, T_s, which accounts for different hour-patient compositions.

Estimating Equation 3.1 using OLS may result in biased estimates for several reasons. First, there could be omitted variables not captured by the rich set of controls and fixed effects. These confounding variables may correlate with our measure of visit length and with some unobserved components in the error term. For example, physicians' good/bad moods or health conditions may affect both visit lengths and medical treatments. Second, given that physicians have complete information on all their on-the-day visits, they may allocate visit lengths based on their current and future patient characteristics. Such

[6]Expected visit length is a measure provided by the outpatient department, which identifies how much time an average visit should take, based on the type of visit, specialization, and administrative duties involved.

anticipation may facilitate simultaneous causation between the time spent with patients and the treatment provided. On the one hand, physicians may decide to spend more time with those patients found to be more challenging, allowing them to assess better if further treatment is required. On the other hand, physicians may decide to provide patients with treatments as a substitute for the time spent. Such substitution decision is plausible as reviewing and testing physicians may differ. To tackle these concerns, I only use first visits, as patients accessing these initial visits have no knowledge of the physician's schedule, nor do physicians know these patients; and use cancellations as an exogenous variation on the physicians' disposable time.

I use prior cancellations to capture exogenous variations in physicians' available time. Those cancellations comprise all the on-the-day visit withdraws, including those prior to their appointment and the no-shows. I define a first visit to be affected by a cancellation if the visit preceding it was canceled using its real cancellation time. In practice, *Prior Cancel* is a dummy variable that takes value 1 if the previously scheduled visit was a no-show,[7] or if another visit, which is supposed to happen later in the same day, is canceled during the current visit. Using the exact cancellation time is important for the study, as physicians can smooth out cancellations for which they have been notified. I use this approach since it represents a lower bound of the impact a cancellation has on subsequent visits, taking as not treated any other first visit that is not immediately after a cancellation. Figure 3.4a displays how cancellations impact the length of subsequent visits. We can see that physicians utilize significantly more time in visits after a cancellation than before. The figure also highlights that the time used in those first visits right after a cancellation is significantly larger than any other first visit. Figure 3.4b shows that physicians cannot anticipate cancellations, changing their reviewing time accordingly. For these reasons, the present analysis defines a treated visit as a first visit immediately after a cancellation; all other first visits as not treated.[8]

The validity of the instrument hinges on various considerations. The first issue relates to the random assignment of cancellation times. Those patients dropping a visit do so without knowing the physician's schedule. However, visits could be more frequently canceled when certain patient characteristics, such as older patients or those with more chronic problems, are present. Moreover, physicians could also decide which patients to take after a visit gets canceled. Table 3.2 displays the covariate test on the patient characteristics and the shared physician-patient characteristics. Prior cancellation does not predict any patient characteristics used in the study, which are visible to physicians.

[7] A no-show is a visit for which its patient never showed up. In other words, I do not leverage on the *extra* visiting time provided by those *pending* patients who did not show up on time to their visits but showed up later in the shift.

[8] Table 3.A1 shows that prior cancellations lead to extra visiting time for all patients reviewed during the physician's shift. Prior cancellations do not lead to extra reviewing time when physicians work overtime.

More importantly, physicians do not select patients based on their shared characteristics, namely sex and age. This fact supports the claim that first visits are randomly affected by prior cancellations.[9]

A second issue pertains to any other utilization of the physician's extra disposable time created by a prior cancellation. Prior to the treated visit, physicians decide how fast to take the new patient, which, in turn, might reduce their working delay. In turn, the estimates presented would be biased if such a less-rushed environment directly affects medical treatment, not only via visit length, thus violating the exclusion restriction. This indirect path is mostly attenuated by the use of fixed effects at the hour and the physician level, as the visits with a prior cancellation are compared to adjacent visits with similar levels of time pressure. Nevertheless, it could still be the case that the allocation of such extra time affects the first visit after a cancellation significantly differently compared to those at different horizons. To test whether a less-rushed environment directly affects the outcomes of interest, I extend Equation 3.1 to include the variable *Delay*, which represents the difference between the visit start time and the visit appointment time. The average *Delay* in the sample is 16.2 minutes. Following Neprash (2016), I instrument the variable *Delay* using a dummy variable to indicate whether the preceding realized visit arrived late to her appointment time, *Prior Late*. The variable takes value 1 if the patient appointed before a given visit arrived at the outpatient department after her scheduled appointment. When patients arrive late to the outpatient department, physicians await them for some courtesy time, which might lead to higher delays suffered by the following patients.[10] Table 3.A4 evidences no clear link of *Delay* directly affecting visit outcomes. Moreover, when comparing the variable *Length* in Table 3.A4 to the main result provided in Table 3.3, we can see how including *Delay* does not affect the predictability of our variable of interest. For such reasons, I dismiss the premise that, in the context of this study, changes in time pressure, originating from sudden schedule changes, affect the outcomes of interest other than through the visit duration.

3.4.1 Outcomes of Interest

I use the previously detailed instrumental variable framework to study how physicians respond to extra time, examining a broad set of outcomes that can be classified into diagnosis provision and treatment choice.

Regarding diagnosis provision, I investigate whether longer visits are beneficial in

[9] As an exception, some specializations in the outpatient department allow their first-visit patients to choose their preferred slot at their corresponding Primary Care Centers. Using only those patients, Table 3.A2 shows that having a prior cancellation is not predictive of either those patients' characteristics or the shared physician-patient characteristics.

[10] Table 3.A3 tests whether the instruments *Prior Cancel* and *Prior Late* predict observable patient characteristics, finding no systematic evidence.

assessing patients' diagnoses. Given that the outpatient department's main objective is to provide a correct assessment of the patient's problems due to their clear-cut medical knowledge, I use the provision of a diagnosis, *Diagnosis*, as a proxy of a visit's successful completion. According to Aranaz et al. (2005), the probability of a diagnostic error in the Spanish healthcare system is 0.13%. It is important to note that making a diagnosis is not excludable from providing other inputs, such as testing, as physicians use tests to assess and corroborate diagnoses. Following that logic, I include as an outcome a variable identifying whether the current first visit had a follow-up visit in the same hospital, named *Follow-up*.

Referring to the treatment choice, I investigate whether visit length is used as a substitute or complement to the provision of tests and drugs during the visit. On the one hand, physicians with extra visiting time may examine patients more thoroughly, inspecting their symptoms more carefully, reducing the need for intensive testing. In such a case, testing would be a substitute for visit length. On the other hand, visit length could complement intensive care as physicians with such extra visiting time could further deepen their knowledge of the clinical case and consequently order more tests. Moreover, extra visit length would give physicians a clearer idea of the patient's needs, thus modifying their drug prescription to more accurate doses.

The variables used to explore how visit length relates to treatment choices are i) *Tests*, which is a dummy variable measuring whether medical tests, e.g., imaging and laboratory tests, have been ordered, ii) *Num. Tests*, which is a variable identifying the absolute number of tests ordered in a given visit, iii) *Test Cost*, which measures the cost of the tests ordered, iv) *Drugs*, which is a dummy variable measuring whether drugs have been prescribed, and v) *Num. Drugs*, which measures the total number of drug doses ordered in a given visit. I compute the testing cost using internal cost information provided by the outpatient department in the sample. As for the number of drugs prescribed, I follow the aggregation method based on the Defined Daily Doses prescribed as proposed by the WHO. A Defined Daily Dose is a measure of drug utilization that stands for the assumed average maintenance dose per day for a drug used for its main indication in adults. I use this measure instead of the number of drugs provided, as it aggregates different drug groups weighted by their relative intensity, avoiding issues related to the drugs' package size and strength.

3.5 Results

Table 3.3 reports the estimation results using the 2SLS model previously outlined.[11] Column 1 introduces our first stage estimates using *Prior Cancel* as the source of exogenous variation and controls by a comprehensive set of fixed effects. Our first coefficient of interest, *Prior Cancel*, tells us that when shocked by a cancellation, physicians spend an average of 1.62 minutes more with the following patient than with any other patient with no immediately prior cancellation. Such a significant effect represents an increase of 12.8% over the average visit duration. It corresponds to the lower bound effect of a cancellation's impact on visit duration, given that visits at higher distances from a notification, used in this study as controls, may also be affected.

In Column 2, I test whether longer visit duration helps physicians to assess patients' diagnoses. We observe that longer visits positively affect the provision of a diagnosis, implying that for every minute spent with a patient, the probability of providing a diagnosis increases linearly by 0.36 percentage points. In other words, compared to the average probability of providing a diagnosis, every extra minute spent with a patient translates into a 4.39% higher chance of providing it. However, the positive relationship between visit length and diagnosis provision could be both measuring a more in-depth examination process driven by longer reviewing time and the fact that physicians had enough time to record the diagnosis. I test that hypothesis by identifying the most repeated diagnosis for each specialization. On the one hand, it could be that the extra time physicians use would only lead to a higher finding rate because they have the time to record the diagnoses or because they are more prone to fall to patients' diagnostic demands. In such a case, both common and uncommon diagnoses would be recorded more frequently as the visit length increases. On the other hand, physicians may use the extra visiting time to provide a more in-depth examination, providing significantly more uncommon diagnoses, since they can screen patients more thoroughly, thus providing more accurate diagnoses. Table 3.4 shows longer reviewing time leads to more uncommon diagnoses, while no effect is found on the provision of those diagnoses repeated most frequently. In fact, providing a common diagnosis takes 12.5 minutes, while an uncommon diagnosis requires an average of 13.3 minutes, suggesting that physicians use extra reviewing time to provide a more precise service.

Back to Table 3.3, we investigate how visit length affects input choices. In Column 3, I explore how reviewing time causally relates to the probability of ordering tests during a given visit. *Length* shows that every extra minute spent reviewing a patient increases the probability of ordering tests by 0.65 percentage points. Compared to the average

[11] For completeness, I include in the Appendix the benchmark specification without controls (Table 3.A5), the ITT estimation (Table 3.A6), and the OLS estimation (Table 3.A7). They are quantitative and qualitatively similar to the benchmark estimation.

visit ordering pattern, a one-minute increase in the visit length due to a prior cancellation implies a 3.6% higher chance of ordering tests. Column 4 broadens the outcome definition by checking whether visit duration affects the number of tests ordered. As in Column 3, we can see how increased visit duration leads to more tests. The estimated effect is low in magnitude, with an increase in the number of tests of 0.0096 per extra minute spent on the consultation. Despite that, compared to the average number of tests ordered, we can see how an increase of one minute in the visit duration implies a 3.35% increase in the number of tests ordered. These two results suggest that test ordering is used to complement visit duration, meaning that when physicians are exogenously exposed to more time, they employ it in ordering more tests. Due to the right-skewed test-ordering distribution, as shown in Table 3.1, the main driver in this relationship is the extensive margin. Column 5 further checks whether increases in visit duration affect testing costs. We can see how a unit increase in visit duration corresponds to an increase in *Test Cost* of €0.8. That means an extra minute on a consultation translates into a 6.35% increase in the average testing cost. This implies that visit duration and total testing cost are complementary inputs.

