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Essays in the Political Economy of Development

by

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Disclosure of coauthor contribution

Surnames and Social Mobility in Hungary over Two Centuries and Five Regimes

Co-authors: Pawel Bukowski, Gregory Clark, Rita Pető
The method applied in the paper was developed by Gregory Clark in previous work (see Clark and Cummins 2014a; Clark 2014; Clark and Cummins 2014b; Clark et al. 2015a). Rita and I collected the data for the project. Data cleaning was carried out by Pawel, Rita and I together. Gregory created the inital empirical evidence which confirmed that the method produces similar results as it did in the case of different countries and time periods. I made the tables, figures and regressions in the form in which they are included in the paper, and wrote the first draft, which was later edited by Pawel.

Deny Thy Father and Refuse Thy Name

Co-author: Rita Pető
The paper was developed in cooperation with Rita from our research project on the economics of name changing. We contributed equally to the idea of the paper, and the data collection and cleaning effort. I came up with the theoretical background and wrote the first draft of the paper, which we later edited and revised in close cooperation.

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Abstract

The three chapters of the thesis investigate how institutions and culture affect and are affected by the economy. The main message of Chapter 1 is that campaigning on highly divisive, identity-based issues can serve as a cheaper alternative to provision of goods and services, so politicians have an economic incentive to cater to hardliners. I formalize and test this hypothesis using Indonesian data. About half of the district governments in Indonesia have been experimenting with divisive and often controversial Sharia-based religious policies since 2000. I estimate the impact of religious policies using difference-in-differences and instrumental variables methods. I show that districts that introduce Sharia policies spend less and create less public services: the conservative estimate of the impact is a 10 percent decrease in both spending and in the value of a standardized government services index. The downstream social effects of cutting service provision and relying on hardliners to win elections are that Sharia policies increase various measures of poverty and foster violence. Model-based welfare calculations suggest that the utility loss of the secular voters is even larger than observed outcomes would suggest.

In Chapter 2 (joint with with Pawel Bukowski, Gregory Clark and Rita Pető) we study the long-run social mobility in Hungary from the late 18th until early 21th century. We measure social mobility using the relative representation of various social groups (identified by surnames) among elite occupations. Using unique historical registries spanning more than two centuries, we are able to estimate the rate of status transmission under different political regimes: feudal and constitutional monarchies (-1918), right-wing authoritarianism (1919-1945), communism (1947-1989) and liberal democracy (1989-). We show that social mobility on the group level is slow. It is faster in regimes that were liberal by the standard of their age (constitutional monarchy and liberal democracy), and slower under right-wing authoritarianism. Surprisingly, we find very limited evidence for accelerating social mobility under communism. Finally, disadvantaged groups that we are able to identify by surname (such as the Romani minority) do not see any meaningful improvement of their status under any of the studied regimes.

In Chapter 3 we study (with Rita Pető) the name changing movement of the late 19th and the early 20th century Hungary using unique, individual level data on family name "Hungarianizations". We argue that self-selection into name changing, which was an important step in the assimilation process, was at least partly driven by economic incentives from the government's

part. Making use of a historical policy shock which involved a one-year campaign among public sector workers to Hungarianize their names, we show evidence that the observed patterns in name changing are consistent with economic self-selection into assimilation, and quantify how much of the variation in name changing is driven by the static and dynamic push effect of policy, and how much is explained by the pull effect of community ties. We find that name changing responds to changes in incentives quite quickly, which is surprising given the results of previous studies on cultural persistence.

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I spent roughly 20% of my life so far working on this dissertation.[1] This means that I have benefited from the help, generosity and patience of a lot of people. First, I am grateful for the invaluable help of my advisor Ádám Szeidl, who spared no time and effort throughout these years to guide and structure my thinking and make me understand how the discipline, and research works in general. I want to thank my coauthors: Pawel Bukowski, Gregory Clark and Rita Pető. I owe a lot to Balázs Reizer, with whom I discussed these papers most extensively. Boza István was the first to read Chapter 1 from cover to cover, and made very detailed comments, and I am much indebted to him for this heroic deed. I learned a lot from the discussions with Alessandro De Chiara, Luca Drucker, Győző Gyöngyösi, Julius Horváth, Marc Kaufman, Miklós Koren, János Köllő, Botond Kőszegi, Rachel Kranton, Timur Kuran, Sergey Lychagin, Arieda Muço, Ferenc Szűcs, Álmos Telegdy, and Andrea Weber; these discussions helped improve the research that eventually became the chapters of this dissertation. I also want to thank Julien Labonne and Arieda Muço for giving my thesis a careful reading and providing excellent feedback.

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[1] In gross terms, that is. The ratio of the number of days that have passed since I enrolled in the PhD program and the number of days that have passed since I was born is 0.1993.

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The effort behind the work in the following chapters is collective. The errors are my own.

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Chapter I

The Public Morals / Public Services Tradeoff: Theory and Evidence from Sharia Regulations in Indonesia

1 Introduction

The emphasis of politics in democracies worldwide is shifting from "what we want" to "who we are" (Fukuyama 2018). The existing literature in economics mostly focuses on the demand side determinants of the increasing salience of ethnic, religious and national identities and of the intolerance that commonly comes along.[2] Less is known about the supply side incentives of politicians to cater to these demands, and the impacts of such decisions.

In this paper I argue that divisive issues are cheap for politicians, while providing goods and services based on a policy platform (when institutions are strong), or building a network of clientele (when institutions are weak) are more expensive. Supply side incentives (such as the lucrativeness of the position) and demand jointly determine which politician acts as a champion of virtues, and which does not. The result is a substitution between public morals and public services: the politician who offers more of one will deliver less of the other.

As a first contribution of the paper I show that the introduction of Sharia-based local government regulations in Indonesia followed this pattern. Over the last two decades since the fall of the military rule in 1998, many of the elected district heads of the newly democratized and decentralized country decided to implement regulations inspired by Sharia, or traditional Islamic law. These regulations often sparked controversy locally, and have inspired an extensive literature in political science, which is, to the best of my knowledge, almost exclusively qualitative.[3]

I measure the impact of Sharia-inspired restrictive policies on a range of government and

[2]Bentzen (2015) and Belloc et al. (2016) find that natural disasters globally increase religiosity.Binzel and Carvalho (2016) show that secularization trends in Egypt reversed as a consequence of declining social mobility. Chen (2010) argues that religion offered ex post insurance for financial distress during the Indonesian financial crisis.Henderson and Kuncoro (2011) show evidence that the voters in the early 2000s in Indonesia chose Islamist parties to curb corruption in local assemblies. The literature on Western populism recently gained momentum (Bursztyn et al. 2017; Di Tella and Rotemberg 2018; Enke 2018; Guiso et al. 2017; Pastor and Veronesi 2018; Rodrik 2017). In broader contexts we know of economic analyses of intergroup conflict (Esteban and Ray 2008; Jha 2013), religious extremism (Iannaccone and Berman 2006).

[3]The earliest paper I encountered is Bush (2008); Buehler (2016) is the most extensive source; Other papers usually focus on a specific case study or a specific characteristics of Sharia-politics in Indonesia(Buehler, 2008; Buehler and Muhtada, 2016; Crouch, 2009; Nastiti and Ratri, 2018; Pisani and Buehler, 2016; Van Dijk and Kaptein, 2018).

societal outcomes suggested by this mechanism. While the existing literature mostly concerns the impact of one particular religious institution or policy, in this paper the "policy" being evaluated has two elements: a restrictive regulation justified on religious grounds, and a change in the nature and the quantity of transactions between voters and the government.[4] To assess the impact of Sharia-inspired regulations in Indonesia, I first compare adopting and non-adopting districts in a difference-in-differences setting. Then I use two different demand-side instruments for religious policies that rely on different identification assumptions. The first uses within-district, village level variation in preferences.[5] The second exploits pre-policy district-level patterns of demand for Sharia interacted with the country wide growth rate of preference for religious policies. The three identification strategies, though relying on different identification assumptions, yield similar results and show a decrease in public good provision and public employment expenditure, while poverty increases and empowerment of radicals makes violent incidents more frequent.[6]

Although I document that Sharia regulations have a negative impact on a range of measures that contribute to material wellbeing, the results do not directly imply that Sharia regulations are decreasing total welfare. The second contribution of the paper is that it measures welfare consequences indirectly, through votes received by incumbents. In order to do this, I first formalize the district head's decision whether to introduce Sharia policies or not. I show that a simple model based on plausible assumptions is able to generate the observed stylized facts on the correlates of Sharia introduction. Then I take the model to the data and estimate bounds on underlying voter utility given the decisions of the incumbent and district characteristics. This exercise suggests that the regulations have a significant direct negative impact on the secular voters beyond the reduction in state services.

In Section 2 I outline the main hypothesis of the paper and how it is related to existing literature. I describe the institutional background and the data sources in Section 3 and then in Section 4 I estimate the causal impact of religious regulations on spending, public services and measures of downstream social effects, such as poverty. I use three different, complementary

[4]Within the context of Islam, the impact of the religious foundations (Kuran, 2004; Bazzi et al., 2018), the pilgrimage to Mecca Clingingsmith et al. (2009), inheritance rules (Alfano, 2017), and fasting during the month of Ramadan (Oosterbeek and van der Klaauw, 2013; Majid, 2015; Campante and Yanagizawa-Drott, 2015) were studied recently.

[5]Village (*desa*) is the smallest administrative level of territories, so the whole country is subdivided to "villages", not just rural areas. The levels are 1) villages (desa or kelurahan) 2) subdistricts (kecamatan) 3) districts or cities - where Sharia regulations are introduced (kabupaten or kota) 4) provinces (propinsi). The average population of a village was almost 3500 in 2011.

[6]I use the INDODAPOER district-level data set of World Bank, and village level PODES data set of the Indonesian Statistical Center (BPS), and the National Violence Monitoring System data of the Indonesian Ministry for Human Development and Culture, and the Indonesian Family Life Survey of the Rand Corporation for auxiliary evidence.

identification strategies. The first design is a simple difference-in-differences (DID) analysis where I exploit district and time variation in the implementation of religious policies. I compare over time outcomes of districts that implement Sharia regulations to districts that do not in a setting with geographical and time fixed effects and a variety of control variables including lagged economic indicators and district government income, which is mostly determined by exogenous factors.[7] I present the results both in fixed effect regression and in event study forms. I find that in election cycles where the incumbent introduces religious policies, spending on public employees is lower by about ten percent, while total spending does not differ significantly. A village-level standardized index of government services drops by 8 percent of a standard deviation following the introduction of the first religious policy in the district, and villages are 1.5 percent more likely to have a slum area over the years that follow.[8] District poverty rate goes up by 1.5 percentage points on average over the study period in these districts after the policy is introduced, and violent incidents become more frequent by .26 percent.

The DID analysis relies on the assumption of parallel trends, meaning that if the religious policies had not been introduced, the outcome variables of adopting and non-adopting districts would have evolved similarly over time. I also need variation in the introduction of Sharia regulations to be driven by demand variation for Sharia that is not correlated with relevant outcome variables. The event study analyses are consistent with this assumption (no pre-trends in outcomes, but visible pre-trend in Sharia demand). If, however, later shocks that shift supply and demand of religious extremism are correlated with these outcomes, the DID estimates are potentially biased. I rule out several alternative causal mechanisms, but demand side omitted variables remain a concern. I use two different sources of variation in demand (one source on the village level and one on the district level) that shift the policy variable exogenously in the second and third empirical designs.

In the second design I hold village level demand for Sharia fixed and use variation in demand for Sharia regulations from other villages in the same district. I capture demand for Sharia with the per capita number of one particular Islamic school in every village (the *pesantren*), as these schools have been previously identified as the most important nodes in local Islamist networks

[7] According to the INDODAPOER data set, the median share of locally generated revenue in the districts is 5% over the study period. Most district government revenue is allocated through government block grants and natural resource revenue redistribution mechanisms. These are formula-based allocation schemes and generate large variation in local government revenue. In 2010, for example, the district at the 99th percentile of the revenue distribution had 17 times more revenue per capita as the district at the 1st percentile.

[8] I use this indicator as the village level proxy for poverty, as poverty rate statistics are not available on the village level in PODES.

that drive the religious turn.[9] For every village I use average Sharia demand in other villages of the same district as an instrument of introducing Sharia policies. This way I can compare villages in different districts that had a similar level of Sharia demand, and use the variation in the probability of introducing Sharia regulations that comes from other villages in the same district.

In an instrumental variables regression with year and village fixed effects I estimate the effect of religious polices on government services at -40 percent of a standard deviation (marginally insignificant) and +7 percent increased probability that the village has a slum (also insignificant). The noisiness can be explained by within-district heterogeneity in effect size - the impact on government services becomes strongly significant on the subsample of villages that themselves had no religious schools, suggesting that more secular areas are impacted more by the policy.

This identification strategy does not depend on the assumption of district level parallel trends, but on an exclusion restriction. This requires that demand for religious policies in one village only affects the outcomes of interest in other villages of the same district through the introduction of a religious policy. The instrument eliminates bias potentially introduced by village level omitted variables that are not correlated between villages. Placebo tests and sensitivity analysis of the estimates using different specifications of the leave-out-mean support the validity of these assumptions. The relevance condition of the instrumental variables method requires that the leave-out-mean of religious schools indeed predicts the introduction of the policy, and not the other way around. I confirm that this condition is met by showing a strong first stage, and an event study on the number of religious schools around Sharia policy introduction. This event study shows that increase in the number of schools predates the introduction of Sharia regulations.

As the leave-out-mean instrument can only be defined for data that I observe on the village level, I also define a district level instrument for district outcomes. In this third design I interact pre-policy district level variation in preference for Sharia with the country level growth rate in Islamic schools to obtain the predicted Sharia demand.[10] I then use this predicted demand to instrument for the introduction of Sharia regulations. The intuition behind this instrument is to only use pre-existing, exogenous trends in demand for religious extremism. Relying on this variation only the estimates will not be affected by that part in the variation in Sharia demand

[9] There are examples both in the political science (Buehler 2008,2016) and in the economics literatures (Bazzi et al., 2018).

[10] The idea is similar to the shift share-type instruments discussed, see Bartik (1991).

which is potentially caused by omitted variables later on.[11] The identifying assumption requires that predicted demand calculated from pre-existing variation in Sharia demand is independent of later unobserved shocks to outcome variables, but otherwise the outcome variables do not have to follow parallel trends.

I estimate the impact of Sharia regulations on the government services index (aggregated to a district level) at -10 percent of a standard deviation in an instrumental variables regression with district and time fixed effects, while the impact on poverty rate in a similar setting is a 5 percentage points increase. Both results are statistically significant. This identification strategy is based on the assumption that pre-policy variation in the preference for Sharia regulations is exogenous to later omitted variables that potentially affect both demand for Sharia and the relevant outcomes. The estimates are robust to choosing different types of baseline variation in Sharia demand, including pre-policy difference in the presence of Islamic schools and a pre-Suharto era historic proxy.[12]

Even though none of them individually eliminates all identification concerns, the results from the three identification strategies together robustly show that government services and material welfare decline as a consequence of Sharia-inspired local government regulations. However, this does not mean that they are altogether welfare decreasing. Campante and Yanagizawa-Drott (2015) show that negative economic impacts of religious institutions can be offset by their positive impact on subjective wellbeing. The results in Section 4 show that politicians push for a substitution between these two, but does not reveal the overall impact on utility and how utility is being redistributed between supporters of religious policies and the rest of the population.

To assess overall welfare implications, I build a model on the introduction of Sharia policies in Section 2. The model focuses on the policy choices of an incumbent politician, who faces the countervailing incentives of keeping spending low (which increases rents given reelection) and probability of reelection high (which can be done by spending on public employment). All state income that is not spent on employment is considered "rent", but it is better to think of it as wasteful spending, as it does not carry the risks usually associated with corruption or outright

[11] An analogue from the migration literature (which extensive uses shift share instruments) is that historic patterns of immigrant communities are exogenous, and total migrant inflows from a particular country are exogenous, too (the first is the "share", second is the "shift" part in the term "shift share instrument"). Then instrumenting inflows to a particular locality amounts to using the variation coming only from the fact that the locality had a migrant community already, and that migrants prefer to go to places where they find their conationals.

[12] Suharto was president between 1967 to 1998. The three specifications I study are: 1) the percentage of all *pesantren* that are in the district in 2000 2) the share of *pesantren* in all comparable educational institutions in the district in 2000. 3) the 1955 vote share of the Masyumi Party, which advocated a widespread adaption of Islamic Law in Indonesia, and was later banned.

stealing. Besides spending on public employment, he or she can introduce religious policy that pleases the hardliners a lot, but alienates everyone else (the moderates) a little bit.

The model has three main predictions. First, the incumbent who introduces the policy will have less people in public employment and thus provide less public services. The reason is that if the policy gives a big enough electoral edge for the office holder, he or she will be better off employing somewhat less people and enjoy higher rents from office. This is the central result, the public morals / public services tradeoff. Second, if the wage at which the incumbent can employ people is high, clientele building is more expensive which makes ideological campaigns more attractive. Third, the incumbent will be less likely to introduce the religious policy if (exogenous) state revenues are abundant. The intuition for this is that such large stake elections need broader coalitions, so catering to hardliners is less attractive. The first prediction contradicts the common notion that hardline politicians are "effective" and "get things done". The second suggests that politicians are more attracted by the extremes in more developed areas (where wages are higher). The third suggests that abundance of money can discourage conflictive politics, which contradicts our present knowledge on the resource curse.[13] The predictions are counterintuitive, but reflect the correlates of Sharia introduction described in Section 3. Districts that introduce Sharia regulations are initially more urban and developed (in terms of GDP/capita), have higher minimum wage and lower poverty rates. Districts that have more revenue from exogenous sources will be less likely to introduce religious policies. These more lucrative districts also see more candidates competing for them and higher chance of the incumbents being ousted from office. These findings, though correlational only, are nevertheless closely in line with the second and third predictions of the model.

Finally, I take the expression on expected votes of the incumbent from the model to the data. My goal is to estimate the taste parameter for Sharia regulations for both hardliners and moderates, given observed district characteristics, policy decisions of incumbents, and their eventual electoral performance. For moderate voters I can estimate this parameter directly. For hardline voters I set up a lower bound that establishes at least how much utility they had to gain in order for the religious policy to be welfare increasing on the aggregate. I find that drops in welfare for the moderates can be four times as high through directly disliking the policy than through the channel of the public morals / public services tradeoff. In the meantime, I find

[13]When institutions are weak, exogenous revenues are widely understood to increase conflict, harm accountability, increase adverse selection and further weaken institutions (Mehlum et al. 2006; Robinson et al. 2006; Ross 2008;Paler 2011;Brollo et al. 2013; Ross 2015; Berman et al. 2017).

that the hardline voters have to like Sharia regulations ten times as much in absolute terms as moderates dislike them in order for the regulations to be welfare increasing on the aggregate. This result suggests that Sharia regulations did indeed decrease total welfare.

The paper is most closely related to the literature on the recent emergence of identity-based politics and conflicts around the world. This phenomenon has been studied within the context of the broader topic of populism (Grossman and Helpman, 2018), and is widely understood to be a consequence of economic globalization and rising inequality (Guiso et al., 2017; Rodrik, 2017; Pastor and Veronesi, 2018). This recent literature in political economy is interested in the preference changes of the broader electorate, which we could call the "demand side" of populism.

Studying the "supply side" presents different questions. If we take demand as given, when and why do politicians cater to identity based issues in the electorate? Which politicians do and which decide not to? The supply and demand side can be contiguous, as populist voters can become populist politicians (Dal Bó et al., 2018). But this is not always the case, and politicians without a populist disposition can strategically re-position themselves to cater to extremist demand.

The idea that politicians strategically go to the extremes is, of course, not new. Glaeser (2005) argues that politicians supply hate-creating stories against groups that were beneficiaries of the policies of their political opponents (i.e. egalitarians incite hatred against rich minorities, while opponents of redistribution incite hatred against poor minorities). Politicians can shift away from the political center just to convince voters that they are not exclusively opportunistic and office-minded (Callander, 2008; Kartik and McAfee, 2007). Perhaps the closest to this paper's analysis is Glaeser et al. (2005), who present a model in which there is an information asymmetry between groups of voters in that they are more familiar with the platforms of their own preferred parties. As candidates have to play for their base to get resources for the election, candidates go to the extreme when communicating with their own, and are more moderate when communicating with the general public.

2 Theoretical background and hypothesis

The hypothesis of the paper is that identity-based extreme policies serve as a cheaper alternative for politicians to supplying public services, or delivering material utility to voters directly. This creates an economic incentive to cater to a hardline voter base. Whether this incentive becomes

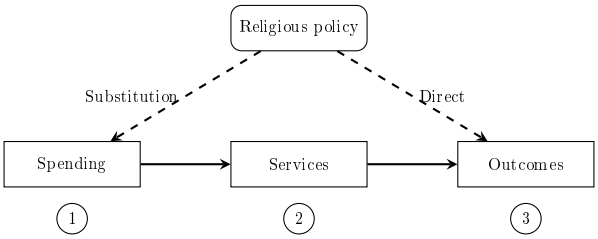
salient depends on contextual factors, such as expected turnout, and the scope for redistribution. The consequence of this supply-driven extremism is that politicians who adopt an extreme agenda purely for cost minimization can exert less effort in, or more rents from office.

Populist politicians do not just shift their policies towards the extremes, but create new themes that make voter identity salient. There is always an economic incentive to do this, as this shifts the focus of voters from potentially costly themes (e.g. redistribution) to less costly ones (e.g. religious, national or ethnic identity). Rational models of voting (following Downs, 1957) are plagued by the problem of turnout: an individual voter pays positive cost of casting her ballot, but her marginal contribution to the election outcome is zero, so nobody should bother to turn out to vote (Geys, 2006). One proposed solution to this problem is that (some) voters vote expressively, not instrumentally: they derive utility not from the outcome of the election, but from expressing that they belong with one of the contestants (Brennan and Hamlin, 1998; Hamlin and Jennings, 2011). If voters with extreme preferences are also more likely to vote expressively, catering to their preferences will be cheaper than turning out voters who make the decision based on material costs and payoffs. Then, whether a politician decides to go down this path will depend on the share of the population who can be turned out to vote with identity-based issues and the opportunity cost of doing so (i.e. how hard it is to turn out voters in a "traditional" way).

In terms of impact, we need to distinguish direct and indirect effects. The direct impact of making policies that cater to hardliners, of course, depends on the actual issue at hand, and to what extent politicians enforce the regulations they make. But if politicians adopted identity-based (in the current case: religious and moral) policies as an alternative to redistributive measures, there will be an additional, indirect channel: the politician will economize on the redistributive agenda, and offer less of whatever he or she otherwise would have offered.

I apply this framework to the Indonesian context and argue that it explains why district heads in Indonesia have been introducing Sharia regulations since district autonomy was established two decades ago. Figure 1 shows the stylized impact mechanism of Sharia-based religious policies based on the tradeoff between supplying public services and supplying religious and moral policies. First, the Sharia implementing politician spends less, creates less services, which translates to worse social outcomes in the district. Second, there is a direct effect which comprises of the actual impact of the policy (e.g. banning alcohol hurts some businesses), and the impact of empowering radicals of the district. After describing the institutional context and the data in

Figure 1: The stylized impact of substituting policy with ideology



The figure shows how religious regulations affect the three sets of outcomes of interest. First, the incumbent who introduces the Sharia regulation cuts spending (Set 1). The consequence is a decrease in the level of public services (Set 2). These are constitute the substitution effect. Lack of government services then have an impact on social outcomes such as poverty and violence, but these are also potentially affected directly by the regulation (direct channel).

Section 3, in Section 4 I identify these channels of impact of Sharia regulations, while Section 5 puts forward the mechanism formally in a way that allows for welfare calculations.

3 Context, data and stylized facts

3.1 Institutional context and local politics

The fall of Suharto in 1998 following the economic crisis of the same year instigated a wave of reforms in Indonesia. The next year saw the first free elections in decades, and the passing of crucial legislation on the decentralization of the government.[14] The administrative level of regencies and cities (which I jointly refer to as "districts" throughout the paper) gained considerable autonomy in a wide range of policies; so wide that it is easier to note the exceptions where the central government maintained authority, such as foreign affairs, national defense, justice, monetary policy and religion.[15]

District executives ("regents" or "mayors") have traditionally been the most important authorities over the choice of district policies. In the Suharto era these executives had been centrally appointed, and after the transition period these office holders were permitted to complete their five year tenures. However, as their appointment had not followed any particular election calendar, the end of their term did not follow one either. As a consequence the newly and democratically elected district legislatures were only able to pick a new district executive for themselves

[14] Laws 22/1999 and 25/1999

[15] Regencies (kabupaten) consist of mostly rural areas; cities (kota) are urban centers. The literature uses the "district" and "region" terms interchangeably.

at some random date between 1999 and 2004.[16] Later, recognizing their de facto authority over policy, the central government introduced direct elections for district heads from 2005 onward.[17] Additional fiscal decentralization came about in 2005 which furthermore increased the capacities of district governments.

One heritage of the Suharto regime is that parties are only weakly institutionalized locally. Rather than being the source from where new candidates for office are recruited, anecdotal evidence suggests that parties offer their support as a commodity for political entrepreneurs who run in district politics (Buehler and Tan, 2007). At any rate, a single-party endorsement for a candidate is the exception rather than the rule. Under this institutional configuration, it is not very surprising that ideological differences between most candidates are not very salient, and factors such as material transfers to voters (in cash or in kind), personal charisma and group loyalty are among the most important ones.

There is a widespread agreement that the political and fiscal empowerment of the districts *de facto* meant the empowerment of the district heads, who enjoy discretion over policy and spending. As a result, the office is a rather attractive one. As early as 2005, mounting a serious campaign for district head required spending between $180,000 to $1.6 million in USD (Rinakit, 2005). Candidates, on the other hand, receive relatively low amount of monetary support from the state, so most sources are round up from private donations. These are subject to donation caps, but sanctions are mostly unenforceable; essentially, the system is designed to be non-transparent (Mietzner et al., 2011). Vote buying is pervasive (Aspinall et al., 2017). Businessmen write checks expecting government contracts should their candidate assume office; candidates themselves accrue significant debts during the election process (Buehler and Tan, 2007). Burhanuddin et al. (2018) reports that as many as one in three voters can be exposed to vote buying in national elections, and posits that in local elections, where electorates are smaller and ties are stronger, it might be even more pervasive.

Cyclicality in government spending suggests that incumbents regularly use government funds to sustain their majorities (Sjahrir et al., 2013, 2014), and certainly there has been significant incumbent advantage present in local elections since the beginning until recently (Martinez-Bravo et al., 2016; Fossati et al., 2017). Particularly important is the maintaining of patronage

[16] This variation in the timing of local government democratization is used in previous literature (Martinez-Bravo et al., 2017; Martinez-Bravo, 2014).

[17] Again, indirectly elected district heads were allowed to complete their tenure, so districts could have their first direct district head election any time between 2005 and 2009. Only very recently have been a centralized effort made to synchronize local elections across the country. District heads are elected via a simple majority; runoffs are only needed if no candidate gets 25% of the vote (see Law 32 of 2004 on regional administration).

networks: bureaucratic jobs are used to maintain patron-client relationships and deliver the votes necessary for reelection of members of the elite (Van Klinken, 2009; Simandjuntak, 2012).[18]

3.2 Sharia regulations

According to the 2010 census, 87% of the population of Indonesia identifies as Muslim, and they are predominantly of the Sunni denomination. Though the first of the "five principles" (pancasila) guiding Indonesian nationalism has been the belief in a one and only God, the Indonesian state is a secular one. Though the national government maintained an exclusive authority in religious issues, local politicians found their way around this and starting from 1999 began to introduce regulations which they perceived as being in line with, or aiming to uphold or restore Sharia, traditional Islamic law. Originally meaning the divine law governing the world, and the way through which this law can be uncovered through deliberation, Sharia over the centuries became a powerful political symbol which condenses a claim to preserve or to create a vaguely defined "Islamic way of life".[19] While five of the ten most populous countries on Earth make reference to Sharia in their legal systems, and a majority of Muslims supported Sharia in the 29 out of 39 countries surveyed by the Pew Research Center in 2013, these figures are not very enlightening as to *what* these people actually support, as the term has no direct mapping into a set of policies.[20] Nevertheless, Sharia as a political symbol has become popular with Islamophobes and Islamists alike, and became popularly (and wrongly) identified with a set of regulations ranging from charitable giving and financial transactions to public morals and elements of family law, such as marriage and inheritance, and also penal punishment.[21]

The regulations adopted by district governments in Indonesia has been focusing on these policy areas. The political science and sociology literature has been studying these regulations

[18] This can be true to the lowest levels of the administration: anecdotal evidence suggests even the jobs of primary school teachers can be part of patronage networks. (Pisani, 2014)

[19] The Encyclopedia of Islam reports "pathway to water" as the original meaning suggested by the etymology of the word "Sharia" (Calder and Berry 2007 in Campo 2009). For the evolution of the concept see Hallaq (2009).

[20] India, Indonesia, Pakistan, Nigeria and Bangladesh are the most populous countries that refer to Sharia in their constitutions. The ambiguity of the term (and its usage) is similar to terms such as "Christian values" or "European values". Being of a vague enough terminology, most people in the West would subscribe to these, without giving a second thought of any policy implications or how some politicians abuse the very same words.

[21] Hallaq (2009) gives an account of how this transformation had its roots in colonialism. Colonial powers kept curbing the legal authority of Islamic scholars to the point where only family law remained under local jurisdiction, effectively turning family law into a core identity issue for the colonized. In the meantime colonialists made an effort to have what they perceived as "Islamic Law" codified in a Western fashion, which was completely alien to previous flexible and deliberative legal practice. Muslim countries that had not been colonized did pretty much the same driven by a pressure to "modernize" and keep up with colonial powers.

for a decade, from the early work of Bush (2008) to the recent comprehensive account of Buehler (2016). I use the list of regulations compiled by these scholars as primary data source along with own data collection and a deeper analysis of the regulations' actual content.[22] It is important to note that these regulations are passed by politicians of a secular background (Pisani and Buehler, 2016) to gain support from religious voters and pressure groups from outside existing patronage networks (Buehler and Muhtada, 2016); also, religious policies cannot be traced back to nation-wide Islamist parties.[23] Figure 2 shows the map of the prevalence of Sharia-based regulations until 2013.

According to Nur Rif'ah (2014), no district level Sharia regulation was ever revoked, and lists several reasons: the regulations were carefully worded so their constitutionality would be hard to challenge. Furthermore, introducing religious policies was a controversial and hard-fought process wherever happened, so their opponents, when took power, decided not enforcing rather than revoking them, to avoid rallying local Islamists against themselves. Finally, though major Muslim organizations of Indonesia (such as NU and Muhammadiyah) take stand against Sharia-introduction on a national level, local chapters do endorse specific regulations. None of the other works that I am aware of mentions revoking any of the Sharia regulations, including Buehler (2016).

I categorize these pieces of legislation into two broad groups, which I call prohibitive and normative. Prohibitive regulations are the ones which severely constrain or ban altogether the selling, distributing and consuming alcohol; increase the crackdown on drugs and prostitution; enforce the retail restrictions imposed by Islamic festivities. Normative regulations, on the other hand, try to actively change behavior: they regulate religious almsgiving, attire, the interaction of sexes in public, required levels of religious knowledge.

The reason why I do not provide a more fine-grained categorization in the analysis is that Buehler (2016) argues that in many cases it is not the exact content of the regulations which matters the most but the fact whether any such regulation takes effect. It might happen, that the regency legislates the banning of alcohol, and vigilante groups start policing places they deem

[22]I would like to thank Giannisa Novi Budiutami for the excellent research assistence she provided with this task.

[23]Buehler (2016) lists six national Islam-based parties in Indonesia, of which four were active in the study period, and none of them was a major one. Within the subsample where I am able to identify party endorsement of incumbents, in 56% of the cases were Sharia regulations adopted by an incumbent which had the support of at least one Islamic party. The party which supported most such incumbents (33%) is the National Mandate Party (Partai Amanat Nasional, PAN). Buehler (2016) does not list this party among those which had the implementation of Sharia in their platform. He concludes that support from Sharia comes from outside the formal political domain.

"immoral" and enforce attire rules which were not at all mentioned in the text. Thus, one channel through which the policies might have an effect is an increased general level of intolerance and public concern about religious values which is independent of the actual content of the legislation (Nastiti and Ratri, 2018).

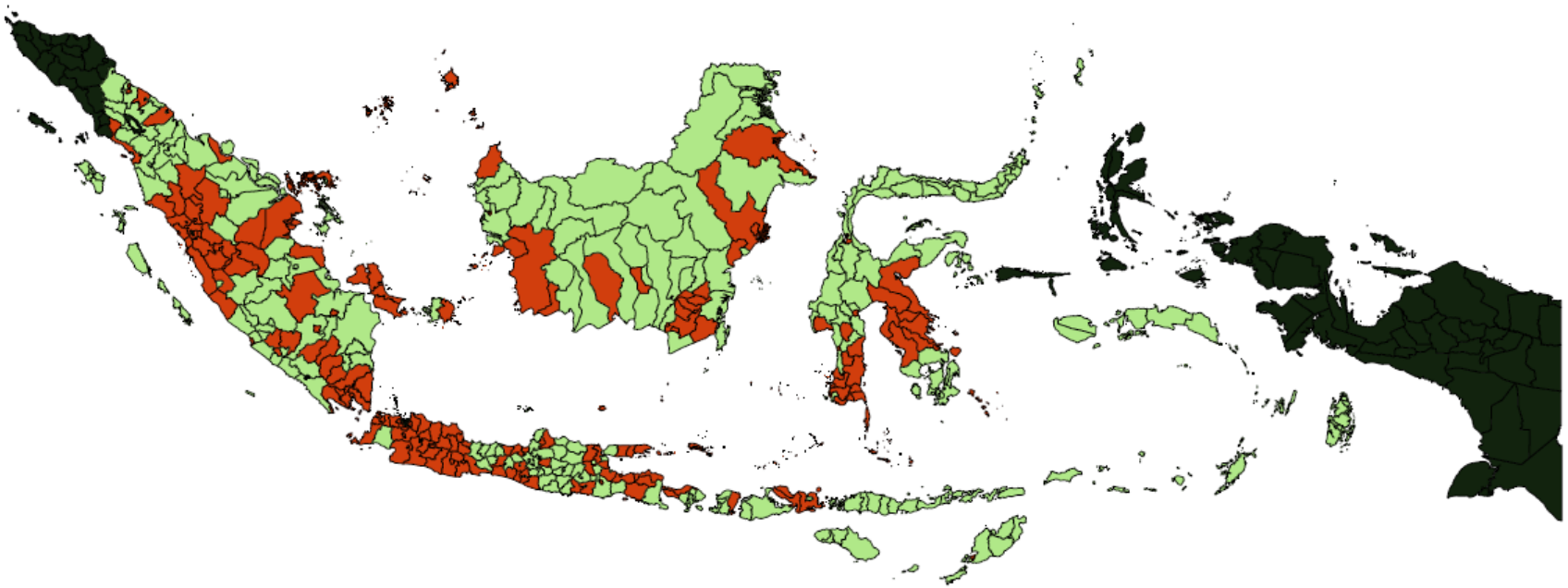
Typical examples of these regulations include the following:

- Regulation 2002/6 of the city of Batam sets to build a society based on Islamic morals; this regulation includes a provision that people of the opposite sex who are not married are prohibited from living under the same roof.[24]
- Regulation 2003/5 of the regency of Bulukumba sets the appropriate dress code for man and women; for women this includes wearing a hijab and garments that cover their hips and ankles.[25]
- Regulation 2001/5 of the city of Cilegon makes it illegal to gamble; to run brothels; to manufacture, store or sale alcoholic beverages or narcotics; all on the grounds that these acts are contrary to religious teachings and local customs. [26]
- Regulation 2008/1 of the same district sets up compulsory extracurricular Islamic educational schemes for pupils learning in secular elementary schools.[27]

Another reason why I am not using more fine-grained categorizations is that these regulations are usually coming in bundles, so disentangling the effects of a specific policy would be econometrically too challenging. Next I traced the regulations in local news outlets, to see if there is evidence for public support, debate, resistance or any hint to what extent the regulations were enforced. I ended up using all Islamic regulations in the analysis which had a reference and the text of which I was able to access.

[24] http://www.jdih.setjen.kemendagri.go.id/download.php?KPUU=13882
[25] http://www.jdih.setjen.kemendagri.go.id/download.php?KPUU=16542
[26] http://www.jdih.setjen.kemendagri.go.id/download.php?KPUU=6686
[27] http://www.jdih.setjen.kemendagri.go.id/download.php?KPUU=7057

Figure 2: Map of regions with religious policies (RPs)



The map shows districts in red which had at least one Sharia-policy by 2013. The districts in black are omitted from the analysis (Aceh, Jakarta and Papua). The shape files show borders in 2009.

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To the best of my knowledge, systematic statistical analysis of the impact of these regulations has not yet been carried out. This is not to say that there is no scholarly interest (moreover, concern) of the effects these regulations might have had. The most often cited negative impacts are on vulnerable groups, such as women, children, the poor, and members of religious communities such as the Ahmadiyah sect (Crouch, 2009; Van Dijk and Kaptein, 2018; Nastiti and Ratri, 2018).

3.3 Data sources

The original dataset on Sharia-based district regulations is based on the list of regulations in Buehler (2016), which only provides the district names, years and regulation numbers, but not further detailed information. We cross-checked the items in the list with the database of local regulations on the homepage of the Ministry of the Interior of Indonesia to get access to their actual texts, and once the titles and content were at hand, to check if similar other regulations exist which the original list did not cover. Then we scraped local news media outlets systematically in the implementing districts to see which regulation left any trace.

Most district level variables are coming from the Indonesia Database for Policy and Economic Research (INDO-DAPOER, World Bank Group, 2015). This dataset compiles different Indonesian official data sources into a single, comprehensive yearly data set until the year 2013.[28] Starting years and periodicity of the variables are determined by the original data sets.

I obtain village level data from the Village Potential Statistics, a survey of the universe of Indonesian villages. These data are collected simultaneously with the population census, or other universal data collection efforts, such as the agricultural and economic censuses. The data that I use include one survey wave prior to decentralization (1996), and five afterwards (2000, 2003, 2005, 2008, 2011). Village identifiers are rather inconsistent across waves, so I can only partially match data across survey waves, relying partly on statistical identifiers and names of localities. The linked sample includes 73% of all villages, about 50.000 villages every year.[29] The data that I use from the village potential survey include village population, basic geographical data, information on local institutions (Islamic schools, police, and whether the village head was

[28] Such as district government financial reports, district level gross regional product from the Central Statistical Office, the National Labour Force Survey (SAKERNAS), the National Socioeconomic Survey (SUSENAS), the Village Potential Survey (PODES)

[29] The size of the successfully linked sample is similar to that in Martinez-Bravo (2014)

elected or appointed), and a standardized index that I construct from the hundreds of variables on local service availability and infrastructure quality.[30]

We hand-collected data on district heads, candidates and election outcomes from online news sources and regional statistical offices.[31] I also use two other data sources to present auxiliary evidence and to check the robustness of the results. One of them is the National Violence Monitoring System (Sistem Nasional Pemantauan Kekerasan, SNPK), an Indonesian database compiled by the Ministry for Human Development and Culture, with support from the Habibie Center and the World Bank, which records all violent incidents with geographical location, incident type, information on victims and perpetrators. Started in 1998 as a pilot program in the island of Maluku, it has been gradually expanded over the following years. It already covered about half of the country in 2005, when the first direct district head elections took place. About 40% of the sampled districts had at least one religious policy in 2013.

The other auxiliary data source is the Indonesian Family Life Survey of the RAND corporation, an on-going longitudinal survey in Indonesia, representative of about 83% of the population and contains over 30.000 individuals (Strauss et al., 2009, 2016). About 50% of the sampled individuals lived in a district that had a religious policy by the 5th wave of the survey.

The study period differs somewhat across empirical specifications due to data availability. Village-level exercises look at the 1996-2011 time frame, district level estimations use data from 2002 to 2013, except those which use election data, as the first direct elections for district heads took place in 2005. The geographical scope is the whole country, except Aceh, Papua and Jakarta. The first two of these enjoy higher degree of autonomy and have a history of strained relationship with the central government. The capital of Indonesia is not self-administered on the level of districts.

3.4 Selection and stylized facts

Table 1 presents descriptive statistics on districts. The first column presents the average across all districts in the first year when the variable was available in INDODAPOER.[32] The second column shows the average of the group which did not end up with a Sharia-regulation until the end of the study period (the "control group"), while the third shows the average that implemented at least one until 2013 ("treated group").

[30] See Appendix A on more information of the government services index.
[31] For helping in the collection of this data I owe gratitude to Giannisa Novi Budiutami and Zsolt Hegyesi.
[32] This mostly means the year 2000, when only 3% of districts had a religious policy.

1. Development indicators.

Districts that eventually introduced Sharia policies had higher GDP, higher GDP per capita, higher urbanization and lower poverty rates at the beginning of the study period. The differences are all statistically significant. This means that Sharia-introducing regions are more developed on average.

2. Public finances.

Table 1 shows that total revenue and expenditure are not statistically significantly different across the two groups initially, while districts that eventually introduce religious policies have somewhat lower per capita figures already in 2001. Fiscal decentralization happened in 2005, which greatly expanded district budgets. From this point on, a substantial part of national resource revenues were allocated to districts and provinces where they were generated, and automatically allocated block grants to local governments were expanded.[33] This exacerbated the already existing selection, though in an observable and exogenous way. The left panel of Appendix Figure F.1, a binned scatterplot, shows the correlation between average block grants per capita and the probability of introducing a religious policy over the whole study period. The unit of observation is a district-election cycle. We see a statistically significant negative correspondence between budget size and the probability of introducing a Sharia regulation.

From the model in Section 5 a similar prediction arises. Higher office value (the model equivalent of a bigger budget) will increase desired reelection probability of the incumbent, which curtails the supply side incentive to introduce Sharia policies.

4. Labor costs.

Ideally, I would want to compare wages across districts, because labor costs are a substantial barrier for an incumbent who has to rely on a patronage network to get reelected. In a booming economy, where wages are high and jobs are abundant, politicians potentially have a harder time buying elections with job offers. As systematic data on a district level was not available for the analysis, I proxy labor costs with the minimum wage.

Generally the minimum wage is set on a provincial level in Indonesia, which is the administrative tier above the districts. Districts have the legal power to set a higher level locally, but

[33] Dana Alokasi Umum, or DAU for short.

Table 1: Descriptive statistics

	All districts	No RP until 2013	RP until 2013	Difference
Number of obs.	286	172	114	
Population (thousands)	698.6	627.20	814.27	187.06
	(616.62)	(528.83)	(725.07)	(77.7)
Log (real GDP/cap) -2002	1.29	1.17	1.46	.29
	(.64)	(.67)	(.55)	(.05)
Poverty rate - 2002	18.4	20.3	15.77	-4.54
	(9.09)	(9.09)	(8.45)	(1)
Poverty Gap (index) - 2002	3.1	3.46	2.61	-.86
	(1.85)	(1.95)	(1.58)	(.2)
% of pop. urban	39.28	33.98	47.34	13.36
	(32.55)	(30.23)	(34.4)	(3.86)
Islamic schools / 100.000 people	11.35	9.19	14.85	5.65
	(16.38)	(15.55)	(17.14)	(2.06)
Log(minimum wage) - 2002	12.24	12.23	12.25	.02
	(.12)	(.13)	(.1)	(.01)
Log(total revenue) - 2001	25.94	25.9	25.99	.09
	(.52)	(.47)	(.60)	(.06)
Log(total revenue pc) - 2001	12.94	13	12.86	-.14
	(.60)	(.57)	(.63)	(.07)
Log(total expenditure) - 2001	25.85	25.81	25.9	.09
	(.52)	(.46)	(.60)	(.01)
Log(total expenditure pc) - 2001	12.85	12.91	12.78	-.13
	(.56)	(.51)	(.61)	(.06)

This table shows summary statistics that describe pre-religious policy differences between districts in Indonesia. Where not indicated, the year is 2000. All data are from the INDODAPOER dataset, except for the Islamic school variable, which is from the 2000 wave of the PODES survey, and it shows the sum of pesantren and madrasah diniyah schools per 100.000 inhabitants of the district. The first column shows the mean of all districts, the second the group of districts that did not have a religious policy (RP) until 2013, the third columns show the districts that had until one at least in 2013. The fourth Column shows the difference between the two groups in the first year the variable is observed. Standard deviations of the variables are found in parenthesis in Columns 1 - 3. In Column 4, the standard error of the difference is in the parenthesis.

not a lower one. This means that the provincial minimum wage is an exogenous lower bound from the point of view of district governments. In the right panel of Figure F.1 I show a binned scatterplot where I plot the probability of having a religious policy against the natural logarithm of the minimum wage in real terms. Again, the unit of observation in the data is a district-election cycle. This shows a positive correlation: where employing people is more costly, Sharia regulations will be more likely to introduced.

