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# The Empire Is Dead, Long Live the Empire! Values and Human Interactions 90 Years after the Fall of the Habsburg Empire

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# The Empire Is Dead, Long Live the Empire!

## Values and Human Interactions 90 Years after the Fall of the Habsburg Empire\*

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Do Empires affect human values and behavior long after their demise? In several Eastern European countries, communities on both sides of the long-gone border of the Habsburg Empire have been sharing common formal institutions for 90 years now. We exploit this geographic discontinuity in a regression-discontinuity design with country fixed effects using data of individuals living inside a restricted band around the former border. We find that historical Habsburg affiliation increases current social capital and trust and reduces corruption in several public services. Past formal institutions can leave a legacy through cultural norms even after generations of common statehood.

Keywords: Institutions; social capital; trust; corruption;  
Habsburg Empire; regression discontinuity

JEL classification: D73, N33, N34

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***Comments welcome!***

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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 round 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, the empire lives on. 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 the cultural values and norms and the ensuing nature of human interactions 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 levels of social capital, trust, and corruption today in Central and Eastern Europe. Our findings add to the quickly growing literature indicating that history can have long-term effects on economic development (cf. Nunn 2009) through its impact on current formal institutions (e.g., North 1981; Engerman and Sokoloff 1997; La Porta, Lopez-de-Silanes, Shleifer, and Vishny 1998; Acemoglu, Johnson, and Robinson 2001) as well as on values, beliefs, and cultural norms (e.g., Putnam, Leonardi, and Nanetti 1993; Greif 1994; Alesina and Fuchs-Schündeln 2007; Guiso, Sapienza, and Zingales 2008a; Tabellini 2010). Our results indicate that long-gone formal institutions can have a lasting impact on cultural norms of behavior.

The Habsburg Empire is historically known as a multi-ethnic state with a relatively well-functioning, respected bureaucracy. Historians characterize the Habsburg bureaucracy as “fairly honest, quite hard-working, and generally high-minded” (Taylor 1948) – in contrast to other Empires in Central and Eastern Europe, like the Russian and Ottoman Empires. We argue that this created trust among its inhabitants in mutual interactions and in the respectability of government institutions. However, the formal institutions ceased to exist with the collapse of the Habsburg Empire after World War I, breaking up into separate nations that have seen several waves of drastic institutional changes since. To test whether the cultural values and 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 social capital, trust, and corruption in

many Central and 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 the status and duration 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 variation.

To identify the effect of the Habsburg Empire on current values and interactions, we therefore devise a geographic regression-discontinuity design that compares individuals living in communities located within a 200-kilometer band around the long-gone Habsburg border. 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 either side of the long-gone Habsburg “border” have been sharing a common statehood for several generations now. Additionally, we control for a large set of individual-level factors such as education, religion, language, wealth indicators, and urbanicity.

Our results suggest that the Habsburg Empire still exerts effects on cultural norms and human interactions today. Comparing individuals left and right of the long-gone Habsburg border, people living in locations that used to be territory of the Habsburg Empire have higher social capital, as measured by membership in civic organizations and political parties. They have higher levels of trust in a variety of state institutions such as the police, courts, and the government. And they are less likely to find it necessary to make unofficial payments to the police and to courts.

A set of specification tests validates a causal interpretation of these effects. First, when we create “placebo” borders by moving the actual Habsburg border 200 kilometers East or West, we do not find an effect. This indicates that our results capture a discontinuity, rather than a general East-West pattern. Second, we verify that altitude does not vary significantly between the two sides of the former Habsburg border, excluding obvious geographic differences between the Habsburg and non-Habsburg sample. Third, we do not find any significant difference between the two sides in terms of medieval city size, access to medieval trade routes, and presence of a

medieval diocesan town. These results indicate that the Habsburg effect is not simply a perpetuation of differences that existed before Habsburg.

Our results indicate that political and judicial institutions that were in effect a long time ago have formed cultural values and norms that prevail until today. These “slow-moving institutions” (Roland 2004) are the link through which distant political and economic history influences current outcomes. Given the waves of migration and displacement that accompanied the institutional disruptions that the successor countries of the Habsburg Empire have experienced since its demise, it seems likely that the cultural norms of behavior did not only survive by intergenerational transmission within family, but also through the persistent nature of continuous reciprocal human interactions in local communities.<sup>1</sup>

The paper is organized as follows. Section II discusses different channels through which history may have a legacy for current outcomes and relates these to the existing literature. Section III provides a brief overview of the history of the Habsburg Empire. Section IV describes our data. Section V develops the empirical identification strategy. Section VI presents our basic results and Section VII additional analyses. Section VIII concludes.

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

What are the mechanisms that lead to the fact that history often has long-term repercussions for economic development today (see also Nunn 2009 for a review)?

First, historical circumstances and events can shape the state and evolution of formal institutions which survive and affect economic interactions and outcomes today (e.g., North 1981). Recent research on the importance of colonial rule for long-term economic development tends to emphasize its impact on current formal institutions, be it through the effect of large-scale plantation production on institutional development through its effect on economic and political inequality (Engerman and Sokoloff 1997), through the introduction of civil vs. common law legal systems (La Porta, Lopez-de-Silanes, Shleifer, and Vishny 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

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<sup>1</sup> Research in economic history investigates different aspects of the Habsburg Empire, for instance, that the trade flows before the collapse of the Habsburg Empire are a good predictor for the borders of its successor states (Heinemeyer, Schulze, and Wolf 2008; Schulze and Wolf 2009).

Atlantic trade affected the evolution of formal institutions in Western Europe, Nunn (2008) shows that external trade in slaves had long-run repercussions for economic development in Africa, and Acemoglu, Cantoni, Johnson, and Robinson (2009) show that French invasion of Central Europe after the French Revolution brought radical institutional changes that left a long-lasting mark on economic development.

A second reason why past developments may be related to current developments is that both are affected by 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 (Diamond 1997; Sachs 2003). The effect of geography may also be intertwined with the development of formal institutions, for example, when a region's geographical endowment makes it lucrative for cash cropping (Engerman and Sokoloff 1997) or when disease environments determine institutional choices due to their effect on settler mortality (Acemoglu, Johnson, and Robinson 2001).

Third, historical events may have caused differential development of people's knowledge, human capital, which caused subsequent differences in economic development. For example, Glaeser, La Porta, Lopez-De-Silanes, and Shleifer (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. 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.

Fourth, history may matter for later economic development by affecting people's cultural norms and values which then persist over time. There is an increasing emphasis in the literature 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 2009; Tabellini 2010). Probably the best-known argument for the importance of the cultural channel is Weber's (1904) hypothesis that a specific Protestant work ethic furthered capitalist development, although Becker and Woessmann (2009) and Cantoni (2009) find little evidence for this specific channel in historical indicators of economic development. Putnam, Leonardi, and Nanetti (1993) conjecture 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, and Guiso, Sapienza, and Zingales (2008a) find supportive evidence. Greif (1994) describes how the collectivist vs. individual attitudes towards contract enforcement of Maghribi vs. Genoese medieval merchants affected their subsequent developments. 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. Cultural evolution may also be closely interrelated with institutional development (Greif 2006). Nunn and Wantchekon (2009) identify cultural norms and formal institutions as two separate channels of the effect of slave trade on economic development in Africa. Cultural norms may also relate to historical education, as shown by Tabellini (2010) who identifies historical measures of literacy and political institutions as correlates of cultural variables today that are related to economic development across European regions. 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, as evidenced in the literature on law and finance.

