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**THE EMPIRE IS DEAD, LONG LIVE
THE EMPIRE! LONG-RUN
PERSISTENCE OF TRUST AND
CORRUPTION IN THE BUREAUCRACY**

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Hainz and Ludger Woessmann

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ABSTRACT

The Empire Is Dead, Long Live the Empire! Long-Run Persistence of Trust and Corruption in the Bureaucracy*

Do empires affect attitudes towards the state long after their demise? We hypothesize that the Habsburg Empire with its localized and well-respected administration increased citizens' trust in local public services. In several Eastern European countries, communities on both sides of the long-gone Habsburg border have been sharing common formal institutions for a century now. Identifying from individuals living within a restricted band around the former border, we find that historical Habsburg affiliation increases current trust and reduces corruption in courts and police. Falsification tests of spuriously moved borders, geographic and pre-existing differences, and interpersonal trust corroborate a genuine Habsburg effect.

JEL Classification: D73, N33, N34 and Z10

Keywords: borders, corruption, Habsburg Empire, institutions and trust

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“No other family has endured so long or left so deep a mark upon Europe: the Habsburgs were the greatest dynasty of modern history, and the history of central Europe revolves around them, not they around it.”

AJP Taylor (1948),
The Habsburg Monarchy 1809-1918

I. Introduction

The famous phrase “The emperor is dead, long live the emperor!” indicates that, even though individual emperors may die, their empire lives on with their immediate successors. But what if not one emperor, but the whole empire itself perishes? In this paper, we show that empires can leave a lasting legacy in cultural norms and the ensuing functioning of state institutions even several generations after their formal institutions have ceased to exist. Specifically, we find that the Habsburg Empire, which went down in 1918, still affects trust and corruption in local public services in Central and Eastern Europe today. Our findings add to a growing literature indicating that history can have long-lasting effects (cf. Nunn 2009) through its impact on current formal institutions¹ or on values, beliefs, and cultural norms.² Our results indicate that long-gone formal institutions can have a persistent impact on cultural norms of social behavior, thereby affecting the functioning of interactions between citizens and the state.

Trust in the key institutions of the state and their proper functioning is crucial in facilitating collective action (Ostrom 1998). Trust among participants (Arrow 1972) and the enforcement of rules and property rights (North 1990) are two leading mechanisms that help to organize human cooperation, interaction, and exchange, thereby providing the cultural and legal underpinning for groups to achieve mutually productive outcomes. As views on the optimal scope of the state differ, we focus our analysis on two public services that enact basic functions of the state which even critical skeptics of government intervention support: the courts and the police as the enforcers of rules in collective action (e.g., Hayek 1960). Rather than analyzing interpersonal trust, we focus on citizens’ trust in these state institutions and on corruption, a leading example of malfunctioning of the interaction of citizens with the state.

¹ E.g., North (1990); Engerman and Sokoloff (1997); La Porta et al. (1998); Acemoglu, Johnson, and Robinson (2001).

² E.g., Putnam (1993); Greif (1994); Alesina and Fuchs-Schündeln (2007); Guiso, Sapienza, and Zingales (2008a); Tabellini (2010).

The Habsburg Empire is historically known as a multi-ethnic state with a relatively well-functioning, respected bureaucracy.³ Historians characterize the Habsburg bureaucracy as “fairly honest, quite hard-working, and generally high-minded” (Taylor 1948, p. 44) – in contrast to other Empires in Eastern Europe, like the Ottoman and Russian Empires (cf. Ingraio 1996, 2000; Subtelny 2007). We argue that this attitude created trust of its inhabitants in the respectability of government institutions, with ensuing effects on the functioning of citizen-state interactions, particularly at the local level. However, the formal institutions of the empire ceased to exist with the collapse of the Habsburg Empire after World War I, breaking up into separate nation states that have seen several waves of drastic institutional changes since.

To test whether the cultural norms originating in the Habsburg Empire still endure today, we use the micro dataset of the 2006 Life in Transition Survey (LiTS) that provides measures of trust and corruption in Eastern European countries. We focus on the 17 countries that comprise the successor states of the Habsburg Empire and their neighboring countries. Drawing on a variety of historical sources, we coded the location of each observation in the LiTS dataset in terms of its affiliation with the Habsburg Empire. Yet, a simple comparison of cultural measures across countries with diverse populations, geographies, and intervening experiences may easily be biased by unobserved heterogeneity.

To identify the enduring effect of the Habsburg Empire, we therefore devise a border specification that compares individuals living in communities located within 200 kilometers of each other on either side of the long-gone Habsburg border, exploiting the geographical discontinuity created by the Habsburg Empire in Eastern Europe. In order not to capture unobserved country heterogeneity, we use country fixed effects to restrict the analysis strictly to variation within individual modern-day countries. This identification exploits the fact that the former Habsburg border cuts straight through five countries today – Montenegro, Poland, Romania, Serbia, and Ukraine. Communities on the two sides of the former border have been sharing a common statehood for generations now. Additionally, we control for a large set of individual-level factors such as education, religion, language, wealth indicators, and urbanity.

Our results suggest that the Habsburg Empire still exerts effects on cultural norms and interactions of humans with their state institutions today. Comparing individuals left and right of

³ When referring to the Habsburg Empire in this paper, we focus on its impact in Eastern Europe and do not refer to Habsburg influence in Western Europe such as in Spain and the Netherlands.

the long-gone Habsburg border, people living in locations that used to be territory of the Habsburg Empire have higher trust in courts and police. These trust differentials also transform into real differences in the extent to which bribes have to be paid for these local public services.

A set of falsification tests validates a causal interpretation of the results. First, when using “placebo” borders 100 kilometers inwards or outwards of the actual Habsburg border, we do not find any effects. This indicates that our results capture a specific Habsburg effect, rather than a general West-East pattern. Second, we verify that altitude does not vary significantly between the two sides of the former Habsburg border, thereby excluding obvious geographical differences between the Habsburg and non-Habsburg sample. Third, we do not find significant differences between the two sides in terms of medieval city size, access to medieval trade routes, and presence of a medieval diocesan town, suggesting that the Habsburg effect is not simply a perpetuation of differences that existed before Habsburg influence. Fourth, we show that there is no Habsburg effect on trust in other people and on membership in civic organizations, corroborating that the identified Habsburg effect is genuine to citizen-state interactions.

Additional aspects of our results include that they are robust when restricting the comparison groups to formerly Ottoman regions and cannot be distinguished between alternative neighboring empires. The Habsburg effect does not vary significantly with the duration of Habsburg affiliation, consistent with models that predict persistent effects of limited exposure. There is no strong evidence of Habsburg effects on trust in central public services. Finally, evidence from a firm dataset, the Business Environment and Enterprise Performance Survey (BEEPS), corroborates the general pattern of results derived from the LiTS household dataset.

Our results show that political and judicial institutions that were in effect a long time ago have formed cultural norms that prevail today. These cultural traits are a link through which distant history affects the present.

The remainder of the paper is organized as follows. Section II provides theoretical and historical background and derives the main hypotheses to be tested. Section III describes our data. Section IV develops the empirical identification strategy. Section V presents our basic results of Habsburg effects on trust and corruption in local public services. Section VI reports a series of falsification tests to support the validity of the identification strategy. Section VII analyzes additional aspects of the Habsburg effect, and Section VIII presents supporting evidence from a business survey. Section IX concludes.

II. Theoretical and Historical Background

This section starts out by discussing different channels through which history may leave a legacy for current outcomes and relates these to the existing literature. Next is a brief overview of the history of the Habsburg Empire as it relates to the subject of our analysis. From this theoretical and historical background, we derive the main hypotheses to be tested in the paper.

A. *Why History Matters: Some Theory, with Reference to Related Literature*

A growing literature investigates the different mechanisms leading to the fact that history often has long-term repercussions for economic development (see Nunn 2009 for a review). First, historical circumstances and events can shape the state and evolution of *formal institutions* that survive and affect economic interactions and outcomes today (e.g., North 1990). For example, recent research on the importance of colonial rule for long-term economic development emphasizes its impact on current formal institutions.⁴ A second reason why past developments may be related to current developments is *geography*. Geographical and ecological factors that do not vary over time, such as climate zone, disease environment, natural endowments, coastal location, and continental orientation may have direct effects on economic development past and present, as well as on the path of history (Diamond 1997; Sachs 2003).⁵ Third, historical events may have caused differential development of people's knowledge, *human capital*, which subsequently led to differences in economic development. For example, Glaeser et al. (2004) argue that European colonization may have left a long-term legacy not because of institutional development, but because colonial settlers brought their human capital with them.⁶

⁴ This effect may work through the bearing of large-scale plantation production on inequality and thus institutional development (Engerman and Sokoloff 1997), through the introduction of civil vs. common law legal systems (La Porta et al. 1998), or through persistence of property-rights institutions determined by initial disease environments (Acemoglu, Johnson, and Robinson 2001). Similarly, Acemoglu, Johnson, and Robinson (2005) argue that access to Atlantic trade affected the evolution of formal institutions in Western Europe. Nunn (2008) shows that external trade in slaves had long-run repercussions in Africa. Jha (2008) argues that medieval trade access led to institutions promoting religious tolerance in India. Acemoglu et al. (2011) show that French invasion of Central Europe after the French Revolution brought radical institutional changes that left a long-lasting mark.

⁵ The effect of geography may interact with the evolution of formal institutions, for example, when the geographical endowment spurs cash cropping (Engerman and Sokoloff 1997) or when disease environments affect institutional choices because they impact settler mortality (Acemoglu, Johnson, and Robinson 2001).

⁶ As another example, Becker and Woessmann (2009) show that the Protestant Reformation affected later economic development within Prussia and across countries by raising literacy levels, required to read the bible. In a similar vein, Woodberry (2004) depicts a positive association between historic Protestant missionaries and modern-day school enrollment across colonized countries.

The fourth mechanism through which history can affect long-term outcomes is the topic of this paper: History can matter for later economic development by affecting people's *cultural norms and values* which then persist over time. Recent literature shows that cultural factors such as trust, manners of social interaction, and other values, beliefs, and norms have important repercussions for economic development (e.g., Algan and Cahuc 2010; Tabellini 2010). Probably the best-known example of the cultural channel is Weber's (1904) hypothesis that a specific Protestant work ethic furthered capitalist development.⁷ Putnam (1993) conjectures that the culture of independence fostered by the experience of free city-states at the turn of the first millennium fostered a culture of independence that left a mark on social capital and economic development in Italy today. Guiso, Sapienza, and Zingales (2008a) find supportive evidence.⁸

Cultural evolution may also be closely interrelated with institutional development (Greif 2006; Greif and Tabellini 2010).⁹ With respect to empires, evidence in Grosjean (2011) suggests that in Eastern Europe, people living in areas that used to be part of the same empire have more similar trust values.¹⁰ Differing cultural norms can affect outcomes even under the same formal institutions, for example when they lead to a different functioning of the formal institutions.

The mechanisms of the transmission of cultural norms over time are not well understood so far. Most of the current literature focuses on intergenerational transmission within families,¹¹ for example when looking at parent-child correlations (Dohmen, Falk, Huffman, and Sunde 2011) or when identifying links of U.S. immigrants with their own or their ancestors' home countries.¹² Alternatively, cultural norms can persist by transmission via schooling, or even through the very nature of local interactions: If a person with a prior of trust in local public services moves into a

⁷ However, Becker and Woessmann (2009) and Cantoni (2009) find little evidence for this specific channel in historical indicators of economic development.

⁸ As a classical example of the cultural channel, Greif (1994) describes how the collectivist vs. individual attitudes towards contract enforcement of Maghribi vs. Genoese medieval merchants affected their subsequent developments. More recently, Alesina and Fuchs-Schündeln (2007) suggest that the differing history of West and East Germany from 1945-1990 left a mark in different political preferences for redistribution.

⁹ Nunn and Wantchekon (2011) identify cultural norms and formal institutions as two separate channels of the effect of slave trade in Africa. Fisman and Miguel (2007) find that both cultural norms of the home country and legal enforcement mechanisms affect parking-violation behavior of United Nations officials in Manhattan.

¹⁰ Cultural norms may also relate to historical education. Tabellini (2010) finds that historical measures of literacy and political institutions are correlated with cultural variables today and subsequently with economic development across European regions.

¹¹ E.g., Bisin and Verdier (2000); Tabellini (2008b); Guiso, Sapienza, and Zingales (2008b).

¹² E.g., Guiso, Sapienza, and Zingales (2006); Giuliano (2007); Tabellini (2008a); Fernández and Fogli (2009); Algan and Cahuc (2010).

town with a corrupt bureaucracy, he might quickly change over to a prior of distrust, and vice versa.¹³ Thus, through continuous reciprocal behavior, a local equilibrium of trust or distrust may be maintained even with migration, and even without any family ties. Despite a multiplicity of possible cultural equilibria, the selected equilibrium can thus be highly persistent (cf. Guiso, Sapienza, and Zingales 2008b; Belloc and Bowles 2009). In line with this argument, Ichino and Maggi (2000) show that workers moving into other branches of a large Italian firm in fact adopt local habits of their new region quickly. Similarly, the large literature on peer effects suggests that people's attributes may affect other people (see Sacerdote 2011 for a survey).

Much of the work on history's role through cultural persistence has, in the tradition of Arrow (1972), focused on trust among people in interpersonal interactions. Little is known, however, on the effect of history on people's trust in the state and the ensuing functioning of state institutions. Such trust in citizen-state interactions gains particular relevance from the importance of rule enforcement in collective action and a well-functioning state (see above). Trust in state institutions is also the more obvious way in which historical empires with the particular character of their bureaucracies may leave lasting legacies.