In the model presented in Section 5 incumbents are constrained by the cost at which they can hire workers into their clientele. This gives rise to a similar association as seen in the data. In districts where this constraint is tighter, the incumbent will be more likely to introduce a Sharia regulation.

5. Districts which have more Islamic schools are more likely to introduce Sharia regulations

Districts which ended up introducing religious policies had on average 11.4 Islamic schools per 100.000 inhabitants in 2000, while those that would not, had 9.2. The difference is statistically significant.

The umbrella term "Islamic school" in this case refers to the Indonesian institutions called *pesantren* and *madrasah diniyah*. The pesantren are traditional boarding schools where students learn along a mixed religious-secular curriculum and receive vocational training as well. These schools are important centers of authority, and their leaders (the *kiyai*) are often courted by politicians during campaigns. Buehler (2016) calls these schools the nodes of the grassroots Islamist movement that rallies people for the cause of Sharia. Madrasah diniyah are religious schools that offer extracurricular religious education for students who otherwise participate in secular education. Other forms of religious education exist within (or closely associated with) the formal education system, so one can religious education without entering the grassroots Islamist movement.[34]

Pesantren and madrasah diniyah are unique as they are the only institutions which mostly focus on religious education and are exclusively private ventures. Because of this, and because of the wide agreement in the literature that these schools are among the main promoters of the religious turn in Indonesia, I will use the presence of these schools as a proxy for the grassroots

[34] In particular, Nahdlatul Ulama and Muhammadiyah (the main Indonesian religious movements, which also happen to be the largest independent Islamic organizations of the entire world) have an extensive educational network covering the whole country, and state institutions that follow a partly religious curricula also exist.

demand for religious legislation.

Table 2 shows a "horse race" where I regress a dummy indicating if the district had a Sharia regulation in place first on the Islamic school variable (Column 1), then I present bivariate specifications with Islamic schools and a selected other potential predictor (Columns 2 to 7), and finally, all these variables combined (Column 8). In Panel A the contemporaneous levels of alternative predictors are included. Column 1 shows raw correlation between Islamic schools and the incidence of religious policies in every district and year where direct elections were already in place. All further columns are estimated with district and year fixed effects included. Standard errors are clustered on the level of districts.

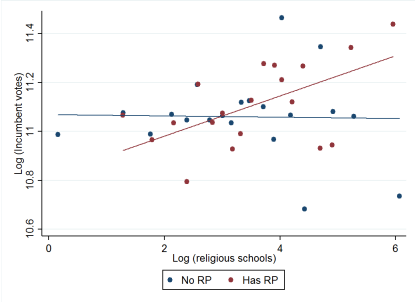
In Columns 2 to 7 I show various other potential variables that might be driving the introduction of religious policies, while Column 8 uses all predictors simultaneously. These variables are GDP per capita, local government revenue per capita from block grants (the largest revenue component for all districts, and the one which is the least variable across years), unemployment rate, inequality and poverty rate. All variables are from INDODAPOER (except for the number of Islamic schools which is aggregated to the district level from PODES). Observation counts differ because of missing data.[35] The inequality measure is defined as the log difference in monthly expenditure of the average household and the household in the lowest income quintile (also from INDODAPOER). Panel B replicates Columns 3 to 8 from Panel A including the change of the variable in question over the past year.

Including additional variables always increase the magnitude of the coefficient on Islamic schools compared to the baseline, suggesting that several factors might be at place which can demand religious policies either from the demand or the supply side, but Islamic schools have an autonomous role. Signs of other predictors are largely in line with either the predictions of the model in Section 5 (such as the positive coefficient on GDP and block grants), or existing literature and common sense (higher inequality is associated with extremism becoming more frequent). The negative coefficients on poverty are somewhat counterintuitive at first glance (as they mean that Sharia regulations are less likely in high poverty areas), but they actually are consistent with the predictions of the model (poor people are probably easier to target when the incumbent is building a clientele).

To sum up, demand for Sharia, as captured by the presence of Islamic schools, is the best predictor of Sharia regulations. This is in line with the difference-in-differences assumption that

[35] INDODAPOER compiles data from several government agencies, so the original data coverage varies a lot across variables.

Figure 3: Incumbent votes and Islamic schools



The figure shows a binned scatter plot shows the log number of incumbent votes (Y axis) against the log number of total pesantren and madrasah diniyah Islamic schools in the district. The linear regression is estimated for districts which had a religious policy already and for those that do not. The bins are created from district-election cycle variables. The incumbent votes are only positively correlated with the number of schools in districts and years where religious policies were introduced. Controls: logs of population, GDP/cap; year dummies

trends in the outcome variables of introducing and non-introducing districts should be parallel, while in Sharia demand it should be not.

While the number of schools is certainly strongly correlated with the incidence of religious policies, it is not clear whether this link has anything to do with the electoral process. Figure 3 provides an illustration for this question. This figure is a binned scatterplot based on district-election cycle observations of data. The horizontal axis represents the log of the number of Islamic schools in the district, while the vertical axis shows the number of votes the incumbent official received during the election. The solid line plots the linear correspondence between these two variables for districts and cycles where the incumbent did not implemented a religious policy. The point estimate of the linear coefficient is precisely estimated at zero. Importantly, these district-election cycle observations constitute the majority of data points.

The dashed line plots the same correspondence for elections following district head terms where a religious policy was introduced. Under such circumstances the incumbent district head votes are positively correlated with the number of schools, and the slope of the estimated linear correspondence is positive and significant. Incumbent votes are only proportional to the number of Islamic schools in those cases where the incumbent engaged in religious policy making, suggesting that these institutions are a good proxy for the size of the electorate who can be mobilized using Sharia regulations.

5. There is no "trivial" economic explanation of the introduction of religious policies.

The fact that distress causes an increase in religious participation and an increased salience

Table 2: Predictors of religious policies

Panel A: $P(religious\ policy_{rt}) = \beta_1 Log(schools+1) + \beta_2 X_{rt} + \alpha_r + \lambda_t$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log(schools+1)	0.00992***	0.0148**	0.0142*	0.0174**	0.0150**	0.0173**	0.0148**	0.0203**
	(0.00214)	(0.00685)	(0.00740)	(0.00680)	(0.00736)	(0.00781)	(0.00681)	(0.00854)
Log(GDP/pc)			0.000205					0.0305
			(0.0418)					(0.0497)
Log(Block grants pc.)				-0.00844				-0.0116
				(0.0128)				(0.0146)
Unemployment rate					0.00125			0.00255
					(0.00254)			(0.00313)
Inequality						0.0492**		0.0371*
						(0.0196)		(0.0194)
Poverty rate							-0.00420*	-0.00184
							(0.00231)	(0.00295)
Observations	3184	3184	2754	3084	2995	2786	3179	2143
Model	OLS	FE	FE	FE	FE	FE	FE	FE

Panel B: $P(religious\ policy_{rt}) = \beta_1 Log(schools+1) + \beta_2 X_{rt} + \beta_3 \Delta X_{rt} + \alpha_r + \lambda_t$

	(1)	(2)	(3)	(4)	(5)	(6)
Log(schools+1)	0.0143*	0.0189***	0.0143	0.0214***	0.0162**	0.0216**
	(0.00742)	(0.00701)	(0.00916)	(0.00769)	(0.00691)	(0.0106)
Log(GDP/pc)	0.0362					0.0787
	(0.0446)					(0.0965)
Δ Log(GDP/pc)	-0.143					-0.132
	(0.0892)					(0.131)
Log(Block grants pc.)		0.00287				0.00106
		(0.0153)				(0.0119)
Δ Log(Block grants pc.)		-0.0117**				-0.00545
		(0.00576)				(0.00559)
Unemployment rate			0.00367			0.00695
			(0.00365)			(0.00560)
Δ Unemployment rate			-0.00308			-0.00410
			(0.00325)			(0.00417)
Inequality				0.0622**		0.0793**
				(0.0313)		(0.0390)
Δ Inequality				-0.0228		-0.0528*
				(0.0205)		(0.0294)
Poverty rate					-0.00502**	-0.00598
					(0.00252)	(0.00365)
Δ poverty rate					0.00468	0.0125
					(0.00302)	(0.00774)
Observations	2754	2935	2685	2305	3082	1481
Model	FE	FE	FE	FE	FE	FE

This table shows a Fixed Effects regression of the dummy for having a religious policy on the log of Islamic schools in the district , against different sets of controls. District and year fixed effects are included and standard errors are clustered on the level of districts. Standard errors are clustered at the district level. *: significant at 10%; **: significant at 5%; ***: significant at 1%.
All control variables are from INDODAPOER. Observation numbers vary because missing data in INDODAPOER, as it collects data from various sources. Panel A shows levels of the alternative control variables, while in Panel B the yearly differences are included as well.

of religion in politics has been documented many times using data ranging from medieval Italy to late 1990s Indonesia (Belloc et al. 2016; Chen, 2010). It is important to note, however, that post-decentralization Indonesia since 2001 was not such an environment, where systematic economic shocks could have been the most important determinant of religious policymaking. To illustrate this, in Panel A of Figure F.4 of the Appendix I plot the average change in per capita GDP of districts over the years of the study period, and the average yearly incidence of religious policies. Average growth rates of the districts remains positive over the whole study period, even in the Great Recession years. There is no visible systematic aggregate relationship between religious policies and GDP growth. Panel B checks if such relationship exists on the disaggregate level. In this binned scatterplot the horizontal axis shows the yearly change in the logarithm of real GDP per capita, while the vertical shows the incidence of religious policy for each bin in the same year, the next year, and the year after that. The estimated slopes of the regression lines are close to zero and are not statistically significant. This suggests that aggregate economic fluctuations are not responsible for the wave of Sharia policies.

4 The impact of religious policies

4.1 Outcome variables of interest

I am measuring the impact of Sharia regulations on three set of variables. The first set contains expenditure variables of the local government, as the substitution channel of divisive policies implies that local government spending decreases and less people are in public employment if religious policies are introduced. I look at the logarithm of the district-level public sector (real) wage bill first. Then, since I do not observe the number of people employed nor public sector wages directly, I define two proxies. The first I call implicit public employment. This measures the number of people that could have been in public employment in the case if every one of them were employed at the minimum wage.[36] The third outcome measures the ratio of implicit public employment to the population of the district.[37]

The second set contains the village level government services index, where the substitution

[36]The minimum wage is obviously not an exogenous variable, but it is set on the provincial level, which is one level above the unit of the current analysis. That is, for every year there are at most 30 different minimum wages corresponding to each province of Indonesia. Indonesia currently has 34 provinces, but Aceh, Jakarta, Papua and Western Papua provinces are excluded from the analysis.

[37]It is typically bigger than 1 (median: 1.46, mean: 1.68), also attesting that public servant compensation is considerably more generous than the minimum wage. Nevertheless, this scaling facilitates the comparison of public employment across districts of different size.

channel also implies a negative impact. If government productivity is not increasing as a consequence of Sharia regulations, less spending translates to less government services provided.

The third set contains outcomes on both the district and the village level. The set includes poverty indicators and different measures of violence. What these variables have in common is that they are all potentially affected through both the indirect and the direct channel. The indirect substitution effect on these outcomes will be plausibly negative, as absence of government services increases poverty (Keefer and Khemani, 2004), and if state activities are in decline, law and order can be at risk.

Qualitative studies also suggest that Sharia regulations sometimes instigate vigilante violence (Pisani, 2014; Buehler, 2016; Nastiti and Ratri, 2018). Buehler writes about the connection between Sharia, corruption and violence in West Java:

> "In West Java, beyond the Islamist movement's core of activists and religious teachers and students, there is an outer layer that is less ideologically inclined. Many of the foot soldiers of Islamist groups here are petty criminals and hoodlums who sought new income streams [..]. This Lumpenproletariat is usually in the forefront of anti-vice demonstrations, sweeps on nightclubs and liquor stores, the extraction of bribes and levies and violence against religious minorities." (Buehler 2016, Chapter 2)

Buehler also tells us that radicals are actively involved in the rent-seeking efforts of politicians. He recounts an example from the city of Bukittinggi, where "the enforcement of anti-vice regulations against certain places was conducted by a group of thugs who were supporters of the mayor" (Buehler 2016, Chapter 7). The anecdotal evidence provided by these sources suggests that violence by religious groups (documented in the SNPK dataset) can be interpreted at least partly as a cover-up for corruption. Unfortunately, there is no comprehensive dataset on explicit corruption cases that I am aware of, so directly the documenting the impact of Sharia policies on corruption is beyond the scope of the current paper.

4.2 Difference-in-differences

In this section I discuss whether the presence of a religious policy changes the way the local

government spends its resources. The outcome that I am looking at is public employment. To capture this effect, I consider the following empirical model in the case of the spending data:

$$y_{rt} = \beta RP_{rc} + \gamma X_{rt} + \alpha_r + \lambda_t + \varepsilon_{rt} \quad (1)$$

where y_{rt} denotes the outcome of interest in district r in time t. RP_{rc} is a dummy indicating if a religious policy has been implemented in district r and election cycle c. Alternatively, I could use the calendar year of the introduction of the policy, which I will indeed do for the other outcome variables. I use this definition in this particular case as I am hypothesizing the existence of two distinct channels of impact of the Sharia regulations, the direct effect of the policies and the indirect effect due to trading off supplying public services for ideology. Changes to expenditure are by definition belonging to this second, indirect effect.

Direct and indirect effects are not necessarily happening at the same time. We know, however, that politicians who introduce austerity measures have a tendency to front-load these over the electoral cycle (König and Wenzelburger, 2017), so one can argue that politicians who know they would introduce Sharia regulations might start cutting back expenditure even before the policies themselves are introduced. This justifies the election cycle based definition of the policy variable RP_{rc} for this particular set of outcomes. The substitution hypothesis amounts to expecting a negative β coefficient on the religious policy dummy.

The rest of the variables in Equation 1 are α_r and λ_t district and time fixed effects respectively, and the vector of control variables X_{rt}. This set in the baseline specification includes the natural logs of population, real GDP per capita and government revenues per capita. Also included are the ratio of the nominal level of government revenues to the nominal level of GDP, and dummies controlling for administrative border changes in the province.[38] As religious policies might affect some of the control variables, I use their lagged values. The error term ε_{rt} represents idiosyncratic heterogeneity in the outcome caused by factors not accounted for by the policy variable, the controls and the fixed effects.

In the case of district outcome variables, such a the number of violent incidents, this slightly changes as the RP_{rc} changes index to RP_{rt-1}. This version of the dummy variable indicates takes the value of 1 if a religious policy was in place in district r at year $t-1$. I use calendar years because spending cuts associated with the substitution effect might not be synchronous

[38] These have an impact on how revenue redistribution across districts are calculated by the central government.

with the direct impact of the policies. Control variables are lagged twice in this case.

The specification takes the following form in the case of village level data (government services index and slum incidence dummy):

$$y_{vw} = \beta RP_{rw} + \gamma X_{vw} + \alpha_v + \lambda_w + \varepsilon_{vw} \tag{2}$$

A single data point refers to a wave-village observation: v indexes village, w indexes survey wave ($w \in \{1..6\}$), r indexes districts. Village- and survey wave fixed effects are used instead of district- and calendar-year dummies, respectively. The first outcome variable is the village level Government Services Index, which I compile from the Village Pontential Survey. It condenses all information on local infrastructure and locally supplied services that are consistently available for the waves of the survey into a single, standardized measure (see Appendix A for details on the content and construction of the survey). The second is a dummy variable indicating if the village had a slum, which is reported directly in the Village Pontential Survey, and is the only consistently available poverty indicator.

The policy variable is in this case defined as taking the value of 1 if a religious policy has been introduced in the district no later than the year preceding data collection from the survey. The set of control variables is in the baseline is limited to the number of Islamic schools (in logs) and log of population due to data limitations. The terms α_v and λ_w are village and time fixed effects.

4.2.1 Identification assumptions

The key identification assumption is that ε_{rt} (and ε_{vw} in the village regressions) does not include unobserved variation that is correlated with both the introduction of the policy and the outcome. To assess the validity of this assumption, one should note that important potential confounders are included in the vector of controls, such as measures of government revenues and GDP, so results are controlling for differences in spending capacities and differences in economic performance.

There are many ways how omitted variables and reverse causality might bias the estimations. First, aggregate economic dynamics might conceal significant economic shocks that shift voter preferences and government behavior independently at the same time. Second, anticipation of

future (hence unobserved) economic shocks can trigger a simultaneous shift in both public service provision and election strategies. Third, changes in the outcome can reflect voter preferences that are correlated with but unrelated to their preference for Sharia-based policies. In particular, crime and corruption have been shown to be an important determinant of voting for Islamic parties in nation-wide elections in Indonesia (Henderson and Kuncoro, 2011). If this is the case, they can be common causes for both the religious policies and the diversion of government resources at the same time. Fourth, ability of politicians might drive the results, if incumbents turn to ideological issues after an unsuccessful term in office and facing a tough reelection campaign. I discuss these threats to identification after the results.

I also present the same results in an event study form. This helps us to assess the validity of the parallel trends assumptions (which in turn is the corollary of the assumption on the error term). In this exercise the sample is defined differently. I define the event year as the year in which the district introduced its first religious policy, and the event window to be ± 4 years to that event. The sample covers every district where the timing of the first religious policy is such that the whole time window is observed. For every district that had such an event I use as a comparison group every other district that had not experienced a religious policy during the same event window. On this sample I estimate the following specification:

$$y_{rt} = \sum_{s=1..4} \beta_{-s} e_{-s} RP_r + \sum_{s=1..4} \beta_{+s} e_{+s} RP_r + \gamma X_{rt} + \alpha_r + e_s + \lambda_t + \varepsilon_{rt}$$

where $e_{s,-s}$ are dummies indicating time relative to the event (so $e_{-3} = 1$ means that the observation represents a data point from 3 years before an event takes place); α_r, λ_t are district and calendar year fixed effects, respectively; RP_r is a dummy indicating if the district experienced the event of introducing the religious policy; while X_{rt} is the same set of controls as before. Coefficients $\beta_{-4..-1,1..4}$ represent the observed difference in the level of the outcome variable between introducing and non-introducing districts at years preceding (-4 to -1) and following (1 to 4) the event, when difference in the event year is normalized to zero. These coefficients are the data points of the event study.

In the village data case the estimating equation becomes:

$$y_{vw} = \beta RP_{rw} + \sum_{s=1..2} \beta_{-s} e_s RP_{r,w-s} + \sum_{s=1..2} \beta_{+s} e_s RP_{r,w+s} + \gamma X_{vw} + \alpha_v + e_s + \lambda_w + \varepsilon_{vw}$$

In this case, as survey waves are happening on average in every 3 years, the event is defined as receiving the first religious policy between two survey waves and the event window is only ± 2 waves to that event.[39] The set of control variables is the same as in the corresponding regressions.

4.2.2 Results

The results from the spending regressions are presented in Table 3. Panel A shows the results for the three outcomes for the whole study period. We observe a negative and significant impact on the log wage bill and implicit employment figures, and an insignificant impact of implicit employment to population ratios. Zooming in on years when the district heads were directly elected and had more discretion over policy (Panel B), the estimated effects are an order of magnitude larger and they are much less noisily estimated. Indeed, districts which introduce religious policies seem to employ less people.

Figure 4 shows the results in event study forms, which are in line with those from the regressions, showing a negative impact on the overall wage bill and the different proxies of public employment figures as well. However, though it is not significant, there is a pre-trend in the data, and the difference between introducing and non-introducing districts becomes insignificant by the fourth year after the event. As in the expenditure specifications the date of the event is defined as the start of the election cycle when Sharia regulations are introduced, it is plausible that the patterns we see in the figures are political budget cycles.[40] Districts that end up introducing Sharia usually have less cash in hand, as it was noted before both in the context of the model and empirically. This might be reflected in the insignificant level difference before the event. However, even these districts boost spending as elections loom closer. We can see this both before the event, and also by the fourth year after the event, when the difference becomes insignificant again. An alternative explanation for the pre-trend would be a fixed cost associated with Sharia at the first year the incumbent takes office. Normalizing the effect at $t = 0$ to 0

[39] To be precise, receiving the first religious policy before the year of the second survey wave. So for the surveys 2005 and 2008 an event is defined to happen in districts that adopted the first religious policy in 2005, 2006 or 2007.

[40] Reported already in the Indonesian context by Sjahrir et al., 2013

would, in this case, mean that the whole event study figure is shifted down.

For the government service village regressions I present the estimated coefficients for Equation 2 in Table 4. Villages of districts that have introduced Sharia-based policies have on average 8.5% of a standard deviation lower level of the Government Service Index. The event study in Figure 5 confirms the magnitude of the estimate and ascertains that there are no significant differences in pre-trends.

Table 3: Baseline specification

Panel A

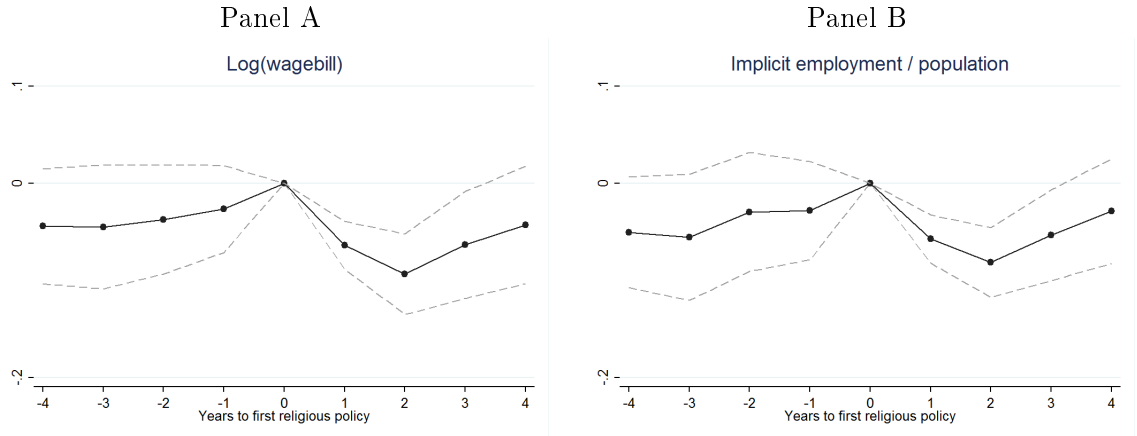
	(1) Log(wbill)	(2) Log(imp. emp.)	(3) Imp. emp. / pop.
Religious policy in cycle	-0.0350*	-0.0377**	-0.0418
	(0.0181)	(0.0173)	(0.0306)
Observations	3662	3683	3683
MeanY	25.53	12.82	1.689

Panel B

	(1) Log(wbill)	(2) Log(imp. emp.)	(3) Imp. emp. / pop.
Religious policy in cycle	-0.116***	-0.117***	-0.181***
	(0.0301)	(0.0296)	(0.0452)
Observations	2503	2524	2524
MeanY	25.53	12.82	1.689

This table shows a Fixed Effect regressions of different expenditure measures of the district government on a dummy indicating if the district had ha religious policy in the given election cycle. District and calendar year fixed effects, income, GDP (lagged values of GDP/capita and its growth rate), election dummies, population (in logs) are included as controls. Standard errors are clustered at the district level. *: significant at 10%; **: significant at 5%; ***: significant at 1%.
Panel A shows the whole study period, Panel B only the election cycles where the district head was directly elected. Election cycles are 5 year long. The first direct election could have occurred any time between 2005 and 2009 as it was scheduled when the incumbent district head completed his or her term.

Figure 4: Expenditure and clientele event studies



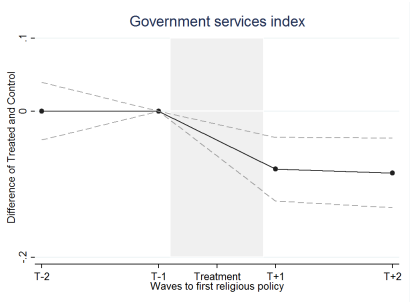
The figures shows event studies comparing different expenditure measures in districts which introduced a religious policy and districts that did not. The figure uses district-year observations. The events are defined as the start of the election cycle that saw the introduction of the religious policy. The term of the district head is five years. The control group is defined as all districts that had no religious policy or not yet within the event window. Panel A shows the log of the total wage bill. Panel B shows the implicit employment measure divided by the population of the district. The minimum wage is set at the provincial level. District fixed effects, income, GDP/capita (logs and lagged growth rates), calendar year dummies, election dummies, population (in logs) are included as controls. The 95% Confidence intervals are based on standard errors clustered at the district level

Table 4: Village panel main results

	(1)
	Gov. services
Religious policy	-0.0854**
	(0.0416)
Observations	273450

This table shows a Fixed Effect regression of the Government services index. The observations are village-year observations from the linked PODES data. Time- and village fixed effects and log of population are included as controls. Standard errors are clustered at the district level. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Figure 5: Government output event study



The figure shows an event study where the outcome variable is the standardized Government services index created from all variables consistently measured throughout the PODES survey (see Appendix A for details). The figure uses village - survey wave observations. An event is defined as the village being in a district that introduced the first religious policy between the two survey waves. The survey takes place together with every national census, in every three years on average. The control variables are village fixed effects, calendar year dummies and log of population. The 95% confidence interval is based on standard errors clustered at the district level.

Table 5 shows the main results for poverty. In Columns 1 and 2 we see the estimated β coefficients from Equation 1. Poverty rates were on average 1.1 percentage points higher in districts that had religious policies in place, while the poverty gap was higher by 0.22 points. These are sizeable effects as they represent 7.3% and 8.5% of the average values, respectively. Both results are strongly statistically significant. In Columns 3 and 4 I estimate the effect on poverty with a trend break at the introduction of the first religious policy. The trend breaks in both measures are positive and statistically significant, while the level shifts are negative though very noisily estimated.[41] In Column 5 I present the estimated β from the village panel, which shows that a village in a district with a religious policy is on average 1.6 percentage points more likely to have a slum area, which is a good 23% percentage of the average probability. This result is more noisily estimated, than the district regressions, but the effect is nevertheless significant at the 5% level. The poverty event studies show a very similar pattern for both district and

Table 5: Social effects main table

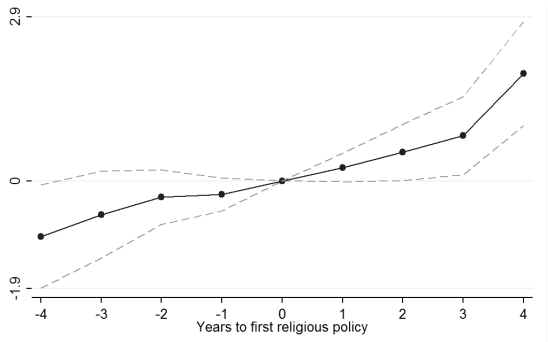
	(1) Poverty rate	(2) Poverty gap	(3) Poverty rate	(4) Poverty gap	(5) P(slum)
Religious policy	1.110***	0.224***	-0.967	-0.272*	0.0157**
	(0.295)	(0.0706)	(0.707)	(0.156)	(0.00678)
Years after RP			0.179**	0.0414**	
			(0.0849)	(0.0187)	
Observations	4049	4049	4049	4049	273450
MeanY	15.07	2.618	15.07	2.618	0.0685
hastrend	No	No	Yes	Yes	No
Data Source	region	region	region	region	village

This table shows Fixed Effect regressions of different social outcomes on a dummy indicating if the district had ha religious policy the year before the outcome was observed. Columns 1 to 4 include data from INDODAPOER, Column 5 shows village data from PODES. Accordingly, columns 1-4 use district-year observations, and the specifications include district fixed effects, income, GDP (lagged values of GDP/capita and its growth rate), calendar year dummies, election dummies, population (in logs) are included as controls. Standard errors are clustered at the district level in all models. *: significant at 10%; **: significant at 5%; ***: significant at 1%.
The poverty rate is the % of people living under the poverty line, the poverty gap is an index measure showing how far is the average poor person living from the poverty line. Columns 3-4 have the same measures interacted with a time trend starting at the year of the first policy. The outcome in Column 5 is a dummy indicating if the village had a slum when the survey was conducted.

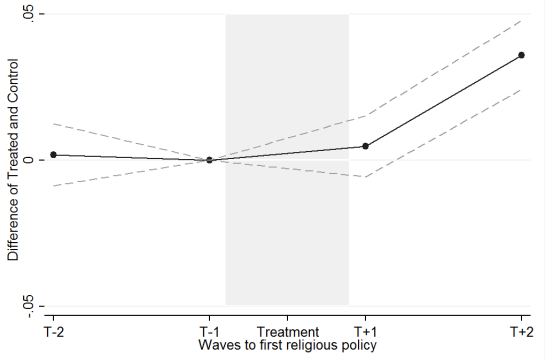
village level data (see Panel A and B of Figure 6).

Figure 6: Poverty event studies

Panel A: Poverty rate (district level)



Panel B: Incidence of slums (village level)



Panel A shows the difference between poverty rates of Sharia-regulation introducing and non-introducing districts. The figure uses district-year observations. The event is defined as the year of the first policy. District fixed effects, income, GDP/capita (logs and lagged growth rates), calendar year dummies, election dummies, population (in logs) are included as controls. The 95% confidence intervals are based on standard errors clustered at the district level.
Panel B shows an event study where the outcome variable is a dummy indicating the presence of a slum region in the village from the PODES survey . The figure uses village - survey wave observations. An event is defined as the village being in a district that introduced the first religious policy between the two survey waves. The survey takes place together with every national census, in every three years on average. The control variables are village fixed effects, calendar year dummies and log of population. The 95% confidence interval is based on standard errors clustered at the district level..

Finally, I check whether violent incidents increase in frequency after Sharia regulations are being passed. Table 6 shows the regression results. It is important to note that the smaller sample size is due to the fact that the National Violence Monitoring Data only covered about 50% of the country for most of the study period (see Table A.1 for details).

The results show that there are 23.33 more violent incidents on average, which translates to about a 28% increase. About 8 more people in every 100.000 falls victim to a violent crime.

[41] This peculiarity can be a consequence of the slope being very steep at the trend break when the policy is introduced.

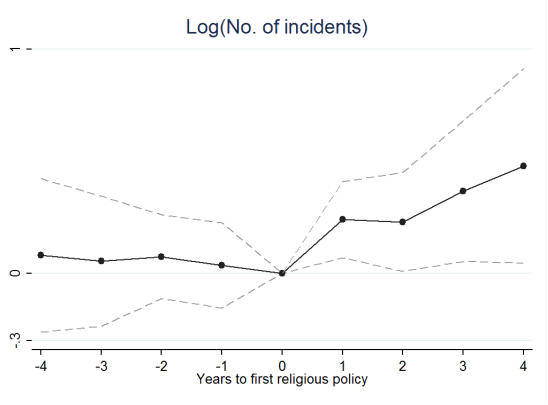
Figure 7 shows the same result in an event study and highlights that there were no significant differences in trends between adopters and non-adopters of religious policies previously.

Table 6: Sharia regulations as predictors of violence

	(1) No. of incidents	(2) No. of incidents/100000 people	(3) Log(No. of incidents)
Had RP (T-1)	23.33**	7.946**	0.279**
	(11.36)	(3.462)	(0.134)
Observations	921	921	921

The table shows Fixed Effects regressions where the outcome variables are different violence measures. The independent variable is a dummy indicating if the district had a religious policy in the previous year. The source of the data is the SNPK dataset, and the sample includes all districts that were covered by the SNPK. (See Figure A.1 for details). The specifications include district and time fixed effects. Standard errors clustered at district borders. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Figure 7: Event study on violent incidents (region)



The figure shows the difference between the log number of violent incidents Sharia-regulation introducing and non-introducing districts. The outcome variable comes from the SNPK dataset. The figure uses district-year observations. The event is defined as the year of the first religious policy. District fixed effects, income, GDP/capita (logs and lagged growth rates), calendar year dummies, election dummies, population (in logs) are included as controls. 95% confidence intervals are based on standard errors clustered at the district level

It is worthwhile to also look at violence statistics in a more disaggregate manner. Table F.9 does that by checking violence by which kind of perpetrators are more likely in districts that have Sharia-policies. Panel A shows a set of regressions where the dependent variables are dummies indicating whether the district in that year saw any violence by either a religious group, the government, or other political players (such as political parties, youth organizations). Panel B puts the number of incidents by the same groups on the left hand side, while Panel C uses the number of victims by each type of organized group. After a religious policy is introduced, the probability that the perpetrator of any given violent incident is a religious group is higher; there are overall more incidents by religious groups and these incidents have a higher number of victims. There is no such significant and systematic pattern with other types of perpetrators. This finding is in line with the anecdotal evidence that Sharia regulations empower local Islamists

(Buehler, 2016; Pisani, 2014). Table F.10 disaggregates incidents that involved human victims by types. Panel A puts the raw number of the left hand side, while Panel B uses the rates over 100.000 people. All types of crimes become more frequent after Sharia policies are introduced. In 6 out of 8 cases, the increase is statistically significant. It is worthwhile to note that there are on average 0.631 more people killed in districts that had religious policies. Though not directly comparable, this is put into some context by the fact that the murder rate (murders per 100.000 inhabitants) in Indonesia was 0.5 in 2016.[42]

Also, these figures are remarkably larger than the ones in the previous table, suggesting that more violence is done by unorganized perpetrators than organized ones. An additional channel through which this could take place is a partial atrophying of the state because of the spending cuts suggested both by theory and evidence. Though we should be cautious about causal interpretation, these figures are showing that there seems to be a direct effect of Sharia regulations through increased violence.[43]

4.2.3 Threats to identification

Local economic shocks

The first threat which can undermine the identification is that aggregate economic fluctuations might conceal more localized significant economic shocks which in turn can affect both voter behavior and government policy. Large-scale industrial investment will, for example, increase total GDP, while in the meantime might crowd out local small scale enterprises, or have devastating impact on local agriculture or the environment in general. If this is the case, the government can respond by realigning its spending to mitigate these particular impacts. In the meantime voter discontent can manifest as a demand to restore justice in a more general understanding. While such events cannot explicitly be controlled for, what I can do is to check whether including more disaggregate economic shocks as controls changes the results in any way.

First, I re-estimate Equation 1 with the lagged value of a rudimentary inequality measure included as a control. This measure is the ratio between the average household expenditure and the average household expenditure of the lowest 20% (Table F.15). Second, I re-estimate the

[42]Source: UNSDC/INTP/CTS statistics. The murder rate only considers intentional homicide, not killings during civil unrest.

[43]The organizations that collect crime data use local media sources, and data collectors might give an extra attention to districts that introduce Sharia-based policies. If this was the case, any event in these districts would have a higher probability of getting noticed, and so the estimate of the difference between violence levels in introducing and non-introducing districts would be biased upwards.

equation with lagged unemployment levels (Table F.16). Third, I re-estimate the specification with lagged values of sectoral changes of GDP, in particular the change in the percentage contribution of manufacturing and finances (Table F.17). I also include the poverty indicators as outcomes in all these robustness checks. None of the alternative specifications exhibit a major qualitative or quantitative difference compared to the baseline.[44] I conclude that there is no evidence that unobserved local economic shocks are driving the introduction of the religious policies.

Expected future economic shocks

The second potential threat to identification is that expecting a future economic shock (such as a budget cut) can simultaneously shift spending patterns and electoral strategies as well. Though I cannot directly control for expectations of politicians, what I can do is to see whether those expectations were justified. In Panels A and B of Figure F.7 in Appendix F I replicate the expenditure event studies with per capita total revenue of the local government and with per capita revenue from block grants (which is the single biggest revenue source and the one exhibiting the less variation over years). Although both event studies exhibit lower per capita incomes for districts that introduced religious policies, the estimated confidence intervals are very wide, so this effect is not significant. However, these lower levels are relative to non-introducing districts each event year, and does not necessarily mean a decrease in revenues in absolute terms. This is shown in Panel C of the same figure, where I plot average yearly growth rates of these revenue terms in all districts that ever had a religious policy, along with the average yearly incidence of the religious policies. On average the districts exhibit increasing revenue over the whole study period, except for the year 2005, when revenues stagnated before a huge increase in 2006; and the year 2009, when they fell dramatically. Fiscal decentralization was legislated in 2005, when the central government delegated huge revenue streams to the district public administrations. The 2009 drop in revenues is attributable to the global financial crises, which caused prices of raw materials to plummet, drying up revenue streams on every level. Note that the average occurrence of religious policies is actually declining over this period.

Though there is no evidence for expected future economic shocks to be driving the results,

[44] The reason why these control variables are not included there in the first place is that these variables are of inferior quality in INDODAPOER; in particular, they exhibit missing data points which are not accounted for by the data description. As a consequence, including these variables would have severely reduced the size of the sample. As part of an unreported robustness check I looked at whether the occurrence of missing data points is correlated with the introduction of the religious policies. It is not.

it is worth mentioning that these would not necessarily undermine the results. Such changes in expectations would simply provide variation in the incentives that incumbents face when considering the introduction of Sharia policies. A district head who, expecting economic hardship, introduces Sharia policies to cut costs in campaigning, would behave exactly according to the hypothesis outlined in this paper. As a consequence, I do not believe that this issue poses a serious threat to identification.

Voter demand

The third main threat to identification is the presence of voter demand for policies that are correlated with but unrelated to their preference for Sharia-based policies. Three different avenues for this argument come up. First, perceived corruption and crime can be correlated with diversion of public revenue, and these might translate into a religion-based argument for restoring justice and lawlessness. Second, religious voters might have preferences for different public goods, or less "secular redistribution" in general, and prefer a smaller government that does not interfere with how they are running their lives. If this is the case, a reduction in government spending can be considered simply a voter demand that is being met independently of religious policies.

Two things should be considered here. First, if demand for Sharia-regulations is due to corruption and crime which is also correlated with diversion of public spending, that should happen $before$ the policy, and not after it. Similarly, government services should be ex ante getting worse, which they are not. A similar argument about timing can be made with the other preference-driven explanation as well.

For a more thorough investigation of these arguments I use two auxiliary data sources, the National Violence Monitoring System (SNPK) and the Indonesian Family Life Survey (IFLS) . Though neither of the two data sources covers the whole country, there is enough variation to carry out a meaningful comparison.

The former allows me to explore whether violent crime can predict the introduction of Sharia regulations. Appendix Table F.18 shows regressions of the religious policy dummy on 1) violent crime event counts 2) number of victims in a 100000 people (who was either injured, kidnapped, sexually assaulted or killed) 3) logarithms of event counts of districts in the SNPK. For each case I estimated a version with lagged values of the variable, and the lagged yearly difference in the variable. I added region and time fixed effects, logarithm of population and GDP per capita,

poverty rate and inequality as controls. There is no specification in which violent crime would be a significant predictor of Sharia regulations.

Petty crimes and crimes against property in general can also be a concern which is not addressed by using SNPK data. Though there is no detailed crime statistics data, the Indonesian Family Life Survey's community survey chapter asked about the perceptions of village notables about crime and corruption in 2007. Unfortunately this sample is extremely limited, and resorts to two respondents per district. Table E.4 in the Appendix shows that crime perception in 2007 in districts that had no religious policy by then does not significantly correlate with introducing a religious policy between 2007 and 2013. Similarly, Table Appendix E looks at villages in 2007 without a religious policy and checks whether their perceived levels and trends of corruption are correlated with later Sharia implementation. Interestingly, it seems that Sharia regulations between 2007 and 2013 were *less* likely to happen in districts where the village leaders reported higher corruption levels in the district. This goes against the argument that demand for Sharia regulations would be a consequence of perceived corruption.

IFLS also gives an opportunity to test indirectly whether religious voters have different preferences for policy. In Table E.5 of the Appendix I check whether different forms of religious cooperation become more frequent in districts that implement Sharia-based policies. If government-provided public services are substituted for services provided by religious communities, these forms should become more frequent. This is not the case for any form of religious cooperation that is queried in the IFLS. People living in districts that introduce Sharia regulations do not receive more donations from religious organizations, do not participate more frequently in religious community work or microfinance, nor do they in more general types of religious community events. To sum up, there is no evidence from the additional data sources that these alternative channels undermine my core empirical strategy.

The result that government services shrink after Sharia regulations are introduced might represent a shift from government provision of services to private provision by religious organizations. In other words, it might be an issue of mismeasurement, not overall decline on services (though this does not explain the increase in poverty). To assess this concern, I re-calculate the index in question excluding all raw variables that plausibly have a private counterpart, such as education. The remaining variables are purely concerning physical infrastructure, such as roads, sanitation and phone availability. In Table F.22 I show all regression results using the Infrastructure Index instead of the Government Services Index. The estimated coefficient is similar in

magnitude in the baseline specification, though not significant. I replicate this exercise in the later specifications as well, with more robust results.

Underperforming incumbents

Finally, ability of politicians can drive the results, if incumbents who underperform (who create less public services) turn to religion for a quick win as they are running for reelection. We can test this hypothesis by looking at the timing of introducing Sharia policies over the electoral cycle. District heads are elected for five year terms, and can be re-elected once. If low incumbent performance is the mechanism behind Sharia regulations, we should see Sharia introduction peaking at around the 4th year of the 1st term of an incumbent. Why late in the cycle? If a Sharia regulation is adopted early, it is more likely done to fulfil an electoral promise. If it is adopted later in the cycle in an election campaign, it is more likely that it serves as a campaign tool and hints at the possibility of this confounding mechanism at work. Why in the 1st term? District heads can only be reelected once, so an incumbent in his second term does not need to campaign for reelection.

Panel A of Appendix Figure F.3 shows the probability of adopting a religious regulation in years defined by the electoral cycle. The solid line corresponds to Sharia regulation likelihood under first term district heads, the dashed to reelected incumbents. 10% confident bands show that there is no statistically significant difference between first- and second-term incumbents, though 2nd term incumbents are on average more likely to introduce Sharia regulations. The fact that Sharia introduction probability is flat for first-term incumbents, and higher for second term incumbents (especially during the two years after election) suggests that regulations are not made to get a "quick win" after an unsuccessful term, as districts heads who are not eligible for reelection are the ones who are most likely to implement them. This is consistent with this paper' model's depiction of local politics: incumbents are the one who can most credibly commit to Sharia, and introduce it upon reelection, as the next term is none of their concern.

Why do they introduce Sharia regulations then? Pisani and Buehler (2016) argued that there are three main reasons why district heads implement Sharia policies. Supply-driven regulations (such as regulation of almsgiving), they argue, benefit the government. Demand driven regulations are either created to please the general populace (e.g. attire regulations), or religious interest groups (e.g. prohibitive regulations). The former are not generating material benefits to anyone, and are more likely around election time. The second group gives an opportunity to

extend the politician's patronage network, and as such creates electoral benefits for him, and material benefits for those who are tasked with enforcing, say, an alcohol ban (or collect protection money to avoid enforcing). As Panel B and C of Figure F.3 show, all the extra probability of incumbents to introduce Sharia come from prohibitive regulations. This is consistent with this paper's theory, which models Sharia-introduction as an agreement between a rent seeking incumbent and a small, organized minority.

Another argument against Sharia policies being driven by underperforming incumbents can be drawn from Table F.14 in the Appendix. This shows the share of Sharia-introducing and non-introducing incumbents who hold specific types of titles. We see that three in four politicians in the sample who introduce Sharia policies has the title "haji", meaning that he or she has completed the pilgrimage to Mecca; among non-introducing politicians 60% uses such a title. We do not know whether those who use of the title have actually completed the pilgrimage, or whether not using it means not having completed the journey or just having decided not to use the title. But it certainly means that a bigger share of incumbents who eventually introduce Sharia decide to signal their piety at the time of running for office. This goes against the argument that incumbents "find religion" only when reelection is looming close. It is also important that a bigger share of the introducing incumbents in the sample has any kind of title. This shows that Sharia-introducing incumbents are not of an ex-ante "worse type", which is again evidence against this confounding mechanism.

Remaining identification concerns

The difference-in-differences approach in any case assumes that the demand for Sharia regulations is exogenous to outcome variables, and parallel trends hold for introducing and not introducing districts in all variables except for demand for Sharia. This is view is supported by Bazzi et al. (2018) who study how unexpected and transitory policy shocks during the Suharto era strengthened Islamist networks in some parts of the country, but not others. If, however, demand shifts for Sharia are not exogenous to the outcomes I study, the difference-in-differences results would possibly be biased. To tackle this issue, I introduce two instrumental variables (IV) strategies to support the results. As some of my outcome variables are on a village level, while others are on the district level, I have one village level and one district level instrument to exploit plausibly exogenous variation in the demand for Sharia.