In Columns 6 and 7, I focus on drug prescription. Column 6 shows that visit duration does not affect the probability of prescribing drugs. However, it does have an effect on the dose prescribed. Column 7 shows how an increase of one minute in a given visit weakly reduces prescription doses by 0.4 units. That sizable effect represents a 20.1% reduction in the average dose. These results indicate that providing time to physicians helps reduce the overall dose provided to patients, acting as a substitute for reviewing time. Under the assumption that a longer visit duration helps the physician to have a clearer idea of the patient's problems, the provision of lower doses of drugs could be understood as a convergence to the optimal prescription.[12]

Lastly, Column 8 analyzes whether a longer visit duration affects the probability of having a follow-up visit. On the one hand, physicians might decide to provide patients with a follow-up visit at the hospital because a more extended visit might imply further tests to be checked in situ. On the other hand, the extra visit length might help assess the patient's diagnosis better, thus redirecting the patient back to the primary health care center of origin. We can observe how a one-minute increase in visit length increases the probability of a follow-up visit by 0.92 percentage points, representing a 3.28% increase over its mean. Table 3.A8 shows that a one-minute increase in reviewing a first visit leads to an increment in the total clinical case duration of 1.16 days. This result suggests that physicians' complementary use of time and testing leads to longer clinical processes.

[12]The medical literature has found negative correlations between consultation length and medical over-prescription, suggesting that longer visits help physicians investing time on the patients' education and psychological support (Dugdale et al., 1999; Ventelou et al., 2010; Allen et al., 2022; Neprash et al., 2023).

All these results suggest that visit length is a key factor in understanding input utilization. However, they could hide an intertemporal input substitution decision followed by physicians, motivated by the extra time available during their first visits. If this were the case, we would expect physicians who were *shocked* during a given first visit to adjust their input utilization during the corresponding follow-up visit inversely.[13] Table 3.5 tests such a hypothesis, using a similar strategy as in Table 3.3, in a subsample of first visits with a follow-up visit in our outpatient department. We can see how increases in visit length during the first visit do not significantly impact the input utilization during the follow-up visit. This result reinforces the idea that physicians do not use extra visit length to transfer treatments intertemporally; instead, they provide patients with extra care they would not have otherwise received in their medical process.

Column 7 in Table 3.5 introduces a new variable identifying whether the same physician conducted first and follow-up visits. We can see how increasing visit length during a first visit relates to keeping the same treating physician. For every extra minute spent on a first visit, the likelihood that a patient will continue with the same physician increases by 1.05 percentage points. This result suggests that longer visits give physicians a reason to keep the same patients, possibly due to their more exhaustive knowledge of the patient's case or increased satisfaction from the visit. Table 3.6 provides further evidence that physicians are pushing for preserving their patients and not the other way around. Physicians achieve this by securing that ordered diagnostic procedures are ready when a follow-up visit occurs, thus keeping the same patients over time. In practice, for every extra reviewing minute spent in a first visit, the probability that physicians cancel the follow-up visit decreases by 12.9%. No effect is found on patient-motivated cancellations.

These results suggest that physicians use the extra time to assess the patient diagnosis better, to recommend further intensive care treatments, and to correct drug prescription excess. Nevertheless, how intensely physicians use such time might depend on multiple factors. In the following subsections, I explore whether patients' characteristics are key in understanding time utilization and shed light on the relevance physicians' contracts have on such a relationship.

3.5.1 Which Patients' Characteristics are Driving These Effects?

In this section, I explore the influence patients and shared patient-physician characteristics have on time utilization. I examine whether the patients' gender influences how physicians use extra visiting time. While patients may differ in required treatments along their gender, the exogenous exposure to cancellations allows us to study whether physicians

[13] I test whether having a prior cancellation predicts any patient characteristic in the sample of follow-up visits. Table 3.A9 in the Appendix shows no systematic sample selection based on observable patient characteristics.

treat them differently. Table 3.7 shows that visit length affects male and female patients differently. Firstly, we can see that after the realization of a cancellation, physicians employ more time similarly with both male and female patients. Nevertheless, physicians use such extra time only input-intensively with female patients, with increased tests ordered and a lower prescription dose. This differential input use is not explained by a systematic difference in their unconditional means, suggesting some limited preferential treatment towards women. I further inspect whether physicians treat patients differently depending on whether they share the same gender as the patient. On the one hand, we could expect that physicians use time more intensively on those patients sharing their gender, following their probable higher *proximity*. On the other hand, given that physicians might be able to screen those patients sharing their gender more quickly, we could expect that extra visiting time could be only used efficiently on patients of other genders. Table 3.8 shows that, when exposed to cancellations, physicians use extra visiting time more intensively only with those patients with a different gender. Putting both results together, they suggest that physicians provide more intensive care to female patients and those patients who do not share their gender.

I then look at whether physicians treat patients differently based on their nationality. Following the previous approach, Table 3.9 analyzes whether physicians treat native patients differently than those born in other countries. While both national and non-national patients get more consultation time after a cancellation, physicians only provide diagnostic inputs and more tests to national patients. The patients' inherent characteristics do not explain such differential productivity by physicians. Moreover, the outpatient department considers non-national patients, if anything, more demanding, indicated by providing them longer expected visit lengths, 15.17 minutes, compared to 14.8 minutes for national patients. The results highlight that, although the outpatient department considers non-national patients more demanding, physicians provide a more valuable service only to national patients when given extra visit time.

Next, I focus on the treatment physicians provide to patients depending on how many days patients have to wait to access the outpatient department. As previously explained in Section 3.2.1, patients are scheduled for a first visit with an outpatient physician at their primary care health centers. At that level, given the hospital scheduling limitations, primary care physicians can decide to speed up patients' first visit with a specialist, implying that those patients with worse health conditions will be granted appointments at shorter notice and flagged as urgent to the outpatient physician. Moreover, given that accessing the emergency room is always an option, those patients waiting for an extended period will presumably be those with less urgent health issues. Table 3.10 provides evidence that physicians use extra visiting time differently depending on the patient's waiting time. Physicians use longer visits to order more tests, decrease the drug

dose prescribed, and provide a diagnosis, but only for those patients whose waiting time was below the average time for their specialization. These results suggest that physicians internalize the time patients wait for the first visit, providing more urgent patients with a more valuable service.

A remaining question would be whether all specializations in the hospital spend reviewing time in the same fashion. To examine that issue, I classify the specialties into internal medicine or surgical, as these two categories require different input compositions. While surgical specialties use pre-established treatment protocols and surgical procedures to find and solve patients' health problems, internal medicine specialties are characterized by more intense use of visiting time and drug prescriptions. Table 3.11 shows how physicians react to extra visiting time, depending on their specialty. On average, we can observe how those visits to internal medicine specialists are characterized by more visiting time and drug prescription than the surgical specialties, which emphasize providing more tests. When physicians are notified of cancellations, those in both categories respond by increasing their visiting time. This extra time is then used by internal medicine physicians to provide patients with more tests but also with a diagnosis (for every extra minute, physicians increase their probability of providing a diagnosis by 7.36% over their average diagnostic probability). Conversely, surgical specializations use the increased time to provide patients with tests and reduce their drug doses, but no impact on the diagnosis rate.[14]

Overall, the way physicians use extra visiting time greatly depends on the patient's inherent characteristics and the physicians' specialization. These results highlight that physicians' reaction to the relaxation of their time constraints is not monotonic across subgroups, especially favoring female, Spanish-born, and more urgent patients.

3.5.2 Role of Physicians' Contracts

In this section, I study how physicians' contracts shape how extra visiting time is used.

According to the general Spanish healthcare legislation, the hospital organizes its employees, which determines that contracts are composed of fixed-wage and flexible components, mainly depending on physicians' tenure.[15] These contracts are updated annually on a per-physician basis, including adapting visit workloads according to the physicians' responsibilities and tenure, which might ultimately lead to differential use of the extra

[14]For completion, I include Table 3.A10, which shows there is no specific time use along the patients' age profile; and Table 3.A11, which shows that the nature of the shock, namely whether the prior visit was a no-show or a notification, does not differently influence how extra reviewing time is used. Contrarily, Table 3.A12 shows that physicians react significantly more to extra visiting time on overloaded days, in which time is most precious.

[15]The fixed component is similar across physicians as it is based on educational attainment, which is, by law, required to be a bachelor's degree in medicine and to have passed a national exam (See Art. 4 in the Royal Decree 127/1984).

time provided by cancellations.[16]

I use physicians' age as a proxy of their tenure, given that i) physicians enter the medical market right after finishing their studies,[17] and ii) the market for physicians enjoys low unemployment.[18] I define physicians to be senior if their age is higher than the median age (≈ 50 years old); otherwise, I define them as junior. As indicated previously, the older physicians are, the more seniority they are likely to have, thus the higher their salary. While the hospital has the incentive to retain these experienced physicians, it cannot freely raise the physicians' salaries, which are publicly regulated. Therefore, senior physicians might be compensated with more advantageous shifts instead. Table 3.12 shows that senior physicians' schedules include lower numbers of patients per hour and fewer overbooked visits, while the expected visit duration is similar to that of junior physicians. Furthermore, Table 3.13 shows that patients visiting senior outpatient physicians do not differ systematically from those visiting their junior colleagues. These tests show that while seniority affects the physician's workload through more relaxed schedules, it does not imply a change in patient composition.[19]

Back to our benchmark specification, Table 3.14 shows how extra visit duration affects the input utilization depending on whether it is provided to senior or junior physicians. The first insight we obtain from Columns 1 and 2 is that both senior and junior physicians similarly react to cancellations by increasing the reviewing time with their subsequent patients. Despite this similar increase, the unconditional visit length for junior physicians is 11.7 minutes, while for their senior colleagues, it is 14 minutes. This shows that even if junior physicians utilize more time, it is not enough to compensate for the difference between the average visit length between these two groups. The way contracts are formulated, being physician-specific, facilitates less rushed environments for older professionals at the expense of their younger colleagues.

This formulation fully determines how extra visit length is used. In Column 3, we can observe that junior physicians use extra visiting time more effectively by providing more diagnoses. Every extra minute a junior physician spends with a patient increases their probability of providing a diagnosis by 0.73 percentage points, i.e., it increases the probability of providing a diagnosis by 9.56% compared to its average. On the opposite extreme, despite spending more time with patients affected by a prior cancellation, senior physicians do not use such *bonus* time to modify their diagnosis provision. These results

[16]For further knowledge on the collective bargaining agreement, please refer to the Resolution EMO/1742/2015 present in the Catalan Regional Bulletin n. 6923.

[17]According to the Spanish Health Ministry, the average age of those physicians entering practice in one of the specialties covered in the sample is 26 years, which corresponds to the age at which students finish their studies (Spanish Health Ministry, 2015).

[18]According to the Spanish Health Ministry, physician's unemployment in 2017 was 2.32% (Spanish Health Ministry, 2019). The unemployment rate in Spain in 2017 was 17.22%.

