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Corruption in Transition Economies: Socialist, Ottoman or Structural?

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Corruption in Transition Economies: Socialist, Ottoman or Structural?

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ABSTRACT

Using data from 64 countries in Eastern Europe and MENA, I study the long-run effects of Ottoman and socialist rule on the incidence of corruption. To proxy Ottoman legacies, I estimate the length of Ottoman rule across all Ottoman successor states. Conditioning on income per capita, I find a robust adverse effect of both socialist and Ottoman legacies on present-day corruption – a finding which reconciles two rival accounts of post-socialist corruption from the transition literature. The results are robust to controlling for potential confounders and instrumenting for per-capita income and Ottoman rule. Yet, the explanatory power of long-run historical determinants of corruption is lower than the contribution of short-run factors. While present-day income alone explains about half of the total variation in corruption across post-socialist countries, Ottoman and socialist legacies jointly account for about one third. Although history does matter, the results suggest that most of the corruption observed today in Eastern Europe, Central Asia and the Balkans may not be inherently 'Eastern' or socialist. Rather, the data are consistent with an interpretation of corruption predominantly as a manifestation of persistent economic under-development.

KEYWORDS

Corruption, Development, Long-run Persistence, Ottoman Empire, Post-Socialist Transition

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HIGHLIGHTS

- Historical legacy effects on corruption in 64 transition and developing economies
- Number of years of rule used as a proxy for Ottoman/socialist legacies.
- Corruption is subject to long-run persistence.
- Historical influences have lower explanatory power than short-run contemporary determinants.
- The incidence of corruption is primarily, though not entirely, a function of the level of development.

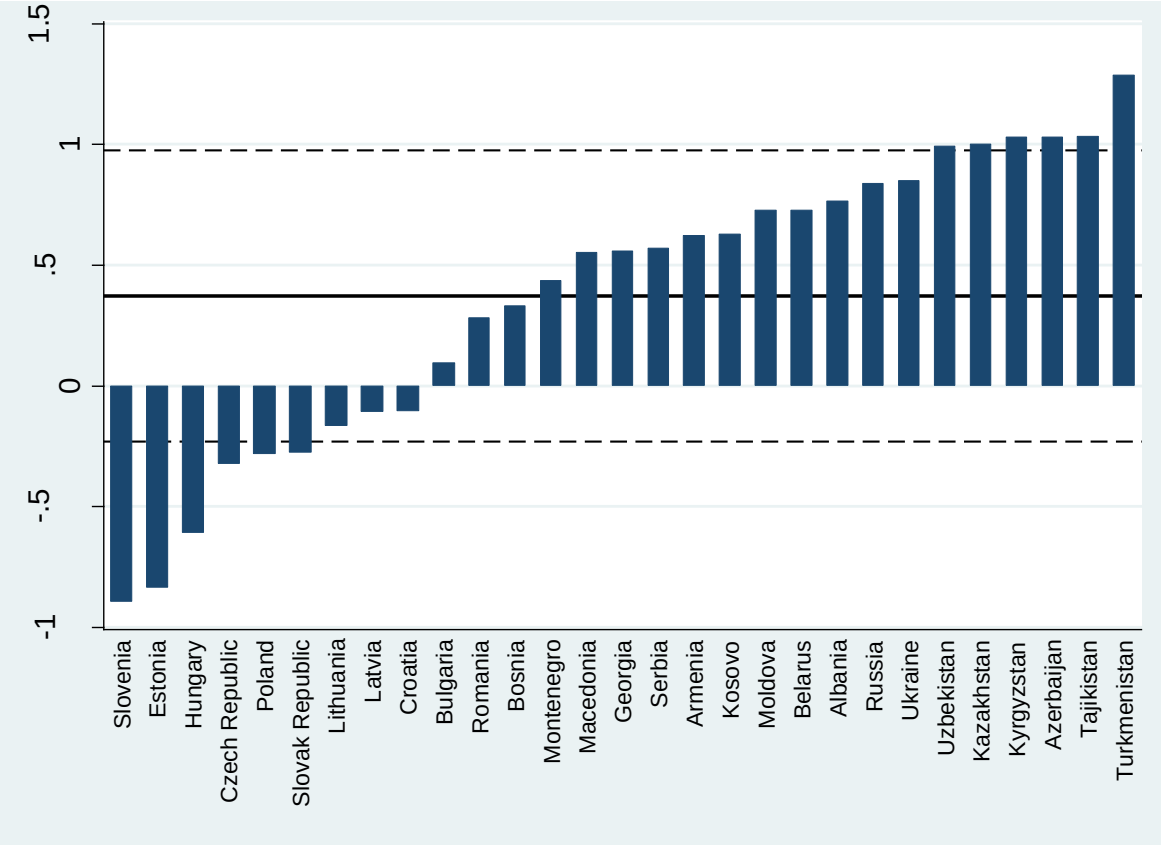
'What is more especially Eastern is the corruption of the officialdom'
Seton Watson (1967[1945]: 147)

1. INTRODUCTION

Since the mid-1990s, the policy community has singled out corruption as a critical obstacle to structural reform and economic development in low- to middle-income economies (Marquette, 2003; Bukovansky, 2006). A discourse of corruption has also featured prominently in the context of post-communist transition. In 2011, for instance, the *Economist* talked of a 'rising tide of sleaze in ex-communist Europe', claiming that 'corruption has replaced communism as the scourge of Eastern Europe' (cited by Holmes, 2013: 1163). Yet, the extent to which corruption is prevalent in post-socialist economies varies widely (see Figure 1). In 2016, for instance, the likes of Estonia and Slovenia received a better score on Transparency International's influential *Corruption Perception Index* than some West-European industrialised countries such as France and Spain (respectively). At the other end of the post-socialist spectrum, Uzbekistan and Ukraine scored worse than Sierra Leone and Malawi. This kind of

evidence has spurred talk of a 'great divide' pitting the successful 'transitioners' of North-Central Europe against the 'laggards' of South-Eastern Europe and the former Soviet Union (Berglof and Bolton, 2002; Darden and Grzymala-Busse, 2006).

FIGURE 1: Levels of Corruption in 29 Transition Economies: Country Data, Average and Standard Deviation (2000-07)



Sources: World Bank, *Worldwide Governance Indicators,* 2017; *Notes:* the scale runs from -2.5, which denotes no corruption, to +2.5, which denotes maximum corruption; the average level of corruption (solid black line) is 0.371, which is above the world average (0.009) over 2000-07. The dashed black lines mark the range of one standard deviation above the below the mean.

Why are some post-communist countries so much more 'corrupt' than others? This paper presents a systematic empirical test of competing explanations of the 'great divide', taking corruption as the institutional outcome of interest. In particular, the paper considers two prominent explanations of institutional divergence. One influential account focuses on the legacies of the socialist experience. Several authors have emphasised the pivotal role of the socialist *nomenklatura* in either facilitating or resisting the establishment of market-compatible institutions, which are instrumental in eliminating rents and reducing corruption (Beck and Laeven, 2006; Treisman, 2002). Other authors, however, contend that the historical antecedents of the post-WWII socialist regimes override the influence of socialist heritage (Moller and Skaaning, 2010). In particular, several scholars point to the legacy of Ottoman rule in South-Eastern Europe and the Balkans as the root cause of the 'great divide' (Dimitrova-Grajzl, 2007; Becker et al., 2016).

Is post-socialist corruption a vestige of the past? If so, is it primarily a manifestation of socialist or pre-socialist legacies? To answer these two questions, I first investigate the relative influence of socialist vs. Ottoman legacies on present-day corruption; secondly, I estimate the combined influence of historical legacies *vis-à-vis* other contemporaneous determinants of corruption, notably the level of economic development. Recent theoretical approaches suggest that corruption, clientelism and patronage may be unsavoury coping strategies that low- to middle-income societies deploy as a response to the manifold challenges of economic under-development. These challenges include the absence of a large capitalist sector offering opportunities for gainful employment and material advancement, low fiscal and redistributive capacity of the state and the inability to finance and implement a universal system of property rights and contract enforcement (Khan, 2005; Uberti, 2016). These propositions imply that corruption should 'wither away' as per-capita income grows. Still, it is possible that a country's historical heritage might be a source of institutional rigidity, imposing a drag on the establishment of the rule of law, even in the presence of rising incomes.

To explore these questions, I estimate a corruption equation on a sample of 64 countries. To my best knowledge, this is the first study that investigates the significance of socialist and pre-socialist legacies (relative to each other and to other contemporaneous determinants of corruption) on the *full* population of post-socialist *and* post-Ottoman countries. The reason why I focus on this particular pre-socialist Empire is that the legacy of Ottoman rule features most prominently in both formal and informal explanations of comparative corruption in post-socialist Eastern Europe.[1] For instance, citing historian Peter Sugar (1977), Bideleux and Jeffries note that by the end of Ottoman rule 'nepotistic Balkan office-holders and oligarchs had learned the tricks and techniques of political chicanery and corruption only too well: it was the Ottoman legacy and training that was reflected in their actions' (2007: 121). In the Balkans, self-serving behaviour by politicians is often cast as a hang-over of an 'Oriental' or socialist 'despotic' past that will progressively fade as a result of 'Europeanisation' (Sulstarova, 2015; Kajsiu, 2015). The discursive prominence of Ottoman and socialist histories in accounts of post-socialist corruption is not surprising. After all, Ottoman rule and the socialist experience jointly account for virtually *all* the history of South-East Europe and the Balkans in the last 5-600 years.

To measure the long-term influence of historical legacies, I follow Dimitrova-Grajzl (2007) in constructing indices for the number of years a given country spent under Ottoman control or socialist rule. A key novelty of this paper is to extend the coverage of this index to encompass *all* the Ottoman successor states, including those located in the Middle East and North Africa. I find a robust adverse effect of *both* socialist and Ottoman legacies on present-day corruption. Since corruption routines tend to persist over time, the influence of socialist and Ottoman rule is still visible today after controlling for levels of economic development and a host of other potential confounders. Holding everything else constant, a one-standard-deviation increase in the duration of Ottoman/socialist rule is estimated to increase corruption by 16/23 percent of a standard deviation, respectively.

[1] The Ottoman Empire also enjoys a similar reputation outside the narrow bounds of the transition literature. Acemoglu and Robinson (2012: 56), for instance, claim that 'it is the institutional legacy of this empire that keeps the Middle East poor today'.

Consistent with the expectation that a legacy of a more recent historical period has a stronger effect on the present, the estimated effect of socialist rule is larger in magnitude than that of Ottoman rule. Further, I find that the overall explanatory power of long-run historical determinants (that is, the influence of Ottoman and socialist legacies combined) is lower relative to the contribution of short-run factors. While present-day income levels explain some 49 percent of the total variation in corruption amongst post-socialist countries, Ottoman and socialist legacies account for 12 and 22 percent, respectively. More starkly, variation in the level of economic development explains as much as 58 percent of corruption variation in the full sample of post-socialist and post-Ottoman countries, whereas Ottoman and socialist legacies account for only 10 and 16 percent. Thus, history contributes less than half as much to the scale of corruption today as the most important contemporaneous factor alone (i.e. income).

Endogeneity, however, may lead to a biased parameter estimate of the income effect (due to simultaneity) or the effect of Ottoman legacies (due to measurement error). In subsequent analysis, I show that my baseline results are robust to instrumenting for per-capita income levels and the length of Ottoman rule. They are also robust to different specifications and estimation techniques, and they hold in different sub-samples. I conclude by suggesting that most of the corruption observed in Eastern Europe, Central Asia and the Balkans that may not be *inherently* 'Eastern' or socialist. Rather, the data are consistent with an interpretation of corruption primarily as an institutional manifestation of economic under-development.

This paper contributes to the literature on the causes of corruption in the transition economies (Treisman, 2002). By documenting some of the long-term consequences of Ottoman and socialist rule, the findings also relate to a broader literature on the enduring effects of historical events (Nunn, 2009). In the empirical literature on post-socialist countries, the study of historical legacies has remained somewhat peripheral, although recent contributions have examined the long-term economic, political and institutional consequences of a number of historical events, including the Holocaust in Western Russia (Acemoglu et al., 2011), and Russian colonial settlement in Central Asia (Aldashev and Guirkinger, 2017) and the Caucasus (Natkhov, 2015).[2]

The remainder of this article is organised as follows. The next section briefly reviews the literature on the causes of corruption. Section 3 presents the empirical specification, describes the data and discusses estimation issues. Section 4 and 5 discuss the regression results and section 6 concludes.

2. CORRUPTION AND HISTORICAL LEGACIES

2.1 The Causes of Corruption: Theory and Evidence

Since Treisman's seminal article (2000), a growing empirical literature has used cross-section data to investigate drivers of corruption at the country level (see Treisman, 2007 for a review). The potential determinants of corruption examined in the literature fall into three broad categories: economic, institutional and socio-cultural.

[2] The fast-growing literature on Ottoman legacies in Eastern Europe is reviewed in detail below.

As noted by Treisman, 'by far the strongest and most consistent finding […] is that lower perceived corruption correlates closely with higher economic development' (2007: 223). Indeed, virtually all empirical studies include GDP per capita as a regressor. A possible explanation for the strength of this empirical relationship is offered by theories that seek to explain the persistence of personalised rule and informal exercises of power in developing countries (Khan, 2005; Uberti, 2016). At low to mid- levels of per-capita income, the 'modern' private sector is typically marginal in the structure of employment. Consequently, there is no generalised interest across social groups in maintaining the viability of capitalist enterprise, and politicians are subject to little self-restraint when it comes to using their power to extract rents from the fledgling private sector (Khan, 2005: 715). Furthermore, the total surplus produced by the private sector and taxed by the state is typically inadequate to finance a system of universal property rights protection and contract enforcement (Uberti, 2016: 338). Property rights instability makes some assets vulnerable to expropriation, and compels asset-holders to purchase informal protection from politicians, typically by offering bribes or political contributions. To make matters worse, the state also has low fiscal and hence redistributive capacity. This means that the budget is typically not the main site of redistributive politics (Khan, 2005: 718). Rather, it is often through informal or corruptive means that individuals and companies are 'taxed' (i.e. asked to pay a bribe) and key resources (public-sector jobs, industrial subsidies) allocated. For all these reasons, the structural characteristics proxied by a low level of GDP per capita are likely to be associated with high levels of (perceived) corruption.