4.3 Instrumenting with local demand variation

The first instrument uses within-district, village level variation in the demand for Sharia. Using this strategy I eliminate village-level omitted variables that might be correlated with demand.

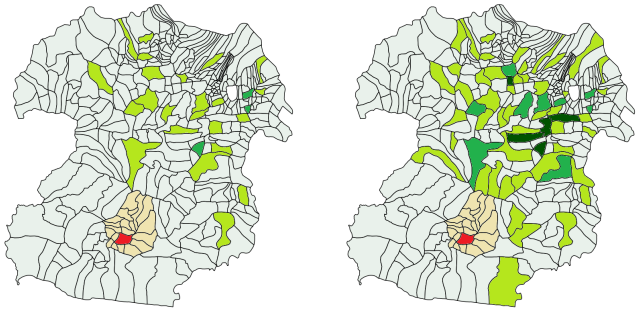
Imagine that we were able to directly observe the demand for Sharia regulations for every village. This may or may not be correlated with our outcomes of interest. Whether Sharia regulations are introduced or not is depending on the district level total demand for Sharia. Consider two districts $D1$ and $D2$, and two villages in them, A and A'. Let's say that A and A' has exactly the same level of Sharia demand, and neither of the districts has Sharia regulations in place. Then Sharia becomes more popular in district $D1$, because religious demand increases in villages outside A (call these villages B to Z). In the meantime, nothing similar is happening in villages $B' - Z'$ in district $D2$. Then, if demand in $D1$ has increased enough, the district will be introducing Sharia regulations. Then we can instrument the introduction of Sharia for village A with demand variation that is coming from the same district (villages B to Z). This way we compare the outcomes of A (which is now in a Sharia district) and A' (which still is not) by using only the variation in Sharia introduction probability that is coming from villages outside A in the Sharia district ($D1$), and villages outside A' in the other ($D2$). Demand for Sharia can itself be endogenous, the identifying assumption is that the confounding omitted variable is not correlated across villages of the same district.

I capture village level demand for Sharia with the number of private Islamic schools in the village, an information which I observe in the PODES survey.[45] Previous literature has been linked these schools to local support for Islamist policies (Bazzi et al., 2018). Figure F.2 shows that increase in the number of religious schools start before the policies are adopted, and not as a consequence of Sharia regulations. I use village level religious intensity (measured as the per capita number of schools) as a measure of Sharia demand on the village level. For any particular village this measure might be endogenous. So, as an instrument, I am going to use the per capita religious intensity of all *other* villages in the same district, or the leave-out-mean of religious schools per capita.

Figure 8 illustrates the idea. Between two time periods Sharia demand increases in some villages of the district, so the overall probability of introducing Sharia policies increases. With

[45] To be precise, the number of *pesantren* and *madrasah diniyah*. There is no Islamic equivalent of a "state church", all educational institutions are private.

Figure 8: Village IV illustration



The figure illustrates how the village level IV works. The maps in Panel A and Panel B show a district in T=1 and T=2, respectively. The smaller shapes correspond to the villages. The shade of green correspond to the per capita number of Islamic schools (pesantren) in the village, which proxies for Sharia demand. For the village marked with red the value of the instrument is the average Islamic school intensity coming from other villages in the same district. By T=2, the number of schools increased in the district, meaning that the average also increased, and for the red village this will mean an increased likelihood that the district introduces Sharia policies. The villages marked in orange are the villages in the same subdistrict. In a robustness check, I calculate the intrument without immediate neighbors of the village in the subdistrict. As these results are almost identical, I conclude that geographic spillovers across villages do not drive the results.

this IV I only use for every village the part of this increased probability that is exogenous (the part for which other villages in the district are responsible).

A thought experiment helps highlighting the logic of identification. Let's say that people who demand religious policies do not care about public services in general, for some unspecified reason. Then, if religious demand increases, demand for public services will decrease at the same time. A rational politician will introduce Sharia policies and cut back public services in a pure demand-driven policy fine-tuning, and it would be misleading to talk about a "tradeoff" between moral politics and public services. If, however, in village A we instrument the introduction of Sharia with Sharia demand in villages $B - Z$ of the same district, then this issue is resolved. Sharia demand can be correlated with demand for roads and landline phones in any village. The identification assumption in this case is that Sharia demand in village C is not correlated with demand for roads and landline phones in village A of the same district.

4.3.1 Identification assumptions

Formally, assume that the model in which the value of the village level outcome is determined takes the following form:

$$y_{vw} = \beta RP_{rw} + \gamma X_{vw} + \alpha_v + \lambda_w + \delta schools_{vw}/pop_{vw} + \varepsilon_{vw}, \tag{3}$$

again, RP is the dummy indicating the presence of a religious policy, X is te vector of controls, α and λ are fixed effects, while ε_{vw} is an error term. The subscripts v, w and r are

indexing villages, survey waves and districts, respectively. Religious intensity $schools_{vw}/pop_{vw}$ can be correlated with the error term ε_{vw}.

Let's model the event whether a religious policy is adopted as a function of the overall religious intensity of the district:

$$RP_{rw} = \eta \frac{\sum schools_{vw}}{\sum pop_{vw}} + \alpha'_r + \lambda'_w + \theta_{rw}$$

Where α'_r, λ'_w are fixed effects, and θ_{rw} is a district level, exogenous error term. The problem with this model is that, as village level religious intensities are correlated with the error term ε_{vw} , the predicted RP_{rw} will also be correlated with the error term in Equation 3. We can define, however, an alternative first stage, using as an instrument the leave-out-mean religious intensity, defined as $LOM_{vw} = \frac{\sum_{v',-v} schools_{v'w}}{\sum_{v',-v} pop_{v'w}}$ for every village v' in district r. The identification assumption is that $E[\varepsilon_{vw}(schools_{v'w}/pop_{v'w})] = 0$, that is, religious intensity can be endogenous in a village, but other intensity in other villages cannot.

The first stage of the statistical model takes the following form:

$$RP_{rw} = \eta LOM_{vw} + \alpha'_v + \lambda'_w + \theta_{rw}. \tag{4}$$

Throughout the paper, instrumental variables regressions are carried out using the software developed by Schaffer (2010).

4.3.2 Results

Panel B of Table 7 shows the estimated coefficients of Equation 4, and the reduced form regression of the Government Services Index on the instrument, while Panel A of Table 7 shows the main IV regression result (column 2) compared to the baseline fixed effects estimate (column 1). The instrumental variable coefficients suggest a much higher (-.44, marginally insignificant) decrease in the Government Services Index due to the policy. The statistical tests reject underidentification and weak identification.[46]

A higher coefficient than in the OLS case indicates that the latter is biased towards zero (which is in line with the stylized fact that *more* developed districts tend to introduce Sharia

[46]The Kleibergen-Paap Wald rk LM statistic is 14.95, the corresponding F statistic (justified due to using clustered SEs) is 19.09.

policies). The noisiness of the estimates in the instrumental variables strategy might be explained by treatment effect heterogeneity, as the IV gives an estimate for the local average treatment effect of the policy. If politicians cut back services in places which do not vote for them anyway, then the secular villages will have higher treatment effects which will be overrepresented in the average. Another explanation is that villages that had the most new religious schools are by construction have less variation in the instrument.

To check if these factors drive the results, in Tables 9 and 10 I re-estimate the IV on the subsample of villages that had no *pesantren* in 2000 or after, and on the subsample which had at least one at some point. Table 9 is of particular interest as it shows the average effect of the religious policy on villages that had no *pesantren*, which amounts to 70% of the villages. The coefficient is bigger in absolute terms, significant at 5% level (and not different from the estimated effect on all villages). In the meantime the estimated effect on the sample of villages with schools (Table 10) does not differ from zero. It might indeed be the policy effect is disproportionately higher for the villages that are less religious.

4.3.3 Threats to identification

The most important question is whether the *pesantren*-type of Islamic schools are a good measure of underlying demand for Sharia regulations. Previous work in the political science (Buehler, 2016) and economics literatures (Bazzi et al., 2018) make a strong case for this assumption, but I assess it using the data at hand.

First I conduct a placebo experiment to show that Islamic school instrument only matters for the outcome variables through the endogenous variable of the Sharia policy. This is equivalent to saying that the coefficient δ equals zero in Equation 3. To check this, I am regressing the Government Service Index on the leave-out-mean in 2000 (after democratization, but before religious policies really took off), and doing the same in a panel setting with the years 1996 and 2000. Table F.19 shows the result, with no sign of such correlation. Table F.20 shows the same exercise with the slum variable as an outcome.[47]

Another formulation of this test can be found in Figure 3 where I plotted the binned scatterplot of incumbent votes on the Y axis against the log of Islamic schools on the X axis for every election event, and estimated a different regression line for election events that were preceded

[47] Tables F.28 and F.29 present the version of the results where the instrument defined with the omission of the whole subdistrict, as defined in the previous robustness check exercise.

Table 7: IV regression: Government services

Panel A: OLS and IV regressions

	Baseline		+ Political vars.	
	(1)	(2)	(3)	(4)
	FE	IV FE	FE	IV FE
Religious policy	-0.0855**	-0.444	-0.0861**	-0.446
	(0.0425)	(0.273)	(0.0423)	(0.274)
Islamic schools / 1000 people	0.0188	0.0316**	0.0186	0.0313**
	(0.0170)	(0.0155)	(0.0170)	(0.0155)
Administrative village			-0.00889	-0.0130
			(0.0366)	(0.0371)
Remote village			0.0271	0.0547
			(0.0603)	(0.0667)
Village has Hansip police			0.0689***	0.0696***
			(0.0141)	(0.0146)
Observations	269552	269552	269201	269201
KPF		10.73		10.59

Panel B: First stage and Reduced form regressions

	First stage		Reduced Form	
	(1)	(2)	(3)	(4)
Islamic schools / 1000 people (other villages)	0.360***	0.358***	-0.160	-0.159
	(0.110)	(0.110)	(0.104)	(0.103)
Islamic schools / 1000 people	-0.00173	-0.00163	0.0324**	0.0321**
	(0.00286)	(0.00285)	(0.0153)	(0.0153)
Administrative village		-0.00337		-0.0115
		(0.0228)		(0.0368)
Remote village		0.0661**		0.0252
		(0.0275)		(0.0609)
Village has Hansip police		0.00385		0.0679***
		(0.00651)		(0.0141)
Observations	269552	269201	269552	269201

Columns 1 and 3 show Fixed Effect regressions of the Government Services Index on the dummy indicating a religious policy, population, district and time fixed effects and the control variables in indicated in the table. Columns 2-4 show Panel IV regressions where the instrument is the leave-out mean of Islamic schools (pesantren and madrasah) for every village. The leave-out mean is defined as the average number of schools, not counting the schools of the particular village. Columns 1 and 2 of Panel B show the First Stage of the IV estimation (the correspondence between the policy dummy and the instrument), Columns 3-4 show the Reduced Form of the IV estimation (the correspondence between the outcome and the instrument. All data are village-level data from INDODAPOER. KPF refers to the robust rk Wald F statistic of the weak instrumentation test (Kleibergen and Paap, 2006). Standard errors are clustered on the level of districts. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table 8: IV regressions: Prevalence of slums

Panel A: OLS and IV regressions

	Baseline		+ Political vars.	
	(1)	(2)	(3)	(4)
	FE	IV FE	FE	IV FE
Religious policy	0.0160**	0.0780	0.0159**	0.0797
	(0.00687)	(0.0642)	(0.00688)	(0.0650)
Islamic schools / 1000 people	0.00598**	0.00377**	0.00606**	0.00380**
	(0.00264)	(0.00172)	(0.00266)	(0.00173)
Administrative village			0.0290***	0.0297***
			(0.0104)	(0.0107)
Remote village			0.00686	0.00197
			(0.00880)	(0.00984)
Village has Hansip police			0.00125	0.00112
			(0.00315)	(0.00314)
Observations	269552	269552	269201	269201
KPF		10.73		10.59

Panel B: First stage and Reduced form regressions

	First stage		Reduced Form	
	(1)	(2)	(3)	(4)
Islamic schools / 1000 people (other villages)	0.360***	0.358***	0.0280	0.0285
	(0.110)	(0.110)	(0.0208)	(0.0209)
Islamic schools / 1000 people	-0.00173	-0.00163	0.00364**	0.00367**
	(0.00286)	(0.00285)	(0.00171)	(0.00171)
Administrative village		-0.00337		0.0295***
		(0.0228)		(0.0104)
Remote village		0.0661**		0.00724
		(0.0275)		(0.00874)
Village has Hansip police		0.00385		0.00143
		(0.00651)		(0.00318)
Observations	269552	269201	269552	269201

Columns 1 and 3 of Panel A show Fixed Effect regressions of the Slum dummy on the dummy indicating a religious policy, population, district and time fixed effects and the control variables in indicated in the table. Columns 2-4 show Panel IV regressions where the instrument is the leave-out mean of Islamic schools (pesantren and madrasah) for every village. The leave-out mean is defined as the average number of schools, not counting the schools of the particular village. Columns 1 and 2 of Panel B show the First Stage of the IV estimation (the correspondence between the policy dummy and the instrument), Columns 3-4 show the Reduced Form of the IV estimation (the correspondence between the outcome and the instrument. KPF refers to the robust rk Wald F statistic of the weak instrumentation test (Kleibergen and Paap, 2006) All data are village-level data from INDODAPOER. Standard errors are clustered on the level of districts. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table 9: IV regression: Effect on government services in villages without *pesantren*

Panel A: Government Services

	Baseline		+ Political vars.	
	(1)	(2)	(3)	(4)
	FE	IV FE	FE	IV FE
Religious policy	-0.0783*	-0.709**	-0.0796*	-0.709**
	(0.0455)	(0.315)	(0.0453)	(0.317)
Islamic schools / 1000 people	-0.00404	0.0169	-0.00427	0.0166
	(0.0138)	(0.0114)	(0.0138)	(0.0114)
Administrative village			-0.0145	-0.0178
			(0.0373)	(0.0402)
Remote village			0.107*	0.134*
			(0.0587)	(0.0732)
Village has Hansip police			0.0768***	0.0777***
			(0.0174)	(0.0186)
Observations	189418	189418	189091	189091
KPF		15.27		15.27

Panel B: Prevalence of slums

	Baseline		+ Political vars.	
	(1)	(2)	(3)	(4)
	FE	IV FE	FE	IV FE
Religious policy	0.0106	0.0174	0.0105	0.0177
	(0.00783)	(0.0346)	(0.00784)	(0.0346)
Islamic schools / 1000 people	-0.000334	-0.000559	-0.000293	-0.000532
	(0.00281)	(0.00237)	(0.00281)	(0.00238)
Administrative village			0.0157*	0.0157*
			(0.00847)	(0.00848)
Remote village			0.0100	0.00974
			(0.00879)	(0.00883)
Village has Hansip police			0.000901	0.000891
			(0.00348)	(0.00347)
Observations	189418	189418	189091	189091
KPF		15.27		15.27

Estimates a version of the specifications in Table 7 and 8and with the sample reduced to villages which never had a *pesantran*-type Islamic school over the study period. These are roughly 70% of the villages. The regressions are otherwise identical. All standard errors are clustered on the district level. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table 10: IV regression: Effect on government services in villages with *pesantren*

Panel A: Government Services

	Baseline		+ Political vars.	
	(1)	(2)	(3)	(4)
	FE	IV FE	FE	IV FE
Religious policy	0.00460	0.169	0.00386	0.182
	(0.0427)	(0.376)	(0.0426)	(0.383)
Islamic schools / 1000 people	0.0518***	0.0505***	0.0515***	0.0502***
	(0.0157)	(0.0152)	(0.0156)	(0.0151)
Administrative village			0.0320	0.0379
			(0.0713)	(0.0733)
Remote village			0.0292	0.00934
			(0.0864)	(0.0922)
Village has Hansip police			0.0561*	0.0568*
			(0.0313)	(0.0315)
Observations	40384	40384	40368	40368
KPF		5.280		5.160

Panel B: Prevalence of slums

	Baseline		+ Political vars.	
	(1)	(2)	(3)	(4)
	FE	IV FE	FE	IV FE
Religious policy	0.00820	0.0746	0.00858	0.0792
	(0.00858)	(0.0897)	(0.00859)	(0.0921)
Islamic schools / 1000 people	0.00149	0.00101	0.00160	0.00109
	(0.00286)	(0.00296)	(0.00288)	(0.00298)
Administrative village			0.0606***	0.0629***
			(0.0212)	(0.0217)
Remote village			-0.00164	-0.00949
			(0.0208)	(0.0213)
Village has Hansip police			-0.00639	-0.00611
			(0.00806)	(0.00786)
Observations	40384	40384	40368	40368
KPF		5.280		5.160

Estimates a version of the specifications in Table 7 and 8 with the sample reduced to villages which had at least one*pesantran*-type Islamic school over the study period. These are roughly 30% of the villages. The regressions are otherwise identical. All standard errors are clustered on the district level. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

by the introduction of a religious policy and for election events where it was not. The figure illustrates that religious schools only seem to matter in terms of incumbent votes when the incumbent introduces the religious policy. Of course, the lack of evidence on a direct impact of religious intensity on outcomes is not an evidence on a lack of impact, but it is consistent with this proposition.

Next I look at the identification assumptions of the instrumental variables regression more directly. The assumption was that Sharia demand in village A is not correlated with the error term in village B. If this assumption is violated, then the other villages' religiosity will not create an exogenous shift in the incidence of religious policies from the point of view of one particular village, and thus the instrument would be compromised. To address this concern, I re-calculate the leave-out-mean for every village now with the omission of not just the village itself, but also all other villages in the same subdistrict, so immediate neighbors Islamic schools are not taken into account either.[48] Tables F.24 and F.25 are the corresponding outputs from this exercise, without any remarkable difference, so local spillovers are arguably not driving the results.

4.4 Instrumenting with pre-policy differences and growth rates

Using pre-policy differences in preference for Sharia and the country-wide growth rate in religiosity I can predict later demand for Sharia, and can use this prediction as an instrument for implementing Sharia policies. This instrument is similar to the shift-share class of instruments, and is defined as follows:[49]

$$shiftshare_{rt} = share_{r,0} * growth_t$$

The first term of the product, $share_{r,0}$ is a measure of the pre-policy variation in the demand for Sharia-policies. I use three different specifications for this. The first is the share of *pesantren* among all comparable level of education in 2000 (this is the closest in definition of the classical shift share instrument). The second is the share that all Islamic schools in a given district represent in of the national total of such schools in 2000. The third is a historical vote share of a party that advocated the full implementation of Islamic Law in the only pre-Suharto era free election in 1955.[50] The results are robust to the choice of the pre-policy Sharia demand variable.

[48] Subdistricts are called *kecamatan*, these are the administrative level between the village and the district.
[49] See Bartik 1991 or Altonji and Card (1991).
[50] Masyumi party. Vote shares are obtained from the http://www.pemilu.asia website; missing data are imputed from provincial averages reported in Feith (1955).

The variable $growth_t$ is the country-wide yearly growth rate of number of *pesantren* schools compared to the baseline level in 2000. The idea behind the instrument is to form a prediction for eventual demand for Sharia using pre-policy demand and the country-wide trend, and then use predicted demand for Sharia instead of the potentially endogenous actual demand.

4.4.1 Identification assumptions

Formally, the estimated model and the corresponding first stage are the following:

$$y_{rt} = \alpha_r + \lambda_t + \beta RP_{rt} + \gamma z_{rt} + \epsilon_{rt},$$

$$RP_{rt} = \alpha_r' +' \lambda_t' + \delta shiftshare_{rt} + \gamma' z_{rt} + \theta_{rt}$$

where r indexes districts, t indexes years, α_r and λ_t are fixed effects, $z_{rt}$is a vector of control variables, ϵ_{rt} is an idiosyncratic error term. RP_{rt} is a dummy indicating if the district had a Sharia regulation in year t. The exclusion restriction for this particular version of the shift-share instrument requires that the error term in the outcome equation is exogenous to instrument, or formally:[51]

$$E(shiftshare_{rt}\epsilon_{rt} \mid z_{rt}) = 0).$$

In plain English this means that we should not be able to predict later shocks to the outcome variables (ϵ_{rt}) given relative demand for Sharia before Sharia policies became available ($share_{r,0}$). I discuss the identification strategy more in Appendix B.

4.4.2 Results

I use this instrument to estimate the effect of the religious policy on 1) the district-aggregated government services index 2) the poverty indicators. I present the results in Table 11. I find that the impact on the district government services index is -9.4 percent of a standard deviation, while the poverty rates are higher by 4.9 percentage points on average. These are larger than their respective difference-in-differences estimates. The impact on the poverty gap index is 1.3 percentage points and the occurrence of violent events is also larger at 3 log points.

[51] See Goldsmith-Pinkham et al., 2018, page 12, "Case 1".

Table 11: Shift-share instruments

Panel A: all years, non-survey year data imputed

	(1) Government services	(2) Poverty rate	(3) Poverty gap	(4) Log(No. of incidents)
Has RP	-0.0946**	4.870***	1.307***	3.055***
	(0.0451)	(1.434)	(0.413)	(1.109)
Observations	3291	3982	3982	1253

Panel B: first stage and reduced forms (all years)

	(1) Has RP	(2) Government services	(3) Poverty rate	(4) Poverty gap	(5) Log(No. of incidents)
School shift share (i)	0.00482***	-0.000456**	0.0220***	0.00590***	0.0141***
	(0.000697)	(0.000203)	(0.00592)	(0.00171)	(0.00297)
Observations	3291	3291	3982	3982	1253

This table shows district instrumental variables estimations of outcomes on the religious policy dummy where the instrument is a shift share of Islamic schools in the district, defined as the share of Islamic schools in Indonesia in the particular district in 2000 multiplied by the country-wise growth rate in the total number of Islamic schools. Variables are either from PODES (population weighted average of the village-level government services index and the shift share instrument) or INDODAPOER (poverty rates and poverty gaps and all other controls). PODES data are linearly interpolated to years where PODES was not conducted. District fixed effects, income, GDP (lagged values of GDP/capita and its growth rate), calendar year dummies, election dummies, population (in logs) are included as controls. All standard errors are clustered on the district level. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

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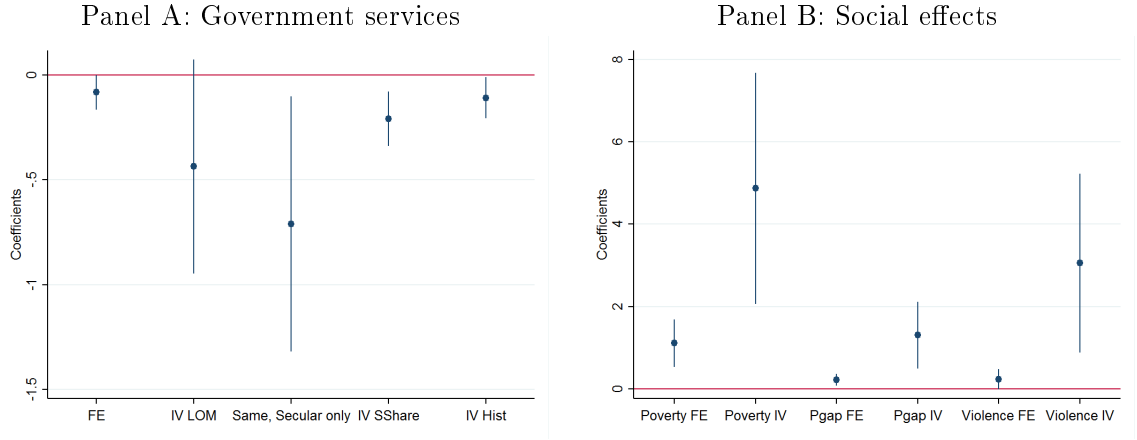
Figure 9: Comparison of estimates



Panel A compares the estimated β parameters of the impact of government services through different empirical specifications, which are the baseline village fixed effect regression (FE), the Leave-out-mean instrument (IV LOM), the same for secular villages only, the shift-share instrument with school shares (IV SShare) and the shift-share instrument with 1955 election data as baseline. Panel B compares the estimated β parameters between district outcome variables in the fixed effect and the shift share IV specification. The outcomes are the poverty rate, the poverty gap index, and the log number of violent incidents.

To compare specifications, Panel A of Figure 9 plots the coefficients for the estimated Sharia regulation impact on government services across the three specifications, while Panel B shows the coefficients comparing fixed effects and IV coefficients for the district outcomes only. I find that Sharia regulations have a negative impact on government services, they increase poverty and violence. These findings are coming from three different data sets and robust to three empirical strategies that rely on different assumptions.

4.4.3 Threats to identification

The validity of the shift-share instrument relies on the question whether initial levels of Sharia demand are independent of the error term in the equation of the outcome variables. I assess this question in detail in Appendix B.

4.5 Heterogeneity analysis

4.5.1 Short- and long-run effects

Now I analyze whether the impact of Sharia-regulations persists. In Table F.8 I present regional regression results where the outcomes are financial and social variables analyzed before, but this time there are two independent variables. A dummy indicates if the district has a religious policy. Another dummy indicates if the regulation was implemented in the current

electoral cycle. This helps differentiating between short-term and long-term effects of Sharia policies.

Panel A shows regressions where the dependent variables are government spending. We see that all the Sharia-induced reduction in spending and employment comes from the contemporaneous electoral cycle (second row). In subsequent years, spending on employment bounces back (first row).

Panel B shows the impact on poverty indicators, which shows the exact opposite: the impact of religious policies is somewhat moderate in the cycle when they were introduced but the negative long-term impact persists after that.

Both results are consistent with the mechanism proposed in the paper. District heads who introduce Sharia regulations may economize on spending, because they can rely on local Islamists to hold on to power. Such a bargain only last for a single electoral cycle, so the next district had has to resume spending or strike a bargain on his own. However, the money not spent is missing on the long run, and negative social effects outlive their causes.

4.5.2 Within-district heterogeneity of the effects

Finally, I am going to explore two potential dimensions of heterogeneity within the effect of religious policies on government output. It is important to note that as I have a single instrument on the village level, this exercise relies only on the geographical and time variation in the policy variable.

I am checking whether the policy effects are different in villages that are further away from the center, and whether they are different in villages that have an office of the district police.[52] Both variables are a proxy for local state capacity, and are also closely correlated (central villages are more likely to have such a police force), but they highlight somewhat different aspects. If state capacity is limited, the district center probably has difficulties projecting power to the peripheral areas of the district, and religious policies might not be enforced in further away villages. Also, district centers are more urban areas with a more vigorous economy, and as such, with potentially higher wages. If the public morals / public service substitution exists, and wages within the district are heterogeneous, a rational incumbent will start the substitution on public sector workers in the center, so reduction in government services should be more severe in central

[52]The actual institution I am looking at is the Pertahanan Sipil, or Civil Defense, which is a police body directly overseen by the district government. Other types of police forces exist as well.

villages. As both mechanisms go in the same direction, they are observationally equivalent, but I can check if the state capacity channel exists in its own right by looking at whether the policy effects are heterogeneous across villages that have a district police and those that do not.

In table F.11 I interact the religious policy dummy with the distance from the center in hundred kilometers (Column 1) and with a dummy indicating the presence of district police (Column 2). Both specifications show that the effect of the policy is mitigated in villages with lower state capacity. In particular, the effect of religious policies is strongest in the center (-.26 standard deviations), and is mitigated with increasing distance from the center. In particular, the effect becomes zero at 45km on average, which suggests that 80% of villages on average experience lower public good provision levels (mean distance: 30 km, median: 23.47km). The police interaction in Column 2 shows that villages with a district police on average have higher government services (by 0.02 standard deviations, not significant), but if their district adopts a religious regulation, the service provision drops by a strongly significant .19 standard deviations. Interestingly, and in line with the distance interactions, villages in these districts that do not have such a police fare relatively better (by 0.08 standard deviations, not significant).

I also present these evidence in form of event studies. In particular, now I only look at districts that receive a religious policy, and compare, in the first exercise, the quartile of villages closest to the center to the most distant quartile of villages, before and after the policy was introduced (figure F.5). In the second exercise I compare villages with district police to villages without district police before and after the policy was introduced (figure F.6). Both event studies show that there were no significant differences in the trends these villages followed before the policy, and that central villages and villages with police experienced a significant reduction in government services after the policy was introduced.

4.5.3 Regulation content

The second dimension of heterogeneity that I look at is the content of the regulations. I classify regulations into either the group of prohibitive regulations, or to the group of normative regulations.[53] First, I re-estimate Equation equation (2) in the following form:

$$y_{vw} = \beta_1 NORMATIVE_{vw} + \beta_2 PROHIBITIVE_{vw} + \gamma X_{vw} + \alpha_v + \lambda_w + \varepsilon_{vw} \qquad (5)$$

[53]For a deeper discussion on the regulations and the classification please see Appendix A.

Then I estimate the same equations with the policy variable interacted with distance. The results can be found in table F.11 and table F.13, respectively. Interestingly, without distance interactions included, one would think that it is entirely the normative regulations that drive the results. But once distance is controlled for, we see that prohibitive regulations are the ones from which the geographical pattern comes from: government services are hit hard at the center, but the effect is mitigated in more peripheral areas. This pattern is consistent with the hypothesis that two parallel channels of religious policies exist: one through public service substitution, and one through the direct effect of the policies themselves. In normative religious regulations probably the first element dominates, so their geographical pattern is weaker. Prohibitive regulations, on the other hand, need active enforcement, thus will be stronger where state capacity is stronger. Also, enforcing them needs public resources to be diverted from other purposes, which would produce the same pattern as we see in table F.13.

5 Welfare analysis

5.1 Setup

This section sets up the decision to introduce Sharia policies formally, to derive welfare implications. The model focuses on the policy decisions of the incumbent (as in Strömberg 2004), who is running for reelection against J challengers. The incumbent is maximizing expected rents from office,

$$V_1 = Pr(reelected) \times M - \omega N_E - s \cdot c$$

where M is the office value, which can be thought of as all the state funds the incumbent can freely dispose of. He has two policy tools at hand.[54] He can build a clientele, meaning that he hires N_E people into public employment at cost ω. He can also decide to implement a Sharia-based religious policy ($s = 1$ if he does so) at fixed cost c.

The probability of reelection is a function of N_E and s, so in the simplest case (when the incumbent is unconstrained in spending state funds), so he will spend on N_E until $\frac{\partial Pr(reelected)}{\partial N_E} = \frac{\omega}{M}$.[55] Public employment in this context serves as a token for any relationship between the elected official and the voter that is contractible to some extent.

[54] I use the male pronoun because in the Indonesian example the vast majority of districts heads are male.

[55] Relaxing this will result in lower reelection probabilities, and the incumbent will choose the corner solution for a larger part of the parameter space (see Appendix C), but does not change the essence of the analysis.

Challenger behavior is taken as exogenous, as if they had a fixed campaign budget $\bar{M}$ which they all spent on mobilizing voters at a per capita price of p. Their decision and their entry can be endogenized, but that does not change the main insights.[56] This is a static setup; politicians and voters only care about the here and now. Incumbent votes are determined by equation

$$Votes_1 = (1-s) \times \underbrace{(\Delta\pi_E N_E + \pi_{NE} N)}_{\text{Inc. votes without Sharia policy}} + s \times \underbrace{(N_R + \Delta\pi_E^S N_E + \pi_{NE}^S (N - N_R))}_{\text{Inc. votes with Sharia policy}} + e_1, \tag{6}$$

while challengers get votes according to

$$Votes_j = (1-s) \times \underbrace{(\Delta\pi_B N_B + \pi_{NB} N)}_{\text{Chall. votes without Sharia policy}} + s \times \underbrace{(\Delta\pi_B^S N_B + \pi_{NB}^S (N - N_R))}_{\text{Chall. votes with Sharia policy}} + e_j. \tag{7}$$

πs are individual voters' probabilities of voting on a given candidate. π_{NE} is the probability that a non-employed votes for the incumbent, while $\Delta\pi_E$ is the extra probability that she votes for the incumbent given she is employed. $\Delta\pi_E^S$ and π_{NE}^S are the same probabilities in the case that $s = 1$, that is, the incumbent introduces a religious policy. Similarly, π_{NB} is the probability that a non-mobilized voter votes for a challenger, while costly mobilization will convey an extra voting probability of $\Delta\pi_B$. N is the total number of the electorate, while N_R is the number of hardline voters, while e_j is an iid. turnout shock. Let's turn to the main assumptions on voting behavior.

The most important assumption is that if the religious policy is introduced ($s = 1$), the hardline voters will vote for the incumbent in any case, so the incumbent will have N_R votes at least. If $s = 0$, there is no difference between the behavior of the hardliners and the moderates.

Next, I assume that $\pi_{NE}^S < \pi_{NE}$ and also $0 < \Delta\pi_E^S < \Delta\pi_E$. The first condition says that the moderate non-employed voter will be less likely to vote for the incumbent if he introduces the religious policy, as it causes disutility directly to this type of voter. The second condition says that people in public employment are more likely to vote for the incumbent, but less so, if the divisive policy is introduced.

Finally, the candidates take voting $\pi_{B,E,NB,NE}$ probabilities as given and independent from each other. An approximate micro-foundation can be provided to this behavior by assuming that voter j gets utility $\beta B + \delta E - s\varsigma + \nu_{ij}$ from candidate i, where B is an indicator of having received a bribe, E is an indicator of being in public employment and s is an indicator if the candidate introduced a the divisive policy, and v_{ij} is an iid. popularity shock. The approximation is in that I assume that there are enough voters so that candidates do not cross-mobilize and employ

[56] As if they, too, maximized $Pr(elected) \times M$, and entered the race until that value was over a certain fixed threshold.

each other's votes, and that candidates consider everyone that they did not mobilize or employ voting for them with the same probability, regardless of them being mobilized or employed by another candidate. This can be thought of as the candidates having a heuristic about their general popularity within the electorate, or that they are approaching different pools of voters altogether, such as their ethnic group or people in their geographical proximity.

If I assume e_j to come from a Type-I extreme value distribution, then the probability of winning for each candidate will conveniently be of a logit form (McFadden et al., 1973). Thus, the election probability for the incumbent will be, without introducing the religious policy:

$$P = \frac{\Delta\pi_E N_E + \pi_{NE} N}{(\Delta\pi_E N_E + \pi_{NE} N) + Jexp(\Delta\pi_B \frac{\bar{M}}{p} + \pi_{NB}(N - s \cdot N_R))}$$

And with introducing it:

$$P = \frac{N_R + \Delta\pi_E^S N_E + \pi_{NE}^S (N - N_R)}{N_R + \Delta\pi_E^S N_E + \pi_{NE}^S (N - N_R) + Jexp(\Delta\pi_B \frac{\bar{M}}{p} + \pi_{NB}(N - s \cdot N_R))}$$

Now the incumbent's problem can be solved. He will introduce the Sharia policy if it gives him higher expected payoff. To see if this is the case, we need to solve for the optimal public employment N_E^* with and without the policy, and check which gives higher expected utility.

5.2 Results

Proposition 1: the decision to introduce Sharia policies.

The incumbent introduces the religious policy iff $N_R > \underline{N_R}$, where $\underline{N_R}$ is a threshold that is a function of the model parameters only. Proof: see Appendix C.

Proposition 2: the substitution between public morals and public services.

If the incumbent decides to introduce the religious policy, he will set lower public employment levels. Proof: see Appendix C. The intuition is that buying votes is costly, and he has an incentive to keep costs down. So if he can deliver some votes at a fixed cost, he will use some of that advantage to keep his variable costs at bay, that is, the size of his clientele.

Proposition 3: comparative statics.

$\frac{\partial N_R}{\partial M} > 0$; $\frac{\partial N_R}{\partial \omega} < 0$. Proof: see Appendix C.

The threshold number of the hardliners over which the incumbent introduces the religious regulation is increasing in office value. That is, the policy will be less likely to occur in districts which have high office value. The intuition for this is the following. The problem of the incumbent is such that he wants to set his re-election probability to a fixed level with public employment. This target probability is increasing in M: the more valuable the office is, the more confident he wants to get about winning the election. Thus he is going to need more votes. On the other hand, religious policies render all votes more costly after the first N_R. So the incumbent would need more votes under higher M, and he would also be doing a worse job at getting them with the religious policy.

On the other hand, the threshold is decreasing in employment cost. That is, if wages are high, the incumbent will be more likely to introduce religious policies. The intuition is that ideology and clientelism are substitute technologies, and ω is the price of clientelism. Thus the incumbent substitutes away from clientelism if its prices are high.

5.3 Welfare

I define total material welfare of electorate as:

$$\sum W_i = N_E\delta + N_B\beta - (N - N_R)\,\varsigma_1 + N_R\varsigma_2$$

Where δ is the individual utility of employment; β is utility from bribe; ς_1 is the utility loss of the individual moderate voter due to the religious policy, ς_2 is the utility gain of the individual hardline voter. Total effect of divisive policies on average welfare then:

$$\bar{W}_S - \bar{W} = \underbrace{\delta\left(\frac{N_E^S}{N} - \frac{N_E}{N}\right)}_{\text{Substitution effect}} \underbrace{-\varsigma_1\left(\frac{N - N_R}{N}\right) + \varsigma_2\frac{N_R}{N}}_{\text{Direct effect}} \tag{8}$$

The first part I call the substitution effect: this is due to the fact that the incumbent is

trading off public services for supplying divisive politics. The second part is the direct effect, which is the utility impact realized through adding up all voter taste and distaste for the issue in question. This would be the utility loss due to having to close a store, having to undergo extra education, or suffering from vigilantism. While the empirical section mostly covered the estimation of the substitution effect, the final structural exercise aims at uncovering what the magnitude of the direct effect might be relative to the substitution effect.

The question whether Sharia regulations increase or decrease total welfare is open ex ante, and I do not aim for a definitive ex post answer either.[57] As I discussed in Section 3, there are many different types of regulations and they often overlap, so disentangling the direct effects of each policy separately is not feasible. Instead I ask the following question: how big an impact of Sharia policies would explain observed voter behavior?

The parameter ς_1 is identified from the relationship between vote counts and public employment with and without Sharia regulations. For ς_2, as I only observe a proxy for the number of hardline voters, I can only identify a lower bound: if the parameter is above (below) that threshold, then the Sharia regulations are welfare increasing (decreasing) on the aggregate.

Two preconditions have to be met to carry out this exercise. First, we need to check whether what the model assumes on voter behavior is by and large plausible. Second, the relevant parts of the model have to be stated in a form that is suitable for empirical analysis.

5.4 Evidence on model assumptions

Estimating the taste parameter crucially hinges upon the question whether the main assumptions of the model and the chosen proxies for implicit employment and hardline community size are plausible. These assumptions and predictions are the following:

- **Assumption 1**: public employees are more likely to vote for the incumbent, and thus more public employees mean higher reelection probability.
- **Prediction 1**: greater public employment levels (as proxied by implicit employment) are associated with more votes for the incumbent.
- **Assumption 2**: public employees are less likely to vote for the incumbent, if he introduced

[57] With this approach I acknowledge the results of Campante and Yanagizawa-Drott (2015) who find that negative effects of religious institutions on economic performance might be offset by positive impact on subjective wellbeing.

religious policies.

- **Prediction 2**: the above empirical correspondence is weaker in elections which are preceded by introduction of a religious policy.
- **Assumption 3**: the devout voters vote for the incumbent if he introduces religious policies.
- **Prediction 3**: if the incumbent introduced a religious policy, he will get additional votes in proportion to the number of Islamic schools of the district.

I use two data sets to investigate the validity of these assumptions. The two data sets on voting behavior have advantages that complement each other's drawbacks well, so together they can be used to paint a more complete picture on the plausibility of the model assumptions and their corresponding empirical predictions.

In the first exercise I am using the universe of district election events from 2005 to 2013 where the number of votes was available, so the variation is rich in space and time.. The merit of this approach is that I can identify how policies, candidate characteristics and district variables are correlated with eventual political outcomes. The drawback of this strategy is that I do not observe individual decisions of voters, just aggregate differences in turnout and number of votes.

In the second exercise I use individual (voter level) data from the fifth wave of the Indonesian Family Life Survey. The merit of this approach is that I can identify how religiosity and being in public employment is correlated with the decision to vote. On the other hand, the data are cross sectional (from 2014), and does not cover the whole country.[58] Also, the respondents are asked if they voted in the past elections, but not asked who they voted for.

Checking the predictions on election data

I observe vote counts for over 70% of the 822 election events that happened between 2005 and 2013. Most missing data are from early years. In the year of the first direct election, the missing rate is 60%, which is dropping on average by 10 percentage points every year to stabilize at 90%

[58] The previous wave of the survey is from 2007, when a significant portion of the districts was still not exposed to direct district elections. However, the data are representative for the whole country on a provincial level upwards. About 50% of the districts in the IFLS areas had religious policies in 2013, which means that these areas are somewhat overrepresented.

in 2010. I link every election event to district data from the past election cycle. I estimate the following linear model:

$$votes_c = \beta_0 + \beta_1 RP_c + \beta_2 N_c + \beta_3 IPE_c + \beta_4 IPE_c \times RP_c + \beta_5 N_c \times RP_c + \beta_6 inc_c + \varepsilon_c$$

Where *votes* is the number votes the incumbent receives, RP is a dummy indicating if a religious policy was introduced in the cycle preceding the election, N is the size of the electorate in the election year, IPE is average implicit employment in the non-election years of the previous cycle (as in election years one cannot differentiate which part can be attributed to the incoming district head), *inc* is a vector of incumbent-specific controls and c indexes district-election cycle observations.

I expect β_3 to be positive, which amounts to meeting Prediction 1 discussed above. I expect β_4 to be negative, as suggested by Prediction 2. This would mean that a bigger public sector employment means more votes, but voters who are attracted by public sector votes are somewhat disillusioned with the incumbent if the engages in extremist policies.

Prediction 3 is harder to check in this specification. First, the statistical model does not differentiate between religious and secular among the non-employed. As a consequence β_5, the coefficient on $N \times RP$, will represent the net effect of the secular being less likely to vote for the incumbent under Sharia policies (π^S_{NE}) and the increase in the voting determination of the devout. The total effect will be a weighted average of these, where the weights depend on the N_R/N ratio. It is, however, plausibly positive, as the incumbent would otherwise lose votes one the net. Including the variable for Islamic schools in the regression does not necessarily mitigate the problem. First, the two variables will be highly correlated, as the total electorate is the sum of the devout and the secular, and the number of schools is an unknown function of the number of the devout. As a consequence, serious multicollinearity should be expected. Second, the relationship between N_R and the school variable, as above discussed, is very complicated, further increasing noise in the estimates. Nevertheless, I estimate versions of the above equation where I include the number of the schools among the regressors in themselves and interacted with the religious policy variable. I estimate two versions, one with levels and one with natural logarithms of the school variable. I also include versions of all specifications with and without incumbent observables, which are limited to information deduced from the name of the politician. These are a dummy for having any honorific title, and two other dummies, one representing the

most important religious subset of titles (the title of *haji* or *hajjah*, which indicates a person who completed the pilgrimage to Mecca), the other the most important secular subset (having a doctorate of any sort).[59]

Results

Table D.2 reports the estimation results. The coefficients β_3 and β_4 have the expected sings and are strongly significant across specifications. According to the baseline specification, each public worker means for the incumbent $\beta_2 + \beta_3$ votes in expectation, which is .34 vote. Each person in the electorate who is not employed represents β_2 incumbent votes in expectation, which is estimated at .18. If the incumbent decides to implement the religious policy, then the expected votes after public employees is $\beta_2+\beta_3+\beta_4+\beta_5 = .17$, while the expected votes for each non-employed is .28. Note that the differential becomes negative, so in this case a larger public sector means less votes on the net. However, this is misleading for the reason stated above: the electorate size here confounds the secular non-employed and the religious non-employed, thus overestimates the baseline incumbent votes.

Another consequence of this is that introducing religious schools and their interactions with the religious policy does not yield significant result, but they both have a positive sign, as expected. Titles of the politicians are associated with higher vote counts.

Checking the predictions on individual survey data

Next I use individual level data from the fifth wave of the Indonesian Family Life Survey to check if voters behave individually in a way that is consistent with the model. That is, I check if workers in government jobs are more likely to vote, if religious policies have any impact on this behavior, and if devout voters are more likely to vote if a religious policy was introduced. I define the sample as all Muslim respondents who were eligible to vote in the last election and in 2007 (the year of the previous wave) lived in a district that had no religious policy. The sample defined this way consists of 8958 individuals.

