Essays in Behavioral Public Economics

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Summary

This dissertation uses detailed micro data to shed light on issues affecting decision making in the administration of public resources. By helping to understand these dynamics, it informs the design of more effective policies, which ultimately translates into an increase in social welfare. In particular, it studies the role of cognitive limitations and social dynamics on the collection and use of public resources. The analysis of this question is carried out in three separate chapters, using administrative data at the sub-national level in a variety of settings.

The first chapter analyzes the presence of interactions between economic agents on the expenditure side of public finances. Specifically, it studies the presence of fiscal interactions on the expenditure decisions of local governments in Colombia. The next two chapters focus on the revenue side of public finances, and analyze the role of social norms and cognitive limitations on individual compliance in two different settings. The second chapter studies the role of intergenerational transmission of social norms on property tax compliance in Argentina. Finally, the third chapter investigates the effect of salience and information simplification on timely compliance for the payment of speeding tickets in the Czech Republic.

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Preface

In recent decades, there has been a growing interest in empirical research in the field of public economics regarding the implications of behavioral and social traits in the interaction between government and individuals. This change of focus, from a more traditional approach that assumes perfectly rational individuals, has had a strong impact in the way economists understand key issues related to efficiency in the administration of public resources, both on the revenue and expenditure side. In turn, this has opened the door to innovative policy design that addresses these behavioral and social dynamics.

From the revenue perspective, traditional economic analysis regards the compliance decision process of taxpayers as a situation in which individuals trade off their potential benefits, from not paying their due obligations, against the potential costs if they are caught. This approach ignores many aspects of the process that influence compliance choices, and could lead to misleading conclusions. For instance, behavioral biases, such as inattention, may cause individuals to make sub-optimal compliance decisions. Likewise, existing social norms in the decision environment can have a positive or a negative impact on the decisions of individuals depending on the social norms to which they conform.

On the expenditure side, governments decide how to spend public resources coming from taxes and other sources, taking into account their restrictions and objectives. These decisions, however, may be affected by the actions of similar economic actors, i.e. other governments, in the form of fiscal externalities. This is especially relevant for sub-national levels of government, where actions taken by one local entity might affect the decisions of the neighboring jurisdictions. These effects have been widely analyzed on tax setting decisions. Usually, local governments compete

to attract a mobile tax base by lowering their taxes, which leads to a "race to the bottom" on taxes, and thereby an inefficient provision of public goods. In contrast, little is known about the presence of similar fiscal externalities on the expenditure side, which is somewhat surprising, given its implications for efficiency in processes of decentralization.

In this dissertation I use detailed micro data to shed light on these issues affecting decision making in the administration of public resources. By helping to understand these dynamics, I am contributing to inform the design of more effective policies, which ultimately translates into an increase in social welfare. In particular, I study the role of cognitive limitations and social dynamics on the collection and use of public resources. I analyze this question in three separate chapters, using administrative data at the sub-national level in a variety of settings.

The first chapter analyzes the presence of interactions between economic agents on the expenditure side of public finances. Specifically, it studies the presence of fiscal interactions on the expenditure decisions of local governments in Colombia. The next two chapters focus on the revenue side of public finances, and analyze the role of social norms and cognitive limitations on individual compliance in two different settings. The second chapter studies the role of intergenerational transmission of social norms on property tax compliance in Argentina. Finally, the third chapter investigates the effect of salience and information simplification on timely compliance for the payment of speeding tickets in the Czech Republic.

Chapter 1, written with Frank Fossen and Lukas Mergele, analyzes the extent to which public expenditure decisions of a local government affect neighbors' local spending within a wide range of expenditure categories in Colombia. During recent decades, there has been a wave of decentralization reforms in several developing countries. The aim of these reforms has been to give more autonomy to local governments in the administration of resources to improve efficiency. This opens the door for potential local fiscal interactions.

Expenditure interactions may arise due to a variety of reasons, and can have different effects on efficiency. Local politicians may engage in yardstick competition, trying to prove their worth to their electorate by mimicking, or exceeding public

expenditures in neighboring jurisdictions. Local governments can also engage in expenditure competition to attract a mobile tax base through targeted expenditures. These two mechanisms would result in positive fiscal interactions. Furthermore, spillovers may arise because of the "public good" nature of the expenditures. In this case the sign of the interactions differ if the goods are complements or substitutes.

We use detailed data covering different spending categories for a significant number of years to analyze the presence of expenditure spillovers between neighboring municipalities in Colombia. In order to distinguish expenditure interactions from spatial autocorrelation, and thus identify causal effects, we implement an instrumental variable approach that exploits exogenous variation in municipalities' exposure to changes in the world market price of oil, given the municipalities' endowment with oil resources.

We find significant spatial autocorrelations in total municipal spending, and also in most expenditure categories. However, results from the instrumental variable approach reveal that there are no causal fiscal interactions in spending between the municipalities. Our results underscore the importance of using exogenous sources of variation for the identification of spatial spillovers. From an applied policy perspective, the results indicate that decentralization did not lead to a race to the bottom in public expenditures in our setting.

Chapter 2 turns to the revenue side of local finances and analyzes the role of social norms on property tax compliance. In particular, it studies the intergenerational transmission of social norms related to tax compliance in the city of Santa Fe in Argentina. Even though there are numerous studies documenting the persistence over time of various cultural traits, there is scarce evidence of the transmission of social norms between generations. This is the first study analyzing the persistence of social norms across more than two generations. Between the end of the 19th century and the beginning of the 20th century, Argentina registered a significant number of immigrants coming mostly from European countries. Thanks to this particular migration history, today's Argentinian population has a wide range of ethnic origins. In particular, the city of Santa Fe captures this background diversity very well, as it was the destination of a considerable number of migrants.

I exploit the genealogical information contained in the surnames of present-day individuals subject to property tax to identify the country of origin of their ancestors. This information is then linked to administrative records on the amount and timing of individual property tax payments and measures of wealth. This allows me to study differences in compliance and economic performance by taxpayers' country of descent. Furthermore, I make use of information on indicators of tax morale and unlawful behavior in the country of origin of the taxpayers' ancestors. With this, I explore correlations with present-day compliance of individuals with roots in those countries.

Results document significant differences in property tax compliance between individuals with ancestry from a specific group of countries, indicating persistence of social norms related to tax compliance. These results are robust to a wide range of estimations from different specifications and within various periods of time. In contrast, I do not find evidence of significant correlations between property tax compliance and contemporary indicators of tax morale and unlawful behavior in taxpayers ancestors' country of origin. This is consistent with the convergence of social values over time for groups of individuals with different ancestry in Argentina. It is also in line with immigrants being self-selected on unobservables. Since they are systematically different than the individuals that stayed in their country of origin, so are the set of social norms that they transmit to future generations.

Chapter 3 was written with Libor Dusek and Christian Traxler. This chapter, like Chapter 2, is related to the collection of revenue for local governments. It studies the role of cognitive limitations on individuals' choices regarding compliance with payments in the presence of a deadline. Specifically, we analyze the effect of salience, simplification, and economic incentives on compliance for speeding tickets.

We first explore individuals' payment choice in a simple theoretical framework. Individuals receive a payment notification and decide if and when to pay. Paying after a deadline triggers an additional penalty which implies higher costs. Due to the complexity of the payment notification, however, both the payment deadline itself and the increased costs may not be fully salient. We discuss that a simplified notification – that makes the deadline and the additional penalty more salient – can increase timely payments. Effects from the letter depend, however, on the extent

and direction of the individuals' mis-perception of the deadline and the penalty for missing it.

To test this prediction, we conduct a field experiment with of the universe of speeding tickets recipients in a suburb of Prague, Czech Republic. The experiment randomly varies the inclusion and content of a complementary cover letter attached to the regular payment notification sent to speeders. All cover letters simplify the main information from the legal text. In addition, they increase either the salience of the payment deadline, the consequences of missing it, or both attributes. This setup allows us to analyze the effect of the salience of these attributes on the individuals' payments. In a next step, we further implement a regression discontinuity design to identify how increasing the fine and the late-pay penalty affects payment behavior.

Results from the randomized control trial document a positive effect from simplification and penalty salience. Making the additional penalties for late payments more salient increases timely compliance. Jointly increasing the salience of the deadline and the penalty produces a larger effect on timely payments, and a persistent effect on payments in general. Outcomes from the regression discontinuity design indicate that increasing the size of the fine and the penalty for paying late reduces payments before the deadline. A comparison of the absolute value of the effects from both exercises reveals that changes in monetary incentives have a larger impact than the salience intervention. From an applied policy perspective, the findings indicate that information simplification and increasing the salience of decision-relevant attributes is a cost-effective enforcement policy.

Chapter 1

Fueling Fiscal Interactions: Commodity Price Shocks and Local Government Spending in Colombia

1.1 Introduction

In the past four decades, decentralization has been a focal point of policy reform in many developing countries, including South American nations. Policy advisors such as the World Bank and the OECD have advocated the delegation of more fiscal autonomy concerning both public expenditures and revenues away from the central government to local governments, including municipalities. More autonomy of local governments opens up the possibility of local fiscal interactions.

To fully understand the consequences of decentralization, reliable estimates of the extent of strategic fiscal interactions of local governments are crucial, because strong interactions may imply externalities and therefore inefficiencies (e.g., Caldeira *et al.*, 2014). On the revenue side, tax competition has received considerable interest and raised concerns that uncoordinated fiscal autonomy may lead to a race to the bottom of tax revenues. Far less is known about local fiscal interactions on

the expenditure side, especially in a developing country context.[1] Strategic competition in expenditures may differ from strategic tax competition (e.g., Wildasin, 1988), so existing empirical evidence on tax interactions does not necessarily carry over to expenditure interactions. Despite the scarcity of evidence, local expenditure spillovers are frequently discussed in policy debates. For example, local policymakers are often concerned that too generous local welfare spending will draw in welfare migrants from surrounding regions (see Figlio *et al.* (1999) for an analysis of welfare competition at the level of US states). Neighboring regions may then save public funds at the expense of the local taxpayers of the focal jurisdiction. Such considerations may lead to an underprovision of certain public goods if local governments are responsible.

Expenditure interactions may also arise if local policymakers engage in yardstick competition and mimicking of peers.[2] Understanding the way local governments interact in terms of public expenditures is key for implementing development policies which aim to improve local public spending. If local policymakers mimick their neighbors, it might be effective to roll out an innovative budget composition in a local jurisdiction that is used as a benchmark, i.e. that is central in the spillover network. Then the expectation is that the policy spreads out to other regions. If public expenditures are strategic complements, decentralized foreign aid to one municipality may entail fiscal efforts in connected regions and trigger a multiplier effect (Caldeira *et al.*, 2014; Glaeser *et al.*, 2003). Public expenditure interactions may be very different for different types of expenditures. Therefore, it is important to distinguish between various categories of local expenditures.

In this paper, we analyze spatial interactions of different types of local public expenditures among municipalities in Colombia. Our data covers the universe of the more than 1000 Colombian municipalities over eleven years from 2000 to 2010 and distinguishes between the eleven most important expenditure categories.

[1] As an illustration, Google Scholar finds 30,000 papers for "tax competition" but only around 600 results each for "expenditure competition" or "spending competition" (September 2016).

[2] The website of the mid-sized municipality Fresno in Colombia suggests that policymakers compete locally: "Municipalities [...] must develop integral systems to generate resources as well as social and economic development based on three basic variables: competitiveness, productivity and employment. Competitiveness is understood as the capacity of the municipality to differentiate itself from the surrounding municipalities to attract investments." `http://fresnoposible.fresnodigital.info/?page_id=56` (accessed 09/10/2016)

Studying Colombia is highly informative because the country has undergone fiscal decentralization reforms similar to those in many countries in South America and the developing world. Colombia is also similar to many Latin American and other developing countries in that they collect substantial royalties for the extraction of natural resources and partly allocate them to subnational governments.

The methodological challenge in the literature on spatial spillovers in general and local fiscal interactions in particular is the identification of causal spatial interaction effects. These must be separated from spatial autocorrelation that is due to spatially clustered unobservable factors (Manski, 1993; Gibbons and Overman, 2012; Revelli, 2015). For identification, we exploit the fact that Colombian municipalities receive royalties that depend on revenues from local oil extraction by private companies. Higher local oil revenues thus relax the budget constraint of a municipality and allow an increase in local public spending. To identify expenditure spillovers, we use exogenous variation in the exposure of municipalities to changing world market prices for oil due to their endowments with oil resources (as measured before our time period of analysis). With our spatial panel instrumental variable (IV) estimator, we control for municipality fixed effects as well as department-specific time fixed effects. This accounts for any cross-sectional differences between municipalities with and without oil resources as well as regional business cycle effects while leaving changes in world market prices of oil for exposed municipalities for identification. World market prices for oil are arguably exogenous for Colombia as a relatively small oil extracting country, which is not a member of OPEC, and certainly for Colombian municipalities.

Our results indicate that spatial interactions of total local public expenditures as well as local spending in almost all categories are small and not significantly different from zero when identifying the spillover effects based on quasi-experimental variation. The only exception is public spending for sport and recreation, where we detect significant spatial interactions that can be interpreted as causal. In contrast, when we use a spatial panel estimator without our additional source of exogenous variation, as done in most of the literature, the results suggest large and significant spatial autocorrelation in total local expenditures and almost all spending categories, even when controlling for municipality and time fixed effects. However, most of these

effects are spurious, as we show by comparison to the consistent estimation based on the quasi-experimental instrument. Thus, the main methodological insight from our analysis is that it is crucial to use additional sources of exogenous variation for the identification of spatial fiscal interaction effects in a quasi-experimental approach.[3]

As spatial interactions are insignificant for total local public spending and most expenditure types, the main policy insight from our paper is that policymakers do not need to be overly concerned about a race to the bottom regarding local public expenditures when pursuing decentralization reform in a developing country. On the other hand, one should also not expect that innovative local expenditure policies spread out through mimicking in substantial ways. A potential caveat is that transfers and royalties that municipalities in Colombia receive are partly earmarked for spending in specific categories. This might limit the scope for expenditure interactions in Colombia, although we discuss below that Colombian local policymakers seem to have significant leeway.

Our approach and results contribute to a small body of emerging literature that employs quasi-experimental sources of variation for identification in the context of spatial fiscal interactions. Concerning expenditure spillovers, Baicker (2005) uses variation in federally-mandated increases in Medicare spending at the state level in the United States, and Isen (2014) employs referendum decisions in counties and municipalities in Ohio. While the former author reports considerable spatial spillovers, the latter does not find any significant interaction effects using his identification strategy. We largely confirm the last result in a developing country context and based on a very different source of exogenous variation, but also show that causal fiscal interaction effects can be identified for the category of sport and recreation.

In the context of tax competition, the literature using quasi-experimental approaches has developed more rapidly. Lyytikäinen (2012) uses changes in minimum property tax rates in Finland for identification, and Baskaran (2014) a fiscal equalization reform in Germany. Both authors report that seemingly large tax interactions become insignificant when using quasi-experimental variation for identification, similar to the result of Isen (2014) concerning public expenditures. Other papers that identify fiscal effects at borders of higher level jurisdictions or language regions re-

[3]Spatial discontinuity designs also fulfill this request.

port significant strategic tax interactions (Eugster and Parchet, 2013; Parchet, 2014; Agrawal, 2015; Agrawal, 2016). It remains unclear whether the inconclusiveness in the spatial tax interactions literature using additional exogenous variation is due to the different institutional settings analyzed or whether it can be explained by differences between the quasi-experimental and spatial discontinuity approaches. Revelli (2015) mentions that by dropping municipalities with low tax rates from his analysis, Lyytikäinen (2012) may have excluded the municipalities most likely to respond to tax competition pressures, which could explain his finding of no interactions.[4]

Most of the literature on spatial fiscal interactions does not use quasi-experimental variation. While that literature is fairly large in the context of tax competition, not many papers exist on public expenditure spillovers. Case *et al.* (1993) provide an analysis at the level of US states. Analyzing spillovers at the municipality-level provides a much larger policy variation compared to the state- or department-level mostly employed in the literature (e.g., Baicker, 2005). Moreover, most papers only analyze few expenditure categories or solely focus on single budgetary items such as health expenditures (Moscone *et al.*, 2007). Borck *et al.* (2007) are among the few authors who use municipality data and distinguish between various spending categories, although only cross-sectionally.

The literature on fiscal interactions largely ignores developing countries since large and complete fiscal policy datasets rarely exist in these regions. It is important to investigate developing countries separately because they are in the focus of decentralization reform efforts. Additionally, they differ from developed countries in their forms of decentralization and institutional as well as budgetary constraints. Akin *et al.* (2005) investigate health care budgets in less than 30 districts in Uganda

[4]To further investigate whether the identification approach influences results within the same country and institutional setting, it would be interesting to compare our approach using quasi-experimental variation between municipalities with an approach using spatial discontinuities at the borders of higher-level jurisdictions. In principle, Colombia could be suitable for such a comparison as departments also receive royalties from the exploitation of natural resources, potentially providing a spatial discontinuity setting. However, institutional details complicate such an analysis because departments near oil producing departments may also receive a share of the royalties ("*escalonamiento*"). At the municipal level, a similar mechanism rarely applies and if so, it affects all municipalities within the same department. In our approach we can account for this by using a control variable (transfers) and department-time fixed effects. Furthermore, departments have much less autonomy than municipalities regarding taxes and public expenditures in Colombia (Bird, 2012). Given the institutional and data issues, a spatial discontinuity approach is not within the scope of this paper, but might be an interesting avenue for future research.

and Arze del Granado *et al.* (2008) local public spending based on cross-sectional data from Indonesia. Agostini *et al.* (2016) and Yu *et al.* (2016) analyze spatial spending interactions in China. Caldeira *et al.* (2014) use a panel of 77 communes in Benin and report strategic complementarity of local public spending. The significant fiscal externalities found in Caldeira *et al.* (2014) question the efficiency of decentralization reform in developing countries. In contrast to our paper, the existing literature in the developing country context does not use quasi-experimental variation for identification and may therefore overestimate expenditure interactions.[5]

Our paper is also related to studies using an identification strategy similar to ours based on Colombian data. Martínez (2016) shows that local governments in Colombia perform better in providing public goods if more revenues come from their own taxes rather than oil royalties. He exploits exogenous variation in oil revenues for identification. Dube and Vargas (2013) investigate effects of income on civil conflict in Colombia using commodity price shocks.

This paper proceeds as follows: The next section reviews the theoretical literature, while Section 1.3 introduces the relevant Colombian context. Sections 1.4 and 1.5 present the data and empirical strategy before we discuss our results in Section 1.6. Section 1.7 concludes the analysis.

1.2 Theory of expenditure interactions

There are at least four different reasons why expenditures might be linked among municipalities. First, yardstick competition (Shleifer, 1985; Besley and Case, 1995) may induce local politicians to mimic their neighbors' policies. This is because voters can assess the quality of their incumbent politician only by observing policies in neighboring jurisdictions. The theory was originally developed to explain spillovers in local tax-setting and has been subject to vast empirical tests (see Allers and Elhorst (2005) for an overview). The mechanism may also apply to local public spending and result in positive interactions among local expenditures. Yardstick competition might be particularly relevant in developing countries with elected local

[5] Yu *et al.* (2016) find spatial interactions within, but not across Chinese provinces, which supports their conclusion that Chinese local leaders engage in tournament competition.

governments like Colombia because information problems are likely to be comparably severe.[6]

Second, expenditures may be linked due to competition for attracting a mobile tax base as suggested by Tiebout-type models (Tiebout, 1956). For instance, municipalities could compete for company settlements or highly-skilled workers (Borck *et al.*, 2007) by investing in their communication infrastructure or education system. In this case, the interjurisdictional spending is positively correlated. The classic model of fiscal spillovers by Zodrow and Mieszkowski (1986) shows how local governments competing for mobile capital will underinvest in public services. Keen and Marchand (1997) argue that non-coordinated spending of local governments under tax competition may be downward biased relative to the efficient level. They also posit that the underprovision of public consumption-oriented investment compared to public production inputs is relatively more pronounced. This is because capital is assumed to be more mobile than households and thus receives a more favorable treatment by the government.

A third explanation for expenditure interactions is the externality-producing nature of public investments (Case *et al.*, 1993), which may either be substitutes or complements to spendings in other regions. The sign of the correlation implied by this channel is ambiguous: On the one hand, a neighboring hospital might be sufficient to fulfill the regional demand for health services, making additional investment in another hospital obsolete. Investments then are substitutes and expected to be negatively correlated among neighbors. On the other hand, if a road construction project is supposed to link two municipalities, public local investments are likely to be complementary and positive expenditure interactions are expected. Hence, investments likely differ in their external effects, which is why we study different spending categories separately.

A fourth potential mechanism that could lead to horizontal interactions between municipalities is competition for bailouts (Baskaran, 2012). Municipal governments may believe that a higher level government authority might bail them out in case of an imminent insolvency, but that a budget constraint at this higher level might limit

[6]Faguet and Sánchez (2013) conclude that decentralization reform has made Colombian mayors more accountable.

the scope for bailouts. A municipality's chances of a bailout and thus incentives to spend and incur debt then depend on other municipalities' expenditure and debt levels, which would lead to horizontal interactions. Municipalities in Colombia are allowed to incur debt, and this led to municipal debt problems in the 1990s. However, reforms in 1997 and 2000 introduced strong borrowing and bailout restrictions for subnational governments, which led to a sizable municipal debt reduction (Villar *et al.*, 2013). Therefore, bailout expectations may not be as relevant in Colombia in our period of analysis as they might have been before.

The first two mechanisms discussed above would lead to positive interactions in local expenditures. The third and forth mechanisms would lead to nonzero interactions, but the sign is ambiguous in these cases. Using data from German federal states, Baskaran (2012) estimates that bailout competition leads to positive interactions as well.

Fiscal competition in developing countries may differ from the case of developed economies usually studied. Caldeira *et al.* (2014) emphasize that poor municipalities in developing countries may be restricted in their spending choices. They develop a model of expenditure competition with a constrained Nash equilibrium. One implication of the model is that there may be *no* strategic interactions despite positive externalities if the level of fiscal resources is insufficient. However, for the case of Benin, the authors empirically find significant positive strategic interactions. Colombia is a developing country (GDP per capita: US$6,056), but less constrained than Benin (GDP per capita: US$762)[7], so we expect to find nonzero fiscal interactions in Colombia as well.

Spatial expenditure reactions may depend on the source of funds used to finance local spending. In Colombia, transfers municipalities receive from the central government as well as royalties from the extraction of natural resources are partially earmarked for spending in specific categories such as education, as detailed in the next section. Basic theory suggests that earmarking should not have practical relevance, because local governments can reallocate other funds and adapt other tax and spending decisions to offset the intended allocation. Thus, a block grant nominally earmarked for a certain category should have the same effect as a general increase in

[7]Figures for 2015, given in current USD and retrieved from World Bank (2017).

local taxpayers' income of the same amount (see Smart, 2007, for an overview on the incentive effects of grants). However, the empirical literature provides evidence that block grants are disproportionately used for public spending rather than for tax cuts and also tend to stick to the spending category they are intended for, a phenomenon known as the flypaper effect (e.g. Hines and Thaler, 1995; Inman, 2008). If earmarking in practice limits the freedom of municipalities in their spending decisions, this may attenuate the scope for expenditure interactions between municipalities in Colombia.

In summary, from the theory we expect to find nonzero and most likely positive expenditure interactions between municipalities in Colombia, although interactions might be smaller than in fully developed economies or in settings with fewer institutional constraints.

1.3 Fiscal policy in Colombian municipalities

Colombia has been a politically, administratively and fiscally centralized country throughout most of its history. However, as many other Latin American countries, Colombia phased in important policies towards decentralization starting from the mid 1980s (Acosta and Bird, 2005; Alesina *et al.*, 2005; Chaparro *et al.*, 2005). The goal of the reforms was to delegate more functions to lower tiers of government, namely Colombia's 32 departments and more than 1000 municipalities. To this end, in 1991, a new constitution introduced a number of provisions regarding the delegation of administrative and fiscal duties from the central to the lower tiers of government. Among them were increases in the amount of transfers to local and regional governments and rules on how to spend these resources. In 2001, a unified system to transfer the resources to the lower tiers of government called *General System of Participations* (SGP by its initials in Spanish) was created.

In fiscal terms, these reforms have made the municipal sector the most important subnational level of government in Colombia (Acosta and Bird, 2005). Municipalities possess three main sources of income. First, municipalities obtain the aforementioned transfers from the central government. Second, municipalities have their own revenues coming from local taxes, mainly the property tax and the ICA tax (a

tax on industrial and commercial activities). Third, municipalities receive royalties from the exploitation of natural resources. Each source accounts roughly for 50%, 30%, and 5% of the total amount received by the municipalities respectively (Bonet *et al.*, 2014). The remaining revenue comes from non-tax income such as capital dividends, leased property or fees from construction permits.

Royalties from oil extraction are particularly relevant for this analysis as we use them as our source of exogenous variation. In Colombia, private companies extract oil and pay a fixed share of their oil revenues (in Colombian Pesos) as royalties according to this formula:

$$Royalty_{i,t} = output_{i,t} \times world\ market\ price_t \times exchange\ rate_t \times fixed\ royalty\ rate \tag{1}$$

The municipalities where the oil was extracted (as well as oil ports) receive a share of these royalties according to a fixed schedule defined by law with rates decreasing in local oil output. For those municipalities extracting oil or with an oil port, royalties are very important, accounting on average for 23% of their total revenues.[8] Oil is the most important natural resource in Colombia and accounts for 69% of total royalties, followed by coal with 23% (Martínez, 2016).

There are several rules on how to spend the revenues from the different sources of income. According to a law introduced in 2001[9], 4% of the federal SGP transfers were to be used for special purposes like food programs in schools or indigenous reservations. Of the remaining 96%, 58.5% had to be used for education, 24.5% for health, and 17% for a general purpose category, including water and sanitation, housing, and agricultural investments, among other items. In 2007 a new change in the legislation[10] gave continuity to the transfers' system, marginally modified the growth rate of the transfers to each category, separated water and sanitation from

[8]We use the term royalties to refer to direct royalties, i.e. royalties that directly go to the oil extracting municipalities. An oil extracting municipality with average (or median) oil output receives 32% of the total local oil royalties and the department receives 52% in this case. Another 8% are allocated to municipalities with oil harbors. The National Royalty Fund distributes the remaining oil royalties (indirect royalties) to municipalities that apply for specific investment projects and get approved (Bird, 2012). Our control variable "transfers" also accounts for these indirect royalties.

[9]Law 715, 2001

[10]Law 1176, 2007

the general purpose category and assigned it 5.4% (leaving 11.6% for general purposes). After the allocation of the transfers to the different categories, the ministries in charge distribute them between the municipalities. In terms of own revenue, municipalities have almost complete freedom to spend the money they collect in taxes. The only provision in this regard is a minor mandatory contribution to the regional environmental agency.

Concerning expenditure of direct royalties from the exploitation of natural resources, legislation mandates that municipalities spend at least 75% in the achievement of coverage goals for health and child mortality, education, and water and sanitation.[11] In the following, we refer to the corresponding spending categories health, education and water as earmarked categories. If all the goals are achieved, local governments can use all the royalty revenues as desired.

The rules on how certain revenue types have to be spent could restrict spending decisions and limit the potential for expenditures interaction across municipalities (Bird and Smart, 2002). However, the evidence shows that local governments in Colombia possess considerable leeway in the composition of their spending. Drazen and Eslava (2010) demonstrate that incumbents increase expenditure in public goods that are more visible to citizens in a bid to increase their vote shares before elections. According to Sánchez *et al.* (2004, p. 3), municipalities may relocate their own-source revenues to circumvent conditionalities and conclude that "there is considerably more fungibility in grants than a strict reading of the law would suggest". Thus, municipalities can direct some of their investments according to their own desires, leaving the door open for expenditure interactions between local governments.

Nevertheless, according to Perotti (2005), the central government's attempt to force municipalities to spend disproportionately more on categories such as health and education has prevented local governments from using resources in other areas like social programs that might have had a larger impact on local poverty reduction and therefore on the welfare of individuals. By identifying categories of public spending where local governments in Colombia compete, our paper informs policymakers which spending areas local governments are focusing on, in turn revealing

[11] The minimum coverage goals for health, child mortality, education, and water are respectively: 100% of health insured population, less than 1% of child mortality before reaching the second year of life, 90% of school enrollment, and 70% of the population with access to clean water.

local electorates' preferences.

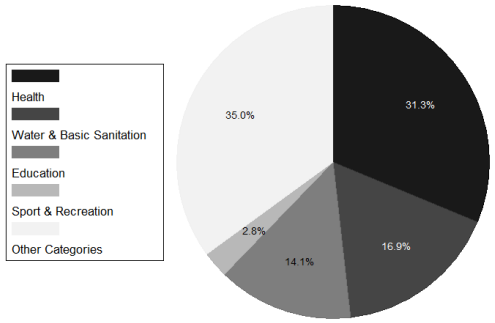
The case of Colombia is informative in a more general context because many developing countries, especially in Latin America, implement similar institutions regarding royalties from natural resources and their allocation to subnational governments. According to Viale and Cruzado (2012), Bolivia, Brazil and Peru allocate a share of income of extractive industries directly to subnational governments, as Colombia does. Ecuador and Venezuela also use payments from the exploitation of natural resources to compensate producing regions. In all the countries mentioned, there are restrictions on how the income from the extraction of natural resources may be used. Otto *et al.* (2006) note that it is standard practice in most African and Asia-Pacific countries to levy royalties on mineral extraction.

1.4 Data

To pursue our analysis of spatial interactions in municipal spending, we require four types of data: Public spending data at the local level, varying over time and across municipalities; information on municipalities' oil extraction; time-series data on world market oil prices; and cross-sectional geo-information on the municipalities' locations to construct spatial weighting matrices.

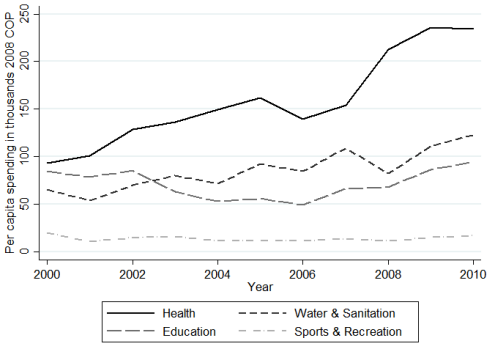
We retrieve local public spending data from the Center for Studies on Economic Development (CEDE) of the University de los Andes, which in turn collected the information from government agencies. The data contain a wide range of government-related variables from the full universe of Colombian municipalities over the years 1993 to 2013. This dataset has previously been used to analyze the effects of fiscal decentralization (Soto *et al.*, 2012), income shocks (Dube and Vargas, 2013) and political stability (Acemoglu *et al.*, 2013b), among other studies. As full coverage is not given for all years and to avoid large differences in the institutional setting, we focus on the period between 2000 and 2010. This results in a strongly balanced panel of 1093 municipalities over eleven years. The data allow us to differentiate between eleven major spending categories at the municipality level. We express all variables in real per capita Colombian Pesos (COP) with the base year 2008 if not stated otherwise.

Figure 1.1a shows the largest expenditure categories in terms of their average shares in total local public spending. About 31% of a municipalities' expenditures go into the area of *health*. Spending in *water and basic sanitation* as well as in *education* account for another 17% and 14%, respectively. The remaining funds are spent on *sport and recreation*, *housing*, *attention to vulnerable groups*, *municipal facilities*, *agriculture*, *disaster prevention*, *community development*, *justice and security* as well as further smaller categories. Figure 1.1b shows that there was an increase in local real expenditures since the year 2000, mirroring the Colombian decentralization process. Table 1.7 in Appendix 1.8 describes the spending categories in more detail and provides some examples.



Note: Relative shares of spending types per capita, averaged over 2000-2010.

(a) Relative shares of expenditure categories



Note: Annual averages across categories, 2000-2010.

(b) Expenditure levels over time

Figure 1.1: Local public expenditures of Colombian municipalities

The data are very complete and have an exceptionally high quality for a developing country context. We did not detect systematic trends of missing values and had to impute less than 3% of the observations by linear intrapolation (see Appendix 1.8). From CEDE, we also retrieve the total transfers to municipalities from the central government and other sources and the municipalities' total and rural population.