Another important determinant of relative corruption, holding income levels constant, is the quality of institutions. Democratic consolidation, in particular, has featured prominently in the literature. Though it might increase corruption in the short run, in the long run democracy fosters transparency and accountability. An increased likelihood of sanctions for malfeasance, in turn, reduces the expected returns from rent-seeking (Mohtadi and Roe, 2003; Rock, 2009). Measures of democracy tend to have the predicted effect in cross-country regressions, even after controlling for GDP per capita. Still, the effect is not always statistically significant and is usually economically small (Treisman, 2000: 433), probably because many of the beneficial effects of democratization run through economic development.

Culture and other (time-invariant or slow-moving) societal characteristics may affect corruption independently of income levels and institutional quality. Recent contributions, in particular, argue that religious tradition shapes individuals' loyalties towards their in-group (Treisman, 2000: 403). Thus, religious beliefs may affect individuals' propensity to subvert impersonal rules for the sake of sectional or private gain. Other perspectives contend that the influence of religion runs exclusively through the impact of religious *institutions.* Timur Kuran (2004) has argued that some aspects of Islamic law, such as the *waqf* system of charitable trusts, stimulated corruption due to its inherent inflexibility. Once those institutions are abolished, religious belief has no influence on the incidence of corruption (Kuran, 2010). Becker and Woessmann (2009) implicitly suggest another avenue through which religion may affect the incidence of corruption. They find that the Protestant reformation in Europe raised literacy levels, which in turn may improve bureaucratic capacity, facilitate the emergence of a civil society and promote the flow of information. These, in turn, are all factors that may contribute to reducing or containing corruption. In addition, some authors have emphasised social cleavages such as ethnolinguistic and

religious fractionalization (Mauro, 1995; Easterly and Levine, 1997). Corruption may proliferate in more ethnically or religiously fragmented societies with less social cohesion and lower trust.

More recently, a new body of literature has explored the role of historical factors in driving present-day corruption. European colonialism, in particular, has been singled out as an important source of variation in the quality of political and legal institutions in the developing world. Treisman (2000: 402) and Djankov et al. (2003) show that the former colonies that adopted common-law systems provide more extensive checks on executive behaviour, which reduce opportunities for corruption. Angeles and Neanidis (2015) suggest that, regardless of legal origin, permanent European settlement in the colonised territories created powerful elites that were able to hold onto their privileges up to the present day. When the power of elites is unconstrained, they argue, opportunities for rent extraction increase and corruption is likely to be more prevalent. Consistent with this mechanism, they find that (holding constant the level of development) former settler colonies (e.g. Algeria, Argentina) are significantly more corrupt today than the formerly colonised regions in which Europeans did not settle in large numbers (e.g. West Africa).[3]

2.2 Historical Roots of Post-socialist Corruption

In the context of post-socialist countries, transition scholars have long emphasised the crucial importance of historical legacies. Two groups of explanation can be distinguished. One group focuses on the legacies of socialism (Jowitt, 1992; Ledeneva, 1998; Karklins, 2005). In Soviet-type regimes, tight party control over elite selection favoured the emergence of informal *nomenklatura* cliques. Initially, the leadership harnessed these networks to circumvent the red tape and ensure the timely implementation of economic plans. Progressively, however, the party leadership began to tolerate corruption and abuse of office as a strategy to reward and accommodate the emerging middle class of party cadres (Karklins, 2005: 82). At the same time, petty informal activities and non-elite (*blat*-based)[4] networks proliferated. The 'second economy' was openly tolerated as it made possible the 'reallocation by private means of a significant fraction of […] national income according to private preferences' (Millar, 1985: 697). Corruption and informal exchange circumvented the allocative rigidities of central planning and increased welfare, meanwhile allowing the communist party to keep up the pretence of a functioning socialist economy.

The transition literature has also stressed the continuity between socialist-era and post-socialist elites. In the post-Soviet countries (as in most Yugoslav successor states plus Albania), former cadres were able to convert their political capital into substantial private wealth, re-inventing themselves as the new propertied class of post-communism (Braguinsky, 2009). Even in the relatively more 'liberal' state-socialist countries of East-Central Europe, a technocratic section of the *nomenklatura* went on to occupy most of the managerial positions in the post-communist corporate sector (Eyal et al., 2000). The persistence of elite power establishes a clear line of continuity between communist and post-communist

[3] An important exception is that of the so-called neo-Europes (e.g. US, Australia). It is interesting to speculate whether the settlement of ethnic Turks in the provinces of the Ottoman Empire might be associated with greater corruption today. In this paper, I do not explore this possibility.

[4] *Blat* is Russian for 'favour'.

corruption. Also, the economic reforms of transition (e.g. trade and price liberalisation and asset privatisation) opened up additional opportunities for rent-seeking and profiteering, further consolidating the corrupt revenue base of the elite (Hellman, 1998; Frydman et al., 1998). 'Taken together', Karklins concludes, 'the legacy of the communist era and the temptations of transition have created a context of significant levels of corruption' (2005: 89).

Of course, the degree to which patronage and informality were entrenched in former socialist countries varied considerably. Beck and Laeven (2006), for instance, claim that the power networks of the *nomenklatura* became more entrenched in countries that experienced socialist rule for longer periods of time. This might explain why the Soviet successor states, where communism lasted over 20 years longer than in East-Central Europe, generally have worse property rights institutions and more corruption today. Indeed, both Treisman (2002) and Beck and Laeven (2006) estimate a negative relation between number of years of socialist rule and various measures of present-day corruption and institutional quality. One problem with these accounts, however, is that they cannot easily accommodate the large differences in corruption levels between countries with socialist regimes of equal duration – e.g. Albania and Slovenia.

A second group of scholars has forcefully resisted explanations of corruption that revolve around the socialist experience, contending that they suffer from excessive 'causal proximity' to the *explanandum* (Kitschelt, 2002). In a comparative analysis of socialist regimes, for instance, Kitschelt et al. (1999) find considerable variation in the prevalence of legal-rational strategies of rule (as opposed to personal networks of loyalty) and in the balance of power between the communist elite and their challengers. Since socialism had a similar duration and similar formal institutions (e.g. central planning) in all the countries they examine, Kitschelt et al. (1999) attribute the observed variation to different patterns of state formation across the polities that preceded the establishment of socialism.[5] Between the 14th century and 1918, most of East-Central Europe was controlled by four states: the Habsburg, Ottoman and Russian Empires and Prussia.[6] Their institutional endowments differed systematically. The Ottoman Empire, in particular, has been singled out for the poor quality of its bureaucratic, fiscal and legal institutions. Indeed, by the late 17th century, the authority of the sultan had broken down, giving way to all manner of corruption in the bureaucratic apparatus and judiciary.

There are at least three aspects of Ottoman institutions that might have encouraged corruption. *First*, the centrepiece of the Ottoman state, its fiscal bureaucracy, suffered from insufficient monitoring and control. Palairet writes that beginning in the 18th century the imperial cavalrymen responsible for tax collection [*sipahi*] 'increasingly treated the imperial tithes as their own perquisites, so in peripheral areas where central authority was least secure [as in the Balkans], the revenues to the Porte diminished, weakening its authority' (1997: 36). Provincial governors now relied on local notables [*ayans*] to act as fiscal intermediaries, while 'treasury holdings in the same provinces were auctioned off as tax-farms by the central government' (Inalcik and Quataert, 1994: 661). A new class of rapacious fiscal entrepreneurs

[5] Similarly, Darden and Grzymala-Busse (2006) link the continuing dominance of *nomenklatura* elites in the post-socialist period to literacy levels in the pre-socialist period.

[6] Of course, most of the Prussian and Habsburg territories were part of the Sacred Roman Empire until its dissolution at the end of the Napoleonic wars. Another important state that ruled over East-Central Europe in the early modern period was the Polish-Lithuanian Commonwealth.

rapidly emerged and illegal levies proliferated. Often in cahoots with the judge of their district, the *ayans* enjoyed considerable 'illegal immunity' and sometimes amassed considerable influence locally, carving out for themselves a local fiefdom and turning into 'quasi-feudal warlords' (ibid.). In Constantinople, 'palace favourites were handing out leases and posts as they pleased and taking money for it' (Inalcik, 1978: 346). 'The position of a judge [*kadi*] was often sold to the highest bidder' (Mendelski and Libman, 2014: 183) and, reportedly, 'twice as much of a particular kadiship's revenue came from injustice as from justice' (Kuran, 2011: 240). Things did not improve significantly with the constitutional reforms of the 19th century. Rather, 'all the steps that had been taken towards rationalising elite recruitment and promotion had been bent to make the bureaucracy into a huge patronage machine run by the sultan' (Vaughn-Findley, 1996: 168).

Second, property rights were poorly defined and unstable. All arable land was formally owned by the state [*miri*] and farmed by smallholder peasants. The risk of arbitrary confiscation was high and local officials and military strongmen often colluded with local judges in a bid to dispossess the peasantry and appropriate their land (Inalcik and Quataert, 1994: 660). *Third*, corporate law did not recognise corporate entities beyond the notoriously short-lived trading partnerships [*mudaraba*], thus inhibiting the development of a legally incorporated private sector (Kuran, 2011). The lack of a vibrant civil society, in turn, further increased the power and discretion of Ottoman officialdom, increasing the likelihood of rule-violating behaviour.

When countries broke away from Constantinople, ex-Ottoman soldiers and administrators typically went on to staff the public administrations of the newly independent states (Özbudun, 1996: 148).[7] Thus, the flaws of Ottoman institutions might have carried over to the Empire's successor states. A line of continuity in the administration might explain why notionally similar socialist institutions functioned differently in different countries – e.g. along patron-client lines in post-Ottoman Albania and in a legal-rational way in post-Habsburg Czechoslovakia (Kitschelt et al., 1999). The persistence of Ottoman legacies may also explain why the post-Ottoman transition countries (e.g. Albania, Serbia) are generally more corrupt today than other post-socialist countries with little or no history of Ottoman domination (e.g. Hungary, Estonia).

2.3 Empirical Studies of Long-run Persistence

More recently, economists have begun to investigate econometrically the effect of historical legacies in post-socialist transition countries. These studies are closely related to a broader literature documenting the long-run persistence of institutions and cultural traits (Alesina and Giuliano, 2015), and the potential for historical shocks to establish persistent equilibria (Nunn, 2009). Relevant studies investigated the effect of pre-socialist imperial rule on corruption (Dimitrova-Grajzl, 2007; Moller and Skaaning, 2010, Becker et al., 2014), financial development (Grosjean, 2011), trust (Becker et al., 2014; Karaja and Rubin, 2017) and the quality of legal and political institutions (Dimitrova-Grajzl, 2007;

[7] The continuity of political elites was less smooth in the non-Muslim successor states since non-Muslims were integrated in the bureaucracy only at the lowest level (Todorova, 1996: 56).

Mendelski and Libman, 2014).[8] Most studies estimate the effects of Ottoman rule relative to the legacies of Habsburg rule (Dimitrova-Grajzl, 2007; Mendelski and Libman, 2014) or relative to a mixed reference category comprising all other empires, including Prussia (Grosjean, 2011). Becker et al. (2014) and Karaja and Rubin (2017), by contrast, elect to focus on the Habsburg Empire, estimating a positive legacy effect relative to that of the Ottoman and Russian Empires taken together. In this paper, I favour the former approach and concentrate on the Ottoman Empire.

To measure the influence of history on present-day outcomes, these studies either employ am indicator variable for the identity of the imperial power of interest (Becker et al., 2014; Mendelski and Libman, 2014) or a continuous measure of the number of years of rule (Dimitrova-Grajzl, 2007; Grosjean, 2007). Consistent with historical accounts, the statistical evidence indicates that the long-term legacies of Habsburg and Prussian domination are generally more favourable than those of Russian and, particularly, Ottoman imperial rule.

On the downside, these studies have produced no consensus on the relative importance of socialist vs. pre-socialist imperial legacies in explaining post-socialist institutional development. As such, they have not resolved the theoretical controversies that have developed in the transition literature. Moreover, despite a general consensus that history matters, we still lack specific knowledge about the *relative* importance of historical vs. contemporary factors in shaping institutional and economic outcomes. The failure to answer these questions stems from the limitations of existing contributions. *First*, while most studies control for contemporaneous determinants of corruption (e.g. present-day income levels), none employs the point estimates to investigate the relative explanatory power of historical vs. contemporary factors. *Second*, most studies investigate *either* the effect of socialist legacies (Treisman, 2002; Beck and Laeven, 2006) *or* the long-run effects of Ottoman and Habsburg rule (Grosjean, 2011; Becker et al., 2014). Failure to account for *both* historical legacies in the same model might bias the point estimates. It also forecloses a systematic investigation of their relative explanatory power.