I estimate the following linear probability model:

$$voted_i = \beta_0 + \beta_1 gworker_i + \beta_2 devout_i + \beta_4 RP_r + \beta_4 gworker_i \times RP_r + \beta_5 devout_i \times RP_r + \boldsymbol{\delta} X_i + \varepsilon_i$$

[59] Other titles include Bachelor's and Master's degrees, professional titles (such as "engineer"), *kiyai* (religious expert on Islam) etc.

where *voted* is a dummy indicating if the person says he voted during the last election, *gworker* is a dummy indicating if the person's primary or secondary job was in public employment, *devout* is a dummy for strong religiosity, RP indicates the presence of a religious policy where he or she lives and X_i is a vector of individual level controls. These include controls for age, sex, marital status, years of schooling, living in an urban environment, religious tradition dummies,[60] and dummies indicating if the person moved recently, voted in 2007, was a government worker in 2007 and whether lived in urban environment in 2007.

The *devout* dummy takes the value of 1 if the person reports that either all the institutions he or she attended were operated by religious organizations, or his or her highest level of education is from a religious school.[61] I chose this measure as opposed to self-reported measures of religiosity due to both theoretical and practical reasons. First, being very religious is neither a necessary nor a sufficient condition to support religious policies. Second, state ideology in Indonesia embraces religious diversity, but is rather suspicious towards atheism, which seems to be true for Indonesians in general, so not many people would admit in a survey that they are not particularly religious.[62] Third, it is straightforward to use the same proxy as in the instrumental variables regressions, and the welfare calculations.

Results

Column 1 in Table D.3 reports the results from an OLS regression. On average, government workers are 8% percent more likely to report to have voted in the preceding district head election. The coefficient on the interaction term with religious policies is -0.064, meaning that government workers are less likely to vote if there are religious policies in place. These are in line with Predictions 1 and 2.

Voting behavior of the devout is not significantly different in absence of religious policies. But if there is any, they are 7% more likely to vote, in line with Prediction 3.

[60] The reference group is Nahdlatul Ulama, a traditionalist Islamic religious movement, which is the largest of its kind in Indonesia, and as such the largest independent Islamic movement in the world with 94 million members. Two-thirds of the respondents in the IFLS felt closest to NU, while about 12% felt closest to Muhammadiyah, the second largest Islamic movement. The rest of respondents are uncommitted. The analyzed subsample reflects these shares.

[61] That is, if the person went through education that provided regular degrees but the institutions themselves had Islamic background, or if the studied in an institution dedicated for Islamic studies, such as a *pesantren* or a *madrasah*. In an unreportred robustness check I estimated the same model with both categories separately, and the results were qualitatively the same.

[62] Belief in the higher power is the first of the five principles of *pancasila* state ideology. Only 2% of the IFLS respondents identify as "not religious"

Also, introducing the religious policies has no significant effect in itself except through this channel ($\hat{\beta}_4$ negative and insignificant).

Importantly we cannot be sure that public sector employees are voting for their employers, and that the devout vote for that person who introduced a policy they probably favor, but it is very plausible. What I can do is to show that government employment only matters in turnout when the name on the ballot is the name of their boss, and that religiosity only matters for turnout when the election is about the leader who introduced the religious policies. To do this, I conduct two placebo tests where the dependent variables are participation in the elections for People's Representative Council (the national legislature), and participation in village head elections. In the former case the voters vote for party lists. The typical district head is a candidate of more than one parties at once (the distribution of the number of nominating parties is symmetric around 2), and anecdotal evidence suggests that the nomination in many cases is bought (Buehler, 2007), so it is not evident whether the incumbent district head (and by extension, their clients) owes any loyalty during these elections. Village head elections, on the other hand, should reflect preferences over very different issues, and probably driven more by personal experience.

No coefficient of interest is statistically different from zero in the placebo tests (Columns 2 and 3), suggesting that the coefficients in the district head election case do reflect the voters' loyalty for their employers (in the case of government employees), and their preference for religious policies (in the case of the devout voters).[63]

Having shown that the data do not contradict the model assumptions and the corresponding empirical predictions, I now connect the relevant aspects of the theory in Section 2 to the data.

5.5 Setting up the welfare calculation

To do this, one should observe Equation 6 in the model, the formula for incumbent votes from Section 2. Importantly, all variables in the equation are either observed in our data (such as incumbent votes, presence of religious policy and electorate size), or can be proxied by an observed variable (as is the case for public employment N_E and religious community size N_R). Parameters of the model then are functions of the δ and $\varsigma_{1,2}$ welfare weights, which we can recover from parameter estimates. Knowing the welfare weights then will give as a voter behavior based

[63] What this test does not rule out is that the government employee turns out to vote, but only to vote *against* his employer, and that the devout voter goes to the ballot box and votes *against* the incumbent who introduced the religious policy, but seems to be unlikely.

estimate of the subjective impact of Sharia policies.

That being said, by proxying N_E with implicit employment and by proxying N_R with the number of Islamic schools we can re-formulate the Equation 6 as follows:

$$Votes_1 = (1-s)\times\underbrace{\left(\Delta\pi_E\alpha_1\frac{wagebill_{it}}{minimumwage_{it}}+\pi_{NE}N\right)}_{\text{Inc. votes without Sharia}}+s\times\underbrace{(\alpha_2 log(schools_{it})+\Delta\pi_E^S\alpha_1\frac{wagebill_{it}}{minimumwage_{it}}+\pi_{NE}^S(N-\alpha_2 log(schools_{it})))}_{\text{Inc. votes with Sharia}}+e_1 \quad (9)$$

The implicit employment (wage bill over the minimum wage) and the number of Islamic schools are proxies which only identify their unobserved counterparts up to scale parameters α_1 and α_2 . This means that we do not know how many voters are represented by a single Islamic school, and thus we do not know what is the probability that an individual devout voter will vote for the incumbent who introduced the religious policy, we only know that a unit increase in the log number of Islamic schools is associated with α_2 increase in votes.

We also do not know how many people are there in the patronage network of the incumbent through public employment. But we do know that a unit increase in implicit employment will be associated with $\Delta\pi_E\alpha_1$ extra votes in the absence of the religious policy, and $\Delta\pi_E^S\alpha_1$ increase in the presence of the religious policy. However, given implicit employment, actual clientele size can vary through at least three different margins. First, there is a wage margin, as wages are not observed, so we do not know how many people are actually employed. Second, there is a loyalty margin as we do not know how likely is that a person who is in the clientele will vote for the incumbent. Third, there is an outreach margin, as a client might very well turn out more than one people to vote. As a consequence we do not directly measure the rate at which a unit increase in the clientele increases votes for the incumbent ($\Delta\pi_E$ and $\Delta\pi_E^S$).

What will pin them down eventually is a functional form assumption on individual voting probabilities based on the model micro-foundations (see Appendix C). Without getting into details here, in the simplest case (where candidate observable characteristics don't come into play) the probabilities will be $\pi_E = \frac{exp(\delta)}{exp(\delta)+J+1}$ in the absence of the religious policy, and $\pi_E^S = \frac{exp(\delta-\varsigma_1)}{exp(\delta-\varsigma_1)+J+1}$ in the presence of one, where J are the number of competitors, δ and ς_1 are the individual utility from employment and secular disutility from Sharia, and in both cases $\pi^{(S)}{}_E = \Delta\pi_E^{(S)} + \pi_{NE}^{(S)}$.

We cannot recover this way the individual utility of the devout from a Sharia regulation (ς_2), as the model assumed that a religious individual would vote for the incumbent with probability 1 if the incumbent introduces the religious policy. The other consequence of this assumption

that it pins down th number of devout by establishing that $\hat{N_R} = \hat{\alpha_2} log(schools_{it})$.

Combining these insights with Equation 9 now identifies parameters $\alpha_{1,2}, \varsigma_1$ and δ non linearly, while the estimated $\hat{\alpha_1}$ and $\hat{\alpha_2}$ parameters can be used to recover an estimate for N_E and N_R through $\hat{N_E} = \hat{\alpha_1} \frac{wagebill_{it}}{minimumwage_{it}}$ and $\hat{N_R} = \hat{\alpha_2} log(schools_{it})$.

5.6 Welfare calculations

I now estimate the parameters from Equation 9 using non-linear least squares. $\hat{\delta} = .61$ (bootstrapped standard error: .05), $\hat{\varsigma} = .27$ (bootstrapped standard error: .30). The median estimate for the share of the secular is $\frac{(N-N_R)}{N} = 90\%$, while a conservative estimate for the substitution effect $\frac{(N_E - N_E^S)}{N}$ would be 10% from, for example Table 3). If that is the case, then substituting into Equation 8 the unobserved direct effect will make up abot 80% of the impact on the secular .

In order for the total effect to be positive, the ς_2 coefficient has to be such that it satisfies

$$\underbrace{\delta\left(\frac{N_E^S}{N} - \frac{N_E}{N}\right)}_{\text{Substitution effect}} - \underbrace{\varsigma_1\left(\frac{N-N_R}{N}\right) + \varsigma_2\left(\frac{N_R}{N}\right)}_{\text{Direct effect}} > 0$$

Rearranging this yields that the average ς_2/ς_1 at which this inequality holds 12.7 (median: 9.5). The corresponding average value for ς_2/δ is 5.66 (median: 4.23) This means that the subjective wellbeing caused by the Sharia regulation must be roughly ten times as much as the disutility on the secular in absolute terms; and it has to be roughly 5 times as much as the subjective utility from having a public sector job. These suggest that it is unlikely that the regulations would be welfare increasing on the aggregate.

6 Conclusion

In this paper I proposed a mechanism which provided a possible explanation why politicians supply divisive, ideological policies. This mechanism suggests that these policies are usually cheaper alternatives to supplying public goods and services, so politicians have an economic incentive to supply ideology instead.

I investigated in detail whether politicians who were running on ideological platforms provided less for their constituents in Indonesia. I used three empirical designs and three independent data sets to deliver compelling evidence of the existence of the "public morals / public services tradeoff". The results suggest that districts that implement Sharia-based regulations have about

8-10% worse public services and expenditure on public employment in a difference-in-differences setting. I also found evidence that the policies have lasting impact on poverty, and on the number of violent incidents. Alternative identification strategies using instrumental variables yielded results that were similar both qualitatively and quantitatively. A model based structural estimation suggested that the total effect on welfare can be five time as high as that caused by the cutback in government services.

An important feature of the studied mechanism is that it is more likely to be at work in districts which are relatively more developed. This effect might be mitigated if development eventually strengthens institutions, making programmatical politics and issues about redistribution more salient, but this question is out of the scope of the current paper. Another interesting question which arises from studying this mechanism whether it means that identity-based divisive policies should become more likely in a country which is moving along its development path.

Importantly, this paper and the stylized model concentrated on a developing country setting where due to weak institutions the politician could only choose between clientele building and ideological policies. An important avenue for future research would be to extend this analysis to a setting with stronger institutions, where the politicians are able to offer programmatical politics as well. It remains to be seen if a substitution between ideology and public services emerges in such a setting, and whether it is empirically relevant in developed countries.

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Chapter II

Surnames and Social Mobility in Hungary over Two Centuries and Five Regimes

with Pawel Bukowski, Gregory Clark and Rita Pető

1 Introduction

Rising social disparities have renewed interest in studying social mobility, especially since the Great Recession (see, for example Aaronson and Mazumder, 2008; Lee and Solon, 2009; Corak, 2013; Chetty et al., 2014; Olivetti and Paserman, 2015; Solon, 2015; Davis and Mazumder, 2017). Because of the data intensiveness of estimation of social mobility, most of these papers study recent generations in established democracies. Using unique historical data from Hungary, we instead look at the impact of political regime changes on social mobility at the top and at the bottom of the social status distribution over the course of centuries.

By estimating rates of status persistence in Hungary through five different political regimes (feudal and constitutional monarchy, right-wing authoritarianism, communism and liberal democracy), we show that institutions are fundamental in determining persistence of social status, and not always in the most intuitive way. Though we find that group level social mobility is generally low, periods of constitutional monarchy and democracy were more efficient in reducing the status advantage of elites. Communism, though explicitly aimed at displacing established elites, had limited success doing so by our measures. Communism also did not produce upward mobility from the bottom of the status distribution, and disadvantaged groups' relative position even worsened after transition in 1989. The repressive right-wing Horthy regime, on the other hand, was consistent in its effectiveness in reducing (in some cases: reverting) upward social mobility for everyone outside traditional elites.

There are two main strands in the literature on social mobility. The first makes use of surveys and administrative datasets and compares the outcomes of parents and children. However, these data have been collected only for the last two or three generations. The other method, outlined in Clark (2014) and Clark et al. (2015b), uses surname distributions in the population to infer trends in intergenerational social mobility. Surname distributions are much easier to find across

countries and over multiple centuries. One drawback is that the method only enables us to study mobility on a group level for groups whose ancestry can be traced by their family names. The other is that it only allows us to trace some aspects of social mobility, that is, the probability differentials across groups to achieve a high status outcome (such as earning a degree from Oxbridge), or a low one (getting into prison).

Individual level estimates usually show around 20% to 30% correlation across generations (see e.g. Olivetti and Paserman, 2015), while group level estimates are in the 70-80% range (see e.g. Clark 2014), so the latter shows much more persistence in status transmission. This is not necessarily a contradiction. For instance, former British Prime Minister David Cameron probably makes less money than his stockbroker father did, yet one might expect that the Cameron family will remain closely related to the British elite, as it has been for the past few centuries (example taken from Major and Machin, 2018).

As we look at a very long time horizon, we take the group-based mobility approach. We look at three different "elite occupations" of Hungary as a measurement of realized social status. These are high school students, medical doctors and army officers whom we identify by surname; in some cases we identify each person in the relevant cohort, in others a sizeable random sample only. Medical doctors offer the longest time horizon, from the establishment of the first medical faculty in Hungary in 1770 up to 2016. We track over time two social groups: one is the group of high status ("privileged") surnames, whose family name ends in the letter "-y" and is closely associated with high status and background in the traditional nobility. The second is low status ("underclass") surnames, defined as ones associated with the Roma minority, and names that were traditionally underrepresented in secondary education. We track the representation of these groups in the "elite occupations" relative to their share in the population and use these figures to infer their group level latent average social status. We then estimate how quickly they regress to, or diverge from, the status of the whole population during the 19th and the 20th century in particular, which saw the highest number of regime changes in Hungarian history.

The main contribution of the paper is that we use the same methodology and the same data sources to provide estimates of social status persistence under very different institutional settings. This provides an opportunity to compare the pace of social mobility within a variety of political and economic regimes. Also, this is the first time the group mobility approach of Clark et al. (2015b) is applied to a former Eastern Bloc country, and we are unaware of any study of social mobility in Hungary of comparable timeframe. We carry out the analysis in a way that allows

for comparisons at the top and at the bottom of the status distribution, and to a limited extent, the social status of women as well.

The literature on the relationship between political institutions and social mobility is scarce, and mostly concerns how social mobility shapes political preferences (Leventoğlu, 2005, 2014; Acemoglu et al., 2017); we know less about how regime changes triggered by external events shape social mobility (such as a Habsburg loss at the Austro-Prussian war triggering constitutional reforms in Hungary; territorial loss at World War I triggering right-wing turn in 1919-20; or Soviet occupation in 1945 and the fall of the Soviet Empire in 1989).

2 Historical background

Since the late 18th century Hungary has experienced many political and economic regime changes. Our analysis starts when Hungary was one of the kingdoms and principalities that constituted the Habsburg Empire.[64] The growing dissatisfaction with the Austrian rule culminated in the revolution of 1848. Though failed eventually, this event paved the way for the constitutional reform of the Compromise of 1867 . The following period brought tranquility and relative prosperity, as Hungary became a coequal constituent of the "Dual Monarchy" of Austria-Hungary .[65]

During World War I, Hungary aligned with the Central Powers, along with Austria, Bulgaria, Germany and the Ottoman Empire.[66] Losing the the war resulted in the collapse of the monarchy, which was first replaced by the short-lived First Republic in 1918, and then by the even shorter lived (133 days) Hungarian Soviet Republic. But the biggest shock came in 1920 when the country lost 2/3 of its territory, mandated by the Treaty of Trianon.[67] The pendulum swung back to the opposite end of the political spectrum and Hungary became a monarchy again (though the throne remained vacant), this time under the leadership of Admiral Miklós Horthy as regent. The country increasingly shifted to the right under its subsequent governments, gradually embraced anti-Semitism and got into the German sphere of influence again. Hungary allied again with Germany in World War II, was occupied by Germany eventually in 1944 nevertheless, and actively participated in the Holocaust sending hundreds of thousands of its citizens to German

[64] Taylor (1976) gives an overview on the history of the Empire with a special focus on the relations between Austria and Hungary.

[65] Kontler (1999) is perhaps the most comprehensive English language overview of the period.

[66] Romsics (1999) gives an excellent account of Hungarian history in the 20th century.

[67] Vardy (1997) gives a detailed description of the severeness of the trauma caused by the Treaty and how it affected the mindset of the whole era that followed.

concentration camps.[68]

As a consequence of the even more devastating loss in World War II, the country was occupied by the Red Army. The Second Republic was proclaimed in 1946, which immediately came under Soviet influence, and very soon under direct communist control, becoming part of the Eastern Bloc of nations as the People's Republic of Hungary.[69] The subsequent oppressive and undemocratic communist governments nationalized much of the economy, engaged in massive forced industrialization, extended public services and lifted many out of poverty, but eventually brought in a long period of stagnation before the fall of communism in the Eastern Bloc in 1989. Proclaimed in that year, the Republic of Hungary became a democracy with a market-based economy largely in private hands.[70]

The consequence of this chain of events is that Hungary had no less than 7 regime changes over two hundred years, but we only focus on the longest and most important ones. In particular, we compare: (1) monarchy up to the Compromise of 1867; (2) constitutional monarchy until World War I (1867-1914); (3) conservative authoritarianism in the interwar years (1920-1939); (4) Communism[71] (1946-1988); and (5) liberal democracy (1989-2016).

3 Methodology and data

3.1 Estimation of social mobility

We use the method detailed in Clark (2014), Clark and Cummins (2014b) and Clark et al. (2015b) to measure the rate of intergenerational social mobility on a group level. A group is defined as the holders of a set of surnames whose paternal ancestors shared similar social status at one point in time in the past.

The goal of the method is to estimate the serial correlation of latent social status over time. Suppose that the realized social status of family i in period t of group g can be described as a sum of a latent, "inherited" component x_t^g and error term ϵ_{it} :

$$y_{it}^g = x_t^g + \epsilon_{it}$$

[68] See the fundamental work of Braham (2000) on the Holocaust in Hungary.

[69] Kenez (2006) and Zhelitski (2018) give accounts on the communist takeover in Hungary.

[70] See Pridham and Vanhanen (2002) for a historical overview on democratization, Stark (1990) for the history of privatization of the economy, and Brown et al. (2006) for an analysis of its economic impact.

[71] With that we treat together the years of indirect communist control during the Second Republic with the decades of the genuine single party rule.

We want to infer the mean social status x_t^g from observing the percentage of the members of the group who make it to the elite occupations. Two assumptions are required. First, that ϵ_{it} is distributed normally with constant variance which is the same for the group x_t^g and the reference population.[72] Second, that the elite occupations that we observe represent some top share of the status distribution, so a current member of the family will make it to the elite occupation if her realized social status is above some threshold

$$y_{it}^g > \underline{y}.$$

The probability that a current member from group g is in the elite occupation, given ϵ is normal, and Φ is the cumulative distribution function of the standard normal distribution function:

$$P_{elite}^g = P(x_t^g + \epsilon_{it} > \underline{y}) = 1 - P(\epsilon_{it} < \underline{y} - x_t^g) = 1 - \Phi\left(\frac{\underline{y} - x_t^g}{\sigma_\epsilon}\right)$$

We can also express the probability P_{elite}^g as the share of people from group g who made it into elite occupations:

$$P_{elite}^g = \frac{\#g \text{ in elite}}{\#g}.$$

Combing the two we get the following expression:

$$\frac{\underline{y} - x_t^g}{\sigma_\epsilon} = \Phi^{-1}\left(1 - \frac{\#g \text{ in elite}}{\#g}\right) \tag{10}$$

The next step is to choose a reference group, to which group g is compared over time. This can be the total population, or a subset of it. By analogy, Equation 10 will also hold for the reference group:

$$\frac{\underline{y} - x_t^{ref}}{\sigma_\epsilon} = \Phi^{-1}\left(1 - \frac{\#ref \text{ in elite}}{\#ref}\right) \tag{11}$$

Figure 10 illustrates the method. It shows that if g is a high status social group, then its status distribution is shifted to the right vis-à-vis the reference group, so a higher percentage of g is found in the elite occupation. Knowing the share of that group's members who are in the

[72] Clark et al. (2015b) argue for the plausibility of these assumptions.

Figure 10: Status distribution of g and the reference group

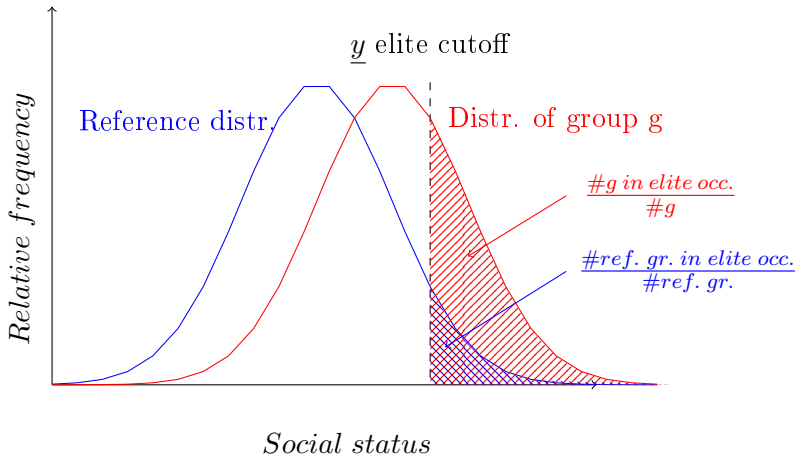
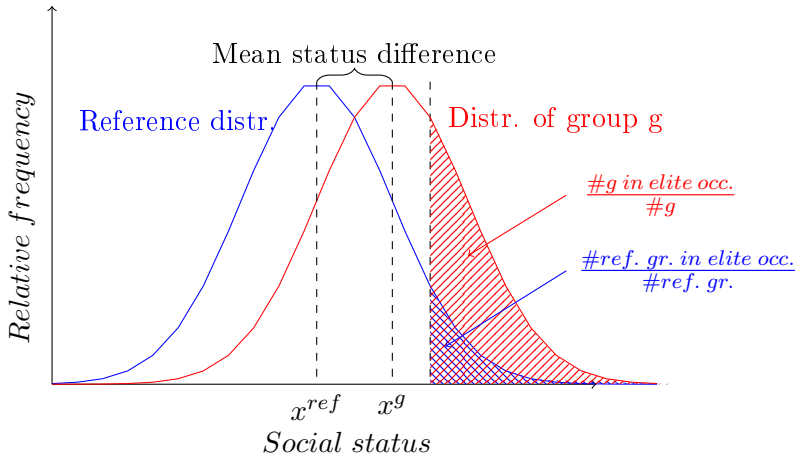


Figure 11: Inverting the status distribution to infer group means



elite occupations, we can infer where the mean of the distribution of status of that group is at, as seen in Figure 11.

As we assumed that the variance is the same for both groups, we can combine Equations 10 and 11 to express the status advantage of group g compared to the reference population:

$$\frac{x_t^g - x_t^{ref}}{\sigma_\epsilon} = \Phi^{-1}\left(1 - \frac{\#ref\ in\ elite}{\#ref}\right) - \Phi^{-1}\left(1 - \frac{\#g\ in\ elite}{\#g}\right) \tag{12}$$

Suppose that latent status is transmitted over time at the rate β_g, then the latent status of group g at time t will be

$$x_t^g = \beta x_{t-1}^g = \beta^t x_0^g.$$

Substituting this expression back to 12 yields $\left(\beta^t x_0^g - x_t^{ref}\right)/\sigma_\epsilon$. If the reference group is chosen well (i.e., its mean is close to zero), then $\left(x_t^g - x_t^{ref}\right)/\sigma_\epsilon \simeq \left(x_t^g\right)/\sigma_\epsilon$ and

$$ln\left(F_\epsilon^{-1}\left(\frac{\#ref\ in\ elite}{\#ref}\right) - F_\epsilon^{-1}\left(\frac{\#g\ in\ elite}{\#g}\right)\right) \simeq -ln\sigma_\epsilon + ln(x_0^g) + ln(\beta)\cdot t + \mu_t. \quad (13)$$

Equation 13 can be used to estimate $\widehat{ln(\beta)}$ by regressing the natural logarithm of the difference of the inverted population shares on a constant and time trend.

The estimated $\hat{\beta}$ is the per-period serial correlation of the latent social status. If $\hat{\beta} < 1$, the group average regresses to the social mean as $\lim_{t\to\infty} \beta^t x_0^g = 0$. Previous literature considers a single time period as either 30 years or a decade. In this paper, we calculate yearly $\hat{\beta}$ coefficients and calculate $\hat{\beta}^{10}$ and $\hat{\beta}^{30}$ for the sake of comparison with previous results.[73]

Our estimation procedure follows two steps. First, we estimate the share of group g in the general population using census data. The shares are relatively stable over time, so we interpolate the gaps between observations. Similarly, we calculate the share of group g in the elite occupation.[74] and use these two shares to calculate the relative representation of group g in the elite occupation:

$$RR_g = \frac{\#g\ in\ elite}{\#elite} / \frac{\#g}{\#pop}. \quad (14)$$

The relative representation is informative in itself, as it shows how over-represented (or under-represented) is group g in the elite occupation compared to its population share. But we can obtain the share of g that is part of the elite occupation, by multiplying RR_g by the total size of the elite occupation:

$$RR_g \frac{\#elite}{\#pop} = \frac{\#g\ in\ elite}{\#g},$$

In the second step, we plug this share into the inverse standard normal distribution function in order to get implied mean social status x_t^g. We use Equation 13 and regress log of the implied social status on a constant and time trend to get $\widehat{ln(\beta)}$.

[73] Clark and Cummins (2014b) and Clark et al. (2015b)

[74] In both steps a random sample suffices as long as group share of g is close to its population share

3.2 The surname distribution over time

Estimating social mobility at a group-level requires less data than at an individual level (Clark et al., 2015b). Three pieces of information are required. First, the surname distribution of all people in Hungary. Second, the surname distribution of people who constituted an elite occupation. Third, the share of this elite occupation in the overall population. The extent to which our analysis can stretch back in history depends on the availability of these data.

Population distribution of surnames

To carry out the analysis, we need to know what percentage the surnames of interest had in the population, for which we need census data, or big enough sample of surnames of people.

The first census in Hungary took place in 1784, but until 1869 the authorities only collected data on the taxable population. Unfortunately, the nobility was exempted from taxation during the feudal period, so the early census has very little information on privileged social groups. Instead, we use a representative sample of the 1869 census, which collected information on all individuals.[75] The sample has 27444 persons with an identifiable surname. We focus on people who did not belong to Eastern Christian denominations (as they mostly lived in areas which were detached from Hungary in 1920); who were men (as we are not able to tell the maiden name of married women, and social status might have an effect on the name distribution through selective marriages) and who were between the ages of 15 to 40. This latter restriction is made to create a sample that is similar in age to the next data source we use (marriage records). Applying the restrictions produces a sample with 4543 observations.

Civil registry of births, marriages and deaths started in Hungary 1895. The records have been systematically digitized by the Hungarian Society for Family History Research, who provides us the sample of surname distributions for each year. We use the surname distribution of brides and grooms as it represents the distribution of the active age population. The data contain 842 thousand surnames of people who got married between 1895 and 1951. As the goal of the Society is to digitalize all the records that are available, we assume that the data at hand are a random sample of all marriages that happened (we discuss this assumption in the Appendix). For years before World War II, the average yearly number of marriages is 16672, while after the number drops to 6774 marriages. This corresponds to a coverage rate of 9.5% in 1938, and 2.5% in 1949

[75] Laboratory of Historical Demography (MPIDR). 1869 Census of Hungary, Version 1.0 [Mosaic Historical Microdata File]. www.censusmosaic.org, 2014.

of the total number of marriages (Balázs, 1993).

For modern data we obtained the actual surname distribution from the population registries of 1998 and 2016. The data provide the exact number of individuals having a specific surname except for those surnames which are only held by a single person.

3.3 Definitions of privileged, underclass and reference groups

Privileged group - names ending in "-y"

We consider surnames that end in "-y" as the privileged group . By the early 19th century surnames ending in "-y" were broadly considered elite names, and holding such a name was associated with being a member of the nobility. Originally, most names ending either in "-i" or "-y" were derived from the place of origin of the family or are patronyms,[76]

Before Hungarian orthography was codified in the early 19th century, the "-y" ending did not have a socially more favorable status over the "-i" ending. There were geographical regularities, such as families from the Eastern part of Hungary preferring the "-i" ending, while their Western counterparts often opted for the "-y" suffix, and many families used both forms interchangeably (Juhász, 2009). However, with the growing demand for written culture and the establishment of a canonical Hungarian grammar, the notion became widespread that the "–y" ending was a marker of nobility, as opposed to the "–i" version. The first official book on Hungarian orthography was published in 1832. Yet, in an 1818 essay on proper Hungarian writing, count József Dessewffy (a prominent aristocrat) explained that Hungarian noblemen were keen to have their names written with the "-y" ending. The likely reason for this fashion, he argued, was that written Hungarian closely followed the rules of pronunciation, so the "-i" ending would have been the default choice of ordinary people for spelling names. This rendered the "-y" version a more archaic, and thus, a more elegant one. When Dessewffy was describing his choice on how to write names, he wrote:

> "I keep the 'y' at the end of surnames, as I do not want to offend anybody; I would rather slip and violate the rules of Debreczen Grammatics than annoy anybody.

[76]The suffix "-fi"/"-fy" being the Hungarian equivalent of the "-son" ending in English, while in the non-patronymic cases conventional wisdom says that the name indicates the location where the estates of the family were originally. For the non-Hungarian reader we note that the palatal versions of the consonants "d", "l", "n" and "t" are written as "gy", "ly", "ny" and "ty" respectively in Hungarian. If the surname's final phoneme is one of these sounds, then the name technically ends in "-y", but does not signal elite status at all, and we do not include it in the privileged group. In fact, the name "Nagy" (meaning "Big" in English) appears as the single most frequent surname in our data over the whole study period.

> However I chose to write his name, Horace would not get offended; he perhaps would not mind even if he lived today. However, I know several Hungarian lords who [..] would very much take umbrage if their surnames were not adorned by a long-tailed 'y' instead of a small 'i'" .(Dessewffy, 1818)

After the revolution of 1848, members of the progressive elite with noble backgrounds started to voluntarily and demonstratively change their names to the more plebeian "-i" ending. Documented examples include such notables as Artúr Görgey/Görgei (Katona, 1988), the general who briefly held the title of dictator of Hungary during the final days of the war of independence in 1849; and Mór Jókay/Jókai, perhaps the most renowned and prolific Hungarian novelist of the 19th century (Mikszáth, 2016). The names ending in '-y' were widely considered as a mark of privilege and were put under protection at the end of the 19th century, when tens of thousands of people decided to "Hungarianize" those family names which suggested a foreign origin.[77] Consequently, it was essentially impossible to adopt such a name since the 1880s (Karády and Kozma, 2002a, page 61). In the few and far between cases that a name ending in "-y" was adopted, it mostly signaled that the family was ennobled at the same time.[78]

Notably, the names of archaic orthography, such as those ending in '-y', are still protected; the 2010 Law on Civil Procedure states that "historic" (article 4/B of §49) and "archaic" (article 4/C of §49) names cannot be adopted.

The 1869 census data show that the "-y" ending names belong to people with the level of illiteracy lower than the average, which is an indication of higher status. In the total sample with recorded surname, the "-y" ending names (2.5% of the sample) have an illiteracy rate of 49%, why the other names have an illiteracy rate of 64%. After introducing the sample restriction, they constitute 2.91% of the sample, and have an illiteracy rate of 29% against a 44% rate for all other names. The differences are significant at all conventional significance levels.

Figure 12 shows population shares of "-y" names from 1869 to 2016. This includes the single data points from 1869, 1998 and 2016, and decade averages for the years between 1895 to 1951. Figure 13 focuses on the marriage registers, where the data are of yearly frequency

In the period before World War II, the share of "-y" surnames remained relatively low, ranging from 2.9% to 3.5%. After the war, the share dropped sharply to about 2% and has continued

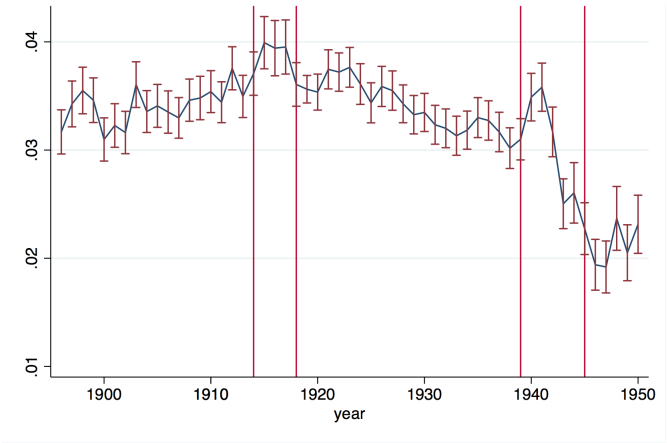
[77] See Chapter 3 of the thesis for a description.

[78] Or was admitted to the Knightly Order of the Vitéz during the Horthy regime. As the head of the state was legally just a regent of the Kingdom of Hungary, he had no constitutional authority to make noblemen.

Figure 12: Population share of "-y" names from 1869 to 2016

The figure shows the average share of names ending in "-y" across different data sources. Vertical lines correspond to start- and end dates of World Wars. 95% confidence bands included.

Figure 13: Population share of "-y" names in the marriage records (1895 to 1951)



The figure shows the average share of names ending in "-y" yearly in the marriage records.. Vertical lines correspond to start- and end dates of World Wars. 95% confidence bands included.

to decline until today (1.9%). There are many potential reasons behind this. The first one is selective war mortality, as the "-y" names were clearly overrepresented in the military elite over the Horthy era (as we show later on in this paper). This is supported by the fact that the share of "-y" names in marriages gradually decreased during World War II. The second reason is that the communist purge of the "Horthysts" disproportionately affected this group. Many of its members chose emigration, were thrown into prison, or even killed. Another potential reason can be spontaneous name changes. Similarly to the mid-19th century, when more democratic minded nobles sometimes switched to a "-i" ending on purpose, members of "-y" named families during communism might have switched to "-i" ending to avoid being identified with the elite of the former regime. Finally, the communist era pro-family policies, such as the tax on childless households in effect between 1950 to 1956, probably contributed more to the fertility of lower status households. We discuss in Section 5.1 how these might affect our estimates.

Disadvantaged groups

We use two approaches to identify underclass groups in Hungarian society. First, we focus on names that were most underrepresented among high school graduates between 1920 and 1945. Here we have to balance two considerations: we need names that are among the rarest in graduates, but frequent in the population (a rare name can be underrepresented in our education data by pure chance). In the first regard, we considered names that had at most 1 high school graduate per 200 people. In the second regard, we considered names that were worn by at least 500 people (634 names), and at least by 1000 people (168 names) in 1998. We looked at two different values to show that the exact location of the cutoff does not affect the results qualitatively. Importantly, a striking number of the names that we identify this way are names that are often associated with the Romani minority.[79] We take this as a sign that this approach identifies low status groups efficiently.

In the second approach, we explicitly measure the status of people with a Romani name in Hungary. Official estimates of the size of this disadvantaged minority vary a lot. Kertesi and Kézdi (1998) estimate their number to be between 424 and 473 thousand in 1998, while the official figures were around 206 thousand in 2001.[80]

[79] Surnames that reflect a profession that was historically common among the Roma, such as Kalányos, Rostás, Kolompár, Orsós etc. (archaic names for spoon-, sieve-, cowbell- and spindle-maker, respectively).

[80] See the population census statistics of 2001. In present day Hungary the law forbids the state from ethnic

Our approach in identifying Romani names is straightforward. We collect all names from the Hungarian Encyclopedia of Family Names (Hajdú, 2010) that are listed as having an etymological origin in a Romani language, or as closely associated with the group.[81] Figure 14 presents the population shares of the three different definitions of low status surnames. The share is increasing over time for all group definitions, though in earnest only after the 1930s.

As the first category of underclass names uses underrepresentation in education in the Horthy era to define the group itself, using it to estimaste relative representation would be a circular endeavor. With this category we only look at the years after World War II. The issue with Romani names is that their bearers did not have any representation in the elite occupations of the earliest studied generations. As a consequence, we cannot look at the evolution of their relative representation before the 1920s.

Reference groups

Our methodology measures the mean status of a surname group relative to a reference population. As a baseline we use the population of the country as a whole. However, since the late 18th century, the distribution of surnames in Hungary has changed a lot not just due to natural population movement (differences in fertility and mortality rates of people with different surnames) but also due to political shocks. To make sure that our estimates are not driven by the choice of the reference group, we explore how historical events affected the surname distributions, and use alternative reference groups as robustness checks.

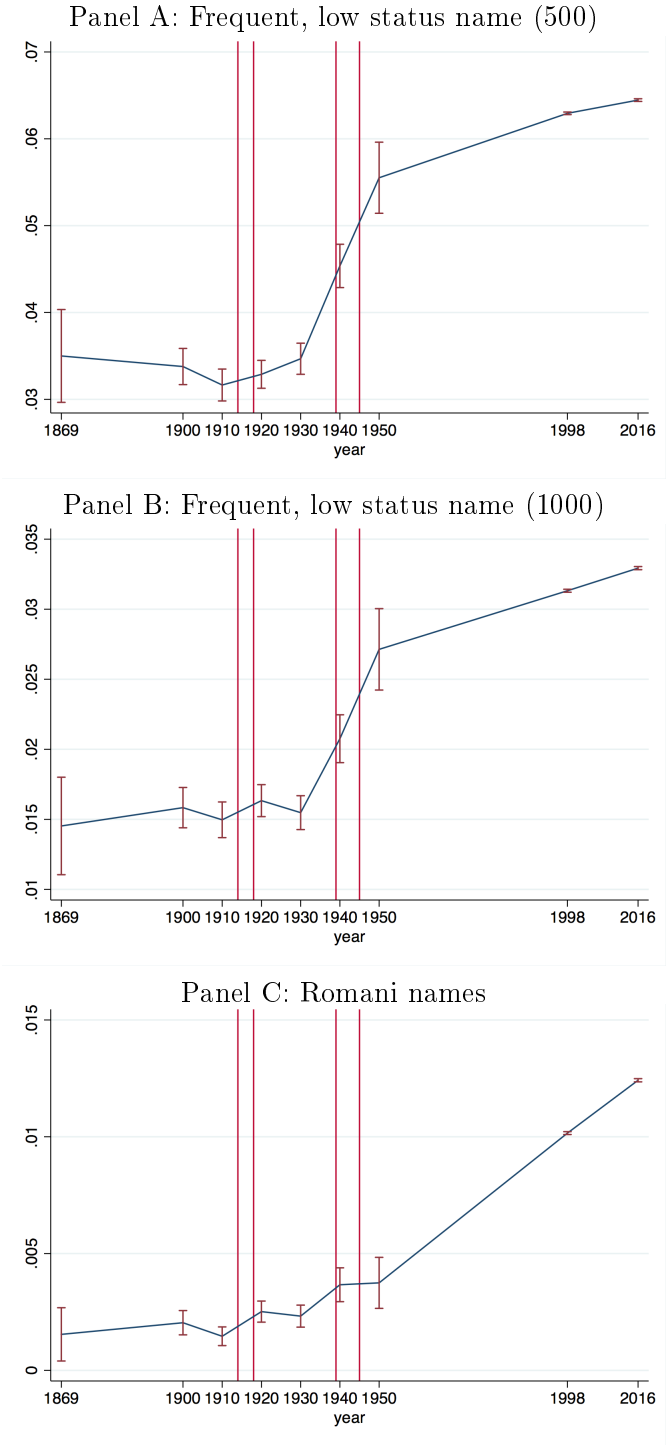
The Treaty of Trianon after World War I changed the ethnic landscape fundamentally. The territories that Hungary lost had higher non-Hungarian share of population, increasing the share of Hungarian names in the remaining are of the country. This compositional change was exacerbated by selective migration (emigration of Slavic minorities, immigration of ethnic Hungarians from lost territories).

Another important factor affecting the surname distribution is the increasing tendency to "Hungarianize" (adopt Hungarian) surnames, starting from the final third of the 19th century (Karády and Kozma, 2002a). Sections 2.3 to 2.5 in the third chapter of the dissertation give a detailed account of the "Hungarianization" movement. What needs to be highlighted, is that

profiling. As a consequence, the census questionnaire only asks whether the responder identifies as Romani or not, and even those who do might not report it, due to historical experiences of persecution or contemporary experiences of prejudice.

[81] The names are: Berki, Káló, Kanalas, Kolompár, Lólé, Móré, Orsós, Pusoma, Rézműves, Rostás, Zámbó, and their common variants.

Figure 14: Population share of low status names from 1869 to 2016



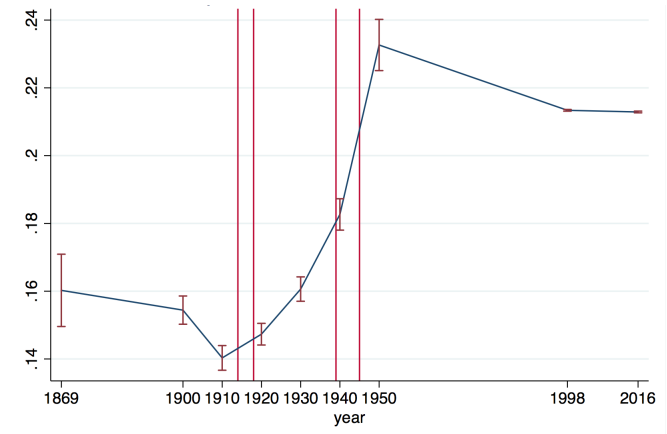
The figure shows the share of different definitions of low status names within society. Vertical lines correspond to start- and end dates of World Wars. 95% confidence bands included.

this was a legal, bureaucratic process, and the law prevented the adoption of "elite names". As a consequence, family name Hungarianization affected the pool of non-elite names only.

Wars change surname distributions if mortality rates are correlated with social status (e.g. if high status people can avoid the draft, or start from higher ranks of the military), but World War II had other devastating effects, too. The Holocaust was one additional major shock to the composition of the population. The consequence of the active participation of the Hungarian government in the deportations was that the Hungarian Jewry was among the Jewish communities that lost most life both in absolute and relative terms (Braham, 2000). In addition, after the war, hundreds of thousands of ethnic Germans fled or were expelled from Hungary. Jews and ethnic Germans who did not "Hungarianize" had distinctive surnames, so their loss contributed to the changes in the name distribution after 1945.

The consequence of dramatic social changes and shifts in the surname distribution can be that the difference $x_t^g - x_t^{ref}$ changes over time because of systematic changes in x_t^{ref} . To mitigate this problem, we anchor our estimates to multiple reference groups. The baseline reference category is the whole society. The second reference category we use is the group of the top20 most frequent Hungarian surnames over the study period.[82]

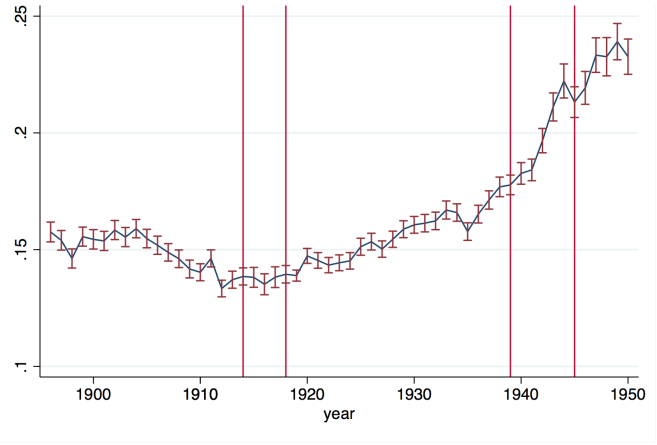
Figure 15: Population share of top20 names from 1869 to 2016



The figure shows the share of names which ever made it to the Top20 most frequent in our data across different data sources. Vertical lines correspond to start- and end dates of World Wars. 95% confidence bands included.