An important question relates to the mechanism of the transmission of cultural values and norms over time. Most of the current literature focuses on intergenerational transmission within families (Bisin and Verdier 2000; Tabellini 2008b; Guiso, Sapienza, and Zingales 2008b), for example when looking at parent-child correlations (Dohmen, Falk, Huffman, and Sunde 2007) or when identifying links of U.S. immigrants with their or their ancestors' home countries (Guiso, Sapienza, and Zingales 2006; Fernandez and Fogli 2007; Giuliano 2007; Tabellini 2008a; Algan and Cahuc 2009). An alternative mechanism, though, would be that cultural values and norms persist through the very nature of local human interactions: If a person with a prior of trust moves into a distrustful town, he might quickly change over to a prior of distrust, and vice versa. Thus, through continuous reciprocal behavior, a local equilibrium of trust or distrust may be maintained even with migration, and even without any family ties. 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, in education and elsewhere, suggests that people's attributes may affect other people (see Sacerdote 2010 for a survey).

In this paper, we aim to test the fourth channel of history on cultural values and norms today, while controlling for the other channels, in the specific case of the Habsburg Empire.

Specifically, we are interested in whether persistent cultural values and norms can be a channel through which historical, but long-gone formal institutions can affect outcomes today.

### III. A Short History of the Habsburg Empire

Beginning in the 11<sup>th</sup> century, Habsburg<sup>2</sup> collected a multitude of different territories and peoples from Spain in the West, to the Netherlands, to Austria, to the Dalmatian coast, to Bohemia, Moravia, Hungary, Bosnia, and Galicia in the East. In ruling these extensive areas, the Habsburg family and dynasty were a decisive factor and an – or even the – most important momentum in keeping the vast country together. The Habsburg project was quite successful: Charles V (1500-1558) ruled the Holy Roman Empire, a realm with 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). Since the focus of our empirical analysis is Central and Eastern Europe, the following overview will mainly cover those parts of the history of the Habsburg Empire pertaining to that area.

The Habsburg identification with Austria began when Rudolf IV of Habsburg was elected German king in 1273. Since then, the Habsburgs continuously expanded their territories, by marriage,<sup>3</sup> by succession to the throne, but also by wars – and even by wars that were waged without Austria. In the 16<sup>th</sup> century, more than half of Europe was ruled by the House of Habsburg. In 1526, it broadened its territory in central Europe: After the young Hungarian King Louis II had died in the Battle of Mohács against the Ottomans, Ferdinand of Austria, the brother of the Holy Roman Emperor Charles V., was elected King of Hungary and Croatia, as well as of Bohemia. External events caused Habsburg’s north-eastward expansion: the First Partition of Poland in 1772 brought Galicia and Lodomeria to Vienna, arranged by Russia and Prussia. The acquisition of Bukovina in 1775 was a side effect of the Treaty of Küçük Kainardca (1774) after the Russo-Ottoman War.

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<sup>2</sup> The name Habsburg derives from the municipality and castle of Habsburg, in what is now Switzerland, where the Habsburg family originates. For simplification, we use generally the term *Habsburg monarchy*, although it is frequently used for the years between 1526 and 1867. The name *Austrian Empire* is officially applied during the years 1804-1867. *Austria-Hungary* or *Austro-Hungarian Empire* describes officially the two states Austria and Hungary within one common reign 1867-1918.

<sup>3</sup> This was only the case for westwards expansion. *Bella gerant alii, tu felix Austria nube* (Let the others wage wars, you, fortunate Austria, marry), as a famous hexameter put it.



With Ferdinand I's succession to Hungary in 1526, Habsburg had to bear the brunt of the Ottoman drive from the Balkans into central Europe. Twice, the expanding Ottoman Empire even tried to capture Vienna, in 1529 and in 1684. The latter battle marked the beginning of the political hegemony of the Habsburg dynasty in Central Europe, as it conquered step by step vast territories along the Danube – in Hungary, nowadays Croatia, Serbia, and Romania constantly driving back the Ottomans, attempting to prevent Russia and its ally Serbia from gaining further territories here, until the conflict with Russia became notorious in the 19<sup>th</sup> century. To maintain a balance of power between the leading European powers, the Treaty of Berlin in 1878 permitted Austria-Hungary to occupy Bosnia, Herzegovina, and the Sanjak of Novi Pazar in Serbia/Montenegro (Glenny 2000). Figure 1 displays a historical map that shows the expansion of the Habsburg Empire around the turn to the 19<sup>th</sup> century.

Until the 18<sup>th</sup> century, the different parts of the Habsburg Empire were only loosely tied together. This changed throughout the 18<sup>th</sup> century when the administration was increasingly centralized. The Habsburgs tried to modernize their realms from within. In contrast, the Ottoman Empire made no successful efforts to overcome its inner contradictions. Its subjects (“*raya*”, “protected flock”) were deprived of political rights, reforms in the late 19<sup>th</sup> century were too weak. Hardly any secular education existed. Bribery was a normal phenomenon, institutionalized and even expected by officials (İnalçık 1996). On the other side, Russia conceded some economic and social modifications – serfdom was abolished in 1861, in Habsburg lands in 1781. But the autocratic monarchy gave no leeway to parliamentary influence until the revolutions of 1917 (Bartlett 2005; Imber 1990). In contrast to these powers, the Habsburg state ruled in a smoother manner, so that for the inhabitants everyday life was more predictable than in the adjacent Ottoman Empire:<sup>4</sup> Habsburg administration was based on a civil law book. Already Maria Theresa (1741-1765) began to establish an administration of civil servants (*Beamtenstaat*) and instituted county governors (*Kreishauptmänner*) to supervise local administration in different parts of the Habsburg Empire. Her son, Josef II (1765-1790), an enlightened, rational, and secularized monarch, (too) radically continued that way. He founded institutions of social

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<sup>4</sup> In his novel “The bridge over the Drina”, Literature Nobel Prize laureate Ivo Andrić describes that the Habsburgers built water pipes, introduced street numbers, road lightning, and passes for livestock; see also Imamović (2007) and Benze (2006).

and medical care, ended censorship in theatres and the press, induced complex legal reforms, and established German as the official language in all parts of his empire.

Despite the national aspirations of the peoples within the empire, some aspects of Habsburg policy were widely accepted. Transfers in the form of subsidies and infrastructure projects such as railroads to less developed regions fostered their integration. In the Habsburg lands, education was more important than in Russia and the Ottoman Empire. Bureaucracy throughout the empire was well respected by the population because of its reliability. Taylor (1948) paraphrases this as follows: “The Austrian bureaucracy was fairly honest, quite hard-working, and generally high-minded, it probably did more good than harm.” Until the end of World War I, the Austro-Hungarian army was a functioning multicultural microcosm and an important instrument for integrating people from all over the Habsburg territories.

Reasons underlying the fall of the Habsburg Empire include national intentions of the Habsburg peoples and the political will of the winning powers of World War I. Together with Austria, the other European multiethnic empires collapsed: the Russian and the Ottoman Empire.

Although the Habsburg Empire was no modern-day democracy, it was well-run compared to the Russian and Ottoman Empires and it might well have left an institutional legacy.<sup>5</sup>

Since some of the Habsburg institutions (e.g. the *Kreishauptmänner*, county governors) have endured for very long, they are likely to have impacted everyday life and interactions. Even when, with the fall of the Habsburg Empire, the formal, fast-moving institutions ceased to exist, the slow-moving institutions that came with them may have persisted.