Third, in all existing studies the identification of the socialist legacy effect comes entirely from variation in the duration of socialist rule between the countries of the former Soviet Union and the East-Central and South-East European states. While these two groups provide sufficient variation, a better identification strategy would also exploit the variation between socialist and *non-socialist* countries. This is especially important if we assume that a few decades of socialist rule might be sufficient to entrench the dominance and privilege of a predatory elite, so that the marginal contribution of a few additional years under the clout of *nomenklatura* should be relatively small. Similarly, *fourth*, all existing studies examine the legacies of Ottoman rule in the Porte's former European territories. To my best knowledge, no attempt has been made to estimate the legacy effects of Ottoman domination in the Empire's Middle Eastern and North African (MENA) territories. Estimation based on a sample of both Balkan and MENA countries increases the number of observations and, consequently, the statistical power of the tests.

[8] A partially related paper is Iyigun (2008), which examines the impact of Ottoman military activities in Europe on the incidence of political and religious conflicts in 16th and 17th century Europe.

Lastly, some recent studies seek to improve causal identification by using sub-national data and regression discontinuity approaches (Becker et al., 2014; Karaja and Rubin, 2017). These strategies allow the investigator to hold constant by construction a number of country-level characteristics (legal institutions, national education policies, etc.) that are difficult to control in cross-country regressions. What is gained in depth, however, is lost in breadth. While they permit a more precise estimation of legacy effects in specific contexts (e.g. the so-called 'cleft' countries, formerly partitioned between two Empires), they hardly warrant out-of-sample generalisations. Furthermore, valuable as they are, regression-discontinuity studies cannot answer a general question: how much of the total variation in present-day corruption amongst post-socialist countries is due to historical legacies.

Thus, to investigate the determinants of the post-socialist 'great divide', I rely on cross-country data and estimate the impact of different historical legacies *conditional* upon each other. To add precision to the estimates of Ottoman legacy and increase variation on my measure of socialist legacy, I assemble a sample including both the post-socialist countries of Eastern Europe and Central Asia, *and* all the post-Ottoman countries located in MENA, very few of which were ever ruled by a Soviet-style socialist regime. To further increase variation in the sample, I also include a 'belt' of adjacent countries that never experienced either Ottoman or socialist rule.[9]

2.4 Theoretical Mechanisms of Historical Persistence

Why should the consequences of historical events that took place hundreds of years ago be observable today? A growing body of literature contends that institutions are subject to persistence (Roland, 2004; Nunn, 2009: 77-9). In this paper, I define institutions as 'routinised practices'.[10] Insofar as corruption is 'institutionalised', the subversion of institutional norms becomes a routine – that is, a 'norm' itself (Uberti, 2016). The general argument is that, if new routines are costly to establish and there are powerful constituencies that benefit from existing arrangements, institutional change will be incremental, cumulative and path-dependent.[11]

Take the case of corruption. For one thing, widespread corruption brings material benefits to existing rent-seekers, creating powerful incentives for them to push for formal institutions (e.g. a weak judiciary) that may be compatible with corruption (Acemoglu et al., 2005; Bates, 2014). In addition, corruption reduces the relative returns to entrepreneurship, inviting new entrants into rent-seeking (Acemoglu, 1995). Only through a sufficiently large shock to the distribution of political-economic power can the high transaction costs of institutional change be overcome and a new equilibrium established (Acemoglu et al., 2005: 392). Exogenous historical events may occasion such a shock. For instance, socialist and Ottoman rulers may establish formal institutions (e.g. central planning) whose flaws in

[9] All countries neighbouring a post-socialist or post-Ottoman country are included (except for Western Europe). These countries are: Bahrein, Bangladesh, Bhutan, Chad, Djibouti, India, Iran, Mali, Mauritania, Morocco, Nepal, Niger, Pakistan and Qatar. The results are robust to dropping this 'belt' of neighbours from the sample.

[10] Unlike 'rules of the game', this definition shifts the focus of analysis away from the normative input to institution-building (e.g. legislative reforms) towards the extent to which rules and norms actually shape and regularise the behaviour of actors. These regular patterns of behaviour are the institutional outcome of interest.

[11] Institutions may also have a tendency to endure because they 'propound and defend broadly held values' (Bates, 2014: 61).

design or implementation lead to the emergence of a corruption routine (e.g. the allocative inefficiencies associated with central planning spawned the economy of favours). The resulting corruption can then persist past the historical termination of their underlying formal institutions (e.g. past the end of socialist rule) because powerful groups emerge that benefit from corrupt exchange and have a vested interest in its continuation.

Appendix I provides a simple formal exposition of the dynamics associated with persistence. It also derives some of the observable implications of these dynamics. An important innovation of my approach is that it allows for the process of self-perpetuation to wear off with the passage of time. Although 'sticky', corruption routines may be subject to a process of 'autoregressive decay'. Unless it is vigilantly enforced and constantly invigorated, a pattern of human behaviour is likely to lose its momentum and eventually break down. An observable implication of the assumption of institutional 'decay' is that the legacy of an historical event that took place further back in the past should be smaller than the legacy of a more recent, but otherwise identical, historical event.

Of course, different historical shocks (e.g. Ottoman conquest and socialist take-overs) may impact corruption routines to varying degrees. Furthermore, some routines may have a greater or lesser tendency to persist than others. The model presented in Appendix I shows that, observed today, the magnitude of the legacy of an additional year of Ottoman/socialist rule results from three components: distance in time, magnitude of the 'impact' effect of the historical shock, and tendency of the ensuing routine to persist. The fact that Ottoman rule preceded socialist rule implies that the legacy of Ottoman rule today should be smaller, *other things equal*. Still, without prior knowledge of the relative size of the two 'impact' effects, or the relative tendency of socialist or Ottoman corruption to perpetuate itself, it is not possible to formulate expectations about the relative magnitude of Ottoman and socialist legacies today. Rather, I let the data speak on this.

3. EMPIRICAL STRATEGY

3.1 Empirical Specification

Empirically, I focus on the following cross-country regression:

$$I_i = I_0 + \hat{\alpha} OTTO_i + \hat{\beta} SOC_i + \boldsymbol{\rho}\boldsymbol{\psi}_i + \varepsilon_i \qquad (1)$$

where I_i is the level of present-day corruption in country *i*, measured using the World Bank's *Control of Corruption* index. This index captures 'perceptions of the extent to which public power is exercised for private gain, including both petty and grand forms of corruption, as well as 'capture' of the state by elites and private interests'. The estimates give the country's score on the aggregate indicator, in units of a standard normal distribution, ranging from approximately 2.5 (most corrupt) to -2.5 (least corrupt). Following You and Khagram (2005), I average the values of the corruption indicator over 2000-07 to reduce measurement error and contain the influence of random fluctuations. The choice of time period is

motivated by two considerations. First, I want to factor out any transitory effect on corruption associated with the early years of post-socialist transition. To simplify the model, I also elect to shut out by construction any effect associated with the Global Financial Crisis and the Arab Spring. $OTTO_i$ and SOC_i measure the number of years of Ottoman and socialist rule in country *i*. The main coefficients of interest are $\hat{\alpha}$ and $\hat{\beta}$, which measure the corruption effect of one additional year of Ottoman and socialist rule, respectively. A positive and significant estimated parameter implies that the respective historical event had an adverse impact on corruption *and* that this effect persisted through to the present day. By contrast, a statistically insignificant estimated parameter may result from either the historical event having little or no impact on corruption *or* from corruption routines being subject to little or no persistence.

$\boldsymbol{\psi}_i$ is a vector of country-level determinants of corruption. From the point of view of corruption theory, the most important covariates here are those measuring structural characteristics of the economy. I use a measure of GDP per capita at market exchange rates (averaged over 2000-07) to capture the aspects of a country's level of development that are relevant for corruption (i.e. size of capitalist sector, the state's fiscal capacity, property rights protection). I also include a measure of oil dependence, on the assumption that countries that are (or have been) more dependent on natural resource exports should have worse institutions and more corruption today, in line with the 'resource curse' literature (Ross, 1999). I also include a range of time-invariant socio-cultural characteristics, such as the share of Protestant and Muslim adherents in total population and an index of religious and ethnolinguistic fractionalisation (Alesina et al., 2003). Lastly, I include the log of average aid inflows as a share of GNI. The effect of aid on corruption is unclear a priori. While aid programmes may contribute to establishing better institutions, lowering corruption, the disbursement of aid 'rents' may also generate new opportunities for rent-seeking and corruption (Auty, 2006). A more detailed description of the variables and data sources is provided in Appendix III. Additional controls that are excluded from the baseline specification due to limited degrees of freedom are presented in section 5.

3.2 Historical Data

SOC_i is the number of years that country *i* spent under a Soviet-style communist regime (divided by 100). The variable, reported in Table 1, is constructed using data from the *CIA Factbook.* In the group of 29 post-socialist countries displayed in Figure 1, the mean and standard deviation of SOC_i are, respectively, 53.6 and 10.9. In the full sample used in the empirical analysis, the standard deviation increases from 10.9 to 27.3, allowing for greater variation.

Coding $OTTO_i$ presents additional challenges, reflecting the vicissitudes of Ottoman conquest and control. Originating in central Anatolia, the Ottoman Empire had expanded rapidly towards Europe since the second half of the 14th century. By 1505, the Constantinople ruled over most of South-Eastern Europe and the Balkans, in addition to Crimea, Southern Ukraine and parts of Southern Russia. In the course of the 16th century, Ottoman sultans embarked on a large-scale Eastern expansion, bringing most of the Levant, the Red Sea coast of the Arabian Peninsula and parts of North Africa under their rule. The territorial reach of the Empire, however, expanded and contracted multiple times. As such, former Ottoman borders might cut through present-day countries, and different regions of present-day countries

might have been subject to Ottoman rule for different lengths of time. While Ottoman suzerainty in the northern Serbian region of Vojvodina ended in 1716, for instance, a semi-autonomous Principality of Serbia was not established until 1829 (Brown, 1996). Still, the new state could only claim control over what is now Central Serbia. The Niš region in the south was not wrenched from Ottoman control until Serbia gained full independence in the wake of the 1878 Russo-Turkish war (Palairet, 1997: 173). In fact, the Sandžak of Novi Pazar (a small Muslim-majority region currently shared between Serbia and Montenegro) was not annexed until the first Balkan War (1912-13).[12]

As Dimitrova-Grajzl acknowledges, 'it is difficult to incorporate the process of border changes with respect to particular parts within states (as opposed to focusing on whole states)' (2007: 545). Yet, she attempts to do so for one of the most important cleft countries, namely Romania[13], coding different durations of rule for the regions of Wallachia, Moldavia and Transylvania, the latter of which was annexed by the Habsburg Empire as early as 1690, some 190 years before Romania's independence from Constantinople.

Replicating this exercise across the other cleft countries (most of which have a history of border changes which is not nearly as clear-cut as Romania's) would require making countless assumptions. Thus, in constructing estimates for the length of Ottoman rule, I elect to proxy the duration of rule by the number of years that elapsed between the first Ottoman conquest of a substantial portion of the present-day territory and the time when the Ottomans abandoned the last (substantial) portion of a present-day country. In constructing the estimates, I take 'substantial' to mean (approximately) one quarter of the present-day territory.[14] Whenever possible, I rely on the same historical and cartographic sources used by Dimitrova-Grajzl (Brown, 1996; O'Brien, 2007). To complement these sources, I also obtain information on historical borders from the *GeaCron Project*, a comprehensive online historical atlas.

While admittedly imperfect, this approach has considerable advantages. First, by taking one quarter of the present-day territory as the threshold of effective rule over a country, I avoid having to make the many additional (and arbitrary) assumptions necessary to account for sub-national border movements, given the lack of detailed historical GIS data and the potential conflict between different historical sources. Second, the 18 cleft countries make up just over 28 percent of the total sample. In the cleft countries located in MENA, most of the territory that was never conquered by the Ottomans consists of sparsely populated desert regions (e.g. southern Libya and Algeria; Eastern Jordan and Western Iraq). Throughout the time periods implied by my estimates the Ottomans controlled the territories where the overwhelming majority of the present-day (and, ostensibly, the historical) population resides. Since *populations* are the carriers of institutional legacies, this particular source of measurement error is unlikely to significantly bias the parameter estimates in my regressions. In Eastern Europe, the only important cleft countries are Romania and Serbia. Croatia, Ukraine and Greece, though technically cleft,

[12] In my sample, Kosovo is treated as a separate country. Given the relatively small size of the Sandžak region, the data sources used in the data construction exercise typically take 1829 or 1878 as the end of Ottoman rule in Serbia.
[13] And Bosnia.
[14] Thus, for instance, since the Sandžak region amounts to a very small portion of present-day Serbia, I consider 1878 (rather than 1913) as the last year of Ottoman rule in Serbia.

were for the most part either fully outside the Ottoman sphere of influence (Croatia, Ukraine) or fully under its grip (Greece) for most of the period running from 1371 to the First World War.[15]

Even so, treating the cleft countries as if they had been *fully* under Ottoman control throughout the period implied by the estimates introduces measurement error. This measurement error is non-classical (non-random) insofar as it affects the cleft countries systematically more than the 'non-cleft' countries. This raises the possibility that the OLS (or WLS) parameters may be subject to (compounded) attenuation bias (see Nunn, 2008: 169-170 for a formal derivation). To allay this concern, I show that my results are robust to excluding all (or sub-sets of) the cleft countries from my sample. In addition, I use the geographical distance from Constantinople to instrument for the length of Ottoman rule. The IV estimates of the Ottoman legacy are, as expected, larger but qualitatively consistent with the corresponding WLS.

In my approach to data construction, I also reject Dimitrova-Grajzl's time-discounting scheme, which assigns half as much importance to years of Ottoman control prior to 1700. This is supposed to 'reflect the view of historians […] that the main legacy of the Ottoman Empire came from the latter period rather than from the initial years when the Ottomans were establishing power in Europe' (2007: 545). Instead of *imposing* a (potentially arbitrary) time-discounting scheme, I leave $OTTO_i$ and SOC_i undiscounted and I subsequently perform a sensitivity analysis to examine the extent to which the estimated parameters depend on prior assumptions about the magnitude of historical 'decay'. The results are qualitatively robust across a wide range of assumptions. The estimates of the duration of Ottoman and socialist rule for the countries included in my sample are displayed in Table 1.