[82]These are Balog, Farkas, Fehér, Fekete, Horváth, Juhász, Kis, Kovács, Lakatos, Mészáros, Molnár, Nagy, Német, Oláh, Pap, Rácz, Simon, Szabó, Szilágyi, Takács, Török, Tóth, Varga, and their most common variants (such as "Kováts", "Németh", "Tót", "Rác", "Takách" etc.). The top20 is relatively stable over time, but some changes do occur, which explins why the list contains 23 names.

Figure 16: Population share of top20 names in the marriage records (1895 to 1951)



The figure shows the share of names which ever made it to the Top20 most frequent in the marriage records. Vertical lines correspond to start- and end dates of World Wars. 95% confidence bands included.

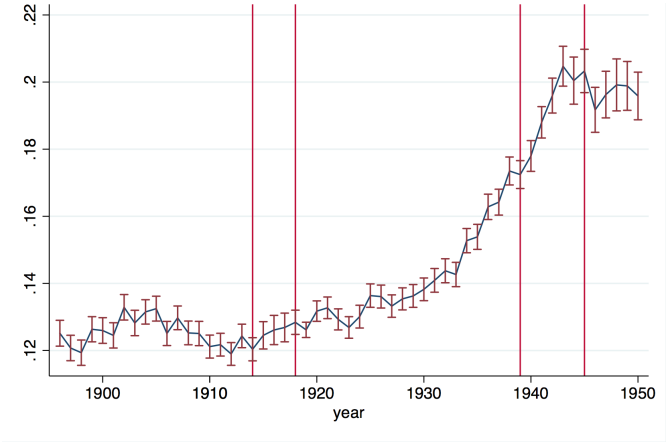
Figure 15 and Figure 16 present the share of top 20 names over the whole study period and during the years when the data are of yearly frequency. They show that the population share of top20 names increased by about 6 percentage points during the interwar period. One reason for this can be fertility rate differentials between lower and upper classes. Another reason could be name changing into top20 names.

Another group we consider, in light of the discussion above, are surnames ending in "-i" which have identical etymology, but lower perceived status than "-y" names. These names also exhibited a steep increase in their population share during the interwar period, by 8 percentage points from 12% to 20% (see Figure 17 and Figure 18). The reasons of the increase are similar to those that drove the increase in the top 20 name share.

Figure 17: Population share of "-i" names from 1869 to 2016

The figure shows the share of names ending in "-i" in our data across different data source. Vertical lines correspond to start- and end dates of World Wars. 95% confidence bands included.

Figure 18: Population share of "-i" names in the marriage records (1895 to 1951)



The figure shows the share of names which ever made it to the Top20 most frequent in the marriage records data. Vertical lines correspond to start- and end dates of World Wars. 95% confidence bands included.

3.4 Definitions of elite occupations and data sources

The ticket to the top - high school graduates

The first elite subpopulation we consider is high school graduates. Although being a graduate is not an occupation, high school graduates until the mid 20th century were the ones who could obtain high level administrative jobs or could go to university. As the brief historical overview below attests, this essentially guaranteed an eventual elite occupation for the graduate.

After the revolution of 1848-49, the Habsburgs introduced a range of Germanizing reforms

in Hungary, including a system of high school education modeled on the Austrian example (Sasfi, 2014). The demand for secondary education remained moderate, as the underdeveloped state bureaucracy and industrial sector did not employ that many graduates. This changed fundamentally after the Austro-Hungarian Compromise of 1867. Karády (1995) argues that by the early 20th century holding a high school diploma was a necessary and sufficient condition for becoming a member of the elite, with a notable exception for those coming from the "fringes" of the society, such as Jews, Germans or people with a working class background.[83]

The Hungarian high school system had two pillars: the humanities-oriented "gimnázium", and the more science and technology oriented "reáliskola". "Gimnázium" retained Latin language as one of its core subjects, and prepared students for law and arts faculties, whereas "reáliskola" prepared for technological or science faculties. The former was the older and more prestigious type, but since 1875 both types of school offered an 8-year curriculum and an identical baccalauréat diploma upon graduation. Those who did not want to pursue another 8 years of education, or did not have the means to apply to these elite institutions, could take advantage of the increasing supply of miscellaneous high school institutions such as civic schools ("polgári iskola") which offered a 6 years program for boys and a 4 years program for girls, and qualified its graduates for specialized schools of higher education, such as commercial schools and teacher training schools.

Karády argues that these civic schools were the institutions that soaked up the cohorts of students with a humble social background, whereas the two main high school types were mostly reserved for the reproduction of the elite. At the turn of the 19th and 20th century, 80% of the 8th-graders had parents with at least high school education (Karády, 1995 p. 663), and were mostly of landowner or civil service background. In addition, the elite high schools offered scholarships to students who came from less affluent noble families.

The system remained largely unchanged until 1945, except for the introduction of girls' high schools, and the merger of the "gimnázium" and "reáliskola" types into a single, 8-years long "középiskola".[84] After World War II all schools were nationalized, and, a 8-year long primary school was introduced, followed by a 4-year long high schools.

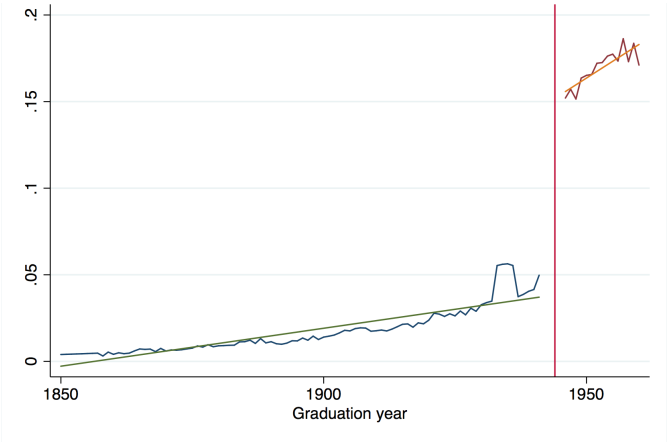
Our data on the total number of high school graduates come from three sources. For the pre-1945 period we use the Statistical Yearbook.[85] Between 1945 and 1960 the data are not

[83] indicating the successful passing of a final exam, "érettségi vizsga" in Hungarian; closest equivalent is the Baccalauréat in France.

[84] Literally meaning "middle school", as the Hungarian equivalent of "high school" refers to tertiary education in Hungarian.

[85] These are a quite reliable for the period between 1885 and 1941. The periodical was published yearly except for the decade starting with 1915 when three respective volumes were published for the 1915-18, 1919-1922, 1922-

Figure 19: High school graduate share in the 18 year old cohort



The figure shows the share of high school graduates in the 18 year old cohort. Source: Statistical yearbooks.

available, except a single data point from 1955 (Erdész and Szabady, 1958). Since 1960 the Central Statistical Office of Hungary reguraly reports the number of high school graduates.

There are two structural breaks in the time series on the total number of high school graduates. The first, in 1919, is due to change of territory and population.. The second, in 1945, is due to a massive expansion the public high school education. Enrollment rate grew from 5% of the 18 year old cohort in 1942 to 17% in 1955.

To resolve the data discrepancy caused by the 1919 border changes, we use the detailed list of all high schools and their number of students from the 1910 Statistical Yearbook. We calculate that 55% of pupils from 1910 were studying in schools that remained within Hungarian borders after 1920. We multiplied all years before 1919 with this figure to obtain the "Trianon border share" of students for pre-Trianon years.

The adjusted time series has no break in 1920 and is well approximated by an exponential trend until the post-WWII reforms. Regressing the log number of graduates on a time trend term results in a coefficient of .0357 (standard error: 0.00088), and an R^2 of 98%. We use this regression to impute the number of graduates for all pre-1945 years where it is missing. There are two data points for the post-World War II years for 1955 and 1960. We apply a linear extrapolation from these data points to all years between 1945 and 1960.

Figure 19 shows the population share of high school graduates relative to the size of the 18

1925 periods. From this data source we know the yearly number of high school graduates except for the years 1916, 1917 and 1918. The 1919-1922 volume starts publishing data according to the country boundaries defined by the Treaty of Trianon. Before 1889, a precursor to this periodical was published by the name "Economic and Statistical Yearbook". Unfortunately, this was not on the level of the later standards and the editing varied a lot over the years. Nevertheless, these books provided 3 additional data points (1885 to 1887).

years old cohort. The peak between 1933 and 1936 is because the cohorts graduating in these years were born between 1916 to 1918 – at the height of World War I. These cohorts are much smaller than previous and later ones, and those graduating represent a higher proportion of the cohort.

The share of high school graduates experienced dramatic increase after 1945. One consequence of the post-WWII high school reform was that a range of other institutions started issuing the same kind of diploma as the elite schools. In addition, many of the working class youth enrolled into fast-track programs that offered the diploma while the student could have remained in the labor force. Arguably high school education lost its high social status at this point.

Our method of estimating social mobility requires the surnames of high schools graduates for each cohort. From this we can calculate the share of the priveleged or underclass groups among graduates. We exploit a unique registry of high-school graduates compiled by historians Zsuzsa Bíró, Viktor Karády and Péter Tibor Nagy. The data contain the surnames of:

- All graduates of"gimnázium" or "reáliskola" between 1850 and 1919;
- Boys who entered these schools in 1860, 1880, 1900 or 1910; All girls who entered in 1910;
- All students who entered a "gimnázium" or a "reáliskola" within the area defined by the Treaty of Trianon, in the years 1920, 1930, 1933, 1940, 1942 (Karády, 2012).

Unfortunately, we only have access to the name distributions for cohorts defined by birth years, so we are unable to distinguish between those who were recorded as freshmen and those who were recorded as graduates. Consequently, we must assume away any differences in drop-out rates between elite and non-elite students.

The elite of skill - medical doctors

Since the earliest recorded history medical doctors have been enjoying a very high status in societies, which is attributed to their training and their particular set of skills. Ideally, the entry into medical professions should be based on merit. However, becoming a doctor needs

substantial investment into one's own human capital. This can become a significant entry barrier for less affluent people. In addition, medical training is highly regulated, resulting in permanent under-supply of doctors and raising the returns from the profession. Privileged groups might find it therefore remunerative to further raise the existing barriers to decrease competition from outsiders. The composition of the medical graduate body is thus an important aspect of social mobility.

The first medical school at a Hungarian university was founded in 1769, and education started the next year with 40 students. The host university moved to Buda in 1777 and then to Pest in 1784. It remained the main domestic supplier of medical graduates until 1872, when the university opened at Kolozsvár (present day Cluj-Napoca in Romania). In the early 20th century, new universities (with medical faculties) were founded at Pozsony (present day Bratislava) and Debrecen. Following the Treaty of Trianon, the university at Kolozsvár moved to Szeged, and the university at Pozsony moved to Pécs.[86] These institutions remain the ones that train physicians to these days.

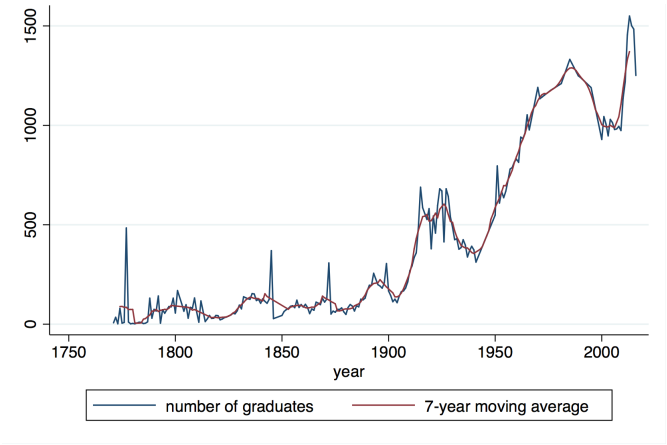
We compiled the list of those who graduated before the mid-20th century from archived yearbooks. [87] The list of those graduating later was made available to us by the State Healthcare Service Center, which keeps an up-to-date database of everyone with a medical license. The total official annual number of medical graduates comes from statistical yearbooks.[88] For the years where we observe both the individual names and the official figures, the coverage of our dataset is almost complete, so for the years where official figures are not available, we assume that the total number of graduates is the number of names in our data.

Figure 20 shows the number of people who graduated as physicians from the foundation of the first medical faculty to present day. The number varies noisily across years, which is due to uncertainty in the exact year of graduation as the academic year starts in the Fall and ends in the Spring. To get rid of this noise, we use moving averaging and choose the narrowest averaging window that eliminates spurious year-on-year spikes in the data (7 years; our results are not sensitive to choosing narrower windows, but this makes visual processing of the figures easier).

We observe periods of growth in the number of graduates in the early 1900s (when the new faculties are founded) and from the 1950s to the 1980s (when higher education is expanded under communism). The number of graduates declined during World War II and after the fall

[86] see Kapronczay (2013); Kiss (2018); Péterffy (2016)
[87] Available at http://library.hungaricana.hu/hu/collection/sote_kozponti_konyvtar_klasszikus_orvosi_konyvek/
[88] The offical figures are available from the mid 1880s until 2000.

Figure 20: Number of medical school graduates



The figure shows the number of medical school graduates. Source: Statistical yearbooks and graduate database.

of communism.

We choose the population aged 25 years as the reference cohort size. The share of graduates does not exceed 0.5% until the 1950s, and only exceeds 1% at the highest peaks in the late 20th century, attesting that doctors have been a narrow elite occupation over the whole study period.

The elite of loyalty - army officers

As a standing army's purpose is to uphold the current establishment and political order against internal and external threats, it is natural that its officers are among its highest and most privileged members. The access to officer corps has always been of political importance, and privileged groups invested in restricting it and maintaining the status quo.[89]

The Compromise of 1867 created three separate armies in Austria-Hungary: the Common Army, the Imperial Austrian Army, and the Royal Hungarian Army (Jewison and Steiner, 2005).[90] To study social mobility Hungary, officers of the Common Army and the Royal Hungarian Army are of particular interest.

Unfortunately, we do not have an exhaustive list of officers from the era. For officers who were born before 1900 we use the catalogue of records from the archive of the Hungarian Military History Museum.[91] We only keep observations that are uniquely identified by full name, birth

[89] See, for example, the case of Britain (Otley, 1970)

[90] The Common Army was called "Die k.u.k. Armee", as in "kaiserlich und königlich" (imperial and royal); the Hungarian army was referred to as "k.u." ("königlich ungarische", or Royal Hungarian); the Austrian was "k.k." ("kaiserlich-königlich", or "imperial-royal"). "K.u.k." officers were trained at the Theresian Military Academy in Wiener Neustadt, "k.u." officers at the Ludovika Academy in Budapest.

[91] We do not observe the records directly; our observations are catalogue elements containing the birth year of each officer, so double-counting is possible. To avoid this issue, we only use observations pertaining to lieutenants,

Table 12: Comparison of data coverage

Elite group:	High school	Medical doctors	Army officers
Time Coverage	1880-1951	1770-2016	1870-1944
Regime Coverage			
Feudal Monarchy/Absolutism		✓	
Liberal Monarchy	✓	✓	✓
Conservative Authoritarianism	✓	✓	✓
Communism	✓	✓	
Liberal Democracy		✓	
Group Coverage			
Privileged	✓	✓	✓
Underclass		✓	

year and mother's name. Furthermore, we only use data of officers who were born between 1850 and 1899. The Royal Hungarian Army was set up following the Compromise of 1867, so the first recruits were born around the year 1850.

The number of officers born before 1895 never exceeds a thousand per year. World War I led to the expansion of the army, and the number of officers born in 1895 increased by 71% compared to 1894 . The coverage drops sharply after the cohort of 1899 and there are only a handful officers in our sample who were born in the 20h century.

During the Horthy-regime (between 1920 and 1944) Ludovika Academy was the only training institution for officers of the Army. We know the graduates by name from the collection of János Szentváry-Lukács.[92] This was relatively small and highly selective institution. Before World War II, the yearly number of graduates fluctuated between 79 (1922) and 246 (1932).

One complicating issue is that we observe graduation year for the Ludovika data, but birth year for previous decades. We therefore assume that cohorts born no later than 1890 graduated at the age of 23, since the WWI expansion affected the birth cohorts of 1891 and later. For the wartime years we assume that officers born in 1891 and 1892 graduated in 1914, 1893 and 1894 in 1914, 1895 and 1896 in 1916, 1897 in 1917, 1898 in 1918, 1899 in 1919. This way we can harmonize the data across sources. Unfortunately, we do not have access to names of army

officers who were trained under communism.

4 Results

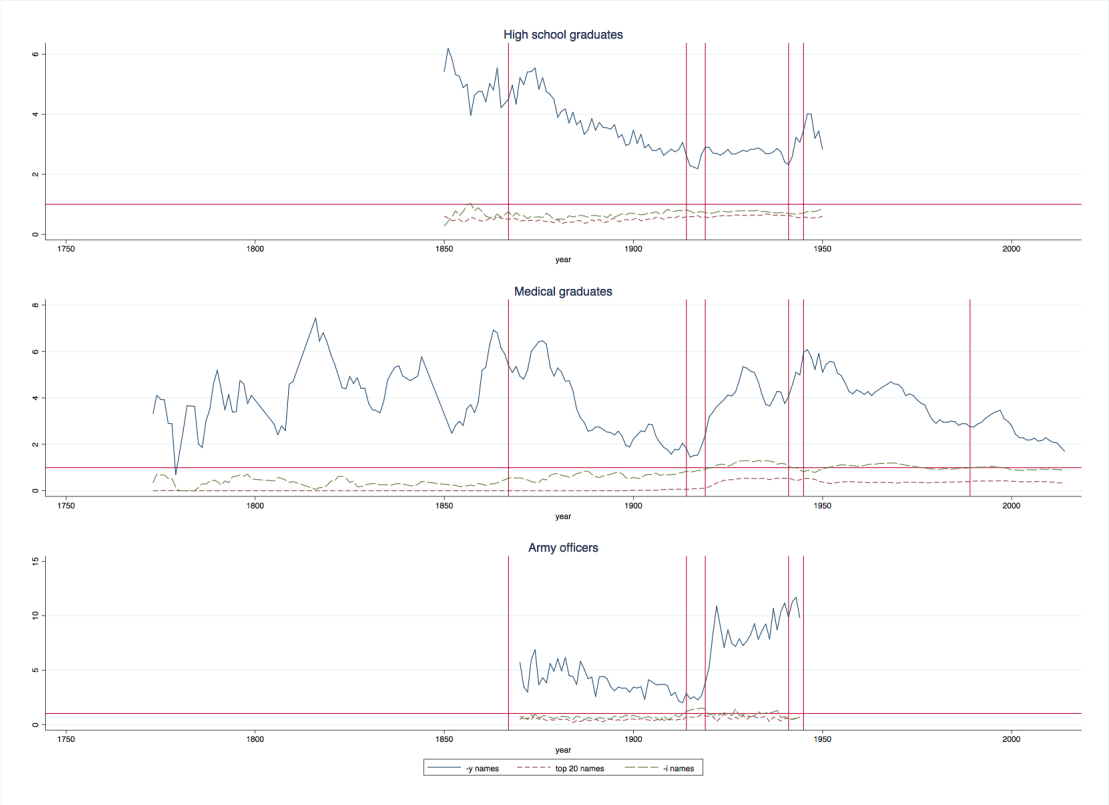
4.1 Relative representation of privileged groups

Figure 21 and Tables F.31 to F.33 present the relative representation (as defined in Equation 14) of "-y" surnames and the reference groups for different types of the elite occupations. The vertical lines correspond to regime changes (as in 1867) and start and end dates of World Wars, while the horizontal line corresponds to the theoretical line of proportional representation, when the group has exactly the same share in the elite occupation as in the population ($RR_g = 1$).

The main result is that "-y" ending names are overrepresented in all elite occupations during all institutional regimes and time periods. Shockingly, even today, more than two generations after all noble titles were abolished and after four decades of communism, people with traditionally noble surnames are significantly overrepresented among medical doctors.[93] This suggests that social status may be very persistent and slowly converge to the social average. A notable exception from this rule is the conservative authoritarian regime of Admiral Horthy between 1920 and 1944, where this convergence either stops (in the case of high school graduates) or even reverts (medical doctors and army officers). As the elite occupations we consider are related to education or public service, the divergence of social groups during the authoritarian regime can be considered as an example of elite capture. It is also interesting to note that during the liberal Austro-Hungarian Dual Monarchy the privileged group converged faster to the mean (for medical doctors) than during communism and the first two decades of liberal democracy.

The alternative reference groups (top20 names and names ending in "-i") are underrepresented in the elite occupations relative to their population shares. There are no clear trends except for a slight increase in status starting during the 1920s. As the relative representation of the two groups remains stable over time, both seem to be good alternative comparison groups for the "-y" ending names. For this reason, we confine calculations using the alternative reference groups

Figure 21: Relative representation of "-y" names and reference groups



The vertical axis corresponds to relative representation ratio. The horizontal line corresponds to proportional representation. The vertical lines show start and end dates of world wars.

to the Appendix as robustness checks.

4.2 Persistence of elite status

We estimate the social mobility rate using the method outlined in Section 3. Figure 22 shows the model defined in Equation 13 estimated for each elite occupation and political regime.

The vertical axis has logarithmic scale and captures the average status advantage of the "-y" names in log standard deviation units. The slope of each line segment shows the estimated $ln(\hat{\beta})$ coefficient of that particular period, and it corresponds to the rate at which the group average regresses to the population average. In particular, the status of the cohort in year $t+1$ is $\hat{\beta}$ times the status of the cohort in year t. A slope of 0 means that $\hat{\beta} = 1$, in other words, that the group is able to perfectly pass on status over time and does not regress to the social mean. A negative slope corresponds to $0 < \hat{\beta} < 1$, which means that the average status converges to the mean,. A positive slope corresponds to $\hat{\beta} > 1$, meaning that the group average is diverging from the mean.

The vertical lines correspond to start and end dates of World Wars and other regime changes: the year of the Compromise of 1867 (medical doctors); the public education reform of 1875 (high school graduates); and t the fall of communism in 1989 (medical doctors).

To enhance clarity, Figure 23 shows all the estimated line segment slopes $\hat{\beta}s$ from Figure 22 along with 95% confidence intervals, in a breakdown by political regime.

The general patterns are similar as in the case of the relative representation plots. Under illiberal regimes, such as monarchy before the Compromise of 1867 and the authoritarian parliamentary system between 1920 and 1944, the privileged group successfully preserved or even extended their access to elite occupations. Interestingly, even during the early years of communism they benefited more from the expansion of secondary education relatively than other groups (Panel A of Figure 22).

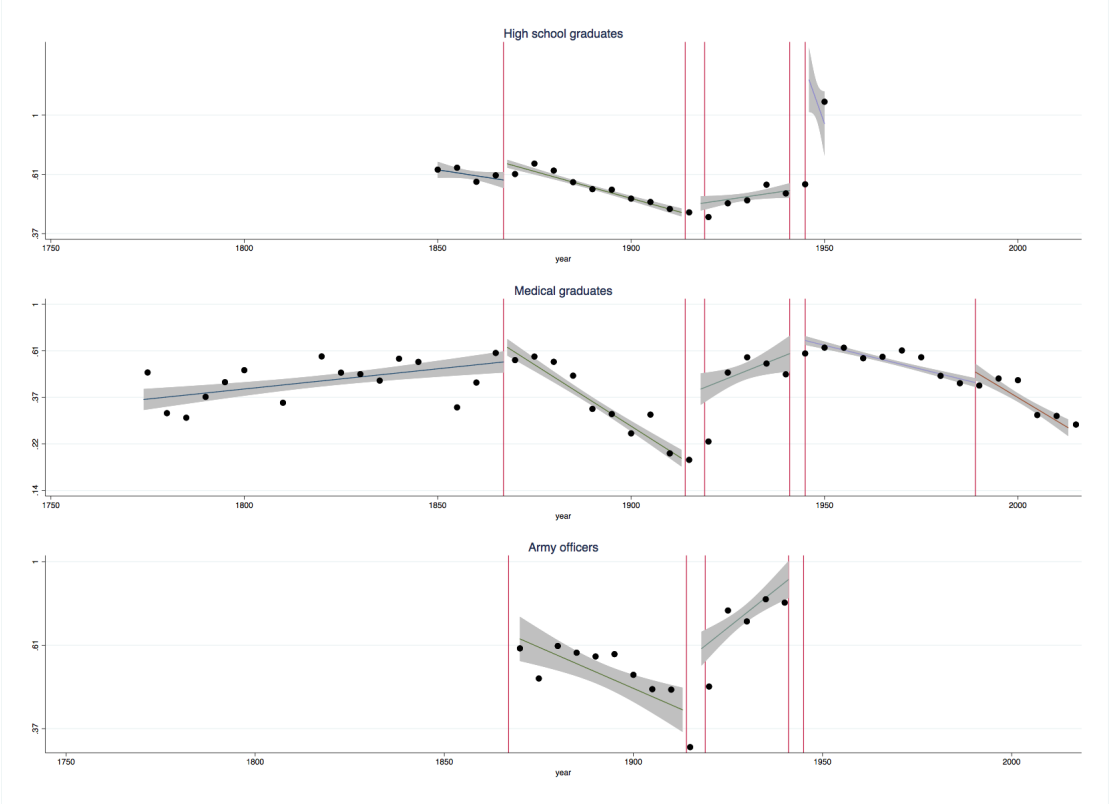
The privileged groups converged to the mean at a similar rate under constitutional monarchy as under liberal democracy. We can compare these rates to social mobility under communism using the doctors data. As Panel B shows, mobility under communism was slower by this measurement. This is striking, as communism explicitly favored upward social mobility and

which is the lowest rank that a professional officer gets upon graduation.

[92] http://www.hungarianarmedforces.com/ludovika/
Accessed: 7 January 2019

[93] Law 1947/IV abolished all titles and privileges (still in effect today).

Figure 22: Average status advantage of "-y" names above the population mean



The figure plots the natural logarithm of status advantage against time and fits a line for each regime according to 13. The slopes are the estimated $ln(\hat{\beta})$ coefficients. The implied $\hat{\beta}$s are shown in Figure 23. 95% confidence bands included.

sought to displace "reactionary" groups from the elite. It is interesting to see that when the elite occupation is small in numbers and is potentially able to directly affect its own succession (as is the case with the quite closed "caste" of the medical profession), the privileged "-y" named group is losing status at a moderate rate even under communism.

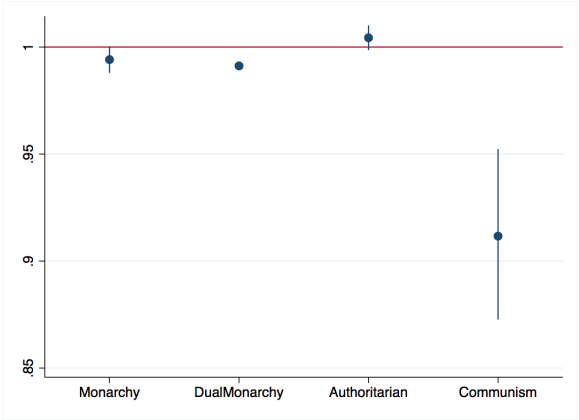
To make sense of the order of magnitude of status persistence, Figure 24, Panel A shows estimates for every regime and elite occupation the years required until the effect of the inherited status shrinks below 10% of a standard deviation (that is, the first t for which $\hat{\beta}^t < .10$).

The dashed line shows the benchmark result from Clark et al. (2015b) (a dashed line). They estimate the persistence rate of Oxbridge graduates at $b = 0.65$.[94] This means that it took 160 years for the status of Oxbridge graduates to shrink to 10% of their former status. In their case, the coefficient does not vary much over time, and provides an extremely good fit for the whole study period of almost 200 years ($R^2 = 0.98$). Of course, the history of political stability in these counties could not have been more different. It is not just that Britain had no Horthy-esque nationalist, authoritarian regime or a communist dictatorship. Even before these, the

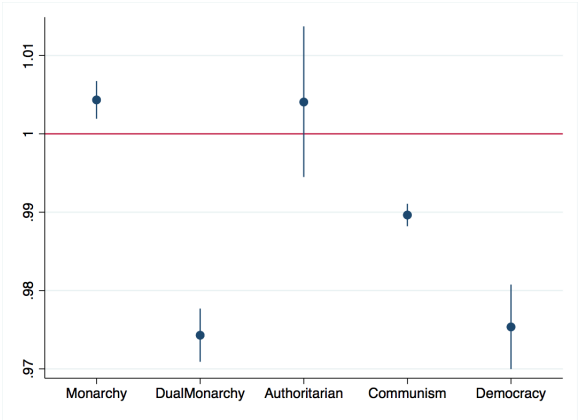
[94] They estimate this rate for every 30 years, so their b coefficient is equivalent to $\hat{\beta}^{30}$ in our case.

Figure 23: Comparison of $\hat{\beta}$ coefficients (reference group: population)

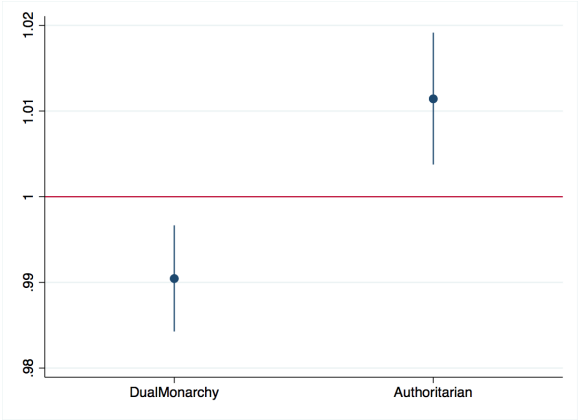
Panel A: High school graduates



Panel B: Medical doctors



Panel C: Officers



The figures show estimated $\hat{\beta}$ coefficients. The horizontal line corresponds $\hat{\beta}$=1. $\hat{\beta} > 1$ means that group status diverges from the mean; $\hat{\beta}$<1 means that it regresses to the population mean. 95% confidence bands included.

direction in which institutions developed in Hungary was much less clear or even unidirectional. Consider the extension of the franchise as a proxy for the general "opening up" of society. In Hungary, the first non-feudal general election was held in 1848; but ballots only became regular after 1861. The franchise was tightened with the Electoral Reform Act of 1872 and established a similar share of people with voting rights as the Reform Bill in Britain in 1832, a generation before (Boros and Szabó, 2008). It was not extended again until the 1910s, and elections were actively manipulated in the regime that followed. Institutional instability was the norm, not the exception.

The difference in our results and the benchmark results from Britain are generally in line with what one would expect. The years needed to regress to 10% of previous status is between 88 (doctors) and 260 (high school). Democracy shows a similarly steep rate of social mobility (with only 92 years), but this estimate is probably a bit exaggerated, and biased by the increasing entry of foreign students to Hungarian medical schools. The patterns during communism are more mixed, with the steepest mobility for high school graduates (25 years), but quite slow rates for medical doctors (221 years, full two generations more than the Clark et al. (2015b) benchmark). The likely reason is that communism was effective at increasing social mobility on the extensive margin, by increasing the overall size of the elite occupation, as it did with high school education. In the case of medical doctors, the task turned out to be much harder.

Panel B presents the number of years after which the expected status increases twofold (the first t for which $\hat{\beta}^t > 2$) for regimes where the "-y" name group is diverging from the social mean. It is important to point out that in these regimes there was effectively *no social mobility* by our measure. This is perhaps unsurprising in the case of a feudal monarchy, which was conservative and favored ancestry above all else. But the Horthy-regime managed to achieve this feat in the 20th century in an even more effective fashion than the Habsburgs a century earlier.

4.3 Upward mobility of the underclass

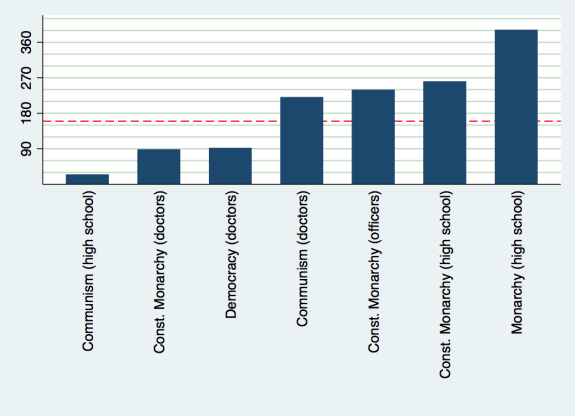
Next, we turn to the analysis of the underclass groups. These include people with Romani surnames[95] and people with relatively frequent names (at least 500 or 1000 holders in 1998) that were underrepresented among high school graduates in the interwar period.

The main problem with the first measure is that many Romani people especially in the 19th century, did not live a sedentary lifestyle and thus did not appear in the official registries.

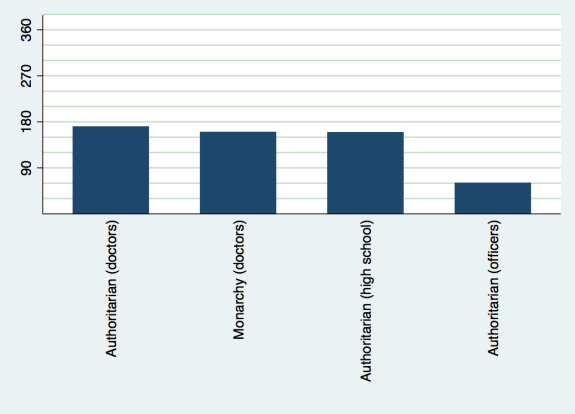
[95] Following Hajdú (2010).

Figure 24: Comparison of $\hat{\beta}$ coefficients (reference group: population)

Panel A: Regimes and elites with $\hat{\beta} < 1$



Panel B: Regimes and elites with $\hat{\beta} < 1$



The vertical axis shows the number of years for which $\hat{\beta}^t < .1$ (Panel A), or the number of years for which $\hat{\beta}^t > 2$ (Panel B). The dashed line is the reference result from Clark et al. (2015b).

Consequently, we probably underestimate their population share, and the results from early decades should be interpreted cautiously. We discuss this in Section 5.1.

On the other hand, the second definition of underclass groups captures a steady stream of people and is much less noisy, but as the definition is based on high school representation between 1920 and 1945, using them in the analysis before 1945, in particular with high school students, is somewhat tautological.[96]

Figure 25 documents the relative representation of the underclass groups among high school graduates (Panel A) and medical doctors (Panel B). The estimated relative representation of Romani names before World War I is noisy, but places this group at the bottom of the social hierarchy. During the Interwar period, however, Romani names start to diverge even further down compared to the other underclass groups. This means that during the authoritarian regime the status of the Romani worsened. After World War II the relative representation of Romani names improved and was at the level of the top 20 underrepresented names.

Strikingly, Panel B, which looks at relative representation among medical doctors after 1945, shows that there is no regression to the social mean for any underclass group. Moreover, the divergence of these groups *increases* with the fall of communism in 1989. This is in stark contrast with the previous result that status persistence significantly decreased among doctors after 1989. The most disadvantaged groups have not been able to reap the benefits of this change.

Figure 26 shows the evolution of the implied social status of the disadvantaged groups. Note that these groups have status below the social average, so the log-linear formulation in Equation 13 can not be applied. Instead, we plot the levels of mean status difference in standard deviation units. We document a very similar pattern, with persistently low status, the absence of upward mobility and the decoupling of the Romani name group, except for short periods of improvement in the 1960s and around 1998.

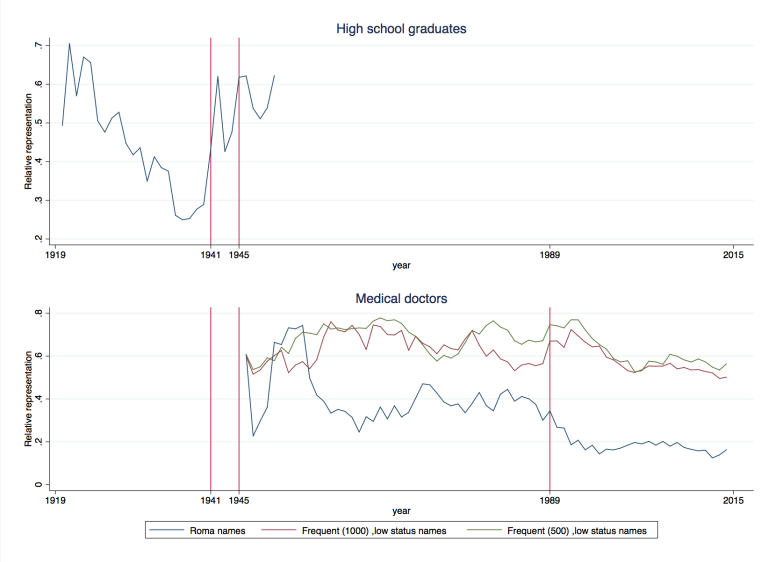
4.4 Social mobility of women

We can use our mother of social mobility to look at status differences by gender. Let's assume that a woman of group g will get into the elite occupation if the following condition holds:

$$y_{it}^{g,woman} = x_t^g + wmn_t^g + \epsilon_{it} > \underline{y}$$

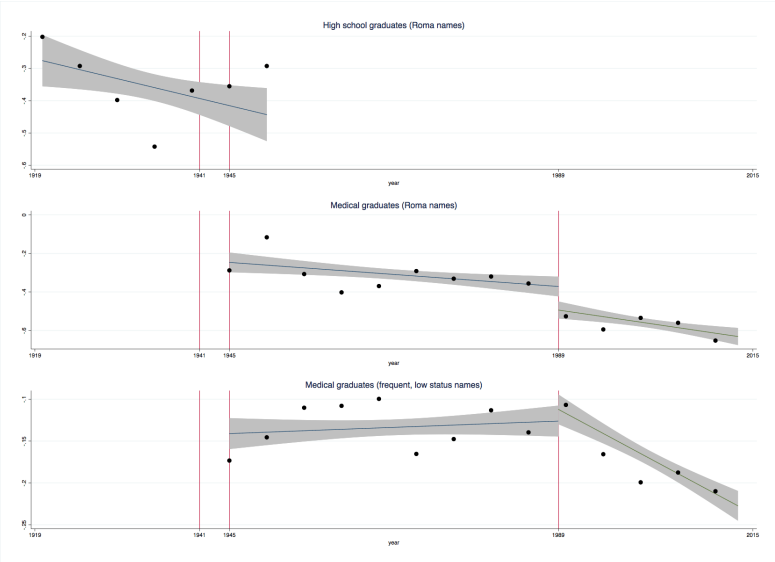
[96] We also exclude the analysis of army officers, as their data come exclusively before 1945, and Romani names among them are extremely rare anyway.

Figure 25: Relative representation of disadvantaged groups in the elite



The vertical axis corresponds to relative representation ratio. The horizontal line corresponds to proportional representation. The vertical lines show start and end dates of world wars.

Figure 26: Implied mean status of disadvantaged groups by representation in the elite



The vertical axis corresponds to mean social status advantage compared to the population mean in standard deviation units. The horizontal line corresponds to the social mean. 95% confidence bands included.

This is identical to our baseline social status model, except for the additional term wmn_t^g, which captures the entry barriers potentially faced by women. If this term is negative, it means that a woman in time t needed to be a more able member of her family (has to have a higher ϵ_{it}) to get into the elite occupation than a man.

We can rearrange this condition similarly as before, to express the probability of a woman from group g getting into the elite as a function of the cumulative distribution function Φ, than invert it to express gender barrier wmn_t^g:

$$wmn_t^g = \underline{y} - x_t^g - \Phi^{-1}(1 - P_{elite}^{g,woman})$$

From the baseline model we know that the term $\underline{y} - x_t^g$ equals $\Phi^{-1}(1 - P_{elite}^g)$, where P_{elite}^g is the probability that anyone from group g (regardless of gender) gets into the elite. Then we can express the gender barrier as

$$wmn_t^g = \Phi^{-1}(1 - P_{elite}^g) - \Phi^{-1}(1 - P_{elite}^{g,woman}). \tag{15}$$

To calculate this, we need to know $P_{elite}^{g,woman}$, the probability that a woman from group g makes it into the elite occupation. This can be expressed as

$$P_{elite}^{g,woman} = \frac{\#g, women\ in\ elite}{\#g, women}$$

Notably, the expression in this case does not involve the choice of a reference category. We can express $P_{elite}^{g,woman}$ as the relative representation of women of group g compared to women in general, multiplied by the population share of women in the elite occupation:

$$\frac{\#g, women\ in\ elite}{\#g, women} = \left(\frac{\#g, women\ in\ elite}{\#women\ in\ elite} / \frac{\#g, women}{women}\right) \cdot \frac{\#women\ in\ elite}{\#women}$$

We can express the latter term as $\frac{\#women\ in\ elite}{\#women} = \frac{\#women\ in\ elite}{\#elite}\frac{\#elite}{\#women}$, so the full expression becomes:

$$P_{elite}^{g,woman} = \left(\frac{\#g, women\ in\ elite}{\#women\ in\ elite} / \frac{\#g, women}{women}\right) \cdot \frac{\#women\ in\ elite}{\#elite}\frac{\#elite}{\#women}.$$

Finally, we further assume that the share of group g women is the same as the population share of group g (there are similar amounts of boys and girls born with a "-y" name), so $\frac{\#g,women}{women} =$

$\frac{\#g}{\#pop}$, and that $\#women \simeq \#pop/2$. With that the expression becomes

$$P_{elite}^{g,woman} = \left(\frac{\#g, women\ in\ elite}{\#women\ in\ elite}\right) / \left(\frac{\#g}{\#pop}\right) \cdot \left(\frac{\#women\ in\ elite}{\#elite}\right) \cdot 2 \left(\frac{\#elite}{\#pop}\right).$$

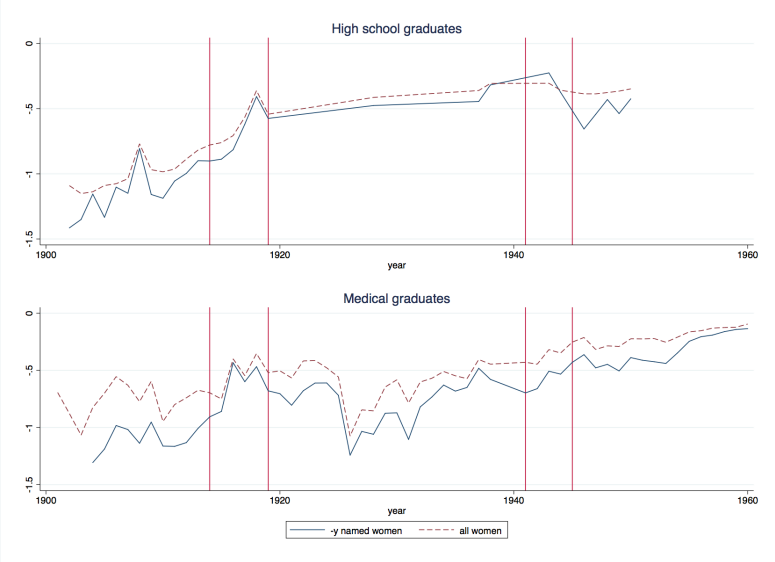
We observe all the figures in brackets. Take the case of medical graduates (occupation) and privileged "-y" name groups as an example. The first bracket contains the share of "-y" named doctors in all female doctors. The second bracket is the share if "-y" names in population. The third is the share of female medical graduates, while the last term is the population share of medical graduates. Knowing P_{elite}^{g} and $P_{elite}^{g,woman}$ we can use Equation 15 to calculate the barrier women faced in entering elite occupations.

To this exercise we can use the high school graduate data and the medical graduate data, as the officer corps remained exclusively male over the study period. There are also limits on the extent to which we can use the data on doctors. The first Hungarian female doctor who graduated from a Hungarian university (Vilma Hugonnai) finished her studies in 1897, and a steady stream of female medical graduates only started in the middle of the next decade. We use first names to infer gender of graduates, and there is no first name data available after 1960, as we only obtained the list of last names from the healthcare administration for privacy reasons. Even in the period between 1900 and 1960, first name data are only available to us only for graduates of Semmelweis university (though it was and remained the dominant medical faculty of the country).

We hand-checked each first name to infer the gender of the graduate. We categorized graduates into or outside the "-y" name group based on maiden names. Where this was not available, we assumed that the person did not have a "-y" name. As the high school data cover the whole territory of pre-1920 Hungary, it includes a huge variety of first names of many different ethnic backgrounds. For tractability, we only considered names which had at least 10 occurrences over the study period, and checked one-by-one whether they were male of female given names.