In this paper, we aim to test the fourth channel of history on cultural norms today, while controlling for the other channels, in the specific case of the Habsburg Empire.¹⁴ Specifically, we are interested in whether persistent cultural norms towards the interaction of individuals and local state authorities can be a channel through which historical, but long-gone formal institutions can affect outcomes today. Beyond trust, we also analyze whether the effect extends to corruption – defined as the misuse of public office for private gains (Svensson 2005) – as a real consequence in the interaction between citizens and the state.

B. Historical Background on the Habsburg Empire

Beginning in the 11th century, the Habsburg dynasty gained a multitude of territories from Spain in the West over the Netherlands, Austria, the Dalmatian coast, Bohemia, Moravia,

¹³ The idea that trust priors adopt quickly to local environments is consistent with the theory of broken windows which suggests that first impressions of local environments affect lawful behavior. It goes back to an experiment showing that an abandoned open car was vandalized quickly in the Bronx but not in Palo Alto, where it was also vandalized as soon as it had a broken window. The effectiveness of broken windows policy, which influenced anti-crime policies in such cities as New York City and Los Angeles, has been validated in Corman and Mocan (2005), and laboratory experiments confirm that first impressions causally affect social behavior (Beckenkamp et al. 2009).

¹⁴ In analyzing effects of empires, Mitchener and Weidenmier (2008) and Grosjean (2011) use gravity models to show that belonging to the same empire in general has effects on trade and cultural integration, respectively.

Hungary, and Bosnia to Galicia in the East. The Habsburg¹⁵ identification with Austria began when Rudolf IV of Habsburg was elected king of the Holy Roman Empire in 1273. Since then, the Habsburgs continuously expanded their territories, by marriage,¹⁶ by succession to the throne, but also by wars – and even by wars that were waged without Austria. In the 16th century, more than half of Europe was ruled by the House of Habsburg, which was itself a decisive factor in keeping the vast country together. Charles V (1500-1558) ruled the Holy Roman Empire, a realm of almost four million square kilometers where “the sun never sets.” For five centuries, Austria was the great Central European superpower, until its dismemberment in World War I (Zöllner 1990). Reasons underlying the ultimate fall of the Habsburg Empire in 1918 include national intentions of the elites of the different peoples living in the Habsburg territories and the political will of the winning powers of World War I.¹⁷

In Eastern Europe,¹⁸ Habsburg broadened its territory in 1526, when – after the death of Hungarian King Louis II – Ferdinand of Austria, brother of Holy Roman Emperor Charles V, was elected King of Hungary, Croatia, and Bohemia. Habsburg now had to bear the main burden of the Ottoman drive from the Balkans into Central Europe. Twice, the expanding Ottoman Empire tried to capture Vienna, in 1529 and 1684. The latter battle marked the beginning of the political hegemony of the Habsburg dynasty in Eastern Europe.¹⁹ Step by step, it conquered vast territories along the Danube – in Hungary, Croatia, Serbia, and Romania – constantly driving back the Ottomans. External events caused Habsburg’s north-eastward expansion: the First Partition of Poland in 1772, arranged by Russia and Prussia, brought Galicia and Lodomeria. The acquisition of Bukovina in 1775 was a side effect of the Treaty of Küçük Kainardca (1774) after the Russo-Ottoman War. Habsburg attempted to prevent Russia and its ally Serbia from gaining further territories in the area, until the conflict with Russia became notorious in the 19th century.

¹⁵ The name Habsburg derives from the municipality and castle of Habsburg, in what is now Switzerland, where the Habsburg family originates. For simplicity, we generally use the term *Habsburg Empire* to refer to the entire Habsburg history, although it is sometimes used more narrowly to refer to the period 1526-1867. The name *Austrian Empire* is officially applied during 1804-1867, and *Austro-Hungarian Empire* officially describes the two states with one common reign during 1867-1918.

¹⁶ As a famous hexameter put it, “Bella gerant alii, tu felix Austria nube” (Let the others wage wars, you, fortunate Austria, marry). This mostly related to the westwards expansion.

¹⁷ For historical background on different economic aspects of the Habsburg Empire, see, e.g., Good (1984), Komlos (1983), and Schulze and Wolf (2009, 2011).

¹⁸ In the remainder of the paper, we use “Eastern Europe” as shorthand for those countries that used to be east of the “Iron Curtain,” which are the countries covered by the LiTS dataset used in our analysis.

¹⁹ See also Iyigun (2008) on the history and repercussions of the Ottomans’ military engagement in Europe.

To maintain a balance of power between the leading European powers, the Treaty of Berlin in 1878 allowed the Austro-Hungarian Empire to occupy Bosnia, Herzegovina, and the Sanjak of Novi Pazar in Serbia and Montenegro (Glenny 2000).

The Habsburg Empire is the prototype of a *Vielvölkerstaat* (state composed of many peoples) that largely respected the identity and local differences of various parts of the empire (Ingrao 2000). Despite the national aspirations of the peoples within the empire, some aspects of Habsburg policy were widely accepted. In particular, the bureaucracy throughout the empire was well respected by the population because of its reliability.²⁰ Taylor (1948, p. 44) paraphrases this as follows: “The Austrian bureaucracy was fairly honest, quite hard-working, and generally high-minded, it probably did more good than harm.”

Originally, the different parts of the Habsburg Empire were only loosely tied together. This changed during the 18th century when the organization was increasingly centralized, although most parts of the actual administration remained highly decentralized. Already Maria Theresa (1741-1765) began to establish a *Beamtenstaat* (an administration of civil servants) and instituted *Kreishauptmänner* (county governors) to supervise local administration in different parts of the empire. Her son, Josef II (1765-1790), an enlightened and secularized monarch, resolutely continued this way. He ended censorship, induced complex legal reforms, established German as official language throughout the empire, promoted public education in the vernacular, and founded institutions of social and medical care.²¹ As discussed in greater detail in Section VII.A below, this administration of the Habsburg Empire differed in central aspects from the neighboring empires. Compared to its neighbors in the east and the north, the Habsburg state ruled in a manner that was more acceptable and predictable for the inhabitants of the empire.

From the theoretical and historical discussion, we thus infer two main hypotheses on the enduring effects of the Habsburg Empire that we want to test in this paper. First, because of the

²⁰ Several elements of the Habsburg bureaucracy survive to this day. For example, Emperor Franz Joseph was known to get up early and expected to be able to reach his civil servants in office as well. In the Czech Republic, offices generally open at 7am to this day.

²¹ Additional well-received aspects of Habsburg policy include transfers in the form of subsidies and infrastructure projects such as railroads to less developed regions in order to foster their integration. In the Habsburg lands, education was also more important than in the Russian and Ottoman Empires. As an exemplification, in his famous novel “The Bridge over the Drina”, Literature Nobel Prize laureate Ivo Andrić describes how the Habsburg rule introduced public infrastructure, well-functioning administration, and public order and created an era of relative security and advancement when it took over Višegrad, a town in the east of Bosnia and Herzegovina near Serbia, in 1878. See also Imamović (2007) and Bencze (2006). In addition, the Austro-Hungarian army was a functioning multicultural microcosm and an important instrument for integrating peoples from all over the Habsburg territories.

historical experience of a relatively decentralized, honest, and widely accepted state bureaucracy, we hypothesize that Habsburg positively affected people's trust in local public services. Second, we expect that this enduring effect on people's values reduces corruption in interactions with the local administration, despite the fact that formal institutions, laws, and legal regulations do not differ anymore between former Habsburg and non-Habsburg areas.

III. Data: Trust and Corruption in the Life in Transition Survey (LiTS)

Our main database is the Life in Transition Survey (LiTS) collected by the European Bank for Reconstruction and Development (EBRD) in 29 countries between August and October 2006. The LiTS aimed at surveying how the transition process after the fall of Communism affected people's lives. Besides socio-demographic information such as age, gender, and education, the survey collected information on trust in public services and whether respondents usually paid bribes in connection with these services.²² In each country, 1,000 households were interviewed, deriving from a sample of 20 households in 50 locations (Primary Sampling Units, PSUs).²³ The LiTS dataset contains information on the municipality of residence of survey respondents. Figure 1 displays the location of municipalities in the LiTS dataset.

We restrict our analysis to countries that are either successor states of the Habsburg Empire or neighboring countries thereof. Austria, not being a transition country, is not part of the survey. Consequently, our dataset covers the following 17 countries in Eastern Europe: Albania, Belarus, Bosnia, Bulgaria, Croatia, Czech Republic, Former Yugoslav Republic of Macedonia, Hungary, Moldova, Montenegro, Poland, Romania, Russia, Serbia, Slovakia, Slovenia, and Ukraine. Table 1 documents which of these countries ever belonged to the Habsburg Empire and which did not. About half of the total sample is made up of respondents living in areas that formerly belonged to the Habsburg Empire.

For each municipality in our data, we collected information on affiliation with the Habsburg Empire, including its duration. Our sources for this information are Hrvatski Povijesni Atlas

²² Other studies that use the LiTS database to study trust and subjective preferences in the economic context include Aghion et al. (2010), Grosjean (2011), and Grosjean and Senik (2011).

²³ The PSUs are usually based on electoral registers or census enumeration areas. The total number of PSUs in the sampling frame per country ranged from 630 in Montenegro to 14,771 in Romania in our five-country sample. In the empirical analysis, we cluster standard errors by PSUs. Per household, one respondent was sampled following the Kish grid method (adult person in household who last had birthday).

(2003), Kinder and Hilgemann (2004), Leisering (2004), Magocsi (2002), Reden (1995), Rothaug (2001), and additional specific investigations. Furthermore, we geo-coded municipality data to compute distances among the locations. We use this information below to restrict the sample to respondents in municipalities within a certain distance of the Habsburg border.

Our analyses focus on two main sets of outcome measures, reflecting trust and corruption in local public services. The LiTS questionnaire surveys trust in public institutions by asking, “To what extent do you trust the following institutions?” with a list of institutions including, among others, courts and police as what are mostly local public services and central state institutions such as political parties, the parliament, the government/cabinet of ministers, the presidency, and the armed forces, as well as trade unions as an example of a non-state institution. In contrast to other comparable surveys on trust (e.g., the World Values Survey or the US General Social Survey), the LiTS asks respondents to express the intensity of their trust beliefs.²⁴ The LiTS survey uses the following five-category scale: complete distrust, some distrust, neither trust nor distrust, some trust, and complete trust. We exclude the few observations reporting the additional category of “difficult to say” from our regression analyses (6.0 percent for courts and 3.6 percent for police, with no difference between Habsburg and non-Habsburg). In addition, the LiTS questionnaire also surveys trust in people by asking, “Generally speaking, would you say that most people can be trusted, or that you can’t be too careful in dealing with people?”

The LiTS questionnaire surveys corruption in public services by asking, “In your opinion, how often is it necessary for people like you to have to make unofficial payments/gifts in these situations?” including courts and the road police as local public services.²⁵ The five-point categorical answer scale ranges from never over seldom, sometimes, and usually to always.²⁶

At a descriptive level, Table 2 shows that distrust in and corruption of courts and police are higher in formerly non-Habsburg areas than in formerly Habsburg areas.

²⁴ The answer categories differ from those in the World Value Survey, which are either “Most people can be trusted” or “Can’t be too careful”. Given this formulation, the response may be “not only shaped by people’s beliefs about others’ trustworthiness, but also by their own preferences towards taking social risks” (Fehr 2009, p. 239). The formulation in the LiTS with a scale from complete distrust to complete trust seems superior in this regard.

²⁵ Another category refers to unofficial payments to “other police,” which is not further specified. Here, we restrict our analysis to the well-defined case of “road police.” However, results (available from the authors on request) are very similar for the remaining category, although mostly at lower levels of statistical significance.

²⁶ While it is hard to validate such corruption measures based on subjective responses, we can at least show external validity with respect to standard corruption measures at the country level: In our 17-country sample, the average of the LiTS corruption measures has a correlation coefficient with the Corruption Perceptions Index 2006 of Transparency International (2007) of .49, statistically significant at the 5 percent level.

As a falsification test, we also analyze measures of membership in organizations, which is often used as a measure of social capital (Glaeser, Laibson, and Sacerdote 2002). The LiTS questionnaire asks, “Are you a member of? (a) a political party, (b) other civic/voluntary organizations (club, association).” The respondent can answer with yes or no.

In addition to the outcome variables, the LiTS dataset provides a rich set of covariates observed at the individual level. These include respondents’ age, gender, education, religion, and indicators for whether respondents worked for income during last twelve months, for whether their native language is an official national language, and for whether they belong to an ethnic minority. In addition, household-level information is reported on household size, the number of children under age 14, and a set of possessions such as cars, credit cards, and mobile phones. Table 3 reports descriptive statistics of these variables.