[19]A total of 13.67% of the visits correspond to 16 physicians who did not want their data to be made public. This section does not consider them.

suggest that visit length expansions are not output-efficient for physicians already enjoying more relaxed schedules. Table 3.15 shows how the extra visiting time helps only junior physicians to provide a more in-depth diagnosis, measured by more uncommon diagnoses. This result suggests that junior physicians effectively use the extra time to provide a more valuable service.

Back to Table 3.14, I display in Columns 4 to 8 how consultation time affects input choice. On the one hand, when exposed to extra time, junior physicians provide patients with more tests at intensive and extensive margins and of higher cost. Quantitatively, for every extra minute a junior physician spends with a patient, the probability of ordering a test increases by 0.68 percentage points (representing a 4% increase over the average ordering probability), the number of tests ordered is increased by 0.013 units (representing a 4.88% increase over their average ordering rate), and testing cost increases by 10.8%. On the other hand, longer visits affect the drug dose level prescribed to patients, as in Table 3.3, through the intensive margin. For every extra minute a junior physician spends with a patient, they decrease the average dose prescribed by 0.68 daily defined doses (representing a reduction in the prescription dose level by 28.82% when compared to their average dose prescribed). Similarly, senior physicians decrease the patient's prescriptions by 0.19 doses (reflecting an average reduction of 8.24% in the prescription doses).[20]

These results highlight that correcting insufficient time per visit might have welfare-improving effects, as in the case of junior physicians. For senior physicians, longer visits do not entail further care expansions, suggesting they are already at their optimal level of time-to-input utilization. These results suggest that defining schedules based on seniority might hinder high costs related to suboptimal utilization of visiting time. In Section 3.6, I provide a quantification analysis stressing these inefficiencies and show that time expansions to less experienced physicians might be cost-effective.

3.6 Quantifying the Cost of a Diagnosis

In this section, I quantify the direct cost of increasing visit length.[21] Suppose we want to increase the probability of providing a diagnosis by one percentage point ($\approx$ 12% at the sample average). We can achieve this in two ways: i) by increasing all physicians' visiting times; or ii) by favoring only those physicians with less experience.

[20] In the same spirit, Table 3.A13 shows that extra visiting time helps least productive physicians catching up in the care provided. At the same time, no effects are found on high-performing doctors.

[21] Throughout the exercise, I assume that the outpatient department's fixed capacities are non-binding along small visit length expansions. Similarly, I do not internalize the positive crowding-out effect more prolonged first visits have on other services, such as the emergency room.

3.6.1 Broad Increase

Let us say we opt to increase the length of all first visits to achieve a one-percentage-point increase in the diagnosis rate. That can be achieved with an increase in the average visit length of 2.77 minutes, using the IV-fixed-effects estimates in Column 3 of Table 3.3.

We calculate the direct costs associated with increasing visit length such that it increases the diagnosis rate by one percentage point, assuming that physicians will optimally utilize their *bonus* visiting time. In our case, using a linear approximation, we have the following:

$$\hat{\Delta}_{minutes} = 2.77 \times 6.55 \times 102.44 \quad = 1,858.62 \text{ minutes per year and physician}$$

where 6.55 refers to the average number of first-visit patients per day and physician, and 102.4 is the average number of days worked per physician. $\hat{\Delta}_{minutes}$ amounts to about 31 hours extra per year and physician, representing a 1.8% increase in the physician's yearly working hours. We now extrapolate our physician-specific estimates to the general Spanish economy, such that:

$$\hat{\Delta}_{cost} = \hat{\Delta}_{minutes} \times 0.5876 \times 1 + 0.0092 \times 10.55 + 0.8045 \times 76,562 \quad \approx €206m$$

where 0.5876 represents the average physician wage per minute,[22] 0.0092 represents the increased probability of scheduling a follow-up visit due to a one-minute increase in the first visit duration, and 10.55 the average follow-up visit length. 0.8045 represents the average treatment cost ordered for every extra minute spent with a patient,[23], and 76,562 refers to the total number of outpatient physicians in Spain in 2018 (Spanish Health Ministry, 2019). Thus, increasing the diagnosis rate in first visits by one percentage point would have an estimated labor cost of €206m for the general Spanish economy.

3.6.2 Tailored Increase

Suppose we now opt to provide more time per visit only to those physicians who will use it more efficiently. Following the previous procedure, I study how many more minutes junior physicians should have to increase their diagnosis rate by one percentage point. That can be achieved by increasing the visit length of junior physicians by 1.37 minutes, using

[22] The average working hours by a physician in the Spanish health system is 1,645 hours, regulated by Decree 2/2012 and Royal Decree 20/2012. The average outpatient physician salary in 2018 is €58,000 (Medscape, 2019).

[23] The average treatment cost is calculated using internal information of the sample outpatient department. Both in this and the following calculations, it is assumed to be representative of the health system as a whole.

the IV-fixed-effects estimates in Column 3 of Table 3.14. This change at the visiting intensive margin helps junior physicians assess their patients adequately while leaving senior physicians' schedules unchanged. Following the same structure as before, we have:

$$\hat{\Delta}_{minutes,junior} = 1.37 \times 6.82 \times 95.45 \quad = 891.82 \text{ minutes per year and junior physician}$$

Now we extrapolate these changes to the overall economy, such that:

$$\hat{\Delta}_{cost} = \hat{\Delta}_{minutes,junior} \times 0.575 \times 1 + 0.0116 \times 10.09 + 1.294 \times 42,863 \quad \approx \text{€}74m$$

where 0.575 represents the per-minute wage,[24] 0.0116 represents the increased probability of scheduling a follow-up visit due to a one-minute increase in the first visit duration, and 10.09 is the average follow-up visit length. 1.294 represents the average treatment cost ordered for every extra minute spent with a patient, and 42,863 represents the estimated number of junior physicians.[25]

In sum, comparing this targeted increase to the previous broad increase in reviewing time, it is more cost-effective in achieving the same result, a one-percentage-point increase in the diagnostic provision. With all due caveats, this exercise highlights how solely exploiting the contracting incentives based on seniority would allow for more efficient diagnostic provision at a reduced cost.

3.7 Conclusion

This paper estimates and provides evidence of the inefficient time allocation in the Spanish outpatient system. I leverage its unique setting and cancellations as random time shocks to provide a causal interpretation of how the amount of reviewing time shapes physicians' decisions. Conceptually, I compare those first visits affected by an unexpectedly longer visit time caused by a prior cancellation to all other first visits, holding all other parameters in the environment constant.

I find that longer first visits lead to a higher likelihood of providing a diagnosis, the main objective of outpatient departments. The effect is driven by uncommon diagnoses, whose provision requires a more in-depth analysis, while no effect is found for the most common diagnoses. Longer first visits increase diagnostic input utilization while decreasing drug dose prescriptions. These results suggest that physicians use the extra visiting

[24]The salary for junior physicians corresponds to a physician with a fixed position, around 40 years old, and 15 years of experience. The annual salary of such a physician is €56,755. For further reference, see OMC (2019).

[25]I use information from the OECD database - Healthcare Utilization. Given that the number of outpatient physicians is not tabulated by age, I assume that the distribution of physicians by age is the same for the overall population of physicians and that of outpatient physicians.

time to assess the patient's health problems in more detail and, in the event of indecision, to request further diagnostic inputs, ultimately improving the service provided. Moreover, I find no evidence of an input substitution effect between first and follow-up visits, suggesting that longer first visits have a lasting impact on the clinical process.

I then look at how relevant working contracts are in shaping physicians' decisions. While the outpatient department has the incentive to retain more experienced physicians, it is unable to freely raise the physicians' salaries, which are publicly regulated, compensating them with more advantageous shifts instead. I find that junior physicians, whose contracts lead to more pressured schedules than their senior colleagues, use extra visiting time efficiently, while senior physicians do not. This result highlights, and I show quantitatively, that policies increasing all reviewing time across the board might prove inefficient.

This avenue of research is extremely important for policymaking, as it emphasizes that current promotion incentives might lead to inefficient input utilization. While this paper has focused on one Spanish outpatient department, the message of this study, concerning the effect of remedying insufficient reviewing time on the workers' decisions is more general. In fact, it relates to all those time-constrained situations in which workers must decide between speeding up their processes and exerting higher effort per task. This study indicates that public welfare may be improved by policies providing additional time to workers most in need.

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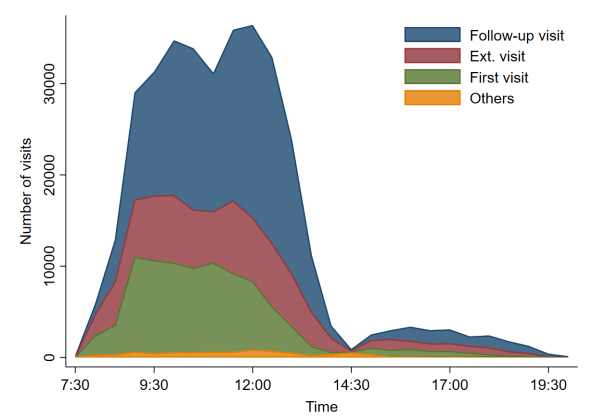
Figures and Tables

Figure 3.1: Daily Physician's Schedule Viewed at 10:00 am.

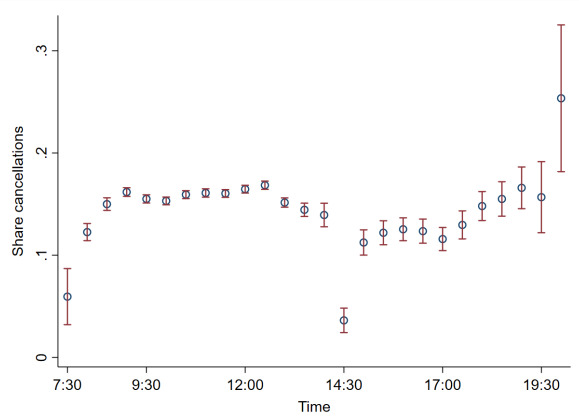
Appointment Time	Patient ID	Patient	Basic Health Zone	Status	Arrival time	Visit type
8:30	1	Antonio García Gracia	Barcelona 2-B	Completed	8:25	Follow-up
9:00	2	Jordi Bosch Fernández	Barcelona 3-A	Not present	-	Follow-up
9:10	3	Montserrat Muñoz Sánchez	Barcelona 4-D	Completed	9:05	First Visit
9:15	4	María del Carmen González Serra	Barcelona 5-D	Completed	9:00	First Visit
9:30	5	Anna Solé Pérez	Barcelona 1-C	Completed	9:10	Follow-up
9:40	6	José Giménez Sánchez	Barcelona 2-E	Completed	9:00	Long Cure
10:00	7	Wei Wang	Barcelona 8-B	Completed	9:40	Injection
10:15	8	María José Pérez Iglesias	Barcelona 4-C	Pending	9:45	First Visit
10:25	9	Montserrat Batlle Figueres	Barcelona 5-C	Pending	-	Follow-up
10:43	10	María del Mar Cardel Pérez	Barcelona 3-E	Cancelled	-	First Visit
11:00	11	Mohammed Alaoui	Barcelona 5-A	Pending	-	Follow-up

Notes: The figure shows how the schedules used in the outpatient department look like, using fictitious information. *Appointment Time* refers to the time at which a patient is appointed to start her visit. *Status* refers to the visiting status, which can be "Completed" if the visit finished already, "Not Present" if the visit was supposed to happen but the patient was not present, "Pending" if the visit will happen later, and "Cancelled" if the visit was appointed for a later time but cancelled earlier on the day. *Arrival time* refers to their arrival time to the outpatient department. If *arrival time* is not displayed (e.g. –), it means the patient has not registered yet at the outpatient department. *Visit type* highlights broadly the type of visit, which can be "First Visit", "Follow-up", "Long Cure", or "Injection".