Figure 27 shows the estimated gender barriers from both data sources. The solid line corresponds to the "-y" named women's status disadvantage compared to the "-y" named population, while the dashed line corresponds to all women's status disadvantage compared to all men. The numbers are very similar for both groups and both data sources. Before 1914, the average woman had to have about one standard deviation point larger individual ability (ϵ_{it}) to get into the elite

Figure 27: Female entry barriers to elite occupations (wmn_t^g)



The figures show the gender barrier calculated from medical and high school graduate data. The unit of measurement is in standard deviation units of latent social status. The red lines correspond to 1914, 1919 and 1942.

occupation than a man of similar inherited status. This entry barrier shrank to .5 by the early 1920s, and keeps decreasing ever since. By the 1960s, the entry barrier went close to zero in the medical profession.

To put these numbers into context, in the previous chapter we calculated that in most cases the status advantage of the "-y" named group was between .37 and .61 units above the general population. This means that the entry barrier of women to elite occupations was of similar order of magnitude as the advantage enjoyed by the privileged "-y" named group. Another way to put this into context is to consider the status disadvantage of Roma names. Their calculated latent status went from .3 points below to .6 below the average from 1945 to 2016. This means that the status disadvantage of women in the interwar years is comparable to the disadvantage of people with Roma names after 1945.

Interestingly, the entry barrier for privileged women compared to privileged man is consistently higher than the entry barrier for women in the general population. Social norms and different outside options might explain this in case of the medical graduates. Of course, this does not mean that privileged women enjoyed a less favorable status in absolute terms. On the contrary, the catching up meant catching up to the whole privileged group, meaning, for example, that after 1943 the calculated social status of "-y" named women exceeds that of the

general population, and remains steadily above that in the 1960s.

5 Discussion and conclusion

5.1 Threats to identification posed by changes in the name distribution

Representativeness of the samples

The method we use requires the knowledge of population shares of surname groups, for which we need large, representative data. In two of three data sources this is fulfilled: for 1869 we use a random sample of the census, for present day data we use the list of all names in the population registry. However, for the years between 1895 and 1950 we proxy surname distribution with a sample name list derived from marriage records collected by the Hungarian Association for Family History Research.

We do not know whether this is a truly random sample of the population, so we ask three questions. Is marriage in general a representative sample of the population? Are the data at hand (a sample of marriages) spatially representative? Is the share of the privileged population similar across sub-regions?

Marriage during the study period was a much more widespread institution in Hungary than it is now: around 40% of the population were married, compared to 32% today.[97] The law and economics literature looks at marriage as a long-term contract: "a fundamental reason to marry is to allow for optimal investment in assets peculiar to the relationship, primarily, but not exclusively, children" (Cohen, 2002). Looking it that way, people with more (assets, social status etc.) at stake should be more likely to marry; in the seminal model of Becker the gain from marrying positively depends on income and human capital (Becker, 1973). As a consequence, higher status people could have been overrepresented among the married (though, as we noted, marriage was and remained very widespread among the adult population in the study period). If this was the case, "-y" named people were overrepresented in the marriage data, meaning that we underestimated their relative representation (as relative representation is the share of "-y" names in the elite occupation divided by the population share of "-y" names), and consequently, their social status. However, as we see in Figure 12, the share of "-y" names in 1900 (when it

[97] 40% corresponds to about 70% of the adult (15+ year old) population. Source: 1890, 1900, 1910 and 1941 census, compared to 2011 census of Hungary.

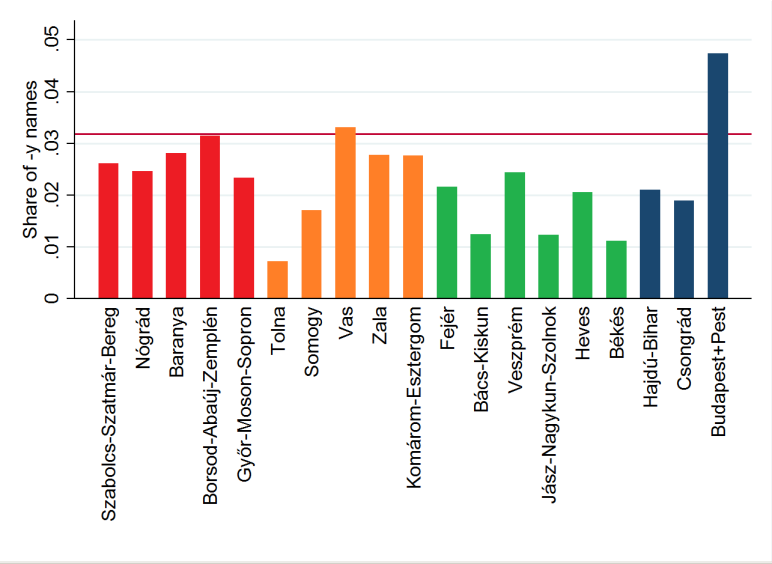
comes from marriage data) is not significantly different from the share of "-y" names in 1869 (when it comes from census data). So this is probably not a huge issue driving the results. In case of Roma names a similar selection bias would mean that actual population shares were higher than suggested by the marriage data (which is plausible), so we overestimated their relative representation, and consequently, their social status. In both cases, marriage data at best caused limited attenuation bias through introducing measurement error. With these caveats, we conclude that using marriage data as a proxy for surname distribution of the population is appropriate. It remains to be seen whether this particular sample of marriages is adequate for the analysis.

To assess this, we checked the number of marriages and the share of "-y" names among the married by county. The dataset has information on the settlement in which the marriage took place, and this was coded to present day counties (or countries, for settlements which were detached in 1920). The present day county system was adopted in 1949, so we looked at county-wise population shares by the 1949 census. We calculated representation ratios for each county (share in marriages / population share in 1949). The sample seems to be not uniformly random across counties, as the capital city of Budapest is overrepresented almost by a factor of two (RR=1.8), while Szabolcs-Szatmár-Bereg county is underrepresented by a factor of twenty (0.052). However, the mean representation ratio is .79 (median is .75), which suggests that besides these two outliers, the sample is quite balanced across counties.

We also calculated the share of "-y" names by county. Again, the capital is where there is a largest share of the privileged group: 4.7%, which compares to an overall average of 3.2% in the population. This is, of course, quite plausible: the "-y" names represent the political and social elites of the era, so they should be overrepresented in the capital. Figure 28 shows the share of "-y" names by present day counties. Observations are ranked by representation ratios, so the leftmost bar corresponds to the county most underrepresented in marriages (Szabolcs-Szatmár-Bereg), while the rightmost is the most overrepresented (Budapest + Pest county). The figure shows that with the exception of Budapest, which is overrepresented in marriages and has the most "-y" names in general, there is no clear correspondence between privileged group share and marriage representation in the data by county. In fact, the correlation between the two is .07 and not significantly different from zero.

We take this as a sign that whatever drives sample selection in marriage records, it is not correlated with the share of "-y" names, so should not cause a bias in our estimates of social

Figure 28: "-y" names by county (1895 to 1950)



The figure shows the share of names ending in "-y" in the marriage record data by present day counties. The horizontal line corresponds to overall average share (3.2%). The colors correspond to representation ratios (RR) of counties in the marriage data (% in marriage records / % in census). Red: RR<.4; Orange: 0.4≤RR<0.8; Green: 0.8≤RR<1.2; Blue: RR≤1.2. Source: Marriage records dataset 1895-1950 (Hungarian Association for Family History Research)

mobility.

Border and population changes

The discussed population changes can potentially affect our estimates of social mobility. In particular, the 1920 border changes and the post-1945 communist takeover could have had an effect on both the size of the elite occupations (high school and medical school graduates, army officers), and the size of the privileged group ("-y" names).

Coverage of the high school graduate data set is affected by the border changes in 1920, as the network of high schools was quite dispersed across the whole historical territory of Hungary. We discussed how we accounted for the trend break in high school graduates in Section 3.4. Our data reflect the facts that the Hungarian medical faculties moved to post-1920 Hungary after the peace was ratified; and that a single institution remained for officer training after the "k.u.k. Army" was dissolved in 1918. In that regard, no further adjustments are needed with the data. However, in all cases access to elite occupations might have changed structurally: consider that the medical faculty was moved from the Transylvanian city of Kolozsvár to Szeged in Southern Hungary. This certainly meant that people in different parts of the country now faced quite different options when considering entering the medical profession.

It is not just the elite access that changes over time, but the underlying population as well.

After the Treaty of Trianon most non-Hungarian speaking areas (and large Hungarian speaking areas as well) were detached from the remainder of the country, seriously impacting its surname distribution. As Figure 12 shows, this did not have a significant impact on the share of "-y" named people in the population. On the other hand, "-y" name share drops drastically after 1945.

These are exactly the reasons why we look at social mobility over time *within* each political regime. Estimating a single rate of mean reversion of social status from 1800 to 2016 would be plagued by all the conceptual and data issues mentioned above. However, if we compare the status of privileged groups yearly from 1920 to 1938, or from 1867 to 1914, these issues do not arise. Then the mean reversion rates are estimated under relatively stable geographic, economic and social conditions, and the changes that do occur are the ones we are interested in.

The impacts of border and population changes thus do not appear in our mobility estimates, but they do appear next to them: they are reflected in the slope shifts in Figure 22. For example, the overrepresentation of "-y" named people jumps discretely after 1945. This is a mechanic consequence of the drastic expansion of high school education after 1945, and the decrease in the share of people with a "-y" name. The interesting part is how quickly it then converges to the mean. In the same figure there is also a discrete jump in the latent status of "-y" named people among officers and medical doctors, but this jump in the slope can be thought of as the impact of the compositional changes discussed previously. The interesting thing is the rate of change of social status afterwards, which is still positive in both cases.

Name changing

Our method relies on the assumption that family names are a proxy of patrilineal inheritance of social status. Name changing would undermine our results if there would have been widespread adoption of privileged family names. To do this, we checked if the historical accounts telling that "-y" names enjoyed protected status are accurate.

We looked at the universe of name changing records between 1867 and 1932 (see Section 2.6 of Chapter 3 for description of the data set). It turns out that in no year of the Austria-Hungary period did the number of people who adopted a "-y" ending surname exceed 50; in most cases it is between 1-5 yearly, consistent with the occasional adoption, divorce or ennoblement of certain individuals.[98] Between 1920 and 1932 there is some increase in the number of cases when "-y"

[98]The range is 0 to 48 cases yearly, the mean is 8.32, the median is 2.

names are adopted through name changing, consistently with the creation of the Order of Vitéz to prop up traditional elites.[99] As Figure 13 shows, this was not enought the stop the shrinkage of the privileged groups in the interwar years.

On the other hand, there is anecdotal evidence that elite families started using "-i" endings after World War II, two distance themselves from the Horthy era during the communist dictatorship. This is indeed consistent with the decline in the "-y" share of the population which we see in 12 after World War II. This is not particularly worrisome for our analysis. The change in the population share of "-y" names would create a slope shift at the regime change (which we do see, mostly with high school graduates in Figure 22). If the percentage of people who start use the "-i" ending is the same in the "-y" named group at large and in their representatives in the elite occupation, this effect is canceled out when calculating relative representation. If this switch is more likely for those who do make it to the elite, then we underestimate the privileged group's relative representation, and consequently, their relative status, and with that *overestimate* social mobility. This would make our main result (that communism is not particularly effective in reducing the status of traditional elites) even stronger.

5.2 Conclusion

We measure inter-generational social mobility rates at the group level in Hungary for as long as 240 years (8 generations) using unique registry data on elite occupations and general population. We observe almost all medical graduates between 1770 and 2016, professional army officers between 1870 and 1944, and a large sample of high school graduates. We analyze how traditionally privileged and underclass groups were represented in the elite occupations over time and under different political regimes,.

Using a simple model of status transmission we estimate the rate at which the realized status of subsequent cohorts of groups at the top and the bottom regress (or not) to the social mean. Our results are robust to various definitions of elite occupation. We find that the rates of status persistence were the lowest during the liberal regimes. On the other hand, the authoritarian regimes negatively affected social mobility. The results on social mobility during communism are mixed. Mobility is higher compared to conservative regimes, and only higher than in democracy

[99] The yearly mean is 105.14, the median is 92.5. We hand-checked some name changers from this period to check if they were members of the Order; the ones we checked were without exception. As the population of Hungary was around 8.000.000 in the 1920s (see Table 1 of Chapter 3) , this means that the average yeary name changing driven increase of the "-y" group was about 0.0013% of the population, and 0.05% of the size of the whole "-y" group.

when it could expand on a massive scale the access to previously elite "occupations" (such as high school graduates). Gender barriers to elite occupations decreased over the study period, and women of privileged background had, by the middle of the 20th century, similar access to the elite as the whole privileged class in general. Last but not least, underclass groups do not benefit from increased social mobility under democracy. Their relative underrepresentation in the elite occupations worsened over time compared to communism.

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Chapter III

"Deny Thy Father and Refuse Thy Name" - The Economic Incentives of Name Changing

with Rita Pető

1 Introduction

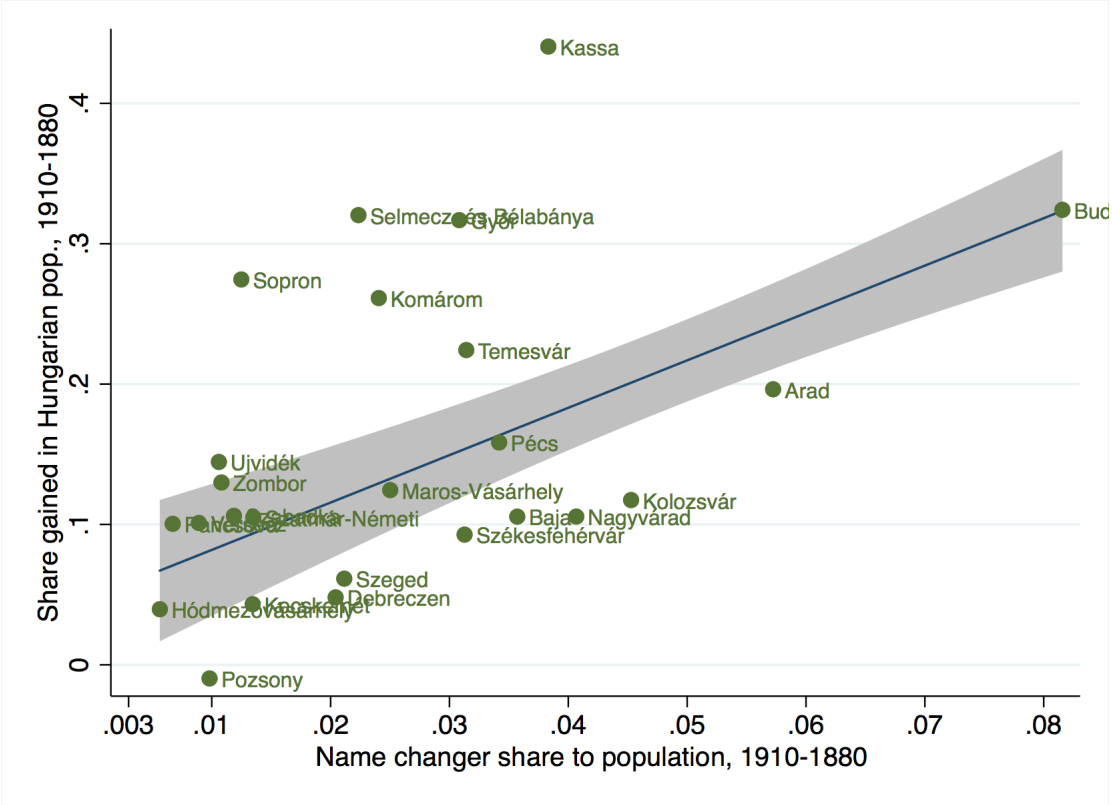
Economists are increasingly convinced that much of the observed variation in economic development can be attributed to cultural traits (Guiso et al., 2006), and there is a growing body of evidence showing that these are transmitted very persistently from one generation to the next: there are results showing that gender roles are persistent (Alesina et al., 2013); (in)tolerance is persistent (Becker et al., 2016b; Jha, 2013); religion is persistent, but so is its effect on economic decisions (Nunn, 2010; Benjamin et al., 2010); susceptibility to corruption is also persistent (Becker et al., 2016a), so is trust (Nunn and Wantchekon, 2011), and the list goes on.

We argue in this paper that a somewhat contradictory statement is also true: short-run economic incentives can have a lasting cultural impact. Using unique, individual level data from Hungary on surname changes that happened between 1867 and 1932, we show that an important element of someone's identity, the family name, responds very rapidly to economic incentives set by the government. We study the extensive and intensive margin name changing responses of different minorities to changing economic incentives during the liberal Austria-Hungary period, and the authoritarian period after World War I.

During the era of the Dual Monarchy of Austria-Hungary, the Hungarian government actively pursued nation building policies, and intended to transform a multi-lingual country into a Hungarian speaking nation state. One important step in the assimilation process was changing the family name from one that had a distinct ethnic character into a Hungarian sounding one. Figure 29 shows that name changing is closely correlated with broader aspects of assimilation: major towns that had more name changing over the three decades between 1880 and 1910 also had much larger increases in the share of population that reported Hungarian as their mother tongue. While the figure should not be interpreted as causal, it shows that name changing should be interpreted as part of a broader acculturation process. After losing World War I, the elite in

the interwar period followed (and revved up) the homogenizing practices of their predecessors, but this time in an asymmetric way, as they then encouraged Christian minorities to assimilate and discouraged Jewish minorities from doing so.

Figure 29: Name changing and the share of Hungarians



Horizontal axis: all individual name changes between 1880 and 1910 as a percentage of the 1910 population in the top 25 Hungarian towns. Vertical axis: difference between share of Hungarian speakers in 1910 and in 1880 in Top 25 towns. The line represents a linear regression of the vertical axis variable on the horizontal axis variable. Source: Farkas and Kovesdi (2015) data set on name changing (see Section 2.6 for detailed description), and census data. 95% confidence bands included.

First, we formulate how rational self-selection into assimilation would work based on the canonical economic self-selection model of Borjas (1987). In our framework people in a minority make a decision by weighing the utility loss from severing ties with their former communities against potentially lucrative job opportunities in the majority population. This is similar to the notion of Austen-Smith and Fryer Jr (2005) that individuals need to consider labor market performance and peer rejection simultaneously. The state can implement policies that push for adopting a Hungarian family name. Such assimilatory policies have a persistent effect in our model because they put in motion dynamic incentives. The psychological cost of leaving one's minority group (the pull effect of community) is proportional to community size, so if today many people decide to assimilate for any reason, it will be less costly for the next cohort tomorrow.

We derive comparative statics of the model, and show that they are consistent with patterns

in name changing around two major policy shifts: one is a one-off assimilation campaign in the public sector in 1898, the second is the economic exclusion of Jews starting in 1920. We consider both the extensive margin (how many people change their name) and the intensive (the skill composition of the name changing cohort over time).

We then estimate how big the economic incentives set by the government needed to be to explain the observed name changing figures. We do this in order to be able to calculate counterfactuals for alternative government policy regimes. By comparing counterfactuals we are able to decompose what share of observed assimilation is attributable to pre-existing community ties; to government policies directly affecting assimilation; and to previous policies that create persistent effects through dynamic incentives. We find that 5% to 10% of all Jewish name changing after 1898 was attributable to the one-year "Hungarianization" campaign of 1898.

The paper contributes to the growing literature on the economics of identity (Akerlof and Kranton, 2000), in particular, the papers that show that identity is endogenous. Cassan (2015) shows evidence for large-scale caste identity manipulation in India, while Atkin et al. (2019) study identity choice through observance of food consumption taboos in the same country. In the former paper the choice is about a dimension of identity which is largely artificial (whether or not one belongs to the "agricultural castes", which were created few years before). In the latter paper it is not the choice of group which is endogenous, but the extent of which members of the group conform to the constraints imposed by the group (Hindus do not become Muslims, but start to abstain from beef and eat more pork when Hindu identity becomes salient). Jia and Persson (2017) show that mixed-ethnicity Chinese couples consider economic incentives and social costs when they choose the ethnic identity of their child. The most closely related paper to ours is perhaps Algan et al. (2013), who study the strategic first name choice of parents in France and calculate the utility from having a child with an Arabic first name. Our paper differs from these in that in our case individuals made a decision about their own group adherence (though it affected their offspring as well), and considered their own lifetime utilities when making the decision on the family name.

Another closely related paper from this literature is the historical study of Botticini and Eckstein (2007), who argue that economic factors drove the size and the geographical distribution of Jewish communities over the last two millennia. Observing Jewish rites required a substantial amount of investment into human capital (i.e. the ability to read and study the Scripture), which could not be made by most subsistence farming communities, so they converted (mostly

to Christianity). Their results suggest that during much of pre-modern history economic selection of Jews into assimilation went in the exact opposite direction than in the time and context that we study.

We study identity choice through the choice of family names. Names have been studied extensively by economists, as they (both family and first names) are a core element of identity (Fryer Jr and Levitt, 2004; Cook et al., 2014), and people with ethnic names often face discrimination (Arai et al., 2008; Edelman et al., 2017). The economic impact of name changing have previously been studied within the context of immigration. Arai and Thoursie (2009) investigate the impact of immigrant family name changing on wages in Sweden; first name choice of immigrants in America also was also strategic and had economic impact (Carneiro et al., 2001; Biavaschi et al., 2013). Our study is different in two important regards from the existing literature. We consider family names, which are important markers of cultural heritage and national identity, but can also be markers of status (Clark, 2014). Also, the people we study are members of minority communities that exist because of previous immigration, but they are not immigrants themselves. These are not people who decided to uproot themselves and start a new life on another continent; they come from communities that had deep, established roots in Hungary, and went on to live in the same country - as different people.

Our paper also provides a potentially unique setting to study discrimination. Previous literature considers discrimination on traits that are close to immutable and are not likely to be changed to avoid discrimination (such as race and sex).[100] In our study, this is not the case, as members of the potentially discriminated groups are all white European males, so name change and acculturation made them observationally identical to members of the majority. Indeed, this was the goal of the political elite at the time. Another interesting facet of our setting from a discrimination perspective is that our results are consistent with protracted, purely taste based discrimination of minorities. With this we add to the large literature that criticizes Becker's (2010) prediction that taste-based discrimination should not prevail in equilibrium.[101]

The paper also contributes to the literature on nation building (Alesina et al., 2000; Alesina and Reich, 2013; Alesina et al., 2017; Bandiera et al., 2018), as we show that not just cultural policies and education (as in Alesina and Reich, 2013 and Bandiera et al., 2018) or shared

[100] Exceptions do exist: evidence shows that race can be endogenous on the margin and respond to economic incentives (specifically, choosing between being categorized as "black" or "mulatto" was endogenous historically in the US, see Bodenhorn and Ruebeck, 2003).

[101] For an early critique, see Arrow (1972), more recent papers include (but are not limited to) Moser (2012), Guryan and Charles (2013), Marom et al. (2016).

experiences (as in Depetris-Chauvin et al., 2018) can have a role in creating more homogenous nations, but labor market incentives as well.

2 Background and data

2.1 A multiethnic country

We study name changes that took place in Hungary during the Austria-Hungary period (1867-1918) and the first years of the right-wing authoritarian regime that followed it (1919-1932). Long before that, by the end of the 18th century, Hungary had become a diverse, multi-ethnic kingdom within the hodgepodge of fiefdoms, counties and principalities that constituted the Habsburg Empire. Only about 40% of the population of Hungary was speaking Hungarian as a mother tongue; the largest linguistic minorities were Germans, Slovaks, Romanians, Croats, Serbs and Ruthenians (Fenyvesi, 1998;Karády, 2001b). The ethnic landscape of the country was the product of almost two centuries of wars and Turkish occupation from the early 16th to the late 17th century, which caused massive population loss and left large swathes of land uninhabited. Feudal rulers seeked to repopulate these areas from other parts of Europe (with Germans and Jews); spontaneous immigration (of mostly Slavic people) happenned from neighboring areas (Fenyvesi, 1998).

The country was equally diverse in terms of religion. Most Hungarians (Magyars) were either Catholic or Reformed Protestants, with a majority of the latter at that time (Karady, 1990). The arriving Croats, Slovaks and Germans were mostly Catholic. Romanians, Serbs and Ruthenians belonged to Eastern Christian denominations (Fenyvesi, 1998). A smaller community was that of Lutherans, who were almost exclusively (78%) Germans and Slovaks (Karády, 2001b). Jews were another important minority who were present in Hungary since the Middle Ages, and after larger influx in the early 19th century their share increased to 4% by the 1860s (Katz, 1990). This influx came both from western parts of the Habsburg Empire, and also from Eastern Europe. The two subpopulation retained some distinctive features, such as their names, as Jews had to adopt German family names in the Habsburg Empire (e.g. Roth, Weiss), while Jews in the 19th century who came from Eastern Europe were more likely to have traditional Jewish names (such as Kohn, Baruch etc.; see Karády, 2001a). Contemporary elites saw immigration, and Jewish immigration in particular as positive, as they were able to fill roles in the service sector that the

largely agrarian population could not (Karády, 2000, 2001c).[102]

The ruling class of the society was much less diverse, as Magyars retained a firm majority share in the nobility. This was not the case among the peasantry and the bourgeoisie. Because of this asymmetry, when modern nationalism appeared in Hungary in the 19th century, it took a top-down character from the very beginning (Karady, 2012). The elite perception was that the country needed to be modernized, and its population "Hungarianized".[103]

2.2 Nation building in the modern era

From the late 19th century, the fundamental goal of Hungarian nationalists was to assimilate and acculturate minorities.[104] The Hungarian elite thought of assimilation as a sort of "social contract". Accepting it meant

> "accepting the hegemony of the Hungarian elite while receiving a share of it; the condition of which was not just internalizing the idea of a Hungarian nation state, but Hungarianizing self-representation, public behavior, social memory through education, and surrounding private and public spaces." Karády (2001b)

How ambitious was this goal? To gauge this, we need to consider how many people might have been targeted in the first place. Column 1 of Table 13 shows the composition of the population by religion and mother tongue according to the 1881 census. In terms of mother tongue, Hungarian speakers actually only constituted a relative majority in 1881 with their 44.92% population share (Column 1 in 13); though their majority was more comfortable in urban centers (59.32%, Column 3). Besides linguistic minorities, one group that was always at the center of assimilatory pressure was the Hungarian Jewry. By this time, only about 4.54% of the overall population were Jews (in contemporary statistical jargon: "Israelites"), but they were highly urban, so they made up

[102] They were recognized as full citizens as early as 1849 (during the revolution), and they had this status upheld in 1867 by the new Parliament after the Compromise with Austria. In 1895, Judaism was recognized as one of the established religions. The Hungarian Jewry was in a uniqe position, as their population share was closer to the respecitve Jewish population shares in Eastern Europe where they were still often persecuted, while their legal status more closely reflected countries of Western Europe, where Jewish minorites were around 1%, but enjoyed full right as citizens very early on (Karády, 2000).

[103] The English literature uses the exonym "Hungarian" and the endonym "Magyar" interchangeably. Consequently, the verb with the meaning "to turn someone or something into Hungarian/Magyar" can either be "Hungarianize" or "Magyarize". We use both forms of the adjective with exactly the same meaning, but prefer the first version of the verb ("Hungarianize") for subjective reasons.

[104] In this ideology they built on earlier historical examples, when Hungary succesfully assimilated peoples like the Cumans or the Iasi in the 13th century, following the demographic and economic collapse caused by the Mongol invasion; and the Pechenegs even earlier than that (Fenyvesi, 1998).

11.41% of the population of the 25 major towns (Column 3), and as many as 19.65% of the population of Budapest, the capital.[105]

The classical liberal, nationalist Hungarian elite in the late 1800s sought to assimilate minorities gradually using the power of positive incentives, elimination of legal and economic barriers, such as drastically cutting legal fees of applying for family name change in 1881 (Karády, 2001b).[106]

Subsequent governments were, however, not equally liberal. This is especially true for the premiership of Dezső Bánffy (1895-1899), which coincided with the high tide of nationalist sentiments around the 1000th anniversary of the Hungarian state in 1896 (Kozma, 1997). Bánffy's position was that no cultural or linguistic autonomy was to be made possible for ethnic minorities in Hungary, and all citizens would have to become Hungarians through assimilation (Bánffy, 1903).

Assimilation meant different things for different minorities. In the case of linguistic or national minorities the elite's goal was to increase the share of Hungarian speakers, on one hand by increasing the official role the Hungarian language played in public life, on the other hand through teaching the language to more and more people.[107]

Adopting the language of the country they lived in has been common practice for Jews literally for millennia (Spolsky, 1985), so in their case self-representation and public behavior were the major dimension of assimilation.[108] The common denominator of policies aimed at ethnic and religious minorities of foreign origin was the goal to "forge" new people who identify culturally as Hungarian. The way one could signal this identification was to accommodate one's most important personal and legal identifier with the Hungarian community, the family name.

[105]The term "Israelite" technically refers to the Twelve Tribes in the Hebrew Bible. The anachronistic use of the biblical term was partly "courtesy" (calling them Jews, an "ethnic" name, would have been considered an anti-Semitic gesture), partly strategic (Jews who spoke Hungarian would count as Hungarians at the census, increasing leverage over other linguistic minorities). See Karády (2015) and Footnote 108 for further discussion. We stick to this terminology for convenience.

[106]In Figure 30 we show name changing case counts in our data, which are identical to the aggregate numbers in Karády and Kozma (2002b). They clearly reflect a sharp increase in 1881 as a consequence of this policy change.

[107]When civil registry of births was introduced in 1895, it became mandatory to choose a Hungarian first name for children regardless of the ethnicity of the baby (so a German baby had to be officially named "József" instead of "Joseph"); the "ethnic version" of the name could only follow the "official" name in parentheses. After 1907, knowledge of the Hungarian language was required from every worker of the public rail road company. The Education Bill of 1907 required all schools to teach the Hungarian language, regardless of ownership status (Karády, 2001b; Héjj and Olszewski, 2015).

[108]Conversion to Christianity only became a freqent complement to name changing for Jews in the late 1930s, as a strategy to escape persecution (Karády, 1997). The elites during the liberal zeitgeist of the Austria-Hungary period did not propose or enforce religious conversion, and it indeed remain sporadic. Assimilated Jews were officially referred to as "Hungarians of the Mosaic faith", or "of the Israelite confession", to avoid suggesting that they somehow still represented a different group altogether (as anti-Semites had suggested), rather than being just another "religious branch" of Hungarians (Karády, 2015).

Table 13: Descriptive statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	<1920				>=1920			
	Hungary		Top 25 Towns		Hungary		Top 10 Towns	
	Pop. (1881)	Changers	Pop. (1881)	Changers	Pop. (1920)	Changers	Pop. (1920)	Changers
N	13.728.622	136.263	1.125.016	63.152	7.980.143	13.571	1.483.096	4.974
Age	n.a.	20.8	n.a.	21.38	n.a.	28.18	n.a.	29.93
Religion								
Share, Israelite	4.54%	52.34%	11.41%	62.45%	6.2%	5.98%	17.07%	10.83%
Share Catholic	47.21%	35.31%	61.34%	29.29%	63.5%	72.75%	60.64%	71.17%
Share Lutheran	8.07%	4.34%	4.86%	3.05%	6.4%	9.07%	4.49%	7.67%
Share Calvinist/Reformed	14.78%	3.11%	14.40%	2.22%	21.4%	6.24%	15.91%	5.28%
Greek (Cath./Orth.)	24.94%	3.60%	6.51%	2.28%	3%	4.03%	1.44%	3.16%
other/n.a.	0.46%	1.30%	n.a.	0.73%	0.1%	1.94%	n.a.	1.89%
Mother tongue								
German	13.10%	n.a.	21.46%	n.a.	6.9%	n.a.	6.06%	n.a.
Slovaks	13.04%	n.a.	4.92%	n.a.	1.8%	n.a.	1.02%	n.a.
Romanians	16.93%	n.a.	1.65%	n.a.	0.8%	n.a.	0.15%	n.a.
Ruthenians	2.49%	n.a.	0.05%	n.a.	-	n.a.	-	n.a.
Croats	4.41%	n.a.	0.21%	n.a.	0.5%	n.a.	0.43%	n.a.
Serbs		n.a.	6.56%	n.a.	0.2%	n.a.	0.11%	n.a.
Hungarian	44.92%	na.	59.32%	n.a.	89.6%	n.a	91.33%	n.a

Column 1: Population of Hungary by religion and mother tongue in 1881. Column 2: Population of all name changers before 1920 by religion. Column 3: Population of Top 25 towns by religion and mother tongue in 1881. Column 4: Population of all name changers in Top 25 towns before 1920. Column 5-8: same information for the time period between 1920 and 1932, with Top 10 towns instead of Top 25.

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2.3 History of family name changing in Hungary

Changing a family name that suggests an "alien" cultural background to one that sounds properly "Hungarian" was one of the most important steps in the assimilation process, and the one which was shared by religious and linguistic minorities. Changing name was strongly correlated with language change, as places where name changing were widespread were also the parts of the country where the share of people who listed Hungarian as their first language increased most rapidly (Katus, 1989; Karády and Kozma, 2002b).

From a legal and political point of view, the importance of the act is revealed by its highly regulated nature. Even from the earliest period of legal name Hungarianization (early 19th century) there are surviving official "cease and desist" orders, that forbade certain individuals from spontaneous Hungarianization (i.e. the use of a Hungarian surname for a person whose name was legally different, see Hornyánszky, 1895).

Why would anyone decide to shed one's inherited family name? For public servants, Hungarianizing a family name could have had immediate and very tangible effect on one's career. However, there was a general feeling that one could not have moved beyond a certain level on the social ladder if one had not demonstratively assimilated. As a famous writer of the time put it:

> "You can only be a Serb or a German if you are [owning] under 500 acres [of land]; over that you have to become Hungarian, if you want to live a life worthy to your wealth." - Ferenc Herczeg, quoted by Karady (2012)

This highlights that choosing a Hungarian sounding name was considered a pledge of allegiance, and a strategic action as well. Despite all the sticks and carrots, name Hungarianization did not become extremely widespread during the era of the Austro-Hungarian Empire. An estimate (Karády, 2001b) puts the share of name changers in these fifty years at just 1% of the potentially relevant population, which shows that the decision remained voluntary, and changers were a very selected population.

Figure 30 shows the number of name changing events from the Compromise between Austria and Hungary in 1867 until 1932. Before the Compromise of 1867, applications for family name changing were to be submitted to a local judge, but the cases were eventually determined by the

imperial central government. After the sovereignty of Hungary was partially restored in 1867, name changing matters were delegated to the Ministry of the Interior of Hungary; the minister had full discretion over determining the course of every application (see Karády and Kozma, 2002b, p. 38). Until 1880 the price of the application procedure was 5 forints, which was quite substantial. The median yearly salary in our (positively selected) sample from the time was 800 forints before 1880. This means that the price of name changing amounted to almost a week worth of elite salary, with an uncertain outcome (see 2.8 for the description of salary data). The burden must have been much higher on a more typical person. This changed in 1881, when the legislature approved of a 10-fold decrease of the procedural cost of name changing to 50 krajcárs (or .5 forints, see Karády and Kozma, 2002b, p. 55). Name changing started in earnest after that, gained additional momentum at the turn of the century, only to became less frequent after World War I.

Which minority group did name changers belong to originally? As ethnicity, national identity or language was not recorded at name changing, we can only infer these indirectly, through religion. Data on the religious composition of name changers show that the most overrepresented group in name changing was the Hungarian Jewry (Karády and Kozma, 2002b; Karády, 2001b). Figure 31 shows that except for the year 1898, from 1881 to 1920 Jews were the single biggest religious group among name changers every year. This is despite that their share in the population was only around 4% to 6%.

The Catholic population (the largest denomination in Hungary) was the second most prominent group among name changers during the period, except for 1898, the single year when it actually topped the list. Hungarians, Slovaks, Croats and Germans were all represented among Catholics in large numbers, so just by religion their ethnic background cannot be inferred. However, 9 out of the 10 most frequent changed family names among Catholic name changers are German.[109] There is 30% overlap between the Top10 Jewish and the Top10 Catholic names among name changers, indicating that there might have been people who converted before name changing.[110]

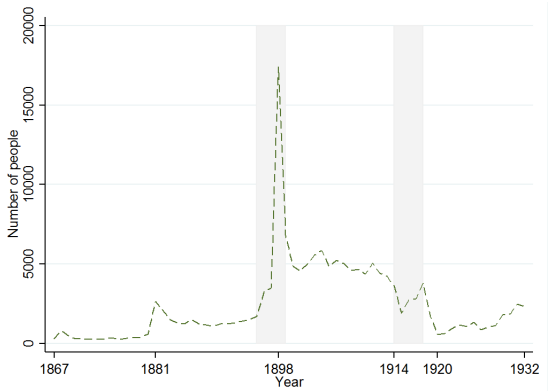
Other Christian denominations (Panel B of Figure 31) are much less frequent among name changers. Surname data from name changing suggest that Lutherans had mostly German and Slovak background (9 to 1 among the Top 10 surnames), while following Eastern rites was as

[109] Weisz, Klein, Fischer, Müller, Schmidt, Krausz, Mayer, Steiner, Braun with Pollák as the only exception.

[110] Weisz, Klein and Pollák are the intersection of the two sets. Pollák is especially hinting at conversion, as it is the traditional surname of Jews who originally came from Poland.

indicator of Romanian or Slavic ethnic origin (again 9 to 1). Reformed (Calvinist) Protestants, the second largest denomination in Hungary, are quite underrepresented among name changers, as this community was overwhelmingly Hungarian by language and ethnicity. The most common names among Calvinist name changers are frequent German names (3 out of the Top 10), others were probably changed for idiosyncratic reasons.[111]

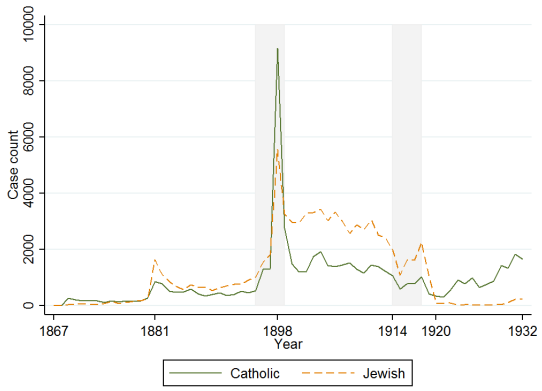
Figure 30: Name changes over time



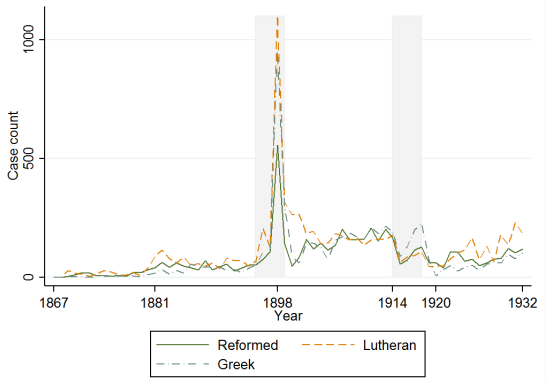
Source: Farkas and Kovesdi (2015) data set (see Section 2.6 for detailed description). The first shaded area corresponds to the Bánffy cabinet; the second shaded area corresponds to World War I, after which the right-wing regime was established.

Figure 31: Religious composition of name changers

Panel A: Catholics and Jews



Panel B: Protestants and Eastern Christians



Source: Farkas and Kovesdi (2015) data set (see Section 2.6 for detailed description). The first shaded area corresponds to the Bánffy cabinet; the second shaded area corresponds to World War I, after which the right-wing regime was established.

[111] Klein, Schwarz and Weisz are German names among the Top 10. An example for idiosyncratic motivation is that of the 31 people who changed their names from "Mocskos", which means "filthy" in Hungarian. Others examples are "Horváth" and "Tóth", which respectively mean "Croat" and "Slovak" in Hungarian, but are actually very common Hungarian surnames, so their bearers' motivation could have been to increase the distinctiveness of their family names.

2.4 Policy shock of 1898

An important episode of name changing policies during the Dual Monarchy was the name Hungarianization campaign of 1898. We rely heavily on this throughout the paper. At the end of the year of 1897, the nationalist Bánffy administration started a coordinated campaign to surge surname changing figures (Karády and Kozma, 2002b). Prime Minister Bánffy, as the leader of a supposedly liberal regime, could not directly force any free citizen into assimilation. The cabinet instead decided to pressure those, whose livelihood they directly controlled - public employees.

Civil servants were instructed to give extra attention to applications in order to avoid backlogs in the administrative process of name changing. Meanwhile, propaganda leaflets on name Hungarianization were circulated among the heads of the regional public administration along with a letter from the prime minister highlighting the exemplary role of public officials in patriotic issues, such as name Hungarianization (Nagy, 1992; Karády and Kozma, 2002b). Unofficially, in many cases public servants and workers at public companies (such as the railways) were given a choice between Hungarianizing their family name or losing their job, with the pressure coming from as high as the ministerial level (Karady, 2012; Karády and Kozma, 2002b).

The campaign did not go unnoticed. Perhaps the harshest critics of the measure were representatives of the German minority, who strongly spoke out against the never-publicly-acknowledged campaign in parliament. The government had to back down eventually, and Bánffy left office in February 1899 (for unrelated reasons).[112] Nevertheless, the campaign was successful: as Figure 30 attests, the number of name changers in 1898 was roughly six time as high as the year before, and three times as high as the year after.

2.5 Exclusive nationalism after World War I

Trends in name changing turned after Hungary lost World War I, and as a consequence, two-thirds of its existing territory. This was a major national trauma, which defined Hungarian politics in the next 25 years, and beyond. However, the Treaty of Trianon created the Hungarian nation state that nationalists had been yearning for, as the remaining parts of the country were overwhelmingly Hungarian (around 90% had Hungarian as a mother tongue). The largest remaining ethnic minorities were Germans (5%), Slovaks (1%); the others were no more than 4% combined (See Kozma, 1997, and also Column 5 of Table 13). Jews (as a religious minority)

[112] The government was unable to renegotiate the budget agreement with Austria.

were about 6% of the population.

The Christian-conservative, increasingly authoritarian regime now took an asymmetric stance towards assimilation. While it increasingly pressured for Hungarianization of ethnic majorities in general, it also increasingly discriminated against Jews, whom the elite largely blamed for the demise of the historical Kingdom of Hungary. Jewish assimilation from the 1920s was seen as something that needed to be stopped, not facilitated.

Legal discrimination against Jews started with the 1920 "numerus clausus" (closed number) law, which restricted the number of Jewish students that could enter higher education. As this law defined Jews on a religious basis, name changing would not have helped avoiding its jurisdiction. Jews were also increasingly discriminated against at the workplace, mostly in the public sector, and this time on a "racial" basis, regardless of their assimilation status (whether they changed name or converted). This racial turn meant that the aspire to assimilate could yield no benefit to the individual; but name changing was not prohibited for Jews just as yet.

The first evidence for denying name changing requests from Jews comes from 1935 (Kozma, 1997), and assimilation was made essentially illegal by the first Anti-Jewish Law in 1938. Nevertheless, the number of Jewish name changers dropped close to zero as early as 1920, and remained minuscule for the rest of the period (Karády and Kozma, 2002b). This was not the case for other groups of the population. Following a general backdrop in the early 1920s, a combination of incentives, pressure at the workplace and public campaigns, the yearly number of name changers increased again and reached hitherto unseen levels in the mid 1930s (Kozma, 1997).

2.6 Data on name changes

The primary data source we use in this paper is an individual level data set on legal ministerial decrees on name changes that took place between 1867 and 1932, compiled by researchers at the Hungarian Society for Family History Research (Farkas and Kovesdi, 2015). These are all legal name changes that took place and survived from the time period, and the coverage is very close to complete.[113]

There are 150 thousand records in this data set, each contains data on a single individual who changed his or her family name. The variables in the data set are the following: the dropped

[113]Information on name changes were made public back then in official gazettes and in collections such as Hornyánszky (1895). Though the association is also collecting information from the period of 1933 to 1955, information is more scarce as World War II took its toll on the data sources (Karády, 2001b). Also, the harsh anti-assimilation stance against Jews and the increasingly agressive Hungarianization of others from 1930s make it doubtful that people had much room for optimizing behavior in this regard after 1933.

and the adopted family name; the first name; occupation; year of birth; place of birth; place of residence; year of birth; age; religion; official permit ID; number of the gazette in which the decision was made public. Religion is known for 98.8% of the name changers.

Column 2 of Table 13 highlights the religious composition of name changers during the Austria-Hungary period; Column 6 shows the same between 1920 and 1932. Column 3 shows that Jews were more than 10 times overrepresented among name changers during the years of the Dual Monarchy. All other groups participated at rates far below their population shares. The second and the third biggest religious denomination among name changers are Catholics and Lutheran Protestants, respectively; these are the religions of the second and the third biggest linguistic minorities: Germans and Slovaks.