We retrieve information on oil extraction by private companies within a municipality from the Ministry of Finance. To get an exogenous indicator of oil extraction, we create a binary variable equaling one if oil was extracted on a municipality's soil at any time during the years 1990 to 1999, the decade preceding our analysis.

To obtain a measure of the value of the oil extracted, we use the oil basket price

in USD provided by OPEC. According to Colombian law,[12] the exchange rate used to calculate the value of the royalties is the average of the daily official exchange rate between the USD and the COP calculated by the Financial Superintendence of Colombia. Thus, we convert oil prices into COP applying the official exchange rate. Finally, we collect the cross-sectional geographical information on municipalities from the Colombian Geographic Institute Agustin Codazzi (IGAC).

Table 1.1 summarizes descriptive statistics of the major variables from the final sample. Figure 1.2 depicts the spatial distribution of local total public expenditures in Colombian municipalities and suggests spatial clustering. In our econometric analysis we investigate whether this is due to causal expenditure interactions.

Table 1.1: Descriptive Statistics

	Mean	Std. dev.
Total Expenditures	507.92	441.15
Health	158.87	89.92
Water and Basic Sanitation	85.84	137.99
Education	71.46	101.10
Housing	14.05	32.87
Sport and Recreation	13.99	23.53
Agriculture	12.77	22.40
Municipal Facilities	12.32	29.90
Attention to Vulnerable Groups	10.60	17.08
Justice and Security	5.59	12.45
Disaster Prevention	3.99	14.21
Community Development	1.72	7.94
Federal Transfers	390.42	268.46
Oil production status (1990-99)	0.06	0.24
Population in thousand inhabitants	39.08	235.98
Rural population share	582.27	236.14

Notes: The table provides averages over the entire sample period 2000-2010. Expenditures and transfers are real per capita values in thousand 2008 COP. $N = 1093$ municipalities.

[12]Law 141 of 1994, law 756 of 2002.

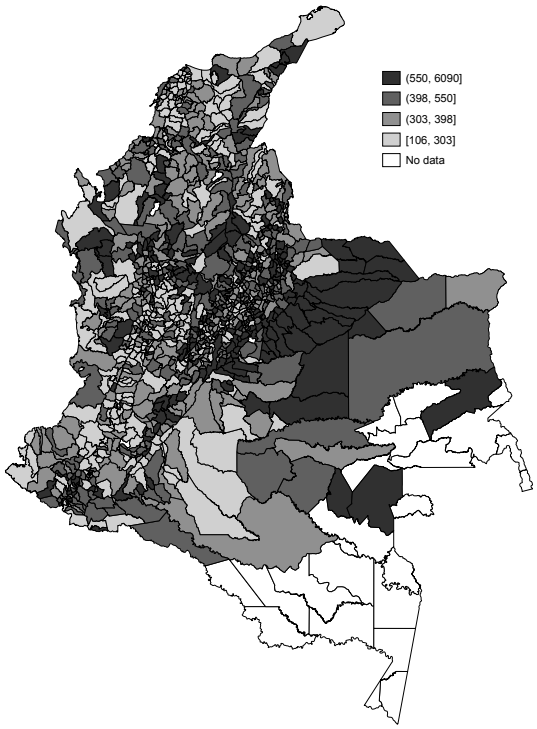


Figure 1.2: Total expenditures per capita in thousand COP by quartiles in 2005. In all the figures, "no data" refers to entities not used in our analysis, see Appendix 1.8.

1.5 Empirical strategy

1.5.1 Model of spatial expenditure interaction

To estimate the effect of neighboring municipalities' expenditures on the spending of the focal municipality, we consider the following model:

$$Y_{i,d,t} = \delta WY + \eta\ oil_i \times p_t^{oil} + WX\theta + X_{i,t}\beta + \alpha_i + \tau_{d,t} + \varepsilon_{i,d,t} \tag{2}$$

where $Y_{i,d,t}$ is the natural log of local public spending per capita (total spending or one of the spending categories) in municipality i of department d in year t. WY denotes the spending in neighboring municipalities (total or in the respective category), where W is a spatial weighting matrix. The coefficient of interest is the spatial autocorrelation coefficient δ, which measures potential spatial interaction effects. The dummy oil_i equals 1 if oil was exploited within a municipality between 1990 and 1999. p_t^{oil} is the natural log of the world market price of oil converted to real COP. The interaction of the previous two variables captures differential effects

of oil price changes on oil endowed and non-endowed municipalities primarily due to the receipt of royalties. The vector $X_{i,t}$ collects additional time-variant covariates: Total spending of a municipality on all categories other than the focal category Y, population of the municipality and its square, the local share of the rural population, and transfers from higher levels of government (including indirect royalties). We also include the spatial lags WX of all X variables (Spatial Durbin Model). This accounts for the possibility that changes in neighboring municipalities such as population growth directly influence a focal municipality's spending. Municipality fixed-effects α_i capture time-invariant unobserved factors such as the distance to the capital, geographical size and climatic conditions. Department-year fixed effects $\tau_{d,t}$ control time variation that affects all municipalities within a department in the same way such as regional economic shocks and changes in department-level regulations and funding.

1.5.2 Oil price shocks as exogenous variation in local spending

In Equation (2), WY is endogenous due to the simultaneous influence of neighboring municipalities on one another. Furthermore, unobserved factors that change over time with variation within departments are contained both in WY and the error term $\varepsilon_{i,d,t}$. To deal with this endogeneity, our empirical strategy employs an instrumental variable approach that isolates exogenous variation in municipal spending.

Similar to Acemoglu *et al.* (2013a), we exploit oil price shocks that affect the finances of some but not all municipalities. We do so by combining information on oil endowments of municipalities with variation in oil prices on the world market over time to extract quasi-experimental variation in spending changes of neighboring municipalities.

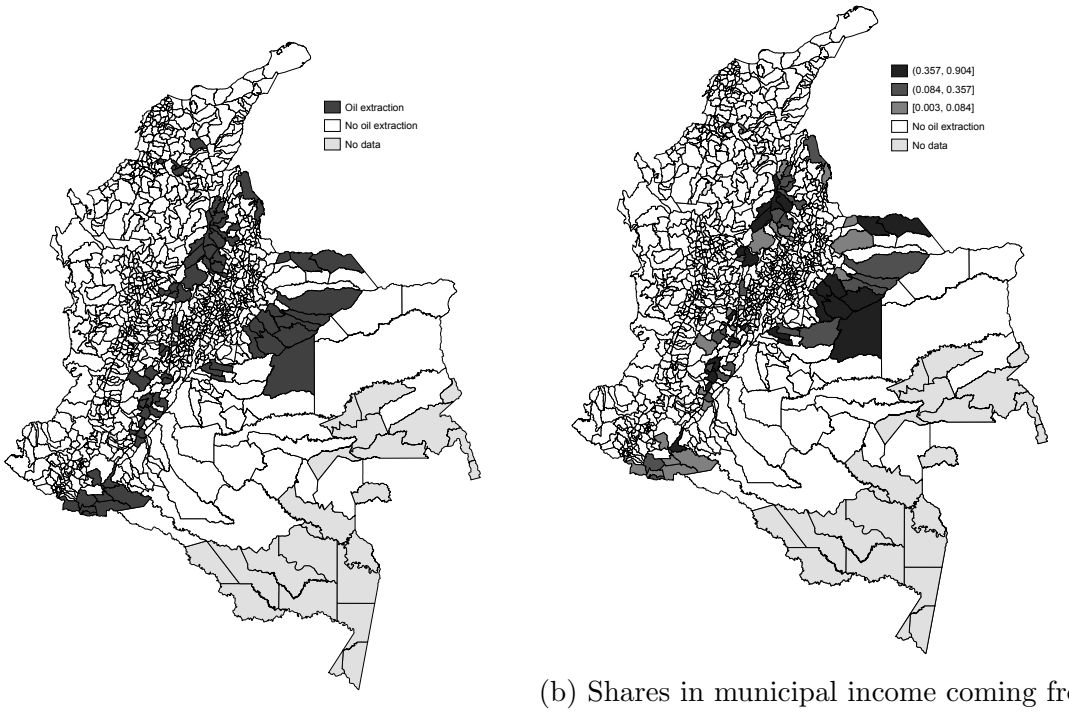
In the 2SLS estimation, the first stage is given by

$$WY = \gamma\ oil_i \times p_t^{oil} + Woil \times p_t^{oil}\lambda + WX\kappa + X_{i,t}\rho + \alpha_i^f + \tau_{d,t}^f + \vartheta_{i,d,t} \qquad (3)$$

where neighboring oil endowment (in the 1990s, before the period of analysis) inter-

acted with current international oil prices in real COP ($Woil \times p_t^{oil}$) is used as the instrument for neighboring spending.

The validity of the instrument relies on two requirements. First, the combination of oil endowment and oil prices must be correlated with local spending (instrument relevance). Oil prices are linked to local spending as Colombian municipalities receive royalties from oil extracted on their soil, relaxing their budget constraint. Federal law mandates royalty payments depend on extracted quantities as well as current world market prices (see Section 1.3). Figure 1.3 suggests that oil production in the 1990s (Panel a) corresponds well with oil royalty income in the middle of our period of analysis (Panel b). Therefore we expect the instrument to be relevant for total local public expenditures and its components. To obtain a strong instrument, the movements of the global oil price must provide sufficient variation to substantially influence spending of oil-endowed municipalities. Depicting the evolution over time, Figure 1.4 illustrates that oil prices were highly volatile over the sample period and, thus, introduce plenty of variation.



(a) Municipalities producing oil (1990-1999)

(b) Shares in municipal income coming from oil royalties by terciles conditional on positive oil royalties (2005)

Figure 1.3: Geography of oil production and municipal oil revenues

Concerning single expenditure categories, municipalities are supposed to spend

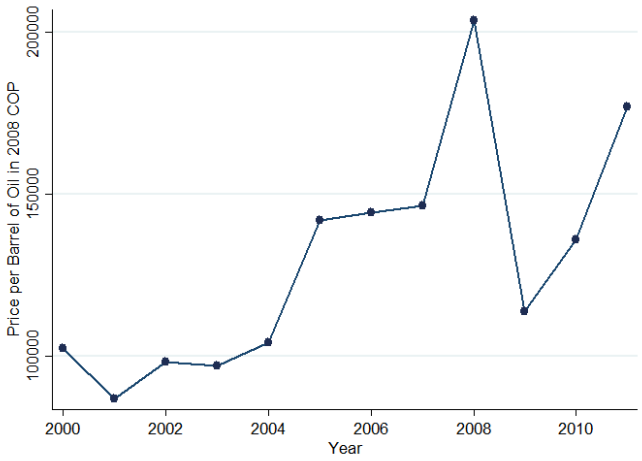


Figure 1.4: Oil price in 2008 real COP (OPEC basket)

75% of the royalties for the earmarked categories health, education and water as long as coverage goals have not been achieved, as explained in Section 1.3. Therefore, we expect the instrument to be strong for the earmarked categories. As mentioned before, royalties can potentially also be used for expenditures in non-earmarked categories, so we test the strength of the instruments for these categories as well. Statistical tests of the strength of the instrument are satisfactory for total expenditures and the earmarked categories but not for most non-earmarked categories; we discuss this in more detail in Section 1.6.1.

The second requirement for a valid instrument is that conditional on the covariates included, the IV must not correlate with the error term in the second stage (exogeneity assumption). As we control for municipality fixed effects, time-invariant differences between oil-endowed municipalities and other municipalities are accounted for. The spatial distribution of oil reserves is not under the control of local governments. Potentially endogenous efforts in oil discovery do not affect our identification because we use oil extraction indicators from 1990 to 1999, the decade preceding our period of analysis. Moreover, municipalities cannot manipulate the extracted quantities as private companies negotiate drilling contracts with the central government. Federal law regulates the royalties paid to the municipalities. Regarding the oil price on the world market, municipalities in Colombia are price takers. Although crude oil is the country's most important export good, Colombia does not rank among the major exporting nations and is not part of OPEC.

The spatial lag of a municipality's total expenditures (excluding the focal category Y) is a potentially important control variable, because oil royalties received by neighboring municipalities are likely to influence their spending not only in the focal category but also in other categories. If these other spatially lagged expenditures were omitted from the regression, our instrument could potentially be correlated with the error term in the second stage equation. In Section 1.6.3, we explore the sensitivity of our results when we treat total expenditures and its spatial lag as endogenous and when we include spending in all categories and their spatial lags separately in the regression.

In Table 1.2, we assess the structural similarity of oil-endowed and non-endowed municipalities using an additional source of data, the Colombian census conducted in 2005. The comparison shows that the two groups of municipalities are observationally equivalent concerning demographic and socio-economic characteristics. The good balance indicates that both municipality types are very similar except for their oil endowments, which supports our identification strategy. Figure 1.3a depicts the two groups of municipalities within a map of Colombia. This map reveals that oil extracting municipalities are somewhat clustered but sufficiently scattered over the country to obtain broad coverage of our instrument.

Table 1.2: Treatment - Control Balance in Observables

	Mean		t-test
Variable	Endowed	Non-endowed	p-value
Poverty rate	0.69	0.69	0.92
Informal employment share	0.95	0.95	0.51
Economic dependence share	0.54	0.54	0.81
Illiteracy share	0.28	0.29	0.46
Child labor share	0.06	0.06	0.59
Children w/o access to education, share	0.14	0.13	0.54
Children w/o access to care services share	0.21	0.20	0.56
Household share w/o health insurance	0.28	0.30	0.35
Household share w/o access to health	0.09	0.09	0.42
Household share w/o access to clean water	0.36	0.37	0.66
Household share w/o a sewage system	0.31	0.34	0.21
Household share w/o floors	0.19	0.21	0.27
Household share w/o walls	0.07	0.07	0.63

Notes.– All rates and shares refer to the households within a municipality. A municipality is defined as endowed if oil was extracted on its soil at any time between 1990 and 1999. Means are unweighted averages across municipality groups. The table shows p-values for t-tests of equal means between the two groups. Variables based on the Colombian 2005 census (cross-section).

Our estimation approach differs from the traditional spatial IV estimator. The latter uses all spatially lagged covariates WX to instrument the neighboring endogenous variable WY, in our case spending (see Anselin, 2008, for an overview). However, whether all WX can be excluded from the second stage equation is questionable (Gibbons and Overman, 2012; Revelli, 2015). For example, population changes in neighboring municipalities may well have a direct effect on spending decisions of the focal municipality or be correlated with spatially clustered time-varying unobserved factors, which would invalidate the traditional instruments. Therefore, we safely control for all WX in the second stage equation. The only excluded instrument is the interaction term of oil endowment of neighboring municipalities before the observation period with the international oil price. As we control for the interaction of the oil endowment dummy of the focal municipality with the oil price as well as municipality and department-time fixed effects in the second stage, the argument of exogeneity of this selected instrument is very strong. In Section 1.6.2, we compare the results from our preferred IV estimator with those from the arguably inconsistent traditional IV estimator.

To construct the spatial weighting matrix, we use the 5-nearest neighbor (NN) criterion as our main approach because the average municipality has five neighbors. As robustness checks, we compare the results to estimations using 4-NN and 6-NN matrices and an economic proximity matrix based on municipalities' average per capita income. To safeguard against potentially remaining serial correlation in the error term even after eliminating the unobserved fixed effects, we report standard errors clustered at the municipal level throughout the paper.

We contrast our IV results with the traditional spatial econometric estimators that use the Quasi Maximum-Likelihood method to estimate fiscal spillovers (Anselin, 2010). We follow the model selection procedure suggested by Elhorst (2010). Starting from OLS and going through the Spatial Lag (SAR), Spatial Error (SEM) and Spatial Durbin Model (SDM), we test hypotheses on whether and what kind of spatial terms should be included. The tests indicate that the SDM is the preferred model and thus confirm our initial model choice.[13] Thus, we estimate Equa-

[13]Detailed results are available from the authors on request.

tion (2) by QML, but without the oil-related variables.[14] For the QML-estimation to be consistent, knowledge of the true data generating process including the spatial weights must be assumed, as criticized by Gibbons and Overman (2012). Therefore, we prefer our IV estimator and present results from the potentially inconsistent QML estimator for comparison only.

1.6 Empirical results

1.6.1 Results based on the quasi-experimental instrument

Table 1.3 reports the main (second stage) results from estimations of Equation (2) using our preferred IV estimator. In this table we present estimates for total local public expenditures, the three categories earmarked for spending royalty income, all non-earmarked categories combined, as well as the specific non-earmarked category sport and recreation. The first row contains the estimated parameter of interest δ, which captures spatial public spending interaction. Our results indicate no significant spatial interactions in total expenditures, the earmarked categories, and the non-earmarked categories combined. The point estimate of spatial interactions for total local public spending is particularly close to zero (-0.081). The exception is the category of local public spending for sport and recreation, where local governments respond significantly to spending decisions of neighboring municipalities; we discuss this result in more detail below.

The control variables confirm expectations. Higher total expenditure as a measure of a municipality's budget (excluding the focal spending category to avoid endogeneity) increases spending in the focal category as well. Similarly, higher transfers from higher levels of government increase total spending and spending in all categories. When the population in a municipality grows, total public expenditures

[14]Moreover, for computational reasons, the year fixed effects are not department-specific in the QML estimations. This does not drive our results, though, because the IV estimates remain similar if we use country-level instead of department-level year fixed effects, see Section 1.6.3. In order to make our IV and QML estimations comparable, we report estimations based on the same sample in our tables. Therefore, we do not apply the bias corrected QML estimator suggested by Lee and Yu (2010) because this would imply losing one year of observations. However, we obtain similar results when implementing the bias correction. The small impact of the bias correction can be explained by our relatively large sample size.

Table 1.3: Main Estimation Results of Expenditure Interactions

	Total	Health	Education	Water	Non-earmarked	Sport & rec.
W_y	-0.081	-0.140	0.282	0.358	0.492	1.257**
	(0.499)	(0.540)	(0.238)	(0.413)	(0.663)	(0.576)
Oil extraction x oil price	0.302***	0.405***	0.921***	0.505***	-0.277**	-0.185
	(0.080)	(0.117)	(0.135)	(0.157)	(0.115)	(0.129)
Total excl. y		0.260***	0.418***	0.587***	0.108***	0.555***
		(0.035)	(0.034)	(0.034)	(0.031)	(0.030)
W_total excl. y		0.087	-0.059	-0.147	-0.047	-0.702**
		(0.129)	(0.114)	(0.261)	(0.076)	(0.303)
Population	6.175***	4.625	16.350***	-7.207**	0.808	0.003
	(2.359)	(3.373)	(4.668)	(3.001)	(3.168)	(3.823)
Population squared	-2.996***	-2.120	-7.159***	3.191**	-0.768	-0.287
	(1.088)	(1.564)	(2.161)	(1.402)	(1.482)	(1.781)
Share of rural population	-0.596***	-0.377**	-1.147***	0.027	-0.182	0.139
	(0.144)	(0.166)	(0.244)	(0.164)	(0.176)	(0.165)
Transfers	0.029***	0.012***	0.020***	0.017***	0.027***	0.012***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
W_population	0.444	-2.140	-10.329	1.750	0.166	-4.647
	(5.820)	(6.037)	(8.862)	(6.878)	(6.885)	(8.744)
W_population squared	0.136	1.119	5.378	-0.628	0.294	2.520
	(2.678)	(2.806)	(4.028)	(3.189)	(3.336)	(4.173)
W_share of rural population	-0.508	-0.082	-0.637	0.177	0.003	0.086
	(0.430)	(0.428)	(0.516)	(0.408)	(0.357)	(0.334)
W_transfers	0.008	-0.006	-0.009	-0.016	-0.004	-0.014*
	(0.018)	(0.010)	(0.009)	(0.011)	(0.026)	(0.009)
Observations	12,023	12,023	12,023	12,023	12,023	12,023
Number of municipalitites	1,093	1,093	1,093	1,093	1,093	1,093
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Department - year FE	Yes	Yes	Yes	Yes	Yes	Yes
AP F-statistic excl. inst.	16.26	22.25	83.01	24.09	9.035	13.99

Notes: Second stage IV estimation results of spatial expenditure interactions. The columns show results for different local public expenditure variables Y. Excluded IV for WY: Endowment of neighboring municipalities with oil in the 1990s interacted with the current world market price for oil. 5-NN spatial weighting matrix. Expenditures and transfers are real per capita values in 2008 COP. All continuous variables in logs. The Angrist-Pischke (AP) first-stage F-statistic of the excluded instrument is also reported. Standard errors given in parentheses are clustered on the municipality level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

increase, but at decreasing marginal rates, which is likely due to economies of scale. A larger share of rural population decreases local public spending.

The F-statistic of excluded instruments is sufficiently large in all columns of this table except for the non-earmarked composite. Table 1.4 presents the corresponding first stage results in more detail. As expected, the interaction of oil extraction of neighboring municipalities with the oil price is positive and highly significant when total spending or the earmarked spending categories of the neighboring municipalities are the dependent variables in the first stage. This indicates that municipalities spend more on the earmarked categories and also spend more in total when they receive more royalties from the extraction of oil. Thus, they largely seem to follow the federal laws regulating the spending of royalty income. In contrast, neighboring municipalities spend less on the non-earmarked categories when they receive more oil royalties. A potential explanation is that the increased expenditures in earmarked categories triggered by higher royalty income directs attention of local politicians and thus spending away from non-earmarked categories.[15]

Table 1.8 in Appendix 1.8 shows the second-stage results for all single non-earmarked categories, where each row represents a separate estimation for the indicated category. We only present the most relevant coefficients and statistics in the columns. For most of these non-earmarked categories, the F-statistic indicates that the instrument is weak, except for the categories sport & recreation and municipal equipment. Thus, our identification strategy allows us to identify the presence of expenditure interactions for total expenditures and the earmarked categories as well as for the two non-earmarked categories mentioned. For municipal equipment, we do not find significant spatial spending interactions.[16]

For the category of sport and recreation, the point estimate of the significant coefficient of spatial interaction is larger than one, indicating an explosive process. When the five nearest neighboring municipalities increase their spending for sport and recreation on average by 1%, this causes the focal municipality to increase its spending in the same category by 1.25%. Given that municipalities spend a com-

[15]For the strength of the instrument, it is only important that it is strongly correlated with spending of neighboring municipalities; the sign of the relationship is irrelevant.

[16]There appears to be weak evidence of significant fiscal interactions also in the category of spending for justice and security, but this result must be interpreted with caution because of the weak instrument for this category.

Table 1.4: Main Specification: First Stage

	W_total	W_health	W_education	W_water	W_non-earmarked	W_sport & rec.
Oil extraction x oil price	-0.020	0.035	-0.034	0.026	-0.045	-0.095**
	(0.031)	(0.042)	(0.058)	(0.061)	(0.049)	(0.041)
W_Oil extraction x oil price	0.281***	0.361***	0.980***	0.582***	-0.291***	-0.322***
	(0.070)	(0.076)	(0.108)	(0.119)	(0.097)	(0.086)
Total excl. y		-0.004	0.011	-0.000	0.010**	-0.014*
		(0.007)	(0.008)	(0.010)	(0.005)	(0.008)
W_total excl. y		0.234***	0.462***	0.622***	0.109***	0.522***
		(0.019)	(0.021)	(0.023)	(0.015)	(0.019)
Population	-0.121	-0.221	-0.063	1.305	-0.918	0.632
	(1.279)	(1.507)	(1.650)	(1.513)	(1.908)	(1.467)
Population squared	0.127	0.155	0.226	-0.596	0.458	-0.270
	(0.600)	(0.702)	(0.770)	(0.710)	(0.901)	(0.687)
Share of rural population	-0.032	-0.066	-0.228***	-0.094	0.121	0.042
	(0.058)	(0.063)	(0.083)	(0.081)	(0.075)	(0.074)
Transfers	0.001	-0.001	-0.001	-0.000	0.003**	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
W_population	8.399***	3.912	11.793***	-4.401	6.386*	7.630***
	(2.279)	(3.008)	(3.567)	(3.094)	(3.423)	(2.935)
W_population squared	-3.867***	-1.835	-4.501***	2.103	-3.287**	-3.797***
	(1.062)	(1.404)	(1.654)	(1.443)	(1.599)	(1.363)
W_share of rural population	-0.691***	-0.586***	-1.685***	-0.458***	0.070	0.178
	(0.121)	(0.135)	(0.181)	(0.147)	(0.149)	(0.140)
W_transfers	0.036***	0.012***	0.021***	0.020***	0.039***	0.008***
	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Observations	12,023	12,023	12,023	12,023	12,023	12,023
R-squared	0.069	0.042	0.212	0.126	0.049	0.113
Number of municipalities	1,093	1,093	1,093	1,093	1,093	1,093
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Department - year FE	Yes	Yes	Yes	Yes	Yes	Yes
AP F-value excl. inst.	16.26	22.25	83.01	24.09	9.04	13.99

Notes: First stage IV results; the second stage is reported in Table 1.3. Expenditures and transfers are real per capita values in 2008 COP. All continuous variables in logs. The Angrist-Pischke (AP) first-stage F-statistic of the excluded instrument is also reported. Standard errors given in parentheses are clustered on the municipality level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

parably small share of their total budget for sport and recreation (see Figure 1.1a), a temporarily explosive pattern during our period of analysis is not implausible.[17] However, the finding of an instable spatial process should be regarded with caution for two reasons. First, a t-test fails to reject the hypothesis that the coefficient is smaller than one (p-value: 0.33), so we do not rule out that the process is stable. Second, when using certain alternative spatial weighting matrices in robustness checks reported in Section 1.6.3, the point estimate of the coefficient drops below one. Figure 1.5 in Appendix 1.8 shows that municipal spending on sport and recreation is clearly clustered both at the beginning and end of our period of analysis, which is consistent with the presence of fiscal interactions, but it is unclear weather these interactions are explosive during this time period.

As there is no spatial interaction in total local spending, the significant interaction in the spending category sport and recreation reflects a change in the composition of the local budget. The finding of fiscal interaction in this area reflects the importance of sports and recreational activities all over Colombia.[18] At least three factors may explain the presence of expenditure interactions in this spending category. Firstly, because this category includes items such as playgrounds or sports fields and instructors, these expenditures are very visible to the local voters and could therefore be used by incumbent local politicians to secure votes in future municipal elections, as argued by Drazen and Eslava (2010). Because of its visibility and voters' awareness, spending in this category may be used primarily in yardstick competition. Secondly, positive interactions could also arise because of complementarities in expenditures in this category between neighboring municipalities. For example, neighboring municipalities might cooperate in certain sports events or programs or jointly build and use sports stadiums. A third reason for the interaction could be competition between municipalities to attract a mobile tax base. For example, bigger and better recreational facilities might attract businesses to the municipality that value employee wellbeing. In this case, municipalities have

[17] The literature on spatial competition in local public welfare spending occasionally reports spatial interaction coefficients larger than one as well (Figlio *et al.*, 1999; Saavedra, 2000).

[18] Case studies indicate that Colombian municipalities devote a high proportion of resource royalties to areas like entertainment and sports complexes (Gaviria *et al.*, 2002). We additionally run separate estimations for different regions of Colombia and find that the statistically and economically strongest spatial interactions in spending for sport and recreation occur in the east of the country.

an incentive to respond to expenditures of their neighbors in this category. Each of these explanations is consistent with the positive estimated interaction effect.

1.6.2 Classical spatial econometric approaches

For comparison with our preferred IV estimates, Table 1.5 presents the estimation results using the traditional QML estimator with municipality and year fixed effects. Based on this estimator that does not exploit quasi-experimental variation, the results suggest highly significant spatial autocorrelation between neighboring municipalities in total local public expenditures as well as all the earmarked categories and the combined non-earmarked categories (as well as sport and recreation).[19]

Table 1.5: QML Results of Expenditure Interactions

	Total	Health	Education	Water	Non-earmarked	Sport & rec.
W_y	0.182***	0.116***	0.148***	0.126***	0.147***	0.0685***
	(0.0147)	(0.0157)	(0.0158)	(0.0148)	(0.0148)	(0.0148)
Total excl. y		0.262***	0.433***	0.587***	0.116***	0.534***
		(0.0350)	(0.0343)	(0.0367)	(0.0304)	(0.0263)
W_total excl. y		-0.000665	0.0537	-0.00902	0.0198	-0.0635
		(0.0357)	(0.0390)	(0.0455)	(0.0199)	(0.0389)
Population	6.910***	4.293	19.30***	-5.039	1.138	1.479
	(2.453)	(3.619)	(4.936)	(3.502)	(3.164)	(3.237)
Population squared	-3.360***	-1.994	-8.497***	2.200	-0.959	-1.001
	(1.129)	(1.672)	(2.281)	(1.642)	(1.470)	(1.495)
Share of rural population	-0.501***	-0.313*	-0.986***	0.0308	-0.0938	0.261*
	(0.147)	(0.170)	(0.246)	(0.152)	(0.159)	(0.150)
Transfers	0.0283***	0.0135***	0.0177***	0.0165***	0.0271***	0.0101***
	(0.00259)	(0.00330)	(0.00330)	(0.00361)	(0.00359)	(0.00299)
W_population	-4.206	-9.690*	-8.972	7.029	1.196	4.388
	(4.083)	(5.759)	(7.909)	(6.884)	(5.558)	(6.673)
W_population squared	2.284	4.705*	4.887	-3.101	-0.462	-2.115
	(1.873)	(2.664)	(3.668)	(3.191)	(2.575)	(3.099)
W_share of rural population	-0.00124	0.138	-0.101	0.255	0.116	0.423
	(0.240)	(0.267)	(0.310)	(0.312)	(0.308)	(0.280)
W_transfers	-0.00285	-1.20e-05	-0.0156**	-0.0129**	0.00313	-0.00649
	(0.00441)	(0.00718)	(0.00634)	(0.00650)	(0.00672)	(0.00545)
Observations	12,023	12,023	12,023	12,023	12,023	12,023
Number of municipalitites	1,093	1,093	1,093	1,093	1,093	1,093
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: QML estimation results of spatial expenditure interactions. The columns show results for different local public expenditure variables Y. Expenditures and transfers are real per capita values in 2008 COP. All continuous variables in logs. Standard errors given in parentheses are clustered on the municipality level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

From these estimates alone, it would be tempting to conclude that significant spatial interaction is present in all these categories of local public expenditures.

[19] This also holds when we use the SAR or SEM models or different weighting matrices.

However, the results from using exogenous variation in the previous section show that this spatial autocorrelation does not indicate causal effects. This methodological comparison highlights the importance of relying on quasi-experimental variation in the causal analysis of fiscal interactions.

As another comparison, Table 1.9 in Appendix 1.8 provides results from the traditional spatial IV estimator that uses spatial lags of all control variables as excluded instruments. As argued in Section 1.5.2, we expect this estimator to be inconsistent, like the QML estimator. The results suggest significant spatial interaction in local spending on education. However, our preferred IV estimator that uses quasi-experimental variation suggests that this correlation is spurious.[20]

1.6.3 Robustness checks

We assess the sensitivity of the results from our preferred IV estimator by employing alternative weighting matrices and specifications. Table 1.6 summarizes the first set of robustness checks for total local public expenditures (first two columns) and local spending for sport and recreation (two rightmost columns). In the first three table rows, we vary the number of neighbors included for constructing the k-nearest neighbor weighting matrix. In row four, we use an inverse distance weighting matrix where distance is defined in an economic way by the difference in income between municipalities.[21] The results show that fiscal interactions in total spending are always insignificant.[22]

The point estimates also indicate strong spatial interactions in sport and recreation in all specifications. Only when using the 6-NN matrix, the coefficient loses statistical significance, suggesting that interactions cannot be detected when neighbors are too far away. When using the 6-NN matrix or the inverse income distance matrix, the point estimate of spatial interaction drops below one, which indicates a stable process, in contrast to our baseline result of an explosive process. Thus, while

[20]Furthermore, when using the traditional spatial IV estimator, the point estimate of the spatial interaction in total local public spending increases, but remains insignificant, and the spatial interaction in sport and recreation loses significance.