## IV. Data

### A. *The Life in Transition Survey (LiTS)*

We use the Life in Transition Survey (LiTS) collected by the European Bank for Reconstruction and Development (EBRD). 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 satisfaction with public services and whether respondents usually paid bribes in connection with these

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<sup>5</sup> Several elements of the Habsburg bureaucracy survive to this day. 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 7 a.m. to this day.

services. Conducted by EBRD between August and October 2006, the survey covers 29 countries in Central and Eastern Europe, the Community of Independent States, Mongolia, and Turkey. In each country, 1,000 households were interviewed, deriving from a sample of 20 households in 50 locations. The LiTS dataset contains information on the municipality of residence of survey respondents. Figure 2 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. Thus our dataset covers the following 17 countries in Central and 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 the duration of membership. Our sources for this information are Hrvatski povijesni (2003), Kinder and Hilgemann (2004), Leisering (2004), Magocsi (2002), Reden (1995), and Rothaug (2001). Furthermore, we geo-coded municipality data to compute distance from the old Habsburg border. We use this information below to restrict the sample to respondents in municipalities within a certain distance of the Habsburg border (see Section V).

### ***B. Definition of Main Variables***

Our first set of outcome variables measures social capital. Social capital is often measured by membership in organizations (Glaeser, Laibson, and Sacerdote 2002). The LiTS questionnaire contains one question about membership in organizations. It is formulated as “Are you a member of? (a) a political party, (b) other civic/voluntary organizations (club, association).” The respondent can answer with yes or no. We investigate how organization membership depends on former affiliation with the Habsburg Empire and other covariates in a probit model.

Our second set of outcome variables refers to trust. In the LiTS questionnaire there are two sets of questions on trust. The first is on general trust and is formulated as “*Generally speaking, would you say that most people can be trusted, or that you can’t be too careful in dealing with*

*people? What would it be today? And before 1989?”* The second is on trust in institutions and is phrased as *“To what extent do you trust the following institutions?”* with a list of twelve institutions (the presidency, the government/cabinet of ministers, the parliament, courts, political parties, armed forces, the police, banks and the financial system, foreign investors, NGOs, trade unions, and religious institutions).<sup>6</sup>

In contrast to other comparable surveys that ask trust questions (e.g., the World Values Survey or the US General Social Survey), the LiTS asks respondents to express the *intensity* of their trust beliefs.<sup>6</sup> The answer can be chosen from the following scale: 1=complete distrust, 2=some distrust, 3=neither trust nor distrust, 4=some trust, 5=complete trust, and 6=difficult to say. We set the last category (6=difficult to say) to missing in the regressions.

Our third set of outcome variables refers to an economic outcome, corruption. The variable is inferred from the following LiTS question: “In your opinion, how often is it necessary for people like you to have to make unofficial payments/gifts in these situations?” with permissible answers: 1=never, 2=seldom, 3=sometimes, 4=usually, and 5=always. Table 2 shows the answers to this question for the following eight situations presented to survey respondents: (1) road police, (2) request official document, (3) other police, (4) courts, (5) medical treatment, (6) public education, (7) unemployment benefits, and (8) other social security benefits. At the descriptive level, it is evident that bribing is less likely in formerly Habsburg areas.

In addition to the three sets of 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 the test language is their native 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 for the full sample.

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<sup>6</sup> The permissible answers differ from those in the World Value Survey, where the answers 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 is likely to be superior.

## V. Empirical Model

To estimate whether historical affiliation with the Habsburg Empire has a lasting effect on cultural and social norms and values, our basic model expresses the cultural variables  $y$  – the measures of social capital, trust, and corruption – of individual  $i$  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  $X$ :

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

Estimation at the individual level in a multivariate regression framework allows controlling for observable factors that vary systematically across individuals in our sample. The rich set of control variables includes individual age, gender, education, religion, working status, language, ethnic minority status, a set of indicators of household property, household size, number of young children, and urban or metropolitan character of the community location.

However, not all important factors may be readily observed. In particular, countries might differ for many other reasons. 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 hand, important general country characteristics such as aspects of geography and formal institutions 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  $\beta$ .

Thus, as a first step, we include a full set of country fixed effects  $\alpha_c$  in the model:

$$(2) \quad y_{ic} = \alpha_c + \beta H_{ic} + X'_{ic} \gamma + \varepsilon_{ic}$$

This model exploits the fact that the former Habsburg border cuts through several Central and 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 these people live in territories that used to be on Habsburg soil and part of them in territories that did not belong to the Habsburg Empire. In this model, identification comes solely from variation within each country. This effectively restricts identification to the sub-sample of countries which 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 soil that used to belong to Habsburg is between one third and one half in Montenegro, Poland, and Romania, while it is low in Ukraine and high in Serbia (see Table 1).

While within-country identification will solve the most severe 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 implement a regression-discontinuity design (RDD) that further restricts the sample to individuals living within a restricted band around the former Habsburg border. The Habsburg border – no longer existent at least since 1918 – gives rise to a geographic discontinuity within the five-country sample. The RDD 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 likely to ensure 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 choose a bandwidth of 200 kilometers (124 miles) around the historical Habsburg border.<sup>7</sup>

To ensure comparability of the treatment and control group on the two sides of the former Habsburg border, rather than literally simply including any observation within 200 km from the Habsburg border in the dataset, we actually implement the RDD sample using a matching algorithm. Individuals are only included in the RDD sample if there is a match in the dataset within 200 km on the other side of non-existent border. That is, observations enter the sample only if reasonably close-by observations exist on the other side. 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 regression-discontinuity sample is illustrated in Figure 2, where the light blue circles and white triangles

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<sup>7</sup> We also experimented with even narrower bandwidths. Results are very similar when the sample is restricted to individuals within 150 km from the Habsburg border. While even narrower bands often tend to lose statistical significance due to small sample sizes, some of the key results even hold with a bandwidth of 50 km (31 miles).

depict the LiTS observations in the RDD band of 200 km around the former Habsburg border. The RDD models also continue including country fixed effects.

In Section VII below, we will test the validity of the RDD in several specification tests. Among others, we spuriously move the discontinuity by 200 km to either side, to ensure that it 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 discontinuity, 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 existence of medieval trade routes and diocesan cities. All specification tests corroborate the validity of the RDD.

Our measures of social capital (membership in associations) are indicator variables, and our measures of trust and corruption are categorical variables indicating the strength and degree of ubiquity of these conditions. The social-capital models will thus be estimated by probit models, whereas the trust and corruption models will be estimated by ordered logit models to account for the ordered nature of these outcome variables. The ordered logit model has the form

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

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.<sup>8</sup>

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<sup>8</sup> Alternatively, we estimated a generalized ordered logit model which allows for non-proportional odds (Williams 2006). Results are very similar. Another alternative is to ignore the categorical nature of the outcome variable and run an ordinary least squares (OLS) regression. Results are qualitatively similar.

## VI. Basic Results

### A. *Social Capital*

The results of the probit regressions of social capital are shown in Table 4 for membership in a civic organization and in Table 5 for membership in a political party. Column (1) of Table 4 shows that respondents in Habsburg areas are more likely to be members in a civic organization. Columns (2) and (3) add socio-demographic variables (age, sex, work status, education, religion, and ethnic background) and control for household characteristics. Adding individual-level controls does not change the Habsburg effect. Once we add household-level controls that partly control for income, the size of the coefficient decreases. Column (4) controls for the area of residence (urban or metropolitan, with rural as base category). The coefficient of the Habsburg indicator remains positive and similar in size.