The picture changes completely after 1920. The striking difference is that, for the first time in the history of name changes, Jews are no longer the most prominent religious group among name changers; though still overrepresented compared to their population shares, they came out a distant second behind Catholics. This is made even more striking by the fact that most potential non-Jewish name changers are not even present in the population anymore, as the Treaty of Trianon made the country overwhelmingly Hungarian speaking at the stroke of a pen, by detaching areas that were home to most linguistic minorities.

2.7 Population data and the study sample

As we study selection into name changing, we need demographic data on communities that might be a subject to this selection mechanism. These come from the decennial censuses conducted between 1881 and 1930. These sources provide settlement level data on population, linguistic and religious composition, and literacy.

Data coverage varies over time; it is most consistently available for the 25 most important towns, including the capital, Budapest.[114] For this reason, in this current draft we only analyze name changing in these 25 towns. Table 13 shows the population composition and the name changer composition in these towns both before 1920 (Columns 3-4) and after (Columns 7-8).

These towns ("Top 25 towns" and "Top 10 towns", thereafter) represent a large share of the population (8% in 1890, and 19% in 1920), and an even larger share of all name changing

[114]These are called "towns with municipal authority" ("törvényhatósági jogú város") and include the following towns: Arad, Baja (*), Budapest (*), Debrecen (*), Győr (*), Hódmezővásárhely (*), Kassa, Kecskemét (*), Kolozsvár, Komárom, Marosvásárhely, Nagyvárad, Pancsova, Pozsony, Pécs (*), Selmec- és Bélabánya, Sopron (*), Szabadka, Szatmárnémeti, Szeged (*), Székesfehérvár (*), Temesvár, Ujvidék, Versecz, Zombor. Stars indicate the 10 towns which were still in Hungary after 1920.

that took place (46% in the Dual Monarchy years, and 36% during the right-wing authoritarian regime). They have a higher share of Israelite population and Israelite name changer population in both periods. Also, they have negligible share of non-German linguistic minorities, with the exception of some towns that were home to a sizeable Serb, Slovak and Romanian population, which were all lost in 1920.

Ideally, we would want to know how many of linguistic or national minorities lived in each of the Top 25 towns, and what share of them changed names in which year. Unfortunately, we do not observe this directly. The first problem is that the census was only conducted in every decade. We get around this problem by interpolating between census years linearly, except between 1910 and 1920. In these years we assume that yearly population growth rates for each linguistic and religious group in this decade was the same as the decade before, and a trend break occurred in 1920 with the new data point. This seems more realistic knowing the amount of internal and external migration that happened when the new borders were delineated.

The second problem is more subtle. The issue is that name changing data features religion, but not ethnic or linguistic background. The consequence is that we know perfectly well the share of the Jewish minority, but no so much the share of Germans, Slovaks etc. However, as we noted earlier, language and religion does correlate even for Christians, and we can use this to proxy for their ethno-linguistic backgrounds.

Panel A of Table 14 shows for every major religious denomination the share of people by mother tongue from the 1881 census. The table shows that even though religion does not tell precisely the linguistic background of a person, it does rule out some backgrounds pretty efficiently. Conditioning on this information we can be pretty sure that a Catholic name changer is not Romanian by language; that Orthodox name changers are not Germans, and so on. In fact, if we further assume that all name changers have a distinct, non-Hungarian background,[115] then knowing the religion tells with great certainty which of two groups the person belongs to.

A non-Hungarian Catholic or a non-Hungarian Lutheran are both most likely to be either German, Slovak and Croat; rather than Romanian, Ruthenian or Serb.[116] On the other hand, Greek Catholics and Greek Orthodox are both much more likely to be either Romanian, Ruthenian or Serb, than German, Slovak or Croat. Panel B of Table 14 shows the collapsed probabilities.

With that in mind, we can further collapse Christian denominations into Eastern Christians

[115] which is very frequently, but not always the case, as we saw previously, see Footnote 111.

[116] The 1881 census treats Serbs and Croats as a single language groups; this is not true in later editions. These confirm that the former are overwhelmingly Greek Orthodox, the latter are Roman Catholic.

Table 14: Language and religion

Panel A: Language share by religion

	% Hungarian	% German	% Slovak	% Romanian	% Ruthenian	% Serb/Croat	% Other
Catholic	44.29%	15.95%	15.79%	0.09%	0.02%	18.44%	5.42%
Greek Cath.	9.04%	0.07%	6.56%	56.69%	22.18%	0.67%	4.82%
Greek Orth.	0.74%	0.27%	0.06%	59.86%	0.08%	35.37%	2.72%
Lutheran	22.31%	34%	38.08%	0.10%	0%	0.07%	5.44%
Calvinist	94.22%	1.20%	0.51%	0.09%	0%	0.04%	3.84%
Israelite	55.84%	33.74%	3.80%	1.28%	1.62%	0.70%	4.02%

Panel B: Share of language groups by religion

	% Hungarian	% German, Slovak, Croat (Western Christian)	% Romanian, Ruthenian, Serb (Eastern Christian)
Catholic	44.29%	50.18%	0.11%
Greek Cath.	9.04%	6.63%	79.54%
Greek Orth.	0.74%	0.33%	95.31%
Lutheran	22.31%	72.15%	0.10%
Calvinist	94.22%	1.75%	0.09%
Israelite	55.84%	37.19%	3.26%

Panel A shows the language composition of major religions. Panel B shows the same data collapsed to ethnic groups which were overrepresented in the same religions. Source: 1881 Census

(Greek Catholic and Orthodox) and Western Christians (Catholic, Lutheran or Calvinist).[117] Though there is no one-to-one mapping between religion and linguistic background, there is a close enough one-to-one mapping between being a Western Christian and being German, Slovak or Croat (GSC); and an even closer one-to-one mapping between being an Eastern Christian and Romanian, Ruthenian or Serb (RRS).

Consequently, we collapse name changer data by year, town, and religious group, which is either Western Christian, Eastern Christian and Israelite. We then collapse census data to get town-year observations on the combined number of the GSC community; the combined number of RRS community; and the number of Israelites. We then match Israelite name changers with Israelite community size directly, and Western Christians with the GSC community size, and Eastern Christians with the RRS community size.

For the purposes of the current draft we stick to the comparative analysis of the Israelite and the Western Christian / GSC community. The reason is data limitation. As Table 13 shows, Eastern Christian / RRS population was very scarce to begin with in the Top 25 towns we consider in the current draft; their population was virtually reduced to zero in our sample after the Treaty of Trianon, as Eastern Christians were concentrated in the 15 towns that were

incorporated into the neighboring countries.

2.8 Economic data

Our paper considers economic selection into assimilation. Ideally we would want to see the wages of all name changers, and the wages they would have had, had they decided not to change surname. In our case this canonical selection problem is exacerbated by the fact that we do not observe factual wages either, not to mention the counterfactuals. In fact the only information we have about labor market outcomes is the self-reported occupation of each name changer.

The second best approach would be to match occupation categories with official wage statistics and use these as a proxy for the wage of the name changer. This is also not an option, as the statistical office did not gather such data during the study period. Also, there is hardly any data on labor market outcomes by locality and religious or linguistic grouping.

As a third best option we use a unique dataset on reserve officers of the Royal Hungarian Army to assign labor market information to name changers.[118] Reserve officers were adult males who had an officer training, but were not full-time employees of the military, and had regular jobs as well. The army kept files of their reserve officers, in which their recorded their civilian occupation and their salaries. This provides exactly what we need - a mapping from occupation to income. We have a sample of 6020 reserve officers with altogether 10055 wage observations between 1862 and 1915. As wages themselves are influenced by a lot of factors, we only use these data to identify high wage occupations. As reserve officers are already a higher-than-average status subpopulation, we consider all occupations within this set as high wage occupations who earned consistently over the respective yearly median over the study period.

Once we identified high wage occupations of the study period using the reserve officer dataset, we flag people in the name changer data set who had such an occupation. We use this information to calculate the average "labor market quality" of name changers in every year and town we observe, by simply calculating the share of people with high wage occupations among all adult male name changers.

[117] given that members of the Calvinist Reformed Church (who are predominantly Hungarian) are 20 times more likely to be German, Slovak or Croat, than Romanian, Ruthenian or Serb.

[118] Part of it was compiled by Viktor Karády and Péter Tibor Nagy, who were kind enough to share this data set with us, along with the rest of the scanned records which they had not processed by then. We processed then the rest by virtue of a generous research grant from The Institute for New Economic Thinking (more precisely, "The History Project" of the Institute), with the help of Zsolt Hegyesi and Orsolya Kerepeczky, who provided excellent research assistance.

3 Model

3.1 Setup

To study economic selection into assimilation, we build on the canonical Roy/Borjas model (Roy, 1951; Borjas, 1987). In our formulation there are two communities, one "Foreign" (F) and one "Hungarian" (H). Members of the former can decide to assimilate into the latter. We analyze this decision in subsequent cohorts of the Foreign community; members of each cohort make independent, one-shot decisions which affect the next cohorts only through the size of the remaining Foreign community. Assimilation is proxied by the decision to change family name. We are interested in the following: 1) the share of name changers in each cohort, 2) the corresponding absolute numbers, 3) their average skill of name changers.

Workers make the decision knowing their counterfactual utilities in both communities. Foreigner utility depends entirely on the log wage, which equals skill; foreigner skill is the sum of mean skill μ and a random component ϵ_F.

$$u_F = w_F = \mu + \epsilon_F$$

If they decide to assimilate (change their family name), their utility depends partly on their log wage in their new community, and on the psychological cost of assimilation:

$$u_H = w_H - \pi(x_{l,t}) = a_t + \mu + \epsilon_H - \pi(x_{l,t}).$$

The log wage of name changers is different in two ways. First, the government can affect the assimilation decision through setting parameter a_t. $a_t > 1$ means subsidizing assimilation, $a_t < 0$ is penalizing it. Their random component (ϵ_H) can also be different, as they, for example, can choose between different employment opportunities. The parameter $\pi(x_{l,t})$ is the identity cost, which is increasing in foreign community size $x_{l,t}$. The letter l indexes locality, t indexes time. For any foreign individual the relevant community size to consider is the one in his or her own hometown. The letter a_t only has time index, meaning that the government implements the same policy towards the members of a community at every locality.[119]

[119] That is, if the government encourages the assimilation of Germans, it will encourage it to the same extent in the town of Sopron in Western Hungary as in the town of Kolozsvár in Transylvania.

We make the simplifying assumptions that a cohorts live for a single period of time, and make an independent decision taking $\pi(x_{l,t})$ and a_t as given, and that migration costs are prohibitive, so no individual will move from one town from another just to have lower identity cost of assimilation.

Evolution of community size:

We assume that the size of next cohort of foreigners is simply $x_{l,t+1} = \left(1 - P_{l,t}^{change}\right) \cdot x_{l,t} \cdot g + \eta_{l,t}$, where $P_{l,t}^{change} = \frac{changers_{l,t}}{x_{l,t}}$ is the probability of name changing in location l and time t, which equals the share of name changers; g is the population growth rate of the Foreign community, and $\eta_{l,t}$ is a locality specific population shock, representing random out-migration, immigration, or urbanization. We assume that this is independent, mean-zero error term (unaffected by government policy, and by local community effects).

Probability of name change:

A person will adopt a Hungarian family name if $u_H > u_F$. If $\epsilon_{F,H}$ are normal, then $\nu = \epsilon_F - \epsilon_H$ is also normal, and the probability of name changing is

$$P_{l,t}^{change} = Prob\left[a_t + \epsilon_H - \pi(x_{l,t}) > \epsilon_F\right] = Prob\left[\epsilon_H - \epsilon_F > \pi(x_{l,t}) - a_t\right] = 1 - \Phi(\frac{\pi(x_{l,t}) - a_t}{\sigma_\nu}) = 1 - \Phi(z_{l,t}).$$

Number of name changers:

The absolute number of name changers will be the name changing probability times the size of the community x_t.

$$changers_t = \left[1 - \Phi(\frac{\pi(x_{l,t}) - a_t}{\sigma_\nu})\right] x_{l,t} \tag{16}$$

Average skill of changers:

The conditional skill of name changers will be (see Borjas, 1987):

$$E(w_H|changer) = \mu + \frac{\sigma_F \sigma_H}{\sigma_v} \left(\frac{\sigma_H}{\sigma_F} - \rho\right) \frac{\phi(z)}{P_{l,t}^{change}}$$

$$E(w_F|non-changer) = \mu + \frac{\sigma_F \sigma_H}{\sigma_v}\left(\rho - \frac{\sigma_F}{\sigma_H}\right)\frac{\phi(z)}{P_{l,t}^{change}}$$

The share and composition of name changers are pinned down by two parameters: the correlation $\rho = \frac{\sigma_{FH}}{\sigma_F \sigma_H}$ and the share of variances, σ_H/σ_F. If $\sigma_H/\sigma_F > 1$ and $\frac{\sigma_F}{\sigma_H} < \rho$, the best of the foreign community will self-select into assimilation and take advantage of the fact that the general population has a more dispersed skill/wage distribution (positive hierarchical sorting). If $\sigma_H/\sigma_F < 1$, the converse will be true.

3.2 Model predictions

Proposition 1a: Name changer share is decreasing in the community size $x_{l,t}$ and increasing in the average premium a_t.

Proof: The expected share of name changers is the same as the probability of an individual name change, which is $P_{l,t}^{change}$. $P_{l,t}^{change}$ is decreasing in $\Phi(z_{l,t})$,which is increasing in $x_{l,t}$ and decreasing in a_t.

Proposition 1b: Average skill of name changers is increasing in $x_{l,t}$.

Proof: if the share of foreigners who assimilate decreases in $x_{l,t}$, that means that the marginal name changer will always be of lower skill when $x_{l,t}$ is smaller. The consequence is a decline in the average skill of name changers when $x_{l,t}$ is decreasing, and an increase, when x_t is increasing.

Proposition 2: Dynamic incentives create persistent responses to temporary assimilatory shocks

Proof: if $x_t << x_{t-1}$ for some exogenous reason, or through a temporary policy shock $a_t > a_{t-1,t+1}$, the cohort in $t+1$ faces lower assimilation cost, so more people assimilate even after the shock is over. This again is carried over to the next cohort, so the share of changers will be on a higher trajectory as before the shock, and will (or will not) revert depending on g.

More formally, the share of name changers in every period is $(1 - \Phi((\pi(x_{l,t}) - a_t)/\sigma_\nu))$, which is decreasing in $x_{l,t}$. We can iterate the equation that governs the evolution of community size backwards,

$$x_{l,t} = (x_{l,t-1} - changers_{l,t-1}) \cdot g + \eta_{l,t-1} = ((x_{l,t-2} - changers_{l,t-2}) \cdot g + \eta_{l,t-2} - changers_{l,t-1}) \cdot g + \eta_{l,t-1},$$

up until some initial value:

$$x_{l,t} = x_{l,0} \cdot g^t - \sum_{k=0..t-1} changers_{l,k} \cdot g^{t-k} + \sum_{k=0..t-1} \eta_{l,k} \cdot g^{t-k} \quad (17)$$

This means that if there was any shock or policy change that increased the number of name changers at one point of time in the history of the community, that will be reflected in lower assimilation costs at any later period.

Proposition 3a: If there is positive hierarchical sorting and $\Phi(z_{l,t}) \cdot g < 1$, then $E(w_{t+1,H}|changer) < E(w_{t,H}|changer)$

Proof: The consequence of $\Phi(z_{l,t}) \cdot g < 1$ is a decreasing minority population ($x_{l,t+1} < x_{l,t}$), so the cohort in $t+1$ faces lower assimilation costs than the cohort in t did, so more people decide to assimilate. In the case of positive hierarchical sorting, those who assimilate are selected from the upper end of the skill distribution, so the marginal worker has lower skill if more people assimilate. Conversely, if they were selected from the lower end (negative hierarchical sorting), decreasing costs would mean that the marginal worker has higher skill.

Proposition 3b: If there is positive hierarchical sorting and $\Phi(z_{l,t}) \cdot g > 1$, then $E(w_{t+1,H}|changer) > E(w_{t,H}|changer)$

Proof: The consequence of $\Phi(z_{l,t}) \cdot g > 1$ is an increasing minority population ($x_{l,t+1} > x_{l,t}$), so the cohort in $t+1$ faces higher assimilation costs than the cohort in t did, so less people decide to assimilate. In the case of positive hierarchical sorting, those who assimilate are selected from the upper end of the skill distribution, so the marginal worker has higher skill if less people assimilate. Conversely, if they were selected from the lower end (negative hierarchical sorting), increasing costs would mean that the marginal worker has lower skill. Whatever the direction, an increasing population exacerbates selection over time.

Considering the practice of assimilation policy (name changing was an entrace to higher tiers of society) and that the relatively highest skilled minority was most overrepresented among name changers (Jews), the negative sorting case does not seem very relevant ex ante.

4 Empirical evidence

In this section we take the predictions of the model to the data. First, we check whether the broad comparative statics are consistent with observed data. Then we estimate the underlying economic incentive parameters a_t that would explain the patterns in name changing we observe. Then we show suggestive evidence that the model predictions hold on the intensive margin;

the observed composition of the name changer cohort also broadly follows the model predictions. Finally, we calculate counterfactual name changing shares for minorities. We use this to calculate what percentage of name changing is the effect of government policy and what percentage is due to community effects (i.e. person A changing name because person B did before her).

4.1 Reduced form evidence

Evidence on community effects

Table 15 shows outputs from regressions where the left hand side is a town-year observation of changer count divided by community size. This is what we call the "changer share" of a community in town l in year t. The right hand side is the natural logarithm of community size. That is, if we compare Western Christian communities in two towns, the one which has one log point higher minority population will be expected to lose 0.06 percentage points less of that population to name changing in that year. The regression includes time and town fixed effects, and we control for the size of the Hungarian speaking population (also in logs). We cannot cluster the standard error estimates, because there are too few towns (fewer than the minimum amount suggested in Angrist and Pischke, 2008). Instead, we calculate wild cluster bootstrapped p-values as suggested by Cameron et al., 2008, which allows for clustering even with relatively few groups.[120]

The regressions show that there is a negative correlation between the size of the minority community and name changing activity from that particular minority community. This is strongly significant in the OLS case; still significant for Western Christians, and marginally insignificant for the Israelite minority if we allow clustered errors. The immediate lesson from this table is that our "super-grouping" of Christian minorities makes sense, as there is systematic co-movement between variables from two data sets along this grouping. We consider this an important reality check.

More importantly, these results are also consistent with our formulation of the identity cost of name changing and also with the resulting Proposition 1a. It is worth noting that there is an order of magnitude difference between Western Christian and Israelite minorities. The reason for that is that, while Jewish communities were present in most major towns, Christian minorities tended to concentrate more geographically (Slovaks in Northern Hungary, Serbs in Southern

[120] We implement the algorithm using the tools provided in Roodman et al. (2019). The method does not provide standard errors; p-values are calculated using the distribution of bootstrap t-statistics.

Hungary etc.).

Table 15: Changer share and community size

	(1) Western Christian β/se/CGM p-val.	(2) Eastern Christian β/se/CGM p-val.	(3) Israelite β/se/CGM p-val.
Log community size	-0.0646***	-0.0128***	-0.00607***
	(0.0034)	(0.0033)	(0.0003)
	(0.02)	(0.52)	(0.13)
Observations	1119	1110	1124

OLS standard errors in first parenthesis. * p<0.10, ** p<0.05, *** p<0.01.
Cameron-Gelbach-Miller (2008) bootstrapped p-values in second parenthesis, clustered on the level of towns. Year- and town-fixed effects included.
Western Christian minorities include Germans, Croats and Slovaks.
Eastern Christian minorities include Romanians, Ruthenians and Serbs.

Evidence on static and dynamic policy effects

In Panel A of Figure 32 we plot the town level average share of family name changers in local minority communities in years around the 1898 Hungarianization campaign. We can think of the 1898 campaign as a single year increase in a_t (the government incentive part within the wage of the potential name changer), or, perhaps more plausibly, a single-year tax levied on everyone who does not change name, in the form of an increased probability of losing the job in the public sector.

The pattern for Western Christians (solid blue line) and Israelite name changers (dashed green line) is very similar. There is a sharp increase in 1898, after which name changing rates do not return to previous levels at once, but regresses sluggishly. If we take the historical accounts at face value, and consider the 1898 campaign of the Bánffy administration a one-time event, the patterns are consistent with the dynamic incentives outlined in Proposition 2. As a large percentage of one's own community decides to assimilate, the psychological cost for the next cohort will be smaller.

In Figure 33 we ask whether the size of the non-Hungarian population matters relative to the Hungarian population. In Panel A we re-calculate the averages from Figure 2A only for towns where the majority was Hungarian speaking over the whole study period. In Panel B we only consider towns where at one point of time non-Hungarians constituted a majority over Hungarian speakers.[121] The figures show much of the observed name changing of Christian majorities comes

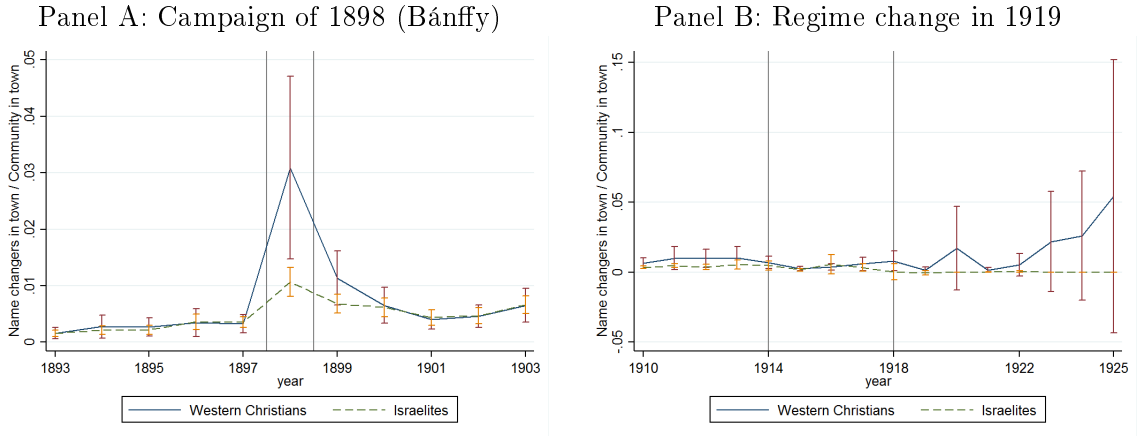
[121]Panel A: Arad, Baja, Budapest, Debrecen, Győr, Hódmezővásárhely, Kolozsvár, Komárom, Marosvásárhely, Nagyvárad, Pécs, Szabadka, Szeged, Székesfehérvár. Panel B: Kassa, Pancsova, Pozsony, Selmec- és Bélabánya, Sopron, Szatmárnémeti, Temesvár, Újvidék, Versec, Zsombor. All towns considered in Panel B were separated from Hungary after World War I, with the exception of Sopron.

from towns where Hungarians constituted a majority over the whole study period, while Jewish name changing observed no such heterogeneity across these two types of towns.

Panel B of Figure 32 shows the second "policy" we study, the dawn of the right-wing authoritarian Horthy regime, which on one hand increased the momentum of Hungarianization, on the other it made driving back "Jewish influence" in public life a central theme. It is important to reiterate that historical accounts find no evidence of institutionally barring Jews from name Hungarianization in the twenties. This was not the case in other areas of life, as institutional discrimination of the Jewish population started as early as 1920 with the passing of the numerus clausus law. Within the framework of the model we can think of this as a massive tax on the lifetime income on the Jewish minority, as they could only get into higher education in much smaller numbers than before. Also, even if public officials did not bar Jews from name changing, they probably did not make it any easier either, and the applicant's psychological cost of getting through the procedure might have been higher in the increasingly anti-Semitic administrative culture. Finally, as the state did not welcome assimilation of Jews, name changing for them plausibly did not extend employment opportunities anymore.

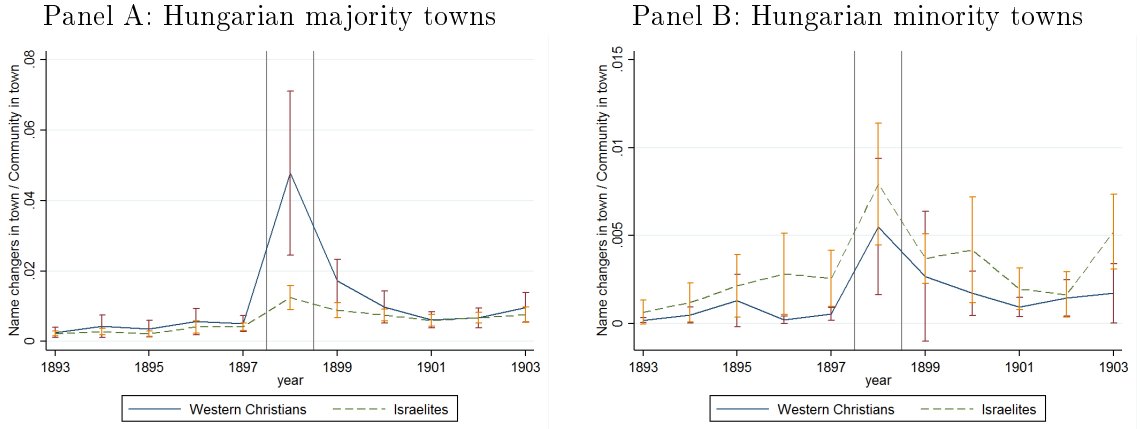
In the language of the model, the new regime had a simultaneous impact on μ, κ, a, σ_H and ϵ_H. All of these were pointing to the direction of a decrease in the probability of name change. This is consistent with the patterns in Panel B of Figure 32. As incentives disappear, Jewish name changing drops close to zero in the observed sample of towns starting from 1920. The new regime still encourages name changing, but only for Christian minorities (though the confidence intervals are very wide, because the estimates come from 10 cities only).

Figure 32: Policy shocks



Panels A and B both plot yearly mean share of name changers by communities with 95% confidence bands in each year. Vertical lines in Panel A highlight the 1898 policy. Vertical lines in Panel B show the starting and ending year of World War I.

Figure 33: Name changing in Hungarian vs. non-Hungarian majority towns



Panels A shows name changing shares by community in towns which were of Hungarian majority all the time between 1880 and 1920. Panel B shows name changing shares in towns which were not of Hungarian majority at least once between 1880 and 1920. Vertical lines highlight the 1898 policy. 95% confidence bands included.

Evidence on sorting

To this point, we provided evidence that the quantity of name changers over time is consistent with a simple model where economic incentives and community ties jointly determine an important part of identity and assimilation status, the family name. Now we show that the model's predictions about the quality of name changers are also observed in the data, further suggesting that wages and labor market perspectives are among the determinants of identity choice.

First, we establish that the case for positive hierarchical sorting into name changing is plausible, at least on the part of the Jewish minority. The Borjas (1987) result was that the best skilled people self-select when the receiving population has a more dispersed skill or wage distribution.[122] In the context of the assimilation model one intuition for this might be that in a closely-knit minority community the wealthy are obliged to take care of the poorest, which would work as redistribution scheme, and compress the income distribution. A high skilled individual then might feel she is better off not paying for supporting the needy, and leave the community. An alternative intuition can be if the minority community simply prohibits some outside occupations for its members.

For the sorting result to hold for name changers we would first have to show that the Jewish skill distribution is less dispersed than the Hungarian. Then show that the ratio of the variances is related to frequency of name changing. Census records in Hungary during the Dual Monarchy

[122] These are essentially the same in his model.

regularly recorded literacy and schooling data by religious denomination. We calculated the town-wise variance of literacy for the Jewish community and the general population using the 1890 census.[123] The variance of the skill distribution of the general population is higher 23 out of 25 cases, which means that the sorting condition holds.

We then calculated the ratio of Jewish name changers between 1890 and 1897 and the Jewish population in 1890. We plotted the latter number against the variance ratio in Figure 34. There is a clear and very strong positive relationship between how much more dispersed the majority population distribution is, and how big a share of the Jewish population decides to change name. In fact, the R^2 attests that the variance ratio alone explains 30% of all variation in the name changer share. The correlation coefficient is 57%. This strongly supports the case for positive hierarchical sorting into assimilation of the Jewish minority.

Next, we show that the model's predictions for the case of positive sorting into name changing are consistent with the observed data. Proposition 1b ascertains that the average quality of name changers is related to community size, as the identity cost of name changing might be prohibitive for lower skilled or lower wage workers. If community ties are weaker however, there might be lower skilled workers for whom name changing becomes profitable on the margin. Proposition 3 extends this with predictions on the time variation in name changing. The consequence of this proposition is that holding population growth rate g fixed, during times when the skill cutoff is lower (over which people change name), the average skill (or wage) of name changers also should be lower; when the skill cutoff is increasing, the average quality of name changing should also increase. If the wage of the name changers cohort moves in the predicted direction with the cutoff, that provides evidence that selection into name changing indeed has an economic dimension.

We do observe the occupation of most name changers, but, unfortunately, reliable labor statistics were not available during the time period. Instead, what we do is that we calculate the share of high status jobs identified from our Reserve Officer Dataset (as we described in the Data section). This is measure is admittedly far from perfect, but it captures the dynamics in the composition of subsequent name changing cohorts.

In Panel A of Figure 35 we plot the estimated skill cutoff of Jewish communities around the campaign of 1898. The unit of measurement is z-score (where $z_{l,t} = \Phi^{-1}(1 - P_{l,t}^{change})$). This captures the skill level over which a person has an incentive to change name, so a lower

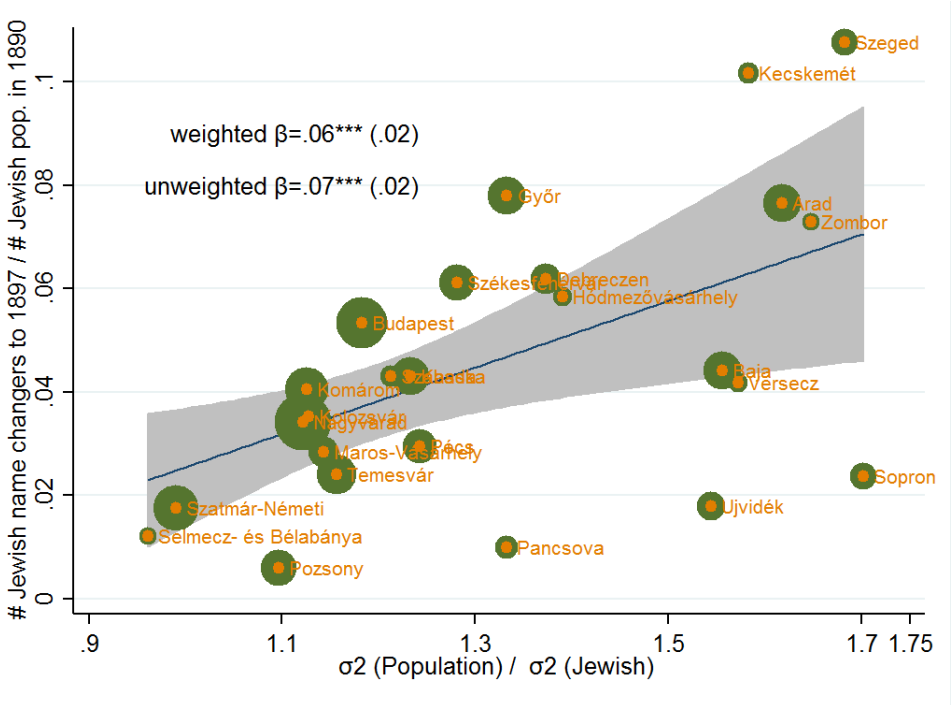
[123] We coded the literacy level "illiterate" as 0, "reads only" as 0.5, and "reads & writes" as 1.

cutoff means more people changing, and on average of a lower quality. As expected, z-scores start to decrease during the Bánffy years and then fall deeply in 1898 when the state engages in Hungarianization. Then, as things return to normal after ousting the prime minister, z-scores increase again to pre-1898 levels.

Panel B of the same figure shows what happens to the share of high skilled workers among Jewish name changers. First, their share is decreasing already before the Bánffy years, suggesting an increasing importance of community ties in selection. There is a dramatic drop in 1898 in cohort quality, and the share of high skilled name changers fall to less than half of their previous share just five years before. In fact, 1898 is the only year in the decade before and after 1898 when the most frequent name changer occupation label is not either "merchant" or "doctor", but "servant". Then, as the upheaval of 1898 subsides, the average quality as proxied by share of high skilled changers starts climbing up again. It, however, settles on a lower average level 5 years after 1898 than it was at 5 years before 1898, consistent with the long-term predictions on name changer cohort composition.

All in all, our data are consistent with the premises of economically motivated, positive hierarchical sorting into name changing, and with its consequences on the skill/wage composition of name changing cohorts.

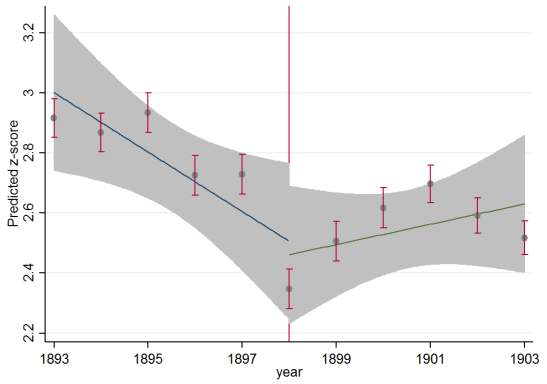
Figure 34: Sorting



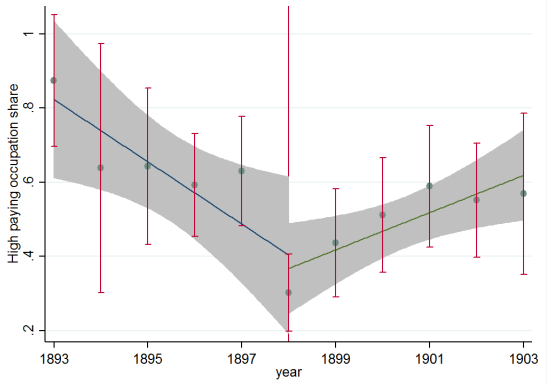
Horizontal axis: ratio of variance of the skill distribution of the general population and the Jewish minority. Vertical axis: Jewish community who changed name between 1890 and 1898 as a share of Jewish population in 1890. Observations are the Top 25 towns; the sizes of the dots represent the relative size of the Jewish community; the line represents an OLS estimate of the linear correspondence between the axis variables. Source: 1890 census and Farkas and Kovesdi (2015) data set. 95% confidence bands included.

Figure 35: Selection

Panel A: average of predicted z-scores



Panel B: share of name changers in high paying occupations



Panel A shows the estimated skill cutoff over which individuals changed their names (estimated from Equation 18, see Section 4.2. A higher cutoff means higher average skill of name changers. This is calculated only from observed name changer shares. Panel B shows the share of high-paid name changers in every year. Red lines indicate 95% confidence intervals of individual data points; the shaded area is the 95% confidence interval of the slope estimate.

4.2 Estimation of government policy parameters

We now turn to estimating the static impact of government on the incentive to change names.

We assume that a member of a minority group in town A will face the same government policy as a member of the same group in town B (but not necessarily a member of another minority in town A). We observe two minority groups (Western Christian and Israelite) living in 25 major towns. We use the cross-sectional and time variation in population data and name changing figures to estimate the policy variable a_t from observed name changer shares and community sizes over time. We do this for the two minorities separately. From Equation 16 we can express the share of people who assimilate relative to their respective community size in every period:

$$P_{l,t}^{change} = \frac{changers_{l,t}}{x_{l,t}} = 1 - \Phi\left(\frac{\pi(x_{l,t}) - a_t}{\sigma_\nu}\right)$$

From which

$$\Phi\left(1 - P_{l,t}^{change}\right)^{-1} = \frac{\pi(x_{l,t}) - a_t}{\sigma_\nu}$$

Assume that $\pi(x_t) = f(x_t) + \kappa + \kappa_l + \kappa_{l,t}$, where κ_l is the location-specific fixed utility cost of name change, $\kappa_{l,t}$ is a location and time specific idiosyncratic error term in identity cost, and $f(x_{l,t})$ is the variable cost of leaving a community of size $x_{l,t}$. Then we can write the previous equation as:

$$\Phi\left(1 - P_{l,t}^{change}\right)^{-1} = \frac{1}{\sigma_\nu}\left(f(x_{l,t}) + \kappa - a - \widetilde{a_t} + \kappa_l + \kappa_{l,t}\right), \tag{18}$$

where $a + \widetilde{a_t} = a_t$ and the average of $\widetilde{a_t}s$ is 0. The left hand side of the equation is the z-score cutoff at which a person is indifferent between keeping and changing names, based on observed name changing shares of the population. The right hand side shows the determinants of that cutoff: local community size and name changing cost enters with a positive sign, meaning that higher costs and larger communities push the cutoff higher, and thus decrease the share of name changers. On the other hand, positive government incentives (if there is such) enters with a negative sign, meaning that their presence decreases the cutoff and so increases the share of name changers.

We can estimate Equation 18 by flexibly controlling for the functional form of $x_{l,t}$ and including location and time dummies. By observing the formula, we see that we cannot identify κ (the fixed cost of assimilation) and a (the time-invariant part in government policy) separately, as their difference is the constant term of the equation. Consequently, we can only estimate $\widetilde{a_t}$s, the part of policy that changes in time.

Based on the reduced form evidence, we use a log functional form for $f(x_{l,t})$. We also control for the number of Hungarians in the town as it might contribute to assimilatory pressure. Finally, we also control for the lagged cumulative number of name changing events in the town. We do this because the recursive formula in Equation 17 shows that $x_{l,t}$ depends on the past number of name changing events. If we perfectly measured $x_{l,t}$, this would not be an issue, but we certainly do it with a measurement error, which then is also correlated with past number of name changing events. This part of the variation in $x_{l,t}$ is exactly the dynamic effect of the policies, and we can assume that it has a systematic variation in time (such as in the aftermath of the 1898 Hungarianization campaign). If we do not account for this by including the lagged cumulative number of name changing events, then the $\widetilde{a_t}$ time fixed effects will pick this up. As we interpret these as the time variant part of government policy, in this case we would overestimate the impact of the government on name changing.

Table 16 shows the estimated parameters of the determinants of $f(x_{l,t})$ across the three minority communities. Again, we report OLS standard errors and Cameron et al. (2008) bootstrap p-values. As the dependent variables are the z-score cutoffs, a positive coefficient means that an increase in the variable decreases the likelihood of name changing, and a negative coefficient means that an increase in the variable increases the likelihood of name changing. If we had reliable wage data on the population, we could calculate σ_v and μ to express this in monetary terms as well, but we do not have such data right now.

The first row of coefficients just reiterate the similar result from Table 15. If the minority community is larger, less people will decide to change their names. It is important to note that, except for Slovaks and Ruthenias, all ethnicities that Christian minorities belonged to had a state either within the Empire (Croats), next to the Empire (Romanians and Serbians), or both (Germans). The existence of formal, state level institutions plausibly mitigates assimilatory pressure. This was, of course, not the case for the Jewish minority.

Though the statisical power is very limited, the second and the third row show a consistently different pattern between Christian and Israelite minorities. Even though not significant, a bigger Hungarian community and a history of past name changing in the community is positively associated with current name changing in the case of Christian communities, while negatively in the case of Jewish communities. The latter result is in line with what the model suggests in Equation 17. The former is rather counterintuitive and suggests that either $x_{l,t}$ is measured poorly, or selection works differently for Christian minorities. We cannot decide based on this

evidence. Clustering the standard errors at the town level does not change the results.

Table 16: Estimated parameters from Equation 18

	(1) Western Christian β/se/CGM p-val.	(2) Israelite β/se/CGM p-val.
Log of minority size	0.442***	0.318***
	(0.0597)	(0.0246)
	(0.00)	(0.00)
Log of Hun. population	0.0249	-0.0133
	(0.0465)	(0.0703)
	(0.58)	(0.87)
Log cumulative changer count (in lags)	0.00858	-0.139***
	(0.0368)	(0.0310)
	(0.84)	(0.00)
Observations	944	873

OLS standard errors in first parenthesis. * p<0.10, ** p<0.05, *** p<0.01.
Cameron-Gelbach-Miller (2008) bootstrapped p-values in second parenthesis, clustered on the level of towns. Year- and town-fixed effects included.
Western Christian minorities include Germans, Croats and Slovaks.

Figures 36 and 37 show the estimated $\frac{\widetilde{a}_t}{\sigma_\nu}$ policy parameters for the events we consider of particular interest: the Hungarianization campaign of 1898 and the regime change in 1919. A higher number means more government incentives to change name. Since $(\kappa - a)$ is not identified separately in the equation, only the time variation in $\frac{\widetilde{a}_t}{\sigma_\nu}$ is of interest, so we choose the coefficient in 1890 as baseline in Panel A, and the coefficient in 1913 as baseline in Panel B.

Estimated policy parameters show a similar pattern in the case of both minorities for the 1898 campaign (Figure 36). The policy is more or less unchanged before 1898, with an insignificant pre-trend in 1896 and 1897. Than there is a large jump in 1898. This jump is equivalent to shifting the cutoff in the wage distribution by one standard deviation unit compared to the baseline in 1890 for Christians, and by 0.5 standard deviation unit for Israelites. This corresponds to the fact that name changing shares, as seen in Figure 32, jumped from the order of magnitude of one tenth of a percentage point to the order of magnitude of a full percentage point in 1898. This is a huge economic shock, consistent with the anecdotal evidence that public employees were tacitly threatened by loss of their jobs and livelihoods in 1898.

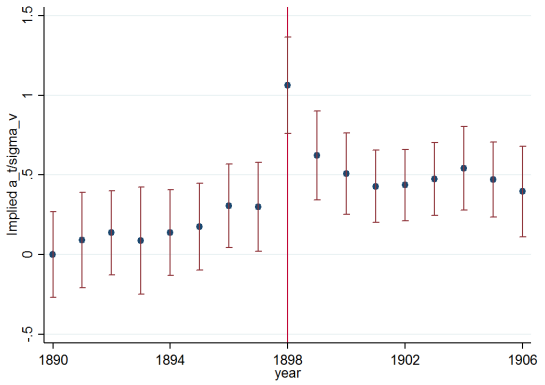
Figure 37 shows the policy parameters estimated from the 1919 regime change. Importantly, this coincides with the signing of the Treaty of Trianon which further decreases the cross-sectional units in our sample (from 25 to 10).

The most important lesson from Figure 37 is that now the government unmistakably treats Christians ans Jews differently. After a general drop in all name changing during the chaotic years of 1919 and 1920, the estimated policy parameters show a small, insignificant increase

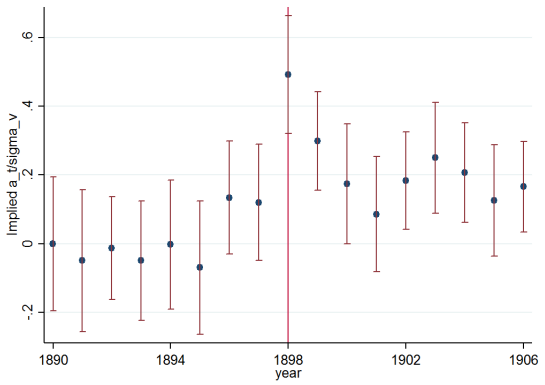
of support for the Hungarianization of Western Christians (Croats, Germans, Slovaks), which eventually resulted in the great name changing wave of the 1930s. Jews, on the other hand, are increasingly discriminated against. Five year into the 1920s the estimated coefficient suggests that the cutoff for name changing shifted a full standard deviation unit to the right compared to previous levels. The share of Jewish name changers drops close to zero.

Figure 36: Estimated $\frac{\widetilde{a_t}}{\sigma_\nu}$ policy effects in the 1898 campaign

Panel A: policy parameter for Western Christians



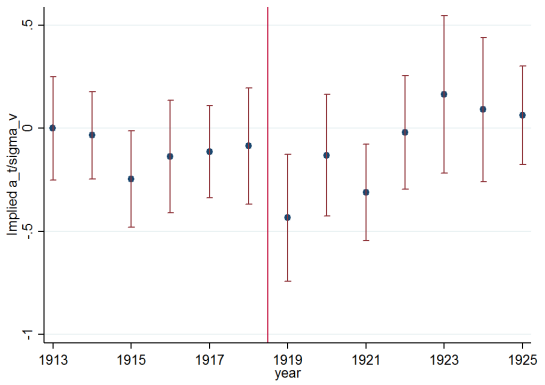
Panel B: policy parameter for Israelites



The figures plot $\frac{\widetilde{a_t}}{\sigma_\nu}$ coefficients estimated from Equation 18. Western Christian minorities include Germans, Croats and Slovaks. The vertical line corresponds to the 1898 Hungarianization campaign. 95% confidence bands included.