[21]Geographical distance is not suitable in the Colombian context because municipalities vary extremely in their area size, see Figure 1.2.

[22]This also holds when we run separate estimations for different regions within Colombia using our baseline 5-NN weighting matrix.

Table 1.6: Robustness Checks: Different Matrices and Specifications

	Total expenditures		Sport & recreation	
Specification	W_y	AP F-statistic	W_y	AP F-statistic
4-NN weighting matrix	-0.010	13.09	1.188**	17.49
	(0.499)		(0.539)	
5-NN weighting matrix	-0.081	16.26	1.257**	13.99
	(0.499)		(0.576)	
6-NN weighting matrix	-0.132	22.87	0.859	23.30
	(0.496)		(0.533)	
Inv. income distance w. matrix	0.526	11.17	0.729*	7.976
	(0.403)		(0.408)	
Inverse hyperbolic sine	-0.121	16.94	1.284**	13.14
	(0.501)		(0.602)	
Country-level year fixed effects	-0.294	18.68	1.188***	28.40
	(0.548)		(0.375)	
Controlling for conflict	-0.099	15.82	1.263**	13.32
	(0.510)		(0.594)	

Notes: Each row represents a different model specification and shows the spatial autocorrelation coefficients for total local public spending and sport & recreation as dependent variables (separately estimated) with the corresponding Angrist-Pischke (AP) first-stage F-statistics of the excluded instrument to the right. Excluded IV for WY: Endowment of neighboring municipalities with oil in the 1990s interacted with the current world market price for oil. Expenditures and transfers are real per capita values in 2008 COP. All continuous variables in logs. Standard errors given in parentheses are clustered on the municipality level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

we find robust evidence of strong spatial interactions in local public spending for sport and recreation, we cannot decide with high confidence whether this process is stable or explosive.

In some spending categories, a number of municipalities report zero spending in some years, for example in the sport and recreation category. To avoid dropping these municipalities, in the baseline specification we add one to all spending variables before taking the logarithm. In the robustness check in row five of Table 1.6, we use the inverse hyperbolic sine transformation instead (again using our preferred 5-NN weighting matrix). This function is similar to the logarithm, but defined for values of zero as well. The results are very similar to the baseline results.

To present a specification fully comparable with our QML estimation, we also run a version of our main IV model where year fixed effects replace department-year fixed effects. The estimates show that this alternation leaves the main results unchanged.

Next, we assess whether the Colombian civil war that started in the 1960s influences fiscal interactions. The violence was particularly severe during the late 1990s

and early 2000s. Civil conflicts in a certain region could induce spatially correlated public spending patterns. Therefore, we include civil war related casualties in the municipality and its spatial lag as additional covariates in our model. Following Dube and Vargas (2013), we retrieve the casualties variable from the Conflict Analysis Resource Center (CERAC) which provides the most comprehensive and independent source of civil war related data. However, the coefficients of these additional controls turn out to be insignificant, and the inclusion of the variables does not affect the estimates of the fiscal interaction coefficients. The inclusion of the variables does not alter the findings in the QML specifications either (results not tabulated). Thus, our results are robust to the inclusion of civil war related variables.[23]

Another concern regarding our model could be potential endogeneity of the total expenditure covariate and its spatial lag. Although we exclude the category of interest Y from the total expenditure controls to avoid a mechanical correlation with the dependent variable, one may be concerned about simultaneity of choices. Therefore, for these two potentially endogenous covariates, we consider using their one-year time lags as excluded instruments, assuming that the error terms in Equation (2) are serially uncorrelated. On this basis we conduct a Hausman test of endogeneity of the two covariates.[24] The test results indicate that the null hypothesis of exogeneity of total expenditure and its spatial lag cannot be rejected. Therefore, our more efficient baseline estimator that treats these controls as exogenous is preferred.

We also estimate more flexible specifications where we include each of the ten other public spending categories and their spatial lags separately in the model as control variables when a specific spending category is the dependent variable. Table 1.10 in Appendix 1.8 reports the results; we do not report the coefficients of the spatial lags of the spending categories for brevity. The findings confirm that there are no significant spatial interactions for the earmarked categories, and they also replicate the size and significance of the interaction in spending for sport and recreation.[25] In summary, we conclude that the main results are robust to specification

[23] As we cannot rule out potential endogeneity of civil conflict, we do not employ this covariate in our main specification but only in this robustness check.

[24] We run this test for the spending category of sport and recreation, where we found the significant spatial interaction effect.

[25] However, this specification does not seem to be suitable for the health category, where the standard error and the point estimate increase a lot. When we treat all expenditure categories

choices as long as identification relies on our quasi-experimental instrument.

1.7 Conclusion

This paper investigates spatial interactions in local public expenditures in a developing country applying a quasi-experimental identification strategy. We use panel data on the universe of the more than 1000 municipalities in Colombia over a period of eleven years to estimate spatial interaction effects in total local expenditures and the eleven most important spending categories. For identification, we rely on exogenous variation in the exposure of individual municipalities to shocks in oil prices on the world market due to their local endowment of oil resources.

For total local public expenditures and most spending categories, the estimates of spatial interactions are small and not significantly different from zero. The notable exception is public expenditures for sport and recreation, where we detect significant and large causal spatial interactions. In contrast, when we use spatial econometric estimators that do not employ quasi-experimental variation, we find strong and significant spatial autocorrelation in almost all categories of public expenditures. Our comparison of methods shows that this spatial autocorrelation cannot be interpreted causally. Therefore, our results highlight the importance of using additional exogenous sources of variation for causal inference on fiscal interactions. This is in line with the results of Isen (2014) concerning spending competition in Ohio and of Lyytikäinen (2012) regarding tax competition in Finland although we do find evidence for significant causal expenditure interactions in the spending category of sport and recreation.

Our findings have important policy implications. The results demonstrate that fiscal decentralization in developing countries does not necessarily lead to a general race to the bottom concerning local public expenditures due to strategic interactions of local governments. Our findings lend support to decentralization reform in developing countries as they alleviate concerns about inefficiencies implied by fiscal

and their spatial lags as endogenous and instrument them with their on-year time lags, the point estimate of the spatial interaction in health spending decreases to -0.08, so the large point estimate seems to be biased.

externalities. On the other hand, one also cannot expect that innovative expenditure policies rolled out in a municipality, perhaps in the context of decentralized foreign aid, will spread out substantially to neighboring municipalities through mimicking of local expenditure policy.

A caveat is in order when applying our findings to other countries. Although Colombian municipalities have considerable leeway in their expenditure decisions, spending rules for royalties and transfers may restrain fiscal interactions in Colombia. Nonetheless, many developing countries have similar institutions in this respect. Countries such as Bolivia, Brazil and Peru directly allocate a share of income from extractive industries to subnational governments, as Colombia does. Most countries that levy royalties on the extraction of natural resources also impose restrictions on how this income can be used. Thus, the case of Colombia is illustrative in a larger context.

More research is needed to investigate whether our findings hold in other countries with and without income from natural resources, the allocation of royalties to subnational governments and earmarking. Future research should also explore different quasi-experimental setups and spatial discontinuity designs, ideally within the same institutional environment, and additional fiscal variables. Another avenue for further research is to address the significant causal spatial interactions in local public expenditures for sport and recreation we found and to examine the mechanisms leading to this novel empirical result.

1.8 Appendix

Supplementary Tables

Table 1.7: Variable Description

Variable	Description
Total	Total amount of money invested in the different categories of each municipality.
Health	Funds invested in health. Includes investment in new infrastructure, maintenance of existing infrastructure, promotion programs, as well as the salaries of the health centers' employees.
Education	Funds invested in education. Includes investment in new infrastructure, maintenance of existing infrastructure, promotion programs, as well as the salaries of teachers.
Water	Municipal investment in aqueducts, sewage systems, garbage collection, new sanitation infrastructure and maintenance of the existing one.
Housing	Accounts for subsidies to buy or improve houses for the poor and expenditures to improve housing conditions of the general population.
Sport and recreation	Funds used to promote sport and leisure activities including infrastructure, programs and instructors.
Agriculture	Payments made to improve the productivity of the agricultural sector like infrastructure, experimental farms and consultants.
Community development	Investment in programs to support public engagement and active citizenship.
Municipal equipment	Investment on municipal buildings, like public offices, market places, cemeteries, public places and slaughter houses.
Vulnerable groups	Investments used for programs directed towards vulnerable groups, including children, elderly, single mothers, displaced and disabled people.
Justice and security	Money used to pay the salaries of police officers and sheriffs, doctors, social workers, and psychologists working for the family service agency.
Disasters prevention	Investment used for disaster relief and prevention.

Source: Based on Acevedo and Bornacelly (2014).

Table 1.8: Results for All Non-earmarked Categories

	W_y	Total excl. y	W_total excl. y	AP F-statistic excl. inst.
Housing	-2.091	0.498***	1.043	3.631
	(1.531)	(0.047)	(0.711)	
Agriculture	-2.690	0.371***	1.083	0.791
	(4.193)	(0.103)	(1.732)	
Com. development	1.757	0.144***	-0.324	2.824
	(1.650)	(0.030)	(0.305)	
Sport & rec.	1.257**	0.555***	-0.702**	13.99
	(0.576)	(0.030)	(0.303)	
Equipment	0.606	0.488***	-0.336	33.75
	(0.392)	(0.035)	(0.210)	
Vuln. groups	-2.600	0.353***	1.100	0.513
	(4.584)	(0.053)	(1.818)	
Justice	1.873*	0.309***	-0.496*	4.591
	(1.107)	(0.033)	(0.285)	
Dis. prevention	1.097	0.333***	-0.421	3.811
	(1.101)	(0.028)	(0.370)	

Notes: Each row represents a separate estimation for different non-earmarked spending categories. IV specification as in Table 1.3. Expenditures and transfers are real per capita values in 2008 COP. All continuous variables in logs. The Angrist-Pischke (AP) first-stage F-statistic of the excluded instrument is also reported. Standard errors given in parentheses are clustered on the municipality level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 1.9: Spatial Lags as Instruments

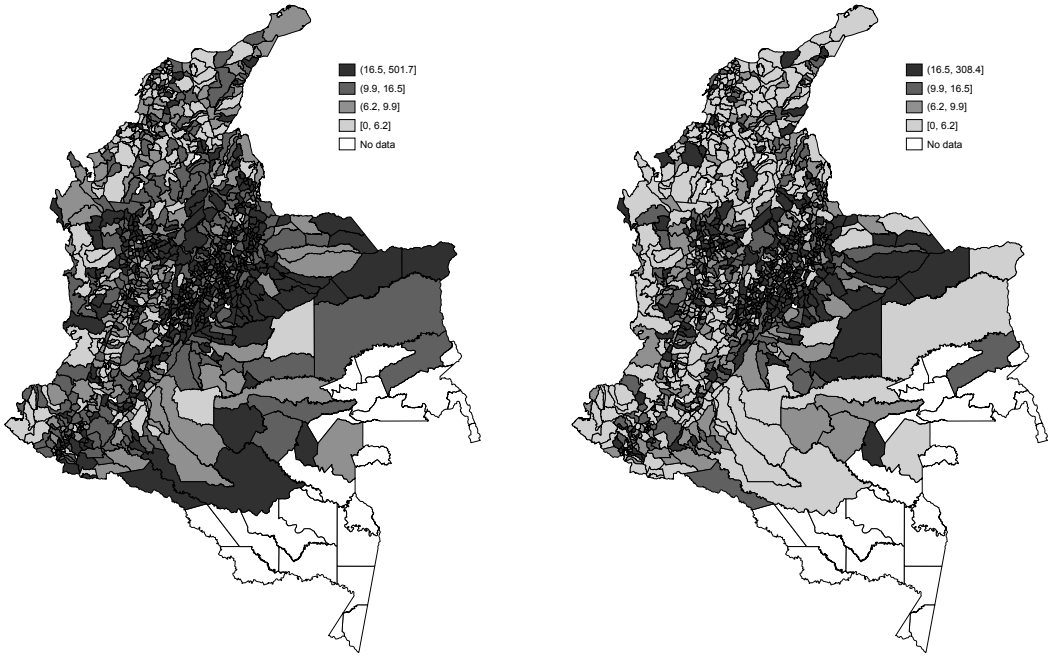
	Health	Education	Water	Non-earmarked	Sport & rec.
W_y	0.130	0.372***	0.091	0.169	-0.100
	(0.136)	(0.073)	(0.071)	(0.124)	(0.075)
Population	5.036	17.598***	-5.832**	0.828	0.902
	(3.258)	(4.442)	(2.899)	(2.988)	(3.120)
Population squared	-2.287	-7.621***	2.618*	-0.713	-0.684
	(1.508)	(2.035)	(1.356)	(1.384)	(1.445)
Share of rural population	-0.362**	-1.171***	-0.021	-0.173	0.192
	(0.166)	(0.243)	(0.155)	(0.159)	(0.145)
Transfers	0.012***	0.020***	0.017***	0.028***	0.010***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
Total excl. y	0.265***	0.424***	0.592***	0.110***	0.531***
	(0.035)	(0.036)	(0.033)	(0.030)	(0.026)
Observations	12,023	12,023	12,023	12,023	12,023
Number of municipalies	1,093	1,093	1,093	1,093	1,093
Municipality FE	Yes	Yes	Yes	Yes	Yes
Department - year FE	Yes	Yes	Yes	Yes	Yes
AP F-statistic excl. inst.	46.46	198.4	189.2	42.95	168.3

Notes: In contrast to the main specification in Table 1.3, here, the spatial lags of all control variables are used as excluded instruments (traditional spatial IV estimator). Expenditures and transfers are real per capita values in 2008 COP. All continuous variables in logs. The Angrist-Pischke (AP) first-stage F-statistic of the excluded instrument is also reported. Standard errors given in parentheses are clustered on the municipality level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 1.10: All Expenditure Categories as Separate Control Variables

	Total	Health	Education	Water	Non-earmarked	Sport & rec.
W_y	-0.081	2.114	0.304	0.282	0.550	1.391***
	(0.499)	(3.036)	(0.246)	(0.537)	(0.883)	(0.469)
Health			0.312***	0.318***		0.070***
			(0.019)	(0.020)		(0.014)
Education		0.326***		0.211***		0.089***
		(0.022)		(0.018)		(0.016)
Water		0.296***	0.181***			0.184***
		(0.029)	(0.015)			(0.014)
Sport & rec.		0.077***	0.079***	0.190***		
		(0.024)	(0.013)	(0.014)		
Housing		0.000	0.017***	0.032***		0.044***
		(0.012)	(0.005)	(0.006)		(0.007)
Agriculture		0.065***	0.071***	0.076***		0.109***
		(0.020)	(0.011)	(0.012)		(0.012)
Community development		-0.021*	0.018*	0.038***		0.031***
		(0.012)	(0.010)	(0.009)		(0.011)
Equipment		0.000	0.021***	0.035***		0.061***
		(0.010)	(0.006)	(0.006)		(0.007)
Vulnerable groups		0.007	0.008	0.075***		0.063***
		(0.017)	(0.008)	(0.009)		(0.010)
Justice		0.036	0.025**	0.074***		0.065***
		(0.039)	(0.010)	(0.010)		(0.012)
Disaster prevention		0.001	0.035***	0.031***		0.032***
		(0.012)	(0.008)	(0.008)		(0.010)
Observations	12,023	12,023	12,023	12,023	12,023	12,023
R-squared	0.045	-0.219	0.357	0.388	-0.041	-0.034
Number of municipalities	1,093	1,093	1,093	1,093	1,093	1,093
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Department - year FE	Yes	Yes	Yes	Yes	Yes	Yes
AP F-statistic excl. inst.	16.26	1.139	82.96	13.96	5.186	19.67

Notes: In contrast to the main specification in Table 1.3, here, for each expenditure category on the left hand side, all other spending categories and their respective spatial lags are included in the estimation as separate control variables. The respective spatial lags of the spending categories on the right hand side are also included in the estimations but are not displayed in the table for brevity. Expenditures and transfers are real per capita values in 2008 COP. All continuous variables in logs. The Angrist-Pischke (AP) first-stage F-statistic of the excluded instruments is also reported. Standard errors given in parentheses are clustered on the municipality level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.



(a) Year 2000 (b) Year 2010

Figure 1.5: Municipal expenditures per capita on sport and recreation in thousand 2008 real COP by quartiles of the pooled sample from 2000-2010.

Data preparation and imputation

From the total number of 1122 annual municipality observations, we drop twenty units that maintain a special territorial status different from municipalities, being situated in very remote and sparsely populated areas. We also delete four new municipalities established after 2007, two remote islands and three municipalities left without direct neighbors. This leaves us with 1093 municipalities in our sample.

Missing values in expenditure variables account for less than 3% of the total municipality-year observations. For about 90% of the municipalities with missing values, we still observe at least nine years. Therefore, we decide against listwise deletion of municipalities with missing values and opt for imputation by linear intrapolation. For covariates, this procedure is necessary in less than 0.01% of all cases.

As a check of the data we compare the expenditure data with the *Ejecuciones Presupuestales Municipales* data base provided by the Colombian National Planning Department (DNP). While the overall consistency is very favorable, the comparison leads us to make some minor outlier corrections. In particular, we impute linear intrapolations for all values that are larger than twenty times a municipality's median in the same expenditure category and correct obvious decimal point errors. These changes affect approximatively another 3% of the observations but lead to an improvement of the data balance. Despite these needs for correction, the resulting dataset as described in Table 1.1 still has an exceptional quality given the developing country setting. One qualification remains: For the years 2006 and 2007, a total of 73 municipality-year pairs exhibit zero expenditures.

Chapter 2

Intergenerational Transmission of Social Norms: Evidence from Argentina

2.1 Introduction

In recent decades there has been a renewed interest in the study of the determinants of tax compliance. In most countries, compliance is high despite low penalties and enforcement probabilities. This is partly because individuals are subject to third-party reporting systems, and are therefore 'unable' to evade taxes (Kleven *et al.*, 2011). Another part of the answer has been attributed to the intrinsic motivation to pay taxes, or tax morale, which in turn is determined by different social and cultural traits (Brink and Porcano, 2016; Luttmer and Singhal, 2014).

These characteristics differ significantly across countries (e.g., Torgler and Schneider, 2004), which implies that a part of the intrinsic motivation to comply with taxes is passed down from one generation to another. The extent to which this happens has received very little attention from researchers. There exist only two studies investigating the intergenerational persistence of social norms and its direct effect on tax compliance, and both focus on the persistence of such norms after only one generation. Halla (2012) shows the presence of a strong correlation between the tax morale of second-generation American-born individuals and the tax morale of their

ancestors' country of origin. The author uses the inherited component of tax morale as an instrument for the actual tax morale of the individuals, and finds a positive and significant causal effect of tax morale on the size of the shadow economy. Similarly, Frimmel *et al.* (2018) find a causal link between non-compliance of parents and their offspring with commuter tax allowances in Austria. Results show that children from less compliant parents tend to be less compliant than the average.

The present study makes a significant contribution to this nascent body of literature. We investigate the intergenerational transmission of social norms related to tax compliance in the long run in a large Argentinian city. In particular, we use detailed micro data on the universe of individuals subject to property tax payments to study the extent to which property tax compliance differs by the origin of taxpayer's ancestry. Access to individual records on individual property tax payments allows us to overcome the problem of measuring tax non-compliance, which usually poses a significant challenge in this strand of literature. Moreover, we explore long term correlations between contemporary tax compliance levels and different measures of unlawful behavior in the country of origin of the taxpayers' ancestors.

Argentina provides an ideal setting to study the intergenerational transmission of socio-economic traits. Throughout its history, the country has been the destination of a considerable number of migration waves stemming from different countries. In particular, at the end of the 19th and the beginning of the 20th century, the country experienced a dramatic increase in immigration, mainly from European countries. Because of this distinct immigration history, present-day Argentinian population is a mix of individuals with different ethnic origins.

The city of Santa Fe reflects this diversity of ancestry. It was the destination of a significant proportion of migrants that arrived in Argentina during the 19th and 20th centuries (Lattes, 1973). This, together with the possibility to study the payment behavior of individuals with different ethnic origins within the same institutional environment, makes our setting especially well-suited for the analysis of the long term persistence of social traits related to tax compliance.

We use this diversity in ancestry of present-day taxpayers to study differences in property tax compliance across individuals with different descent. For this we exploit

a unique feature in our data that allows us to identify the most likely country of origin of the immigrant ancestors of the taxpayers: their names. Our data contains the full name of the individuals subject to tax compliance in the city of Santa Fe. With the help of a software specialized in onomastics, we determine the country of origin of the surnames in our dataset. Using additional sources, we are able to narrow down the level of geographical disaggregation and link the surnames to their most likely origin at the within-country level. With this information, we then estimate differences in measures of wealth and tax payments across individuals with different ancestry.

First we explore differences in measures of wealth by country of origin in our data. Results point to significant differences in wealth, captured by property square meter value, between groups of individuals with different descent. These findings are consistent with differences in human capital (Sánchez-Alonso, 2019) and economic performance (Arroyo Abad and Sánchez-Alonso, 2018; Pérez, 2019; Sánchez-Alonso, 2019) across groups of individuals with different nationalities that migrated to Argentina during the 19th and 20th century.

We then study how compliance differs by country of descent. We find significant differences in timely payments between individuals with Spanish, Italian, and Belgian origins. These differences are consistent across a range of robustness checks. Italian descendants are more likely to pay their property taxes before the deadline than individuals with Belgian and Spanish ancestry. We do not find, however, consistent patterns of non-compliance across these groups of origin. Results in this regard do not hold when correcting for potential selection bias in our data.

Our results also show that a national rather than a regional component of the social values related to tax compliance is persistent across generations. Findings do not change with the geographical disaggregation of the place of origin of the surnames. When looking at variation in property tax payments across regions of the same country of origin, we find that most regions within a country present very similar patterns of compliance.

Finally, we investigate the presence of long-term relationships between measures of tax compliance in our data and indicators of tax morale and unlawful behavior

in the country of origin of the taxpayers. We do not find evidence of meaningful correlations between the variables in the analysis. This could be explained by the convergence of social values of the descendants of migrants over the generations (Giavazzi *et al.*, 2019). Another factor that can help explain this result is selection in the migrant population. Migrants possess traits that differ substantially from that of the individuals that stay in their country of origin. Therefore the set of values they transmit to their descendants may differ from the set of values prevailing in the country they left behind (Abramitzky *et al.*, 2012; Klein, 1983; Sánchez-Alonso, 2019).

This study contributes to the literature mainly in two ways. Firstly, to the best of our knowledge, this is the first study to analyze the intergenerational transmission of social norms related to tax compliance in the long run. As mentioned before, the existing studies only focus on the persistence of these traits across two generations. Secondly, we analyze transmission of social values related to tax compliance in a developing country. Given the significant differences in many dimensions between developed and developing countries, conclusions from our setting might differ from analyses carried out in developed countries. Furthermore, our study shows the potential of exploiting the information contained in the names of individuals to answer relevant questions in economics.

Our first set of results relates to a body of literature analyzing the persistence of cultural norms brought by migrants from their country of origin into their destination country. Rice and Feldman (2006) document strong correlations between civic values in European countries and those of American-born individuals descending from immigrants born in these countries. Giuliano (2007) finds persistence in living arrangements of the country of emigration of the parents of second-generation individuals living in the United States. Mocan and Pogorelova (2015) study how the culture of leisure of the country of origin of the forebears affects individuals' labor supply. Results differ with gender, with origin of the ancestors affecting female labor supply, but with no effects for males. More recently, Giavazzi *et al.* (2019) investigate the extent of convergence of different values of immigrants settling in the United States. The authors find that persistence differs significantly with the number of generations, with second generations showing the highest degree of values

persistence. The authors also argue that the speed of convergence of values with that of the host country differs both by values and by country of origin.

In the field of economic development, Algan and Cahuc (2010) use the inherited component of trust in US immigrants measured by survey questions to identify the causal effect of trust on economic growth in their countries of origin over time. The authors find that inherited trust explains a significant part of economic development. Similarly, Tabellini (2010) finds a positive effect of cultural values like trust, respect, and individual effort perceptions on economic development at the within country level.

On a more general level, our study relates to the literature documenting the persistence of socio-economic values over long periods of time using micro data. Giuliano and Nunn (2013) find long-term persistence of democracy. Specifically, the authors show that pre-industrial local democracy is correlated with present-day positive beliefs of national democracy. Voigtländer and Voth (2012) find persistence over 600 years of anti-Semitic attitudes in Germany, and finds that pogroms during the black death epidemics in the 14th century predicted violence against Jews and votes for the Nazi party in the 20th century. Alesina *et al.* (2013) argues that current gender norms are influenced by pre-industrial agricultural practices. Results from comparisons of gender norms between children of immigrants in the United States and Europe confirm this pattern. Guiso *et al.* (2016) find long-term persistence of historical shocks on present-day social values. The authors find that cities that transitioned to self-governance in the Middle Ages have higher contemporary civic capital.

The paper proceeds as follows. Section 2.2 explains the institutional background of property tax in the city of Santa Fe, and the data sources used throughout the paper. Section 2.3 analyzes differences in property tax compliance by country of ancestry of taxpayers. Section 2.4 presents long-term correlations between measures of unlawful behavior of the countries of origin of the surnames in our sample and present-day property tax compliance. Section 5 concludes.

2.2 Institutional background and data

Property taxes in Argentina are set by the sub-national governments and are administered locally. They are an important source of revenue for local governments. In the city of Santa Fe they account for more than 30% of local revenues. The amount of the tax is a function of the assessed property value, land area, constructed area, and location-specific multipliers, among other attributes. At the beginning of each year, taxpayers decide either to pay the yearly tax in monthly installments or pay the full amount for the entire year in advance. Taxpayers choosing the first option receive quarterly mailings from the city containing the bills for each of the three subsequent months, which must be paid monthly.

Enforcement of property tax payments is slow and relatively lenient, mainly due to administrative constraints. Penalties for non-compliance include economic sanctions, monthly interest rates on the amount of the debt, and foreclosures. However, they are not strictly enforced, and only high amounts of unpaid taxes receive the attention of the authorities.

For our analysis we make use of administrative data on the universe of individuals subject to property tax in Santa Fe for the years 2005 to 2017. The data contains information at the individual level about tax amounts, due dates, payment dates, official value of the property, total area, constructed area, and the full name of the property owner. Table 2.1 shows descriptive statistics for these variables. Notably, 40% of the tax bills are paid on time, the average monthly tax liability is 68 Argentinian pesos, the average property has an assessed value of 353,000 Argentinian pesos. The typical property has an area of 1,532 square meters and a constructed area of 118 square meters.

We use various sources to retrieve data about the ancestry of taxpayers in Santa Fe. For the identification of the country of origin of the surnames we employed a software provider specialized in onomastics called *NamSor Applied Onomastics*. It applies AI algorithms to the full name of an individual to determine its most likely country of origin. The software has been previously used in marketing, policy, and academic research around the world to answer questions regarding diversity,

Table 2.1: Descriptives

Variable	Obs	Mean	Std. Dev.
% paid on time	121,505	0.414	0.326
Area	118,705	1,532.483	89,461.31
Constructed Area	118,705	116.440	344.0562
Tax amount	121,505	67.969	272.665
Property value	118,705	353,039.8	3,446,330

Notes: Area and constructed area are expressed in square meters. Tax amount and property value are expressed in Argentinian pesos.

integration, and foreign direct investment bias, among other related topics[1]. Table 2.2 shows the main countries of origin for the surnames in the city of Santa Fe, with the respective number of taxpayers assigned to each. The algorithm identified a total of 127 countries from which the last names in our sample originate[2]. The five most common countries of ancestry are Spain, Italy, France, Portugal and Germany, and together account for the origin of 77% of the surnames in our sample, while individuals with Spanish and Italian descent account for 67% of the total. These numbers are well in line with the proportion of nationalities of immigrants coming to Argentina between the 19th and 20th century (see Sánchez-Alonso, 2019).

The use of the software greatly facilitated the identification of the ethnic origin of the taxpayers in our sample. In contrast, the collection of origin information at the within-country level was a demanding task. Since regional concentration of surnames is a good proxy for the place of origin of the ancestors of present-day individuals (see, e.g., Cheshire, 2014; Longley *et al.*, 2007), we used web scrappers on websites containing information about contemporary distributions of surnames at the within-country level[3]. Due to data availability, we were able to collect the most likely region of ethnic origin for about 85% of the individuals with surnames stemming from Spain, Italy, France and Germany; four of the countries from which most of the last names in our sample come from. The level of geographical disaggregation available varied from

[1]for more information visit www.namsor.com.

[2]See Table 2.8 in the appendix for a complete list of the countries and the number of individuals with ancestry from each of them.

[3]Specifically, we used the website of the Spanish statistics institute, www.ine.es, to retrieve the distribution of Spanish surnames, www.paginebianche.it and www.italyheritage.com for Italian surnames, www.geopatronyme.com and www.verwandt.de for French and German surnames respectively.

Table 2.2: Main Countries of Origin

country of origin	observations	share
Spain	42,608	34.98
Italy	38,867	31.91
France	4,240	3.48
Portugal	3,886	3.19
Germany	3,810	3.13
Israel	1,856	1.52
Switzerland	1,576	1.29
Angola	1,526	1.25
Belgium	1,160	0.95
India	1,048	0.86
Other countries	21,232	17.44

Notes: the 'other countries' category encompasses the remaining 117 countries in our data. See Table 2.8 in the appendix for a complete list of the countries.

country to country (province, region, department and land for Spain, Italy, France and Germany respectively). In order to identify meaningful socio-economic regions within the countries, we use the second level of the Nomenclature of Territorial Units for Statistics (NUTS-2 for its initials in French) classification determined by the statistical office of the European Union, Eurostat, and group them according to socio-economic relevance and the number of observations available (see Table 2.9 for a complete list of the within-country regions, the corresponding NUTS-2 entities they encompass and the number of surnames with origins in each region).

Furthermore, we use additional sources to obtain the data necessary to analyze long-term correlations between tax compliance in our setting and measures of unlawful social behavior in the countries of origin of the surnames. We retrieve homicide rates from the dataset of the United Nations office for drugs and crime. The data contains homicide counts and rates by country for the years between 2000 and 2012. Corruption data was taken from Fisman and Miguel (2007). The study uses an index that measures subjective perceptions of corruption in different countries for the period 1998 to 2004. The index is centered around zero, with higher values indicating higher levels of corruption. Tax morale information was taken from the World Values Survey. This source has been extensively used in the tax morale literature (see, e.g., Alm and Torgler, 2006; Halla, 2012; Torgler, 2006). We use this data to

construct an average index of tax morale by country over the years 2000 to 2014.

2.3 Intergenerational transmission of tax compliance

2.3.1 Ancestry and wealth measures

Before moving to the central question of this section, let us first explore potential differences in property characteristics across groups of countries of origin of the taxpayers' forebears. During the decades of mass migration to Argentina in the 19th and 20th centuries, there were differences in human capital measured by literacy rates across groups of migrants, with individuals coming from north-western European Countries like Germany having higher literacy rates than immigrants from southern Europe (Sánchez-Alonso, 2019). These differences can be transmitted down to future generations (Borjas, 1992). There were also differences in economic performance between groups of migrants from different nationalities once settled in Argentina. For instance, Sánchez-Alonso (2019), and Arroyo Abad and Sánchez-Alonso (2018), document differences in access to property and earnings between communities of Spanish and Italian origins. Some of these differences were passed on to the next generation (Pérez, 2017).

To see if there is some degree of persistence in economic performance, we explore differences in wealth measured by property size, built-up area, property value, and tax amount between groups of individuals with different countries of ancestry. For this we take the average of these variables over the years 2005 to 2017 by taxpayer and then run OLS regressions with dummies for country of descent on the right-hand side. Table 2.3 shows regression outcomes for the five main countries of origin in terms of the number of descendants in our sample, taking Spain as the benchmark. Results show that there are some significant differences, especially in property size, built area, and square meter value across countries of ancestry (Table 2.10 in the appendix show estimation results for the 10 main countries).