Since respondents living in the same state as of 2006 might have been exposed to a different set of (common) institutions (national independence, communism, and transition) since the fall of the Habsburg Empire in 1918, it is important to control for this by including a full set of country fixed effects in our regressions. This is done in column (5), where identification now comes from differences between respondents in Habsburg- and non-Habsburg areas *within* the same current state. The estimated Habsburg coefficient indeed drops, as expected, but it stays statistically significant.

Next we restrict the sample in two steps to implement the regression-discontinuity design. In a first step, we use a sample that contains only the five countries that encompass both areas that were Habsburg and areas that were not Habsburg (Montenegro, Poland, Romania, Serbia, and Ukraine). In a second step, we further restrict this sample to those households living in municipalities that are within 200 kilometers of the Habsburg border. This serves to control for unobserved heterogeneity between survey respondents that live several hundred or thousand kilometers apart. Arguably, closer to the Habsburg border, respondents are more likely to have the same unobserved characteristics.

In these restricted samples, the size of the Habsburg coefficient further drops but remains significant at the 10 percent level. The coefficient in column (7) implies that a respondent living in the former Habsburg area is 1.2 percentage points more likely to be a member of a civic organization. Given that on average only 4 percent of the households in our sample are members of a civic organization, this is a quantitatively significant effect.



We run the same regressions for membership in a political party as a dependent variable. The results are reported in Table 5. The coefficient of the Habsburg indicator is positive and significant throughout. Its significance and magnitude increases to the one percent level from the ten percent level in column (1) once additional covariates are added in column (2). The increase in magnitude is highest when country fixed effects are included in column (5). Even in the restricted samples (columns (6) and (7)), the size of the coefficient does not change much. The coefficient implies that the probability of being a member of a political party is 2 percentage points higher if the respondent lives in a former Habsburg municipality. Given that in our sample 9 percent of the respondents are members of a political party, this a sizable effect.

### ***B. Trust***

When analyzing trust, we use the same estimation sample as in the final column of Tables 4 and 5. The restricted sample with respondents in the five countries that were partly Habsburg and partly non-Habsburg and within 200 km from the Habsburg border is our preferred estimation sample because it allows us to control for current-country fixed effects and to control for heterogeneity across larger geographic areas by restricting attention to respondents close to the old Habsburg border.

The results of the ordered logit regressions of trust in general and trust in state institutions are shown in Table 6. The specification used is the same as the one of the final column in Table 4. The number of observations varies between regressions because answers were missing and because the response “difficult to say” was set to missing. Here we concentrate on coefficient estimates and the implied direction of the effects.

We run 14 separate regressions. The first two regressions have trust in other people as a dependent variable; the other twelve regressions analyze trust in all kinds of state institutions (legislature, executive, and judiciary) as well as banks and the financial system, foreign investors, non-governmental organizations (NGOs), trade unions, and religious institutions. For state institutions, the results are very clear. The Habsburg effect is always positive and significant. This implies that respondents living in former Habsburg municipalities have more trust in all state institutions included in the questionnaire. Given that we have already shown that respondents in the former Habsburg area are more likely a member of a political party, it is not too surprising that they have more trust in political parties. As we will investigate corruption in

the next subsection, it is worth noting that the Habsburg effect is positive for the police and courts.

This higher level of trust is also found for other institutions, such as banks and financial system, foreign investors, NGOs, and religious institutions. However, the effect for trade unions is not statistically significant, and the point estimate is much smaller.

Interestingly, the effect of Habsburg on the current general trust level in other people is insignificant. However, it is highly significant for the period before 1989. Here the transition of the economic and political system may have brought about important changes. Before the socialist system broke down, people on Habsburg soil may have cooperated relatively more. This could have changed after the economic transition started to affect the optimization of individual behavior also in terms of “networking”. Before 1989, the remembered political experience of the Habsburg time past might have acted like a brace fostering trust.<sup>9</sup>

### ***C. Corruption***

Table 7 shows coefficients from ordered logit regressions of corruption in contacts with the road police. Here we concentrate on coefficient estimates and the implied direction of the effects. Again, we restrict attention to the sample of five countries that were partly Habsburg and partly non-Habsburg. Column (1) shows that respondents in Habsburg areas are less likely to give high scores on the corruption variable.<sup>10</sup> Obviously, the effect may depend on whether a respondent did have contact with the road police at all. Column (2) controls for this variable. While respondents that had contact with the road police in the previous twelve months are more inclined to report bribes, the Habsburg coefficient does not change much and, if anything, becomes larger, possibly reflecting lower incidence of interaction with the police in Habsburg areas. Columns (3) through (7) follow the same logic as in Tables 4 and 5. Column (3) adds basic socio-demographic variables (age, gender, education, work status, religion, and ethnic background), which leaves the Habsburg coefficient largely unchanged. The same is true when household characteristics are added in column (4). Column (5) controls for urban and

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<sup>9</sup> A note of caution relates to the fact that the two questions on trust towards other individuals before 1989 and after 1989 may lead older respondents to glorify old times, whereas younger respondents may not be able to make a meaningful comparison at all. These effects could potentially vary by affiliation with Habsburg Empire. The direction of the effect is, however, a priori unclear.

<sup>10</sup> Results from ordinary least squares (OLS) regressions that treat the categorical outcome as a continuous variable show qualitatively similar results.

metropolitan area, column (6) adds a full set of country fixed effects, and column (7) restricts to the regression-discontinuity sample near the former border. Overall, there is clear evidence that bribes to the road police are significantly less likely to occur on the Habsburg side.

Table 8 looks at bribes in contacts with other public institutions. For seven out of eight outcomes, we find a negative Habsburg effect, i.e., bribes are less likely. The effect is statistically significant in five out of eight cases.

## **VII. Additional Analyses**

We assess the robustness of our results by employing various specification tests (see Imbens and Lemieux 2008). First, we check whether we find a treatment effect when spuriously moving the Habsburg border. We should not find any effect when comparing municipalities on both sides of a meaningless border. Second, we look at possible jumps in the value of other covariates at the Habsburg border. We do so by checking whether municipalities on both sides of the Habsburg border have similar geographic characteristics (altitude). We restrict this robustness check to geographic characteristics because individual and household characteristics might potentially vary as a *result* of the Habsburg Empire, whereas geographic characteristics are arguably exogenous. Third, we look at indicators pre-dating the (expansion of the) Habsburg Empire to see whether the effects we are measuring after the fall of the Habsburg Empire only perpetuate pre-existing differences before the Habsburg Empire came into being. In contrast to the previous robustness check which uses largely time-constant geographic characteristics, this check uses indicators that might vary over time.

### ***A. Is the Habsburg Border Spurious?***

In the first robustness check, we proceed as follows: we keep *all Habsburg municipalities* in our five-country sample and define as the new treatment indicator all municipalities in the 200 km border strip. This experiment amounts to moving the Habsburg border 200 km to the West and comparing Habsburg municipalities within 200 km of the actual Habsburg border to Habsburg municipalities outside the 200 km border area. As expected, we find the new (wrong) Habsburg indicator to be generally insignificant in regressions of social capital and corruption measures where we control for the same set of variables as in column (5) of Table 4.

In a similar vein, we can move the actual Habsburg border by 200 km to the East. We implement this by restricting our estimation sample to *all non-Habsburg municipalities* and (wrongly) re-define all (non-Habsburg) municipalities within 200 km of the actual Habsburg border as Habsburg. Again, we find the new (wrong) Habsburg indicator to be generally insignificant in regressions of social capital and corruption measures.