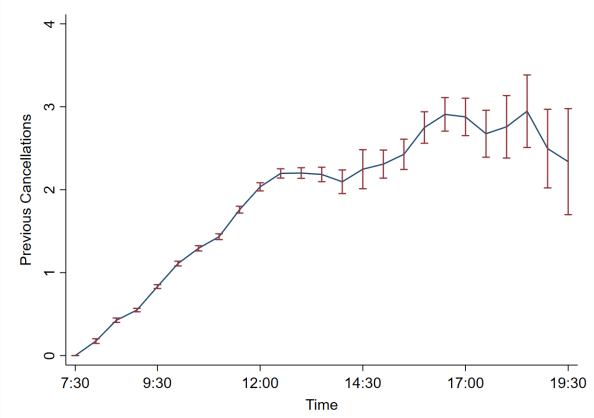
Figure 3.2: Distribution of Visits Over the Day



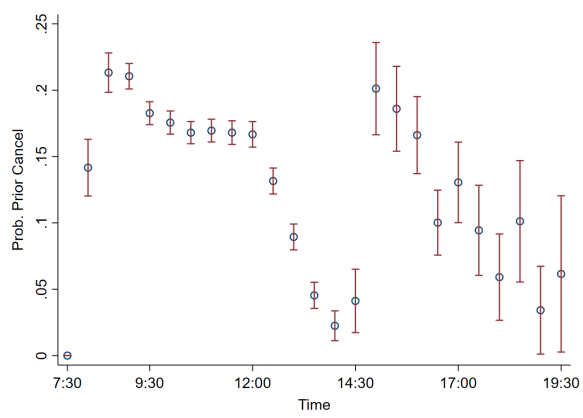
(a) Distribution by visit type



(b) Share of cancellations (Appointment)



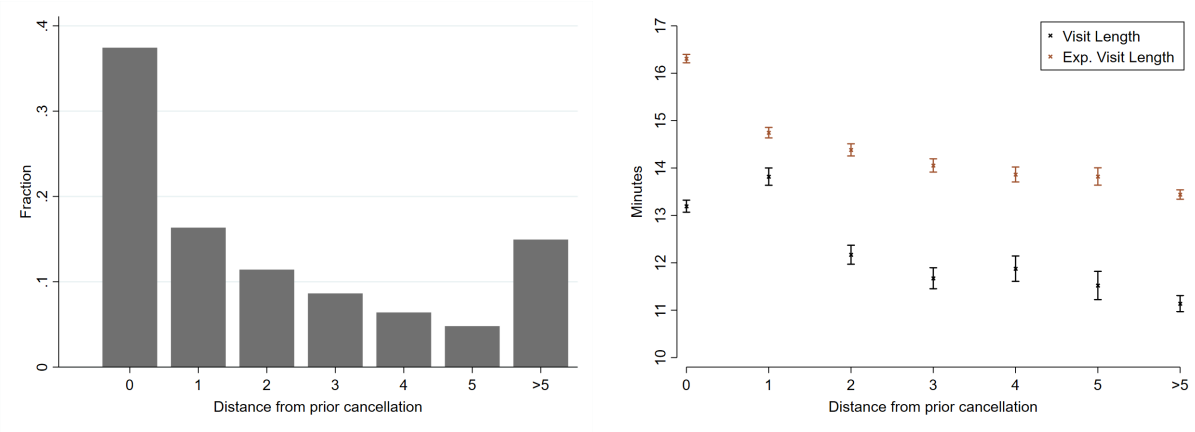
(c) Previous cancellations (First visits)



(d) Prob. Prior cancel (First visits)

Notes: The figure reports how visits and cancellations span over the schedule. Subfigures 3.2a and 3.2b use the sample including all the visits, namely first, external, and follow-up visits, cancelled or not, while subfigures 3.2c and 3.2d only use our final sample of first visits. Subfigure 3.2b displays the share of cancellations as to when those visits were appointed. Subfigures 3.2c and 3.2d use the real notification time of those cancellations as in our main analysis. Prior cancel identifies those visits that had their prior visit slot cancelled using their real cancellation time. All subfigures use 30-minutes bin sizes.

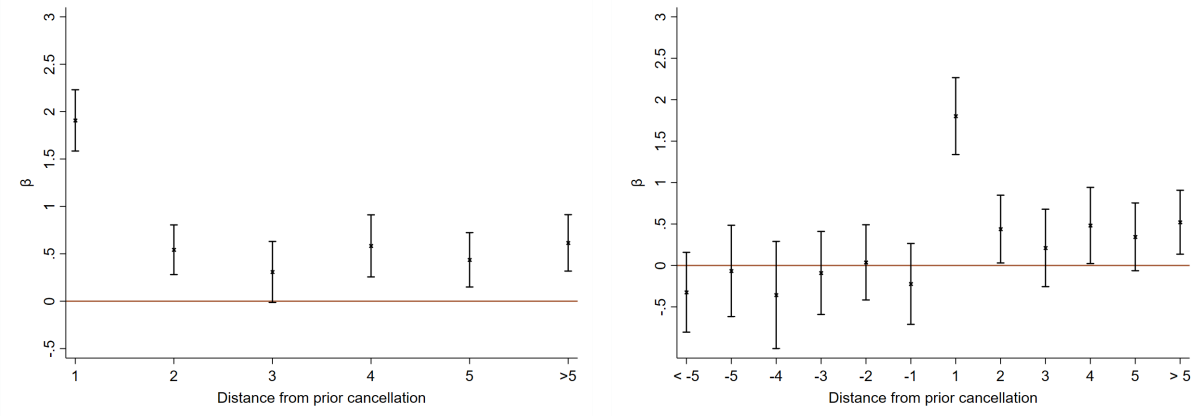
Figure 3.3: Distances to Prior Cancellation



(a) Distribution (b) Unconditional means

Notes: The figure reports the proportion of visits by distance to a cancellation and their visit lengths. The sample use corresponds to the final sample as exposed in Table 3.1. Subfigure 3.3a shows the proportion of visits which had no previous cancellation (distance 0), a cancellation in the previous visit (distance 1), and so forth. Subfigure 3.3b displays the unconditional mean of both visit length and expected visit length by the distance to a preceding cancellation.

Figure 3.4: First Stage at Multiple Distances



(a) All shifts (b) Shifts with one or no cancellation

Notes: The figure reports how cancellations impact surrounding visits. Subfigure 3.4a uses the final sample as exposed in Table 3.1, and shows graphically the first stage results using dummy variables identifying those visits at 1, 2, 3, 4, 5, or more than 5 visits from a cancellation. Subfigure 3.4b uses only those shifts with one or no cancellations, and shows graphically the first stage results using dummy variables identifying those visits at 1, 2, 3, 4, 5, or more than 5 visits from a cancellation, both prior and posterior to a cancellation. The results presented in both figures include all the fixed effects and controls as in our benchmark specification (see Table 3.3). Confidence intervals at the 95%.

Table 3.1: Summary Statistics

	Mean	SD	Min	Max	N
Patient characteristics					
Male	0.45	0.50	0	1	67530
Age	58.85	19.55	0	106	67530
Reference BHZ	0.60	0.49	0	1	67530
Distance from hospital (km)	4.37	12.87	0	1979	67530
Born in Spain	0.68	0.47	0	1	67530
Public coverage	0.98	0.12	0	1	67530
Chronic condition	0.06	0.23	0	1	67530
Physician characteristics					
Physician: Male	0.59	0.49	0	1	66350
Physician: Age	49.78	9.32	32	65	58301
Visit characteristics					
Visit length (mins)	12.58	9.59	1	120	67530
Follow-up visit	0.28	0.45	0	1	67530
Out of agenda	0.15	0.35	0	1	67530
Internal referral	0.11	0.32	0	1	67530
Waiting list (days)	29.73	52.02	0	770	67530
Waiting room (mins)	27.22	32.62	0	545	67530
Tests	0.29	0.75	0	15	67530
Test cost	12.67	50.58	0	2019	67530
Drugs	2.04	27.34	0	2600	67530
Diagnosis	0.08	0.27	0	1	67530

Notes: The table provides a summary statistics for our sample of interest. Reference BHZ is an indicator variable that identifies whether the patient comes from a Basic Health Zone covered by the outpatient department. Distance from hospital is a variable that measures how many kilometers apart is the patient's Basic Health Zone centroid from the hospital using a linear distance algorithm. Public coverage is an indicator variable that identifies whether the treated patient is covered by the general public health insurance. Chronic condition is an indicator variable that identifies if the patient previously was been diagnosed any chronic condition. Visit length identifies how long a visit is using the patient's profile opening and closure in the physician's terminal. Out of agenda identifies whether the visit was placed in a slot not covered by the physician's agenda (visit schedule). Internal referral identifies if the visit was appointed by another hospital physician as opposed to a general practitioner. Waiting room is a variable that measures how many minutes has the patient been waiting prior to the visit start. Test cost indicates the testing cost per visit in euros. The variable Drugs captures the number of drugs prescribed measured using the Defined Daily Dose (DDD) definition. Diagnosis is an indicator variable identifying if a visit led to the definition of a precise diagnosis. Physician related variables such as age or sex have missing observations as some physicians preferred not disclosing such information. All other variables are self-explanatory.