From columns 1 to 4 of Table 2.3 we have that descendants from French immi-

Table 2.3: Differences in Wealth Measures

Variables:	(1) Propery area (m^2)	(2) Built area (m^2)	(3) Tax amount (Pesos)	(4) Property value ('000 Pesos)	(5) Value per m^2 (Pesos)
Italy	595.813	-3.473	1.543	10.921	268.068***
	(701.790)	(2.640)	(2.291)	(30.090)	(51.449)
France	-811.687***	-9.289***	-3.561	-30.340	117.020
	(289.313)	(3.473)	(2.500)	(19.624)	(71.634)
Portugal	46.440	-7.464*	-0.982	-6.243	176.255
	(691.909)	(4.029)	(7.034)	(27.330)	(156.511)
Germany	-622.936*	-6.266	-3.742	-34.846*	653.059***
	(329.783)	(4.543)	(2.601)	(20.481)	(136.426)
Other countries	663.329	-5.450*	3.373	-19.118	78.367*
	(601.331)	(2.812)	(3.097)	(15.856)	(40.232)
Constant	1,234.222***	119.592***	85.367***	356.400***	2,899.670***
	(284.475)	(2.293)	(1.769)	(14.060)	(25.305)
Post-est. tests (p-values):					
Italy = France	0.029	0.046	0.026	0.168	0.061
Italy = Portugal	0.541	0.263	0.717	0.628	0.568
Italy = Germany	0.067	0.499	0.028	0.133	0.006
France = Portugal	0.175	0.665	0.714	0.375	0.725
France = Germany	0.281	0.521	0.945	0.824	0.000
Germany = Portugal	0.305	0.816	0.696	0.303	0.020
Observations	118,708	118,708	121,809	118,708	118,656

Notes: the benchmark category is Spain. the category 'other countries' encompasses individuals with ancestors from the other 122 countries in our data. Value per square meter in Column 5 was calculated by dividing the valuation of the property by its area. Robust standard errors are given in parenthesis. The lower panel shows p-values for Wald tests. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

grants have significantly smaller properties and built areas than those of Spanish descent. The average property size belonging to French descendants is 812 square meters smaller and has nine square meters less constructed area than properties from individuals with Spanish ancestors. Also French descendants pay lower taxes than taxpayers with Italian ancestors. There are also some differences in our wealth measures for individuals with German ancestry. They have smaller and cheaper properties than Spanish descendants, although the coefficients are slightly significant. Their properties are also smaller and taxes are lower compared with properties owned by and tax amounts paid by those of Italian descent.

Perhaps one of the most relevant measures of wealth in our sample is the value per square meter. Column 5 present the results for this variable. It is interesting to see that properties of taxpayers with German ancestry have a significant higher value per square meter than all other ancestry groups from the table. Also Italian descendants' properties have a higher valuation per square meter than individuals of most of the other ethnic origins, except for German and Portuguese descendants. These results seems to suggest that to some degree, the differences in human capital and economic performance between groups of immigrants mentioned at the beginning of this section were passed on over time, and are visible in our data in the form of wealth differences measured by property square meter value.

2.3.2 Ancestry and property tax compliance

We now proceeded to analyze potential differences in the patterns of property tax payments for different ancestry groups. As a starting point, we collapse our panel into a cross-section. Since we are mainly interested in the effect of the origin of the taxpayers' forebears, and this characteristic does not change over time, we take the average of payment dummies (see below) for all tax bills over all years for each individual. This way we have a measure of the average payment behavior of individuals over time. We then run the following OLS estimations:

$$Payments_i^{\tau} = \alpha + \beta Country_i + X\gamma + e_i \tag{1}$$

where $Payments_i^{\tau}$ captures the proportion of payments made τ={on time, in

advance, within 30 days, within 1 year} by taxpayer i over the period 2005-2016. Note that for this and all subsequent analysis we drop all observations for which we do not observe a full year of payment history after the due date of the bill[4]. $Country_i$ is the country of origin of the forebears of individual i, here we will focus on the ten countries where most of the ancestors of tax payers originated (see Table 2.4 below). The rest are grouped in a category named *'other countries'*. X is a vector of controls including the averages of the property tax amount and property area.

Table 2.4 summarizes the outcomes of the estimations. Results point to statistically significant differences between groups of individuals with family names stemming from different countries. Timely compliance among Spanish descendants is 40%. One year after the deadline, this number goes up to 60%. Taking this group as a benchmark, individuals with an Italian surname are, on average, more punctual with their payments: they are 1.7 percentage points more likely to pay on time, and once the deadline has passed they are more likely to make payments within 30, 180 and 360 days by 1.9 pp.

French, German and Swiss descendants are also more punctual with their payments as compared to individuals with a Spanish surname. They pay more punctually and make more payments on average within the other windows of time in the table. With respect to the rest of the countries in our data, grouped in the 'other countries' category, descendants of Spaniards seem to be less likely to make payments on time, and within 30, 180 and 360 days. We also find that tax payers of Belgian descent are less punctual on average and are less likely to make payments within the other periods.

It is also interesting to see that the payment patterns for individuals with Portuguese descent is very similar to those with Spanish ancestry. The differences mentioned above between Spanish descendants and those of Italian, French, German, Swiss, and Belgian descent also hold when comparing these countries with Portugal, as indicated in the lower panel of Table 2.4. Columns 5 to 8 illustrate that all the aforementioned effects hold after the introduction of controls. All significant

[4]Given that our last observation point is April 21 2017, we are not able to follow the payment history during one year after the due date, for bills with a payment deadline after April 21 2016. Consequently, we drop these observations from the analysis.

Table 2.4: Property Tax Payments by Country of Descent

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Payments:	On time	30 days	180 days	360 days	On time	30 days	180 days	360 days
Italy	0.017***	0.019***	0.019***	0.019***	0.017***	0.019***	0.019***	0.019***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
France	0.014***	0.019***	0.020***	0.020***	0.013**	0.018***	0.020***	0.020***
	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)
Portugal	-0.002	-0.002	-0.003	-0.005	-0.000	-0.001	-0.002	-0.004
	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Germany	0.014***	0.013**	0.015**	0.016***	0.015***	0.013**	0.015**	0.016**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Israel	0.004	0.006	0.008	0.007	0.005	0.007	0.010	0.008
	(0.008)	(0.008)	(0.009)	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)
Switzerland	0.016*	0.020**	0.018*	0.017*	0.017**	0.021**	0.020**	0.020**
	(0.008)	(0.009)	(0.009)	(0.010)	(0.008)	(0.009)	(0.009)	(0.010)
Angola	0.005	0.007	0.007	0.009	0.005	0.007	0.008	0.011
	(0.008)	(0.009)	(0.009)	(0.010)	(0.008)	(0.009)	(0.009)	(0.010)
Belgium	-0.091***	-0.110***	-0.117***	-0.123***	-0.091***	-0.110***	-0.117***	-0.123***
	(0.010)	(0.011)	(0.011)	(0.012)	(0.010)	(0.011)	(0.011)	(0.012)
India	0.005	0.007	0.008	0.009	0.005	0.007	0.007	0.008
	(0.010)	(0.011)	(0.011)	(0.012)	(0.010)	(0.011)	(0.011)	(0.012)
Other countries	0.009***	0.010***	0.011***	0.010***	0.008***	0.009***	0.010***	0.010***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Constant	0.407***	0.496***	0.550***	0.603***	0.406***	0.495***	0.549***	0.600***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Post-est. tests (p-values):								
Italy = France	0.609	0.984	0.912	0.783	0.520	0.902	0.903	0.787
Italy = Portugal	0.001	0.001	0.000	0.000	0.002	0.001	0.001	0.000
Italy = Germany	0.685	0.329	0.464	0.658	0.727	0.327	0.461	0.636
France = Portugal	0.030	0.009	0.004	0.002	0.057	0.016	0.007	0.004
France = Germany	0.953	0.465	0.523	0.592	0.843	0.512	0.516	0.578
Germany = Portugal	0.031	0.066	0.030	0.015	0.041	0.086	0.043	0.022
Switzerland = Portugal	0.064	0.040	0.049	0.047	0.070	0.039	0.041	0.036
Belgium = Portugal	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Controls	-	-	-	-	Yes	Yes	Yes	Yes
Observations	121,505	121,505	121,505	121,505	118,705	118,705	118,705	118,705

Notes: the table shows results from LPM estimations following the structure in Equation (1). The benchmark category is Spain. the category 'other countries' encompasses individuals with ancestors from the other 117 countries in our data. The lower panel shows p-values for Wald tests. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

coefficients remain virtually unchanged.

One potential concern is that outcomes from these cross-sectional estimations based on average estimates over 12 years might be concealing some dynamics over time that might affect the results. In a first step to dive into this issue, we use the panel structure of our dataset. Table 2.11 in the appendix shows the output of random-effects models with the same structure of equation 1 using all the years in our sample (2005 - 2016), and dummies for payments before the deadline and within 30, 180 and 360 days. The unit of observation in this case is the tax bill. Consequently, we cluster standard errors at the individual level. Results are very similar to the cross-sectional ones, also after the introduction of controls. All significant effects and point estimates are almost the same.

To further assess if results differ with the time period of analysis, we split our sample into periods of four and two years based on the due date of the bills, and estimate the same specifications from equation 1 within these time periods. Results for these time windows are very similar. Tables 2.12 and 2.13 in the appendix present the outcomes of estimations for periods of two years. Results for estimations by periods of four years (2005 to 2008, 2009 to 2012, and 2013 to 2016) are summarized in Table 2.5 below.[5]

Outcomes for Italian descendants are very similar to the ones obtained by averaging all years in our data, although the coefficients are slightly smaller. Taxpayers with Italian roots are more compliant than those with Spanish or Portuguese ancestors, irrespective of which time window is being considered. Similarly, effects on payments for individuals with Belgian descent remain negative and significant across all estimations. For individuals with French ancestry, only results for payments within 360 days appear to be robust across time periods, and the significance of the difference in payments within 30 days is not constant when looking at narrower time windows (see Tables 2.12 and 2.13 in the appendix). Outcomes for taxpayers with German and Swiss descent appear to be very sensitive to the choice of the period of analysis, which casts doubt on the reliability of estimates for these countries of origin obtained in the previous estimations.

[5]Payments within 180 days were excluded from the table for the sake of brevity. However, similar as in Table 2.4, effects on payments within 180 days are very similar to those within 360 days.

Table 2.5: Property Tax Payments by Country of Descent in Periods of 4 Years

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	2005 - 2008			2009 - 2012			2013 - 2016		
Payments:	On time	30 days	360 days	On time	30 days	360 days	On time	30 days	360 days
Italy	0.010***	0.013***	0.015***	0.015***	0.018***	0.016***	0.016***	0.018***	0.018***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
France	0.011***	0.018***	0.019***	0.012*	0.021***	0.024***	0.011	0.013*	0.015**
	(0.004)	(0.005)	(0.005)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Portugal	0.001	0.003	-0.000	-0.003	-0.004	-0.006	0.003	0.001	-0.004
	(0.004)	(0.005)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Germany	0.001	0.001	0.002	0.001	-0.002	-0.000	0.019***	0.017**	0.019***
	(0.004)	(0.005)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Israel	-0.000	-0.000	0.001	-0.002	-0.000	0.006	-0.001	0.007	0.008
	(0.006)	(0.007)	(0.008)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Switzerland	0.015**	0.020***	0.025***	0.014	0.016	0.016	0.016	0.019*	0.015
	(0.007)	(0.007)	(0.009)	(0.010)	(0.011)	(0.011)	(0.010)	(0.011)	(0.011)
Angola	0.008	0.012	0.019**	0.003	0.009	0.010	0.006	0.005	0.003
	(0.007)	(0.007)	(0.009)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Belgium	-0.074***	-0.098***	-0.127***	-0.129***	-0.153***	-0.166***	-0.110***	-0.128***	-0.131***
	(0.007)	(0.009)	(0.011)	(0.012)	(0.013)	(0.014)	(0.012)	(0.013)	(0.014)
India	-0.008	-0.009	-0.003	0.007	0.009	0.007	0.010	0.016	0.014
	(0.008)	(0.009)	(0.011)	(0.013)	(0.014)	(0.013)	(0.013)	(0.013)	(0.014)
Other countries	0.003	0.004*	0.004	0.001	0.003	0.002	0.007**	0.009**	0.010***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
Constant	0.277***	0.371***	0.525***	0.502***	0.611***	0.709***	0.488***	0.567***	0.646***
	(0.003)	(0.003)	(0.004)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)
	(0.003)	(0.003)	(0.004)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)
Post-est. tests (p-values):									
Italy = France	0.810	0.215	0.430	0.672	0.698	0.261	0.412	0.448	0.742
Italy = Portugal	0.0399	0.0374	0.00809	0.00831	0.00342	0.00204	0.0570	0.0243	0.00352
Italy = Germany	0.0357	0.0123	0.0210	0.0496	0.00614	0.0235	0.718	0.878	0.837
France = Portugal	0.0849	0.0131	0.00954	0.0897	0.0117	0.00147	0.396	0.243	0.0452
France = Germany	0.0779	0.00512	0.0196	0.233	0.0174	0.0110	0.384	0.663	0.692
Germany = Portugal	0.961	0.758	0.804	0.629	0.891	0.526	0.0937	0.117	0.0192
Observations	100,467	100,467	100,467	106,816	106,816	106,816	118,676	118,676	118,676

Notes: the table shows results from LPM estimations following the structure in Equation (1) within periods of 4 years.All specifications contain controls for tax amount and property area. The benchmark category is Spain. the category 'other countries' encompasses individuals with ancestors from the other 117 countries in our data. The lower panel shows p-values for Wald tests. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

So far we have found a consistent pattern of differences in property tax payments made within a period of time between some countries of origin. We have not yet attempted to study differences in a more direct measure of non-compliance. Therefore, in a next step we consider a different set of indicators for property tax compliance. First we split our sample into periods of three years. Within this time lapse we calculate the share of monthly bills payed before and after the deadline, and the share of bills for which no payment is observed at any point in time for each individual. We then estimate equation 1 with these variables on the left-hand side.

Results present a similar pattern as the estimations from above. Table 2.6 illustrates the outcomes for relevant countries. In all columns taxpayers with an Italian surname have a higher probability of paying before the deadline and consequently make fewer late payments as compared to individuals of Spanish descent in all the periods. However, the differences with respect to individuals with Portuguese ancestors are not consistent across estimations. Belgian origin is significantly correlated with a higher proportion of late payments and a smaller share of early payments with respect to Spanish descent in all specifications. Results for other countries of origin are not consistent across specifications.

Given that we do not observe the actual payment behavior of individuals who did not pay the tax bill before the last point of observation in our data, results for our measure of non-compliance based on late payments in Columns 2, 6, 10 and 14 of Table 2.6 might be biased. To correct for this, we estimated a two-stage Heckman model (Heckman, 1979) for the same 3-three year periods with the share of bills paid after the deadline as the dependent variable in the main equation. We use both total and constructed area, tax amount, and official value of the property as independent variables in the selection equation. Results from Table 2.6 suggest that truncation bias is indeed relevant for these estimations (the null hypothesis of no selection is rejected in all specifications: see the lower panels in Table 2.6), and consequently the statistical significance of the coefficients for the Italian and Belgian surnames does not hold in all the specifications, which casts doubts on the validity of our previous results of differences in non-compliance based on the share of bills paid after the deadline.

From the set of results we have so far, we can draw some general conclusions.

Table 2.6: Property Tax Payments by Country of Descent for Periods of 3 Years

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	2005 - 2007				2008 - 2010			
Share of payments:	Early	Late	Never	Late (heckman)	Early	Late	Never	Late (heckman)
Italy	0.006***	-0.012***	-0.012***	-0.009***	0.010***	-0.014***	-0.012***	-0.009**
	(0.001)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)	(0.004)
Belgium	-0.058***	0.055***	0.156***	0.020***	-0.117***	0.113***	0.150***	0.040**
	(0.006)	(0.007)	(0.014)	(0.008)	(0.011)	(0.012)	(0.013)	(0.020)
Constant	0.212***	0.753***	0.161***	0.673***	0.442***	0.497***	0.152***	0.293***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.013)
Post-est. tests (p-values):								
Italy = Portugal	0.132	0.018	0.005	0.883	0.004	0.158	0.000	0.514
Selection				0.000				0.000
Observations	98,775	98,775	98,775	98,775	103,993	103,993	103,993	103,994
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	2011 - 2013				2014 - 2016			
Share of payments:	Early	Late	Never	Late (heckman)	Early	Late	Never	Late (heckman)
Italy	0.014***	-0.016***	-0.013***	-0.008	0.012***	-0.009***	-0.009***	-0.005
	(0.003)	(0.003)	(0.003)	(0.005)	(0.002)	(0.002)	(0.002)	(0.006)
Belgium	-0.130***	0.141***	0.168***	0.047**	-0.085***	0.106***	0.122***	0.025
	(0.013)	(0.013)	(0.014)	(0.023)	(0.010)	(0.010)	(0.011)	(0.026)
Constant	0.527***	0.452***	0.202***	0.149***	0.375***	0.344***	0.202***	-0.120***
	(0.002)	(0.002)	(0.002)	(0.013)	(0.003)	(0.002)	(0.002)	(0.038)
Post-est. tests (p-values):								
Italy = Portugal	0.027	0.126	0.000	0.842	0.184	0.061	0.006	0.696
Selection				0.000				0.000
Observations	108,444	108,444	108,444	108,445	118,492	118,492	118,492	118,493

Notes: The table summarizes results from OLS estimations with the share of monthly payments within 36 months made early, late or never made on the left hand side. Columns 4, 8, 12 and 16 present outcomes from Heckman two-stage estimations of the share of late payments within 36 months as dependent variable. Heckman estimations use constructed area, tax amount and property value as independent variables in the selection equation. For these same columns, the table also shows the p-value of the null hypothesis of no selection bias. All specifications include controls for tax amount and property area. The benchmark category is Spain, estimates for France, Portugal, Germany, Israel, Switzerland, Angola, India, and a category encompassing individuals with ancestors from the other 117 countries in our data, are omitted from the table. The lower panel shows p-values for Wald tests. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

There are consistent patterns in differences in payment behavior between groups of taxpayers with a common descent. These results are robust to changes in the period of analysis, the introduction of controls and changes in the specifications. Individuals with Italian ancestors are more likely to pay the monthly property tax bills on time than taxpayers with Spanish ancestry. In turn, the latter are on average more compliant with their payments than individuals with Belgian roots. However, the extent to which non-compliance, understood as the proportion of bills paid after the deadline, differs across these countries of origin is unclear. The differences are not significant when correcting for the potential bias caused by the presence of truncation in our sample. As for differences in tax payments for the other countries of origin we analyzed, the estimates are not robust to changes in the parameters of the specifications, therefore cannot be regarded as valid.

2.3.3 Differences in tax compliance within countries of origin

Results from the previous section point to differences in timely compliance with property tax payments when grouping taxpayers according to the country from which their ancestors originated. However, countries generally encompass diverse regions that can be very different from each other in many regards. Traits like languages, institutions, social norms, and levels of economic performance differ drastically across regions in many countries around the world. More relevant to our setting, there is also some evidence of differences in tax morale at the within-country level (see, e.g., Torgler and Schneider, 2004). For example, it is a well-known fact that there exist strong differences in the economic performance between northern and southern latitudes in countries like Italy or Spain. Therefore analyzing differences in the compliance of individuals according to the origin of their ancestors at the country level, might conceal potentially relevant regional differences in social norms related to property tax compliance.

To uncover potential differences in property tax payments of individuals according to their ancestry at the within-country level of disaggregation, we perform a range of estimations similar to the ones analyzed in the previous section. This time,

however, we include a set of dummies for the region inside the country from which the taxpayers' ancestors came from. As mentioned in section 2.2, we were able to identify the region of descent of 85% of taxpayers with ancestors from Spain, Italy, and Germany. Therefore, for these countries of origin, we limit the analysis to said observations.

The regions were formed according to the number of observations assigned to each of the NUTS-1 units in each country. For Spain, we divided observations between north and south. In the case of Italy, the division was made between north, center and south. French origins were assigned to Paris basin, east, north, and the rest of the country. Germany was divided into North Rhine-Westphalia, Baden-Württenberg, Berlin, Bavaria and the rest of the states. See Table 2.9 in the appendix for detailed information about the composition of the regional divisions used here and the number of observations assigned to each.

We then proceed to estimate all the specifications from the last section with dummies for the aforementioned regions, keeping dummies for the remaining countries we analyzed in the last section on the right hand side. Table 2.7 presents the results for cross-sectional estimations analogous to equation 1, with payments made on time and within 30, 180, and 360 days on the left-hand side (Outcomes for the additional estimations are available in Tables 2.14 to 2.18 in the appendix).

In general, findings are very similar to those from the previous section. Taking descendants from southern Spaniards as a benchmark, individuals with ancestors from all Italian regions are more likely to pay the bills for property tax on time. They are also more likely to make payments within 30, 180, and 360 days. There are no significant differences in payment patterns between Italian regions. These findings are consistent across all specifications. Regarding the other countries, taxpayers with ancestry from the Paris basin of France are more likely to make payments within 360 days, and in some regressions there is some indication that having origins from the north of the country is correlated with a higher likelihood of making payments within 180 and 360 days, however results are not robust to changes in the specifications. There are no consistent payment differences for individuals with ancestors from the regions in Germany. Results from estimations of non-compliance based on the share of monthly tax bills paid late within three years are also not significant when

correcting for truncation bias.

Table 2.7: Property Tax Payments by Regions of Origin

Payments	(1) On time	(2) 30 days	(3) 180 days	(4) 360 days	(5) On time	(6) 30 days	(7) 180 days	(8) 360 days
Northern Spain	0.004	0.006*	0.007*	0.007*	0.005	0.007*	0.008**	0.008**
	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
Northern Italy	0.021***	0.024***	0.025***	0.025***	0.021***	0.024***	0.025***	0.025***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Central Italy	0.015***	0.018***	0.017***	0.016***	0.016***	0.019***	0.018***	0.017***
	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)
Southern Italy	0.021***	0.025***	0.026***	0.026***	0.022***	0.026***	0.026***	0.026***
	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)
France Paris basin	0.019*	0.023**	0.025**	0.028***	0.019*	0.022**	0.025**	0.029***
	(0.010)	(0.010)	(0.011)	(0.011)	(0.010)	(0.011)	(0.011)	(0.011)
Eastern France	0.007	0.017	0.021*	0.020*	0.009	0.019*	0.023**	0.023**
	(0.010)	(0.011)	(0.011)	(0.011)	(0.010)	(0.011)	(0.011)	(0.011)
Northern France	0.025**	0.032***	0.031***	0.032***	0.023**	0.030**	0.029**	0.029**
	(0.011)	(0.012)	(0.012)	(0.012)	(0.011)	(0.012)	(0.012)	(0.012)
Rest of France	0.011	0.015	0.015	0.015	0.011	0.016	0.017	0.016
	(0.014)	(0.015)	(0.015)	(0.016)	(0.014)	(0.015)	(0.016)	(0.016)
North Rhine-Westphalia	-0.007	-0.011	-0.008	-0.003	-0.007	-0.011	-0.008	-0.005
	(0.019)	(0.021)	(0.021)	(0.022)	(0.019)	(0.021)	(0.021)	(0.022)
Baden-Württenberg	0.012	0.017	0.022	0.023	0.016	0.020	0.025	0.025
	(0.019)	(0.020)	(0.020)	(0.021)	(0.019)	(0.021)	(0.020)	(0.021)
Berlin	0.019	0.020	0.024	0.024	0.020	0.022	0.025	0.026
	(0.015)	(0.016)	(0.016)	(0.016)	(0.015)	(0.016)	(0.016)	(0.016)
Bavaria	-0.021	-0.028	-0.031	-0.030	-0.024	-0.032	-0.034	-0.033
	(0.019)	(0.021)	(0.021)	(0.022)	(0.019)	(0.021)	(0.022)	(0.023)
Rest of Germany	0.034***	0.037***	0.039***	0.041***	0.035***	0.038***	0.041***	0.042***
	(0.011)	(0.012)	(0.012)	(0.012)	(0.011)	(0.012)	(0.012)	(0.012)
Belgium	-0.089***	-0.106***	-0.113***	-0.119***	-0.089***	-0.106***	-0.113***	-0.119***
	(0.010)	(0.011)	(0.012)	(0.012)	(0.010)	(0.011)	(0.012)	(0.012)
Constant	0.405***	0.492***	0.546***	0.599***	0.404***	0.491***	0.545***	0.595***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Controls	-	-	-	-	Yes	Yes	Yes	Yes
Observations	114,969	114,969	114,969	114,969	112,315	112,315	112,315	112,315

Notes: the table shows results from LPM estimations following the structure in Equation (1). The benchmark category is southern Spain. Portugal, Israel, Switzerland, Angola, India and a category including the other 117 countries in our data are excluded from the table. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Contrary to what one would expect given the existing contrasts between regions inside a country, we do not find evidence of differences in payment patterns between taxpayers with descent from different regions of a country. This suggests that, despite regional differences in many regards, the inherited part of social values related to tax compliance is shared by the average descendant of the individuals of a country. In our setting, the differences in this inherited trait translate into differences on timely payments between individuals with Spanish and Italian descent.

2.4 Long-term persistence of social norms

The existing literature on intergenerational transmission of social values, and its direct effect on tax compliance documents the persistence of such traits over the course of one generation (Frimmel *et al.*, 2018; Halla, 2012). In this section we study the persistence of social traits related to tax compliance and economic outcomes from the country of origin of the ancestors of tax payers in the city of Santa Fe.

Tax morale is an important determinant of tax compliance (Halla, 2012), and according to Luttmer and Singhal (2014), it encompasses various cultural characteristics, like the level of corruption in a country (DeBacker *et al.*, 2015), or the size of the shadow economy (Halla, 2012; Schneider, 2005). Following these ideas, we turn to investigate the existence of potential correlations between current measures of crime, corruption, and tax morale in countries of descent and indicators of property tax compliance for individuals with ancestors stemming from those countries.

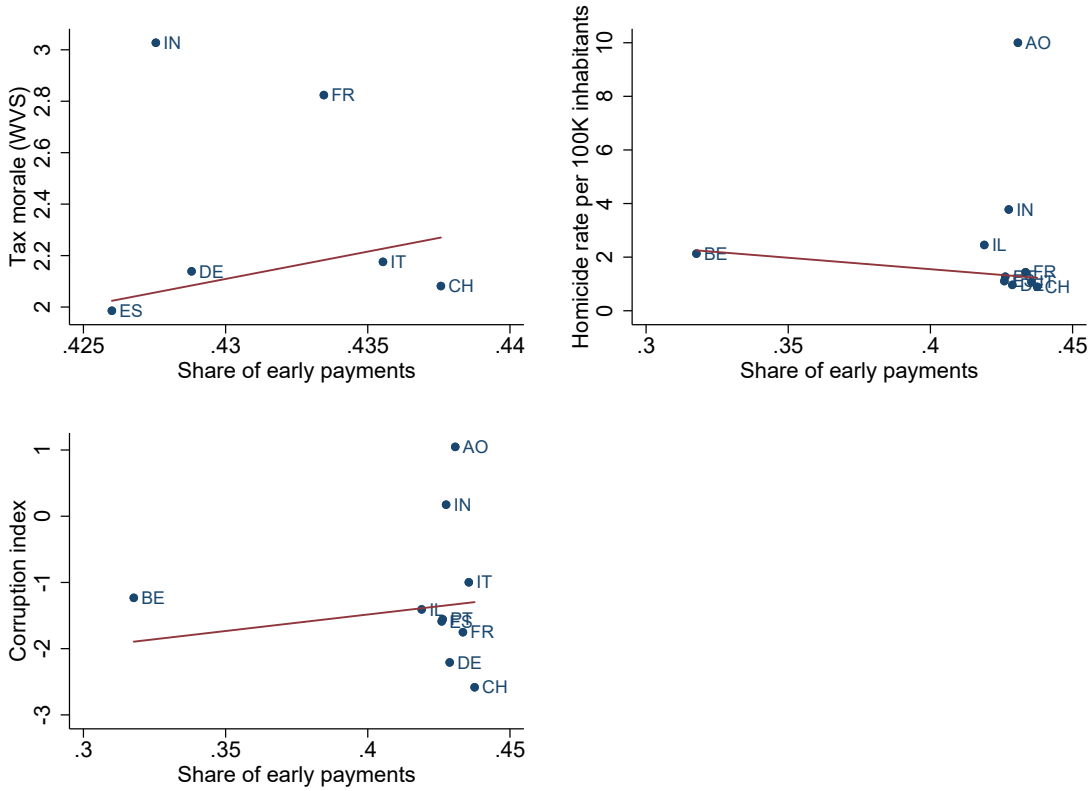
For this exercise we use the average of individual share of payments made on time, late, and never made by country of origin as measures of property tax compliance. Indicators of corruption in countries of origin were taken from the corruption index in Fisman and Miguel (2007). It assigns each country a score based in subjective perceptions of corruption. The index is centered around zero, with higher values indicating more corruption. Measures for tax morale come from the World Values Survey, as is standard in the literature. Finally, as an indicator of crime we use homicide rates by country from the United Nations office for drugs and crime.

Figure 2.1 shows scatter plots illustrating the correlation between the share of property tax payments made on time during the whole period of analysis and indicators for tax morale, corruption and crime for some of the ten countries of origin we used for the analysis in the previous section[6]. Similar figures showing correlations for other measures of property tax compliance can be found in Graphs 2.2 and 2.3 in the appendix.

We do not find evidence of significant correlations between property tax compliance in the city of Santa Fe and tax morale or measures of unlawful behavior in the countries of origin. There is also no significant relationship between these

[6]The number of countries in each panel of the the graph was dictated by data availability

Figure 2.1: Property tax compliance correlations



Notes: The top-left panel uses tax morale indicators from the World Values Survey averaged over the period 2000-2014. The top-right panel uses homicide rates per 100.000 inhabitants from the United Nations office for drugs and crime over the period 2000-2012. The lower-left panel uses the corruption index from Fisman and Miguel (2007). The variable on the x axis in all panels is the share of early payments made during the whole period of analysis.

variables when all countries of origin in our data are included[7]. Results are similarly inconclusive for the other measures of tax compliance.

The absence of correlation between tax compliance measures and tax morale in the country of origin might suggest that social values related to tax compliance could have evolved differently in the country of origin and in Argentina since the time of migration. These values developed over time in both countries in such a way that present-day outcomes are no longer correlated. One reason for this is that individuals that emigrated to Argentina during the 19th and 20th centuries were systematically different from their countrymen that chose to stay behind (Abramitzky *et al.*, 2012; Klein, 1983; Sánchez-Alonso, 2019), and therefore might have transmitted different social values to their respective descendants. Another factor that might help to explain to some extent is the convergence of social values over time Giavazzi *et al.* (2019). Present-day taxpayers might be the third or fourth generation that has been living in Argentina, so their set of social values might have converged to those existing in the country to such an extent that they are no longer related to the social values of their ancestors.

2.5 Concluding remarks

We analyzed the intergenerational transmission of social values related to tax compliance. Exploiting the genealogical information contained in the surnames of present-day taxpayers, we identified the country of origin of their ancestors. This information, together with administrative records of property tax payments, allowed us to study differences in compliance by taxpayers' country of descent. Results from a broad range of specifications document the presence of intergenerational transmission of social norms related to tax compliance. We find significant differences in compliance for some groups of ancestry in our data. Taxpayers with a common country of descent share similar social values related to tax compliance, despite their ancestors originating from considerably different regions within the same country. Consistent with convergence of social values over time and self-selection of migrants, we do not find evidence of significant correlations between property tax compliance

[7]These set of results are available from the author upon request.

in our setting and contemporary indicators of tax morale and unlawful behavior in the taxpayers ancestors' country of origin. Our data did not allow us to analyze the degree of convergence of social values over time or address the issue of self-selection of migrants. This seems like a promising avenue for future research.