### ***B. Does the Habsburg Empire Differ In Its Geographic Characteristics?***

In the second robustness check, we use the sample of municipalities in our five-country sample (Montenegro, Poland, Romania, Serbia, and Ukraine) within 200 km of the Habsburg border. This is our main estimation sample where the regression-discontinuity design is most credible. We regress the altitude of the sample municipalities on a Habsburg indicator. The coefficient on the Habsburg indicator is 40 (meters) with a  $t$ -value of 1.01. In an alternative specification, we use robust regression analysis, where outliers (in terms of altitude) are given less weight. This exercise leads to a point estimate on the Habsburg coefficient of 19 (meters) with a  $t$ -value of 0.82. We conclude from this robustness check that Habsburg municipalities do not systematically differ in altitude compared to non-Habsburg municipalities. This addresses 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.

### ***C. Do the Estimated Habsburg Effects Pick up Pre-Habsburg Differences?***

A potential worry is that the Habsburg Empire might have expanded into geographic areas that were distinct from areas outside the (new) Habsburg border in important 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 collect a series of variables capturing economic development, exposition to outsiders, and cultural features pre-Habsburg. We then compare municipalities on both sides of the Habsburg border to see whether there are significant differences in these variables.

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 (cf. Bairoch 1988; Acemoglu, Johnson, and Robinson 2002). We use data on urban population by 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 the years 1000, 1200, 1300, 1400, and 1500, i.e., well before the maximal expansion of the Habsburg Empire, as indicators of economic development. In separate regressions, we regress city sizes on indicators of (i) whether these cities were ever part of the Habsburg Empire, (ii) whether they were part of the Habsburg Empire in 1500, and (iii) whether they were part of the Habsburg Empire in 1600. In all three cases, we use a discontinuity sample of municipalities within 200 kilometers of the respective borders – i.e., of the border in its maximum extension in case (i); of the year 1500 Habsburg border in case (ii); and of the year 1600 border in case (iii). In all cases, we find that cities inside and outside the Habsburg borders do not differ systematically in population size. If anything, the signs tend to suggest that cities on the Habsburg side were somewhat smaller.

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 we regress an indicator variable for cities that were major trading cities (Magocsi 2002) on our Habsburg indicators, we do not find a statistically significant effect.

Finally, values in different parts of Eastern Europe might have been influenced by a strong presence of the Church. We use indicator variables for whether a city was a diocesan town in 1450 (Magocsi 2002). Again, the location of diocesan towns does not vary by later affiliation with the Habsburg Empire.

We take this third set of robustness checks as evidence that the Habsburg Empire did not systematically expand into certain areas to exploit pre-existing advantages in terms of economics, trust, and values. Together, we conclude from the three sets of robustness checks that we identify a genuine effect of the former Habsburg border and do not pick up confounding effects.

## VIII. Conclusions

The results of this paper show that even 90 years after its demise, the Habsburg Empire lives on in the values and interactions of the people living within its long-gone borders. Comparing individuals living on either side of the former Habsburg border, we find that respondents on a current household survey who live on former Habsburg soil have higher levels of social capital, as indicated by membership in civic organizations and political parties. They also have more trust in state institutions such as the police, courts, and the government. Moreover, they are less likely to pay bribes for a variety of public services, demonstrating that the institutional heritage influences not only preferences and unilateral decisions but also bilateral bargaining situations.

We establish this result on the basis of a geographic regression-discontinuity design. We exploit the fact that the Habsburg border does not always coincide with current national borders. Whereas many current states in Central and Eastern Europe were historically inside or outside the Habsburg Empire in their entirety, five countries comprise both Habsburg and non-Habsburg areas. Using this five-country sample, we can identify the Habsburg effect when comparing communities within a range of 200 km on both sides of the former border of the Habsburg Empire. This approach has the advantage that respondents face the same institutions today because they live *within* current national borders but differ in their exposure to the formal institutions of the Habsburg Empire. We can additionally control for observed variations in education, religion, language, and wealth at the individual level.

Given this setting, we can rule out other channels of historical influence often discussed in the literature. For more than three generations, territories on the two sides of the former Habsburg border have been sharing a common set of formal institutions. By restricting the analysis to territories close to each other around the former border, geographic differences can be ruled out. Additional specification tests confirm that locations on the two sides do not differ significantly in terms of geographic altitude. The micro dataset allows us to control for differences in education at the individual level. The same is true for religion, ruling out another potential cultural channel. As a result, the observed Habsburg legacy can be pinned down to the cultural and social norms of behavior observed in the data. In the case of the Habsburg Empire, history matters for current attitudes and behavior not because formal institutions have survived, but because individual values, beliefs, and norms have survived.

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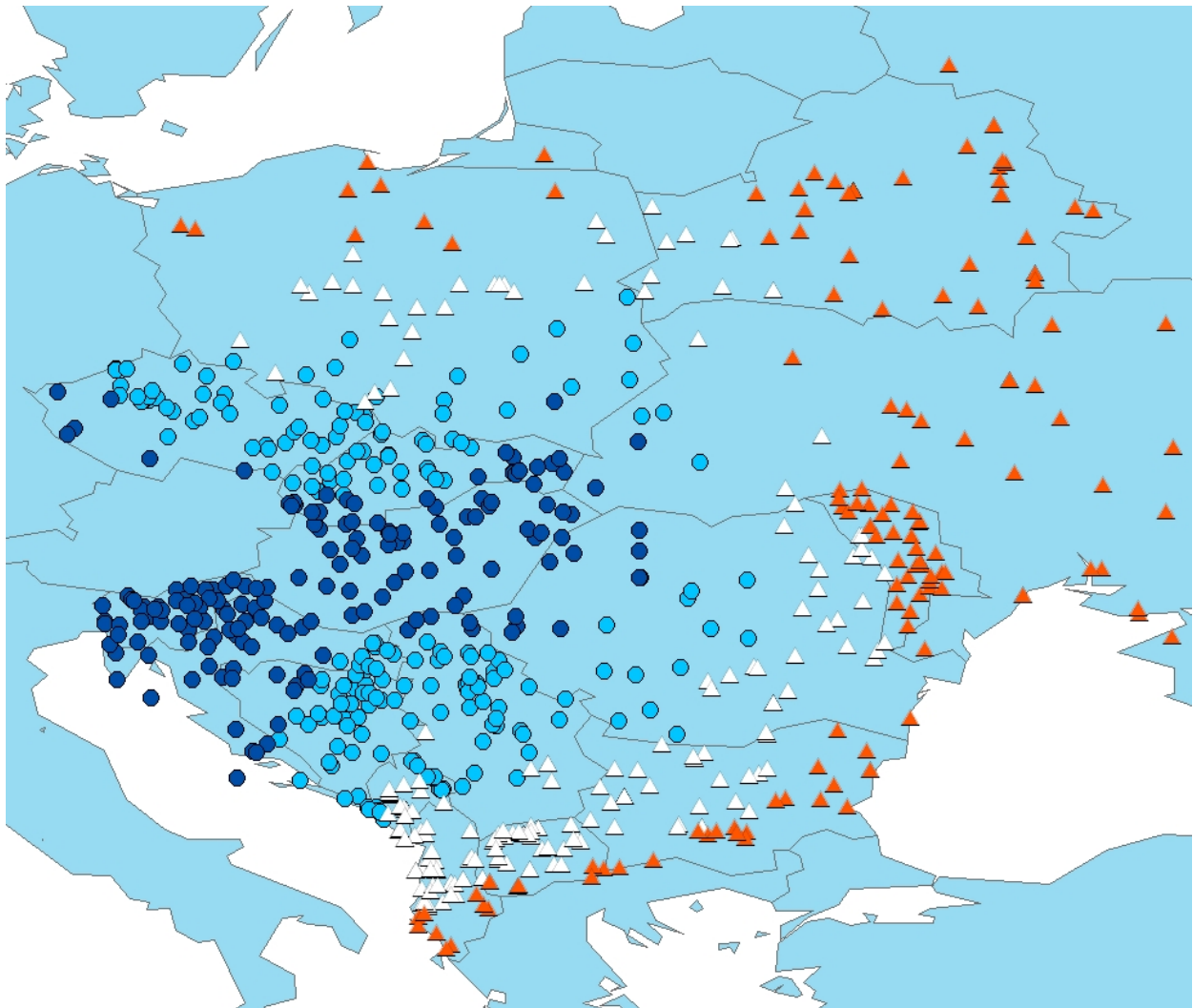
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Figure 1: HISTORIC MAP OF THE HABSBURG EMPIRE



Source: Dierke (1899).