3.3 *Estimation Issues*

In line with the existing empirical literature, I assume that the timing and location of Ottoman conquest and defeat is exogenous to economic and institutional outcomes. Although the Ottoman state had a well-developed navy, it never employed its naval capabilities in geographical exploration efforts or long-distance overseas voyages (Inalcik, 1978). As such, it is unlikely that Ottoman leaders might deliberately have selected regions with favourable institutional or factor endowments, conquering them first and giving them up last. In this sense, Ottoman expansionism differed qualitatively from European colonial ventures, which pitted European powers against each other in a scramble for the best territories (Feyrer and Sacerdote, 2009). Similarly, the timing of communist and anti-communist revolutions in Eastern Europe and Central Asia is largely the product of exogenous shocks (Beck and Laeven, 2006: 159). The communist take-overs took place in the wake of the two World Wars. The anti-communist uprisings of 1989-1990 coincided with the end of the Cold War era. In any case, instrumenting for $OTTO_i$ (besides accounting for any endogeneity bias resulting from measurement error) allows me to also address the possibility that the estimates may reflect the impact of omitted determinants. In my baseline regressions, I also assume that contemporaneous income is exogenous to corruption. Still, the findings are qualitatively consistent with a 2SLS specification that relaxes the exogeneity assumption by instrumenting for income

[15] The 1821-32 Greek War of Independence led to the liberalization of about half the territory of modern Greece.

TABLE 1: Ottoman and Socialist Rule

i	SOC_i	Period(s) of Ottoman Rule	$OTTO_i$	i	SOC_i	Period(s) of Ottoman Rule	$OTTO_i$
Turkey	0	1350-1923	5.73	Hungary	0.40	1526-1699	1.73
Macedonia	0.46	1371-1913	5.42	Croatia*	0.48	1527-1691	1.64
Greece*	0	1372-1912	5.40	Yemen*	0.20	1518-1636; 1873-1919	1.64
Kosovo	0.47	1390-1912	5.22	Oman*	0	1660-1742	0.82
Montenegro	0.47	1390-1879	4.89	UAE*	0.72	1660-1742	0.82
Serbia*	0.47	1390-1879	4.89	Sudan*	0	1820-1882	0.62
Bulgaria	0.45	1395-1878	4.83	Eritrea*	0.17	1847-1882	0.35
Albania	0.46	1468-1912	4.44	Afghanistan	0.10		0
Romania*	0.42	1476-1878	4.02	Armenia	0.69		0
Israel	0	1518-1918	4.00	Azerbaijan	0.69		0
Jordan	0	1518-1918	4.00	Belarus	0.69		0
Lebanon	0	1518-1918	4.00	Czech Republic	0.41		0
Syria	0	1518-1918	4.00	Estonia	0.51		0
Saudi Arabia*	0	1517-1916	3.99	Ethiopia	0.17		0
Bosnia	0.49	1482-1878	3.96	Kazakhstan	0.55		0
Iraq*	0	1534-1918	3.84	Kyrgyzstan	0.55		0
Libya*	0	1544-1911	3.67	Latvia	0.46		0
Egypt*	0	1517-1882	3.65	Lithuania	0.50		0
Kuwait*	0	1536-1623; 1638-1898	3.47	Poland	0.45		0
Algeria*	0	1516-1830	3.14	Russia	0.74		0
Moldova	0.51	1504-1812	3.08	Slovak Republic	0.41		0
Cyprus	0	1571-1878	3.07	Slovenia	0.46		0
Tunisia*	0	1575-1881	3.06	Tajikistan	0.62		0
Ukraine*	0	1476-1774	2.98	Turkmenistan	0.66		0
Georgia*	0.69	1575-1829	2.54	Uzbekistan	0.67		0

Sources: author's calculations based on data from Brown, 1996; O'Brien, 2007; and the *GeaCron Project* (http://geacron.com/). The cleft countries are marked with an asterisk. The research and coding was done jointly with Can Cinar (University of Otago). The computation of $OTTO_i$ assumes that Ottoman rule began and ended midway through the reported year. If so, the length can be computed by subtracting the year marking the beginning of rule from the year marking the end.

Following Treisman (2000: 416), my baseline estimates of equation (5) use Weighted Least Squares (WLS) – a strategy which seeks to address the fact that the dependent variable is measured with error.[16] Each observation is weighted by the inverse of the variance of corruption ratings for each country.[17] The WLS estimator places greater emphasis on those observations that are based on more reliable corruption data, producing more efficient estimates. Still, my substantive results do not depend critically on the choice of WLS over standard OLS.

4. EMPIRICAL RESULTS

4.1 WLS Estimates

The baseline estimates of eq. (1) are presented in Table 2. Model 1 regresses average corruption levels during 2000-7 on the historical legacy variables alone. Both $OTTO_i$ and SOC_i enter positive, as expected, but are not statistically significant at conventional levels. The estimated coefficients, however, do not identify a pure *corruption* effect if historical legacies also influence contemporary corruption indirectly through other plausible routes, e.g. by affecting contemporary economic development.[18] Regarding $OTTO_i$, a statistically insignificant estimate may result if the total legacy effect is the mix of two opposite influences: an adverse direct effect on corruption, and a beneficial indirect effect through economic development. The latter may result because MENA countries are overrepresented in the sample, and for these countries, unlike for European countries, the Ottoman legacy may have had a positive impact on development. If so, omitting GDP from the regression biases the estimate of $\hat{\alpha}$ downwards. By contrast, omitting GDP is not expected to substantially bias the estimated effect of socialism ($\hat{\beta}$). A more detailed discussion of the economic legacy of Ottoman and socialist rule, and the consequences of omitting GDP, is presented in Appendix IV. In any case, given the theory and the large body of evidence on the close relation between corruption and development, a model that assumes income to have no effect on corruption is likely to be seriously misspecified.

Model 2 includes all the three main variables of interest: $OTTO_i$ and SOC_i and GDP per capita. As expected, conditioning on income significantly increases the magnitude of the estimated effect of Ottoman rule, whereas the estimated effect of socialism remains unchanged, but gains statistical significance.[19] Column 3 reports a more extensively specified model that controls for other potential determinants of corruption, based on the discussion in section 2, while models 4 and 5 enter $OTTO_i$ and SOC_i individually, as in previous studies of Ottoman (Dimitrova-Grajzl, 2007; Grosjean, 2011) and socialist legacies (Beck and Laeven, 2005).

Even when $OTTO_i$ and SOC_i are entered simultaneously (models 2-3), however, neither $\hat{\alpha}$ nor $\hat{\beta}$ loses statistical significance. In addition, all models indicate a strong negative association between per-

[16] The 2SLS regressions that follow employ analytical weights to the same effect.
[17] The variances are obtained by squaring the observations' standard errors, which are provided by the World Bank, and averaging over 2000-7.
[18] Additionally, Ottoman rule might be expected to have increased ethno-religious fractionalisation.
[19] A cross-model Wald test cannot reject the null that the coefficients on SOC_i are equal to each other (*p*-value = 0.948). The cross-model equality of the coefficients on $OTTO_i$ is marginally rejected at the 10% level (*p*-value = 0.109).

capita income and corruption, confirming theoretical expectations and past empirical findings. All the other coefficients are either statistically insignificant or, whenever they are statistically significant, they enter with the expected sign. For instance, oil dependence and (religious) fractionalisation are estimated to have an adverse impact on corruption, in line with the arguments presented in section 2. The only exception is ethno-linguistic fractionalisation, which is estimated to have an alleviating effect on corruption. Although this is contrary to expectations, the effect is only significant at 10% in models 4 and 5, and is far from robust.

TABLE 2: Determinants of Corruption, Baseline Results

Estimator:	WLS (1)	WLS (2)	WLS (3)	WLS (4)	WLS (5)	2SLS (6)	2SLS (7)
Ottoman Rule ('00 yr)	0.011	0.056**	0.056**	0.044*		0.060*	0.087**
	(0.041)	(0.026)	(0.023)	(0.024)		(0.034)	(0.042)
Socialist Rule ('00 yr)	0.398	0.413**	0.589***		0.517***	0.593***	0.538**
	(0.305)	(0.194)	(0.186)		(0.192)	(0.191)	(0.216)
Log GDP per capita		-0.376***	-0.423***	-0.439***	-0.393***	-0.425***	-0.524***
		(0.039)	(0.049)	(0.052)	(0.049)	(0.051)	(0.083)
Log Oil Exports			0.076***	0.085***	0.068***	0.079***	0.084***
			(0.025)	(0.026)	(0.025)	(0.025)	(0.027)
Log Aid			-0.041	-0.031	-0.022	-0.040	-0.091
			(0.042)	(0.045)	(0.043)	(0.044)	(0.059)
Islam (share pop)			0.335*	0.165	0.365**	0.317*	0.289
			(0.173)	(0.177)	(0.180)	(0.178)	(0.195)
Protestant (share pop)			-1.790*	-1.995*	-2.261**	-1.747	-1.362
			(1.021)	(1.099)	(1.046)	(1.049)	(1.202)
Ethnolinguistic Fract.			-0.188	-0.407*	-0.358*	-0.175	-0.155
			(0.215)	(0.220)	(0.212)	(0.229)	(0.241)
Religious Fract.			0.566**	0.808***	0.657**	0.567**	0.624**
			(0.253)	(0.260)	(0.261)	(0.260)	(0.286)
Constant	0.165	3.075***	3.123***	3.524***	3.035***	3.134***	3.856***
	(0.245)	(0.318)	(0.423)	(0.436)	(0.440)	(0.428)	(0.642)
F-test (1-stage *OTTO*)						51.4***	17.2***
F-test (1-stage GDP)							20.7***
Sargan test [*p*-value]							[0.739]
Durbin χ^2 [*p*-value]						[0.757]	[0.239]
Observations	64	64	64	64	64	63	61
R-squared	0.03	0.61	0.80	0.76	0.78		

Notes: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. In all models, the inverse of the variance of corruption ratings are used as analytical weights. Years of Ottoman Rule is treated as endogenous in model 6 (instrument: distance from Constantinople), while models 7 treat both Years of Ottoman Rule and Log (GDP per capita) as endogenous (instruments: distance from Constantinople, percentage of land within 100 km of coast, share of exports in GDP). First-stage results not reported to save space. The Durbin χ^2 score tests whether the variables that are specified as endogenous can be treated as exogenous. The Sargan test is a test of instrument validity. The F-test is a test of the joint significance of the instruments in the first-stage regressions.

In additional tests not reported in Table 3, I find that the point estimates of the variables of interest remain significant if I compute the standard errors by bootstrapping with 100 iterations, as in Dimitrova-Grajzl (2007: 550), and also after excluding two somewhat influential observations from the

sample (Chad and Hungary).[20] The unweighted OLS estimates of the extensive specification (not reported) are all within about 13 percent of the WLS estimates reported in column 3.

4.2 IV Regressions

A potential concern with the WLS estimates of the income effect is that they might reflect the potential effect of corruption on economic growth (Mauro, 1995), or the influence of omitted confounders. While the former would inflate the OLS estimates, the latter may bias the estimates in either direction.[21] At the same time, the historical duration of Ottoman rule is measured with error, leading to potential attenuation. To address these concerns, I also run instrumental-variable (2SLS) regressions.[22]

Model 6 instruments for the length of Ottoman rule using the present-day country's geographical distance from Constantinople (see Appendix III for details). The Ottoman Empire was a regional power whose expansionary ambitions were largely limited to its neighbouring territories (Inalcik, 1978). Accordingly, the duration of Ottoman rule in a given country is highly negatively correlated with its distance from Istanbul (coeff. = -0.66).[23] The identifying assumption for instrument validity is that (conditional on the other regressors) the distance from Constantinople has no independent impact on corruption beyond its effects working through the duration of Ottoman rule.[24] I contend that this assumption is plausible, and a statistically insignificant Sargan test statistic (model 7) is consistent with this interpretation. Yet, a justified concern might be that geographical remoteness might influence not just the length of Ottoman rule, but also its quality. Local elites in far-flung provinces may be more difficult to monitor and control, leading to a higher scale of corruption.[25] For this reason, the results should be interpreted with caution. Bearing this in mind, treating $OTTO_i$ as endogenous produces estimates of the effect of Ottoman rule that are only about 7 percent higher than the corresponding WLS coefficients (model 3), suggesting that measurement error may not be a significant cause of attenuation.

To isolate an exogenous component of economic development, model 7 also instruments for GDP using the fraction of a country's land within 100 km from an ice-free coast (as in Angeles and Neanidis, 2015), and the (lagged) share of export revenues in total GDP. Coastal proximity (coeff. = 0.50) and export orientation (coeff. = 0.42) are highly correlated with income. Coastal proximity influences a country's climate, which, in turn, has been argued to affect agricultural productivity and the disease environment (Mellinger et al., 2000). Export orientation has long been known to promote economic growth (Balassa, 1978). Conditional on the other second-stage regressors, these two instruments should

[20] Influential observations are defined as those that exert more than twice the average leverage.

[21] For instance, democratization may both increase per-capita income and reduce corruption, leading to an upward bias in the OLS estimates. However, if democratisation stimulates economic growth but leads to higher corruption in the short-run (Mohtadi and Roe, 2003; Rock, 2009), the OLS estimator is biased towards zero.

[22] To address the poor finite-sample properties of 2SLS, I employ a variance estimator that makes a degrees-of-freedom adjustment (Cameron and Trivedi, 2005: 102).