Table 3.2: Covariate Test

	(1) Male	(2) Age	(3) Ref. BHZ	(4) Dist. BHZ	(5) Chronic	(6) Pub. Cov	(7) Spanish	(8) Waiting list	(9) Same sex	(10) Same age
Prior Cancel	-0.0039	0.2213	0.0057	-0.0434	0.0032	0.0003	-0.0052	1.0568	-0.0030	0.0046
	(0.0045)	(0.1812)	(0.0056)	(0.1492)	(0.0023)	(0.0012)	(0.0047)	(0.7029)	(0.0046)	(0.0043)
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67530	67530	67530	67530	67530	67530	67530	67530	66350	58301
Dep. Var. Mean	0.447	58.85	0.598	4.365	0.0582	0.984	0.677	29.73	0.517	0.152

Notes: The table tests whether having a prior cancellation predicts the patient and the shared physician-patient characteristics. Ref. BHZ is an indicator variable that identifies if the patient comes from a Basic Health Zone covered by the hospital. Dist. BHZ measures how many kilometers apart is the patient's Basic Health zone from the hospital using a linear distance algorithm. Chronic is an indicator variable that identifies if the patient previously had any chronic condition. Pub. Cov. identifies if a visit was covered by the public insurance scheme. Spanish identifies those patients born in Spain. Waiting list measures the days that patients wait to access a first visit from their corresponding Primary Care center. Same sex identifies if both physician and patient share the same sex. Same age identifies if both physician and patient have a similar age, measured using a 10 years window. All other variables are self-explanatory. Standard errors are clustered at the physician level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.3: Effect of Visit Length on Visit Outcomes - Main Analysis

	(1) Length	(2) Diagnosis	(3) Test	(4) Num. Tests	(5) Testing cost	(6) Drug	(7) Num. Drugs	(8) Follow-up
Length		0.0036**	0.0065***	0.0096**	0.8045**	-0.0010	-0.4106*	0.0092***
		(0.0018)	(0.0023)	(0.0042)	(0.3470)	(0.0011)	(0.2166)	(0.0032)
Prior Cancel	1.6222***							
	(0.1598)							
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67530	67530	67530	67530	67530	67530	67530	67530
Dep. Var. Mean	12.58	0.0819	0.181	0.286	12.67	0.0333	2.043	0.280
F - Stat	–	104.3	104.3	104.3	104.3	104.3	104.3	104.3

Notes: The reported regressions correspond to the 1st Stage (Col. 1), and the 2nd Stage with multiple outcome variables (Col. 2-8). For information on the outcome variables, please refer to Section 3.4.1. All regressions include the following controls: Patient sex, age, square age, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient was born in Spain, whether the patient is covered by the public insurance, the days and squared days passed since the first visit referral, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. See Table 3.2 for further reference. Standard errors are clustered at the physician level. F-Stat corresponds to the first-stage F-statistics measure proposed by Montiel Olea and Pflueger (2013). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.4: Effect of Visit Length on Diagnosis Provision

	(1) Diagnosis	(2) Common	(3) Uncommon
Length	0.0036**	0.0001	0.0034**
	(0.0018)	(0.0008)	(0.0014)
Month-Year FE	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	67530	67530	67530
Dep. Var. Mean	0.0819	0.0123	0.0695
F - Stat	104.3	104.3	104.3

Notes: The reported regressions correspond to the 2nd Stage with the following outcomes: i) the probability of a diagnosis (Col. 1), ii) the probability of a common diagnosis (Col. 2), and iii) the probability of an uncommon diagnosis (Col. 3). Diagnoses are classified as common identify those diagnoses most repeated in a given specialization, while uncommon represent any other non modal diagnosis. See Table 3.3 for further reference on the controls used. Standard errors are clustered at the physician level. F-Stat corresponds to the first-stage F-statistics measure proposed by Montiel Olea and Pflueger (2013). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.5: Effect of Current Visit Length on the Next Visit Outcomes

	(1) Length	(2) F. Length	(3) F. Tests	(4) F. Num. Tests	(5) F. Drugs	(6) F. Num. Drugs	(7) Same Physician
Length		0.0953	0.0008	-0.0013	0.0008	0.5331	0.0105***
		(0.1848)	(0.0034)	(0.0055)	(0.0008)	(0.4414)	(0.0037)
Prior Cancel	1.8596***						
	(0.2439)						
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14350	14350	14350	14350	14350	14350	14350
Dep. Var. Mean	14.39	11.19	0.143	0.195	0.00613	0.552	0.656
F - Stat	–	58.82	58.82	58.82	58.82	58.82	58.82

Notes: The reported regressions correspond to the 1st Stage (Col. 1), and the 2nd Stage with multiple outcome variables (Col. 2-7). The sample used refers to all those visits that had a follow-up visit appointed on that same first visit. The outcomes used refer to the follow-up visit. For information on the outcome variables, please refer to Section 3.4.1. *Same Physician* is a dummy variable that takes value one if the visit was conducted by the same physician that conducted the first one, and zero otherwise. All regressions include the following controls: Patient sex, age, square age, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient was born in Spain, whether the patient is covered by the public insurance, the days and squared days passed since the first visit referral, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. All the controls used are measured as in the first visit. See Table 3.2 for further reference. Standard errors are clustered at the physician level. F-Stat corresponds to the first-stage F-statistics measure proposed by Montiel Olea and Pflueger (2013). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.6: Effect of Current Visit Length on the Next Visit Cancellation

		Next visit cancelled		
	Length	All	By patient	By physician
	(1)	(2)	(3)	(4)
Length		-0.0001	0.0036	-0.0037**
		(0.0048)	(0.0043)	(0.0018)
Prior Cancel	1.8328***			
	(0.1947)			
Month-Year FE	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Dep. Var. Mean	14.34	0.240	0.211	0.0287
F - Stat	–	89.65	89.65	89.65

Notes: The reported regressions correspond to the 1st Stage (Col. 1), and the 2nd Stage with multiple outcome variables (Col. 2-7). The sample used refers to all those visit that had a follow-up visit appointed on that same visit. All regressions include the following controls: Patient sex, age, square age, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient was born in Spain, whether the patient is covered by the public insurance, the days and squared days passed since the first visit referral, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. All the controls used are measured as they were during the first visit. See Table 3.2 for further reference. Standard errors are clustered at the physician level. F-Stat corresponds to the first-stage F-statistics measure proposed by Montiel Olea and Pflueger (2013). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.7: Effect of Visit Length on Visit Outcomes - By Patient sex

	(1) Length	(2) Length Male	(3) Diagnosis	(4) Test	(5) Num. Tests	(6) Test Cost	(7) Drug	(8) Num. Drugs
Length			0.0033	0.0081**	0.0107	1.4358***	-0.0019	-0.5883**
			(0.0022)	(0.0034)	(0.0067)	(0.4917)	(0.0019)	(0.2329)
Length × Male			0.0005	-0.0033	-0.0024	-1.3088*	0.0020	0.3682
			(0.0028)	(0.0044)	(0.0088)	(0.7509)	(0.0025)	(0.2928)
Male	-0.0163	12.3993***	-0.0052	0.0315	0.0138	16.6131*	-0.0232	-4.0772
	(0.1029)	(0.5845)	(0.0352)	(0.0565)	(0.1139)	(9.6802)	(0.0313)	(3.6727)
Prior Cancel	1.5313***	0.0901						
	(0.1754)	(0.0844)						
Prior Cancel × Male	0.2067	1.5743***						
	(0.1656)	(0.2835)						
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67530	67530	67530	67530	67530	67530	67530	67530
Joint Length p-value	–	–	0.0949	0.106	0.120	0.810	0.995	0.446
Dep. Var. Mean	–	–	0.0819	0.181	0.286	12.67	0.0333	2.043
F - Stat	–	–	19.15	19.15	19.15	19.15	19.15	19.15

Notes: The reported regressions correspond to the 1st Stage regression (Col. 1-2) and the 2nd Stage with multiple outcome variables (Col. 3-8). The table presents the interaction of *Length* and the patient's sex (*Male*). For information on the outcome variables, please refer to Section 3.4.1. All regressions include the following controls: Patient age, square age, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient was born in Spain, whether the patient is covered by the public insurance, the days and squared days passed since the first visit referral, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. See Table 3.2 for further reference. Joint Length p-value is the joint p-value of both *Length* and *Length* × *Male*. Standard errors are clustered at the physician level. F-Stat corresponds to the first-stage F-statistics measure proposed by Kleibergen and Paap (2006). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.8: Effect of Visit Length on Visit Outcomes - By Patient-Physician sex

	(1) Length	(2) Length Male	(3) Diagnosis	(4) Test	(5) Num. Tests	(6) Test Cost	(7) Drug	(8) Num. Drugs
Length			0.0046**	0.0103***	0.0220***	1.4593***	0.0008	-0.3642
			(0.0021)	(0.0036)	(0.0064)	(0.4992)	(0.0016)	(0.2773)
Length × Same sex			-0.0021	-0.0073*	-0.0242***	-1.2207*	-0.0036	-0.0894
			(0.0027)	(0.0044)	(0.0091)	(0.7327)	(0.0026)	(0.2728)
Same sex	-0.0772	12.3465***	0.0292	0.0940*	0.3127***	16.4118*	0.0428	0.6551
	(0.1067)	(0.5926)	(0.0332)	(0.0561)	(0.1161)	(9.2885)	(0.0319)	(3.4255)
Prior Cancel	1.6723***	0.0824						
	(0.1621)	(0.1057)						
Prior Cancel × Same sex	-0.0264	1.4853***						
	(0.1727)	(0.2761)						
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	66350	66350	66350	66350	66350	66350	66350	66350
Joint Length p-value	–	–	0.261	0.288	0.718	0.639	0.116	0.0506
Dep. Var. Mean	–	–	0.0819	0.181	0.286	12.67	0.0333	2.043
F - Stat	–	–	17.44	17.44	17.44	17.44	17.44	17.44

Notes: The reported regressions correspond to the 1st Stage regression (Col. 1-2) and the 2nd Stage with multiple outcome variables (Col. 3-8). The table presents the interaction of *Length* and whether the patient and physician have the sex (*Same sex*). For information on the outcome variables, please refer to Section 3.4.1. All regressions include the following controls: Patient age, square age, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient was born in Spain, whether the patient is covered by the public insurance, the days and squared days passed since the first visit referral, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. See Table 3.2 for further reference. Joint Length p-value is the joint p-value of both *Length* and *Length* × *Same sex*. Standard errors are clustered at the physician level. F-Stat corresponds to the first-stage F-statistics measure proposed by Kleibergen and Paap (2006). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.9: Effect of Visit Length on Visit Outcomes - By Nationality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Length	Length Spanish	Diagnosis	Test	Num. Tests	Test Cost	Drug	Num. Drugs
Length			0.0044**	0.0054*	0.0095*	0.9784**	-0.0000	-0.5015
			(0.0021)	(0.0030)	(0.0057)	(0.4856)	(0.0014)	(0.3364)
Length × Non-Spanish			-0.0024	0.0031	0.0001	-0.5006	-0.0028	0.2617
			(0.0035)	(0.0059)	(0.0096)	(0.8946)	(0.0042)	(0.4364)
Non-Spanish	0.1674	12.2274***	0.0243	-0.0395	-0.0079	5.6797	0.0300	-3.5730
	(0.1450)	(0.5984)	(0.0465)	(0.0754)	(0.1228)	(11.1152)	(0.0487)	(5.5017)
Prior Cancel	1.6390***	0.0538						
	(0.1748)	(0.0677)						
Prior Cancel × Non-Spanish	-0.0507	1.5372***						
	(0.2392)	(0.2804)						
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67530	67530	67530	67530	67530	67530	67530	67530
Joint Length p-value	–	–	0.501	0.0629	0.178	0.468	0.392	0.269
Dep. Var. Mean	–	–	0.0819	0.181	0.286	12.67	0.0333	2.043
F - Stat	–	–	20.59	20.59	20.59	20.59	20.59	20.59