IV. Empirical Model

To estimate whether historical affiliation with the Habsburg Empire has a lasting effect on trust and corruption in local public services, our basic model expresses the outcome variables y – the measures of trust and corruption – of individual i living in location l in country c as a function of an indicator whether the individual lives in a community that historically used to be affiliated with the Habsburg Empire, H , and a set of individual-level and household-level control variables X :

$$(1) \quad y_{ilc} = \alpha + \beta H_{ilc} + X'_{ilc} \gamma + \varepsilon_{ilc}$$

Estimation at the individual level in a multivariate regression framework allows controlling for observable factors that vary systematically across individuals in our sample. We divide the rich set of control variables into two parts. The first set of controls includes variables that are arguably exogenous to the Habsburg effect – individual age and gender – as well as variables whose effect we want to separate from any possible effect of the historical Habsburg administration – language, ethnic minority status, and religion. The second set of controls includes variables that may be viewed as endogenous to the Habsburg effect and are thus not included when estimating the reduced-form Habsburg effect – variables such as individual working status, sets of indicators of household property and of education, urban or metropolitan character of the community location, household size, and the number of young children. Given

that the LiTS sampling frame drew 20 individuals per location l , we allow for clustering of the standard errors ε at the PSU level l throughout this paper.

Identification in this setting is hampered by the fact that not all important factors may be readily observed. Most obviously, trust and corruption may differ across countries for reasons other than past affiliation with the Habsburg Empire. When identification comes from cross-country variation between such former-Habsburg countries as the Czech Republic or Slovenia on the one hand and such non-Habsburg countries as Belarus or Moldova on the other, important general country characteristics such as aspects of formal institutions and geography may go unobserved. Such unobserved country heterogeneity, if correlated both with former Habsburg status and with cultural norms, would introduce omitted variable bias in the estimates of β .

To address such bias from omitted country variables, as a first step we include country fixed effects in the model specification. This model specification exploits the fact that the former Habsburg border cuts through several Eastern European countries. People living in these countries have been sharing a common set of formal institutions at least since the dissolution of the Habsburg Empire in 1918. But part of the population lives in territories that used to be part of the Habsburg Empire and another part in territories that did not belong to the Habsburg Empire. By including country fixed effects in the model, identification comes solely from variation within each of these countries. This effectively restricts identification to the sub-sample of countries that encompass both areas that used to be part of Habsburg and areas that were never part of Habsburg. This sub-sample includes five countries: Montenegro, Poland, Romania, Serbia, and Ukraine. The within-country share of individuals in our data who live on territory that used to belong to Habsburg is between 40 and 48 percent in Montenegro, Poland, and Romania, while it is low in Ukraine and high in Serbia (see Table 1). The Appendix describes how, after 1918, these countries ended up combining Habsburg and non-Habsburg parts. It points out that the newly formed countries mostly united historically related territories and that the Habsburg border did not coincide with earlier ethnological separations.

While within-country identification solves the most obvious issues of unobserved heterogeneity, countries like Poland, Romania, and in particular Ukraine still have such a sizeable geographical expansion that important factors may yet remain omitted from the model. Therefore, as a second step, we further restrict the sample to individuals i living within a restricted band around the former Habsburg border:

$$(2) \quad y_{ilc} = \alpha_c + \beta H_{ilc} + X'_{ilc} \gamma + \varepsilon_{ilc} \quad \text{if } i \in \text{border region}$$

where α_c is a full set of country fixed effects and border region refers to a close geographical band around the Habsburg border. The Habsburg border – no longer existent at least since 1918 – gives rise to a geographic discontinuity within the five-country sample. This border specification identifies the Habsburg effect by comparing individuals left and right of the former Habsburg border living in reasonable proximity to one another. Proximity of residence, within a common region that is divided by a non-existing border, is meant to induce similarity in important unobserved variables. To balance the desire for a narrow band to ensure similarity against the desire for large enough samples to retain statistical power, we compare individuals living in communities located within 200 kilometers (124 miles) of each other on either side of the historical Habsburg border. In robustness analyses, we also use even narrower bands.

To ensure that the “control group” to which the Habsburg “treatment group” is compared does not include locations that had actually also been exposed to the Habsburg treatment at some time, throughout the definition of the Habsburg variable H is an indicator of whether a location has ever been part of the Habsburg Empire. By contrast, defining the Habsburg variable by Habsburg affiliation at any particular point in time would mean that part of the “control group” had also received a “Habsburg treatment” at some point in history. For example, defining the Habsburg treatment by its borders just before its demise in 1918 would mean that several regions that had been part of Habsburg until 1908 would constitute a substantial part of the control group in the border sample. However, as one robustness analysis below, we show that results are robust in a specification that restricts the analysis to locations that were part of the Habsburg Empire in 1900 (and their control locations) while dropping all locations from the analysis that had been part of the Habsburg Empire at some time but were no longer in 1900.

The specification of our basic model assumes that the Habsburg treatment effect is independent of the duration of treatment. To test for the validity of this assumption, we will also estimate models that allow the Habsburg effect to differ by length of a community’s affiliation with the Habsburg Empire:

$$(3) \quad y_{ilc} = \alpha_c + \beta_1 H_{ilc} + \beta_2 D_{ilc} + X'_{ilc} \gamma + \varepsilon_{ilc} \quad \text{if } i \in \text{border region}$$

where D_{ilc} is the duration of the Habsburg affiliation of the community in which individual i lives. By centering duration at 100 years of affiliation, the main Habsburg dummy H measures

the effect of having been part of the Habsburg Empire for 100 years and D measures whether a longer or shorter duration than 100 years differentially affects the outcomes.

To ensure even better comparability of the treatment and control groups on the two sides of the former Habsburg border, rather than literally including any observation within 200 km from the Habsburg border in the sample, we implement the border sample using a geographic matching algorithm. Individuals are included in the border sample only if there is a match in the dataset within 200 km on the other side of the Habsburg border. That is, observations enter the sample only if reasonably close-by observations exist on the other side of the border. This means that the bandwidth is effectively much smaller than 200 km, because there is usually no community placed directly at the border. It also means that the effective bandwidth varies across regions depending on whether a match exists on the other side of the border or not. This way, our results are not driven by observations that cannot be matched with similar close-by observations. The border sample is illustrated in Figure 1, where the black dots depict the locations of the LiTS observations in the band of 200 km around the former Habsburg border.²⁷

Our empirical identification is designed to identify a specific mechanism through which the history of the Habsburg Empire affects outcomes today. Of the four main mechanisms discussed in the literature (see Section II.A above) – formal institutions, geography, human capital, and cultural norms – we aim to shut down the first three ones. By identifying within regions that have been part of one country for more than 90 years,²⁸ we exclude the possibility that Habsburg history matters through differences in formal institutions like written laws today. By focusing on a narrow band of observations, we also exclude the channel that history matters because of geographical differences (which we further test below). Finally, having been part of the same country for three generations since 1918 means that formal education systems have been the same, and we control for differences in educational attainment at the individual level. This setup allows us to focus on the effect of historical affiliation with the Habsburg Empire on observed

²⁷ Note that given the definition of the Habsburg variable by whether the individual lives in a location that has *ever* been part of the Habsburg Empire, the relevant Habsburg “border” actually never existed in the specific shape, but is defined by the enveloping shape of Habsburg borders at any time in history. Consequently, once the Habsburg status of each specific location is properly coded, our matching algorithm approach also has the advantage of not depending on an exact drawing of the enveloping Habsburg border (see Ingraio, Samardžić, and Pešalj 2011 for an example for border drawings in sparsely inhabited areas that are disputed among historians).

²⁸ While this is true for all countries in our five-country sample, not all of them existed in their current borders for the past 90 years. The Ukraine belonged to the USSR and Serbia and Montenegro to (the Kingdom and later the Republic of) Yugoslavia.

cultural norms today, including aspects of the latter that may be intertwined with the functioning of formal institutions or with educational content.

Our border identification strategy has some similarity with a geographic regression discontinuity design in that it exploits a discontinuous geographic variation (Lee and Lemieux 2010), but it also has important differences. First, our historical setting of a long-gone political border contrasts with modern-day examples of a geographic regression discontinuity design where the immediate (short-run) impact of the introduction of some policy in one geographic area is contrasted with a control on the other side of a border. For example, rather than being orthogonal to other observable factors, we do actually *expect* the Habsburg treatment to have affected other covariates in the model, as it has been affecting trust and corruption in local public services for much more than a century now. What is relevant is that the Habsburg treatment can historically be viewed as “exogenous”, and we will test this in a battery of falsification tests below. Second, the LiTS sampling framework drew 50 municipalities per country across the whole country and did not zoom in on the Habsburg border. As a consequence, there are too few observations located directly at the border to neatly identify a jump at the discontinuity.

Third, and most importantly, in contrast to the basic assumption of a regression discontinuity design, there are a number of reasons to expect some sort of diffusion to and interdependence between neighboring towns across the former Habsburg border. Such reasons include migration and marriage between neighboring towns; local spillovers whereby inhabitants of an Ottoman town just across the border observe that well-functioning public services may have positive consequences, so that they may imitate them to some extent; “frontier” effects in that authorities may behave differently if located close to the enemy, such as when a Habsburg local authority may behave more authoritarian if the Ottomans are next door; and political competition across neighboring locations. All of these effects would mean that the Habsburg status of one town may affect outcomes in towns that are located directly on the other side of the border. Note that any of such diffusion or interaction effects work against our identification strategy finding a significant difference between Habsburg and non-Habsburg locations in the border sample.

Despite these differences from a classical regression discontinuity design, for comparison and to ensure that our results are not driven by peculiarities of our matching algorithm, Appendix Table A.1 reports results for standard regression-discontinuity type specifications. These specifications define the sample in terms of GIS-computed distance to the former Habsburg

border, rather than by the matching algorithm used in our main specification. Within the discontinuity sample of +/- 200 km, they also probe different specifications of controlling for distance to border. We employ, in alternative specifications, a linear distance-to-border term, a quadratic term in distance to border, and distance to border interacted with an indicator for the respective side of the border. Results are very similar to those reported in our main specification.

In Section VI below, we will test the validity of the border identification model in several falsification tests. Amongst others, we spuriously move the border by 100 km to either side, to ensure that the estimated effect does not simply capture linear effects of the outward expansion of the Habsburg Empire. We test for geographical differences of observations on either side of the border, to ensure that the Habsburg border does not coincide with obvious geographical barriers. We also test whether observations on either side of the border differ in dimensions that existed before the expansion of the Habsburg Empire, such as medieval city sizes and the location of medieval trade routes and diocesan cities. Finally, we contrast the results on trust in public services with interpersonal trust. All specification tests corroborate the validity of the border specification with respect to trust and corruption in local public services.

Columns (7)-(10) of Table 3 compare the Habsburg and the non-Habsburg part of the border sample in terms of their background variables. The fact that there are hardly any differences confirms that the border specification focuses the analysis on a treatment and control group that are very similar apart from their difference in Habsburg treatment status. The only differences that seem to come out are a higher share of respondents from an ethnic minority on the Habsburg side and the somewhat higher level of education on the Habsburg side. The former is consistent with the inclusive nature of the Habsburg rule, the latter with their stronger education efforts. Note also that the Muslim share is in fact higher on the *Habsburg* side. In our regressions, we control for these and other variables to account for these limited differences on the two sides of the border.

The measures of trust and corruption are categorical variables indicating the strength and degree of ubiquity of these conditions. To account for the ordered nature of the outcome variables, we estimate ordered logit models, which have the following form:

$$\begin{aligned}
& \text{logit}(p_1) = \log \frac{p_1}{1-p_1} = \alpha_1 + \beta' X \\
& \text{logit}(p_1 + p_2) = \log \frac{p_1 + p_2}{1-p_1-p_2} = \alpha_2 + \beta' X \\
& \vdots \\
& \text{logit}(p_1 + p_2 + \dots + p_k) = \log \frac{p_1 + p_2 + \dots + p_k}{1-p_1-p_2-\dots-p_k} = \alpha_k + \beta' X \\
& \text{with } p_1 + p_2 + \dots + p_k + p_{k+1} = 1
\end{aligned}
\tag{4}$$

where X here contains both the indicator for affiliation with the Habsburg Empire and the set of control variables. In this proportional-odds model, the odds ratio of the event is independent of the category j . The odds ratio is assumed to be constant for all categories.

Alternatively, we estimated a generalized ordered logit model which allows for non-proportional odds (Williams 2006; see also Maddala 1983; Long 1997). Results are very similar. Another alternative is to ignore the categorical nature of the outcome variable and perform ordinary least squares (OLS) regressions (e.g., Blanchflower and Oswald 2004). Again, results are qualitatively similar.

V. Results

This section reports our main results on the effect of historical affiliation with the Habsburg Empire on current people's trust in two essential public services with which citizens interact – courts and police – and on real consequences for corruption in the citizen-state interactions.

A. Main Results

In Table 4, we start with the full sample of 17 countries that were formerly part of the Habsburg Empire or neighboring countries thereof. All models control, at the individual level, for age, gender, native language, ethnic minority status, and six categories of religious affiliation. The results show that living in a location that used to be part of the Habsburg Empire is significantly positively associated with higher levels of trust in courts and trust in the police. These higher levels of trust also show up in the actual interactions between citizens and the two types of public authorities, in terms of significantly lower incidences of unofficial payments when interacting with the courts and with the road police. To account for the fact that such assessments may depend on whether respondents had contact with the specific public service at

all, the latter specifications control for whether respondents used the service in the previous twelve months. Respondents with contacts report significantly higher levels of corruption.