[23] Distance alone explains some 44 percent of the total variation in length of rule in the full sample and 58 percent in a sub-sample of 31Ottoman successor states.

[24] Additionally, the instrument is unlikely to be correlated with the (non-classical) measurement error subsumed in the second-stage error term, as being a cleft country is only weakly related to the distance from Constantinople.

[25] Still, the balance of historical evidence supports the view that, in the 18th century at least, *all* provinces of the Empire (including those close to Constantinople, e.g. Anatolia, Thrace and Bulgaria) were run by local notables turned warlords who collected illegal levies and held back most taxes formally owed to the central government (Inalcik and Quataert, 1994: 658-77).

only be expected to influence corruption through their effect on economic development. Barring an implausible theory of geo-climatic determinism, coastal proximity should have no direct impact on corruption (Lewis and Wigen, 1997). While exposure to *imports* may undermine the rents of domestic producers, thereby mitigating corruption and rent-seeking (Ades and Di Tella, 1999), there is no reason to expect *export* orientation to have an analogous effect.[26]

In the first-stage (note reported), all the instruments, enter with the expected sign and significance level, with an F-statistic of joint significance greater than 10. In this specification, the coefficient on $OTTO_i$ increases quite substantially (55 percent) relative to the corresponding WLS model, but so does the effect of income (by 24 percent), leaving our substantive conclusions unchanged. The coefficient on SOC_i is fairly stable throughout. It should be noted, though, that a Durbin χ^2 test cannot reject the null that the specified endogenous regressors may be treated as exogenous. If so, the gain in reliability from using 2SLS may not be worth the associated loss of precision.

4.3 Interpretation of the Results

Taken together, the results reported in Table 2 provide convincing evidence that the incidence of corruption in transition economies may be influenced both by the recent legacies of socialism and by the legacies of the more distant past. The statistical and economic significance of $\hat{\alpha}$ and $\hat{\beta}$ imply that the institutional routines established under Ottoman and socialist rule had an adverse impact on corruption, *and* that these routines have persisted over time, leaving a 'trace' that is observable in the relative incidence of corruption across countries today. While it is certainly plausible that 'developments during the socialist period […] were affected by the culture [and institutions] formed by the Ottoman/Habsburg legacies' (Dimitrova-Grajzl, 2007: 548), our findings suggest that socialist rule left an additional, independent mark on corruption over and above the effect of pre-socialist legacies. The findings are consistent with the explanation that socialist rule increased corruption by further promoting elite entrenchment, though this explanation cannot be tested directly using my data. Overall, the results lend credence to *both* the main rival explanations of post-socialist corruption advanced in the transition literature. The role of socialism and Ottoman history should be seen as complementary elements of an account of post-socialist corruption.[27]

The magnitude of the socialist legacy effect, however, is 6-10 times larger than the Ottoman legacy effect. This result is consistent with the proposition that historically established routines have a tendency to 'decay' over time. Since Ottoman rule preceded socialism, 'decay' implies that the legacy effect today of an additional year of Ottoman rule should be smaller than the effect of one additional year of socialist rule, all else equal. Still, the relative magnitude of the estimated coefficients may be

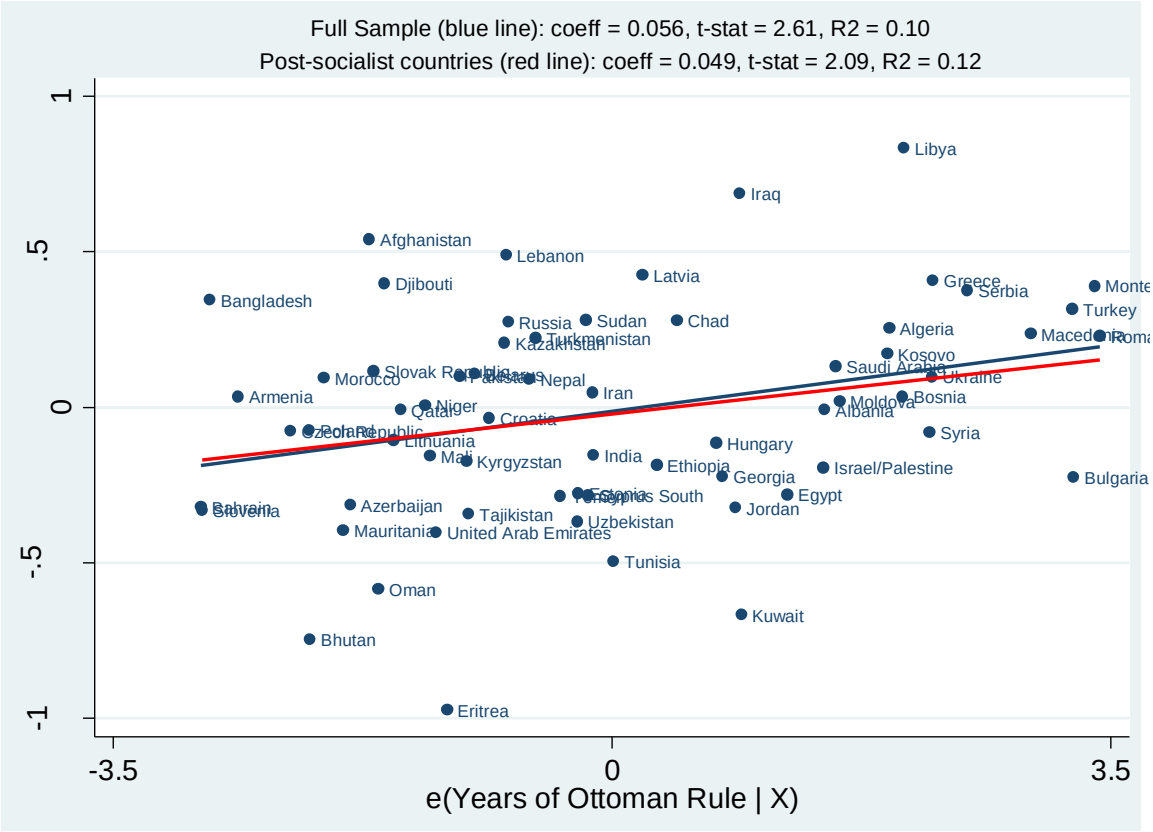
[26] In the sample, coastal proximity and export orientation are almost exactly orthogonal (coeff = -0.004). Coastal proximity is also unrelated to import dependence (coeff = -0.028), which might have an alleviating effect on corruption independently of economic growth (Ades and Di Tella, 1999). In any case, including the share of imports in GDP as an additional control in the second-stage regression does not significantly alter the 2SLS estimates (results available upon request).

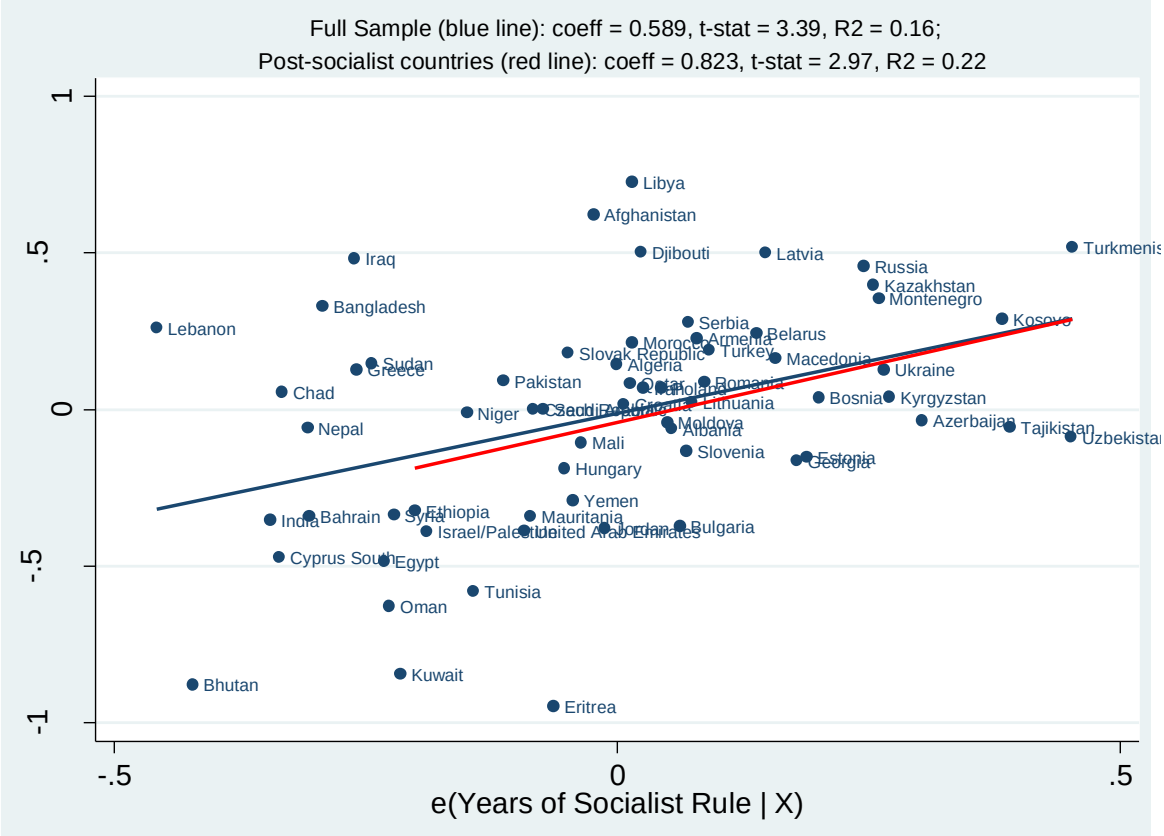
[27] In addition, there is no evidence of an interaction effect between them – that is, there is no evidence that the Ottoman legacy on corruption was *itself* exacerbated by socialist rule (the results of the interaction model are available upon request).

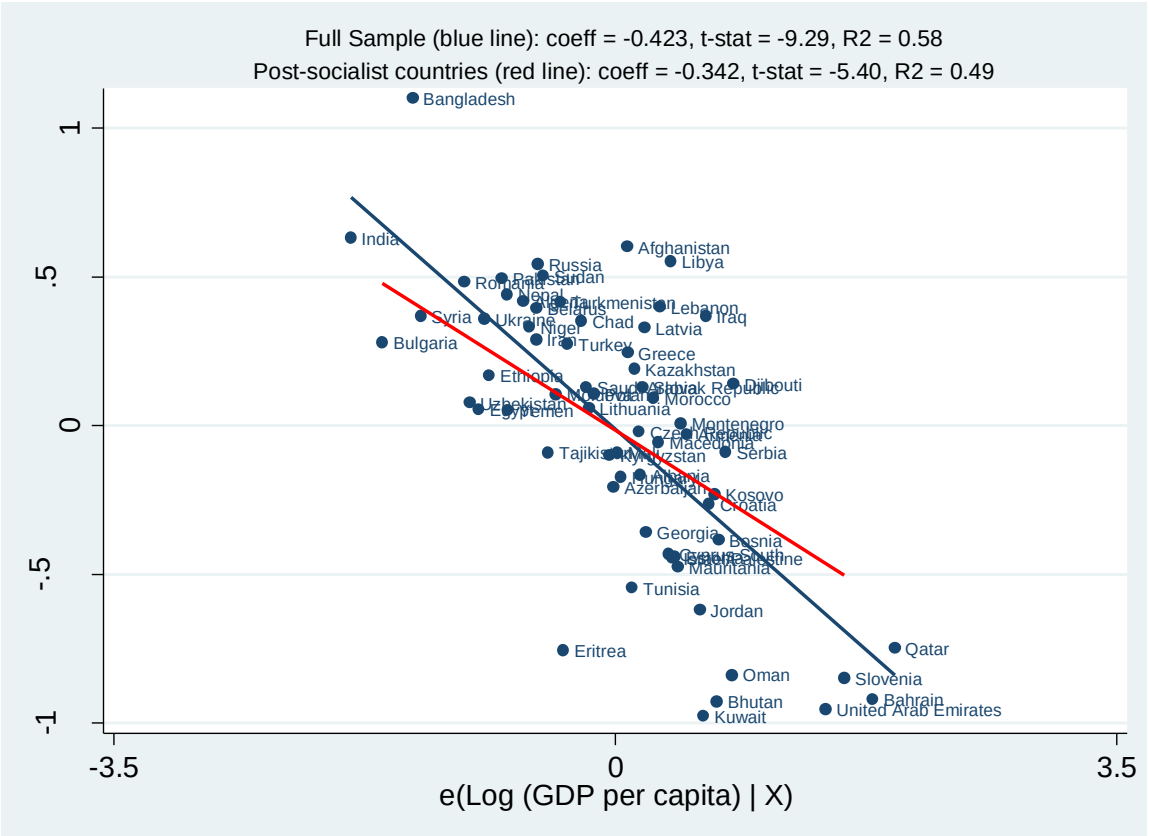
reconciled with alternative explanations (see Appendix I). A larger effect associated with the socialist legacy, for instance, may result from socialism having a more detrimental 'impact' shock on corruption than Ottoman rule. It is not possible to distinguish between these alternative mechanisms using our data.

Regardless of their relative magnitude, the estimated effects indicate that history can be a 'burden' for subsequent institutional change. Economic development, I argued, generates new incentives and opportunities to entrench the rule of law and weed out corruption, as evidenced by richer countries being less 'corrupt'. Still, all else equal, countries with more unfavourable historical legacies tend to remain relatively more 'corrupt' even as they develop economically – although, of course, their 'disadvantage' might be progressively eliminated if historically inherited routines decay and, eventually, disappear, leaving more room for other influences to play out.