Figure 2: LOCATION OF MUNICIPALITIES IN THE LiTS 2006 DATASET



Notes: Light blue and dark blue circles show LiTS municipalities that belonged to Habsburg. White and orange triangles show LiTS municipalities outside Habsburg areas. The light blue circles and white triangles lie within 200 km of the former Habsburg border.

Table 1: THE SAMPLES AND COUNTRIES' HABSBURG STATUS

	Full sample			RDD sample		
	not Habsburg (1)	Habsburg (2)	Total (3)	not Habsburg (4)	Habsburg (5)	Total (6)
1 Albania	1,000 <i>100.0</i>	0 <i>0.0</i>	1,000 <i>100.0</i>			
2 Belarus	1,000 <i>100.0</i>	0 <i>0.0</i>	1,000 <i>100.0</i>			
3 Bosnia	0 <i>0.0</i>	1,000 <i>100.0</i>	1,000 <i>100.0</i>			
4 Bulgaria	1,000 <i>100.0</i>	0 <i>0.0</i>	1,000 <i>100.0</i>			
5 Croatia	0 <i>0.0</i>	1,000 <i>100.0</i>	1,000 <i>100.0</i>			
6 Czech Rep.	0 <i>0.0</i>	1,000 <i>100.0</i>	1,000 <i>100.0</i>			
7 FYR Macedonia	1,000 <i>100.0</i>	0 <i>0.0</i>	1,000 <i>100.0</i>			
8 Hungary	0 <i>0.0</i>	1,000 <i>100.0</i>	1,000 <i>100.0</i>			
9 Moldova	1,000 <i>100.0</i>	0 <i>0.0</i>	1,000 <i>100.0</i>			
10 Montenegro	600 <i>60.0</i>	400 <i>40.0</i>	1,000 <i>100.0</i>	600 <i>60.0</i>	400 <i>40.0</i>	1,000 <i>100.0</i>
11 Poland	660 <i>66.0</i>	340 <i>34.0</i>	1,000 <i>100.0</i>	480 <i>60.0</i>	320 <i>40.0</i>	800 <i>100.0</i>
12 Romania	520 <i>52.0</i>	480 <i>48.0</i>	1,000 <i>100.0</i>	500 <i>65.8</i>	260 <i>34.2</i>	760 <i>100.0</i>
13 Russia	1,000 <i>100.0</i>	0 <i>0.0</i>	1,000 <i>100.0</i>			
14 Serbia	100 <i>10.0</i>	900 <i>90.0</i>	1,000 <i>100.0</i>	100 <i>11.1</i>	800 <i>88.9</i>	900 <i>100.0</i>
15 Slovakia	0 <i>0.0</i>	1,001 <i>100.0</i>	1,001 <i>100.0</i>			
16 Slovenia	0 <i>0.0</i>	1,001 <i>100.0</i>	1,001 <i>100.0</i>			
17 Ukraine	880 <i>88.0</i>	120 <i>12.0</i>	1,000 <i>100.0</i>	60 <i>42.9</i>	80 <i>57.1</i>	140 <i>100.0</i>
Total	8,760 <i>51.5</i>	8,242 <i>48.5</i>	17,002 <i>100.0</i>	1,740 <i>48.3</i>	1,860 <i>51.7</i>	3,600 <i>100.0</i>

Number of observations. Row shares in italics below. Data source: Life in Transition Survey (LiTS) 2006; see main text for details.

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

	(1) Road police			(2) Request official document			(3) Other police			(4) Courts		
	not Habsburg	Habsburg	Total	not Habsburg	Habsburg	Total	not Habsburg	Habsburg	Total	not Habsburg	Habsburg	Total
Never	4,983 <i>56.9</i>	5,561 <i>68.9</i>	10,544 <i>62.7</i>	5,148 <i>58.8</i>	6,208 <i>77.0</i>	11,356 <i>67.6</i>	5,719 <i>65.5</i>	6,358 <i>78.9</i>	12,077 <i>71.9</i>	5,790 <i>66.3</i>	6,256 <i>77.8</i>	12,046 <i>71.8</i>
Seldom	1084 <i>12.4</i>	911 <i>11.3</i>	1,995 <i>11.9</i>	1,318 <i>15.1</i>	838 <i>10.4</i>	2,156 <i>12.8</i>	1,208 <i>13.8</i>	776 <i>9.6</i>	1,984 <i>11.8</i>	993 <i>11.4</i>	724 <i>9.0</i>	1,717 <i>10.2</i>
Sometimes	1,343 <i>15.4</i>	997 <i>12.4</i>	2,340 <i>13.9</i>	1,325 <i>15.1</i>	703 <i>8.7</i>	2,028 <i>12.1</i>	1,048 <i>12.0</i>	625 <i>7.8</i>	1,673 <i>10.0</i>	1,050 <i>12.0</i>	627 <i>7.8</i>	1,677 <i>10.0</i>
Usually	838 <i>9.6</i>	437 <i>5.4</i>	1,275 <i>7.6</i>	612 <i>7.0</i>	215 <i>2.7</i>	827 <i>4.9</i>	489 <i>5.6</i>	220 <i>2.7</i>	709 <i>4.2</i>	530 <i>6.1</i>	302 <i>3.8</i>	832 <i>5.0</i>
Always	504 <i>5.8</i>	163 <i>2.0</i>	667 <i>4.0</i>	346 <i>4.0</i>	99 <i>1.2</i>	445 <i>2.7</i>	271 <i>3.1</i>	81 <i>1.0</i>	352 <i>2.1</i>	376 <i>4.3</i>	136 <i>1.7</i>	512 <i>3.1</i>
Total	8,752 <i>100.0</i>	8,069 <i>100.0</i>	16,821 <i>100.0</i>	8,749 <i>100.0</i>	8,063 <i>100.0</i>	16,812 <i>100.0</i>	8,735 <i>100.0</i>	8,060 <i>100.0</i>	16,795 <i>100.0</i>	8,739 <i>100.0</i>	8,045 <i>100.0</i>	16,784 <i>100.0</i>