Because results in the 17-country sample might be partly driven by cross-country differences in trust and corruption, we restrict the analysis further to more readily comparable comparison groups. In Table 5, we use the border sample of respondents living within 200 km of the former Habsburg border in those five present-day countries that were partly Habsburg. This reduced sample contains about 3,600 observations, but the clustering in 180 locations means a substantial reduction in statistical power. The models include country fixed effects, so identification comes from within-country variation close to the former Habsburg border. The results show that living in a former Habsburg location has a significant positive effect on the reported level of trust in courts, with a point estimate somewhat larger than in the 17-country sample. The point estimates on the other three variables are smaller than in the 17-country sample, indicating that the previous results may be biased upwards by unobserved cross-country differences. Still, also in this specification, Habsburg affiliation has a significant negative effect on the corruption of courts and road police, and the positive effect on trust in police reaches statistical significance at the 12 percent level. Thus, while statistical precision is somewhat lower in the more demanding border sample specification, the results indicate that affiliation with the Habsburg Empire had a significant effect on higher trust and lower corruption in local public services.

Table 6 adds further control variables at the individual and household level to the model. While the control variables in Table 5 are arguably exogenous to the Habsburg treatment, those additionally added in Table 6 are potentially affected by past Habsburg affiliation. In principle, affiliation with the Habsburg Empire might have affected levels of income and wealth as well as education, urbanization, and fertility, so these variables might constitute channels through which the Habsburg effect comes about. However, although the additional control variables increase the variance in trust and corruption accounted for by the models, results on the effect of having been part of the Habsburg Empire are hardly affected. This suggests that property, education, urbanization, and fertility are unlikely to be major channels of the Habsburg effect. In the remainder of the paper, we stick with this longer set of control variables.

Going beyond statistical significance, we can discuss the size of the effects estimated by the ordered logit models by referring to marginal effects. All dependent variables are measured in five categories, with answers on the trust variables ranging from “complete distrust” to

“complete trust” and answers on the bribe variables ranging from “never” to “always.” As the detailed results reported in Appendix Table A.2 show, when holding the other variables constant at their means, having been part of the Habsburg Empire increases the probability of moving to a higher category of trust in courts by 2.2 percentage point, on average across the five categories. The average of the absolute value of the percentage change across categories is 1.2 percentage points for trust in the police, 2.3 percentage points for bribes to courts, and 2.9 percentage points for bribes to the road police. Viewed relative to the average share of 20 percent in each of the five categories, Habsburg affiliation thus moves the trust and corruption categories by 6-15 percent on average. This is substantially larger than the average absolute marginal effects of most other variables such as gender, age, being employed, speaking the native language, belonging to an ethnic minority, living in an urban or metropolitan area, and household size, albeit smaller than the average absolute marginal effects of some education levels and of a few of the religious comparisons. Trust and corruption in courts and police are thus affected by former Habsburg affiliation in a quantitatively important way.

Appendix Table A.3 shows the marginal effects of Habsburg affiliation category by category. For instance, affiliation with the Habsburg Empire reduces the probability of reporting complete distrust in courts by 4.6 percentage points and increases the probability of reporting some trust by 3.3 percentage points. The categories of complete distrust on the one hand and some trust and complete trust on the other hand are also the most affected ones in the case of trust in the police. By contrast, the main effect of Habsburg on corruption of courts and road police appears in raising the category of never having to bribe by 5.9 and 7.3 percentage points, respectively, with most of the reduced incidence appearing in the categories of seldom and sometimes.

B. Basic Robustness Checks

Several basic tests confirm the robustness of our main results. First, to check whether results depend on any particular country in the border sample, we run five separate regressions where we drop one country at a time. The reported results are robust in this analysis.²⁹

Second, OLS regressions ignoring the ordered nature of the trust and corruption outcomes also yield the same qualitative results.

²⁹ This robustness test also ensures that results are not sensitive to the Hungarian part of the Habsburg Empire, which in our five-country sample was active in one country (Romania) only. Detailed results on the different robustness checks are available from the authors upon request.

Third, choice of the width of the band around the border is subject to a trade-off between geographical proximity and sample size. We thus also experimented with bandwidths even narrower than the 200 km (124 miles) of the main analysis. Results are very similar – in terms of size and statistical significance – when the sample is restricted to individuals within 150 km (93 miles) of the Habsburg border. When reducing the bandwidth even further, statistical precision naturally tends to be reduced due to small sample sizes, but point estimates often get larger. For example, when estimating the model of Table 5 in a 100 km (62 miles) border sample, the point estimates on trust in courts, trust in police, and bribes to courts increase (in absolute terms) to .279, .228, and -.577, respectively (significant at the 6, 10, and 1 percent level), whereas the coefficient on bribes to road police is reduced to -.231 and loses statistical significance.

VI. Falsification Tests

This section provides deeper analyses of whether the results so far can be interpreted as a genuine effect of the Habsburg Empire on trust and corruption in local public services. They are devised to test whether we can interpret our border sample analysis as a “historical experiment” where Habsburg affiliation can be viewed as “randomly assigned.” They aim to rule out that the identified effect reflects other things such as selective expansion of the Habsburg Empire, in which case the results may reflect pre-existing differences. The Appendix provides narrative background that as a general pattern, in our border sample the Habsburg border separated quite homogeneous areas, so that the Habsburgs did not just take over those areas where people were already more trusting. To test this empirically, this section provides a number of falsification tests of our border specification, several of which are derived from regression discontinuity analysis (Imbens and Lemieux 2008; Lee and Lemieux 2010). We check whether there are treatment effects when spuriously moving the Habsburg border inwards or outwards; whether the Habsburg border is associated with differences in geographical characteristics or indicators pre-dating the Habsburg Empire; and whether Habsburg effects emerge in interpersonal interactions.

A. Pseudo Borders: Moving the “Treatment” Inward or Outward

A potential concern with the findings so far is that, rather than capturing a genuine Habsburg effect, they might only reflect continuous differences in the West-East dimension or in remoteness from Austria. A first falsification test is therefore to check whether a similar “effect”

is found when spuriously moving the border inward or outward. There should not be an effect when comparing locations on either side of these meaningless borders.

To keep comparability to the previous analyses, we stick with the five-country border sample and implement the falsification test as follows. In a first exercise (Table 7, first row), estimation is restricted to the Habsburg municipalities in the 200 km band of the true Habsburg border and “treatment” is defined as being “to the left” of a pseudo border that is moved 100 km inwards relative to the true Habsburg border. In the second exercise (second row), estimation is restricted to the non-Habsburg municipalities in the 200 km band of the true Habsburg border and “treatment” is defined as being “to the left” of a pseudo border that is moved 100 km outwards relative to the true Habsburg border. By effectively restricting the analysis to two 100 km bands, this analysis is directly comparable to the results of the 100 km border sample just discussed.³⁰

Results show no statistically significant “effect” of these pseudo borders whatsoever. In fact, the majority of estimates points in the “wrong” direction, with lower trust and higher corruption on the apparent “Habsburg” side of the analysis. These findings support the interpretation that our results capture a genuine Habsburg effect and not just a West-East difference.

B. Geographical Comparison of Habsburg and Non-Habsburg Areas

A second potential concern with the analysis so far is that during its expansion, the Habsburg dynasty might have chosen to expand only into certain types of geographical regions and, for example, stop before more mountainous regions. We therefore analyze possible jumps in the value of this geographical measure at the Habsburg border. We restrict this robustness check to this geographic characteristic because individual and household characteristics might potentially vary as a result of the Habsburg Empire, whereas this geographic feature is arguably exogenous.

In column (1) of Table 8, we regress the altitude of the municipalities in the border sample on a Habsburg indicator. The coefficient on the Habsburg indicator is 37.2 meters, with a t -value of 0.91. Habsburg municipalities thus do not systematically differ in altitude compared to non-Habsburg municipalities. This finding dilutes potential concerns that the Habsburgs might have fought their way through some valley and were stopped at a mountain, or, conversely, that they systematically chose mountainous locations in proximity to their neighbors – either of which might potentially be correlated with attitudes towards trust and corruption.

³⁰ Note that moving the whole band by 200 km would move the pseudo border outside our five-country sample.

C. Comparison of Pre-Existing Factors between Habsburg and Non-Habsburg Areas

Another potential concern is that the results found after the fall of the Habsburg Empire only perpetuate pre-existing differences before the Habsburg Empire came into being (see also the Appendix for narrative background that dilutes this concern). In principle, the Habsburg Empire might have expanded into areas that were distinct from areas outside the (new) Habsburg border in dimensions related to our outcomes. For instance, the expansion of the Habsburg Empire might have stopped short of areas that were less economically developed and might have been harder to develop. Similarly, areas outside the Habsburg Empire might have differed in their values, beliefs, and levels of trust already before the Habsburg Empire came into being.

To address these issues, we collected a series of variables capturing economic development, exposition to outsiders, and cultural features that pre-date the expansion of the Habsburg Empire. We then test whether there are significant differences in the variables between municipalities on both sides of the Habsburg border. In contrast to the previous robustness check that uses a time-constant geographic characteristic, this check uses indicators that might vary over time.

Economic historians often use urban population as a proxy for pre-industrial economic prosperity because cities could only be supported in areas with high agricultural productivity, advanced economic specialization, and developed transport systems (Bairoch 1988; Acemoglu, Johnson, and Robinson 2002). We use data on urban population from Bairoch, Batou, and Chèvre (1988) to construct a measure of urban population in cities in our sample of Eastern European countries. We use city size in 1400, well before the Eastern expansion of the Habsburg Empire, as an indicator of economic development. Cities inside and outside the Habsburg borders do not differ systematically in population size. If anything, the sign suggests that cities on the Habsburg side used to be somewhat smaller (column (2) of Table 8). Similar results are obtained when using city size in the years 1000, 1200, 1300, and 1500, and when measuring affiliation with the Habsburg Empire in 1600 (not shown).

Our second measure uses major trade routes in 1450 as indicators of interaction with foreign traders. Exchange with foreign parties is likely to affect the trust levels of people. When regressing an indicator of major trading cities, derived from Magocsi (2002), on our Habsburg indicator, we do not find a statistically significant effect (column (3)).

Finally, cultural values in different parts of Eastern Europe might have been influenced by a strong presence of the Church. We use an indicator variable for whether a city was a diocesan

town in 1450, also from Magocsi (2002). Again, the location of diocesan towns does not vary significantly by later affiliation with the Habsburg Empire (column (4)).

This last set of robustness checks suggests that the Habsburg Empire did not systematically expand into certain areas to exploit pre-existing advantages in terms of economics, trust, and values. Together, the three sets of robustness checks suggest that the provided analyses identify a genuine effect of the former Habsburg border and do not pick up confounding effects.

D. Habsburg Affiliation and Interpersonal Trust and Interactions

Our main hypothesis is that the localized and well-respected administration of the Habsburg Empire had long-term effects on trust and corruption in local public services. The historical background suggests that the Habsburg Empire affected person-state interactions but interfered little with local cultures and interpersonal interactions (Section II.B). A further falsification test is therefore whether historical Habsburg affiliation affects standard measures of interpersonal trust and social capital.

In the same way that the LiTS surveyed trust in courts and police, it also surveyed trust in other people and private entities. As shown in column (1) of Table 9, there is no significant effect of Habsburg affiliation on trust in other people, and the point estimate is actually negative. Similarly, column (2) shows that there is no Habsburg effect on trust in trade unions, a prominent example of a private entity with which many people interact that is surveyed by the LiTS.

Another measure of interpersonal interaction is the extent of membership in organizations, which is commonly used as a measure of social capital (see Glaeser, Laibson, and Sacerdote 2002). LiTS respondents are asked separately about membership in political parties and in civic organizations. Habsburg affiliation does not have a significant effect on either of these measures (columns (3) and (4)).

Overall, the Habsburg Empire does not seem to exert persistent effects on trust in people and private entities and on social capital and the engagement of citizens in their community. This corroborates the result that the Habsburg effect is one related to person-state interactions at the local level and not to any kind of interaction between individuals or between individuals and private entities, which has no relation to past and present experiences of the local bureaucracy. These results also provide additional evidence that the Habsburg expansion is unlikely to have been selective, as it affects only a specific aspect of trust and not trust in general.

VII. Additional Aspects of the Habsburg Effect

This section probes particular aspects of the Habsburg effect in greater depth. In particular, we analyze whether the effect of the Habsburg Empire differs by alternative neighboring empires, whether the duration and timing of Habsburg affiliation alters the effect, and whether Habsburg affiliation has an effect on trust in central public services.

A. *Alternative Neighboring Empires*

In the analyses so far, affiliation with the Habsburg Empire has been defined as a binary variable. The implicit assumption is that affiliation with the Habsburg Empire has a homogeneous effect, independent of the comparison group on the other side of the border and independent of the duration of affiliation. Here we investigate whether there is in fact heterogeneity in these dimensions.

Historical background. Three different empires ruled in the area that forms the control group for the Habsburg treatment: the Ottoman Empire, Russia, and Prussia. The Habsburg political structure and administration described in Section II.B above differed in important aspects from these Eastern neighbors (for comparative treatments, cf. Ingraio 1996; Subtelny 2007).

The Ottoman Empire pursued no comparable reforms (see Jones 1987, ch. 9, for an overview). It has been characterized as a “typical despotism” (Landes 1998, p. 398), a large entity with oppressive and exploiting rulers, politically and economically stagnant since the 17th century. Its subjects (*raya*, “protected flock” of the sultan) were deprived of political influence, and reforms in the late 19th century remained relatively weak. Hardly any secular education existed. Bribery was an accepted institutionalized phenomenon, perceived as normal, even expected by officials (Shaw 1976; Imber 1990; İnalçık 1996; Eldem 2006). Corruption constituted a systemic trait of the entire administrative system and had become an intrinsic and hardly avoidable part of local administration that alienated the subject peoples (Findley 1989).