2.6 Appendix

Table 2.8: List of Countries and Number of Individuals

Country name	N	Country name	N	Country name	N	Country name	N
Spain	42,541	Russian Federation	285	Morocco	118	Togo	44
Italy	38,721	Armenia	285	Uganda	115	Qatar	43
France	4,234	Tunisia	281	Serbia	115	Yemen, Rep.	40
Portugal	3,869	Cote D'Ivoire	276	Egypt, Arab Rep.	113	Myanmar	35
Germany	3,801	Tanzania	272	Bulgaria	107	North Korea	34
Israel	1,851	Ukraine	269	Bahrain	104	Gambia, The	34
Switzerland	1,576	Madagascar	261	Botswana	102	Montenegro	33
Angola	1,523	Niger	260	Norway	101	Cameroon	32
Belgium	1,158	Iraq	242	China	101	Jordan	28
India	1,046	Vietnam	237	Mozambique	97	Gabon	25
Netherlands	837	Mali	236	Taiwan, China	96	Estonia	24
Austria	748	West Bank And Gaza	230	Croatia	94	Kyrgyz Republic	24
United Kingdom	740	Rwanda	228	Lithuania	94	Malawi	22
Poland	707	Ghana	225	Comoros	93	Afghanistan	17
Sri Lanka	543	Cyprus	223	Korea, Rep.	83	Libya	16
Iceland	537	Japan	219	Hungary	74	Bosnia And Herzegovina	15
Mongolia	492	Senegal	218	Mauritius	74	Hong Kong, China	12
Indonesia	478	Kazakhstan	204	Syrian Arab Republic	73	Eritrea	10
Romania	460	Burundi	201	Mauritania	72	Uzbekistan	7
Turkey	441	Nigeria	200	Zambia	71	Oman	5
Ireland	430	Finland	199	Zimbabwe	67	Kuwait	5
Moldova	424	Bangladesh	197	Macedonia, Fyr	66	Central African Republic	4
Denmark	420	Georgia	176	Nepal	64	United Arab Emirates	2
Pakistan	400	Benin	162	Chad	61	Turkmenistan	1
Lebanon	397	Sweden	162	Somalia	53	Tajikistan	1
Guinea	370	Greece	155	Slovak Republic	52		
Ethiopia	368	Liberia	144	Thailand	51		
South Africa	358	Kenya	131	Azerbaijan	51		
Namibia	357	Iran, Islamic Rep.	131	Congo, Rep.	50		
Algeria	336	Congo, Dem. Rep.	131	Sudan	50		
Lesotho	332	Malaysia	130	Brunei Darussalam	47		
Czech Republic	331	Latvia	126	Cambodia	45		
Belarus	291	Slovenia	126	Lao Pdr	45		
Burkina Faso	289	Albania	121	Saudi Arabia	44		

Notes: The table shows the complete list of countries of origin of the ancestors of present-day taxpayers in the city of Santa Fe. The table also shows the number of individuals with ancestry from each country.

Table 2.9: List of Regions Within Countries

Country	Region of analysis	N	NUTS - 2	Country	Region of analysis	N	NUTS - 2
Spain	North	25,996	Madrid	France	Paris basin	1,208	Île-de-France
			Galicia				Champagne-Ardenne
			Basque Country				Picardy
			Asturias				Upper Normandy
			Balear Ilands				Centre
			Castilla and Leon				Lower Normandy
			Cataluna				Burgundy
			Navarra		East	1,169	Lorraine
			Aragon				Alsace
			Cantabria				Franche-Comté
			Rioja		North	983	Nord-Pas-de-Calais
	South	13,436	Andalucia		Rest	564	Pays de la Loire
			Canarias				Brittany
			Valencia				Poitou-Charentes
			Murcia				Aquitaine
			Extremadura				Midi-Pyrénées
			Castilla-La Mancha				Limousin
			Melilla				Rhône-Alpes
			Ceuta				Auvergne
Italy	North	22,211	Lombardy				Languedoc-Roussillon
			Piedmont				Provence-Alpes-Côte d'Azur
			Veneto				Corsica
			Friuli-Venezia-Giulia	Germany	Berlin	521	Berlin
			Emilia-Romagna		Baden-Württemberg	321	Baden-Württemberg
			Liguria		North Rhine-Westphalia	302	North Rhine-Westphalia
			Trentino-South Tyrol		Bavaria	283	Bavaria
			Aosta Valley		Rest	962	Lower Saxony
	Center	5,777	Lazio				Hesse
			Tuscany				Rhineland-Palatinate
			Marche				Hamburg
			Umbria				Schleswig-Holstein
	South	9,028	Siciliy				Saxony
			Campania				Saarland
			Apulia				Bremen
			Sardinia				Brandenburg
			Calabria				Mecklenburg-Vorpommern
			Abruzzo				Thuringia
			Basilicata				Saxony-Anhalt
			Molise				

Notes: The table shows the groups of NUTS-2 entities that were used in the analysis in section 2.3.3 to produce Table 2.9. The table shows the number of surnames that originated from a specific region within a country. The regions into which NUTS-2 entities are grouped were selected according to socio-economic relevance and number of observations.

Table 2.10: Differences in Wealth Measures (10 Countries)

Variables:	(1) Property area (m)	(2) Built area (sq. Mts.) (m)	(3) Tax amount (Pesos)	(4) Property value ('000s Pesos)	(5) Value per m (Pesos)
Italy	595.813	-3.473	1.543	10.921	268.068***
	(701.805)	(2.640)	(2.291)	(30.091)	(51.450)
France	-811.687***	-9.289***	-3.561	-30.340	117.020
	(289.319)	(3.473)	(2.500)	(19.625)	(71.636)
Portugal	46.440	-7.464*	-0.982	-6.243	176.255
	(691.924)	(4.029)	(7.034)	(27.331)	(156.514)
Germany	-622.936*	-6.266	-3.742	-34.846*	653.059***
	(329.790)	(4.543)	(2.601)	(20.482)	(136.429)
Israel	4,030.886	-2.700	20.000	-25.065	306.039**
	(4,865.782)	(5.936)	(22.689)	(22.599)	(149.449)
Switzerland	1,584.926	-2.287	-1.046	39.620	-0.521
	(1,705.888)	(7.801)	(4.259)	(49.892)	(108.126)
Angola	-789.133***	-2.386	-4.230	10.904	-97.240
	(295.175)	(6.825)	(3.056)	(36.913)	(166.683)
Belgium	-743.421**	-31.716***	-14.098***	-119.583***	-891.806***
	(307.308)	(5.177)	(5.358)	(19.864)	(101.396)
India	-919.168***	0.921	6.028	-29.763	64.405
	(289.188)	(12.436)	(11.000)	(34.722)	(137.009)
Other countries	559.653	-5.019*	3.618	-19.097	130.819***
	(622.292)	(2.970)	(3.210)	(16.420)	(44.142)
Constant	1,234.222***	119.592***	85.367***	356.400***	2,899.670***
	(284.481)	(2.293)	(1.769)	(14.061)	(25.306)
Post-est. tests (p-values):					
Italy = France	0.029	0.046	0.026	0.168	0.061
Italy = Portugal	0.541	0.263	0.717	0.628	0.568
Italy = Germany	0.066	0.499	0.028	0.133	0.006
France = Portugal	0.175	0.665	0.714	0.375	0.725
France = Germany	0.281	0.521	0.945	0.824	0.000
Germany = Portugal	0.305	0.816	0.696	0.303	0.020
Observations	118,708	118,708	121,809	118,708	118,656

Notes: the table shows the outcomes of OLS estimations. The benchmark category is Spain. the category 'other countries' encompasses individuals with ancestors from the other 117 countries in our data. Value per square meter in Column 5 was calculated by dividing the valuation of the property by its area. Robust standard errors are given in parenthesis. The lower panel shows p-values for Wald tests. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 2.11: Property Tax Payments by Country of Descent - Panel Estimations

Payments:	(1) On time	(2) 30 days	(4) 360 days	(5) On time	(6) 30 days	(8) 360 days
Italy	0.016***	0.018***	0.018***	0.016***	0.018***	0.018***
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
France	0.014***	0.019***	0.021***	0.013**	0.018***	0.020***
	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)
Portugal	-0.002	-0.002	-0.005	-0.001	-0.001	-0.004
	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)
Germany	0.013**	0.012*	0.015**	0.014**	0.012*	0.015**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Israel	0.003	0.006	0.007	0.005	0.007	0.008
	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.009)
Switzerland	0.016*	0.019**	0.017*	0.017**	0.020**	0.019**
	(0.008)	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)
Angola	0.005	0.007	0.009	0.005	0.007	0.010
	(0.008)	(0.009)	(0.009)	(0.008)	(0.009)	(0.010)
Belgium	-0.096***	-0.115***	-0.127***	-0.095***	-0.114***	-0.127***
	(0.010)	(0.011)	(0.012)	(0.010)	(0.011)	(0.012)
India	0.005	0.007	0.009	0.005	0.007	0.008
	(0.010)	(0.011)	(0.011)	(0.010)	(0.011)	(0.012)
Other countries	0.008***	0.009***	0.010***	0.007**	0.009***	0.009***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Constant	0.415***	0.504***	0.610***	0.413***	0.502***	0.608***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Post-est. tests (p-values):						
Italy = France	0.653	0.958	0.729	0.545	0.938	0.761
Italy = Portugal	0.001	0.000	0.000	0.002	0.001	0.000
Italy = Germany	0.558	0.248	0.550	0.608	0.253	0.537
France = Portugal	0.026	0.007	0.002	0.057	0.015	0.003
France = Germany	0.901	0.357	0.479	0.962	0.416	0.490
Germany = Portugal	0.043	0.084	0.018	0.058	0.113	0.028
Controls	-	-	-	Yes	Yes	Yes
Observations	13,948,139	13,948,139	13,948,139	13,820,129	13,820,129	13,820,129
Number of clusters	121,505	121,505	121,505	118,705	118,705	118,705

Notes: The table shows the results of random effects panel estimations. The benchmark category is Spain. the category 'other countries' encompasses individuals with ancestors from the other 117 countries in our data. The lower panel shows p-values for Wald tests. Standard errors clustered at the individual level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.12: Property Tax Payments by Country of Descent in Periods of 2 Years (2005 - 2010)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	2005 - 2006			2007 - 2008			2009 - 2010		
Payments:	On time	30 days	360 days	On time	30 days	360 days	On time	30 days	360 days
Italy	0.004***	0.004***	0.010***	0.013***	0.015***	0.015***	0.014***	0.018***	0.016***
	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
France	0.004*	0.007***	0.011***	0.015**	0.024***	0.022***	0.009	0.018**	0.021***
	(0.002)	(0.002)	(0.004)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Portugal	-0.001	-0.000	-0.002	0.003	0.003	0.001	-0.006	-0.005	-0.005
	(0.002)	(0.002)	(0.004)	(0.007)	(0.008)	(0.008)	(0.007)	(0.008)	(0.008)
Germany	0.002	0.001	0.004	0.002	0.000	0.001	-0.004	-0.006	-0.005
	(0.002)	(0.002)	(0.004)	(0.007)	(0.008)	(0.008)	(0.007)	(0.008)	(0.008)
Israel	0.002	0.001	0.000	-0.004	-0.005	-0.003	-0.003	-0.004	0.004
	(0.003)	(0.003)	(0.006)	(0.010)	(0.011)	(0.011)	(0.010)	(0.011)	(0.011)
Switzerland	0.008**	0.010***	0.023***	0.019*	0.025**	0.023**	0.011	0.019*	0.023**
	(0.003)	(0.004)	(0.007)	(0.010)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Angola	0.003	0.007*	0.017***	0.008	0.010	0.014	-0.001	0.008	0.008
	(0.004)	(0.004)	(0.007)	(0.011)	(0.012)	(0.011)	(0.011)	(0.012)	(0.011)
Belgium	-0.032***	-0.039***	-0.085***	-0.115***	-0.156***	-0.168***	-0.125***	-0.153***	-0.162***
	(0.004)	(0.004)	(0.008)	(0.012)	(0.014)	(0.014)	(0.012)	(0.014)	(0.015)
India	-0.003	-0.005	-0.007	-0.014	-0.017	-0.007	0.006	0.005	0.008
	(0.004)	(0.004)	(0.008)	(0.013)	(0.014)	(0.014)	(0.013)	(0.014)	(0.014)
Other countries	0.001	0.001	0.002	0.001	0.003	0.002	-0.002	0.002	0.000
	(0.001)	(0.001)	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Constant	0.123***	0.152***	0.355***	0.435***	0.592***	0.697***	0.482***	0.617***	0.720***
	(0.001)	(0.001)	(0.002)	(0.005)	(0.006)	(0.005)	(0.002)	(0.002)	(0.002)
Post-est. tests (p-values):									
Italy = France	0.865	0.347	0.771	0.740	0.237	0.299	0.441	0.963	0.441
Italy = Portugal	0.021	0.046	0.004	0.152	0.105	0.068	0.005	0.003	0.007
Italy = Germany	0.210	0.165	0.146	0.112	0.049	0.059	0.013	0.002	0.006
France = Portugal	0.108	0.030	0.017	0.185	0.036	0.032	0.111	0.019	0.009
France = Germany	0.411	0.084	0.189	0.147	0.019	0.028	0.176	0.016	0.008
Germany = Portugal	0.443	0.674	0.292	0.892	0.792	0.952	0.820	0.941	0.968
Observations	97,193	97,193	97,193	100,065	100,065	100,065	102,039	102,039	102,039

Notes: the table shows results from LPM estimations following the structure in Equation (1) within periods of 2 years. All specifications contain controls for tax amount and property area. The benchmark category is Spain. the category 'other countries' encompasses individuals with ancestors from the other 117 countries in our data. The lower panel shows p-values for Wald tests. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.13: Property Tax Payments by Country of Descent in Periods of 2 Years (2011 - 2016)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	2011 - 2012			2013 - 2014			2015 - 2016		
Payments:	On time	30 days	360 days	On time	30 days	360 days	On time	30 days	360 days
Italy	0.016***	0.018***	0.017***	0.014***	0.015***	0.014***	0.015***	0.018***	0.017***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
France	0.014**	0.020***	0.023***	0.010	0.012*	0.015**	0.009	0.011	0.014*
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Portugal	0.001	-0.001	-0.007	0.007	0.003	-0.006	-0.002	-0.001	-0.004
	(0.007)	(0.008)	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)	(0.008)	(0.008)
Germany	0.006	0.002	0.005	0.014*	0.011	0.011	0.016**	0.014*	0.018**
	(0.007)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Israel	-0.004	0.001	0.006	-0.008	0.004	0.012	-0.001	0.004	0.001
	(0.010)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)
Switzerland	0.018	0.018	0.014	0.014	0.018	0.011	0.012	0.016	0.014
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Angola	0.008	0.010	0.011	0.003	0.003	-0.000	0.007	0.004	0.004
	(0.011)	(0.012)	(0.011)	(0.011)	(0.012)	(0.012)	(0.011)	(0.012)	(0.012)
Belgium	-0.134***	-0.155***	-0.170***	-0.134***	-0.154***	-0.166***	-0.109***	-0.128***	-0.128***
	(0.013)	(0.014)	(0.014)	(0.013)	(0.014)	(0.014)	(0.012)	(0.013)	(0.014)
India	0.009	0.013	0.010	0.009	0.009	0.008	0.009	0.021	0.016
	(0.013)	(0.014)	(0.014)	(0.013)	(0.014)	(0.014)	(0.013)	(0.014)	(0.014)
Other countries	0.003	0.004	0.004	0.001	0.003	0.005	0.008**	0.010**	0.009**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Constant	0.532***	0.621***	0.718***	0.537***	0.620***	0.707***	0.479***	0.562***	0.637***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Post-est. tests (p-values):									
Italy = France	0.781	0.799	0.359	0.609	0.687	0.920	0.403	0.391	0.596
Italy = Portugal	0.037	0.011	0.001	0.317	0.124	0.009	0.018	0.014	0.005
Italy = Germany	0.178	0.036	0.102	0.947	0.585	0.670	0.952	0.618	0.933
France = Portugal	0.167	0.034	0.002	0.699	0.379	0.036	0.235	0.208	0.079
France = Germany	0.409	0.075	0.056	0.748	0.904	0.692	0.513	0.804	0.652
Germany = Portugal	0.593	0.742	0.241	0.490	0.458	0.097	0.072	0.140	0.031
Observations	106,541	106,541	106,541	109,954	109,954	109,954	118,202	118,202	118,202

Notes: the table shows results from LPM estimations following the structure in Equation (1) within periods of 2 years. All specifications contain controls for tax amount and property area. The benchmark category is Spain. the category 'other countries' encompasses individuals with ancestors from the other 117 countries in our data. The lower panel shows p-values for Wald tests. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.14: Property Tax Payments by Region of Descent - Panel Estimations

Payments:	(1) On time	(2) 30 days	(3) 360 days	(4) On time	(5) 30 days	(6) 360 days
Northern Spain	0.004	0.007*	0.007*	0.005	0.007*	0.008**
	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
Northern Italy	0.021***	0.023***	0.024***	0.021***	0.024***	0.025***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Central Italy	0.016***	0.019***	0.017***	0.017***	0.020***	0.018***
	(0.005)	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)
Southern Italy	0.020***	0.025***	0.025***	0.021***	0.025***	0.025***
	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)
France Paris basin	0.019**	0.023**	0.029***	0.018*	0.022**	0.029***
	(0.010)	(0.010)	(0.011)	(0.010)	(0.010)	(0.011)
Eastern France	0.007	0.017	0.020*	0.008	0.019*	0.023**
	(0.010)	(0.011)	(0.011)	(0.010)	(0.011)	(0.011)
Northern France	0.025**	0.032***	0.032***	0.023**	0.030**	0.029**
	(0.011)	(0.011)	(0.012)	(0.011)	(0.012)	(0.012)
Rest of France	0.011	0.015	0.015	0.011	0.015	0.015
	(0.014)	(0.015)	(0.015)	(0.014)	(0.015)	(0.016)
North Rhine-Westphalia	-0.005	-0.009	-0.001	-0.005	-0.009	-0.003
	(0.019)	(0.020)	(0.021)	(0.019)	(0.020)	(0.022)
Baden-Württenberg	0.013	0.018	0.024	0.016	0.021	0.026
	(0.019)	(0.020)	(0.020)	(0.019)	(0.020)	(0.021)
Berlin	0.017	0.018	0.022	0.017	0.019	0.024
	(0.014)	(0.016)	(0.016)	(0.015)	(0.016)	(0.016)
Bavaria	-0.021	-0.028	-0.030	-0.024	-0.032	-0.033
	(0.019)	(0.021)	(0.022)	(0.019)	(0.021)	(0.022)
Rest of Germany	0.032***	0.035***	0.039***	0.034***	0.037***	0.041***
	(0.011)	(0.011)	(0.012)	(0.011)	(0.012)	(0.012)
Belgium	-0.093***	-0.111***	-0.123***	-0.093***	-0.110***	-0.123***
	(0.010)	(0.011)	(0.012)	(0.010)	(0.011)	(0.012)
Constant	0.412***	0.500***	0.606***	0.411***	0.499***	0.604***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Controls	-	-	-	Yes	Yes	Yes
Observations	13,179,731	13,179,731	13,179,731	13,058,200	13,058,200	13,058,200
Number of clusters	114,969	114,969	114,969	112,315	112,315	112,315

Notes: The table shows the results of random effects panel estimations. The benchmark category is northern Spain. The table displays coefficients of dummies for region of origin of the individuals' ancestors within Spain, Italy, France and Germany (See Table 2.9 for information about the geographical entities grouped under each region). Observations for these countries were limited to those that were assigned a region within the respective country, otherwise they were excluded from the analysis. Estimates for Portugal, Israel, Switzerland, Angola, India and a category grouping the remaining 117 countries in our data are omitted from the table. Standard errors clustered at the individual level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.15: Property Tax Payments by Region of Descent for Periods of 4 Years

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	2005 - 2008			2009 - 2012			2013 - 2016		
Payments:	On time	30 days	360 days	On time	30 days	360 days	On time	30 days	360 days
Northern Spain	0.001	0.004	0.005	0.003	0.005	0.007	0.004	0.006	0.008*
	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)
Northern Italy	0.011***	0.015***	0.019***	0.018***	0.021***	0.020***	0.019***	0.021***	0.024***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)
Central Italy	0.010**	0.015***	0.019***	0.019***	0.024***	0.023***	0.015**	0.018***	0.017**
	(0.004)	(0.004)	(0.005)	(0.006)	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)
Southern Italy	0.012***	0.017***	0.018***	0.019***	0.024***	0.022***	0.021***	0.025***	0.024***
	(0.003)	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
France Paris basin	0.014*	0.019**	0.024**	0.022*	0.026**	0.033***	0.015	0.020	0.031**
	(0.008)	(0.008)	(0.010)	(0.012)	(0.013)	(0.012)	(0.012)	(0.013)	(0.013)
Eastern France	0.011	0.025***	0.025**	0.013	0.027**	0.028**	-0.003	0.003	0.009
	(0.008)	(0.009)	(0.010)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Northern France	0.007	0.020**	0.025**	0.021	0.032**	0.033**	0.025*	0.024*	0.023
	(0.008)	(0.009)	(0.011)	(0.013)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
Rest of France	0.009	0.018	0.014	-0.005	0.005	0.013	0.014	0.016	0.011
	(0.011)	(0.013)	(0.015)	(0.018)	(0.019)	(0.018)	(0.018)	(0.019)	(0.019)
North Rhine-Westphalia	0.000	-0.003	-0.000	0.016	0.015	0.015	-0.016	-0.017	-0.002
	(0.015)	(0.016)	(0.020)	(0.024)	(0.025)	(0.025)	(0.023)	(0.025)	(0.025)
Baden-Württenberg	0.012	0.011	0.010	-0.012	-0.004	0.019	0.015	0.032	0.036
	(0.015)	(0.016)	(0.019)	(0.024)	(0.025)	(0.024)	(0.023)	(0.024)	(0.024)
Berlin	0.007	0.012	0.014	0.007	0.004	-0.004	0.025	0.026	0.034*
	(0.011)	(0.013)	(0.015)	(0.018)	(0.020)	(0.019)	(0.018)	(0.019)	(0.019)
Bavaria	-0.009	-0.016	-0.008	-0.024	-0.028	-0.029	-0.027	-0.040	-0.041
	(0.015)	(0.016)	(0.020)	(0.025)	(0.026)	(0.026)	(0.025)	(0.026)	(0.026)
Rest of Germany	0.003	0.011	0.014	0.009	0.010	0.015	0.039***	0.039***	0.042***
	(0.009)	(0.010)	(0.011)	(0.014)	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)
Belgium	-0.074***	-0.096***	-0.124***	-0.127***	-0.149***	-0.162***	-0.107***	-0.124***	-0.127***
	(0.008)	(0.009)	(0.011)	(0.012)	(0.014)	(0.014)	(0.013)	(0.014)	(0.014)
Constant	0.277***	0.370***	0.523***	0.499***	0.608***	0.706***	0.486***	0.564***	0.641***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Observations	95,139	95,139	95,139	101,259	101,259	101,259	112,592	112,592	112,592

Notes: the table shows results from LPM estimations following the structure in Equation (1) with dummies for the regions of origin of the individuals' ancestors within Spain, Italy, France and Germany within periods of 4 years (See Table 2.9 for information about the geographical entities grouped under each region). Observations for these countries were limited to those that were assigned a region within the respective country, otherwise they were excluded from the analysis. Estimates for Portugal, Israel, Switzerland, Angola, India and a category grouping the remaining 117 countries in our data are omitted from the table. All specifications contain controls for tax amount and property area. The benchmark category is southern Spain. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.16: Property Tax Payments by Region of Descent for Periods of 2 Years (2005 - 2010)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	2005 - 2006			2007 - 2008			2009 - 2010		
Payments:	On time	30 days	360 days	On time	30 days	360 days	On time	30 days	360 days
Northern Spain	0.001	0.002*	0.003	-0.000	0.004	0.006	0.001	0.005	0.004
	(0.001)	(0.001)	(0.003)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Northern Italy	0.005***	0.006***	0.012***	0.013***	0.018***	0.019***	0.015***	0.019***	0.017***
	(0.001)	(0.001)	(0.003)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Central Italy	0.005**	0.007***	0.014***	0.012*	0.018***	0.020***	0.019***	0.026***	0.023***
	(0.002)	(0.002)	(0.004)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Southern Italy	0.005***	0.007***	0.013***	0.017***	0.023***	0.019***	0.017***	0.024***	0.019***
	(0.002)	(0.002)	(0.003)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
France Paris basin	0.008**	0.009**	0.016**	0.021*	0.029**	0.032**	0.024*	0.031**	0.032**
	(0.004)	(0.004)	(0.008)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Eastern France	0.002	0.010**	0.015*	0.014	0.032**	0.028**	0.003	0.018	0.017
	(0.004)	(0.005)	(0.008)	(0.012)	(0.013)	(0.013)	(0.013)	(0.014)	(0.013)
Northern France	0.005	0.010**	0.018**	0.009	0.028*	0.031**	0.003	0.018	0.030**
	(0.004)	(0.005)	(0.009)	(0.013)	(0.015)	(0.014)	(0.014)	(0.015)	(0.014)
Rest of France	0.006	0.007	0.004	0.006	0.012	0.008	0.001	0.009	0.006
	(0.006)	(0.007)	(0.012)	(0.018)	(0.019)	(0.019)	(0.018)	(0.019)	(0.019)
North Rhine-Westphalia	-0.000	-0.002	0.007	0.007	0.002	-0.003	0.019	0.020	0.004
	(0.008)	(0.008)	(0.015)	(0.024)	(0.027)	(0.026)	(0.025)	(0.026)	(0.026)
Baden-Württenberg	-0.001	0.003	0.010	0.035	0.027	0.020	-0.028	-0.006	0.016
	(0.007)	(0.007)	(0.014)	(0.025)	(0.026)	(0.026)	(0.025)	(0.026)	(0.025)
Berlin	0.011*	0.014**	0.015	0.005	0.016	0.016	0.005	0.007	-0.006
	(0.006)	(0.007)	(0.012)	(0.018)	(0.020)	(0.020)	(0.019)	(0.021)	(0.020)
Bavaria	0.001	-0.004	0.002	-0.017	-0.025	-0.017	-0.021	-0.028	-0.028
	(0.007)	(0.007)	(0.015)	(0.024)	(0.026)	(0.026)	(0.026)	(0.028)	(0.027)
Rest of Germany	0.001	0.006	0.012	0.001	0.012	0.014	-0.009	-0.004	0.004
	(0.004)	(0.005)	(0.009)	(0.014)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Belgium	-0.032***	-0.037***	-0.083***	-0.115***	-0.153***	-0.165***	-0.124***	-0.151***	-0.160***
	(0.004)	(0.004)	(0.008)	(0.012)	(0.014)	(0.015)	(0.013)	(0.014)	(0.015)
Constant	0.123***	0.151***	0.353***	0.435***	0.590***	0.694***	0.482***	0.615***	0.719***
	(0.001)	(0.001)	(0.003)	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)
Observations	91,965	91,965	91,965	94,759	94,759	94,759	96,695	96,695	96,695

Notes: the table shows results from LPM estimations following the structure in Equation (1) with dummies for the regions of origin of the individuals' ancestors within Spain, Italy, France and Germany within periods of 2 years (See Table 2.9 for information about the geographical entities grouped under each region). Observations for these countries were limited to those that were assigned a region within the respective country, otherwise they were excluded from the analysis. Estimates for Portugal, Israel, Switzerland, Angola, India and a category grouping the remaining 117 countries in our data are omitted from the table. All specifications contain controls for tax amount and property area. The benchmark category is southern Spain. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.17: Property Tax Payments by Region of Descent for Periods of 2 Years (2011 - 2016)

	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
		2011 - 2012			2013 - 2014			2015 - 2016	
Payments:	On time	30 days	360 days	On time	30 days	360 days	On time	30 days	360 days
Northern Spain	0.003	0.005	0.007	0.005	0.006	0.009*	0.005	0.008	0.010**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)
Northern Italy	0.019***	0.022***	0.020***	0.016***	0.017***	0.019***	0.019***	0.023***	0.025***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Central Italy	0.018***	0.023***	0.024***	0.021***	0.024***	0.024***	0.015**	0.020***	0.019***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Southern Italy	0.019***	0.023***	0.024***	0.020***	0.023***	0.019***	0.017***	0.021***	0.023***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
France Paris basin	0.019	0.020	0.030**	0.015	0.019	0.032**	0.011	0.016	0.025*
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Eastern France	0.018	0.028**	0.030**	0.000	0.005	0.007	-0.008	0.000	0.010
	(0.013)	(0.014)	(0.013)	(0.013)	(0.014)	(0.013)	(0.013)	(0.014)	(0.014)
Northern France	0.031**	0.039***	0.035**	0.029**	0.028*	0.029**	0.025*	0.026*	0.019
	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)	(0.014)	(0.014)	(0.015)	(0.015)
Rest of France	-0.013	-0.005	0.011	0.011	0.015	0.010	0.019	0.018	0.014
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.018)	(0.019)	(0.019)
North Rhine-Westphalia	0.015	0.015	0.027	0.010	0.011	0.027	-0.032	-0.031	-0.013
	(0.025)	(0.026)	(0.025)	(0.025)	(0.026)	(0.025)	(0.024)	(0.026)	(0.026)
Baden-Württenberg	-0.006	-0.008	0.014	0.011	0.027	0.034	0.019	0.039	0.047*
	(0.025)	(0.026)	(0.025)	(0.024)	(0.025)	(0.024)	(0.024)	(0.025)	(0.024)
Berlin	0.013	0.008	0.002	0.009	0.012	0.018	0.025	0.023	0.031
	(0.019)	(0.020)	(0.020)	(0.019)	(0.020)	(0.019)	(0.019)	(0.020)	(0.019)
Bavaria	-0.017	-0.021	-0.023	-0.011	-0.025	-0.036	-0.047*	-0.060**	-0.048*
	(0.026)	(0.027)	(0.027)	(0.026)	(0.027)	(0.027)	(0.026)	(0.027)	(0.027)
Rest of Germany	0.016	0.016	0.019	0.031**	0.032**	0.033**	0.042***	0.038***	0.042***
	(0.014)	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
Belgium	-0.132***	-0.152***	-0.166***	-0.131***	-0.150***	-0.161***	-0.107***	-0.124***	-0.122***
	(0.013)	(0.014)	(0.015)	(0.013)	(0.014)	(0.015)	(0.013)	(0.014)	(0.014)
Constant	0.529***	0.618***	0.713***	0.534***	0.616***	0.701***	0.477***	0.558***	0.632***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Observations	100,991	100,991	100,991	104,296	104,296	104,296	112,142	112,142	112,142