FIGURE 2: Partial Correlation Plots: Years of Ottoman Rule, Years of Socialist Rule, Log(GDP per capita)







Notes: all plots are based on Model (3) in Table 3. The diagrams plot the residuals obtained from regressing the outcome (corruption) and the variate of interest (e.g. ln GDP per capita) on all the other covariates (X).

Although statistically significant, the overall contribution of historical legacies to present-day corruption is, however, not very large. Based on the estimates of model 3 (Table 2), it takes about 100 additional years of Ottoman domination, or 10 additional years of socialist rule, for present-day countries

to experience an increase in corruption of 0.06 index points, roughly the difference between Albania and Russia (Figure 1). Alternatively, a one-standard-deviation increase in the duration of Ottoman and socialist rule increases corruption today by 16 and 23 percent of a standard deviation, respectively. By contrast, a one-standard-deviation increase in per-capita income reduces corruption by 88 percent of a standard deviation – an effect which is 5.5 times as large as the effect of Ottoman rule, or 3.8 as large as that of socialist rule. The historical legacy variables have low explanatory power. Confirming this finding, removing the Ottoman and socialist legacy variable reduces the model's R^2 by only 2 and 4 percentage points, respectively (Table 2, models 4 and 5). Omitting the log of per-capita GDP (not shown), in contrast, leads to as much as a 28 percentage-point drop in goodness of fit.[28]

The basic results from Table 2 are illustrated visually in the partial correlation plots shown in Figure 2. The diagrams demonstrate that the statistical relationships are not driven by outliers. They also indicate that the partial effect of each of the three variables of interest (after partialling out the influence of the other covariates based on the full sample) does not change substantially in the sub-sample of post-socialist transition economies. Based on the parameter estimates from Model 3 (Table 2), Ottoman/socialist legacies explain, respectively, 12 and 22 percent of the total variance in corruption amongst 33 post-socialist countries (red curves in Figure 2). Income differences, by contrast, account for 49 percent of the observed variance – between 2.2 and four times as much. Thus, although they find some confirmation in the data, historical heritage arguments explain a lesser share of the variation in corruption levels today. Most (but not all) of the corruption that engulfs transition economies today is entirely unrelated to their socialist or 'Oriental' histories. On the contrary, the largest fraction of cross-country variation is driven by differences in levels of economic development. This result resonates both with past empirical findings and with recent theories of the causes of corruption (Khan, 2005; Uberti, 2016). Not least, it accords with the notion that historical legacies have a tendency to dissipate

Of course, while the explanatory power of history is low *on average*, the influence of history varies across different country pairs. For purely illustrative purposes, consider the case of Poland and Kosovo, two countries that lie almost exactly on the regression line. While Poland is comparable to Greece or Portugal in terms of level of economic development, Kosovo is comparable to Morocco or Indonesia. Kosovo spent 522 years under Ottoman rule (and is now overwhelmingly Muslim), whereas no part of Poland was ever conquered by the Porte. Furthermore, socialism lasted two additional years in Kosovo compared to Poland. Accordingly, the level of (perceived) corruption today is much higher in Kosovo (0.623) than in Poland (-0.280). How much of this difference is due to the contemporary income gap, and how much to the 'burden' of history? The point estimates in Model 3 (Table 2) imply that the more favourable historical legacies inherited by Poland (relative to Kosovo) account for 33.6 percent of the observed difference in corruption levels. Income differences, however, explain 59.1 percent of the observed difference – a significantly larger share.[29]

[28] This ΔR^2 is the product of the amount of observed variation left unexplained by the restricted model and the partial correlation between the omitted covariate and the dependent variable (Greene, 2003: 34).

[29] These factors jointly account for 92.7 percent of the observed difference. The remaining difference is due to Kosovo being majority Muslim and to other unobserved country characteristics.

5. ROBUSTNESS ANALYSIS

To further explore the persistence of historical legacies on corruption, I conduct a number of robustness tests.[30] In Table 3, I test the robustness of my conclusions to alternative operationalisations of the main variables of interest. Model 1 measures corruption using Transparency International's *Corruption Perception Index* instead of the World Bank's Control of Corruption indicator.[31] Model 2 uses a simple dummy variable to control for socialist legacies instead of my detailed measure of the length of socialist rule. Model 3 distinguishes between the socialist regimes of East-Central Europe and those of the Third World and Central Asia (e.g. Tajikistan, Ethiopia). Model 4 uses a weighted measure of the length of socialist rule that controls for whether a socialist regime tolerated a modicum of individual freedom and openness to the West (as per Dimitrova-Grajzl, 2007: 549). Finally, Model 5 uses a PPP-adjusted measure of GDP, instead of GDP at market-exchange rates. All the evidence presented in Table 3 corroborates my previous findings.

TABLE 3: Robustness Analysis - Alternative Measures of Corruption, Socialism and Income

Dependent Variable:	TI	WGI	WGI	WGI	WGI
Estimator:	OLS	WLS	WLS	WLS	WLS
	(1)	(2)	(3)	(4)	(5)
Ottoman Rule ('00 yr)	0.068**	0.050**	0.049**	0.056**	0.048*
	(0.027)	(0.024)	(0.024)	(0.023)	(0.025)
Socialist Rule ('00 yr)	0.620***				0.737***
	(0.220)				(0.206)
Socialist Dummy		0.242**			
		(0.108)			
Socialist Rule (Europe)			0.734***		
			(0.231)		
Socialist Rule (MENA)			0.449*		
			(0.235)		
Socialist Rule (Weighted)				0.277***	
				(0.098)	
Log GDP per capita, FX$	-0.445***	-0.430***	-0.428***	-0.406***	
	(0.060)	(0.051)	(0.049)	(0.051)	
Log GDP per capita, PPP					-0.433***
					(0.061)
Control Variables & Constant	Yes	Yes	Yes	Yes	Yes
Observations	63	64	64	64	64
R-squared	0.73	0.78	0.80	0.79	0.75

Notes: standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. TI stands from Transparency International; WGI stands for Worldwide Governance Indicators. FX$ = current dollars at market exchange rates; PPP = purchasing power parity.

Table 4 explores the robustness of my results to the inclusion of additional controls from the corruption literature. For ease of comparison, column 1 reprints my baseline WLS model (column 3, Table 2). Models 2 and 3 condition on other (potentially relevant) exogenous historical characteristics,

[30] I conduct these tests using WLS, rather than 2SLS, in order to increase efficiency.
[31] Because of missing values in this indicator's standard errors, I fit the model with OLS rather than WLS.

namely the identity of the former European coloniser and EU membership.[32] Model 4 controls for the total number of years of civil and intra-state conflict during 1946-2007, while model 5 includes the share of import expenditure in total GDP (Ades and Di Tella, 1999). In all cases, the results are qualitatively unchanged. Of particular interests are the regressions reported in column 6 and 7, which condition on *contemporary* institutional and policy characteristics. As mentioned earlier, the transition literature has suggested that successful progress in democratization and marketization might contribute to containing corruption; reform laggards, in contrast, tend to remain more 'corrupt' (Hellman, 1998; Frydman et al., 1998; Beck and Laeven, 2006). To incorporate these arguments, models 6 and 7 include a lagged average (1997-2003) of the *Freedom House Index* and the Heritage Foundation's *Economic Freedom Index*.[33]

TABLE 4: Robustness Analysis – Additional Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Ottoman Rule ('00 yr)	0.056**	0.052**	0.058**	0.052**	0.054**	0.042*	0.027
	(0.023)	(0.024)	(0.025)	(0.023)	(0.023)	(0.025)	(0.024)
Socialist Rule ('00 yr)	0.589***	0.514*	0.590***	0.663***	0.809***	0.535**	0.402**
	(0.186)	(0.264)	(0.188)	(0.211)	(0.219)	(0.200)	(0.190)
Log GDP per capita	-0.423***	-0.420***	-0.427***	-0.403***	-0.371***	-0.434***	-0.384***
	(0.049)	(0.050)	(0.052)	(0.056)	(0.055)	(0.050)	(0.051)
British Colony		-0.096					
		(0.147)					
French Colony		0.009					
		(0.169)					
Italian Colony		0.089					
		(0.302)					
EU			0.041				
			(0.197)				
Log years of conflict				0.017			
				(0.022)			
Imports / GDP (lagged)					-0.008**		
					(0.003)		
Democracy (lagged)						-0.075	
						(0.047)	
Economic Freedom (lagged)							-0.012**
							(0.005)
Controls & Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	64	64	64	64	62	61	58
R-squared	0.80	0.80	0.80	0.80	0.82	0.81	0.84
Adjusted R-squared	0.77	0.76	0.76	0.76	0.78	0.78	0.81

Notes: WLS regressions with standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. The parameters of the other regressors are not reported to save space.

Both variables enter with the expected sign. In fact, the estimated coefficient on $OTTO_i$ decreases in magnitude (and significance) when democracy and marketisation are controlled for. Speculatively, this could suggest that part of the negative long-run impact of Ottoman rule on corruption runs through the

[32] I do not code Romania and Bulgaria as EU members since they only joined in 2007. Still, their inclusion does not alter my results.

[33] To avoid spurious correlations, I expunge the corruption and property rights components from the original Economic Freedom Index. The labour component is expunged to increase the number of years in the average.

quality of (contemporary) democracy and economic regulation. As the socialist regimes imploded, the ruins of pre-socialist institutions re-emerged powerfully, influencing the actual operation of the new institutions of political pluralism and economic freedom (which in turn had an impact on the scale of corruption). That said, these models should be interpreted with caution. Perception-based measures of corruption may be highly correlated with other (semi-contemporaneous) institutional characteristics simply because they reflect the same underlying perceptions of the institutional environment (Treisman, 2007: 222). Although part of this problem is addressed by lagging and using institutional and corruption variables from different sources, the possibility of spurious correlation cannot be ruled out completely.

TABLE 5: Robustness Analysis – Sub-samples

	Full sample (1)	Without cleft (2)	Post-socialist (3)	Post-Ottoman (4)	Without belt (5)	Europe (6)
Ottoman Rule ('00 yr)	0.075***	0.073***	0.068***	0.142***	0.083***	0.089**
	(3.60)	(3.07)	(3.06)	(2.83)	(3.28)	(2.63)
Socialist Rule ('00 yr)	0.613***	0.612***	1.060***	0.307	0.705***	0.620***
	(3.96)	(3.47)	(3.01)	(1.01)	(3.42)	(2.76)
Log GDP per capita	-0.414***	-0.411***	-0.364***	-0.421***	-0.396***	-0.413***
	(13.16)	(11.44)	(8.85)	(6.64)	(9.93)	(9.22)
Log Oil Exports	0.113***	0.116***	0.126***	0.131***	0.122***	0.109***
	(6.27)	(5.23)	(4.91)	(3.29)	(5.82)	(4.58)
Europe						-0.065
						(0.18)
Europe × Ottoman Rule						-0.022
						(0.44)
Europe × Socialist Rule						0.173
						(0.31)
Other control variables	No	No	No	No	No	No
Observations	64	46	33	31	50	64
Adjusted R-squared	0.75	0.77	0.83	0.62	0.76	0.74

Notes: WLS regressions with *t*-statistics in parentheses; *** p<0.01, ** p<0.05, * p<0.1. The constant is not reported to save space.

In Table 5, I check the extent to which my results hold in a number of sub-samples. Because restricting the sample reduces the degrees of freedom, I compare the estimates from the restricted samples to a more parsimonious model with only oil exports as a control (model 1). Model 2 excludes the cleft countries, which are known to introduce attenuation bias in the estimates of $OTTO_i$. Model 3 only includes post-socialist countries, while model 4 only includes Ottoman successor states. The results are qualitatively unaltered, the only exception being that socialist legacies do not appear to increase corruption (at conventionally significant levels) in the post-Ottoman sub-sample. Also, socialist legacies have a significantly *larger* impact in a sub-sample of post-socialist countries, in which SOC_i has *less* variance (coeff = 1.060, compared to 0.613 in the full sample). This finding suggests that existing studies (Beck and Laeven, 2006) might be over-stating the influence of socialist legacies. The results are also practically unchanged after dropping the 'belt' of adjacent countries that never experienced either Ottoman or socialist rule – though, of course, the estimates are slightly less precise (model 5).

To investigate whether the magnitude of the historical legacy effects changes systematically across macro-regions, I introduce a continent dummy for Europe and interact it with $OTTO_i$ and SOC_i (model 6). The long-run impact of Ottoman rule on corruption is estimated relative to the (average) magnitude of other, non-Ottoman pre-socialist legacies. This implicit 'reference category' comprises a rather diverse collection of historical polities: Prussia, and the Habsburg and Russian Empires in Europe, and the likes of the Safavid Empire or the Moroccan Sultanates in MENA. Thus, splitting the sample along the Europe/MENA divide allows me to test whether the institutional legacies of Ottoman rule might have been *less* favourable than those of other pre-socialist European polities but nevertheless *more* favourable that those of other non-Ottoman pre-modern states in MENA. This hypothesis is firmly rejected by the data. Both the Europe dummy and its interaction with $OTTO_i$ are statistically insignificant, indicating both that Europe is not, *qua* Europe, less 'corrupt' than MENA, and that the Ottoman legacy on corruption, however small in magnitude, is observable across *all* macro-regions of this vast historical empire.