Russia conceded some economic and social modifications – serfdom was abolished in 1861, compared to 1781 in Habsburg. But the autocratic monarchy gave no leeway to real parliamentary influence until the end of the monarchy in 1917 and did not allow for decentralized political development (Bartlett 2005; Subtelny 2007). Prussia, by contrast, while also known for a well-developed bureaucracy and education system, conceded less autonomy to its outer territories than Habsburg.

Results. In our border sample, nearly three quarters of the observations in the three control empires belonged to the Ottoman Empire, with Russia and Prussia dividing the remaining observations. Consequently, while individual sample sizes for the Russian and Prussian alternatives are too small to yield enough statistical power, one robustness specification is to restrict the control group to only those areas that belonged to the Ottoman Empire. Our results, reported in the upper panel of Table 10, are fully robust in this restricted sample.

If, alternatively, we use the full border sample and include a dummy variable controlling for those non-Habsburg areas that were *not* part of the Ottoman Empire, this variable is never significant. This indicates that we cannot detect a statistically significant difference in trust and corruption levels between Ottoman and non-Ottoman parts of the non-Habsburg control group (not shown). Consequently, we cannot reject the interpretation that our results are homogeneous with respect to neighboring empires, although they are most cleanly identified in the comparison to previously Ottoman areas.

Note also that substantial parts of our Habsburg treatment group at some stage in history had also been part of the Ottoman Empire. In fact, also among the observations in our border sample that had ever been part of the Habsburg Empire, nearly three quarters had also at some stage been part of the Ottoman Empire, making Ottoman influence something that hardly differentiates the Habsburg treatment and control groups in our analysis. To rule out that our results capture an “Ottoman effect” rather than a Habsburg effect, it is thus possible to restrict the whole analysis to a sample of areas that have all at some stage been part of the Ottoman Empire. One part of this sample has been part of the Habsburg Empire afterwards, the other part has not.³¹ As reported in the bottom panel of Table 10, our results come out even more clearly in this specification.

In an alternative specification to account for Ottoman influence (not shown), we include in the full sample an indicator for having ever been part of the Ottoman Empire as an additional “treatment” indicator alongside the indicator for ever having been part of the Habsburg Empire. The Ottoman indicator is never significant, and the Habsburg effect remains robust (although statistical significance becomes marginal for some outcomes). Our results thus reflect an influence of Habsburg affiliation (relative to lack of Habsburg affiliation), and no similar results are given for Ottoman affiliation (relative to lack of Ottoman affiliation).

³¹ While many regions changed from Ottoman to Habsburg rule over history, in our sample there are no incidents of changes in the opposite direction.

There results indicate that in our analyses, the Habsburg effect is mainly identified from a comparison with control locations that were part of the Ottoman Empire, but also that they capture a genuine Habsburg effect rather than an Ottoman effect.

B. Duration of Affiliation with the Habsburg Empire

A second dimension of possible heterogeneity relates to the duration of affiliation with the Habsburg Empire. The most straightforward way to test this, as described in equation (3) above, is to allow the Habsburg effect to vary with the duration of a community's affiliation with the Habsburg Empire. The upper panel of Table 11 shows both the binary main Habsburg variable and the duration variable, which is centered at 100 years (close to the average Habsburg affiliation in our border sample, see Table 3). The coefficient on the main Habsburg variable can thus be interpreted as the effect of being affiliated with the Habsburg Empire for 100 years. The duration coefficient measures whether differing duration of affiliation changes the impact on the outcomes. It turns out that, with one exception, the duration effect is statistically insignificant. In the exception – trust in courts – the duration estimate is actually negative, but this also loses significance once the Sanjak area (see below) is dropped from the analysis.

This simple duration analysis masks important specifics, though. In particular, there are three areas that had been part of the Habsburg Empire at some time in history but were no longer in 1900 (see Figure 1): Silesia in Poland, which belonged to the Habsburg Empire from 1526-1742; the Kingdom of Serbia and the Banat of Craiova in Romania, 1718-1739; and West Galicia, which came to Habsburg in the Third Partition of Poland in 1795 but was lost again in 1809. While limited sample sizes prevent specific analyses in these areas, the specifications reported in the bottom panel of Table 11 drop the areas individually and jointly, to check that these historical affiliations are not solely driving our results. Qualitative results are robust in the sample that drops these three areas from the analysis, although the effect on trust in courts loses statistical significance. This last specification is effectively an analysis of the effect of having been part of the Habsburg Empire in 1900, rather than the definition used in our main analysis of having ever been part of the Habsburg Empire.

Beyond these specific areas that capture variation in the termination of affiliation with the Habsburg Empire, there is basically no within-country variation within our border sample in the start and thus duration of affiliation with the Habsburg Empire. Basically all remaining locations

in the border sample came to the Habsburg Empire during the 18th century. The one exception is the Sanjak, an area on both sides of the border between Serbia and Montenegro where Habsburg troops were stationed nearly to the end (1908), but only since 1878. As the last row of Table 11 shows, the trust results lose significance in the sample without the Sanjak, whereas the corruption results are robust.

Given this limited and specific variation in duration of Empire affiliation, the analyses of duration effects in our application are limited and do not allow strong conclusions on the validity of different hypotheses of how changes in trust and corruption come about and persist. However, the presented results do not provide evidence in favor of the hypothesis that the strength of the Habsburg effect *increases* with the duration of affiliation with the Habsburg Empire. Rather, they are consistent with an interpretation that there is an effect of ever having been exposed to Habsburg rule, rather than of the length of exposure (thereby also supporting our main specification that defines Habsburg affiliation as having ever been part of the Habsburg Empire). This finding contrasts with descriptive evidence in Grosjean (2011) that only history of several centuries of common empire exerts cultural effects. The absence of a duration effect is consistent with theoretical models such as Guiso, Sapienza, and Zingales (2008b) in which a long-term equilibrium can be tipped by short-term exposure.

C. Trust in Central Public Services

An open question is whether Habsburg raised individuals' trust in the state only at the local level or more generally also at the central level. It seems more likely that trust in the state survived for local public services, where the conduct and execution could have remained different despite the same formal legal rules. The courts and the police may well function differently in different parts of the country. By contrast, at the level of central public institutions, there is only one parliament, one president, etc., at least for the within-country variation exploited in our border sample. However, differential trust in central state institutions may still have survived even though centralized state institutions have been the same for former Habsburg and non-Habsburg areas for a long time, for example because individuals may at least partly extrapolate their local experiences with the state to the central level, because individuals on the Habsburg side may perceive the same central state agencies differently, or because of actual local differences in the impact of policies implemented by central authorities.

Table 12 reports results on the effect of Habsburg affiliation on the levels of trust in a number of central public services: political parties, the parliament, the government, the presidency, and the armed forces. While all coefficients are positive, none of them reaches statistical significance at conventional levels, and most point estimates are quite low. One might be tempted to read these results as another falsification test for our results on local public services, which in total are much stronger. However, given that all point estimates for central public services are positive and quantitatively relevant effects cannot always be rejected, another plausible reading is that individuals on the Habsburg side indeed extrapolated at least part of their positive experience with local public services to trust in state institutions more generally.

VIII. Complementary Evidence from a Business Survey

Results so far refer to the experience of *individuals* with local public services. In terms of economic relevance, the trustworthiness and impartiality of local public services in *business* disputes may have specific relevance. In particular when it comes to trust in the court system and to sizable bribes, the experience of firms may be of particular interest. As a final addition, we therefore complement the evidence on trust of individuals in local public services by looking at the experience that firms have.

Suitable data come from the Business Environment and Enterprise Performance Survey (BEEPS), which was conducted by the EBRD and the World Bank in Eastern European countries in 2005. The purpose of the survey was to better understand constraints that hinder the development of businesses. Businesses were asked to answer in a fashion reflecting “only [their] perception and experience of doing business in [their] country.” The BEEPS sampling aimed at representativeness in manufacturing and services. Similar to the LiTS, also in the BEEPS we know the location of respondents. From this, we geo-coded municipality data to compute distances and again restrict the sample to respondents in municipalities within 200 km of each other on either side of the former Habsburg border in the five-country sample. This yields a border sample of roughly 1,000 firms.

In surveying firms’ business experience, the BEEPS asks questions about the role of the court system in resolving business disputes. Additional information allows us to control for eight sector dummies, the year when the firm began operations in the country, and indicators for firm size and ownership structure, as well as country dummies. As shown in Table 13, firms on the

Habsburg side of the former border are significantly more likely to consider courts as being fair and impartial. There are also positive point estimates, albeit short of statistical significance at conventional levels, on considering courts as being honest and uncorrupted and on firms' confidence that the legal system will uphold their contract and property rights in business disputes. Possibly, the wording of the latter question ("legal system") may draw firms to think also of the functioning of the central legal system as opposed to the local courts. Comparable questions on trust and corruption of the police are not available in the business survey.

The BEEPS also surveys membership in business associations or chambers of commerce. We do not find a significant Habsburg effect on this, with the point estimate even negative (not shown). Again, the Habsburg effect is not visible in inter-firm interactions but only in firm-state interactions in terms of firms' perception of the courts being fairer and more impartial.

Results based on the business survey thus confirm the household survey results: Firms on the Habsburg side of the long-gone border within the same country have higher trust in the courts.

IX. Conclusions

Our results show that even nearly a century after its demise, the Habsburg Empire lives on in the attitudes towards and interactions with local state institutions of the people living within its former borders. Comparing individuals living on either side of the long-gone Habsburg border, we find that respondents in a current household survey who live on former Habsburg territory have higher levels of trust in courts and police. They are also less likely to pay bribes for these local public services, demonstrating that the institutional heritage influences not only preferences and unilateral decisions but also bilateral bargaining situations in citizen-state interactions.

We establish this result on the basis of a border specification that is identified from a geographic discontinuity. We exploit the fact that the Habsburg border does not always coincide with current national borders. Whereas many current states in Eastern Europe were historically inside or outside the Habsburg Empire in their entirety, five modern-day countries comprise both Habsburg and non-Habsburg areas. Using this five-country sample, we identify the Habsburg effect by comparing communities in close geographic proximity within a range of 200 km on either side of the former Habsburg border. This approach has the advantage that respondents have been sharing a common set of formal institutions for a long time now because they live within the same current national borders, but differ in their historical exposure to the formal

institutions of the Habsburg Empire. We additionally control for observed variations in education, religion, language, and wealth at the individual level. Given this setting, we rule out most aspects of other channels of historical influence discussed in the literature, such as geography, formal institutions in the form of written laws, and human capital in the form of educational degrees. In the case of the Habsburg Empire, history matters for current attitudes and behavior not because formal institutions persisted, but because individual cultural norms with regard to local public services survived. A series of falsification tests corroborate this interpretation.

Results on effects of the duration of exposure to the Habsburg Empire and on the specific mechanisms through which the Habsburg effect has survived are necessarily more speculative. The limited within-country variation in duration of Habsburg affiliation exploited in our border analysis prevents strong conclusions, but at least does not provide supportive evidence that duration of exposure matters, suggesting that even short interventions can have long-persistent effects. Since the demise of the Habsburg Empire, substantial waves of migration and displacement accompanied the institutional disruptions that the successor states of the Habsburg Empire have experienced. This suggests that the cultural norms of behavior are unlikely to have survived solely by intergenerational transmission within families, but rather also through channels such as the persistent nature of continuous reciprocal interactions in local communities, the content of knowledge and behavioral patterns conveyed in schools, or the quality of human capital of citizens, judges, and policemen. Additional analyses that investigate in greater detail the mechanisms through which the empire effect comes about and prevails – presumably requiring different areas of application – constitute interesting directions for future research. At a general level, our application of the Habsburg Empire shows that past formal institutions can leave a legacy through cultural norms even after generations of common statehood.

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Appendix: Post-1918 Emergence of Countries with Habsburg and Non-Habsburg Parts

Our identification in the border sample notably derives from the fact that there are five modern-day countries that combine territories that were and were not part of the Habsburg Empire. It is important to analyze how this formation came about so that countries ended up combining the different parts. In particular, understanding the background of this helps ease concerns that the within-country separation by the former Habsburg border might be correlated with ethnic or cultural differences that date back even further in history.

After World War I, the victorious allies pressed for the right of self-determination of peoples, in particular within the defeated Habsburg Monarchy. With that premise, many borders were newly defined to merge peoples with a common language. Consequently, as a general rule (which by necessity hides specific historical exceptions), the combination of Habsburg and non-Habsburg parts in one country meant that areas that had been reasonably natural units before were now joined together (again). In Romania and the Ukraine, areas that already previously tended to be inhabited by ethnologically common people with a common language were joined together. The same is true for the Galician parts of Poland. In Serbia, a national assembly of the former Habsburg part deliberately voted to join the non-Habsburg part after World War I. In Montenegro, the former Habsburg and non-Habsburg parts formed a natural geographical unit for a long time. The one clear exception to this general pattern is the Silesian part of Poland, where a different area was integrated into an otherwise quite foreign country.³²

The relative internal homogeneity of the forming countries even before they came into being after World War I can also document itself in the fact that their political borders were already visible in the economy well before the new countries were formed, as shown by Schulze and Wolf (2009, 2011). Their studies document, furthermore, that the fact that the fault lines along which the Habsburg Empire was to break up are evident long before its demise cannot be explained by factors such as administrative barriers, geography, or infrastructural differences.

The following provides a rough overview of the cases of the five countries which, by focusing on the general patterns, of course does not do justice to all specifics.

³² In line with this exception status, the analyses in Section VII.B reveal that results get stronger when disregarding Silesia.