Notes: The reported regressions correspond to the 1st Stage (Col. 1), and the 2nd Stage with multiple outcome variables (Col. 2-8). The table presents the interaction of *Length* and whether the patient was born in Spain (*Spanish*). For information on the outcome variables, please refer to Section 3.4.1. All regressions include the following controls: Patient sex, age, square age, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient is covered by the public insurance, the days and squared days passed since the first visit referral, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. See Table 3.2 for further reference. Joint Length p-value refers to the joint significance of *Length* and *Length* × *Spanish*. Standard errors are clustered at the physician level. F-Stat corresponds to the first-stage F-statistics measure proposed by Montiel Olea and Pflueger (2013). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.10: Effect of Visit Length on Visit Outcomes - By Waiting List

	(1) Length	(2) Length WaitLong	(3) Diagnosis	(4) Test	(5) Num. Tests	(6) Test Cost	(7) Drug	(8) Num. Drugs
Length			0.0061**	0.0109***	0.0153***	0.7824*	-0.0023	-0.4661**
			(0.0025)	(0.0033)	(0.0058)	(0.4064)	(0.0016)	(0.2311)
Length × WaitLong			-0.0091**	-0.0149**	-0.0191**	0.0976	0.0042	0.1840
			(0.0037)	(0.0061)	(0.0097)	(0.5968)	(0.0035)	(0.2255)
WaitLong	-0.7258**	11.6228***	0.1012**	0.1604**	0.1836	-4.6967	-0.0540	-2.7691
	(0.3084)	(0.5002)	(0.0494)	(0.0768)	(0.1196)	(7.3844)	(0.0408)	(2.8035)
Prior Cancel	1.6671***	0.0187						
	(0.1834)	(0.0498)						
Prior Cancel × WaitLong	-0.1346	1.3745***						
	(0.2117)	(0.2154)						
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67530	67530	67530	67530	67530	67530	67530	67530
Joint Length p-value	–	–	0.221	0.370	0.593	0.0860	0.432	0.274
Dep. Var. Mean	–	–	0.0819	0.181	0.286	12.67	0.0333	2.043
F - Stat	–	–	25.17	25.17	25.17	25.17	25.17	25.17

Notes: The reported regressions correspond to the 1st Stage regression (Col. 1-2) and the 2nd Stage with multiple outcome variables (Col. 3-9). The table presents the interaction of *Length* and whether the patient had to wait more than the average service waiting list (*WaitLong*). For information on the outcome variables, please refer to Section 3.4.1. All regressions include the following controls: Patient age, square age, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient was born in Spain, whether the patient is covered by the public insurance, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. Joint Length p-value refers to the joint p-value of *Length* and *Length* × *WaitLong*. See Table 3.2 for further reference. Standard errors are clustered at the physician level. F-Stat corresponds to the first-stage F-statistics measure proposed by Kleibergen and Paap (2006). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.11: Effect of Visit Length on Visit Outcomes - By Specialization Type

	(1) Length	(2) Diagnosis	(3) Tests	(4) Num. Tests	(5) Test Cost	(6) Drugs	(7) Num. Drugs	(8) Follow-up
Panel A: Internal medicine specialization								
Length		0.0055***	0.0060	0.0110**	1.1976**	0.0003	-0.6096	0.0136***
		(0.0015)	(0.0039)	(0.0055)	(0.5922)	(0.0011)	(0.5049)	(0.0034)
Prior Cancel	2.2361***							
	(0.3030)							
Observations	23339	23339	23339	23339	23339	23339	23339	23339
Dep. Var. Mean	15.65	0.0747	0.197	0.278	14.07	0.0295	2.840	0.343
F - Stat	–	55.86	55.86	55.86	55.86	55.86	55.86	55.86
Panel B: Surgical specialization								
Length		0.0022	0.0063**	0.0087	0.4616	-0.0020	-0.2617**	0.0065
		(0.0028)	(0.0028)	(0.0061)	(0.3715)	(0.0016)	(0.1070)	(0.0048)
Prior Cancel	1.3514***							
	(0.1691)							
Observations	44141	44141	44141	44141	44141	44141	44141	44141
Dep. Var. Mean	10.95	0.0857	0.172	0.291	11.94	0.0353	1.624	0.248
F - Stat	–	65.23	65.23	65.23	65.23	65.23	65.23	65.23
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The reported regressions correspond to the 1st Stage (Col. 1), and the 2nd Stage with multiple outcome variables (Col. 2-8). Panel A includes all observations covering visits that happened in an internal medicine specialization, while Panel B includes those that happened at a surgical specialization. For information on the outcome variables, please refer to Section 3.4.1. All regressions include the following controls: Patient sex, age, square age, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient was born in Spain, whether the patient is covered by the public insurance, the days and squared days passed since the first visit referral, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. See Table 3.2 for further reference. The specialties classified as internal medicine are: Allergology, Cardiology, Dermatology, Endocrinology, Internal Medicine, Neurology, Oncology, Pain pathologies, Pulmonology, and Rheumatology; while those specialties classified as surgical are: Cardiovascular surgery, General surgery, Maxillofacial surgery, Ophthalmology, Orthopedics, Otolaryngology, and Urology. Standard errors are clustered at the physician level. F-Stat corresponds to the first-stage F-statistics measure proposed by Montiel Olea and Pflueger (2013). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.12: Visit characteristics by Senior Physicians

	(1) Exp. Visit Length	(2) Overbook	(3) Visits/hour	(4) Overloaded day
Senior Physician	-0.2446	-0.0367***	-0.2967***	-0.0829***
	(0.1983)	(0.0115)	(0.0955)	(0.0268)
Month-Year FE	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes
Specialty FE	Yes	Yes	Yes	Yes
Observations	58301	58301	58301	58301
Dep. Var. Mean	14.93	0.212	4.255	0.240

Notes: The table tests whether senior physicians, measured as those above the median age in the outpatient department, have different type of visits. Exp. Visit Length is a hospital-provided variable that measures how long a given visit should be. Overbook is an indicator variable that identifies those visits that were appointed on the time slot of a prior visit. Overloaded day is an indicator variable that identifies those days in which the total expected visiting time a physician has, exceeds the time he/she is at the outpatient department. All other variables are self-explanatory. Standard errors are clustered at the physician level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.13: Patient characteristics by Senior Physicians

	(1) Male	(2) Age	(3) Ref. BHZ	(4) Dist. BHZ	(5) Chronic	(6) Pub. Cov	(7) Spanish	(8) Waiting list
Senior Physician	-0.0115	0.5690	0.0243	-0.0957	-0.0007	-0.0237	0.0099	-3.8768*
	(0.0093)	(0.3451)	(0.0306)	(0.5525)	(0.0027)	(0.0157)	(0.0168)	(2.0910)
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Specialty FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	58301	58301	58301	58301	58301	58301	58301	58301
Dep. Var. Mean	0.447	58.85	0.598	4.365	0.0582	0.984	0.677	29.73

Notes: The table tests whether senior physicians, measured as those above the median age in the outpatient department, have different type of patients compared to their junior colleagues. Ref. BHZ is an indicator variable that identifies if the patient comes from a Basic Health Zone covered by the hospital. Dist. BHZ measures how many kilometers apart is the patient's Basic Health zone from the hospital using a linear distance algorithm. Chronic is an indicator variable that identifies if the patient previously had any chronic condition. Pub. Cov. identifies if a visit was covered by the public insurance scheme. Spanish identifies those patients born in Spain. Waiting list measures the days that patients wait to access a first visit from their corresponding Primary Care center. All other variables are self-explanatory. Standard errors are clustered at the physician level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.14: Effect of Visit Length on Visit Outcomes - By Seniority

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Length	Length Senior	Diagnosis	Test	Num. Tests	Test Cost	Drug	Num. Drugs	Follow-up
Length			0.0073**	0.0068*	0.0130***	1.2940**	-0.0017	-0.6812*	0.0116***
			(0.0030)	(0.0036)	(0.0049)	(0.5066)	(0.0016)	(0.4093)	(0.0044)
Length × Senior			-0.0075*	-0.0029	-0.0071	-1.1112*	0.0009	0.4865	-0.0064
			(0.0042)	(0.0046)	(0.0091)	(0.6618)	(0.0021)	(0.3965)	(0.0060)
Senior	-0.7348***	11.1466***	0.0473	0.0473	0.1047	17.6521**	-0.0135	-6.2473	0.1078
	(0.2682)	(1.0392)	(0.0523)	(0.0558)	(0.1189)	(7.9696)	(0.0239)	(4.9516)	(0.0729)
Prior Cancel	1.7592***	-0.0574**							
	(0.2560)	(0.0274)							
Prior Cancel × Senior	-0.1712	1.7390***							
	(0.3677)	(0.2621)							
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	58301	58301	58301	58301	58301	58301	58301	58301	58301
Joint Length p-value			0.949	0.223	0.443	0.680	0.598	0.0720	0.245
Dep. Var. Mean			0.0763	0.169	0.266	11.97	0.0384	2.363	0.283
F - Stat			22.05	22.05	22.05	22.05	22.05	22.05	22.05

Notes: The reported regressions correspond to the 1st Stages (Col. 1-2), and to the 2nd Stage with multiple outcome variables (Col. 3-8) and visit length interacted by the physician's seniority. For information on the outcome variables, please refer to Section 3.4.1. All regressions include the following controls: Patient sex, age, square age, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient was born in Spain, whether the patient is covered by the public insurance, the days and squared days passed since the first visit referral, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. See Table 3.2 for further reference. Joint Length p-value is the joint p-value of both *Length* and *Length* × *Senior*. Standard errors are clustered at the physician level. F-Stat corresponds to the first-stage F-statistics measure proposed by Kleibergen and Paap (2006). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.15: Effect of Visit Length on Diagnosis Provision - By Seniority

	(1) Length	(2) Length Senior	(3) Diagnosis	(4) Common	(5) Uncommon
Length			0.0073**	0.0009	0.0064***
			(0.0030)	(0.0014)	(0.0021)
Length × Senior			-0.0075*	-0.0010	-0.0065**
			(0.0042)	(0.0016)	(0.0033)
Senior	-0.7348***	11.1466***	0.0473	-0.0004	0.0476
	(0.2682)	(1.0392)	(0.0523)	(0.0199)	(0.0415)
Prior Cancel	1.7592***	-0.0574**			
	(0.2560)	(0.0274)			
Prior Cancel × Senior	-0.1712	1.7390***			
	(0.3677)	(0.2621)			
Month-Year FE	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	58301	58301	58301	58301	58301
Joint Length p-value	–	–	0.949	0.945	0.959
Dep. Var. Mean	–	–	0.0763	0.0110	0.0653
F - Stat	–	–	22.05	22.05	22.05

Notes: The reported regressions correspond to the 1st Stages (Col. 1-2), and to the 2nd Stage with the following outcomes: i) the probability of a diagnosis (Col. 3), ii) the probability of a common diagnosis (Col. 4), and iii) the probability of an uncommon diagnosis (Col. 5). Diagnoses are classified as common identify those diagnoses most repeated in a given specialization, while uncommon represent any other non modal diagnosis. All regressions include the following controls: Patient sex, age, square age, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient was born in Spain, whether the patient is covered by the public insurance, the days and squared days passed since the first visit referral, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. See Table 3.2 for further reference. Joint Length p-value is the joint p-value of both *Length* and *Length* × *Senior*. Standard errors are clustered at the physician level. F-Stat corresponds to the first-stage F-statistics measure proposed by Kleibergen and Paap (2006). *** p<0.01, ** p<0.05, * p<0.1.