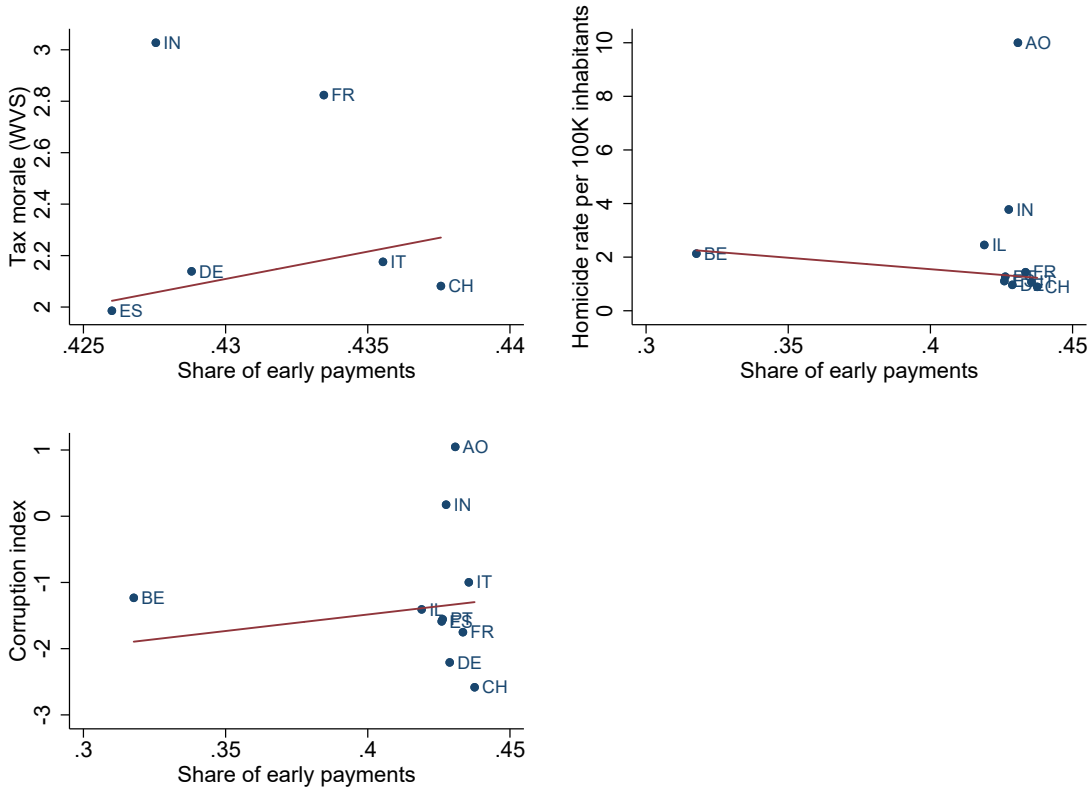
Notes: the table shows results from LPM estimations following the structure in Equation (1) with dummies for the regions of origin of the individuals' ancestors within Spain, Italy, France and Germany within periods of 2 years (See Table 2.9 for information about the geographical entities grouped under each region). Observations for these countries were limited to those that were assigned a region within the respective country, otherwise they were excluded from the analysis. Estimates for Portugal, Israel, Switzerland, Angola, India and a category grouping the remaining 117 countries in our data are omitted from the table. All specifications contain controls for tax amount and property area. The benchmark category is southern Spain. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.18: Property Tax Payments by Region of Descent for Periods of 3 Years

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	2005 - 2007				2008 - 2010			
Share of payments:	Early	Late	Never	Late (heckman)	Early	Late	Never	Late (heckman)
Northern Italy	0.006***	-0.013***	-0.012***	-0.011***	0.008**	-0.014***	-0.010***	-0.010*
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.005)
Central Italy	0.006**	-0.014***	-0.014***	-0.011***	0.013**	-0.018***	-0.019***	-0.008
	(0.003)	(0.003)	(0.005)	(0.003)	(0.006)	(0.006)	(0.005)	(0.009)
Southern Italy	0.009***	-0.010***	-0.012***	-0.006**	0.013***	-0.017***	-0.011***	-0.013*
	(0.002)	(0.003)	(0.004)	(0.003)	(0.005)	(0.005)	(0.004)	(0.007)
Belgium	-0.057***	0.056***	0.157***	0.021***	-0.117***	0.114***	0.150***	0.040**
	(0.006)	(0.007)	(0.014)	(0.008)	(0.011)	(0.012)	(0.013)	(0.019)
Constant	0.211***	0.752***	0.160***	0.676***	0.442***	0.496***	0.152***	0.296***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.012)
Selection (p-value)				0.000				0.000
Observations	93,493	93,493	93,493	93,493	98,548	98,548	98,548	98,548
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	2011 - 2013				2014 - 2016			
Share of payments:	Early	Late	Never	Late (heckman)	Early	Late	Never	Late (heckman)
Northern Italy	0.014***	-0.017***	-0.012***	-0.009	0.012***	-0.008***	-0.008***	-0.007
	(0.004)	(0.003)	(0.003)	(0.006)	(0.003)	(0.003)	(0.003)	(0.007)
Central Italy	0.017***	-0.015**	-0.014**	-0.008	0.009*	-0.015***	-0.014***	-0.006
	(0.006)	(0.006)	(0.006)	(0.010)	(0.005)	(0.004)	(0.004)	(0.012)
Southern Italy	0.017***	-0.019***	-0.015***	-0.009	0.012***	-0.006	-0.007**	-0.002
	(0.005)	(0.005)	(0.005)	(0.008)	(0.004)	(0.004)	(0.004)	(0.010)
Belgium	-0.130***	0.142***	0.169***	0.047**	-0.085***	0.107***	0.123***	0.025
	(0.013)	(0.013)	(0.014)	(0.022)	(0.010)	(0.010)	(0.011)	(0.027)
Constant	0.526***	0.452***	0.202***	0.158***	0.376***	0.344***	0.202***	-0.139***
	(0.002)	(0.002)	(0.002)	(0.013)	(0.003)	(0.002)	(0.002)	(0.042)
Selection (p-value)				0.000				0.000
Observations	102,825	102,825	102,825	102,825	112,417	112,417	112,417	112,417

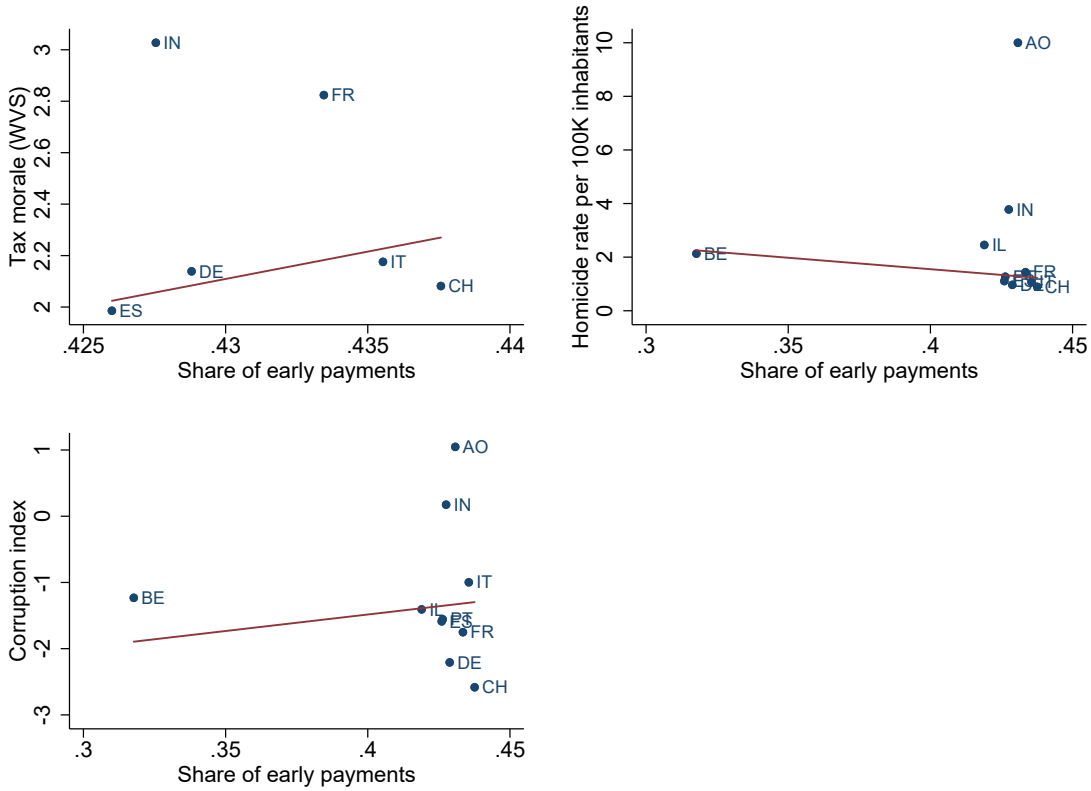
Notes: The table summarizes results from OLS estimations with the share of monthly payments within 36 months made early, late or never made on the left hand side and dummies for the regions of origin of the individuals' ancestors within Spain, Italy, France and Germany within periods of 3 years (See Table 2.9 for information about the geographical entities grouped under each region). Observations for these countries were limited to those that were assigned a region within the respective country, otherwise they were excluded from the analysis. The benchmark category is northern Spain, estimates for southern Spain, regions within France, regions within Germany, Portugal, Israel, Switzerland, Angola, India and a category grouping the remaining 117 countries in our data are omitted from the table. Columns 4, 8, 12 and 16 present outcomes from Heckman two-stage estimations of the share of late payments within 36 months as dependent variable. Heckman estimations use constructed area, tax amount and property value as independent variables in the selection equation. For these same columns, the table also shows the p-value of the null hypothesis of no selection bias. All specifications include controls for tax amount and property area. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Figure 2.2: Property Tax Compliance Correlations - Late Payments



Notes: The top-left panel uses tax morale indicators from the World Values Survey averaged over the period 2000-2014. The top-right panel uses homicide rates per 100.000 inhabitants from the United Nations office for drugs and crime over the period 2000-2012. The lower-left panel uses the corruption index from Fisman and Miguel (2007). The variable on the x axis in all panels is the share of late payments made during the whole period of analysis.

Figure 2.3: Property Tax Compliance Correlations - Non-payments



Notes: The top-left panel uses tax morale indicators from the World Values Survey averaged over the period 2000-2014. The top-right panel uses homicide rates per 100.000 inhabitants from the United Nations office for drugs and crime over the period 2000-2012. The lower-left panel uses the corruption index from Fisman and Miguel (2007). The variable on the x axis in all panels is the share of non-payments during the whole period of analysis.

Chapter 3

Salience, Simplification and Timely Compliance: Experimental Evidence on the Enforcement of Speeding Tickets

3.1 Introduction

In many domains, authorities rely on sending notifications to enforce compliance with outstanding payments. Typically lawyers are involved in drafting these notifications or, at least, there are laws that impose constraints on their format and content. As a result, payment notifications often appear like complex, convoluted legal texts. The key information is often presented in a way that is hard to understand, making it difficult to correctly infer important attributes of the payment liabilities. This is crucial, as many of these attributes play – at least for a fully rational, attentive and cognitively unconstrained decision maker – a significant role in shaping individual responses to the notifications.

This project studies the impact from simplifying information and increasing the salience of different attributes hidden in such complex legal texts. In particular, we focus on payment notifications for drivers who receive a speeding ticket.[1] We first

[1]To guide our analysis and ensure its transparency, we registered a pre-analysis plan before the

explore the payment choice in a simple theoretical framework in the spirit of Heffetz *et al.* (2016) and Altmann *et al.* (2017). Individuals receive a payment notification and decide if and when to pay. In each period, they compare the (stochastic) opportunity costs of paying now with the option value of postponing the payment into the future. If a given payment deadline is missed, an additional penalty applies and thus increases the costs. A fully informed agent anticipates this effect occurring at the deadline. However, the payment notification might appear highly complex. Hence, both the payment deadline itself and the increased costs for missing the deadline may not be fully salient to all individuals. Based on our model, we then discuss how a simplified payment notification – that makes the deadline and the extra penalty more salient – can increase timely payments.

We then test our predictions empirically, analyzing the effect of simplification and increased salience on the timely payment of speeding tickets. More specifically, we run a field experiment with the universe of drivers who received speeding tickets in a suburb of the city of Prague, in the Czech Republic. With the cooperation of the local authorities, we randomized the inclusion and content of a complementary cover letter sent along with the regular payment notification to drivers that were caught speeding. This regular payment notification consists of a heavily convoluted legal text. Several key parameters – in particular, the payment deadline (15 days after receiving the ticket) and the additional fine for late payments – are somewhat hidden in the complex, legal text.

Against this benchmark, we test the effects from adding one-page cover letters, that simplify the information provided in the legal text and highlight the main choice-relevant attributes of the notification. The randomized control trial follows an incomplete 2x2 factorial design: a control group receives only the standard legal text of the summons. Three different treatment groups get the same standard text plus a simplifying cover letter. The cover letters make (1) the payment deadline, (2) the additional penalty, or (3) both attributes very salient.

To get a sense of the extent and direction of the mis-perception of these attributes in the population, we conduct an online survey among a relevant sample of drivers. Respondents are confronted with a hypothetical scenario mimicking the setup of the

start of the trial (Dusek *et al.*, 2017).

field experiment. We then collect their perceptions regarding different attributes of the setup, including the deadline and the late-pay penalty. This exercise also allows us to identify the extent to which the cover letters correct the mis-perceptions of these attributes.

Our results from the field experiment document a positive and persistent effect from jointly increasing the salience of the deadline and the penalty for late payments. The cover letter making salient both attributes increases the rate of timely compliance and the proportion of payments made over time with respect to the control group. Making salient the late-pay penalty only, also increases payments before the deadline, although to a lesser extent. The effects of this letter do not persist over time. In contrast, increasing the salience of the deadline alone yields no significant effects on payments.

The results are consistent with individuals mis-perceiving the penalty for missing the deadline and, to a much lesser extent, the deadline itself. Consistent with our theoretical framework and the results from the survey, the cover letters effectively correct both under- and overestimation of the deadline and the late-pay penalty. This triggers two different effects that move in opposite directions. Correcting deadline underestimation reduces payments before the deadline, whereas reducing overestimation has the opposite effect on timely payments. These effects are of similar size, and thus cancel each other. Similarly, Correcting for under- and overestimation of the penalty for missing the deadline leads to an increase and reduction of payments before the deadline respectively. In this case, however, the positive effect on timely compliance dominates. This links our findings to a growing body of literature that shows positive effects from making the costs of non-compliance more salient (see, e.g., Cranor *et al.*, 2018; Dur and Vollaard, 2013; Dwenger *et al.*, 2016; Fellner *et al.*, 2013).

Our setting further allows us to study the effect of 'traditional' economic incentives on payment behavior. For this we take advantage of a discontinuous increase in the amount of the fine after a certain driving speed threshold. This local variation allows us to implement a regression discontinuity design to identify the effect of increasing the fine and the costs for missing the deadline on timely compliance. The answer is not trivial from the perspective of our theoretical model. On the

one hand increasing the fine reduces payments before the deadline. On the other hand increasing the late-pay penalty has a positive impact on timely payments. The empirical results are consistent with a much stronger effect of the fine increase. Our findings indicate that increasing the fine and the penalty for paying late, reduces timely payments.

A quick comparison of the absolute value of the effects on timely payments from both including a cover letter and changing the economic incentives, reveals that the latter have a much larger impact than the former. Doubling the fine and the penalty for missing the deadline causes an effect on payments that is almost three times larger than the effect produced by the most comprehensive cover letter. This is in line with similar findings in other domains, where economic incentives cause a larger impact than behaviorally motivated interventions (e.g., Chetty *et al.*, 2014; Ito *et al.*, 2018).

The present study also contributes to the understanding of attribute salience on the decision of individuals. A number of studies indicate that the degree of salience of relevant attributes in the decision process, has significant effects on the choices of individuals (Bordalo *et al.*, 2013; Chetty *et al.*, 2009; Meiselman, 2018; Taubinsky and Rees-Jones, 2016). Most notably, our research adds to a nascent literature that studies the effect of information simplification on behavior. Even though there are a number of studies analyzing simplification in different domains like savings decisions (Beshears *et al.*, 2013), college enrollment(Bettinger *et al.*, 2009; Dynarski and Scott-Clayton, 2008), micro-finances (Drexler *et al.*, 2014), information simplification has received little attention – in particular, in the domain of legal payment notifications. The only exception is De Neve *et al.* (2019) who analyze the effect of information simplification, combined with deterrence and tax morale texts on tax compliance in Belgium[2]. Our study differs from this paper mainly in the focus of our analysis and the setup of our experiment. While we analyze the *salience* of the attributes by providing a cover letter that is complementary to a text available to all individuals, their paper rather focus on the effect of the content of different texts related to

[2]A related study from a different area is Persson (2018), who theoretically analyzes how information simplification affects cognitively biased individuals. The author models the effect of information overload on decision. She shows that providing an excessive amount of information negatively affect the choices of inattentive individuals.

deterrence and tax morale on payments.

From an applied policy perspective, our findings show that information simplification and increasing attribute salience is a cost-effective enforcement policy. It reduces the number of cases that move to further enforcement steps (bringing along additional administrative costs, processing time, etc.), thus saving the enforcement authority a significant amount of resources. Moreover, it has a direct positive effect on revenue, as it increases the proportion of payments altogether.

The remainder of this paper is organized as follows: Section 3.2 describes the institutional context of this study. Section 3.3 discusses a theoretical framework to model payment decisions. The following Section 3.4 details our experimental designs. We first present the treatments, predictions as well as the results from a survey experiment that explores treatment effects on (mis)perceptions. Thereafter we turn to the results from the RCT. In Section 3.5, we then explore a regression discontinuity design. After discussing the design and its validity, we present the main empirical results. The final section 3.6 concludes.

3.2 Institutional background

We study the payment of speeding fines issued by the local authorities in Ricany, a town in the south west of Prague in the Czech Republic. The authorities manage more than 30 radar systems in the town and in nearby villages which all face heavy commuting traffic to and from Prague.[3] The radars are installed along different roads with a speed limit of typically 50km/h.[4] Each radar measures the speed of every passing vehicle, takes a digital picture and identifies the number plates of the car. An enforcement authority then automatically processes the data of cars that were speeding above a given cutoff (excluding emergency vehicles like ambulances or police cars).[5]

Similar to other contexts of traffic law enforcement (see, e.g., Goncalves and Mello, 2017; Traxler *et al.*, 2018), penalties for speeding increase stepwise in the

[3] On an average day, the radars used in our analysis, register more than 25,000 drive troughs.
[4] Among 31 radars, 3 are installed at roads with a speed limit of 40km/h.
[5] Further details on the institutional setting are discussed in Dusek and Traxler (2019).

speed. For a speed above a certain enforcement cutoff but less than 20km/h above the speed limit, car owners are charged with a low fine of 900 CZK (approx. EUR 35 or 3.5% of the average monthly wage). A speed between 20 and 40km/h above the limit results in an intermediate fine of 1,900 CZK (approx. EUR 75). Speeding with more than 40km/h above the limit triggers higher penalties and a different enforcement procedure. In addition, such speeding offenses are very rare (we only observe one during our trial period). The institutional discussion below as well as our analysis in the subsequent sections therefore focuses on speeding tickets with low or medium fines.

Authorities send a summons ('speeding tickets') to the registered address of the vehicle's owner either by regular mail or by e-mail.[6] The speeding tickets communicate the relevant fine – 900 or 1,900 CZK – and a payment deadline of 15 days after receiving the summons. If the stipulated fine is paid before the deadline, the case is closed.

If the fine is not paid by the deadline, authorities are expected to initiate a trial-like process. In case the car owner is convicted, he faces an additional fine bringing the total cost to 1,500 – 2,500 CZK for low severity cases (speeding up to 20km/h above the limit) and between 2,500 – 5,000 CZK for medium severity offenses. In addition, a deduction of demerit points is applied. Accounting for the relatively high enforcement costs, however, the authorities accept payments of the basic fines even if they are delayed by one or more weeks. Moreover, roughly 30–60 days after sending the summons, authorities either send a further mailing (or e-mail) or make phone calls, which serves as reminder and warning. Only thereafter a formal process would start.

A key feature of the institutional set-up – which is quite commonly observed in other enforcement contexts – is the highly convoluted nature of the summons: it is full of legal terms and relatively lengthy (see Appendix 3.9). The speeding tickets contain, in addition to numerous legal extracts, information about the time and date of the traffic violation, the amount of the fine, the payment deadline as well as information regarding the consequences of missing the deadline. One can

[6]The electronic mail is sent through an official e-governance platform called 'databox'. Mostly companies make use of databox, although some individuals also choose to use this service.

question, whether the text conveys the relevant information regarding the fines, the deadline for paying it, and the consequences of missing the deadline. The latter are summarized in the vague phrase "*... the office will continue investigating the offense*". It requires knowledge of administrative law to understand that the speeding violation will be processed under a different legal procedure with several possible outcomes, among which a payment of a higher fine is the most likely. Despite being explicitly stated thrice, the salience of the payment deadline might be compromised, too, by the plethora of legal details in the text. The next section discusses what the limited salience of these attributes implies for individuals' choices.

3.3 Theoretical framework

We consider a simple model of an individual's decision about if and when to pay a speeding ticket. The driver receives a speeding ticket in period $t = 0$. The ticket stipulates a fine f, a payment deadline, T, and an (expected) penalty for late payments, K. [7] Paying the fine is associated with some transaction and opportunity costs c_t. After the realization of these costs, which are i.i.d. and drawn from a given distribution F, the driver decide whether to pay now, in period t, or whether to wait another period and hope for a lower cost realization in the future. The incentive to wait and postpone the payment, however, is constrained by the deadline T. If the deadline passes without having paid the fine, the individual's (expected) payment obligation increases. The costs for paying the speeding ticket in period t are then given by $C(t)$, with

$$C(t) = \begin{cases} c_t + f & \text{if } t \leq T \\ c_t + f + K & \text{if } t > T \end{cases}$$

The structure of this problem in fact describes many situations with (legal) payment notifications that communicate a given payment deadline and consequences for non-compliance. As discussed in Altmann *et al.* (2017), the structure also captures a much broader set of economically relevant choices of costly task completion under a

[7] Assuming risk neutrality, one could consider the case where a driver expects to get away with just paying the 'regular' fine f after the deadline with probability $1 - p$. With probability p, she would expect to pay f plus an additional penalty, κ. The expected late pay penalty would then equal $K = p\kappa$.

deadline (e.g., submitting work duties before a due date, buying a product before a price increase). Several recent studies analyze this dynamic problem (e.g., Altmann *et al.*, 2017; Heffetz *et al.*, 2016; Taubinsky, 2014) following the logic of traditional job-search models (e.g., McCall, 1970; Mortensen, 1970; Stigler, 1961). Below we will augment the framework from these studies by introducing non-salience (or misperceptions) regarding the deadline as well as the late pay penalty.

Benchmark Predictions

For the benchmark case of a full informed, rational individual, the optional solution to the problem is characterized by a cutoff rule (for analytical details see, e.g., Altmann *et al.*, 2017). For any given period $t \leq T$, there exists a cutoff $\hat{c}_t$ such that the fine is paid whenever the cost c_t is below the cutoff. When the realized costs are higher, the agent prefers not to pay but to wait for the next period $t+1$. Once the deadline gets closer, however, the option value of further postponing the payment shrinks: there are fewer periods left that could yield a low cost draw before the payment obligation increases at the deadline. The cutoff $\hat{c}_t$ is thus increasing in t for $t \leq T$. In turn, this means that the probability of paying the fine in period t (conditional on not having paid before) increases up to the deadline.

Economic Incentives

A comparative static that is worth discussing concerns the fine f. Consider the decision to pay a ticket at the last day (i.e., right before the deadline). Would a rational agent be more likely to pay the fine for a minor offense or the higher fine for an intermediate speeding offense (see Section 3.2)? Intuitively, one would expect to observe less compliance (i.e., timely payment) with higher fines. Analytically, however, the answer is complex. Among others, this is due to the fact that in our context the late-pay penalty K increases with f (i.e., $\partial K/\partial f > 0$; see the previous Section). This makes it, *cet. par.*, more costly to 'miss the deadline'. In addition, higher costs also reduce the option value to further postpone the payment into the future. Hence, in contrast to the higher costs (of a higher fine) which make a payment less likely in the (pre-deadline) presence, there are also forces that render a delayed

payment less attractive. The comparative static is therefore theoretically ambiguous and remains an empirical question (see Appendix 3.7 for a formal derivation of the comparative static).

Mis-perceptions

Accounting for the complex legal text of the speeding ticket, we next consider the case where both, (i) the deadline T and/or (ii) the costs of missing it, K, might not be fully salient to the driver. Following Taubinsky and Rees-Jones (2016), we model possible mis-perceptions by an individual that considers the deadline $\tilde{T}(\theta_T) = \theta_T T$ and a penalty for late payments, $\tilde{K}(\theta_K) = \theta_K K$. The two θ parameters – which are assumed to satisfy $0 < \theta < \infty$ – capture whether an individual over- ($\theta > 1$) or underestimates ($\theta < 1$) the deadline and the late pay penalty, respectively. The benchmark case from above is nested by $\theta_K = \theta_T = 1$.

It is straightforward to show that an individual's likelihood of paying the fine before the deadline T, is cet. par. increasing in θ_K: the higher the perceived costs of missing the deadline, the more likely the individual will pay in any pre-deadline period t. Individuals that over- [under-] estimate K, are thus more [less] likely to comply with the payment deadline. In a similar vein, one can show that the likelihood of observing a pre-deadline T payment is cet. par. *lower* for individuals with $\theta_T > 1$. Intuitively, an individual who overestimates the deadline length ($\tilde{T} > T$) expects to have more opportunities to draw low costs c_t before the late-pay penalty kicks in. In any period $t \leq T$ she will consider a higher option value from postponing the payment (implying a lower cutoff, $\hat{c}_t^{\theta_T>1} < \hat{c}_t^{\theta_T=1}$). As a consequence, it is less likely that she will have paid by the (true) deadline.

The case for someone who underestimates the deadline ($\tilde{T} < T$) is less clear. On the one hand, this person will feel 'more pressured' during the first $\tilde{T}$ periods after $t = 0$ (anticipating a lower option value from postponing and thus accepting higher cost realizations, $\hat{c}_t^{\theta_T<1} > \hat{c}_t^{\theta_T=1}$).[8] Hence, as compared to an individual who

[8]In fact, one can show that for any period $t \leq \tilde{T}$ the agent considers a cutoff $\hat{c}_t^{\theta_T<1}$ which equals to the cutoffs $\hat{c}_\tau^{\theta_T=1}$ of a fully rational agent (without mis-perceptions), for any $\tau - T = t - \tilde{T}$ (Altmann *et al.*, 2017, compare Appendix A.3 in). To put it more technically: during the first $\tilde{T}$ periods, the mis-perceiving agent acts as a fully rational agent during the last $\tilde{T}$ periods before the deadline T.

anticipates the correct deadline, he will be more likely to have paid by period $\tilde{T}$.

On the other hand, for periods $\tilde{T} < t \leq T$, an individual with correct deadline perceptions might be more likely to pay (conditional on not having paid before). This is due to the fact that (a) the proximity to the (correct) deadline drives up c_t (see above); and (b) that the individual with mis-perceptions now acts as if she would face the late-pay penalty (which tends to lower her propensity to pay relative to the agent with $\theta_T = 1$). The bottomline is that during this period there might be a catch-up, such that the overall effect of underestimating the deadline on the likelihood of observing a pre-deadline payment is ambiguous.

3.4 The Field Experiment

Our experiment varied the inclusion and content of a complementary cover letter that was sent, along with the standard summons, to drivers that were speeding (above the enforcement speed limit). These brief, one-page cover letters simplified the main information from the speeding ticket and made salient two particular elements that are critical for the payment decision: the payment deadline (T) and/or the consequences for missing it (K). In cooperation with the enforcement authority, we randomly assigned speeding offenses into a control group or one of three treatment groups described below.

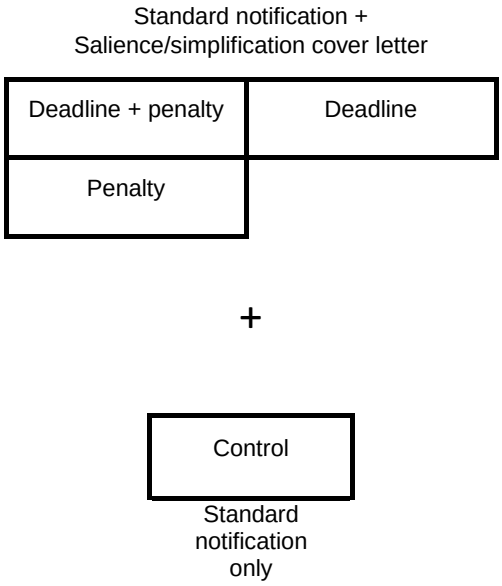
3.4.1 Treatments

The *control* (C) group only received the standard legal notification (i.e., the summons) without a cover letter. All three treatment groups received a cover letter on top of this standard notification. The cover letter of our *Deadline + Penalty* ($D\&P$) treatment clearly stated the deadline for paying the speeding ticket and the consequences of missing that deadline. The relevant excerpts of the cover letter read "Please pay the amount in full and make sure it is credited to the city's account **within 15 days** after receiving this summons." followed by "If you do not pay the whole amount the office will continue investigating the offense. The amount that

you will potentially have to pay **may be as high as 2,500 CZK**".[9]

To study which of these two elements has a stronger impact on the payment behavior, we implemented two additional treatments that exposed speeding ticket receivers to increased salience of either the payment deadline or the consequences of missing it (the experimental design is summarized in figure 3.1). The cover letter from our *Deadline* (D) treatment contained the same text from above stating the deadline, but not the text related to the penalties of not paying in time. Whereas the text on the letter from the *Penalty* (P) treatment only mentioned the consequences of late payments (without repeating the specific deadline). Otherwise, the wording of the texts in all the letters was identical. (Appendix 3.9 contains the full text of the cover letters for all the three treatments.)

Figure 3.1: Experimental design



[9]For medium-severity speeding violations (i.e., for a speed in the range of 21 - 40km/h above the limit), this part reads 'as high as 5,000 CZK' Bold font was also used in the actual cover letter in the same parts of the text as in these two text extracts

3.4.2 Predictions

Based on the discussion from Section 3.3 we can immediately derive several predictions. First, we expect the treatments that make the penalties more salient (the P and the $D\&P$ treatment) to increase the salience of K. Relative to the control group, both treatments should reduce mis-perceptions, thus shifting $\tilde{K}$ towards the true K. (In terms of the notation from Section 3.3: the treatments will push θ_K towards unity.) As discussed above, this should *increase* the rate of timely (pre-deadline) payments, for drivers that tend to underestimate K (i.e., whenever $\tilde{K} < K$). The opposite prediction emerges for drivers who overestimate K. Among the latter, the treatments would *decrease* the rate of timely payments (relative to the control group).

Second, the impact of making the deadline more salient (the D and the $D\&P$ treatment), again depends on the direction of the mis-perception. For drivers who overestimate the deadline length ($\tilde{T} > T$), the treatment should increase the rate of timely payments (i.e., payments within 15 days). As discussed in Section 3.3, the prediction for those who underestimate the deadline ($\tilde{T} < T$) is ambiguous. For this case we could, in principle, observe a lower rate of timely payments.

Third, there is scope for an interaction effect. The impact of the $D\&P$ treatment on payment rates might be larger than the sum of the effects from the D- and the P-treatment, whenever both treatments work into the same direction. If, for instance, drivers underestimate the late-pay penalty and overestimate the deadline (i.e., if $\tilde{K} < K$ and $\tilde{T} > T$) we could, in principle, observe a positive interaction.

3.4.3 Survey Experiment

The predictions make clear that the treatment impact hinges on the 'direction' of the individuals' mis-perceptions. To study this point empirically, we conducted a survey experiment that exposed a sample of $N = 1609$ individuals (aged 18 or above and holding a driving license) to our different treatments. It was conducted online with a professional survey provider that maintains a panel of respondents that is representative for the Czech population. Our survey participants were on average 43 years old, half of them were females and 18% had a university degree. All

observable characteristics are balanced across treatments, suggesting that random treatment assignment worked (see Table 3.7).

The participants were first exposed to a hypothetical scenario description which explained that they were detected speeding at 16km/h above the speed limit. In a second step, they were randomly assigned to either the standard notification (control treatment) or to one of the three cover letters from our RCT (followed by the standard notification).[10] Thereafter – and without the opportunity to click back into the text of the summons or the cover letter – individuals were asked questions about their perceptions regarding the deadline (T) and the penalty for missing it (K). The survey experiment thus provides a measure of the extent and direction of mis-perceptions (in the control group) as well as the treatments' effect on these mis-perceptions.

Perceptions about the deadline

Participants perceptions about the parameter T were assessed with a question asking "when do you think you have to pay the full amount of the fine?". The four response options were 'within 7', 15, 30, or 'within 60 or more' days after the deadline. Using binary response dummies as dependent variables (e.g., $T7_i$ indicating that individual i responded indicating 'within 7 days') we then estimate linear probability models of the form:

$$T7_i = \beta_0 + \beta_1 \text{Deadline}_i + \beta_2 \text{Penalty}_i + \beta_3 \text{Deadline\&Penalty}_i + X_i\gamma + \varepsilon_i \qquad (1)$$

The constant, β_0, thus indicate the fraction giving this response in the control treatment. The effect from exposure to one of the three treatments is captured by the estimates for β_1, β_2 and β_3. We will estimate such models with and without including a control vector X_i. Estimation results are presented in Table 3.1.