TABLE 6: Robustness Analysis – Controlling for the Effect of Other Empires

	(1)	(2)	(3)	(4)	(5)	(6)
Ottoman Rule ('00 yr)	0.056**	0.058**	0.060**	0.066**	0.076***	0.066***
	(0.023)	(0.024)	(0.023)	(0.029)	(0.027)	(0.024)
Socialist Rule ('00 yr)	0.589***		0.475**	0.444	0.558**	0.473*
	(0.186)		(0.226)	(0.336)	(0.226)	(0.247)
Russian Rule ('00 yr)		0.200**	0.087		0.021	0.095
		(0.082)	(0.096)		(0.106)	(0.097)
Safavid Rule ('00 yr)						0.048
						(0.072)
Mughal Rule ('00 yr)						0.071
						(0.099)
Log GDP per capita	-0.423***	-0.421***	-0.418***	-0.411***	-0.413***	-0.397***
	(0.039)	(0.056)	(0.049)	(0.057)	(0.038)	(0.056)
Log Oil Exports	0.076***	0.077***	0.075***	0.066**	0.111***	0.076***
	(0.025)	(0.025)	(0.025)	(0.029)	(0.024)	(0.025)
Control variables & constant	Yes	Yes	Yes	Yes	No	Yes
Observations	64	64	64	48	44	64
Adjusted R-squared	0.77	0.75	0.76	0.72	0.76	0.76

Notes: WLS regressions with standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; the parameters of the other regressors are not reported to save space.

A lingering concern with my estimates is that they do not explicitly model the effect of other imperial legacies, which might introduce bias. Anecdotal evidence suggests that Russian imperial rule in Eastern Europe promoted corruption, while the Habsburg and Prussian states left behind a legacy of rule-following behaviour in the public administration, as evidenced in the East/West divide in contemporary Poland (Moller and Skaaning, 2010) or the North-West/South-East divide in contemporary Romania (Becker et al., 2014). If so, measuring the corruption effect of the Ottoman Empire against a mixed reference category comprising the (highly 'corrupt') Russian Empire might produce misleading results.

More worryingly, Russian imperial rule is highly collinear with the incidence of socialist rule, as countries that were still part of the Russian Empire at the time of the October Revolution (e.g. Ukraine, Belarus) went communist at the same time as Russia. Thus, my estimate of the corruption effect of socialism may be spuriously picking up the effect of an earlier historical legacy.

To address these concerns, I construct estimates of the number of years of Russian imperial rule for my sample countries (see Appendix V). The sources and methodology employed to code this variable are the same as the ones used to construct $OTTO_i$. Model 1 in Table 6 reprints my baseline model. As shown in column 2, the Russian rule variable enters positive and significant in a model that does *not* control for the length of socialist rule. Russian rule, however, loses significance when SOC_i is included (model 3). As expected, once the effect of Russian domination is factored out, the coefficient on $OTTO_i$ increases in magnitude (but only by about 7 percent), while the coefficient on SOC_i decreases by about 19 percent, suggesting that earlier estimates of the effects of socialist rule might also partly reflect the influence of pre-socialist legacies. Model 4 factors out the impact of Russian imperial legacies by restricting the sample to countries that were never ruled by the Russian Empire. Reassuringly, the point estimates are very similar – although, as expected, the standard errors are uniformly larger.

Model 5 excludes the cleft countries (see Appendix V), while model 6 checks for the sensitivity of the estimates to the choice of reference category on the Eastern flank of the Ottoman borders. The Safavid and Mughal Empires were two of the most important early-modern empires that ruled parts of the Middle East and South-Asia during the 16-18th century, often entering into military confrontation with the Ottoman Empire. Due to their similarities, these three states are sometimes referred to collectively as the 'gunpowder empires' (Hodgson, 1977). Controlling for the length of Safavid and Mughal rule (see Appendix V), however, does not significantly alter my results.

Lastly, I check the sensitivity of my results to incorporating several alternative time-discounting schemes. As discussed earlier (see also Appendix I), the magnitude of the legacy of an historical event is a function of: (a) the event's distance in time; (b) the magnitude of the shock originally imparted by the event; and (c) the propensity of the ensuing (corruption) routine to persist over time (the time-discount rate). While (a) can be observed, (b) and (c) are both unobserved, so it is impossible to recover an estimate of the impact effect and time-discount rate from the regression parameters.

That said, it is instructive to check the sensitivity of the regression results to alternative assumptions regarding persistence. To do so, I re-estimate my baseline model (model 3, Table 2) using alternative measures of the duration of Ottoman and socialist rule that discount the contribution of past years by an exponential factor η (see Appendix II for details of how the time-discounted variables are constructed). The interpretation of η is that in any given year only $100 \times \eta$ percent of the corrupt transactions concluded in the previous year take place again. When $\eta = 1$, the corruption routines induced by a given historical shock persist fully into the future. Dimitrova-Grajzl (2007: 545) assumes (rather plausibly) that the legacy of Ottoman rule halves in size every 301 years (= 2001-1700), which implies that $\eta = \exp(\ln 0.5/301) = 0.998$.[34] Table 7 checks the sensitivity of the estimated parameters to building $OTTO_i$ and SOC_i on different values of η. Column 1 assumes that $\eta = 0.\overline{9} \cong 1$ (the results are

[34] From $0.5 = \eta^{301}$, take logs (with base η), apply the change of base formula, re-arrange and exponentiate.

practically identical to those in Table 2). Columns 2-5 allow for a progressively increasing tendency of corruption routines to 'decay' over time, with η ranging from 1 to 0.96 in 0.01 intervals.

TABLE 7: Sensitivity Analysis – Alternative Time-Discounting Schemes

	(1)	(2)	(3)	(4)	(5)
Time-discount rate (η)	1	0.99	0.98	0.97	0.96
Ottoman Rule ('00 yr)	0.056**	0.762**	4.083***	15.069***	47.840**
	(2.43)	(2.59)	(2.78)	(2.78)	(2.71)
Socialist Rule ('00 yr)	0.589***	0.969***	1.425***	1.967***	2.623***
	(3.16)	(3.39)	(3.44)	(3.34)	(3.26)
Log GDP per capita	-0.423***	-0.429***	-0.433***	-0.433***	-0.433***
	(8.67)	(8.84)	(9.01)	(9.04)	(9.01)
Control variables & constant	Yes	Yes	Yes	Yes	Yes
R-squared	0.799	0.801	0.804	0.803	0.800

Notes: WLS regressions with *t*-statistics in parentheses; *** p<0.01, ** p<0.05, * p<0.1; the parameter estimates of the other covariates are not reported to save space. The number of observations is 64.

In terms of statistical significance, the results are consistent throughout. Yet, in light of the model presented in Appendix I, the interpretation of the point estimates changes. So far, I have interpreted the regression coefficients as measuring the long-run *legacy* of one year of rule: how much corruption would increase today had a country been subject to one additional year of rule *in the past*. The coefficients reflect both the original impact of the event *and* the subsequent process of historical 'decay'. Alternatively, I could have interpreted the coefficients as measuring the (contemporaneous) *impact* of one year of rule, *on the assumption that* the corruption routines set into motion by the original event persisted unchanged (see Appendix I). Both of these interpretations are applicable the estimates reported in column 1. The regressions in columns 2-5 incorporate the time-discounting process in $OTTO_i$ and SOC_i. Effectively, they reconstruct the magnitude of the original impact based on the legacy of corruption observed today, and an assumption about the speed of historical decay. Thus, the coefficients should be interpreted as the impact effect of one year of rule (the effect at year's end), on the assumption that corruption routines decay at a rate of 1, 2, 3, etc. percent a year. They answer the question: how much corruption would increase today, had a country been subject to an additional year of rule *today.*

Since there are no principled grounds to favour one particular time-discount rate, it is not possible to establish which model identifies the 'true' magnitude of the contemporaneous impact effect. Still, it is worth noting that the estimates of the impact become implausibly large as η declines. The coefficient on $OTTO_i$ in model 4 (table 7), for instance, implies that corruption increased by 0.15 index points (22 percent of today's standard deviation) during *each year* of Ottoman domination. Also, for the reasons discussed earlier, it is implausible to assume full persistence. If so, it may be possible to speculate that the 'true' magnitude of the impact is identified by models that assume $1 > \eta > 0.97$, confirming the plausibility of Dimitrova Grajzl's time-discount assumption (0.998). This discussion suggests, tentatively,

that rather than explaining the legacy of imperial/socialist corruption in terms of a large original impact coupled with fast historical 'erosion', it may be more plausible to posit a more modest original shock coupled with a relatively higher persistence of the fall-out from the shock.

6. CONCLUSION

In institutional economics, there is now a large literature that 'provides support for the notion that history can matter [for present-day outcomes] through the evolution and persistence of early institutions' (Nunn, 2009: 78). The evidence presented in this paper corroborates existing findings in this literature. Using data from the full population of post-socialist and post-Ottoman polities, I showed that, holding everything else constant, the countries that experienced socialist or Ottoman rule for a longer period of time tend to have more corruption today. Although I stop short of offering direct evidence of the persistence mechanism (or a precise estimate of the rate of persistence), persistence (though less than 'full') explains why the adverse effects of Ottoman and socialist rule are still observable in cross-country data today. In this sense, the debate in the transition literature on the primacy of socialist vs. pre-socialist legacies on corruption is a moot one. My empirical findings suggest that *both* socialism and Ottoman rule had an independent impact on corruption, having facilitated the emergence of corruption routines whose 'trail' is still visible today.

Note that the effects of Ottoman and socialist rule identified in this paper represent the 'direct' effect of historical events on present-day institutional outcomes, factoring out the influence of contemporary income levels, amongst others. Of course, Ottoman and socialist rule may also have had a parallel impact on economic development, which is an important determinant of corruption. Estimating the *total* effect of Ottoman and socialist rule on corruption is beyond the scope of this paper. Tentatively, the evidence presented in Appendix IV suggests that the adverse direct institutional effect might be at least partially cancelled out by the indirect effect running via economic development. If so, the Ottoman legacy on corruption would be, on net, zero.

Whether or not Ottoman domination had an independent economic effect, my findings indicate that the 'burden' of history explains but a fraction of the total variation in (perceived) corruption amongst post-socialist countries. In other words, the magnitude of legacy effects, though statistically significant, is not economically large. Most cross-country variation in corruption, I find, is due to differences in the contemporary structural characteristics of a country's economy, as proxied by income levels and resource dependence. These results are also robust to controlling for the potential endogeneity of income levels to corruption.

The results have a number of important theoretical and policy implications. *First*, they lend credence to recent theories that see corruption primarily as a manifestation of economic under-development (Khan, 2005; Uberti, 2014). If income levels are the main drivers of corruption, the best anti-corruption policies may not be those targeting graft head-on. To some extent, policies that promote economic growth may also, indirectly, take care of the problem of political corruption. *Second*, the small magnitude of historical legacy effects (and the smaller relative size of the legacy effect from an historical

event, Ottoman domination, that is further back in time), indicates that historically determined institutions may not persist entirely unchanged. Rather, they may be subject to a process of slow-moving erosion or decay. This explanation resonates with accounts that see political institutions as relatively fast-moving, as compared to cultural norms and values (e.g. adherence to religious beliefs), which tend to change less frequently, and certainly more slowly, than political institutions (Roland, 2004). It also serves to strike an optimistic note on the feasibility of implementing anti-corruption policies. While historical legacies matter, they do not consign countries indefinitely to a state of endemic corruption. Rather, economic and structural transformation may contribute to reducing corruption even in countries held down by legacies of predation and warlordism.

Lastly, my account calls for a measure of scepticism about informal explanations of post-socialist corruption as a legacy of assorted 'Oriental despotisms' – including the Ottoman Empire and Soviet-style socialism. While these regimes *did* leave an observable 'trace' in the institutional endowments of post-socialist countries, their economic significance as predictors of corruption is dwarfed by economic structure. Accordingly, accounts of post-socialist transition should resist the essentialising tendency of certain discourses of 'Ottoman' and socialist backwardness (Sulstarova, 2015). To be sure, the legacy of state socialism and Ottoman domination should be clearly acknowledged. In fact, the results of this study cast a shadow on historical accounts that either question the impact of Ottoman rule on corruption (Özbudun, 1996: 135-6) or claim that the Ottoman legacy was quickly overcome and 'relegated to the realm of perception' (Todorova, 1996: 69). The results also cast doubt on the analysis of the Ottoman political system by early-modern European thinkers, e.g. Machiavelli and Jean Bodin, who 'praised the Ottoman military and administrators' incorruptibility, discipline and obedience' (Quataert, 2000: 7). The Ottoman political system *did* encourage corruption, and this corruption persisted over time. On balance, however, the corruption observed in post-socialist countries today should be seen primarily (though, again, not entirely) as a symptom of persistent economic under-development and continuing dependence on primary commodity exports, rather than a vestige of the past.

Future studies should further elucidate the relative importance of long- and short-run determinants of corruption across a wider sample of countries. At the same time, more theoretical work remains to be done to clarify how historical events shape early institutions and how these early routines are perpetuated and transmitted. The accompanying empirical work should also develop new techniques to measure institutional persistence and 'decay'. The social, political and economic diversity that characterises the post-Ottoman space may offer a fertile ground for further empirical research.