	(5) Medical treatment			(6) Public education			(7) Unemployment benefits			(8) Other social security benefits		
	not Habsburg	Habsburg	Total	not Habsburg	Habsburg	Total	not Habsburg	Habsburg	Total	not Habsburg	Habsburg	Total
Never	3,586 <i>41.1</i>	4,364 <i>54.2</i>	7,950 <i>47.4</i>	5,265 <i>60.3</i>	5,949 <i>73.8</i>	11,214 <i>66.7</i>	6,392 <i>73.1</i>	6,921 <i>85.9</i>	13,313 <i>79.2</i>	6,177 <i>70.7</i>	6,861 <i>85.2</i>	13,038 <i>77.7</i>
Seldom	1139 <i>13.0</i>	898 <i>11.2</i>	2,037 <i>12.1</i>	1,105 <i>12.6</i>	693 <i>8.6</i>	1,798 <i>10.7</i>	983 <i>11.2</i>	563 <i>7.0</i>	1,546 <i>9.2</i>	1047 <i>12.0</i>	555 <i>6.9</i>	1,602 <i>9.5</i>
Sometimes	1,651 <i>18.9</i>	1,280 <i>15.9</i>	2,931 <i>17.5</i>	1,129 <i>12.9</i>	811 <i>10.1</i>	1,940 <i>11.6</i>	785 <i>9.0</i>	361 <i>4.5</i>	1,146 <i>6.8</i>	873 <i>10.0</i>	370 <i>4.6</i>	1,243 <i>7.4</i>
Usually	1,249 <i>14.3</i>	907 <i>11.3</i>	2,156 <i>12.8</i>	750 <i>8.6</i>	369 <i>4.6</i>	1,119 <i>6.7</i>	351 <i>4.0</i>	138 <i>1.7</i>	489 <i>2.9</i>	377 <i>4.3</i>	171 <i>2.1</i>	548 <i>3.3</i>
Always	1111 <i>12.7</i>	606 <i>7.5</i>	1,717 <i>10.2</i>	490 <i>5.6</i>	242 <i>3.0</i>	732 <i>4.4</i>	232 <i>2.7</i>	78 <i>1.0</i>	310 <i>1.8</i>	259 <i>3.0</i>	97 <i>1.2</i>	356 <i>2.1</i>
Total	8,736 <i>100.0</i>	8,055 <i>100.0</i>	16,791 <i>100.0</i>	8,739 <i>100.0</i>	8,064 <i>100.0</i>	16,803 <i>100.0</i>	8,743 <i>100.0</i>	8,061 <i>100.0</i>	16,804 <i>100.0</i>	8,733 <i>100.0</i>	8,054 <i>100.0</i>	16,787 <i>100.0</i>

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?" Number of observations; share in italics below.

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

Table 3: DESCRIPTIVE STATISTICS

	Mean	StdDev	Min	Max
	(1)	(2)	(3)	(4)
Part of Habsburg empire	.51	.50	0	1
Duration of affiliation with Habsburg (in years)	132.02	179.57	0	635
Distance to Vienna (in km)	715.91	665.08	51.24	8074.17
<b>Individual-level variables</b>				
Age of respondent	47.40	17.81	17	92
Male respondent	.43	.50	0	1
Worked for income during last 12 months	.49	.50	0	1
Native language	.92	.28	0	1
Ethnic minority	.07	.26	0	1
<i>Education (omitted category: no degree)</i>				
Compulsory schooling education	.19	.39	0	1
Secondary education	.22	.41	0	1
Professional, vocational school or training	.35	.48	0	1
Higher professional degree (university, college)	.17	.38	0	1
Post graduate degree	.01	.10	0	1
<i>Religion (omitted category: atheist)</i>				
Buddhist	.0009	.03	0	1
Jewish	.002	.04	0	1
Christian	.76	.43	0	1
Muslim	.11	.32	0	1
Other	.01	.12	0	1
<b>Household-level variables</b>				
HH has a car	.49	.50	0	1
HH has a secondary residence	.14	.35	0	1
HH has a bank account	.46	.50	0	1
HH has a credit/debit card	.38	.49	0	1
HH has a mobile phone	.74	.44	0	1
HH has a computer	.37	.48	0	1
HH has a access to internet at home	.23	.42	0	1
HH size (equivalent scale)	1.88	.69	1	6.50
HH number of children under 14	.36	.74	0	7

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

Table 4: MEMBERSHIP IN CIVIC ORGANIZATION: MARGINAL EFFECTS FROM PROBIT

	17 countries in Eastern Europe (former Habsburg and neighbors)				5 countries that are partly Habsburg +/- 200km to Habsburg border		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Part of Habsburg empire	.055 (.004)***	.054 (.004)***	.037 (.004)***	.036 (.004)***	.015 (.007)**	.011 (.006)*	.012 (.007)*
Age of respondent		.0002 (.0001)**	.0005 (.0001)***	.0005 (.0001)***	.0003 (.0001)***	-.0002 (.0002)	-.0002 (.0002)
Male respondent		.020 (.003)***	.020 (.003)***	.020 (.003)***	.018 (.003)***	.011 (.005)**	.012 (.006)**
Worked for income during last 12 months		.010 (.004)**	.002 (.004)	.002 (.004)	.003 (.003)	.003 (.005)	.006 (.007)
Controls for education level (6 categories)		yes	yes	yes	yes	yes	yes
Controls for religious affiliation (6 categories)		yes	yes	yes	yes	yes	yes
Native language		.013 (.006)**	.011 (.006)**	.011 (.006)**	.005 (.006)	.012 (.007)*	.014 (.008)*
Ethnic minority		-.002 (.006)	.001 (.006)	.002 (.007)	.004 (.007)	.013 (.010)	.024 (.013)*
Controls for HH-level property (8 variables)			yes	yes	yes	yes	yes
HH size (equivalent scale)			-.003 (.003)	-.004 (.003)	-.003 (.003)	.0003 (.004)	.0008 (.005)
HH number of children under 14			.001 (.003)	.001 (.003)	.001 (.002)	-.001 (.003)	-.002 (.004)
Urban area				-.00009 (.004)	.0008 (.003)	-.0002 (.005)	.003 (.007)
Metropolitan area				-.015 (.004)***	-.005 (.004)	-.010 (.006)*	-.009 (.008)
Full set of country dummies					yes	yes	yes
Obs.	16950	16866	16806	16806	16806	4958	3573
Pseudo-R2	.030	.061	.075	.077	.124	.081	.080

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

Marginal effects and standard errors from probit estimation.

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

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



Table 5: MEMBERSHIP IN POLITICAL PARTY: MARGINAL EFFECTS FROM PROBIT

	17 countries in Eastern Europe (former Habsburg and neighbors)				5 countries that are partly Habsburg +/- 200km to Habsburg border		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Part of Habsburg empire	.008 (.004)*	.013 (.004)***	.016 (.004)***	.014 (.004)***	.026 (.007)***	.020 (.008)***	.021 (.009)**
Age of respondent		0.000 (.0001)	.0003 (.0001)**	.0003 (.0001)**	.0005 (.0001)***	.0006 (.0002)**	.0007 (.0003)**
Male respondent		.038 (.004)***	.035 (.004)***	.034 (.004)***	.023 (.003)***	.021 (.007)***	.031 (.009)***
Worked for income during last 12 months		.001 (.004)	-.0002 (.004)	-.0002 (.004)	.007 (.004)*	.028 (.008)***	.034 (.010)***
Controls for education level (6 categories)		yes	yes	yes	yes	yes	yes
Controls for religious affiliation (6 categories)		yes	yes	yes	yes	yes	yes
Native language		-.029 (.008)***	-.028 (.008)***	-.028 (.008)***	-.019 (.007)***	-.011 (.011)	-.0006 (.015)
Ethnic minority		-.0006 (.007)	-.00005 (.007)	.0002 (.007)	-.003 (.006)	.003 (.012)	.002 (.015)
Controls for HH-level property (8 variables)			yes	yes	yes	yes	yes
HH size (equivalent scale)			.016 (.003)***	.015 (.003)***	.004 (.003)	.005 (.006)	.007 (.007)
HH number of children under 14			.002 (.003)	.002 (.003)	.002 (.002)	-.003 (.005)	-.004 (.005)
Urban area				-.009 (.004)**	-.008 (.003)**	-.006 (.007)	-.003 (.009)
Metropolitan area				-.018 (.005)***	-.019 (.004)***	-.041 (.007)***	-.054 (.009)***
Full set of country dummies					yes	yes	yes
Obs.	16974	16891	16827	16827	16827	4947	3564
Pseudo-R2	.0004	.043	.056	.058	.137	.152	.161

Dependent variable is answer to the question “Are you a member of a political party?”