Poland: Polish population had been living in the Polish parts of Galicia before they came to Habsburg. Galicia's main part joined the Habsburg Empire in the First Partition of Poland in 1772 and stayed until 1918. North Galicia joined the Habsburg Empire in the Third Partition of Poland in 1795 and stayed only until 1809. That is, Galicia used to belong to Poland, then came to Habsburg through power politics driven by Russia and Prussia, and then returned to Poland after World War I. The situation is quite different in Silesia. Silesia used to be under the rule of the Crown of Bohemia, which passed to Habsburg in 1526. After the Silesian Wars, it was incorporated by Prussia in 1742 and later remained part of the Weimar Republic. Only after World War II did it join Poland.

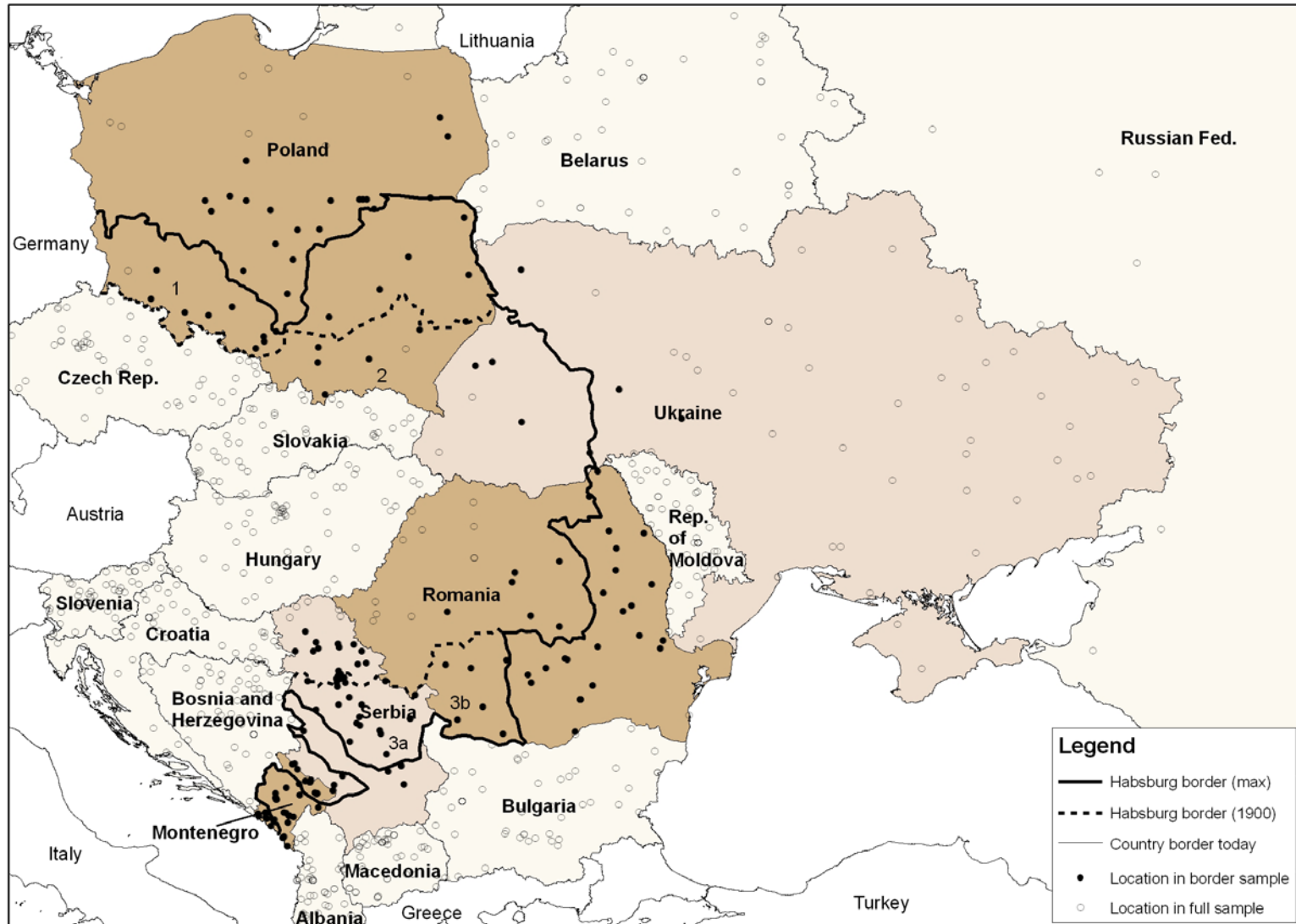
Ukraine: East Galicia and northern Bukovina used to be part of Habsburg, with strong Ukrainian populations. In connection with World War II, they became part of Soviet Ukraine.

Romania: Three historical regions and principalities form Romania: Wallachia, Moldova, and Transylvania. They all had large Romanian population, but whereas Transylvania used to be close to the Habsburgs and part of Hungary, the other two were ruled indirectly by the Ottomans. Interestingly, already in 1600 the three were briefly united. In 1859, Moldova and Wallachia formed the modern Romanian state. Transylvania was passed to it after World War I as an area of Austria-Hungary that had a Romanian population.

Serbia: The Vojvodina, which had been part of Habsburg resp. Hungary, came to the Kingdom of the Serbs, Croats, and Slovenes after World War I. A quickly constituted national assembly actually voted in favor of joining the Serbian kingdom in 1918. After World War II, it remained an autonomous province in the Yugoslav Republic of Serbia. Other parts of Serbia that had been affiliated with Habsburg are the so-called "Kingdom of Serbia" south of the rivers Sava and Danube, ruled by the Habsburg crown from 1718-1739 and 1788-1791, and the Serbian part of the Sanjak, where Habsburg garrisons were stationed from 1878-1908.

Montenegro: The gulf and harbor of Kotor had offered access to the Adriatic Sea to the Serbian empire in the Middle Ages and later to the Republic of Venice, before it became part of the Habsburg crown land Dalmatia in 1797 (with the short Napoleonic interference of 1805-1815). Since 1918, the Bay of Kotor belonged to the Kingdom of the Serbs, Croats, and Slovenes (since 1929: Kingdom of Yugoslavia), first within the Zeta Banovina (an administrative body), after World War II as part of the Yugoslav Socialist Republic Montenegro. Another area formerly affiliated with Habsburg is the Montenegrin part of the Sanjak (1878-1908).

Figure 1: THE HABSBURG EMPIRE IN EASTERN EUROPE AND THE LiTS LOCATIONS



Habsburg border in maximum expansion, Habsburg border in 1900, borders of countries today, and location of the observations in the LiTS 2006 dataset contained in the border sample and in the 17-country sample. Former Habsburg territories that are no longer part of Habsburg in 1900: 1 Silesia (1526-1742); 2 West Galicia (1795-1809); 3a Kingdom of Serbia and 3b Banat of Craiova (1718-1739).

Table 1: SAMPLES AND HABSBERG STATUS

	17-country sample						Border sample					
	Habsburg		not Habsburg		Total		Habsburg		not Habsburg		Total	
	Obs.	%	Obs.	%	Obs.	%	Obs.	%	Obs.	%	Obs.	%
Albania	0	0.0	1,000	100.0	1,000	100.0						
Belarus	0	0.0	1,000	100.0	1,000	100.0						
Bosnia	1,000	100.0	0	0.0	1,000	100.0						
Bulgaria	0	0.0	1,000	100.0	1,000	100.0						
Croatia	1,000	100.0	0	0.0	1,000	100.0						
Czech Republic	1,000	100.0	0	0.0	1,000	100.0						
FYR Macedonia	0	0.0	1,000	100.0	1,000	100.0						
Hungary	1,000	100.0	0	0.0	1,000	100.0						
Moldova	0	0.0	1,000	100.0	1,000	100.0						
Montenegro	400	40.0	600	60.0	1,000	100.0	400	40.0	600	60.0	1,000	100.0
Poland	440	44.0	560	56.0	1,000	100.0	420	52.5	380	47.5	800	100.0
Romania	480	48.0	520	52.0	1,000	100.0	260	34.2	500	65.8	760	100.0
Russia	0	0.0	1,000	100.0	1,000	100.0						
Serbia	880	88.0	120	12.0	1,000	100.0	780	86.7	120	13.3	900	100.0
Slovakia	1,001	100.0	0	0.0	1,001	100.0						
Slovenia	1,001	100.0	0	0.0	1,001	100.0						
Ukraine	120	12.0	880	88.0	1,000	100.0	80	57.1	60	42.9	140	100.0
Total	8,322	49.0	8,680	51.0	17,002	100.0	1,940	53.9	1,660	46.1	3,600	100.0

17-country sample: 17 countries that were formerly part of the Habsburg Empire or neighboring countries thereof.

Border sample: respondents living within 200km of respondents on the other side of the former Habsburg border in those five countries that are partly Habsburg.

Data source: Life in Transition Survey (LiTS) 2006; see main text for details.

Table 2: TRUST AND CORRUPTION IN LOCAL PUBLIC SERVICES: HABSBERG VS. NON-HABSBERG

	Trust in courts						Trust in police					
	Habsburg		not Habsburg		Total		Habsburg		not Habsburg		Total	
	Obs.	%	Obs.	%	Obs.	%	Obs.	%	Obs.	%	Obs.	%
Complete distrust	2,149	26.1	2,789	32.4	4,938	29.3	1,398	17.0	2,224	25.8	3,622	21.5
Some distrust	1,636	19.9	1,510	17.5	3,146	18.7	1,223	14.9	1,363	15.8	2,586	15.4
Neither trust nor distrust	1,860	22.6	1,619	18.8	3,479	20.7	2,043	24.8	1,646	19.1	3,689	21.9
Some trust	1,715	20.9	1,678	19.5	3,393	20.2	2,447	29.7	2,241	26.0	4,688	27.8
Complete trust	371	4.5	503	5.8	874	5.2	820	10.0	827	9.6	1,647	9.8
Difficult to say	495	6.0	517	6.0	1,012	6.0	292	3.6	317	3.7	609	3.6
Total	8,226	100.0	8,616	100.0	16,842	100.0	8,223	100.0	8,618	100.0	16,841	100.0

	Bribes to courts						Bribes to road police					
	Habsburg		not Habsburg		Total		Habsburg		not Habsburg		Total	
	Obs.	%	Obs.	%	Obs.	%	Obs.	%	Obs.	%	Obs.	%
Never	6,368	77.8	5,685	66.1	12,053	71.8	5,665	69.0	4,879	56.7	10,544	62.7
Seldom	734	9.0	984	11.4	1,718	10.2	914	11.1	1,081	12.6	1,995	11.7
Sometimes	643	7.9	1,034	12.0	1,677	10.0	1,011	12.3	1,329	15.4	2,340	13.9
Usually	308	3.8	525	6.1	833	5.0	453	5.5	822	9.5	1,275	7.6
Always	137	1.7	376	4.4	513	3.0	165	2.0	502	5.8	667	4.0
Total	8,190	100.0	8,604	100.0	16,794	100.0	8,208	100.0	8,613	100.0	16,821	100.0

Trust: Answer to the question: "To what extent do you trust the following institutions?"

Corruption: Answer to the question: "In your opinion, how often is it necessary for people like you to have to make unofficial payments/gifts in these situations?"

Data source: Life in Transition Survey (LiTS) 2006; see main text for details.

Table 3: DESCRIPTIVE STATISTICS

	17-country sample				Border sample					
	Mean (1)	StdDev (2)	Min (3)	Max (4)	Both sides		Habsburg side		non-Habsburg side	
					Mean (5)	StdDev (6)	Mean (7)	StdDev (8)	Mean (9)	StdDev (10)
Part of Habsburg Empire	.49	.50	.00	1.00	.54	.50	1.00	.00	.00	.00
Duration of affiliation with Habsburg (in years)	126.06	176.20	.00	635.00	52.41	78.74	97.45	84.49	.00	.00
Individual-level variables										
Age of respondent	47.51	17.74	17.00	92.00	46.46	17.85	47.57	17.33	45.18	18.36
Male respondent	.43	.50	.00	1.00	.45	.50	.45	.50	.45	.50
Worked for income during last 12 months	.49	.50	.00	1.00	.45	.50	.45	.50	.44	.50
Native language	.91	.28	.00	1.00	.93	.26	.91	.29	.95	.22
Ethnic minority	.09	.28	.00	1.00	.09	.28	.12	.33	.05	.21
<i>Education (omitted category: no degree)</i>										
Compulsory schooling education	.20	.40	.00	1.00	.19	.39	.19	.39	.18	.39
Secondary education	.21	.41	.00	1.00	.20	.40	.17	.38	.24	.43
Professional, vocational school or training	.35	.48	.00	1.00	.38	.49	.40	.49	.36	.48
Higher professional degree (university, college)	.18	.38	.00	1.00	.16	.37	.17	.37	.16	.36
Post graduate degree	.01	.10	.00	1.00	.01	.10	.01	.11	.01	.10
<i>Religion (omitted category: atheist)</i>										
Buddhist	.001	.03	.00	1.00	.0008	.03	.001	.03	.0006	.02
Jewish	.002	.04	.00	1.00	.002	.04	.002	.04	.002	.04
Christian	.77	.42	.00	1.00	.91	.29	.89	.32	.93	.26
Muslim	.11	.31	.00	1.00	.05	.23	.08	.27	.03	.16
Other	.01	.12	.00	1.00	.006	.08	.007	.08	.005	.07
Household-level variables										
Household has a car	.48	.50	.00	1.00	.51	.50	.52	.50	.51	.50
Household has a secondary residence	.13	.34	.00	1.00	.16	.37	.15	.36	.17	.38
Household has a bank account	.44	.50	.00	1.00	.43	.50	.45	.50	.42	.49
Household has a credit/debit card	.37	.48	.00	1.00	.34	.48	.35	.48	.34	.47
Household has a mobile phone	.72	.45	.00	1.00	.75	.43	.74	.44	.76	.43
Household has a computer	.35	.48	.00	1.00	.39	.49	.37	.48	.40	.49
Household has access to internet at home	.22	.41	.00	1.00	.25	.43	.24	.43	.26	.44
Household size (equivalent scale)	1.87	.68	1.00	6.50	1.94	.69	1.97	.71	1.90	.67
Household number of children under 14	.36	.74	.00	7.00	.39	.78	.42	.82	.35	.74

17-country sample: 17 countries that were formerly part of the Habsburg Empire or neighboring countries thereof.