Appendix

3.A Tables

Table 3.A1: Effect of a Prior Cancellation on Visit Length - Time to End Shift

	(1) 6 Hours Length	(2) 5 Hours Length	(3) 4 Hours Length	(4) 3 Hours Length	(5) 2 Hours Length	(6) Last Hour Length	(7) Overtime Length
Prior Cancel	2.5271***	1.9414***	1.6238***	1.6337***	1.7079***	0.6215**	-0.3333
	(0.6106)	(0.2728)	(0.2345)	(0.2409)	(0.2304)	(0.2858)	(1.1706)
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2014	6558	12031	14323	14687	11964	3862
Dep. Var. Mean	13.44	13.29	12.64	12.64	12.18	12.42	10.92

Notes: The reported regressions correspond to the first stage estimation using *Prior Cancel* as the main regressor. The ending time in a given shift is calculated using appointment times. All regressions include the following controls: Patient sex, age, square age, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient was born in Spain, whether the patient is covered by the public insurance, the days and squared days passed since the first visit referral, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. See Table 3.2 for further reference. Standard errors are clustered at the physician level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.A2: Covariate Test - Patient Choice Specializations

	(1) Male	(2) Age	(3) Ref. BHZ	(4) Dist. BHZ	(5) Chronic	(6) Pub. Cov	(7) Spanish	(8) Waiting list	(9) Same sex	(10) Same age
Prior Cancel	-0.0037	0.2639	0.0108	-0.2057	0.0038	-0.0003	-0.0051	1.2624	0.0010	0.0088*
	(0.0054)	(0.2226)	(0.0066)	(0.1277)	(0.0032)	(0.0010)	(0.0055)	(0.9336)	(0.0056)	(0.0052)
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	47832	47832	47832	47832	47832	47832	47832	47832	46652	40682
Dep. Var. Mean	0.465	60.28	0.645	3.812	0.0610	0.991	0.703	31.63	0.526	0.143

Notes: The table tests whether having a prior cancellation predicts the patient and shared physician-patient characteristics, on those specializations in which patients can choose their preferred slot. Ref. BHZ is an indicator variable that identifies if the patient comes from Basic Health Zone covered by the hospital. Dist. BHZ measures how many kilometers apart is the patient's Basic Health zone from the hospital using a linear distance algorithm. Chronic is an indicator variable that identifies if the patient previously had any chronic condition. Pub. Cov. identifies if a visit was covered by the public insurance scheme. Spanish identifies those patients born in Spain. Waiting list measures the days that patients wait to access a first visit from their corresponding Primary Care center. Same sex identifies if both physician and patient share the same sex. Same age identifies if both physician and patient have a similar age, measured using a 10 years window. All other variables are self-explanatory. Standard errors are clustered at the physician level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.A3: Covariate Test - Late Prior Patient

	(1) Male	(2) Age	(3) Ref. BHZ	(4) Dist. BHZ	(5) Chronic	(6) Pub. Cov	(7) Spanish	(8) Waiting list	(9) Same sex	(10) Same age
Prior Cancel	-0.0039	0.2141	0.0060	-0.0497	0.0032	0.0002	-0.0052	1.0555	-0.0032	0.0046
	(0.0045)	(0.1806)	(0.0056)	(0.1495)	(0.0023)	(0.0012)	(0.0047)	(0.7017)	(0.0046)	(0.0042)
Prior Late	0.0009	-0.3156	0.0112	-0.2361***	0.0003	-0.0046**	-0.0016	-0.2962	-0.0096	-0.0011
	(0.0062)	(0.2046)	(0.0068)	(0.0824)	(0.0024)	(0.0020)	(0.0042)	(0.8013)	(0.0059)	(0.0031)
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67530	67530	67530	67530	67530	67530	67530	67530	66350	58301
Dep. Var. Mean	0.447	58.85	0.598	4.365	0.0582	0.984	0.677	29.73	0.517	0.152

Notes: The table tests whether a late arrival of the previous patient predicts the current patient and shared physician-patient characteristics. Ref. BHZ is an indicator variable that identifies if the patient comes from Basic Health Zone covered by the hospital. Dist. BHZ measures how many kilometers apart is the patient's Basic Health zone from the hospital using a linear distance algorithm. Chronic is an indicator variable that identifies if the patient previously had any chronic condition. Pub. Cov. identifies if a visit was covered by the public insurance scheme. Spanish identifies those patients born in Spain. Waiting list measures the days that patients wait to access a first visit from their corresponding Primary Care center. Same sex identifies if both physician and patient share the same sex. Same age identifies if both physician and patient have a similar age, measured using a 10 years window. All other variables are self-explanatory. Standard errors are clustered at the physician level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.A4: Effect of Visit Length and Delay on Visit Outcomes

	(1) Length	(2) Delay	(3) Diagnosis	(4) Tests	(5) Num. Tests	(6) Test Cost	(7) Drugs	(8) Num. Drugs	(9) Follow-up
Length			0.0036**	0.0062***	0.0095**	0.7484**	-0.0005	-0.3482**	0.0105***
			(0.0017)	(0.0024)	(0.0045)	(0.3344)	(0.0009)	(0.1726)	(0.0032)
Delay			-0.0000	-0.0003	-0.0001	-0.0686	0.0006*	0.0764	0.0016*
			(0.0006)	(0.0006)	(0.0010)	(0.0802)	(0.0003)	(0.0686)	(0.0008)
Prior Cancel	1.6256***	-1.1688**							
	(0.1595)	(0.4783)							
Prior Late	0.1356	6.1898***							
	(0.1120)	(0.6744)							
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67530	67530	67530	67530	67530	67530	67530	67530	67530
Dep. Var. Mean	12.58	16.20	0.0819	0.181	0.286	12.67	0.0333	2.043	0.280
F - Stat	–	–	42.39	42.39	42.39	42.39	42.39	42.39	42.39

Notes: The reported regressions correspond to the two 1st Stages (Col. 1-2), and the 2nd Stage with multiple outcome variables (Col. 3-9). Prior Late is an indicator variable that identifies whether the previous patient arrived to the hospital after her scheduled visit time. For information on the outcome variables, please refer to Section 3.4.1. All regressions include the following controls: Patient sex, age, square age, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient was born in Spain, whether the patient is covered by the public insurance, the days and squared days passed since the first visit referral, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. See Table 3.A3 for the corresponding instrument covariate test. F-Stat corresponds to the first-stage joint F-statistics measure proposed by Kleibergen and Paap (2006). Standard errors are clustered at the physician level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.A5: Effect of Visit Length on Visit Outcomes - No Controls

	(1) Length	(2) Diagnosis	(3) Test	(4) Num. Tests	(5) Test Cost	(6) Drug	(7) Num. Drugs	(8) Follow-up
Length		0.0021	0.0066***	0.0098**	0.7940**	-0.0012	-0.4193*	0.0093***
		(0.0022)	(0.0023)	(0.0043)	(0.3484)	(0.0011)	(0.2182)	(0.0032)
Prior Cancel	1.6205***							
	(0.1585)							
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No	No	No
Observations	67530	67530	67530	67530	67530	67530	67530	67530
Dep. Var. Mean	12.58	0.0819	0.181	0.286	12.67	0.0333	2.043	0.280
F - Stat	–	105.8	105.8	105.8	105.8	105.8	105.8	105.8

Notes: The reported regressions correspond to the 1st Stage (Col. 1), and the 2nd Stage with multiple outcome variables (Col. 2-8). For information on the outcome variables, please refer to Section 3.4.1. Standard errors are clustered at the physician level. F-Stat corresponds to the first-stage F-statistics measure proposed by Montiel Olea and Pflueger (2013). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.A6: Effect of a Prior Cancellation on Visit Outcomes - ITT

	(1) Diagnosis	(2) Test	(3) Num. Tests	(4) Test Cost	(5) Drug	(6) Num. Drugs	(7) Follow-up
Prior Cancel	0.0058*	0.0105**	0.0155**	1.3050**	-0.0016	-0.6661*	0.0149***
	(0.0029)	(0.0041)	(0.0072)	(0.5999)	(0.0017)	(0.3433)	(0.0052)
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67530	67530	67530	67530	67530	67530	67530
Dep. Var. Mean	0.0819	0.181	0.286	12.67	0.0333	2.043	0.280

Notes: The reported regressions correspond to the ITT estimation using *Prior Cancel* as the main regressor. For information on the outcome variables, please refer to Section 3.4.1. All regressions include the following controls: Patient sex, age, square age, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient was born in Spain, whether the patient is covered by the public insurance, the days and squared days passed since the first visit referral, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. See Table 3.2 for further reference. Standard errors are clustered at the physician level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.A7: Effect of Visit Length on Visit Outcomes - OLS

	(1) Diagnosis	(2) Test	(3) Num. Tests	(4) Test cost	(5) Drug	(6) Num. Drugs	(7) Follow-up
Length	0.0011***	0.0012**	0.0029***	0.2017***	0.0003***	0.0210**	0.0034***
	(0.0002)	(0.0005)	(0.0011)	(0.0569)	(0.0001)	(0.0100)	(0.0006)
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67530	67530	67530	67530	67530	67530	67530
Dep. Var. Mean	0.0819	0.181	0.286	12.67	0.0333	2.043	0.280

Notes: The reported regressions correspond to the OLS estimation using *Length* as the main regressor. For information on the outcome variables, please refer to Section 3.4.1. All regressions include the following controls: Patient sex, age, square age, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient was born in Spain, whether the patient is covered by the public insurance, the days and squared days passed since the first visit referral, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. See Table 3.2 for further reference. Standard errors are clustered at the physician level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.A8: Effect of Visit Length on Clinical Process Duration

	(1) Length	(2) Case duration	(3) Length	(4) Case duration
Length		1.1678**		0.6699
		(0.5564)		(0.9142)
Prior Cancel	1.6222***		1.8414***	
	(0.1598)		(0.1961)	
Month-Year FE	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	67530	67530	18846	18846
Dep. Var. Mean	12.58	30.13	14.36	108
F - Stat	–	104.3	–	89.24