Comparing first the constant terms across Columns, one observes that in the baseline treatment 69.1%, a clear majority, provide the correct answer (see Column 3). The high share of correct responses suggest that, despite the convoluted legal

[10]The web display of the mailings used exactly the same layout as the letters used in the field experiment.

nature of the summons, the deadline (which is noted in three different parts of the text; see appendix 3.9) was sufficiently salient to our survey participants. Put differently, there is only a modest level of deadline mis-perceptions: 13.1% underestimate the deadline length (Column 1) whereas 16.6% (Column 5) plus 1.2% (Column 7) overestimate it. Hence, the scope for the treatments to reduce mis-perceptions is limited. Notwithstanding, the estimates document a consistent impact from the D and the $D\&P$ treatments.

Table 3.1: Treatment Effects on Deadline Perceptions

Responses:	(1) within 7 days	(2) within 7 days	(3) within 15 days	(4) within 15 days	(5) within 30 days	(6) within 30 days	(7) within 60+ days	(8) within 60+ days
Deadline	-0.086***	-0.088***	0.169***	0.171***	-0.078***	-0.077***	-0.005	-0.005
	(0.020)	(0.020)	(0.029)	(0.029)	(0.023)	(0.023)	(0.007)	(0.007)
Penalty	-0.028	-0.029	-0.016	-0.015	0.051*	0.052*	-0.007	-0.007
	(0.023)	(0.023)	(0.033)	(0.033)	(0.028)	(0.028)	(0.007)	(0.007)
Deadline & Penalty	-0.073***	-0.073***	0.148***	0.148***	-0.076***	-0.075***	-0.000	-0.000
	(0.020)	(0.020)	(0.029)	(0.029)	(0.023)	(0.023)	(0.008)	(0.008)
Constant	0.131***	0.163***	0.691***	0.620***	0.166***	0.194***	0.012**	0.023*
	(0.017)	(0.028)	(0.023)	(0.044)	(0.019)	(0.037)	(0.006)	(0.013)
F-Tests (p-values):								
$D = P$	0.002	0.001	0.000	0.000	0.000	0.000	0.656	0.732
$D = D\&P$	0.394	0.343	0.421	0.377	0.909	0.913	0.503	0.453
$P = D\&P$	0.020	0.021	0.000	0.000	0.000	0.000	0.270	0.270
Controls	-	Yes	-	Yes	-	Yes	-	Yes

Notes: The table presents LPM estimates of equations following the structure from (1). N = 1,609. Control variables include age, gender, and education dummies. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

The two treatments that make the deadline more salient increase the number of correct responses by about 15 to 17 percentage points. The P treatment, in contrast, has no effect (Column 3). The estimates further show that the increase in correct perceptions is achieved by a reduction in the fraction of respondents that underestimate the deadline length (Column 1) as well as a reduction in the share of individuals that expect a deadline length of 30 days (Column 5). All estimated treatment effects are virtually unchanged if one includes control variables. This pattern is reassuring and again consistent with successful randomization.

To wrap-up, the survey indicates that the D and $D\&P$ treatments successfully increase the salience of the correct deadline. The increase in correct perceptions, however, is associated with both, a (i) reduction in over- ($\theta_T > 1$) and (ii) a reduction in underestimations ($\theta_T < 1$) of the deadline length. The theoretical framework from Section 3.3 from above suggests that the former effect (i) would increase the rate of payments within 15 days. While the latter effect (ii) is ambiguous, it could in

principle run against the former. The survey evidence therefore suggests that the impact of the D and $D\&P$ treatments on timely payments might be small or even zero.

Perceptions about the late-pay penalty

Let us now turn to the perceptions regarding the late-pay penalty (K). Most respondents are well aware of the fact that not paying the ticket before the deadline leads to a penalty. In the baseline treatment, 81.2% expect an increase in the payment obligation (see Column 1, Table 3.2). LPM estimates further show that the two treatments that highlight the penalty (i.e., P and $D\&P$) further increase this share by 6 and 9 percentage points. The deadline treatment (D), in contrast, has no effect (Columns 1 and 2). Hence, this is a first piece of evidence indicating that the P and $D\&P$ treatment increase the expected penalty K (for the interpretation of K in expectation terms, see fn. 7).

Columns (3) – (8) explore different responses to the question '*'what would be the total amount you would eventually have to pay?'*. (Note that the question does not directly refer to the parameter K but rather asks about $f + K$. Our motivation for doing so is that the summons as well as our cover letters talk about the increased total payment obligation (i.e., the level) rather than the increase (the difference).)[11] The constant terms from Columns (3), (5) and (7) indicate a pronounced level of mis-perceptions in the amount of post-deadline payment obligations. As compared to the more modest deadline mis-perceptions discussed above, there is also a larger share of respondents that underestimates $f+K$: 53% expect a penalty of 2,000 CZK or less (Column 3). In addition, 36% anticipate a penalty of 3,000 CZK or more (Column 7).

The P and the $D\&P$ treatment strongly alter these mis-perceptions. The share of respondents expecting 2,500 CZK increases, from a baseline of 11%, by 42 and 47 percentage points, respectively (Column 5). The shift in perceptions is primarily due to a drop in underestimations (-30 to -34 percentage points, see Column 3).

[11] Note further that individuals, who did not expect an increase in the penalty (who responded 'no' to the question discussed above), were not asked about the expected post-deadline payment obligation. The estimates assigned these individuals to the ≤ 2000 response.

However, the P and $D\&P$ treatment also lowers overestimations (-13 percentage points, see Column 7). All these effects remain unchanged when we control for individual characteristics[12]

Table 3.2: Treatment Effects on Penalty Perceptions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Responses	Expect higher costs		≤ 2000 CZK		2500 CZK		≥ 3000 CZK	
Deadline	0.007	0.005	0.063*	0.058*	-0.039*	-0.037*	-0.025	-0.021
	(0.027)	(0.027)	(0.035)	(0.035)	(0.020)	(0.021)	(0.034)	(0.034)
Penalty	0.065**	0.064**	-0.295***	-0.298***	0.423***	0.423***	-0.127***	-0.125***
	(0.026)	(0.026)	(0.033)	(0.033)	(0.030)	(0.030)	(0.032)	(0.032)
Deadline & penalty	0.091***	0.090***	-0.339***	-0.342***	0.467***	0.467***	-0.127***	-0.125***
	(0.024)	(0.024)	(0.032)	(0.031)	(0.029)	(0.029)	(0.032)	(0.032)
Constant	0.812***	0.834***	0.530***	0.477***	0.111***	0.131***	0.359***	0.391***
	(0.019)	(0.033)	(0.025)	(0.045)	(0.016)	(0.036)	(0.024)	(0.044)
F-Tests (p-values):								
D = P	0.024	0.022	0.000	0.000	0.000	0.000	0.001	0.001
D = D&P	0.001	0.000	0.000	0.000	0.000	0.000	0.001	0.001
P = D&P	0.242	0.238	0.127	0.126	0.208	0.206	0.999	0.992
Controls	-	Yes	-	Yes	-	Yes	-	Yes

Notes: The table presents LPM estimates of equations following the structure from (1). N = 1,609. The dependent variable in Columns (1) – (2) is a dummy indicating that an individual responded *yes* to the question *'If you do not pay the full amount of the fine by the deadline, would you expect to pay a higher fine?'*. Columns (3) – (8) are based on subsequent question regarding the late-pay penalty. The dependent variable in Columns (3) and (4) captures responses indicating *1,500*, *2,000* CZK, as well as responses of individuals that answered *'no'* when asked if they would expect a higher fine if they did not pay by the deadline. Columns (5) and (6) indicate responses of *2500* CZK and Columns (7) and (8) pair response with *3,000*, *3,500*, *4,500* and *more than 4,500* CZK. Control variables include age, gender, and education dummies. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Similar effects are observed for the survey participants' expectations regarding demerit points (a further element of the late-pay penalty). In the baseline treatment, 32% expect to get demerit points for missing the deadline. In the P and $D\&P$ treatments, this rate increases by 30 percentage points (see Table 3.8 in the Appendix for the full estimates).

Together with the findings from above, this suggests that the treatments which make the late-pay penalty more salient, should trigger a deterrence effect. Firstly, and quite generally, the P and $D\&P$ treatment effects on expectations and perceptions appear much stronger than the comparable effects for the deadline treatments. As in the case of the deadline treatments, the penalty treatments trigger some opposing effects, too: they not only reduce the share of individuals underestimating the costs of missing the deadline, but also lower the fraction that overestimates it. Qualitatively, however, the former effect is much strong. Moreover, the treatments

[12]One further observation is hard to rationalize: the deadline treatment seems to shift misperceptions towards an underestimation of the payment obligations; however, these effects are relatively small and only weakly significant.

have unambiguous effects on several other dimensions of K (demerit points and the likelihood p, see fn. 7). The survey evidence therefore suggest that one might expect the treatments to increase the share of timely, pre-deadline payments.

Finally, the potential interaction effects of the $D\&P$ treatment – i.e., whether this intervention would yield a larger effect as the sum of the D and P treatment alone – are hard to assess based on the survey results. It is worth nothing, however, that none of the treatment effects reported in Tables 3.1 and 3.2 are significantly different between the un-interacted and the interacted treatments (see the F-tests in Tables 3.1 and 3.2).

3.4.4 RCT Implementation and Sample

Between November 2017 and April 2019 we randomly assigned $N = 50{,}524$ speeding offenses to one of our treatment branches (control, deadline, penalty or deadline & penalty). We excluded high severity violations and speeding offenses committed by excused public service vehicles (see Section 3.2). Due to a technical problem in the course of the trial, no observations were assigned to the deadline & penalty treatment for a period of three months (between August and November 2018). After the error was corrected, we re-balanced the proportion of subsequent speeding offenses assigned to each treatment, so that by the end of the trial the number of observations across treatments would be balanced. We account for this issue in all our estimations below by controlling for the week the tickets were sent.

For each speeding violation, we have a significant amount of information that allows us to track the whole history of the ticket's enforcement process. Our data include, among other information, the vehicle's speed at the time of the offense, the amount of the fine, the date and time of the speeding offense, the date the ticket is generated, sent, received and eventually paid by the driver among (see Table 3.3).

Notably, speeding infractions happen mostly around midday, the average driving speed captured by the radars is 65km/h (15km/h above the speed limit). After being sent, it takes about 5 days for an average ticket to be received by the driver. Almost half of the offenses are committed by vehicles belonging to a company, which means that the speeding ticket is sent to the company the car is registered to. The

rest are privately owned vehicles. 13% of the cars were caught speeding more than once, and were therefore treated multiple times. It is also important to note that the bulk of our observations come from low severity offenses. Only 7% (3,196) of violations are of medium severity. The table also reveals that randomization worked as intended. All the variables are balanced across groups with only a few exceptions for punctual cases.

Table 3.3: Sumamry Statistics Across Treatments

	Total	Control	Deadline	Penalty	Deadline & penalty	P-value
Average measured speed	64.61	64.58	64.57	64.65	64.66	0.601
	(5.203)	(5.344)	(5.379)	(5.275)	(4.674)	
Average fine amount (CZK)	836.3	834.9	835.5	835.5	840.0	0.037
	(230.4)	(234.2)	(231.6)	(231.9)	(221.6)	
Hour of the day	12.78	12.83	12.87	12.83	12.55	0.814
	(4.903)	(4.996)	(5.044)	(5.035)	(4.404)	
Days between sending and reception	5.045	5.069	5.098	5.022	4.974	0.763
	(6.159)	(6.490)	(5.887)	(5.846)	(6.422)	
% low severity offenses	0.937	0.933	0.935	0.934	0.948	0.363
	(0.243)	(0.250)	(0.247)	(0.249)	(0.222)	
% tickets to cars owned by corporations	0.456	0.448	0.448	0.460	0.470	0.077
	(0.498)	(0.497)	(0.497)	(0.498)	(0.499)	
% tickets sent through databox	0.395	0.390	0.386	0.397	0.410	0.293
	(0.489)	(0.488)	(0.487)	(0.489)	(0.492)	
% vehicles treated multiple times	0.133	0.137	0.136	0.134	0.123	0.380
	(0.339)	(0.344)	(0.343)	(0.341)	(0.328)	
Number of tickets	50,524	13,830	13,136	13,069	10,489	
Number of vehicles	40,993	12,788	12,188	12,123	9,739	

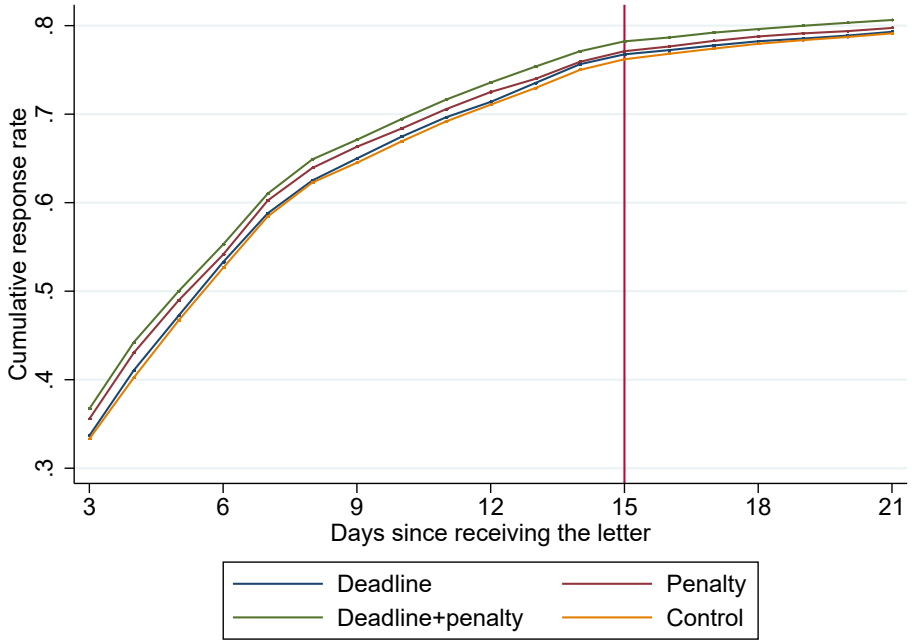
Notes: Standard errors are written in parenthesis. Last column of the table present p-values from F-tests for equality of the respective variable across the treatment groups

3.4.5 Results

We begin with a descriptive analysis. Figure 3.2 illustrates the cumulative payment rates across treatment conditions. The image is scaled to show the first 3 to 21 days after receiving the summons (Figure 3.4 in the appendix covers a longer time period). The figure shows that, as expected, some cover letters were more successful in inducing individuals to pay the tickets on time than others.

Treatment D has no effect on increasing pre-deadline payments as compared to the control group. Treatments P and $D\&P$, on the other hand, increase payments before the deadline with respect to the control group. The effect of treatment P is

Figure 3.2: Cumulative Response Rates by Treatment



Note: the payment deadline (15 days) is indicated with the red vertical line. The figure shows the cumulative response rate between the 3rd and 21st day after receiving the summons, for the control and each of the treatment groups.

stronger in the first days after the letter is received, and vanishes right before the deadline. The impact of the combined treatment $D\&P$ persists over time, even after the deadline has passed. These graphical results already confirm the expectations of low to null effects when increasing the salience of the deadline, and a positive impact on timely payments when the penalty for late payments is made salient.

To analyze these effects in detail, we (parametrically) estimate the impact of our treatments on the probability of payment within different periods of time. Table 3.4 presents results of linear probability model estimations of the form:

$$y_{i\tau} = \beta_0 + \beta_1 \text{Deadline}_i + \beta_2 \text{Penalty}_i + \beta_3 \text{Deadline\&penalty}_i + X_i\gamma + W\delta + e_i \quad (2)$$

where $y_{i\tau}$ is a dummy taking the value of one if a payment was made within a time interval of $\tau = \{15 \text{ days}, 30 \text{ days}, \text{any time}\}$ after receiving the speeding ticket[13]. β_1 to β_3 capture the effect of our treatments on the dependent variable, and X_i is a vector of controls and W is a set of dummies for the week the tickets were sent.

[13]In the pre-analysis plan, we also considered estimating responses within 60 and 100 days. See table 3.9 in the appendix for results over these time periods

Consistent with the graphical analysis, the deadline treatment D does not have a significant effect on payments before the deadline (or any other time intervals from the table). Results do not change when adding a set of controls in columns 2, 4 and 6. This null effect is consistent with the survey evidence suggesting that (i) most individuals have a correct perception of the deadline (see Section 3.4.3) and (ii) treatment D produces two effects of similar size on payments that go in opposite directions, therefore canceling each other. From Section 3.4.3 we have that treatment D effectively corrects deadline mis-perceptions by reducing both under- and overestimations of the deadline. Here we observe that these changes in perceptions affect payment decisions according to the predictions from Section 3.3. By reducing deadline overestimation ($\tilde{T} < T$), D increases payments before the deadline. At the same time, the treatment discourages payments in early periods after receiving the ticket. These effects must be of similar size, therefore, we do not observe a significant increase in timely compliance from this treatment.

Table 3.4: Probability of Payment

	(1)	(2)	(3)	(4)	(5)	(6)
Payments:	15 days		30 days		Any payment	
Deadline	0.006	0.004	0.002	-0.001	-0.002	-0.001
	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Penalty	0.010*	0.008*	0.006	0.004	0.002	0.001
	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Deadline & penalty	0.020***	0.020***	0.013**	0.013***	0.014***	0.010**
	(0.006)	(0.005)	(0.006)	(0.005)	(0.005)	(0.004)
Constant	0.751***	1.166***	0.771***	1.173***	0.909***	1.214***
	(0.023)	(0.036)	(0.022)	(0.034)	(0.015)	(0.030)
F-Tests (p-values):						
$D = P$	0.501	0.389	0.377	0.224	0.441	0.447
$D = D\&P$	0.026	0.004	0.045	0.004	0.003	0.009
$P = D\&P$	0.092	0.030	0.196	0.065	0.017	0.048
Controls	-	Yes	-	Yes	-	Yes
Observations	50,524	47,852	50,524	47,852	50,524	47,852

Notes: The table presents LPM estimates of equations following the structure from (2). All specifications contain dummies for the week the speeding ticket was sent. Specifications 2, 4 and 6 include a vector of variables controlling for severity of the offense, plate number of the vehicle, day of the week when the offense took place, holidays, number of days between the offense and both ticket sending and reception, hour of the day, measured speed at the time of the offense, amount of the fine, direction of the drive through (from or to Prague), whether the ticket was sent through regular mail, whether the ticket was issued to a vehicle that belongs to a company. Standard errors given in parentheses are clustered at the vehicle level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

In contrast, the two treatments that make the penalty salient (P and D&P)

cause a significant increase in timely compliance with respect to the control group (Column 1). Making salient the penalty for not paying before the deadline alone (P) increases timely payments by 1 percentage points, whereas the effect of the combined treatment D&P is 2 pp. Relative to the control group, this is a 1.3 and 2.6 percent increase. The survey evidence indicates that these effects are driven mostly by the correction in underestimation of the late-pay penalty (K). On average, individuals underestimate the penalty for missing the deadline to a considerable extent. P and D&P effectively increase its salience and reduces underestimation of this parameter, which triggers a deterrence effect that increases payments before the deadline (see Section 3.4.3). There is however, an effect working in the opposite direction, as the treatments also reduce overestimation of K, and consequently, payments made before the deadline. However, the extent of overestimation of the penalty seems to be small (see Section 3.4.3) and thus the positive effect dominates and produces an overall positive impact in payments before the deadline. It is reassuring to observe that all results in the table remain the same after the introduction of controls.[14]

It is interesting to see that the effect of the D&P treatment is significantly different from that of the other two treatments (see the post-estimation tests on the lower panel of the table). This suggests that correcting the mis-perception of T and K at the same time has a somewhat magnifying effect not only on timely payments, but also payments in general. Columns 3 to 6 in the table show that the effect of treatment D&P persists over time (see table 3.9 in the appendix for estimations of payments within longer periods of time), and remains at 1.4 pp (1.0 pp when introducing controls in column 6) when looking at the whole trial period .

In a next step we turn to the duration analysis of ticket payments. For this we estimate Cox proportional hazard models with the following structure:

$$h(t) = h_0(t)exp(\beta_0 + \beta_1 \text{Deadline} + \beta_2 \text{Penalty} + \beta_3 \text{Deadline\&penalty} + X\gamma + W\delta) \quad (3)$$

where $h(t)$ is the hazard rate for paying the ticket in t and $h_0(t)$ is the baseline hazard function. We estimate the equations with truncation at 15, 30 and 60 days after receiving the ticket. Table 3.5 presents the outcomes of the estimations. Results

[14]Due to missing values in the control variables, the number of observations is smaller in the specifications where controls are included in Table 3.4

are in line with the graphical and the estimation analysis from above. Individuals that received the letter making salient both the deadline and the penalty for late payments (D&P) exhibit a higher probability of making payments. On average this group has a hazard rate that is 6 to 7 percent higher (depending on the number of days after which the data is truncated) than the hazard rate in the control group. Estimates of the ratios for this treatment slightly increase when a set of controls are introduced. Hazard ratios from the penalty treatment P are smaller than those from D&P, and only become slightly significant with the introduction of controls[15]. There are no significant effects in the Deadline treatment with respect to the control group.

Table 3.5: Duration to Payment

	(3)	(4)	(5)	(6)	(7)	(8)
	15 days		30 days		60 days	
Deadline	0.990	0.992	0.995	0.994	1.004	0.998
	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)
Penalty	1.025	1.038*	1.026	1.039*	1.031	1.042*
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
Deadline & penalty	1.060**	1.064**	1.064**	1.066**	1.072***	1.073***
	(0.027)	(0.027)	(0.027)	(0.027)	(0.028)	(0.027)
Post-est. tests (p-values):						
D = P	0.141	0.049	0.190	0.054	0.254	0.066
D = D&P	0.008	0.007	0.010	0.006	0.010	0.004
P = D&P	0.195	0.331	0.165	0.295	0.131	0.229
Controls	-	Yes	-	Yes	-	Yes
Observations	46,971	44,299	46,971	44,299	46,971	44,299

Notes: The table presents hazard ratios from estimations of Cox proportional hazard models following the structure of (3). All specifications contain dummies for the week the speeding ticket was sent. Specifications 2, 4 and 6 include a vector of variables controlling for severity of the offense, plate number of the vehicle, day of the week when the offense took place, holidays, number of days between the offense and both ticket sending and reception, hour of the day, measured speed at the time of the offense, amount of the fine, direction of the drive through (from or to Prague), whether the ticket was sent through regular mail, whether the ticket was issued to a vehicle that belongs to a company. Standard errors given in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

In sum, increasing the salience of the deadline does not cause a significant increase in payments as compared to the control group due to the similar opposing effects that are at work when deadline mis-perceptions are corrected. On the other

[15]The number of observations is smaller in specifications including controls in Table 3.5 due to missing values in these variables. Also the smaller number of observations with respect to estimations in Table 3.4 is because the estimation procedure excludes observations with payments made the same day the ticket is received (i.e. zero duration).

hand, increasing the salience of the penalty for late payments significantly increases timely compliance. Interestingly, jointly making salient both attributes is particularly effective in inducing individuals to pay their tickets on time. It also encourages ticket payments in the long run. Evidence also suggests that simplification messages per se (i.e. cover letters) seem to have minor effects. Otherwise we would have observed at least some effects from the deadline treatment (D).

Cost-benefit analysis

Even though the effects of our treatments might seem small in magnitude, they have important revenue implications for the authorities in our setting. Assuming that the authorities implement our most successful treatment (D&P), a simple back of the envelope cost-benefit analysis reveals that simplifying information and making salient both the deadline and the penalty for missing it, is a cost-effective intervention. It saves the traffic authorities considerable amounts of money in enforcement procedures.

As mentioned in section 3.2, if a ticket is not paid by the deadline, authorities start additional enforcement procedures. For this, workers at the office of traffic have to review the follow-up process in each case. These procedures take about 15 minutes per speeding ticket.[16] Average hourly wage in the traffic department is about 200 CZK, thus the processing of each unpaid ticket amounts to 50 CZK. Hence, the increase in timely payments caused by the cover letter (0.02) translate into a reduction of 1 CZK in administrative costs. Beyond saving enforcement costs, the cover letter also increases the probability of making any payment by 0.014. With an average fine of 830 CZK, this amounts to an increase in revenue by 12 CZK per ticket. Putting these numbers together yields a marginal benefit of 13 CZK.

As for the marginal cost, it is zero for tickets sent via email, which corresponds to 40% of all tickets (see table 3.3). The cost of printing and sending the letter through regular mail is at most 2 CZK, meaning that the average marginal cost is about 1 CZK. Thus, the cover letter yields a net marginal benefit of 12 CZK for the

[16]Information about hourly wages, processing time for unpaid tickets, and additional information mentioned below was obtained through conversations with the traffic authorities of Ricany

authorities. Moreover, the radar system generates roughly 3,000 tickets per month, which translates into a monthly net benefit for the authorities of 36,000 CZK.

3.5 Regression Discontinuity Design

To study the effect of 'traditional' economic incentives on payment behavior of ticket receivers, we take advantage of the speed threshold at which speeding is classified as an intermediate rather than a minor offense. Recall from Section 3.2 that individuals face a low penalty (900 CZK) if they are caught driving at a speed of at most 20km/h above the speed limit (with a total cost of at most 2500 CZK when paying after the deadline). The amount of the fine jumps to 1900 CZK (with a total cost of at most 5000 CZK when paying late) if the speed slightly exceeds 20km/h above the limit. That is, at this speed threshold, the amount of the fine discontinuously increases by 111%.

This local variation allows us to implement a regression discontinuity design (RDD) to identify how an increase in the fine and the late-pay penalty affects payment behavior. Recall from Section 3.3 that our theoretical framework provides no clear prediction for this effect: in principle, the effect could go either way. On the one hand a higher fine f, would *cet. par.* reduce payments before the deadline. On the other hand, the associated increase in the costs for late payments reduces the incentives to postpone paying the ticket into the future and makes missing the deadline more costly, which in turn would increase timely payments.

3.5.1 Validity of Design

The running variable of our RDD is thus measured speed. For the identification strategy to produce unbiased estimates, we need to be sure that individuals cannot fully control the speed at which they drive to keep it just below the threshold. We argue that two important features in our setting ensure that this is the case.

First, the way speed is measured by the radars makes it difficult for drivers to aim for a specific speed level. A Radar system consist of a pair of measuring devices placed along a strip of road at about 500 meters away from each other. Each device

measures the time at which the vehicle passes in front of it. Speed is then calculated from the time difference between the two measurements. This makes aiming for a specific driving speed challenging. It would require drivers to keep a constant speed between the two devices and to have full knowledge about how the radar systems work, which is unlikely.

Second, even if drivers are fully informed of the functioning of the radars and skilled enough to aim for a particular speed just below that of the medium severity threshold throughout the whole strip of road, they are not aware of the speed level that is relevant for the allocation of the fines. In order to account for potential measurement errors, authorities apply a correction procedure to the measured speed of the vehicles in favor of the drivers before assigning them the corresponding fine. The correction consists on rounding and subtracting 3km/h to the measured speed of the cars. This means that the effective threshold at which offenses go from low to medium severity is actually 23km/h above the speed limit. Since this de facto correction procedure is hardly known, we consider sorting around the threshold very unlikely.

Figure 3.5 and 3.6 in the appendix provide graphical evidence of the validity of our approach. The figures show that the density of the running variable is smooth around the threshold. Additional balancing tests of observables at both sides of the threshold are provided in Table 3.10 in the appendix. Apart from some selected cases, the results confirm that observables are balanced around the threshold, which together with the outcomes of the graphical analysis, make clear that there is no systematic sorting around the threshold[17]. Therefore we can be confident that the identification strategy will allow us to obtain unbiased estimates.

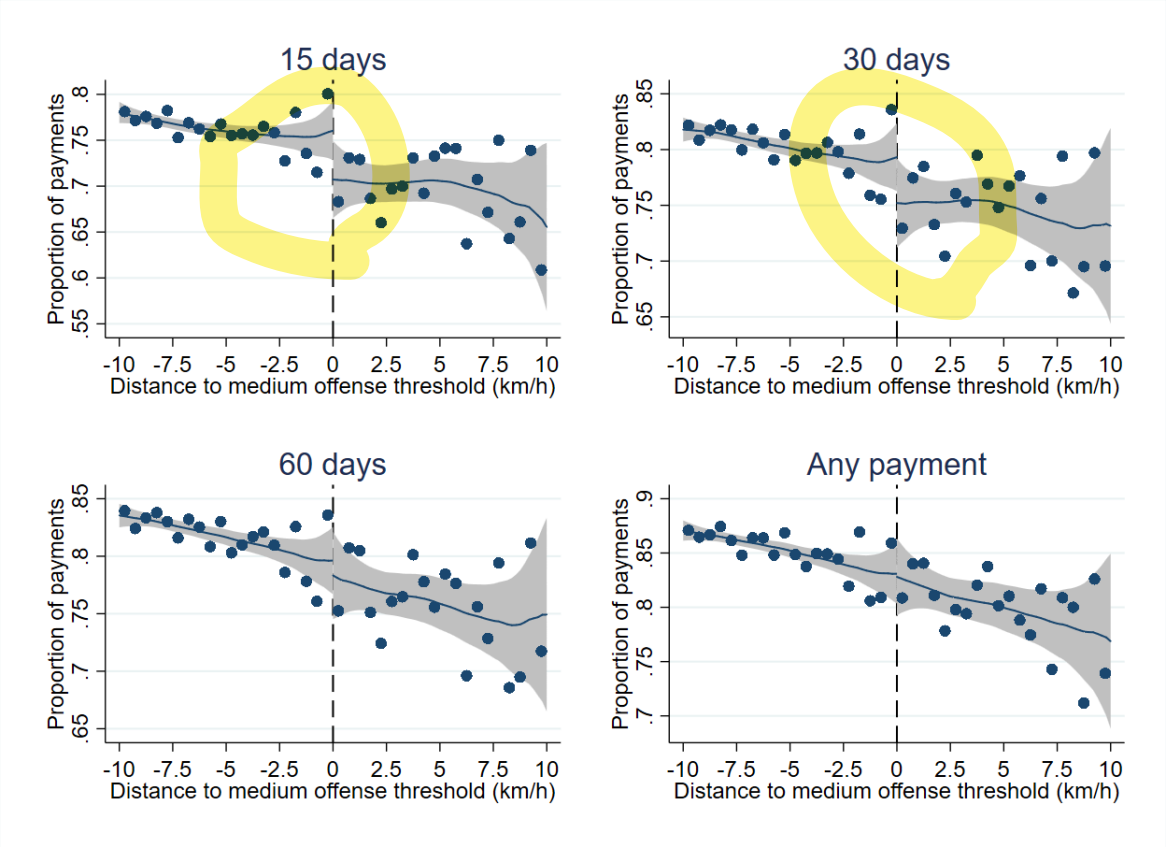
3.5.2 Results

We proceed first with a graphical analysis of how a cost increase affects the probability of payment. Figure 3.3 shows how increasing the amount of the fine and the late-pay penalty affects the probability of payment within different periods of time. Increasing the fine from 900 to 1900 (and correspondingly the cost of late payments

[17]See Dusek and Traxler (2019) for additional validity checks for an RDD approach around this threshold

from at most 2500 CZK to at most 5000 CZK) significantly reduces the probability of observing payments before the deadline of 15 days. We obtain a similar figure for payments within 30 days. The monetary cost change does not seem to affect neither the probability of paying within 60 days nor the probability of making any payment. These graphical results indicate that an increase in the cost of the fine (f) has a negative effect on timely compliance.

Figure 3.3: Probability of payment - full sample



Notes: The graphs show the average probability of payment within bins of width 0.5. Measured speed in the horizontal axis is centered around the 23km/h threshold above the speed limit. Local polynomials of degree one fitted on both sides of the threshold were estimated using triangle kernel and bandwidth calculated using MSE optimal bandwidth selection procedures by Calonico *et al.* (2014). Shaded areas represent 95% confidence intervals.