Border sample: respondents living within 200km of respondents on the other side of the former Habsburg border in those five countries that are partly Habsburg.

Data source: Life in Transition Survey (LiTS) 2006; see main text for details.

Table 4: TRUST AND CORRUPTION IN COURTS AND POLICE: 17-COUNTRY SAMPLE

	Trust in courts	Trust in police	Bribes to courts	Bribes to road police
	(1)	(2)	(3)	(4)
Part of Habsburg Empire	.141 (.067)**	.311 (.064)***	-.603 (.100)***	-.586 (.088)***
Age of respondent	-.003 (.001)**	.006 (.001)***	-.005 (.001)***	-.012 (.001)***
Male respondent	-.074 (.031)**	.003 (.031)	.014 (.040)	.087 (.038)**
Native language	.198 (.095)**	.217 (.091)**	-.201 (.136)	-.235 (.130)*
Ethnic minority	-.016 (.102)	-.028 (.091)	-.289 (.111)***	-.335 (.104)***
Controls for religious affiliation (6 categories)	yes	yes	yes	yes
Used service in last 12 months			1.069 (.087)***	1.203 (.066)***
No. of observations	15,830	16,232	16,794	16,821
No. of PSUs	850	850	850	850
Pseudo- R^2	.002	.005	.023	.045

Coefficients and standard errors from ordered logit (ologit) estimations.

17-country sample: 17 countries that were formerly part of the Habsburg Empire or neighboring countries thereof.

Dependent variable in columns (1) and (2) is answer to the question “To what extent do you trust the following institutions?” Column (1): The courts. Column (2): The police. Answer categories are: 1=Complete distrust; 2=Some distrust; 3=Neither trust nor distrust; 4=Some trust; 5=Complete trust. Category 6=Difficult to say set to missing in regressions.

Dependent variable in columns (3) and (4) is answer to the question “In your opinion, how often is it necessary for people like you to have to make unofficial payments/gifts in these situations?” Column (3): Interact with the courts. Column (4): Interact with the road police. Answer categories are: 1=Never; 2=Seldom; 3=Sometimes; 4=Usually; 5=Always.

Standard errors (clustered at the level of PSUs) in parentheses: * significance at ten, ** five, *** one percent.

Data source: Life in Transition Survey (LiTS) 2006; see main text for details.

Table 5: TRUST AND CORRUPTION IN COURTS AND POLICE: BORDER SAMPLE

	Trust in courts (1)	Trust in police (2)	Bribes to courts (3)	Bribes to road police (4)
Part of Habsburg Empire	.229 (.132)*	.169 (.109)	-.373 (.205)*	-.342 (.174)**
Age of respondent	-.00009 (.002)	.009 (.002)***	-.006 (.004)*	-.018 (.003)***
Male respondent	-.018 (.067)	-.035 (.066)	.026 (.097)	.275 (.080)***
Native language	-.240 (.188)	.004 (.152)	.030 (.311)	.116 (.274)
Ethnic minority	-.330 (.135)**	-.309 (.125)**	-.149 (.203)	-.178 (.171)
Controls for religious affiliation (6 categories)	yes	yes	yes	yes
Used service in last 12 months			.936 (.194)***	1.104 (.142)***
Country fixed effects	yes	yes	yes	yes
No. of observations	3,359	3,409	3,572	3,574
No. of PSUs	180	180	180	180
Pseudo- R^2	.019	.015	.038	.054

Coefficients and standard errors from ordered logit (ologit) estimations.

Border sample: respondents living within 200km of respondents on the other side of the former Habsburg border in those five countries that are partly Habsburg.

Dependent variable in columns (1) and (2) is answer to the question “To what extent do you trust the following institutions?” Column (1): The courts. Column (2): The police. Answer categories are: 1=Complete distrust; 2=Some distrust; 3=Neither trust nor distrust; 4=Some trust; 5=Complete trust. Category 6=Difficult to say set to missing in regressions.

Dependent variable in columns (3) and (4) is answer to the question “In your opinion, how often is it necessary for people like you to have to make unofficial payments/gifts in these situations?” Column (3): Interact with the courts. Column (4): Interact with the road police. Answer categories are: 1=Never; 2=Seldom; 3=Sometimes; 4=Usually; 5=Always.

Standard errors (clustered at the level of PSUs) in parentheses: * significance at ten, ** five, *** one percent.

Data source: Life in Transition Survey (LiTS) 2006; see main text for details.

Table 6: ADDITIONAL CONTROL VARIABLES

	Trust in courts	Trust in police	Bribes to courts	Bribes to road police
	(1)	(2)	(3)	(4)
Part of Habsburg Empire	.223 (.130)*	.128 (.107)	-.369 (.207)*	-.353 (.176)**
Age of respondent	-.002 (.003)	.008 (.003)***	-.006 (.004)	-.015 (.003)***
Male respondent	.005 (.071)	-.033 (.067)	.006 (.099)	.242 (.081)***
Native language	-.190 (.178)	.026 (.153)	.049 (.315)	.149 (.276)
Ethnic minority	-.323 (.138)**	-.322 (.125)**	-.156 (.203)	-.179 (.168)
Controls for religious affiliation (6 categories)	yes	yes	yes	yes
Worked for income during last 12 months	-.021 (.076)	.108 (.076)	.177 (.110)	.222 (.095)**
Controls for household property (7 variables)	yes	yes	yes	yes
Controls for education level (6 categories)	yes	yes	yes	yes
Urban area	-.105 (.128)	-.015 (.117)	.040 (.231)	.077 (.188)
Metropolitan area	-.110 (.158)	-.199 (.148)	.020 (.279)	.059 (.238)
Household size (equivalent scale)	.073 (.068)	.106 (.063)*	.097 (.084)	.145 (.072)**
Household number of children under 14	.016 (.047)	-.026 (.044)	-.068 (.066)	-.041 (.047)
Used service in last 12 months			.953 (.192)***	1.040 (.141)***
Country fixed effects	yes	yes	yes	yes
No. of observations	3,359	3,409	3,572	3,574
No. of PSUs	180	180	180	180
Pseudo- R^2	.023	.018	.041	.063

Coefficients and standard errors from ordered logit (ologit) estimations.

Border sample: respondents living within 200km of respondents on the other side of the former Habsburg border in those five countries that are partly Habsburg.

Dependent variable in columns (1) and (2) is answer to the question “To what extent do you trust the following institutions?” Column (1): The courts. Column (2): The police. Answer categories are: 1=Complete distrust; 2=Some distrust; 3=Neither trust nor distrust; 4=Some trust; 5=Complete trust. Category 6=Difficult to say set to missing in regressions.

Dependent variable in columns (3) and (4) is answer to the question “In your opinion, how often is it necessary for people like you to have to make unofficial payments/gifts in these situations?” Column (3): Interact with the courts. Column (4): Interact with the road police. Answer categories are: 1=Never; 2=Seldom; 3=Sometimes; 4=Usually; 5=Always.

Standard errors (clustered at the level of PSUs) in parentheses: * significance at ten, ** five, *** one percent.

Data source: Life in Transition Survey (LiTS) 2006; see main text for details.

Table 7: PSEUDO BORDERS: MOVING THE BORDER INWARDS AND OUTWARDS

	Trust in courts	Trust in police	Bribes to courts	Bribes to road police
	(1)	(2)	(3)	(4)
Moving border 100km inwards	.006 (.170)	-.004 (.136)	.212 (.286)	-.167 (.231)
Moving border 100km outwards	.076 (.231)	-.029 (.234)	.521 (.459)	.454 (.406)

All reported estimates are coefficients of the variable “Part of Habsburg Empire” in the model specification of Table 6.

Coefficients and standard errors from ordered logit (ologit) estimations.

Border sample: respondents living within 200km of respondents on the other side of the former Habsburg border in those five countries that are partly Habsburg.

In the first row, estimation is restricted to Habsburg locations (within the 200km band of the true Habsburg border) and treatment is defined as being “to the left” of a pseudo border that is moved 100km inwards relative to the true Habsburg border. In the second row, estimation is restricted to non-Habsburg locations (within the 200km band of the true Habsburg border) and treatment is defined as being “to the left” of a pseudo border that is moved 100km outwards relative to the true Habsburg border.

Standard errors (clustered at the level of PSUs) in parentheses: * significance at ten, ** five, *** one percent.

Data source: Life in Transition Survey (LiTS) 2006; see main text for details.

Table 8: GEOGRAPHIC AND PRE-EXISTING DIFFERENCES BETWEEN HABSBURG AND NON-HABSBURG

	Geography	Indicators of pre-Habsburg development		
	Altitude of LiTS municipality in 100 meters (1)	City size in 1400 in 1,000s (2)	Trade route in 1450 (dummy) (3)	Diocesan city in 1450 (dummy) (4)
Part of Habsburg Empire	.372 (.407)	-.435 (.904)	-.065 (.082)	-.051 (.071)
No. of observations	158	115	115	115
R^2	0.0053	.002	.006	.005

Coefficients and standard errors from ordinary least squares estimations.

Border sample: locations within 200km of locations on the other side of the former Habsburg border in those five countries that are partly Habsburg. In column (1), the unit of observation is a LiTS location. In columns (2)-(4), the unit of observation is a town in the Bairoch (1988) data.

Standard errors in parentheses: * significance at ten, ** five, *** one percent.

Data source: Life in Transition Survey (LiTS) 2006; Bairoch, Batou, and Chvre (1988); Magocsi (2002); see main text for details.

Table 9: INTERPERSONAL TRUST AND MEMBERSHIP IN ORGANIZATIONS

	Trust in other people	Trust in trade unions	Membership in political parties	Membership in civic organizations
	(1)	(2)	(3)	(4)
Part of Habsburg Empire	-.064 (.139)	.027 (.141)	.020 (.013)	.006 (.008)
Age of respondent	-.002 (.003)	-.003 (.003)	.0007 (.0003)**	-.0002 (.0002)
Male respondent	-.040 (.070)	-.071 (.072)	.031 (.008)***	.012 (.007)*
Native language	.184 (.138)	-.252 (.194)	-.0003 (.016)	.015 (.008)*
Ethnic minority	-.204 (.134)	-.107 (.139)	.002 (.015)	.025 (.015)*
Controls for religious affiliation (6 categories)	yes	yes	yes	yes
Worked for income during last 12 months	-.040 (.078)	-.088 (.085)	.034 (.010)***	.006 (.007)
Controls for household property (7 variables)	yes	yes	yes	yes
Controls for education level (6 categories)	yes	yes	yes	yes
Urban area	-.030 (.133)	-.256 (.135)*	-.003 (.012)	.002 (.009)
Metropolitan area	.070 (.158)	-.156 (.144)	-.054 (.010)***	-.010 (.011)
Household size (equivalent scale)	-.054 (.064)	.017 (.069)	.007 (.007)	.0008 (.005)
Household number of children under 14	.019 (.045)	-.025 (.046)	-.003 (.005)	-.002 (.004)
Country fixed effects	yes	yes	yes	yes
No. of observations	3,388	3,137	3,564	3,573
No. of PSUs	180	180	180	180
Pseudo- R^2	.008	.020	.161	.079

Columns (1) and (2): Coefficients and standard errors from ordered logit (ologit) estimations. Columns (3) and (4): marginal effects and standard errors from probit estimations.

Border sample: respondents living within 200km of respondents on the other side of the former Habsburg border in those five countries that are partly Habsburg.

Dependent variable in columns (1) and (2) is answer to the question “To what extent do you trust the following [...]?” Column (1): Other people. Column (2): Trade unions. Answer categories are: 1=Complete distrust; 2=Some distrust; 3=Neither trust nor distrust; 4=Some trust; 5=Complete trust. Category 6=Difficult to say set to missing in regressions.

Dependent variable in column (3) is answer to the question “Are you a member of a political party?”.

Dependent variable in column (4) is answer to the question “Are you a member of (other) civic/voluntary organizations?”.

Standard errors (clustered at the level of PSUs) in parentheses: * significance at ten, ** five, *** one percent.

Data source: Life in Transition Survey (LiTS) 2006; see main text for details.

Table 10: HABSBURG VS. OTTOMAN EMPIRES

	Trust in courts	Trust in police	Bribes to courts	Bribes to road police
	(1)	(2)	(3)	(4)
Sample restricted to Ottoman Empire in the control group				
Part of Habsburg Empire	.373 (.166)**	.250 (.133)*	-.472 (.222)**	-.403 (.210)*
No. of observations	2,963	2,999	3,152	3,154
No. of PSUs	159	159	159	159
Pseudo-R ²	.025	.018	.039	.059
Sample restricted to locations that were ever part of the Ottoman Empire				
Part of Habsburg Empire	.417 (.168)**	.286 (.138)**	-.456 (.252)*	-.356 (.227)
No. of observations	2,393	2,424	2,539	2,539
No. of PSUs	128	128	128	128
Pseudo-R ²	.034	.022	.021	.049

Coefficients and standard errors from ordered logit (ologit) estimations in the model specification of Table 6.