Notes: The reported regressions correspond to the 1st Stage (Col. 1 & 3), and the 2nd Stage (Col. 2 & 4). The variable Case duration measures the number of days, after a first visit, that has taken a clinical process to end. Columns 1 and 2 use the whole sample and provide a value 0 to those first visits that had no follow-up, and Columns 3 and 4 use only those first visits that scheduled a follow-up visit. All regressions include the following controls: Patient sex, age, square age, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient was born in Spain, whether the patient is covered by the public insurance, the days and squared days passed since the first visit referral, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. See Table 3.2 for further reference. Standard errors are clustered at the physician level. F-Stat corresponds to the first-stage F-statistics measure proposed by Montiel Olea and Pflueger (2013). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.A9: Covariate Test - Follow-up Visits

	(1) Male	(2) Age	(3) Ref. BHZ	(4) Dist. BHZ	(5) Chronic	(6) Pub. Cov	(7) Spanish	(8) Waiting list	(9) Same sex	(10) Same age
Prior Cancel	-0.0071	0.2368	0.0158	0.2433	0.0045	0.0031	-0.0226**	1.9820	0.0043	0.0072
	(0.0096)	(0.4141)	(0.0113)	(0.3040)	(0.0051)	(0.0023)	(0.0108)	(1.2038)	(0.0095)	(0.0087)
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14350	14350	14350	14350	14350	14350	14350	14350	14266	12530
Dep. Var. Mean	0.432	62.16	0.663	4.078	0.0702	0.977	0.696	27.09	0.523	0.134

Notes: The table tests whether having a prior cancellation predicts the patient and the shared physician-patient characteristics in a sample of visits with a follow-up appointments. Ref. BHZ is an indicator variable that identifies if the patient comes from a Basic Health Zone covered by the hospital. Dist. BHZ measures how many kilometers apart is the patient's Basic Health zone from the hospital using a linear distance algorithm. Chronic is an indicator variable that identifies if the patient previously had any chronic condition. Pub. Cov. identifies if a visit was covered by the public insurance scheme. Spanish identifies those patients born in Spain. Waiting list measures the days that patients wait to access a first visit from their corresponding Primary Care center. Same sex identifies if both physician and patient share the same sex. Same age identifies if both physician and patient have a similar age, measured using a 10 years window. All other variables are self-explanatory. Standard errors are clustered at the physician level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.A10: Effect of Visit Length on Visit Outcomes - By Retired Patients

	(1) Length	(2) Length Retired	(3) Diagnosis	(4) Test	(5) Num. Tests	(6) Test Cost	(7) Drug	(8) Num. Drugs
Length			0.0035	0.0043	0.0107	0.7382	-0.0010	-0.4595*
			(0.0029)	(0.0031)	(0.0066)	(0.4982)	(0.0013)	(0.2470)
Length × Retired			0.0000	0.0051	-0.0019	0.1852	-0.0001	0.1144
			(0.0043)	(0.0053)	(0.0095)	(0.7283)	(0.0016)	(0.2279)
Retired	0.2615	12.6102***	-0.0057	-0.0886	-0.0202	-5.4413	0.0006	-1.4926
	(0.1708)	(0.5866)	(0.0569)	(0.0661)	(0.1227)	(9.4129)	(0.0207)	(3.0157)
Prior Cancel	1.5211***	-0.0264						
	(0.1618)	(0.0627)						
Prior Cancel × Retired	0.2585	1.7836***						
	(0.1876)	(0.2516)						
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67530	67530	67530	67530	67530	67530	67530	67530
Joint Length p-value	–	–	0.177	0.0180	0.144	0.0683	0.413	0.142
Dep. Var. Mean	–	–	0.0819	0.181	0.286	12.67	0.0333	2.043
F - Stat	–	–	27.58	27.58	27.58	27.58	27.58	27.58

Notes: The reported regressions correspond to the 1st Stage regression (Col. 1-2) and the 2nd Stage with multiple outcome variables (Col. 3-8). The table presents the interaction of *Length* and whether the patient's age is over 65 (*Retired*). For information on the outcome variables, please refer to Section 3.4.1. All regressions include the following controls: Patient sex, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient was born in Spain, whether the patient is covered by the public insurance, the days and squared days passed since the first visit referral, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. See Table 3.2 for further reference. Joint Length p-value is the joint p-value of both *Length* and *Length* × *Retired*. Standard errors are clustered at the physician level. F-Stat corresponds to the first-stage F-statistics measure proposed by Kleibergen and Paap (2006). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.A11: Effect of Visit Length on Visit Outcomes - By Shock Type

	(1) Length	(2) Diagnosis	(3) Tests	(4) Num. Tests	(5) Test Cost	(6) Drugs	(7) Num. Drugs	(8) Follow-up
Panel A: No-Show								
Length		0.0036**	0.0078***	0.0111**	0.7637*	-0.0010	-0.3939*	0.0084***
		(0.0018)	(0.0027)	(0.0047)	(0.3910)	(0.0010)	(0.2106)	(0.0032)
Prior No-Show	1.6082***							
	(0.1594)							
Observations	66320	66320	66320	66320	66320	66320	66320	66320
Dep. Var. Mean	12.53	0.0817	0.181	0.286	12.63	0.0332	2.055	0.280
F - Stat	–	103	103	103	103	103	103	103
Panel B: Notification								
Length		0.0037	-0.0036	-0.0022	1.1875	-0.0011	-0.5477	0.0149
		(0.0046)	(0.0058)	(0.0097)	(0.8695)	(0.0027)	(0.3340)	(0.0091)
Prior Notification	1.7260***							
	(0.2885)							
Observations	57702	57702	57702	57702	57702	57702	57702	57702
Dep. Var. Mean	12.39	0.0816	0.180	0.286	12.59	0.0319	2.055	0.279
F - Stat	–	36.20	36.20	36.20	36.20	36.20	36.20	36.20
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The reported regressions correspond to the 1st Stage (Col. 1), and the 2nd Stage with multiple outcome variables (Col. 2-8). Panel A includes all observations, but those with a prior withdrawal, while Panel B includes all observations, but those with a prior no show up. For information on the outcome variables, please refer to Section 3.4.1. All regressions include the following controls: Patient sex, age, square age, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient was born in Spain, whether the patient is covered by the public insurance, the days and squared days passed since the first visit referral, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. See Table 3.2 for further reference. Standard errors are clustered at the physician level. F-Stat corresponds to the first-stage F-statistics measure proposed by Montiel Olea and Pflueger (2013). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.A12: Effect of Visit Length on Visit Outcomes - By Overloaded Days

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Length	Length Non Overload	Diagnosis	Test	Num. Tests	Test Cost	Drug	Num. Drugs
Length			0.0043	0.0099*	0.0132*	0.9296	-0.0015	-0.5138*
			(0.0027)	(0.0054)	(0.0079)	(0.5892)	(0.0030)	(0.2643)
Length × Non Overload			-0.0011	-0.0052	-0.0054	-0.1730	0.0007	0.1523
			(0.0037)	(0.0059)	(0.0099)	(0.7140)	(0.0033)	(0.2747)
Non Overload	0.6932***	12.0272***	0.0042	0.0458	0.0549	2.9035	-0.0115	-1.6484
	(0.1776)	(0.5285)	(0.0461)	(0.0715)	(0.1144)	(8.5425)	(0.0393)	(3.3703)
Prior Cancel	1.9344***	0.0763						
	(0.2555)	(0.0887)						
Prior Cancel × Non Overload	-0.4097	1.4005***						
	(0.2570)	(0.2099)						
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67530	67530	67530	67530	67530	67530	67530	67530
Joint Length p-value	–	–	0.184	0.0406	0.140	0.0699	0.434	0.136
Dep. Var. Mean	–	–	0.0819	0.181	0.286	12.67	0.0333	2.043
F - Stat	–	–	27.58	27.58	27.58	27.58	27.58	27.58

Notes: The reported regressions correspond to the 1st Stage regression (Col. 1-2) and the 2nd Stage with multiple outcome variables (Col. 3-8). The table presents the interaction of *Length* and whether the physician had a non-pressing day (*Non Overload*). The variable *Non Overload* identifies those days in which the total expected visit length exceeds the physician's daily schedule. For information on the outcome variables, please refer to Section 3.4.1. All regressions include the following controls: Patient sex, age, square age, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient was born in Spain, whether the patient is covered by the public insurance, the days and squared days passed since the first visit referral, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. See Table 3.2 for further reference. Joint Length p-value is the joint p-value of both *Length* and *Length* × *Retired*. Standard errors are clustered at the physician level. F-Stat corresponds to the first-stage F-statistics measure proposed by Kleibergen and Paap (2006). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.A13: Effect of Visit Length on Visit Outcomes - By High-Performing Physicians

	(1) Length	(2) Length High-Performing	(3) Diagnosis	(4) Test	(5) Num. Tests	(6) Test Cost	(7) Drug	(8) Num. Drugs
Length			0.0023	0.0056**	0.0116**	0.7769**	-0.0003	-0.3945
			(0.0016)	(0.0025)	(0.0053)	(0.3669)	(0.0008)	(0.2833)
Length × High-Performing			0.0041	0.0026	-0.0064	0.0846	-0.0021	-0.0510
			(0.0048)	(0.0052)	(0.0084)	(0.7778)	(0.0026)	(0.3128)
High-Performing	-1.2899	9.3470***	-0.1316**	-0.1999**	-0.2265	-15.6842	0.0448	1.6781
	(1.6542)	(1.8655)	(0.0540)	(0.0800)	(0.1508)	(9.5695)	(0.0435)	(4.8503)
Prior Cancel	1.8436***	-0.0519***						
	(0.2197)	(0.0195)						
Prior Cancel × High-Performing	-0.5610*	1.4276***						
	(0.3258)	(0.2391)						
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67530	67530	67530	67530	67530	67530	67530	67530
Joint Length p-value	–	–	0.156	0.0810	0.434	0.225	0.350	0.0280
Dep. Var. Mean	–	–	0.0819	0.181	0.286	12.67	0.0333	2.043
F - Stat	–	–	18.13	18.13	18.13	18.13	18.13	18.13

Notes: The reported regressions correspond to the 1st Stage regression (Col. 1-2) and the 2nd Stage with multiple outcome variables (Col. 3-8). The table presents the interaction of *Length* and whether the physician's average time used to provide a diagnosis is lower than the average time used in her specialization (*High-Performing*). For information on the outcome variables, please refer to Section 3.4.1. All regressions include the following controls: Patient sex, age, square age, whether the patient is from the reference BHZ, the distance from the patient BHZ to the hospital, whether the patient is a chronic, whether the patient was born in Spain, whether the patient is covered by the public insurance, the days and squared days passed since the first visit referral, whether the visit was forced into the agenda, and whether the visit was referred by a colleague. See Table 3.2 for further reference. Joint Length p-value is the joint p-value of both *Length* and *Length* × *Retired*. Standard errors are clustered at the physician level. F-Stat corresponds to the first-stage F-statistics measure proposed by Kleibergen and Paap (2006). *** p<0.01, ** p<0.05, * p<0.1.