To analyze in more detail the extent of the effect of a monetary cost increase on payments, we implemented a regression discontinuity design around the speed threshold (23km/h) that triggers an increase in the fine and the late-pay penalty. For this we estimated non-parametrically the following equation:

$$y_{i\tau} = \alpha D_i + g_+(s_i)D_i + g_-(s_i)(1 - D_i) + \mu_i \tag{4}$$

were $y_{i\tau}$ is a dummy equal to one if ticket i was paid within a time period of $\tau = \{15 \text{ days}, 30 \text{ days}, 60 \text{ days}, \text{any time}\}$ after it was received by the driver. s_i is the measured speed at which the vehicle was driving at the time of the offense, D_i is a dummy that equals one if the vehicle's speed is above the cutoff of 23km/h and $g(s_i)$ is a function that captures how $y_{i\tau}$ varies with measured speed.

Table 3.6 summarizes the outcomes of the estimations. The table shows that in fact, increasing the fine and the associated late-pay costs discouraged timely payments and payments within 30 days after the speeding ticket was received. In the language of Section 3.3, jointly increasing f and K by about 100%, reduces the proportion of timely payments by 5.4 percentage points. This represents a 6.7% drop in timely compliance relative to the proportion of payments made before the deadline within 0.5km/h below the cutoff. This finding indicates that the reduction on payments before the deadline resulting from a larger f, dominate the potential positive effect on timely payments from increasing the costs of missing the deadline (K).

Table 3.6: RDD Estimates

	(1)	(2)	(3)	(4)
Payments:	15 days	30 days	60 days	Any payment
Bias-corrected	-0.054**	-0.042*	-0.012	-0.004
	(0.027)	(0.026)	(0.024)	(0.023)
Y (left)	0.801	0.836	0.836	0.859
Relative effect	-6.69%	-5.08%	-1.43%	-0.48%
Effective obs. (left)	4451	4368	4676	4559
Effective obs. (right)	1771	1757	1802	1784
Optimal Bandwidth	4.030	3.978	4.150	4.083

Notes: The table presents bias-corrected RD estimates of (4) with robust variance estimators (Calonico et al., 2014), implemented with the 2018/09 version of the rdrobust package (Calonico et al., 2017). All specifications use local linear point estimators and local quadratic estimates for the bias correction. Effective number of observations used to the left and the right of the 23km/h cutoff as well as MSE optimal bandwidths estimated with a triangular kernel are indicated in the lower panel of the table. Y (left) indicates the mean of the dependent variable in the 0.5km/h bin below the cutoff. The table also shows the effect size relative to the mean outcome in the 0.5km/h bin below the cutoff, Number of observations in all specifications is 50,524. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

The negative effect on payments from a larger f persists during the first month after the speeding ticket is received, although with a smaller magnitude. Doubling the fine and the late-pay costs reduces payments within this period by 4.2 pp (or

5% relative to payments right below the cutoff). We do not find significant effects when considering longer periods of time.

This set of results seem to be large when put into perspective with the outcomes from the empirical analysis in the last section. On the one hand, doubling the amount of the fine and the late-pay penalty reduced timely payments by 5.4 pp. On the other hand, the most effective cover letter in the previous section (D&P) increased payments before the deadline by 2 pp. This means that doubling the monetary costs of the fine and the late-pay penalty produces an effect on timely compliance that is almost three times larger, in absolute value, than the effect of providing a simplifying cover letter that makes salient the deadline and the late-pay penalty. These findings highlight the relative importance of purely economic incentives on payment decisions with respect to a behaviorally motivated intervention.

3.6 Conclusion

We analyzed the effect of information simplification, attribute salience and economic incentives on speeding ticket payments closely following a pre-analysis plan. Results from a survey and a randomized control trial highlight the importance of salience and simplification. Simplifying the information and jointly increasing the salience of the deadline and the penalty for late payments leads to higher timely compliance and overall speeding ticket payments than a low salience scenario. Separately making salient either attribute barely has an effect on payments. Making the deadline salient has no effect on payments in our setting, whereas increasing the salience of the late-pay penalty has a modest positive effect on timely compliance.

Regression discontinuity design estimations document a much larger impact of economic incentives on payment behavior than the salience intervention. Doubling the fine and the penalty for late payments significantly reduce timely payments. The absolute value of the effect from changes in economic incentives almost triples that of the salience intervention. Our results highlight the potential of providing simple information to correct for individuals' mis-perceptions about decision-relevant parameters as a cost-effective tool to enforce payments. This policy recommendation

is of particular relevance given the tendency of governments around the world to use overly complicated texts to enforce payments.

3.7 Appendix: Theory

In Section 3.3 we discussed the comparative statics of a pre-deadline payment w.r.t. the fine f. To derive the argument formally, note that the relevant cutoff for the decision to pay in period T (conditional on not having paid before) is

$$-c_T - f \geq \delta V_{T+1} \iff c_T \leq \hat{c}_T := -(f + \delta V_{T+1}). \tag{5}$$

Note further that the discounted (δ) option value V_{T+1} is negative, which means that $\hat{c}_T$ will be typically positive (unless f is very large). In particular, under the cutoff rule, the option value is given by

$$V_{T+1} = \int_0^{\hat{c}_{T+1}} (-c - f - K) dF(c) + (1 - F(\hat{c}_{T+1}))\, \delta V_{T+2}, \tag{6}$$

i.e., the probability of drawing a sufficiently low cost c and thus paying the fine f plus the late-pay penalty K in $T+1$ plus the probability of not paying in $T+1$ times the option value of further postponing the payment into the future.

Applying Leibniz's rule of integral differentiation to the cutoff $\hat{c}_T$ defined in (5), we obtain

$$\begin{aligned} \frac{\partial \hat{c}_T}{\partial f} &= -1 - \delta \frac{\partial V_{T+1}}{\partial f} \\ &= -1 - \delta \left(\underbrace{\int_0^{\hat{c}_{T+1}} (-1 - \frac{\partial K}{\partial f}) dF(c)}_{A} + \underbrace{(1 - F(\hat{c}_{T+1}))\, \delta \frac{\partial V_{T+2}}{\partial f}}_{B} \right. \\ &\quad \left. + \underbrace{(-\hat{c}_{T+1} - f - K - \frac{\partial F(\hat{c}_{T+1})}{\partial \hat{c}_{T+1}}) \frac{\partial \hat{c}_{T+1}}{\partial f}}_{C} \right). \end{aligned} \tag{7}$$

Term A is clearly negative. Term B is negative, as long as the option value clearly decreases as the payment obligation (here: f and, indirectly, K) increases. The last term, C, would only be negative if $\partial \hat{c}_{T+1}/\partial f \geq 0$. The sum in the brackets $(A + B + C)$ is multiplied by $-\delta$ and would thus turn positive, potentially offsetting the -1. In this case, one could obtain $\partial \hat{c}_T/\partial f > 0$. Whenever $\partial \hat{c}_{T+1}/\partial f < 0$, however, the sum $A + B + C$ could be positive (before multiplication with $-\delta$).

In this case, we would obtain $\partial \hat{c}_T / \partial f < 0$. The comparative static is therefore ambiguous.

3.8 Appendix: Complementary figures and tables

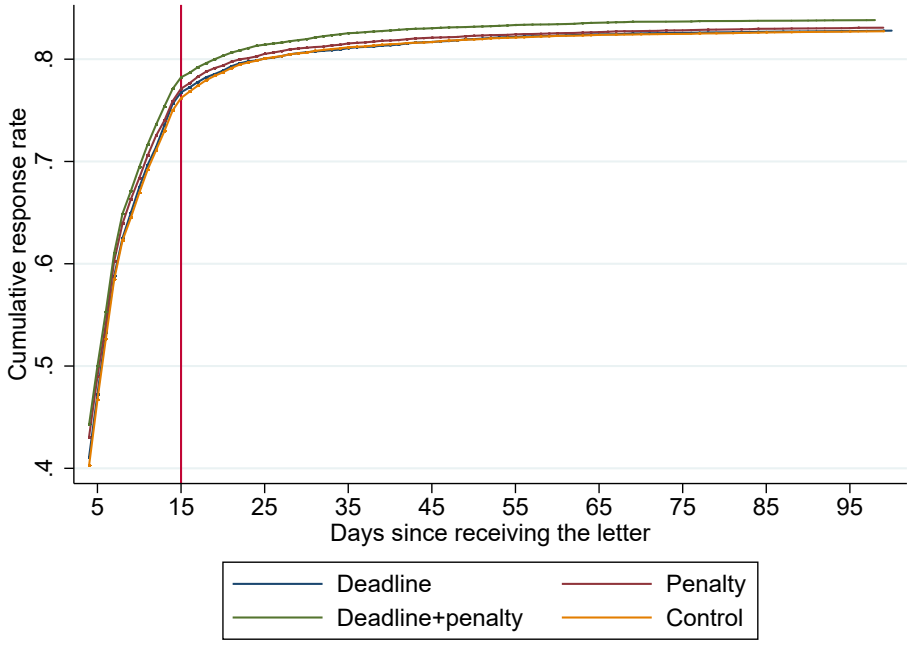


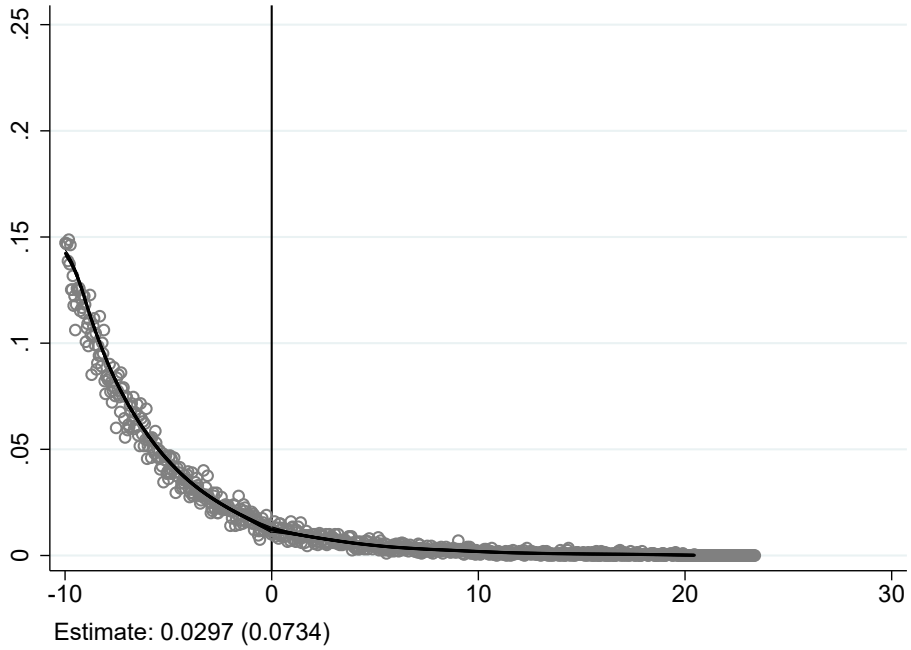
Figure 3.4: Cumulative response rates by treatment
Note: the payment deadline (15 days) is indicated with the red vertical line. The figure shows the cumulative response rate between the 3rd and the 21st day after receiving the summons, for each of the treatment and control groups.

Table 3.7: Survey descriptive statistics

	Control	Deadline	Penalty	Deadline & penalty	Total
Age	43.51	43.52	43.74	43.86	43.66
	(15.03)	(13.99)	(13.80)	(14.29)	(14.28)
Male	0.510	0.523	0.481	0.500	0.503
	(0.501)	(0.500)	(0.500)	(0.501)	(0.500)
Apprenticeship education	0.381	0.450	0.406	0.405	0.410
	(0.486)	(0.498)	(0.492)	(0.491)	(0.492)
Elementary school education	0.0842	0.0628	0.0529	0.0756	0.0690
	(0.278)	(0.243)	(0.224)	(0.265)	(0.254)
High school education	0.324	0.339	0.355	0.329	0.337
	(0.469)	(0.474)	(0.479)	(0.471)	(0.473)
University or higher education	0.210	0.148	0.186	0.190	0.184
	(0.408)	(0.356)	(0.390)	(0.393)	(0.388)
city of 100,000 + inhabitants	0.223	0.206	0.219	0.227	0.219
	(0.417)	(0.405)	(0.414)	(0.419)	(0.414)

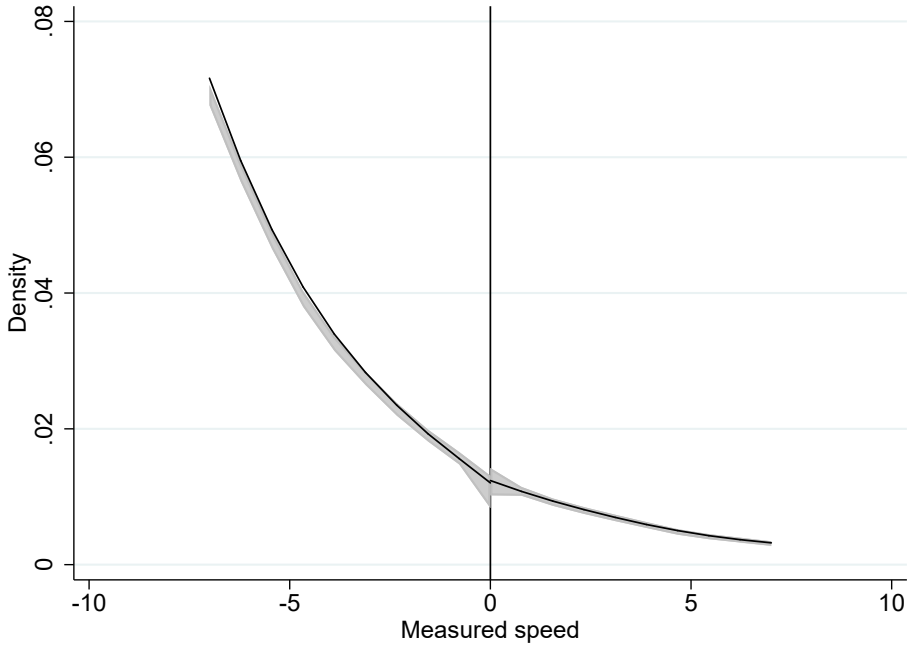
Notes: N=1609. Standard errors in parentheses.

Figure 3.5: Measured speed density plot for the full sample



Note: The graph shows the distribution of measured speed centered around the 23km/h speed limit threshold at which the severity of the offense changes from low to medium, with the corresponding 95% confidence intervals. Estimates at the bottom of the figure are from McCrary's (2008) heaping test.

Figure 3.6: Measured speed density plot for the full sample



Note: The graph shows the density plot of the running variable in the RDD estimations for the full sample of the RCT. Measured speed in the horizontal axis is centered at the 23km/h above the speed limit threshold. Local polynomials of second degree with 95% confidence are depicted. Triangle kernel and MSE optimal bandwidth selection procedures by Calonico *et al.* (2014) were used in the estimation of the polynomials.

Table 3.8: Treatment Effects on Expectations of Addition of Demerit Points

	(1)	(2)	(3)	(4)	(5)	(6)
Responses:	Yes		No		Maybe	
Deadline	0.015	0.018	-0.072**	-0.076**	0.057**	0.058**
	(0.033)	(0.033)	(0.035)	(0.035)	(0.026)	(0.027)
Penalty	0.308***	0.309***	-0.300***	-0.301***	-0.008	-0.008
	(0.034)	(0.034)	(0.033)	(0.033)	(0.024)	(0.024)
Deadline & penalty	0.310***	0.311***	-0.298***	-0.299***	-0.012	-0.012
	(0.033)	(0.033)	(0.033)	(0.033)	(0.024)	(0.024)
Constant	0.324***	0.287***	0.535***	0.567***	0.141***	0.146***
	(0.023)	(0.045)	(0.025)	(0.046)	(0.017)	(0.033)
F-Tests (p-values):						
$D = P$	0.000	0.000	0.000	0.000	0.014	0.013
$D = D\&P$	0.000	0.000	0.000	0.000	0.008	0.007
$P = D\&P$	0.955	0.964	0.938	0.935	0.859	0.868
Controls	-	Yes	-	Yes	-	Yes

Notes: The table presents LPM estimates of equations following the structure from (1). N = 1,609. The dependent variable in Columns (1) – (2) is a dummy indicating that an individual responded *yes* to the question *'If you do not pay the full amount of the fine by the deadline, would you expect to get demerit points within the demerit point system?'*. The dependent variable in Columns (3) and (4) captures responses indicating *no*. Columns (5) and (6) indicate responses of *maybe*. Control variables include age, gender, and education dummies. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3.9: Probability of payment

	(1)	(2)	(3)	(4)
Payments:	60 days		100 days	
Deadline	0.002	-0.001	0.002	-0.001
	(0.005)	(0.004)	(0.004)	(0.004)
Penalty	0.004	0.002	0.004	0.003
	(0.005)	(0.004)	(0.005)	(0.004)
Deadline & penalty	0.011*	0.010**	0.012**	0.011**
	(0.006)	(0.004)	(0.006)	(0.004)
Constant	0.783***	1.199***	0.785***	1.209***
	(0.022)	(0.032)	(0.022)	(0.031)
F-Tests (p-values):				
$D = P$	0.633	0.436	0.544	0.335
$D = D\&P$	0.110	0.015	0.074	0.007
$P = D\&P$	0.224	0.078	0.193	0.059
Controls	-	Yes	-	Yes
Observations	50,524	47,852	50,524	47,852

Notes: The table presents LPM estimates of equations following the structure from (2). All specifications contain dummies for the week the speeding ticket was sent. Standard errors given in parentheses are clustered at the vehicle level. Specifications (3) and (4) include a vector of variables controlling for severity of the offense, plate number of the vehicle, day of the week when the offense took place, holidays, number of days between the offense and both ticket sending and reception, hour of the day, measured speed at the time of the offense, amount of the fine, direction of the drive through (from or to Prague), whether the ticket was sent through regular mail, whether the ticket was issued to a vehicle that belongs to a company. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3.10: Balance of Observables around the 23km/h threshold

	(1) Car belongs to a corporation	(2) Plate code #2	(3) Plate code #3	(4) Tuesday	(5) Wednesday	(6) Thursday	(7) Friday	(8) Saturday	(9) Sunday	(10) Holiday
Estimate	-0.008	0.055	-0.018	0.009	-0.009	0.019	-0.038**	-0.046*	0.041*	-0.010
	(0.028)	(0.033)	(0.026)	(0.020)	(0.018)	(0.022)	(0.019)	(0.026)	(0.023)	(0.012)
Y(left)	0.501	0.516	0.270	0.126	0.109	0.138	0.126	0.220	0.176	0.0587
Optimal bandwidth	4.694	3.353	4.202	3.968	4.308	3.386	4.150	3.438	4.808	4.846
	(11) Days between offense and ticket	(12) Driving direction (from Prague)	(13) Email	(14) 12-6am	(15) 6-9am	(16) 9am-12pm	(17) 12-3pm	(18) 3-6pm	(19) 6-9pm	(20) 9pm-12am
Estimate	1.181**	0.016	-0.006	0.021	0.003	0.043*	0.020	-0.007	-0.043**	-0.047**
	(0.547)	(0.029)	(0.031)	(0.019)	(0.020)	(0.025)	(0.028)	(0.019)	(0.019)	(0.022)
Y(left)	5.932	0.557	0.434	0.106	0.0733	0.205	0.246	0.138	0.126	0.106
Optimal bandwidth	2.594	4.320	3.802	3.925	2.799	4.034	3.484	4.776	4.363	2.950

Notes: The table presents bias-corrected RD estimates with robust variance estimators (Calonico et al., 2014), implemented with the 2018/09 version of the rdrobust package (Calonico et al., 2017). All specifications use MSE optimal bandwidth estimated with a triangular kernel, local linear point estimators and local quadratic estimates for the bias correction. Y (left) indicates the mean of the dependent variable in the 0.5km/h bin below the cutoff. Number of observations for specification 11 is 50,388, for all other specifications is 50,524. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

3.9 Appendix: Cover letters and summons

Cover letters

The texts used in the different cover letters was the following (English translation):

Deadline (D) treatment:

Dear Sir/Madam,

We summon you to pay the prescribed amount for a speeding violation. We encourage you to carefully read the information contained in the attached pages and take appropriate action.

Please pay the amount in full and make sure it is credited to the city's account **within 15 days** after receiving this summons.

The city office of Ricany, legal division, department of fines

Penalty (P) treatment:

Dear Sir/Madam,

We summon you to pay the prescribed amount for a speeding violation. We encourage you to carefully read the information contained in the attached pages and take appropriate action.

If you do not pay the whole amount the office will continue investigating the offense. The amount that you will potentially have to pay **may be as high as CZK 2,500**.[18] In addition, the driver may be **added points** within the demerit point system.

The city office of Ricany, legal division, department of fines

[18]For medium-severity speeding violations (i.e., for a speed of more than 20km/h above the limit), this part reads 'as high as CZK 5,000'.

Deadline & Penalty ($D\&P$) treatment:

Dear Sir/Madam,

We summon you to pay the prescribed amount for a speeding violation. We encourage you to carefully read the information contained in the attached pages and take appropriate action.

Please pay the amount in full and make sure it is credited to the city's account **within 15 days** after receiving this summons.

If you do not pay the whole amount the office will continue investigating the offense. The amount that you will potentially have to pay **may be as high as CZK 2,500**.[19] In addition, the driver may be **added points** within the demerit point system.

The city office of Ricany, legal division, department of fines

[19]This part is again adjusted for medium-severity speeding violations (see previous footnote).

Standard notification

The next pages present the structure and layout of the standard legal notification sent to all drivers in our trial. (English translation from the original in Czech).

Docket number: XXXX Proceedings number: XXXX

The town authority of Ricany

Legal department,

Office of speeding cameras and vehicle weights

Contact official: name, phone Car operator: name, address

Date:

Summons for the payment of a prescribed amount under § 125h, paragraph 1, of Act No. 361/2000 Coll. on the Road Traffic and Amendments to Other Laws, as subsequently amended.

The town authority of Říčany, legal department, office of traffic violations, as a town authority of a municipality with extended jurisdiction, competent to the administrative proceedings under the provision of **§ 125h, paragraph 1, of law nb. 361/2000** on the Road Traffic and Amendments to Other Laws, as subsequently amended (hereinafter "road traffic law"), **summons** the operator of vehicle with license plate **2SJ7260** (hereinafter "vehicle"), with which road traffic law was breached on **03.01.2016** at **14:57** on the road situated on street Říčanská (between streets Kolovratská and Březinova in the length of 335,360 metres) in the municipality of Říčany in the direction from Voděrádky,

for the payment of sum

amounting to **900 Kč** (nine hundred Czech korunas) to the bank account 35-320390319/0800, variable symbol 9116001251, (message for the receiver: XXXXXXX), and to do so within 15 days since the delivery of this summons. Kindly pay your attention to crediting the said account with the prescribed sum within the given time limit.

The illegal act has been committed by an unspecified driver, who has not respected the traffic signs [INSERT: location-specific text describing the speed limit at a given location], by which he has violate the section [INSERT: specific paragraph of the road traffic law, depending on severity.] This violation appears to satisfy the definition of an offense under the section [INSERT: the appropriate section] of the road traffic law. The offense has been documented by an automated speed measurement system [INSERT details pertaining to this offense.]

Based on the findings of the town authority of Říčany, legal department, office of traffic violations, you are the operator of the vehicle with license plate **XXXXXX** and, according to the provision of **§ 10, paragraph 3** of the road traffic law you are obliged to ensure that, while using the vehicle on the road, all driver's duties and road traffic rules are being followed as prescribed by this law.This particular offense constitutes a breach of the driver's duties and,

concurrently, of the road traffic rules. The offense which shows indicia of a violation under the road traffic law; there was no traffic accident.

Because the above-mentioned offense may be, in accordance with the legal provisions, handled by a ticket procedure and, as it was previously mentioned, other conditions were also fulfilled for the special procedure of the administrative agency for the application of the strict liability of the vehicle operator under the provision of **§ 125h, paragraph 1** of the road traffic law, the prescribed amount of **900 Kč** has a legal basis according to the provision of **§ 125h, paragraph 2** of the road traffic law.

Notice:

1. If the prescribed amount is paid by the due date at the latest, i.e. if it is be credited to the said account within 15 days since the delivery of this summons, the town authority of Říčany, legal department, office of traffic violations, will defer the case in accordance with the provisions of **§ 125h, paragraph 5** of the road traffic law. Otherwise, it will proceed with the investigation of the offense.
2. If the prescribed amount is paid after the due date, the town authority of Říčany, legal department, office of traffic violations, will, in accordance with the provisions of **§ 125h, paragraph 7** of the road traffic law, immediately return it to the vehicle operator and will proceed with the investigation of the offense.
3. Concurrently, in accordance with the provisions of **§ 125h, paragraph 6** of the road traffic law, we instruct you that if you do not pay the prescribed amount, you may report, in writing, the identity details of the vehicle's driver who was driving the vehicle at the time of the offense to the town authority of Říčany, legal department, office of traffic violations, and you may do so within 15 days since the delivery of this summons. To report these details, you may use the attached form. Providing the identity details of the driver is considered, according to the provision of § 125h, paragraph, a submission of an explication.
4. In case that you neither pay the prescribed amount nor make use of your right to report the details of the identity of the vehicle's driver nor provide any other explanation within the given time limit, the town authority of Říčany, legal department, office of traffic violations, will proceed with the investigation of the offense.

If you regard it necessary to provide an oral explanation on record, you may visit our authority's office during the office hours (Monday and Wednesday 07:30-12:00 and 12:30-18:00; Tuesday and Thursday: 07:30-12:00) at Olivova 1800, Říčany.

Upon the payment of the prescribed amount, there are no penalty points imposed on the vehicle operator within the framework of the demerit point system of drivers under the provision of § 123a to § 123f of the road traffic law. This summons is not an administrative adjudication. Appeal against this summons is not admissible.

Contact officials’ signatures.

Attachments:

- Photo from the location of the speed measurement
- Form for the identity details of the vehicle driver at the time of the offense
- Postal money order

Original standard notification

The next pages present an example of the original standard legal notification in Czech sent to all drivers in our trial

Číslo jednací spisu: OPE/16/190

Číslo jednací: OPE/16/4042/RA

Městský úřad Říčany
Odbor právní, oddělení přestupkové

Olivova 1800
251 01 Říčany

Vyřizuje:
Mgr. Klára Hohošová 323 618 175
Ing. Iveta Hübnerová 323 618 176

Provozovatel:

Datum: 25.01.2016, Říčany

Výzva k zaplacení určené částky podle § 125h odst. 1 zákona č. 361/2000 Sb., o provozu na pozemních komunikacích a o změnách některých zákonů, ve znění pozdějších předpisů

Městský úřad Říčany, odbor právní, oddělení přestupkové, jako obecní úřad obce s rozšířenou působností, příslušný ke správnímu řízení podle ustanovení **§ 125h zákona č. 361/2000 Sb.**, o provozu na pozemních komunikacích a o změnách některých zákonů, ve znění pozdějších předpisů (dále jen „zákon o silničním provozu"), **vyzývá** provozovatele motorového vozidla registrační značky **2SJ7260** (dále jen „vozidlo"), s nímž došlo ke spáchání přestupku dne **03.01.2016 v 14:57** hodin na pozemní komunikaci ulice Říčanská (mezi ulicemi Kolovratská a Březinova v délce 335,360m) v obci Říčany směr od Voděrádek,

k uhrazení částky

ve výši **900 Kč** (devětset korun českých) na účet **35-320390319/0800**, variabilní symbol **9116001251**, (zpráva pro příjemce: [redacted], a to **do 15 dnů** od doručení výzvy. Věnujte laskavě pozornost tomu, aby byla v dané lhůtě tato částka připsána na zmíněný účet.

Přestupek byl ve výše uvedenou dobu spáchán blíže neurčeným řidičem v místě, kde je nejvyšší dovolená rychlost 50 km/h. Automatizovaným technickým prostředkem byla naměřena rychlost vozidla **61 km/h** po odečtení tolerance měřícího zařízení 3 km/h. Jedná se o přestupek proti bezpečnosti a plynulosti provozu na pozemních komunikacích podle ustanovení **§ 125c odst. 1 písm. f) bod 4** zákona o silničním provozu, tj. překročení nejvyšší dovolené rychlosti v obci o méně než 20 km/h. Tímto protiprávním jednáním byla porušena povinnost uložená řidiči v ustanovení **§ 18 odst. 4** zákona o silničním provozu, které udává, že v obci smí jet řidič rychlostí nejvýše 50 km/h, a jde-li o dálnici nebo silnici pro motorová vozidla, nejvýše 80 km/h.

Přestupek je průkazně zdokumentován.

Na základě zjištění Městského úřadu Říčany, odboru právního, oddělení přestupkového, jste provozovatelem motorového vozidla registrační značky **2SJ7260** a podle ustanovení **§ 10 odst. 3** zákona o silničním provozu je Vám uložena povinnost zajistit, aby při užití vozidla na pozemní komunikaci byly dodržovány povinnosti řidiče a pravidla provozu na pozemních komunikacích stanovená tímto zákonem.

V tomto případě se jedná o porušení povinností řidiče a současně pravidel provozu na pozemních komunikacích, které vykazuje znaky přestupku podle zákona o silničním provozu, k dopravní nehodě nedošlo.

Protože uvedený přestupek lze v souladu s právním předpisem projednat v blokovém řízení, a jak je v předchozím uvedeno, jsou splněny i ostatní podmínky zvláštního postupu správního orgánu pro uplatnění objektivní odpovědnosti provozovatele vozidla podle **§ 125h odst. 1** zákona o silničním provozu, je určení částky **900 Kč** podle **§ 125h odst. 2** zákona o silničním provozu právně podloženo.

Poučení:

1. Bude-li určená částka uhrazena nejpozději v den splatnosti, tj. připsána na zmíněný účet **do 15 dnů** od doručení této výzvy, Městský úřad Říčany, odbor právní, oddělení přestupkové, věc v souladu s ustanovením **§ 125h odst. 5** zákona o silničním provozu **odloží**. V opačném případě bude pokračovat v šetření přestupku.

2. Jestliže bude určená částka uhrazena po dni splatnosti, Městský úřad Říčany, odbor právní, oddělení přestupkové, ji v souladu s ustanovením **§ 125h odst. 7** zákona o silničním provozu bezodkladně **vrátí** provozovateli vozidla a bude pokračovat v šetření přestupku.

3. Současně Vás v souladu s ustanovením **§ 125h odst. 6** zákona o silničním provozu poučujeme, že neuhradíte-li určenou částku, můžete Městskému úřadu Říčany, odboru právnímu, oddělení přestupkovému, písemně sdělit údaje o totožnosti řidiče vozidla, který v době spáchání přestupku řídil, a to ve lhůtě **do 15 dnů** od doručení této výzvy. K tomuto sdělení lze využít přiložený formulář. Sdělení údajů o totožnosti řidiče vozidla se považuje ve smyslu § 125h odst. 6 zákona o silničním provozu za podání vysvětlení.

4. V případě, že v dané lhůtě ani neuhradíte určenou částku, ani nevyužijete svého práva písemně sdělit údaje o totožnosti řidiče či podat jiné vysvětlení, bude Městský úřad Říčany, odbor právní, oddělení přestupkové, pokračovat v šetření přestupku.

Pokud považujete za nezbytné podat vysvětlení ústně do protokolu, lze náš úřad navštívit v úředních hodinách (pondělí a středa: 07:30-12:00 a 12:30-18:00 hodin; úterý a čtvrtek: 07:30-12:00 hodin) na adrese Olivova 1800, Říčany.

V souvislosti s úhradou určené částky se neukládají provozovateli vozidla body v rámci bodového hodnocení řidičů s ohledem na ustanovení § 123a až § 123f zákona o silničním provozu. Tato výzva není správním rozhodnutím. Odvolání proti této výzvě není přípustné.

Mgr. Klára Hohošová
Ing. Iveta Hübnerová

Přílohy: Fotografie z místa měření
Formulář pro údaje o totožnosti řidiče vozidla v době spáchání přestupku
Složenka

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Paper: “Fueling fiscal interactions: commodity price shocks and local government spending in Colombia”
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