Border sample: respondents living within 200km of respondents on the other side of the former Habsburg border in those five countries that are partly Habsburg.

Standard errors (clustered at the level of PSUs) in parentheses: * significance at ten, ** five, *** one percent.

Data source: Life in Transition Survey (LiTS) 2006; see main text for details.

Table 11: DURATION OF HABSBERG AFFILIATION

	Trust in courts (1)	Trust in police (2)	Bribes to courts (3)	Bribes to road police (4)
Control for duration of affiliation with the Habsburg Empire				
Part of Habsburg Empire	.232 (.126)*	.130 (.107)	-.370 (.207)*	-.352 (.175)**
Duration of affiliation with Habsburg (in 100 years)	-.197 (.093)**	-.039 (.074)	-.056 (.156)	-.095 (.134)
No. of observations	3,359	3,409	3,572	3,574
No. of PSUs	180	180	180	180
Pseudo- R^2	.024	.018	.041	.063
Dropping areas (and their correspondents on the other side of the border) that were no longer part of Habsburg in 1900				
<i>Dropping Silesia (1526-1742)</i>				
Part of Habsburg Empire	.268 (.137)*	.193 (.113)*	-.355 (.219)	-.383 (.189)**
No. of observations	3,037	3,088	3,232	3,234
No. of PSUs	163	163	163	163
Pseudo- R^2	.025	.021	.036	.062
<i>Dropping Kingdom of Serbia and Banat of Craiova (1718-1739)</i>				
Part of Habsburg Empire	.139 (.139)	.131 (.114)	-.374 (.220)*	-.354 (.190)*
No. of observations	2,789	2,826	2,980	2,982
No. of PSUs	150	150	150	150
Pseudo- R^2	.020	.018	.049	.067
<i>Dropping West Galicia (1795-1809)</i>				
Part of Habsburg Empire	.220 (.136)	.142 (.111)	-.408 (.207)**	-.359 (.179)**
No. of observations	3,167	3,202	3,352	3,354
No. of PSUs	169	169	169	169
Pseudo- R^2	.023	.017	.037	.057
<i>Habsburg in 1900 (dropping all three areas that had formerly been part of Habsburg and their correspondents)</i>				
Part of Habsburg Empire	.221 (.173)	.230 (.139)*	-.555 (.239)**	-.452 (.231)**
No. of observations	2,097	2,119	2,240	2,242
No. of PSUs	113	113	113	113
Pseudo- R^2	.025	.020	.037	.055
Dropping Sanjak (1878-1908)				
Part of Habsburg Empire	.086 (.147)	-.020 (.109)	-.441 (.222)**	-.497 (.187)**
No. of observations	3,094	3,129	3,256	3,258
No. of PSUs	164	164	164	164
Pseudo- R^2	.017	.016	.046	.071

Coefficients and standard errors from ordered logit (ologit) estimations in the model specification of Table 6.

Border sample: respondents living within 200km of respondents on the other side of the former Habsburg border in those five countries that are partly Habsburg.

Standard errors (clustered at the level of PSUs) in parentheses: * significance at ten, ** five, *** one percent.

Data source: Life in Transition Survey (LiTS) 2006; see main text for details.

Table 12: TRUST IN CENTRAL PUBLIC SERVICES

	Trust in political parties (1)	Trust in the parliament (2)	Trust in the government (3)	Trust in the presidency (4)	Trust in the armed forces (5)
Part of Habsburg Empire	.065 (.115)	.129 (.132)	.132 (.130)	.193 (.118)	.073 (.122)
Age of respondent	.004 (.003)	.006 (.003)**	.008 (.003)***	.008 (.003)***	.006 (.003)**
Male respondent	.014 (.074)	.029 (.075)	-.092 (.076)	-.096 (.074)	.049 (.068)
Native language	-.173 (.187)	-.239 (.208)	-.198 (.209)	-.171 (.220)	-.024 (.209)
Ethnic minority	-.137 (.175)	-.163 (.171)	-.260 (.163)	-.268 (.134)**	-.241 (.137)*
Controls for religious affiliation (6 categories)	yes	yes	yes	yes	yes
Worked for income during last 12 months	-.051 (.084)	-.039 (.081)	-.014 (.079)	-.023 (.072)	-.051 (.071)
Controls for household property (7 variables)	yes	yes	yes	yes	yes
Controls for education level (6 categories)	yes	yes	yes	yes	yes
Urban area	-.235 (.112)**	-.187 (.127)	-.176 (.129)	-.046 (.121)	.056 (.132)
Metropolitan area	-.086 (.156)	-.117 (.167)	-.192 (.163)	-.100 (.151)	-.258 (.159)
Household size (equivalent scale)	.040 (.072)	.023 (.069)	-.0003 (.067)	.064 (.067)	.051 (.067)
Household number of children under 14	.028 (.046)	.091 (.050)*	.096 (.052)*	.060 (.051)	-.087 (.046)*
Country fixed effects	yes	yes	yes	yes	yes
No. of observations	3,352	3,396	3,386	3,380	3,149
No. of PSUs	180	180	180	180	180
Pseudo- R^2	.019	.034	.031	.027	.024

Coefficients and standard errors from ordered logit (ologit) estimations.

Border sample: respondents living within 200km of respondents on the other side of the former Habsburg border in those five countries that are partly Habsburg.

Dependent variable is answer to the question “To what extent do you trust the following institutions?” Column (1): Political parties. Column (2): The parliament. Column (3): The government. Column (4): The Presidency. Column (5): The armed forces. Answer categories are: 1=Complete distrust; 2=Some distrust; 3=Neither trust nor distrust; 4=Some trust; 5=Complete trust. Category 6=Difficult to say set to missing in regressions.

Standard errors (clustered at the level of PSUs) in parentheses: * significance at ten, ** five, *** one percent.

Data source: Life in Transition Survey (LiTS) 2006; see main text for details.

Table 13: RESULTS FROM A BUSINESS SURVEY

	Description of court system in resolving business disputes:		
	fair (1)	honest (2)	upholds our property rights (3)
Part of Habsburg Empire	.274 (.118)**	.135 (.155)	.185 (.145)
Year when firm began operations in country	-.004 (.003)	-.008 (.004)*	-.002 (.003)
Sector dummies (8 categories)	yes	yes	yes
Firm size medium (50-249 employees)	.494 (.129)***	.346 (.159)**	.329 (.195)*
Firm size large (250-9,999 employees)	.123 (.159)	.420 (.203)**	.728 (.201)***
Percentage of firm owned by foreigners	.003 (.002)	.003 (.002)	.001 (.002)
Percentage of firm owned by government	.003 (.002)	.001 (.002)	.002 (.002)
Country fixed effects	yes	yes	yes
No. of observations	1,029	1,009	997
No. of PSUs	168	168	171
Pseudo- R^2	.028	.025	.014

Coefficients and standard errors from ordered logit estimations.

Border sample: firms located within 200km of firms on the other side of the former Habsburg border in those five countries that are partly Habsburg.

Dependent variable in columns (1) and (2) is answer to the question “How often do you associate the following descriptions with the court system in resolving business disputes?”. Answer categories are: 1=Never; 2=Seldom; 3=Sometimes; 4=Frequently; 5=Usually; 6=Always.

Dependent variable in column (3) is answer to the question “To what degree do you agree with this statement? ‘I am confident that the legal system will uphold my contract and property rights in business disputes.’” Answer categories are: 1=Strongly disagree; 2=Disagree in most cases; 3=Tend to disagree; 4=Tend to agree; 5=Agree in most cases; 6=Strongly agree.

Standard errors (clustered at the level of PSUs) in parentheses: * significance at ten, ** five, *** one percent.

Data source: Business Environment and Enterprise Performance Survey (BEEPS) 2005; see main text for details.

Table A.1: SPECIFICATION IN A CLASSICAL REGRESSION DISCONTINUITY FRAMEWORK

	Trust in courts	Trust in police	Bribes to courts	Bribes to road police
	(1)	(2)	(3)	(4)
Controlling for distance to border	.378 (.185)**	.286 (.187)	-.651 (.290)**	-.421 (.267)
Controlling for quadratic in distance to border	.374 (.187)**	.281 (.186)	-.679 (.279)**	-.445 (.258)*
Controlling for distance to border and its interaction with a Habsburg dummy	.372 (.186)**	.278 (.186)	-.678 (.273)**	-.449 (.251)*

All reported estimates are coefficients of the variable “Part of Habsburg Empire” in the model specification of Table 6.

Coefficients and standard errors from ordered logit (ologit) estimations.

Border sample: respondents living within 200km of the former Habsburg border, based on GIS-computed distance to border, in those five countries that are partly Habsburg.

Standard errors (clustered at the level of PSUs) in parentheses: * significance at ten, ** five, *** one percent.

Data source: Life in Transition Survey (LiTS) 2006; see main text for details.

Table A.2: AVERAGE ABSOLUTE MARGINAL EFFECTS

	Trust in courts	Trust in police	Bribes to courts	Bribes to road police
	(1)	(2)	(3)	(4)
Part of Habsburg Empire	.022	.012	.023	.029
Age of respondent	.0002	.0007	.0005	.001
Male respondent	.0005	.003	.0003	.020
Worked for income during last 12 months	.019	.002	.003	.012
Compulsory schooling education	.032	.031	.009	.014
Secondary education	.114	.144	.069	.118
Professional, vocational school or training	.083	.140	.080	.068
Higher professional degree (university, college)	.059	.045	.012	.009
Post graduate degree	.104	.116	.011	.025
Buddhist	.072	.077	.110	.078
Jewish	.002	.010	.011	.018
Christian	.007	.003	.0007	.025
Muslim	.002	.018	.013	.017
Other	.020	.012	.0007	.008
Native language	.010	.0003	.011	.0008
Ethnic minority	.004	.006	.005	.009
Household has a car	.007	.002	.002	.015
Household has a secondary residence	.013	.017	.011	.018
Household has a bank account	.027	.017	.002	.011
Household has a credit/debit card	.023	.018	.011	.029
Household has a mobile phone	.048	.031	.012	.035
Household has a computer	.015	.012	.006	.015
Household has access to internet at home	.022	.027	.020	.072
Urban area	.011	.001	.003	.006
Metropolitan area	.011	.019	.001	.005
Household size (equivalent scale)	.007	.010	.006	.011
Household number of children under 14	.002	.002	.004	.003
Used service in last 12 months			.075	.096

Average absolute marginal effects of each independent variable for the model specification of Table 6 (including country fixed effects), holding the others constant at their mean. For instance, the upper-left entry of .022 indicates that having been part of the Habsburg Empire increases the probability of moving to a higher trust category by 2.2 percentage point, on average across the five categories. Table A.3 displays these effects category by category.

Dependent variable in columns (1) and (2) is answer to the question “To what extent do you trust the following institutions?” Column (1): The courts. Column (2): The police. Answer categories are: 1=Complete distrust; 2=Some distrust; 3=Neither trust nor distrust; 4=Some trust; 5=Complete trust. Category 6=Difficult to say set to missing in regressions.

Dependent variable in columns (3) and (4) is answer to the question “In your opinion, how often is it necessary for people like you to have to make unofficial payments/gifts in these situations?” Column (3): Interact with the courts. Column (4): Interact with the road police.” Answer categories are: 1=Never; 2=Seldom; 3=Sometimes; 4=Usually; 5=Always.

Data source: Life in Transition Survey (LiTS) 2006; see main text for details.

Table A.3: MARGINAL EFFECTS OF HABSBERG EMPIRE BY CATEGORY

	Trust in courts (1)	Trust in police (2)		Bribes to courts (3)	Bribes to road police (4)
Complete distrust	-.046	-.020	Never	.059	.073
Some distrust	-.009	-.009	Seldom	-.020	-.021
Neither trust nor distrust	.011	-.001	Sometimes	-.023	-.033
Some trust	.033	.018	Usually	-.010	-.013
Complete trust	.012	.012	Always	-.005	-.006
Average absolute marginal effect	.022	.012	Average absolute marginal effect	.023	.029

Marginal effects of past affiliation with the Habsburg Empire in the model specification of Table 6, holding other variables constant at their mean. For instance, the upper-left entry of -.046 indicates that having been part of the Habsburg Empire decreases the probability of answering “complete distrust” by 4.6 percentage points. The last row displays the average absolute percentage change across categories and corresponds to the same number in Table A.2.

Dependent variable in columns (1) and (2) is answer to the question “To what extent do you trust the following institutions?” Column (1): The courts. Column (2): The police. Answer categories are: 1=Complete distrust; 2=Some distrust; 3=Neither trust nor distrust; 4=Some trust; 5=Complete trust. Category 6=Difficult to say set to missing in regressions.

Dependent variable in columns (3) and (4) is answer to the question “In your opinion, how often is it necessary for people like you to have to make unofficial payments/gifts in these situations?” Column (3): Interact with the courts. Column (4): Interact with the road police.” Answer categories are: 1=Never; 2=Seldom; 3=Sometimes; 4=Usually; 5=Always.

Data source: Life in Transition Survey (LiTS) 2006; see main text for details.