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APPENDIX I: A Simple Model of Historical Persistence

I model institutional change as a dynamic process in which the dependent variable I_{it} (institutional characteristics in country *i* in year *t*) depends on its past realisations:

$$I_{it} = \eta I_{i,t-1} + \alpha \boldsymbol{X}_{it} \qquad (A1)$$

where η is the magnitude of the persistence effect. $\boldsymbol{X}_{it}$ is a dummy variable denoting an exogenous historical event (e.g. Ottoman imperial rule) in country *i* in year *t*, so that $\boldsymbol{X}_{it} = 1$ when the event occurs and $\boldsymbol{X}_{it} = 0$ when it does not. α measures the impact of the event on the outcome. Substituting a similar expression for $I_{i,t-1}$ into (A1) and iterating over *n* years, I obtain an expression for the quality of institutions in country *i* at the end of the time process. Calling the end-point (or present time) T, I obtain:

$$I_{iT} = I_i^0 + \alpha\left(\boldsymbol{X}_{iT} + \eta \boldsymbol{X}_{i,T-1} + \cdots + \eta^n \boldsymbol{X}_{i,T-n}\right) \qquad (A2)$$

where $I_i^0 = \eta^n I_{iT-n}$ is an initial conditions variable that reduces to 0 when $n \to \infty$.[35] If $\eta = 1$, the time process reduces to a unit root and (A2) becomes:

$$I_{iT} = I_i^0 + \alpha \sum_{k=0}^{n} \boldsymbol{X}_{i,T-k} = I_i^0 + \alpha(B_i - E_i) \qquad (A3)$$

(A3) indicates that the total effect of Ottoman domination on present-day institutions in country *i* is equal to the product of the impact of one year of rule (α) times the total number of years of rule (i.e. the years for which $\boldsymbol{X}_{i,T-n} = 1$). Alternatively, this quantity may be expressed as the number of years that elapsed between the first (B_i) and the last (E_i) year of Ottoman rule, where E_i and B_i are measured relative to the present time (i.e. Ottoman rule began "B_i years ago" and ended "E_i years ago") and $E_i < B_i$.[36] Equation (A3) corresponds to regression equation (1).

A more plausible assumption, however, is that $0 < \eta < 1$, which implies that institutional routines are subject to a process of 'autoregressive decay'. If this is the case, then the legacy effect (call it $\hat{\alpha}$) of one year of Ottoman rule is smaller than α. Also, $\hat{\alpha}$ approaches zero the more the event recedes back into the past, for the contribution of years in the more distant past is now discounted by a factor $\eta^n < 1$, which decays exponentially in *n*. More formally, (A2) can be re-written as:

$$I_{iT} \quad = I_i^0 + \alpha \sum_{n=E_i}^{B_i-1} \eta^n = \; I_i^0 + \alpha(\eta^{E_i} + \cdots + \eta^{B_i-1}) \qquad (A4)$$

In this equation, the exponential decay process is just a *discounted* sum of the years of Ottoman rule (i.e. the years for which $\boldsymbol{X}_{i,T-n} = 1$), from the most recent year ($n = E_i$ years prior to the present time T) to the most distant one ($n = B_i$ years prior to T).[37]

Now, if we estimate $I_i = \hat{\alpha}(B_i - E_i) + \varepsilon_i$, when the 'true' relation is $I_i = \alpha(\eta^{E_i} + \cdots + \eta^{B_i-1})$, then the OLS estimator for $\hat{\alpha}$ is given by:

$$\text{plim}\ \hat{\alpha} = \alpha \frac{\text{Cov}(B_i - E_i,\ \eta^{E_i} + \cdots + \eta^{B_i-1})}{\text{Var}(B_i - E_i)} \qquad (A5)$$

Based on (A5), it is easy to see that plim $\hat{\alpha}$ approaches $\alpha[\text{Cov}(B_i - E_i,\ 0)/(\text{Var}(B_i - E_i)] = 0$ as E_i and B_i tend to infinity (i.e. the more an historical period recedes back in time). In addition, plim $\hat{\alpha}$ approaches zero when $\eta \to 0$ (i.e. the more an historically established routine has a tendency to decay rapidly), and

[35] Or to a non-zero constant if we assume that at the beginning of the time process institutional arrangements were very similar across countries.

[36] The second equation in (A3) holds if I assume, as I do in the data construction, that the rule began and ended midway through the reported year (see Table 1). If so, a simple count of the years of rule (the $\boldsymbol{X}$'s) equals the difference between the last and the first year.

[37] Since the rule is assumed to begin and end midway through the first and last reported year, and the time discount rate is annual (rather than biannual), I drops the first year of rule. The approximation involved is, clearly, extremely small.

when $\alpha \to 0$ (i.e. if the historical event had no shock impact on the institutional routine in the first place). From (A5) it is also easy to see that, if the 'true' η equals 1, then $\text{plim}\,\hat{\alpha} = \alpha$ and the estimated coefficient should be interpreted as the impact effect.

APPENDIX II: Time-discounted Estimates of Ottoman and Socialist Rule

Note that the exponential decay process in equation (A4) has the form of a geometric series. When $\eta < 1$, this series can be written in closed form:

$$(\eta^{E_i} + \cdots + \eta^{B_i - 1}) = \frac{\eta^{E_i} - \eta^{B_i}}{1 - \eta} \qquad (A6)$$

Expression (A6) is used to construct time-discounted measure of the duration of Ottoman and socialist rule. Since corruption is measured as an average during 2000-7, I assume that the present time corresponds to $T = 2003$. If $\eta = 1$, the length of Ottoman rule in, say, Albania (which lasted between 1468 and 1912) equals $\frac{(B_i - E_i)}{100} = \frac{\{[2003-1468]-[2003-1912]\}}{100} = 4.44$. If $\eta = 0.998$, then the time-discounted measure of Ottoman rule for Albania becomes $\left\{\frac{[0.998^{(2003-1912)} - 0.998^{(2003-1468)}]}{[1-0.998]}\right\}/100 = 2.45$. This measure approaches the undiscounted sum if $\eta \to 1$. For instance, if $\eta = 0.9999$, the discounted measure is 4.30. Note that, if Ottoman rule had lasted between 1558 and 2002, then the undiscounted sum would remain the same (4.44), but the discounted measure would increase to 2.94. Clearly, more recent years of rule contribute more to the time-discounted sum than years in the more distant past. Since most of this period of rule dates back to several centuries prior to the present time, the discounted measure (2.94) is still substantially smaller than the undiscounted sum (4.44).

APPENDIX III: Variable Description

Variable	Code	Description	Source
Corruption Index	*corr*	Perceived levels of corruption in the public sphere (average during 2000-7). Ranges between a theoretical minimum of -2.5 (least corrupt) and a theoretical maximum of 2.5 (most corrupt).	World Bank, *Worldwide Governance Indicators, 2017*
	corr_se2	Standard error of the estimate of corruption, squared	Ibid.
	corr_ti	Perceived levels of corruption in public sphere (2000-7). Rescaled over -/+2.5	TI, *Corruption Perceptions Index,* 2017
Ottoman Rule	*ottoman*	See section 3 (in '00 years)	Author's construction
Distance from Constantinople	*gcdist_ist*	Great-circle distance from Istanbul's Ataturk airport to the main airport of the present-day country's capital city ('00 km)	www.greatcirclemapper.net
Russian/Safavid/ Mughal Rule	*russian; safavid; mughal*	Number of years under Russian imperial/Safavid/Mughal rule	Author's construction
Socialist Rule	*socialist*	Number of years under socialist rule	Author's construction based on *CIA Factbook* data
	soc_europe	equals *socialist* for European countries, and 0 otherwise	Author's construction
	soc_mena	equals *socialist* for MENA countries, and 0 otherwise	Author's construction
Socialist Dummy	*soc_dummy*	equals 1 if *socialist* ≠ 0, and 0 otherwise	Author's construction
Socialist Rule (Weighted)	*soc_wtd*	Equals (*socialist* · 2) if country is 'strict' socialist regime and (*socialist* · 1) otherwise.	Based on Dimitrova-Grajzl, 2007: 549
Ottoman/socialist Rule (discounted)	*o1-o975; s1-s975*	Time-discounted measure of duration of Ottoman/socialist rule. See Appendix II for details	Author's construction
Log GDP per capita	*Ln_Y*	GDP per capita in nominal US dollars, average over 2000-7	World Bank, *World Development Indicators,* 2016
Log GDP per capita, PPP	*Ln_Y_PPP*	GDP per capita in PPP US dollars, average over 2000-7	Ibid.
Log Oil Exports	*Ln_oil*	Natural log of oil rent earnings (% GDP), average 1970-2007	Ibid.
Log Aid	*Ln_aid*	Net ODA received (% GNI), average during 2000-07	Ibid.
Islam	*islam*	Fraction of total population that is Muslim	Parker, 1997
Protestant	*prot*	Fraction of total pop. that is Protestant	Ibid.
Ethnolinguistic Fract.	*f_ethnolang*	Average of the ethnic and linguistic fractionalization indices	Alesina et al., 2003
Religious Fract.	*f_relig*	Index of religious fractionalization	Ibid.
European Coloniser	*brit*	equals 1 if former British colony	Author's construction
	french	equals 1 if former French colony	Author's construction
	italian	equals 1 if former Italian colony (Libya, Eritrea)	Author's construction
EU	*EU*	equals 1 for Višegrad countries, Baltic states, Greece and Cyprus.	Author's construction

Log(years of conflict)	*Ln_conflict*	Number of years of conflict during 1946-2007, weighted by intensity level. Conflicts with more than 1000 battle-related deaths per year are weighted twice as much.	Uppsala Conflict Data Programme, 2016
Land within 100 km of coast	*near_coast*	Percentage of land within 100 km of nearest ice-free coast. For present-day Serbia, *near_coast* is assumed to equal 0.	Nunn and Puga, 2012
Imports / GDP	*L_imports*	Imports of goods and services (% GDP), average 1997-2003	World Bank, *World Development Indicators*, 2016
Exports / GDP	*L_exports*	Exports of goods and services (% GDP), average 1997-2004	Ibid.
Democracy	*demo_fhi*	Average of Political Rights and Civil Liberties indices, 1997-2002	Freedom House, 2017
Economic Freedom	*econfred_hv*	Average of 7 dimensions of economic freedom: business, trade, fiscal, monetary, investment and financial freedom, and freedom from government. Average over 1997-2003.	The Heritage Foundation, 2015

APPENDIX IV: The Economic Effects of Ottoman and Socialist Rule

Omitting income from eq. (1) might severely bias the estimates of the corruption effect if Ottoman and socialist rule also influence corruption through economic development. The economic effect of Ottoman and socialist legacies is difficult to establish a priori. On the one hand, Grosjean (2011) finds a negative (but insignificant) effect of the length of Ottoman rule on income levels in a sample of East-European regions. On the other, the historical literature has typically cast the Ottoman Empire as a driver of long-distance trade and pre-industrial accumulation (Inalcik and Quataert, 1994). Thus, it is possible that Ottoman rule might have left a negative economic legacy on its successor states relative to the more industrially advanced Empires of East-Central Europe (e.g. Austria-Hungary and Prussia), but a *positive* legacy relative to other non-Ottoman societies in MENA. The caliphates of the Arabian Peninsula, for instance, maintained an essentially nomadic-pastoralist economy until after WWII (Al-Rasheed, 2010).

This hypothesis is consistent with the data (Table AIII.1). On balance, however, the Ottoman Empire was probably more akin to Prussia and Austria from the point of view of economic structure than to the nomadic-pastoralist economies of its Eastern neighbours. Accordingly, the overall income effect in the full sample is positive and significant (column 1, Table AIII.1). If Ottoman rule produced two countervailing effects on corruption and income levels, the consequence of omitting income from my corruption regression is to bias the estimated corruption effect *downwards*.

TABLE AIV.1: The Effect of Ottoman and Socialist Rule on Per-capita Income Today

	Full sample	Europe	MENA
	(1)	(2)	(3)
Ottoman Rule (’00 yr)	0.202***	-0.256***	0.366***
	(0.074)	(0.031)	(0.110)
Socialist Rule (’00 yr)	-0.036	-4.172***	-0.678
	(0.949)	(0.422)	(0.681)
Constant	7.456***	10.993***	7.022***
	(0.332)	(0.208)	(0.370)
Observations	64	23	41
R-squared	0.08	0.76	0.23

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the average of log (GDP per capita) during 2000-7.

Growth in at least some of the socialist countries of East-Central Europe was rapid in the short term, spurred by a state-enforced regime of high savings and aggressive investment. Still, the complete elimination of markets and private property should be expected to reduce static efficiency and stifle innovation, hampering growth in the long run. This hypothesis finds confirmation in a restricted sample of (mostly socialist) East-European countries (column 2). The negative effect of socialist rule, however, becomes statistically insignificant in the unrestricted sample (column 1). A possible explanation is that state socialism was no worse from the point of view of economic efficiency than the non-socialist authoritarian (e.g. Ba’athist) regimes that emerged in many post-Ottoman successor states in MENA. If in the long run socialist rule has no statistically significant effect on economic development, omitting income from a corruption regression should not significantly bias the estimated coefficient on SOC_i.

APPENDIX V: Other Empires

i	Period of Rule	Value
Panel I: Russian Empire		
Russia	1721-1917	1.96
Ukraine*	1721-1917	1.96
Estonia	1722-1917	1.95
Latvia	1722-1916	1.94
Lithuania	1796-1915	1.19
Belarus	1794-1917	1.23
Moldova	1813-1917	1.04
Poland*	1816-1915	0.99
Georgia	1802-1917	1.15
Azerbaijan	1814-1917	1.03
Armenia	1830-1917	0.87
Kazakhstan*	1799-1917	1.18
Uzbekistan	1877-1917	0.4
Tajikistan	1877-1917	0.4
Kyrgyzstan	1877-1917	0.4
Turkmenistan	1886-1917	0.31
Panel II: Safavid Empire		
Afghanistan*	1511-1722	2.11
Armenia*	1502-1584; 1612-1722	1.9
Azerbaijan*	1502-1583; 1612-1722	1.91
Georgia*	1502-1579; 1612-1722	1.87
Iran	1502-1736	2.34
Iraq	1510-1536; 1624-1639	0.41
Pakistan*	1510-1536; 1624-1639	1.87
Syria*	1504-1515	0.11
Turkmenistan*	1511-1736	2.25
Panel III: Mughal Empire		
Afghanistan*	1526-1739	2.13
Bangladesh	1577-1721	1.44
India*	1527-1803	2.59
Pakistan*	1559-1750	1.91

Sources: author's estimations (with Can Cinar) based on data from the *GeaCron Project; Notes:* cleft countries are marked with an asterisk