Marginal effects and standard errors from probit estimation.

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

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

Table 6: TRUST IN PEOPLE AND INSTITUTIONS

	Habsburg effect	No. observations	Pseudo- $R^2$
	(1)	(2)	(3)
Trust other people: today	-.0008 (.074)	3388	.008
Trust other people: before 1989	.169 (.080)**	3074	.024
The presidency	.253 (.071)***	3380	.028
The government/cabinet of ministers	.234 (.071)***	3386	.031
The parliament	.249 (.071)***	3396	.035
Courts	.348 (.073)***	3359	.024
Political parties	.166 (.071)**	3352	.019
Armed forces	.180 (.073)**	3149	.025
The police	.265 (.072)***	3409	.019
Banks and the financial system	.316 (.074)***	3343	.026
Foreign investors	.378 (.074)***	3168	.022
Non governmental organisations	.279 (.075)***	3185	.032
Trade unions	.100 (.075)	3137	.020
Religious institutions	.343 (.073)***	3374	.071

Dependent variable in rows (1) and (2) is answer to the question “Generally speaking, would you say that most people can be trusted, or that you cant be too careful in dealing with people?” with the following categories: 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 rows (3) to (14) is answer to the question “To what extent do you trust the following institutions?” with the same response categories.

All regressions use the same specification as in Table 4, column (7).

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

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

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

Table 7: BRIBES TO ROAD POLICE: ORDERED LOGIT ESTIMATES

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Part of Habsburg empire	-.447 (.059)***	-.466 (.059)***	-.393 (.062)***	-.393 (.062)***	-.398 (.063)***	-.137 (.075)*	-.344 (.087)***
Interacted with road police in last 12 months		1.200 (.070)***	1.042 (.074)***	1.053 (.076)***	1.055 (.076)***	1.083 (.078)***	1.044 (.093)***
Age of respondent			-.012 (.002)***	-.012 (.002)***	-.013 (.002)***	-.012 (.002)***	-.015 (.003)***
Male respondent			.139 (.062)**	.127 (.062)**	.135 (.063)**	.168 (.064)***	.239 (.078)***
Worked for income during last 12 months			.264 (.065)***	.293 (.068)***	.291 (.068)***	.220 (.069)***	.225 (.084)***
Controls for education level (6 categories)			yes	yes	yes	yes	yes
Controls for religious affiliation (6 categories)			yes	yes	yes	yes	yes
Native language			-.512 (.093)***	-.461 (.095)***	-.456 (.094)***	-.044 (.101)	.154 (.159)
Ethnic minority			-.310 (.120)***	-.334 (.120)***	-.335 (.119)***	-.228 (.122)*	-.177 (.144)
Controls for HH-level property (8 variables)				yes	yes	yes	yes
HH size (equivalent scale)				.091 (.052)*	.097 (.053)*	.130 (.054)**	.145 (.063)**
HH number of children under 14				-.082 (.041)**	-.082 (.042)*	-.072 (.043)*	-.042 (.048)
Urban area					.203 (.070)***	.180 (.072)**	.062 (.089)
Metropolitan area					.032 (.087)	.087 (.092)	.062 (.110)
Full set of country dummies						yes	yes
Border sample (+/- 200km)							yes
Obs.	4992	4992	4973	4958	4958	4958	3574
Pseudo-R2	.005	.028	.043	.047	.048	.072	.062

Dependent variable 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? Interact with the road police.” with the following categories: 1=Never; 2=Seldom; 3=Sometimes; 4=Usually; 5=Always.

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

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

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

Table 8: BRIBES TO VARIOUS OFFICIALS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Part of Habsburg empire	-.344 (.087)***	-.110 (.092)	-.337 (.100)***	-.393 (.101)***	.065 (.074)	-.133 (.091)	-.201 (.106)*	-.221 (.105)**
Used this service in last 12 months	1.044 (.093)***	.935 (.097)***	1.165 (.144)***	.951 (.171)***	1.081 (.067)***	1.194 (.118)***	1.555 (.203)***	1.386 (.171)***
Age of respondent	-.015 (.003)***	-.007 (.003)**	-.012 (.003)***	-.006 (.003)**	-.006 (.002)***	-.010 (.003)***	-.010 (.003)***	-.008 (.003)**
Male respondent	.239 (.078)***	.198 (.083)**	.187 (.088)**	.004 (.087)	.025 (.066)	.236 (.083)***	.187 (.100)*	.143 (.099)
Worked for income during last 12 months	.225 (.084)***	.243 (.094)***	.200 (.099)**	.180 (.097)*	.228 (.074)***	.172 (.092)*	.037 (.110)	.145 (.111)
Controls for education level (6 categories)	yes	yes	yes	yes	yes	yes	yes	yes
Controls for religious affiliation (6 categories)	yes	yes	yes	yes	yes	yes	yes	yes
Native language	.154 (.159)	.035 (.171)	.075 (.174)	.054 (.177)	.066 (.141)	.107 (.174)	.105 (.209)	.234 (.220)
Ethnic minority	-.177 (.144)	-.133 (.155)	-.148 (.169)	-.151 (.182)	.022 (.122)	.024 (.157)	-.296 (.203)	-.185 (.196)
Controls for HH-level property (8 variables)	yes	yes	yes	yes	yes	yes	yes	yes
HH size (equivalent scale)	.145 (.063)**	.076 (.072)	.173 (.073)**	.097 (.076)	.110 (.056)*	-.017 (.072)	.082 (.084)	.038 (.086)
HH number of children under 14	-.042 (.048)	-.048 (.056)	-.078 (.059)	-.068 (.058)	-.026 (.044)	-.112 (.058)*	.036 (.064)	.009 (.061)
Urban area	.062 (.089)	-.035 (.098)	-.076 (.106)	.028 (.104)	.064 (.079)	-.080 (.099)	-.205 (.116)*	-.101 (.116)
Metropolitan area	.062 (.110)	-.377 (.125)***	-.055 (.129)	.017 (.125)	.022 (.100)	-.316 (.119)***	-.329 (.145)**	-.231 (.145)
Full set of country dummies	yes	yes	yes	yes	yes	yes	yes	yes
Obs.	3574	3574	3571	3572	3571	3572	3574	3568
Pseudo-R2	.062	.061	.058	.041	.045	.065	.060	.057

Dependent variable 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?” with the following categories: 1=Never; 2=Seldom; 3=Sometimes; 4=Usually; 5=Always. Column (1): Interact with the road police; column (2): Request official documents (e.g. passport, visa; birth or marriage certificate, land register, etc) from authorities; column (3): Interact with the police on matters other than traffic and other than requesting documents; column (4) Go to courts for a civil matter; column (5): Receive medical treatment in the public health system; column (6): Receive public education (university, college, vocation); column (7): Request unemployment benefits; column (8): Request other social security benefits.

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

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

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