Figure 37: Estimated $\frac{\widetilde{a_t}}{\sigma_\nu}$ policy effects around the end of World War I and the right-wing takeover

Panel A: policy parameter for Western Christians



Panel B: policy parameter for Israelites

The figures plot $\frac{\widetilde{a_t}}{\sigma_\nu}$ coefficients estimated from Equation 18. Western Christian minorities include Germans, Croats and Slovaks. The vertical line corresponds to the end of World War I. 95% confidence bands included.

4.3 Counterfactual analysis and decomposition of name changes

Last but not least, we are interested in the dynamic effects of government policy on name changing through affecting the strength of community ties as suggested in Equation 17. The idea is that once the government affected assimilation at one point in time, it has persistently facilitated name changing for further cohorts, by reducing their identity cost of name changing. We measure how important this channel is, by using the campaign of the Bánffy government as a well-defined policy shock.

We calculate counterfactual scenarios through first simultaneously estimating the following system of equations using Seemingly Unrelated Regressions:

$$\begin{gathered}\Phi\left(1-P_{l,t}^{change}\right)^{-1}=\frac{1}{\sigma_\nu}\left(f(x_{l,t})+(\kappa-a)-\widetilde{a_t}+\kappa_l+\kappa_{l,t},\right)\\ x_{l,t}=(x_{l,t-1}-changers_{l,t-1})\cdot g+\eta_{l,t-1}\end{gathered} \tag{19}$$

Table 17 shows the estimates of the same parameters from this model as those estimated in Table 16. The point estimates and their significance levels are almost identical. The important difference compared to the estimates based on Equation 18 is that here we jointly estimate the population growth rate parameter and the policy parameters. This shows that the Western population is actually shrinking (at a "growth rate" of -0.009% a year), and the Jewish population is increasing at a rate of 2.9% (urbanization, internal and external migration could have contributed to this high rate).

Table 17: Parameter estimates from system of Equations 19

	(1) Western Christian	(2) Israelite
Log of minority size	0.427***	0.332***
	(0.0439)	(0.0243)
Log of Hun. population	0.0168	-0.0457
	(0.0491)	(0.0451)
Log of cumulative changer count (in lags)	0.0163	-0.132***
	(0.0342)	(0.0237)
Estimated pop. growth	0.991***	1.029***
	(0.000579)	(0.000473)
N	807	797

Standard errors in parenthesis. * p<0.10, ** p<0.05, *** p<0.01.
Year- and town-fixed effects included in the regression.
Western Christian minorities include Germans, Croats and Slovaks.

In the previous exercises we took $x_{l,t}$ community sizes as given in every year (with the noted caveats). Now, having estimated the growth rate g along with $(\kappa-a), \widetilde{a_t}, \kappa_l$, we are able to feed

these parameters into the model along with starting values for $x_{l,t}$ and $changers_{l,t-1}$ and the size of the Hungarian community, and simulate counterfactuals where we vary only $\widetilde{a_t}$ between scenarios.

Let's call the baseline predicted time path (when we change no parameters) Scenario 0, or the factual data. We consider the following counterfactual scenarios in contrast to the baseline:

1. A scenario in which the 1898 campaign never happened, and the policy variable $\widetilde{a_t}$ remained at the 1897 level throughout the year;
2. A scenario in which the Bánffy-government never happened, and the $\widetilde{a_t}$ parameters remained fixed at the 1895 level until 1899;
3. A scenario where $\widetilde{a_t}$ remained fixed at 1894 levels for all subsequent years.

If we denote the sum of name changers across the 25 towns at time t in scenario s as $\sum_l Changers_{t,s}$, then the following differences are of interest to us:

$D_1 = \sum_l Changers_{l,t,s=0} - \sum_l Changers_{l,t,s=1}$ is the total effect of the 1898 campaign. This sum in $t = 1898$ is what we call the static effect of the policy - the number of extra people in the 25 towns whom the government conviced to change name in 1898, compared to how much it would have a year before. The sum in $t > 1898$ is the dynamic effect - the number of name changers who made this decision because name changing became less costly following the 1898 policy.

$D_2 = \sum_l Changers_{l,t,s=0} - \sum_l Changers_{l,t,s=2}$ is the total effect of the Bánffy administration's assimilatory policies on name changing. In this case the sum over $t = 1895..1898$ is the static effect, and $t > 1898$ are dynamic effects.

$D_3 = \sum_l Changers_{l,t,s=2} - \sum_l Changers_{l,t,s=3}$ shows how much of post-1898 name changing is explained by changes in government policy relative to the Pre-Bánffy years (as opposed to community effects).

Figure 38 shows the results for Western Christians, Figure 39 for the Jewish communities.

Panel A1 in both figures contrasts the simulations under different scenarios to the actual observed data. The vertical axis shows the average share of people who assimilate in any town (relative to the current, yearly adjusted size of the community). The solid lines represent observed data, the dashed lines represent predicted factual values from the model. As we see,

the model predicts observed name changing quite well up until 1898, and then it becomes biased systematically, but in different directions. For the Western Christian minorities, the model overestimates name changing. In the case of the Jewish minorities, it underestimates it.

Panels A2 present the percentage of name changing events that are attributable to the policy change as a percentage of all name changes in any given year; in other words, the shares $D_1/\sum_l Changers_{l,t,s=0}$ (dashed line) and $D_2/\sum_l Changers_{l,t,s=0}$ (solid line). We call these impulse-response functions, as they show the time path of the isolated effect of the policy in question. The vertical red lines correspond to the start and the end date of the Bánffy administration, so the part of the impulse-response functions within these are the static effects of the policy, and to the right of the second line is the dynamic effect.

Again, the two communities we study show remarkable differences. While the Christian name changers show a bigger immediate, static response to the Bánffy-shock compared to Jewish name changers (95% as opposed to 75%), the former group shows no sign of a dynamic effect later on. Conversely, a sizeable chunk of all name changing after 1898 is due to dynamic effects of the Bánffy-policies. In particular, around 5% according to Scenario 1, and around 10% according to Scenario 2.

Panels B1 and B2 of both figures show this result using absolute name changer counts in the 25 towns we study. The area in blue shows the dynamic and static effects of the policy shocks combined (D_1 and D_2 in B1 and B2, respectively). The red area represents the share of additional name changing that is due to incentives after the Bánffy era. The green are reflects the counterfactual name changer counts, had the 1894 policies remained for the whole study period - these are the pure community effects.

Lacking dynamic effects, and a population growth rate close to zero, the latter would have remained flat for Germans, Slovaks etc. On the other hand, the Jewish communities experienced both population growth and dynamic effects of name changing, which would have resulted in a steady flow of name changers over the study period, even without any government intervention.

We can think of three main reasons why our calculations are so remarkably different across Western Christian minorities and Jews. The first, perhaps most obvious reason is that there is no such minority as a distinct "Western Christian" minority; this is an artifact of our necessity to consider Germans, Slovaks and Croats together, as name changing records do not contain ethnicity, just religion. Even if the model provided a good description of the underlying mechanism, this problem contributes to the overestimation of the importance of community ties for

these minorities. To see this, consider the example of the Northern Hungarian town of Kassa (now Košice in Slovakia). The 1891 census reported around 4200 Slovaks and 2000 Germans in the town. In our model, we would treat them as a single community with 6200 members. This means that our expectations of community ties are 50% higher than reality for Slovaks, and as much as 3 times as high as the real ones for Germans.[124] This problem is further exacerbated by the possibility of Hungarian to Hungarian name changes, which certainly happen from time to time (see Footnote 111). These events introduce noise that affects the results in a non-trivial way, as they add a positive measurement error to name changing events, and a negative error to community size $x_{l,t}$.

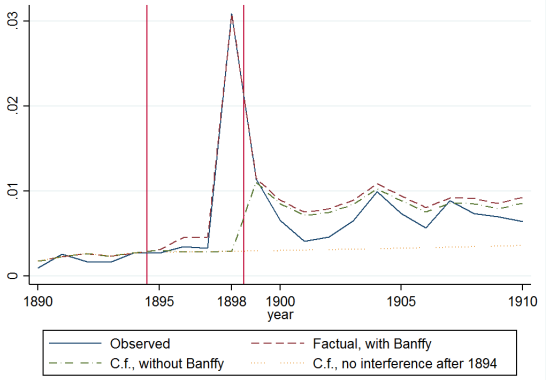
A second potential explanation would be if the size of the relevant community was not just observed with an error, but was conceptually different for distinct minority groups. As we mentioned already, the fundamental aspects of distinction for Christian minorities were family name and language, while these were family name and religion for Jews. A problem is that, while Hungarian government never engaged in forced conversion of Jews to Christianity, it did engage in an increasing push for the use of the Hungarian language in public (and private) life. Though this started in earnest after 1907, the push for increased Hungarian language use was there earlier on.[125] This means that there might have been another, competing Hungarianization policy besides incentivizing name change. If this was the case, we would attribute its effect to name changing policies through omitted variable bias, and would see that a bigger proportion of name changing was attributed to policy (rather than to community) for Christian name changers as for Jews. This is indeed what we see in Figure 38. Another smoking gun in this regard is that we estimate a slight shrinkage of linguistic minorities over time ($g < 1$), which might be the product of other assimilatory policies, not name changing.

[124] Admittedly, most towns were less diverse in terms of their Western Christian minorities as Kassa was; and in most cases Germans were the single large such minority group.

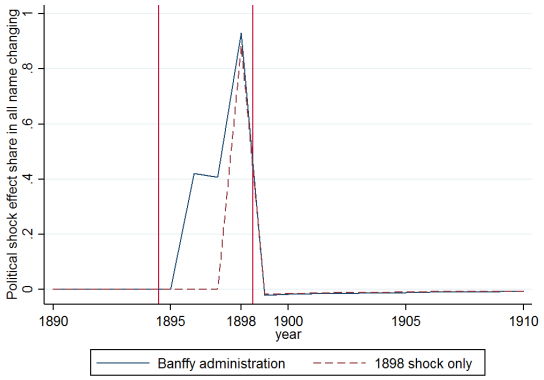
[125] The so-called Lex Apponyi was adopted in 1907 which mandated that all schools (regardless of their ownership status) had to teach the Hungarian language. See Footnote 107

Figure 38: Model simulation for Western Christian communities

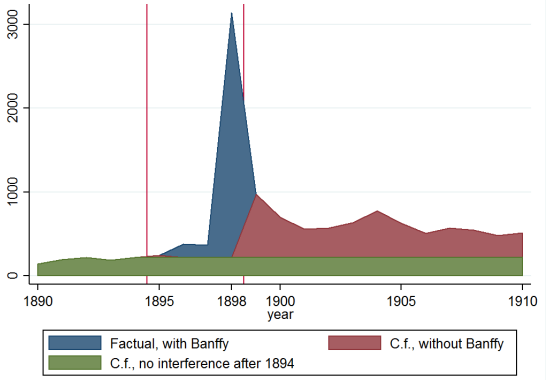
Panel A1: Name changers' share in community



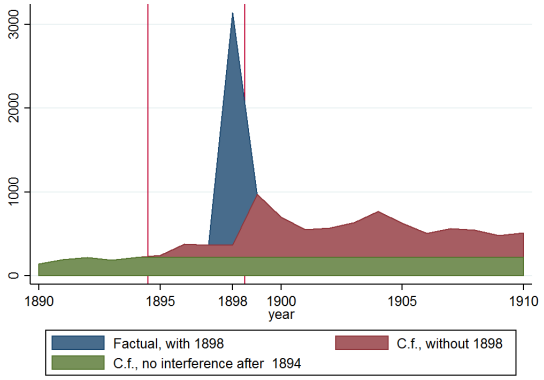
Panel A2: Impulse response function



Panel B1: Decomposing the effect (shock: Bánffy adm.)



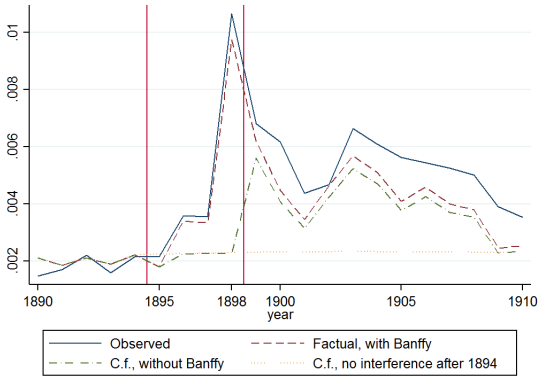
Panel B2: Decomposing the effect (shock: 1898)



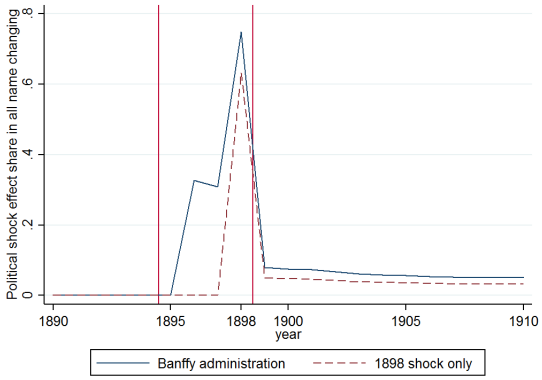
Panel A1 shows simulated name changer shares for the Western Christian minority (using system of equations 19) compared to actual name changer shares. Panel A2 shows impulse-response functions to the Bánffy policies impact. Panels B1 and B2 decompose the effect of the whole Bánffy administration (B1) and the 1898 policy only (B2) into three components: 1) community effects (green); 2) static and dynamic effect of the policy in question (blue); 3) and all policy impact coming from later periods (red).

Figure 39: Model simulation for Israelite communities

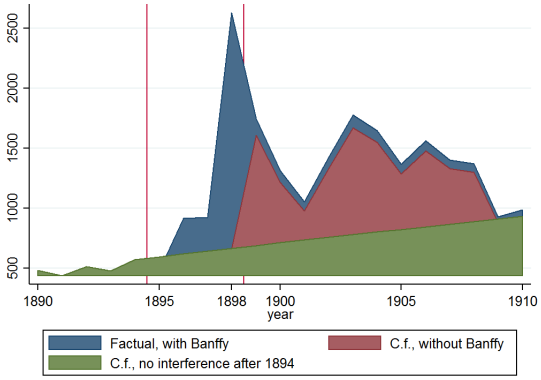
Panel A1: Name changers' share in community



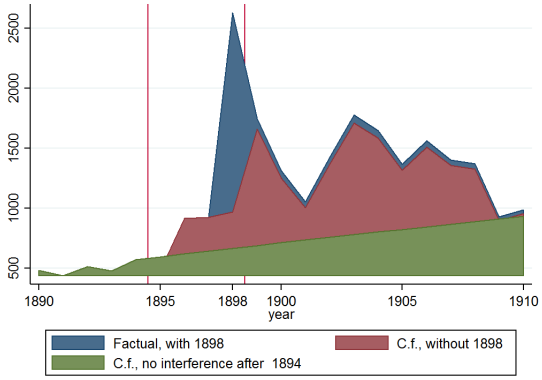
Panel A2: Impulse response function



Panel B1: Decomposing the effect (shock: Bánffy adm.)



Panel B2: Decomposing the effect (shock: 1898)



Panel A1 shows simulated name changer shares for the Israelite minority (using system of equations 19) compared to actual name changer shares. Panel A2 shows impulse-response functions to the Bánffy policies impact. Panels B1 and B2 decompose the effect of the whole Bánffy administration (B1) and the 1898 policy only (B2) into three components: 1) community effects (green); 2) static and dynamic effect of the policy in question (blue); 3) and all policy impact comping from later periods (red).

5 Conclusion

Our paper documented that family name, a core aspect of personal and national identity, responds to economic incentives on the very short run. We studied the family name changing movement in Hungary during the Austria-Hungary period and the 1920s, presenting unique, individual level data on people who decided to Hungarianize their family names. We were also able to make use of a policy shock, the nationalist campaign of 1898, during which the government actively pushed for the name Hungarianization of public sector employees.

We showed that the observed patterns in name changing are broadly consistent with a basic model of economic selection, in which the decision to change name depends on the pull effect

of community, the political push presented by the government, and the individual's prospective utilities in either the minority or the majority community. The model had specific predictions about yearly share of name changers in a given community and the average skill of those who change name. Focusing on the Top 25 towns for which census information was relatively easily available, we showed that the observed data are consistent with model predictions.

Armed with the model, we calculated counterfactual name changing trajectories to see how many of the name changers changed because of contemporaneous government polices, and how many did so because of previous policies which persistently lowered the cost of assimilation for later cohorts. We found that the Hungarianization campaign in 1898 had long term, persistent effect on name changing trajectories over the following generation.

Appendix A : Data description

Coverage of violence data

Table A.1: Coverage of National Violence Monitoring System by provinces and years

	Province/year	1997	1998-2004	2005-2010	2011	2012	2013	2014
1	Aceh		✓	✓	✓	✓	✓	✓
2	Maluku		✓	✓	✓	✓	✓	✓
3	North Maluku		✓	✓	✓	✓	✓	✓
4	Papua		✓	✓	✓	✓	✓	✓
5	West Papua		✓	✓	✓	✓	✓	✓
6	West Kalimantan	✓	✓	✓	✓	✓	✓	✓
7	Central Kalimantan		✓	✓	✓	✓	✓	✓
8	East Nusa Tenggara		✓	✓	✓	✓	✓	✓
9	Central Sulawesi		✓	✓	✓	✓	✓	✓
10	Jakarta/Bogor/Depok/Tangerang/Bekasi			✓	✓	✓	✓	✓
11	Lampung			✓	✓	✓	✓	✓
12	West Nusa Tenggara			✓	✓	✓	✓	✓
13	East Kalimantan					✓	✓	✓
14	North Kalimantan					✓	✓	✓
15	North Sumatra			✓	✓	✓	✓	✓
16	East Java			✓	✓	✓	✓	✓
17	South Sulawesi			✓	✓	✓	✓	✓
18	North Sulawesi			✓	✓	✓	✓	✓
19	West Sumatra							✓
20	Riau Islands							✓
21	Riau							✓
22	Bengkulu							✓
23	Jambi							✓
24	Bangka Belitung Islands							✓
25	South Sumatra							✓
26	West Java							✓
27	Banten							✓
28	Yogyakarta							✓
29	Central Java							✓
30	Bali							✓
31	South Kalimantan							✓
32	West Sulawesi							✓
33	Gorontalo							✓
34	Southeast Sulawesi							✓

Source: http://snpk.kemenkopmk.go.id/

Government services index

The PODES (Pendataan Potensi Desa - Village Potential Database) is a government survey conducted in each census year since 1983. It covers the universe of villages in Indonesia, and gives detailed information on village characteristics in every wave. It is important to note that the term "village" is an administrative concept; rural and urban communities are both surveyed.

Using PODES poses two major empirical challenges. First, the village identifier variables are not consistent over time, so I had to match the villages across the waves based on the geographical

names of the districts, subdistricts and villages. With this method I was able to match 73% of all the villages, which is in the same ballpark as the efficiency of Martinez-Bravo (2014), who implemented the same strategy for matching the data across waves.

Second, the the data coverage over years is inconsistent. Data collection for PODES is linked to data collection for the census, so in years when a plain population census is implemented, the data content will be somewhat different than in years of the agricultural census, or the economic census. I identified all variables that are consistently reported over the waves. Since I have a single explanatory variable, looking at the correlation of each survey variable and the single right hand side variable would raise the prospect of multiple inference. In order to avoid that, I took all variables that reflect government services such as education and infrastructure (the detailed list is provided below), and used them to create an additive index using the method described in Anderson (2008).[126] The procedure takes the following steps:

1. Adjust signs of all variables so that a higher value corresponds to the better outcome
2. Demean outcomes and divide them by the standard deviation of the "control group" (all villages that did not introduce religious policies until the end of the study period)
3. Create weights for each variable -the weights will be the sum of the row entries of the inverted covariance matrix of the variables
4. Create the index, which will be the weighted sum of the variables.

The variables which are included in the index are:

- Number of schools for each main Indonesian education tiers (SD, SMTP, SLTP, SMU)
- Number of vocational education institutions
- Number of households that have access to electricity

[126] I omit the variables related to healthcare, as over the study period Indonesia starded experimenting with central government-financed healthcare schemes which grew steadily in coverage and funding, thus the data on healthcare infrastracture mostly reflect central, and not local government policies.

- Number of households with landline phone subscriptions[127]
- Type of road lights
- Type of cooking fuel
- Type of waste disposal
- Type of sewage disposal

The four infrastructure variables are measured on ordinal scale; the best value typically corresponds to state provision of a centralized public service.

[127]Though steadily increasing, mobile phone penetration rates were still comparatively low by the end of the study period (the exact rates were 0.1%, 0.8%, 4%, 9%, 25% and 42%), so landlines were still a relevant factor in wellbeing. Data source: World Bank (https://data.worldbank.org/indicator/IT.CEL.SETS.P2?locations=ID); accessed: 08/29/2018

Appendix B : Shift-share instruments

Papers using Bartik-type instruments have been criticized recently for having a "black box" character, not being clear about what exactly their identification assumptions were. Jaeger et al. (2018) argue that shift-share IV-s in general conflate short-run and long-run effects of the endogenous variable, and propose including the lagged values of the shift-share as additional instruments to remedy this problem. My results are robust to this extension.

Goldsmith-Pinkham et al. (2018) opens up the "black box" of the Bartik IV, and formally derives the identification assumptions behind the shift-share instruments. In what follows, I rely on their analysis to make the case for my particular choice of instrument. The general case of shift-share IV has three dimensions: a geographical, a temporal, and a third dimension, which depends on the context: it can represent country of origin (in the migration literature, see Altonji and Card, 1991), or industry (as in Bartik, 1991). The idea is to form a prediction of the variable of interest (size of migrant community, employment in an industry) by using an initial share and later country-wide growth rates: migrants go to locations with a bigger migrant community; sectors which have a higher initial employment share also represent higher proportion of later employment growth etc.

In the case of the current paper there is a single "sector", representing "Sharia demand". The intuition behind the identification is that country-wide increase in religiosity translates to Sharia demand more easily in regions where Islamists were more institutionalized before democratization and decentralization in 1999-2000, so we can use variation at this time (or even earlier on) to form predictions for later years. This predicted Sharia demand will be the instrument for Sharia policy introduction later on. As there is a single sector, and the time dimension is also limited (yearly regional outcome variables are only available until 2013), this is the case discussed in Goldsmith-Pinkham et al. (2018) as "Case 1". In this case the country-wide growth rate is taken as non-stochastic, so the exclusion restriction really concerns the initial levels of Sharia demand:

$$E(share_{r0}\epsilon_{rt} \mid z_{rt}) = 0).$$

This assumptions is not directly testable, but the authors recommend three empirical tests to judge its plausibility. The first test they recommend is to check if initial shares are correlated with initial levels of potential observable confounders (this recommendation is based on Altonji et al., 2005). Fortunately, this question in the context I am studying has been extensively

investigated in Bazzi et al. (2018). They show how an unexpected (and later repealed) land reform policy helped to build up Islamist networks in the 1960s in Indonesia. They find that the policy in question created exogenous variation in the wealth of Islamists, which they used to grow their networks and further their agenda. This translated into more *pesantren* and a higher chance of having Sharia regulations later on. They find that this Islamist expansion did *not* translate into any observable difference in a range of development outcomes by the early 2000s (before the regulations were introduced). The development outcomes they study include land dispersion, share of irrigated goods, public service provision, agricultural productivity, education attainment.

The second test Goldsmith-Pinkham et al. (2018) suggest is to check pre-trends in the variables. I studied this question in Section 4 extensively, and were not able to find observable pre-trends.

As a third test the authors suggest to use alternative estimators to the Two-Staged Least Squares as well, such as the Limited Information Maximum Likelihood estimator, compare the estimates and check for the usual tests of under- and weak identification. In an unreported robustness check I performed these and concluded that the results are robust to the choice of the estimator and the tests do not provide any smoking guns for the invalidity of the instrument. In conclusion, I found no evidence against the use of the shift-share instrument.

Because I am not aware of existing papers that have used the shift-share instrument in a political economy setting, as a final exercise I derive formally how it works in the case studied in this model and what kind of estimation biases it helps to prevent. Let's assume that the presence of *pesantren* Islamic schools is a good measure for local Sharia demand, so introduction is driven by the model that can be characterized as

$$RP_{rt} = \alpha'_r +' \lambda'_t + \eta schools_{rt} + x_{rt} + \nu_{rt}.$$

The variable x_{rt} is an unobserved endogenous confounder that also effects the outcome variable, and $E[x_{rt} schools_{rt}] \neq 0$. The shift-share IV assumes that schools/Sharia preference grow yearly at a rate that is the sum of an exogenous and an endogenous term:

$$schools_{rt} = schools_{r,0} \prod_{t=1..T} (\delta_t + \delta_{rt}), \tag{20}$$

where δ_t is the nation-wide growth rate of preference for Sharia and is exogenous, so $E[x_{rt}\delta_t] = 0$

and $E[\delta_t \delta_{rt}] = 0$, but $E[x_{rt}\delta_{rt}] \neq 0$. Then $schools_{rt}$ can be decomposed as

$$schools_{rt} = schools_{r0} \prod_{k=1..t} \delta_k + schools_{r0} \left[\sum_{l=2..t} \delta_{r,l-1} \prod_{m=1..l-1} \delta_m \prod_{n=l..t-1} (\delta_{n+1} + \delta_{r,n+1}) \right]$$

The first term $schools_{r.0} \prod_{k=1..t} \delta_k$ is the shift share. The second term contains all terms from Equation 20 that has at least one district level δ_{rt} term. If the exclusion restriction holds, each element of this expression is 0 in expectation and the sum is independent of the shift share. Let's call the second, endogenous term $\widetilde{schools}_{rt}$, then the first stage can be rewritten as:

$$RP_{rt} = \alpha'_r +' \lambda'_t + shiftshare_{rt} + \widetilde{schools}_{rt} + x_{rt} + \nu_{rt}$$

As $shiftshare_{rt}$ is independent of x_{rt} and $\widetilde{schools}_{rt}$, estimating instead the following first stage will yield consistent result:

$$RP_{rt} = \alpha'_r +' \lambda'_t + shiftshare_{rt} + \theta_{rt}$$

For a concrete example, suppose that increase in demand for Sharia is driven by the supply of clerics: national trends in religiosity and external factors (such as the yearly pilgrimage quota allocated to Indonesians) determine the overall number of people who can found Islamic schools (δ_t). Their location choice is, however, not exogenous, and quite plausibly they will also have economic considerations. If, for example, they move to places that experience an economic boom, δ_{rt} will be correlated with economic outcomes, such as GDP. The shift-share captures that clerics will have a higher probability of moving to areas with an existing Islamist networks (where $share_{0t}$ is high), which is not correlated with later economic outcomes (as shown in Bazzi et al., 2016).

The value added of the shift-share instrument in this setting is that it allows for persistent unobservable confounders, that is, divergent trends in outcomes of "treated" and "not treated" regions, as long as those trends are independent from the initial differences in religiosity and the country-wide overall trend.

Appendix C : Proofs of propositions

There are an incumbent and J challengers, whose behavior is taken as exogenous. In this setting, the only player is the incumbent, and we are considering his decision problem.

The incumbent maximizes

$$V = Pr(reelected) \times M - \omega N_E - s \cdot c \tag{21}$$

The challengers spends $N_B = \bar{M}/p$ on bribing voters, where $\bar{M}$ is their campaign budget and p is the cost of bribing a voter. The number of challengers can be endogenized by assuming that they enter the race until their expected utility is above some threshold, but this is not necessary to make the points about incumbent behavior. Votes are given by

$$Votes_I = (1-s) \times \underbrace{(\Delta\pi_E N_E + \pi_{NE} N)}_{\text{Inc. votes without Sharia}} + s \times \underbrace{(N_R + \Delta\pi_E^S N_E + \pi_{NE}^S (N - N_R))}_{\text{Inc. votes with Sharia}} + e_1, \tag{22}$$

$$Votes_j = (1-s) \times \underbrace{(\Delta\pi_B N_B + \pi_{NB} N)}_{\text{Chall. votes without Sharia}} + s \times \underbrace{(\Delta\pi_B N_B + \pi_{NB} (N - N_R))}_{\text{Chall. votes if inc. does Sharia}} + e_j. \tag{23}$$

If the turnout shocks e follow a Type-I extreme value distribution, the probability that any particular candidate wins will be given by the logit formula (McFadden et al., 1973). The decision variables of the incumbent are the public employment level $N_E \geq 0$ and the decision to implement the divisive policy, $s \in \{0, 1\}$. In the simplest version of the problem the incumbent is optimizing without a constraint, meaning that he can spend as much on public employment as he wants to. There will be, however, an implicit constraint, as he cannot have a reelection probability above 1 , so spending more than M/ω will result in a negative utility no matter what. He is better off not spending at all, given that the turnout shocks grant him a positive reelection probability even without spending.

In what follows I take the following steps:

1. Solve for the condition of the incumbent under which he chooses $s = 1$
2. I show that if he chooses $s = 1$, N_E will be set lower than in an alternative setting where the divisive policy is not available. This is the public morals / public services tradeoff.
3. Show how the condition in (1) depends on the economic parameters, M and ω.

The decision to introduce the religious policy

I first calculate optimal N_E given s, and than show which are the conditions under which $V(s=1) > V(s=0)$. This will be the condition to introduce the religious policy. Optimal public employment level is given by the first order condition in Equation 21:

$$\frac{\partial V}{N_E} : \frac{\partial P(reelected)}{\partial N_E} = \frac{\omega}{M}$$

The conditional logit formula for the winning probability is

$$P(reelected) = P_r = \frac{exp(\Delta\pi_E(s)N_E + \pi_{NE}(s)(N - s \cdot N_R) + s \cdot N_R)}{1 + exp(\Delta\pi_E(s)N_E + \pi_{NE}(s)N) + Jexp(\Delta\pi_B \frac{\bar{M}}{p} + \pi_{NB}(N - s \cdot N_R))} \tag{24}$$

From which the marginal probability takes the simple form $\frac{\partial P_r}{\partial N_E} = \Delta\pi_E(s)P_r(s)(1 - P_r(s))$. This means that the optimal public employment will be implicitly given by the quadratic equation

$$P_r^*(s) = \frac{1}{2} \pm \frac{1}{2}\sqrt{1 - 4\frac{\omega}{M \cdot \Delta\pi_E(s)}} \tag{25}$$

Where the second derivative test reveals that the smaller root is a local minimum, and the larger root is a local maximum.[128] This means that for a given parameter vector $(\omega, M, \Delta\pi_E(s))$ the incumbent will have a preferred reelection probability $P_r^*(s)$.

Lemma 1: $P_r^*(s=1) < P_r^*(s=0)$ **if** $\Delta\pi_E(s=1) < \Delta\pi_E(s=0)$**.**

The incumbent will target a lower reelection probability under the Sharia policy if introducing it makes clientele building less effective. This directly follows from the fact that $P_r^*(s)$ is increasing in $\Delta\pi_E(s)$. Similarly, $P_r^*(s)$ is increasing in M and decreasing ω. The bigger the stakes are at the election, the surer he wants to get in winning. If getting votes is more costly, the optimal reelection probability will be lower.

The identity $P_r^*(s) = P_r(s)$ gives implicitly $\Delta\pi_E(s)N_E$, the ex post clientele size of the incumbent:[129]

[128] There are no real roots if $\omega > \frac{1}{4}M \cdot \Delta\pi_E(s)$. If wages are high or office value is too low, the payoff will be a decreasing function of employment over its whole domain of N_E, so the incumbent will not employ anyone and his votes will be given by $\pi_{NE}(s)N + e$.

[129] With the approximation that the approximation that $log(1+x) \simeq log(x)$ for high enough x.

$$\Delta\pi_E(s)N_E = log\left(\frac{P_r^*(s)}{1-P_r^*(s)}\right) - s\cdot(1+\pi_{NB}-\pi_{NE}(s))\cdot N_R + (\pi_{NB}-\pi_{NE}(s))\,N + logJ + \Delta\pi_B\frac{\bar{M}}{p} \tag{26}$$

There are three main parts in this expression that behave differently under the decision to implement the divisive policy.

- The ex-post clientele size is smaller under the religious policy through the terms $log\left(\frac{P_r^*(s)}{1-P_r^*(s)}\right) - s\cdot(1+\pi_{NB}-\pi_{NE}(s))\cdot N_R$.[130] The incumbent knows that vote buying is less efficient, so he will set a lower target (first term), and he will want to economize on vote buying to extent that he gets the religious vote (second term)
- The incumbent takes into account that the non-employed people are less likely to vote for him under religious regulations, which gives an incentive to compensate for this loss. This is encompassed in the term $(\pi_{NB}-\pi_{NE}(s))\,N$, which is larger under the divisive policy.
- $logJ + \Delta\pi_B\frac{\bar{M}}{p}$ is the pressure of competitors, which does not depend on s.

A sufficient condition for the total effect of s on $\Delta\pi_E(s)N_E$ to be negative is that

$$(\pi_{NB}-\pi_{NE}(s=1))\,N < (1+\pi_{NB}-\pi_{NE}(s=1))\cdot N_R,$$

or $(\pi_{NB}-\pi_{NE}(s=1))\,(N-N_R) < N_R$, which is intuitive: the incumbent can only rely on the hardline voter community if its size is higher than the number of votes a challenger gets "for free" from among the moderate population. If the religious community is smaller than that, then the incumbent would have to employ more people and also pay the fixed cost of divisive policies, so in this case he will prefer purely clientelist competition.

What we are after is whether the incumbent employs more or less people under the divisive policy, that is, the sign of $\Delta N_E = N_E(s=1) - N_E(s=0)$. To determine that, let's denote $\Delta\pi_E(s=1) = \Delta\pi_E(s=0) + \Delta_s$, where Δ_s is the difference in the additional voting probabilities in public employment under $s=0$ and $s=1$. Then we can can write up the definition of ΔN_E in terms of ex post clientele sizes observed in Equation 26:

[130] The term $log\left(\frac{P_r^*(s)}{1-P_r^*(s)}\right)$ is monotonically increasing in P_r^*, so it will be smaller if $s=1$.

$$\Delta N_E = \frac{1}{\Delta\pi_E(s=1)}\left(\Delta\pi_E(s=1)N_E(s=1) - \Delta\pi_E(s=0)N_E(s=0)\right) - \frac{\Delta_s}{\Delta\pi_E(s=1)}N_E(s=0)$$

Equation 26 gives the first term:

$$\Delta\pi_E(s=1)N_E - \Delta\pi_E(s=0)N_E = C_P + C_N + C_R$$

The difference in ex-post clientele sizes is given by the sum of the probability channel, the population channel and the hardline voter channel. These describe the competing forces at work in choosing optimal employment as $C_P + C_R$ will have different signs as C_N.

The probability channel $C_P = log\left(\frac{P_r^*(s=1)}{1-P_r^*(s=1)}\right) - log\left(\frac{P_r^*(s=0)}{1-P_r^*(s=0)}\right)$ is negative: the incumbent wants to employ less people under the divisive policy because he knows he is less efficient at it.

The hardline vote channel $C_R = -(1 + \pi_{NB} - \pi_{NE}(s=1)) \cdot N_R$ is negative: the incumbent wants to employ less people as he knows he gets the hardline vote with certainty.

The population channel $C_N = -\left(\pi_{NE}(s=1) - \pi_{NE}(s=0)\right)N$ is positive: the incumbent also gets less votes from the non-committed voters, as they dislike the religious policy. He wants to compensate this with additional employment.

$$\Delta N_E = \frac{1}{\Delta\pi_E(s=1)}\left(C_P + C_N + C_R - \Delta_s N_E(s=0)\right)$$

This will be smaller than zero if $-C_P - C_R > -\Delta_s N_E(s=0) + C_N$. The left hand side is the number of employees he wants to cut because of the hardline votes come in ($-C_R$) and because vote buying is now less efficient (-C_P). The right hand side is number of voters he would lose given he employed the same amount of people as without policy ($-\Delta_s N_E(s=0)$) and the number he would lose because the non-committed voters vote for him with a smaller probability (C_N). Intuitively, the incumbent considers whether setting $s=1$ on the net gives him more votes or less votes. If he is on the positive side, he will adjust employment downward accordingly.

This shows that he will cut employment under the divisive policy in most cases, but it remains to be seen whether there can be cases where $\Delta N_E > 0$ and $s=1$ simultaneously.

The incumbent will introduce the religious policy if $V(s=1) > V(s=0)$, or $M\Delta P_r^* - \omega\Delta N_E - c > 0$. From this N_R can be expressed to show that the incumbent introduces the divisive policy if the size of the hardline voter community exceeds a certain threshold $N_R > \underline{N_R}$

$$\underline{N_R} = \rho\left((C_P + C_N - \Delta_s N_E(s=0)) - \frac{\Delta\pi_E(s=1)}{\omega} M\Delta P_r^* + c\right) \tag{27}$$

All terms are functions of parameters of the model only. We now can show that the incumbent who introduces the divisive policy will cut back employment.

Proposition 1: the incumbent employs less people if he introduces the religious policy.

Proof.

The incumbent would employ *more* people in this case if $-C_P - C_R < -\Delta_s N_E(s=0) + C_N$. On the other hand, Equation 27 implies that $-C_P - C_R > -\Delta_s N_E(s=0) + C_N - \frac{\Delta\pi_E(s=1)}{\omega} M\Delta P_r^* + c$. This can only hold if $-\frac{\Delta\pi_E(s=1)}{\omega} M\Delta P_r^* + c < 0$. However, Lemma 1 showed that $\Delta P_r^* < 0$, meaning that this expression is positive. So if the incumbent introduces the divisive policy, it means that he cuts back employment.

Proposition 2: The threshold community size at which the incumbent introduces Sharia increases in M, decreases in ω.

Proof: Office value M.

The first part of the propostion amounts to showing that $\partial\underline{N_R}/\partial M > 0$. There are three terms in Equation 27 that depend on M. These are C_P, $-\Delta_s N_E(s=0)$, and $-\frac{\Delta\pi_E(s=1)}{\omega} M\Delta P_r^*$.

The parameter M only enters the second term through P_r^*, so we only have to consider $\frac{-\Delta_s}{\Delta\pi_E(s=0)} log\left(\frac{P_r^*(s)}{1-P_r^*(s)}\right)$. As $C_P = log\left(\frac{P_r^*(s=1)}{1-P_r^*(s=1)}\right) - log\left(\frac{P_r^*(s=0)}{1-P_r^*(s=0)}\right)$, it is convenient to investigate the two terms together. We have to decide if $log\left(\frac{P_r^*(s=1)}{1-P_r^*(s=1)}\right) - \left(1 + \frac{\Delta_s}{\Delta\pi_E(s=0)}\right) log\left(\frac{P_r^*(s=0)}{1-P_r^*(s=0)}\right)$ is increasing in M.

Given the definition if Δ_s, this simplifies to $log\left(\frac{P_r^*(s=1)}{1-P_r^*(s=1)}\right) - \left(\frac{\Delta\pi_E(s=1)}{\Delta\pi_E(s=0)}\right) log\left(\frac{P_r^*(s=0)}{1-P_r^*(s=0)}\right)$.

Let's call $log\left(\frac{P_r^*}{1-P_r^*}\right) = f(M \cdot \Delta\pi_E(s))$ where f is monotonically increasing over $M \cdot \Delta\pi_E(s)$. Then we can reformulate the terms as

$$f(M \cdot \Delta\pi_E(s=1)) - \left(\frac{\Delta\pi_E(s=1)}{\Delta\pi_E(s=0)}\right) f(M \cdot \Delta\pi_E(s=0))$$

From that the derivative with respect to M will be

$$f'\Delta\pi_E(s=1) - \left(\frac{\Delta\pi_E(s=1)}{\Delta\pi_E(s=0)}\right) f'\Delta\pi_E(s=0)$$

Which is simply $f'\Delta\pi_E(s=1) - f'\Delta\pi_E(s=1)$, and this term is zero. The divisive policy decreases optimal reelection probability through making clientelism less effective, so the bar that has to be jumped is lowered. This enters the decision to introduce the religious policy through two channels. First, the bar is set lower, so a smaller hardline base is enough to jump it. Second, if the competition in general requires large clienteles to be built ($N_E(s=0)$ is high), and clients dislike the divisive policy a lot ($-\Delta_s$ is high), he only wants to introduce the religious policies if the size of the religious community is large. These two channels that work through efficiency of clientelism offset each other on the margin.

It is left to see that $-\frac{\Delta\pi_E(s=1)}{\omega} M\Delta P_r^*$ is increasing in M. The derivative $\frac{1}{\omega}\left(-\Delta P_r^* - \frac{\partial\Delta P_r^*}{\partial M} M\right)$. $-\Delta P_r^*$ is positive given Lemma 1. From the definition, the sign of $-\frac{\partial\Delta P_r^*}{\partial M_2}$ remains to be seen. From Equation 25 we know that

$$-\Delta P_r^*(s) = \frac{1}{2}\sqrt{1 - 4\frac{\omega}{M\cdot\Delta\pi_E(s=0)}} - \frac{1}{2}\sqrt{1 - 4\frac{\omega}{M\cdot\Delta\pi_E(s=1)}}$$

The derivative of which is

$$-\frac{\partial\Delta P_r^*}{\partial M} = \frac{\omega}{2M^2}\left(\frac{1}{\Delta\pi_E(s=0)\sqrt{1-\frac{4\omega}{\Delta\pi_E(s=0)M}}} - \frac{1}{\pi_E(s=1)\sqrt{1-\frac{4\omega}{\Delta\pi_E(s=1)M}}}\right)$$

which, given $M > 0$ is positive if

$$\left(\frac{1}{\Delta\pi_E(s=0)\sqrt{1-\frac{4\omega}{\Delta\pi_E(s=0)M}}} - \frac{1}{\Delta\pi_E(s=1)\sqrt{1-\frac{4\omega}{\Delta\pi_E(s=1)M}}}\right) > 0$$

Which can be rearranged into the following condition:

$$\Delta\pi_E(s=1) + \Delta\pi_E(s=0) > \frac{4\omega}{M}$$

In an interior solution with positive employment (see footnote 128) we also have to have that

$$1 - 4\frac{\omega}{M_2\cdot\Delta\pi_E(s)} > 0$$

From which

$$\Delta\pi_E(s) > \frac{4\omega}{M_2}$$

From which it follows that for every $\Delta\pi_E(s=1)$ or $\Delta\pi_E(s=0)$ that is part of an interior solution

$$\Delta\pi_E(s=1) + \Delta\pi_E(s=0) > \frac{4\omega}{M_2}$$

So the threshold defined in Equation 27 is increasing in M.

Proof: Employment cost ω

The first part of the argument that it is decreasing in ω, is identical before, as ω enters the C_P and $N_E(s=0)$ the same way, except that the logarithmical terms are monotonically decreasing in ω. We still have show that $-\frac{\Delta\pi_E(s=1)}{\omega}M\Delta P_r^*$ is decreasing in ω. The partial derivative is $-M\left(\frac{-1}{\omega^2}\Delta P_r^* + \frac{1}{\omega}\frac{\partial\Delta P_r^*}{\partial\omega}\right)$. The first term is negative, given Lemma 1. The partial derivative of the second term is:

$$-\frac{\partial\Delta P_r^*}{\partial\omega} = \frac{-4}{M}\left(\frac{1}{\Delta\pi_E(s=0)\sqrt{1-\frac{4\omega}{\Delta\pi_E(s=0)M}}} - \frac{1}{\pi_E(s=1)\sqrt{1-\frac{4\omega}{\Delta\pi_E(s=1)M}}}\right)$$

Since $\frac{-4}{M}$ is negative, the expression will be negative if the second term is positive, which had been shown in the previous derivation.

Appendix D : Political behavior

Table D.2: Total incumbent votes and incumbent decisions

	Incumbent votes					
	(1)	(2)	(3)	(4)	(5)	(6)
RP before election	19429.5	11729.0	22129.6	14621.1	9838.9	-4099.4
	(13746.7)	(13591.1)	(14460.3)	(14287.7)	(14263.7)	(13841.6)
Electorate size	0.175***	0.164***	0.174***	0.164***	0.159***	0.154***
	(0.0243)	(0.0231)	(0.0234)	(0.0223)	(0.0249)	(0.0242)
Implicit employment	0.157***	0.171***	0.158***	0.170***	0.158***	0.171***
	(0.0545)	(0.0528)	(0.0514)	(0.0494)	(0.0529)	(0.0520)
Implicit employment × RP	-0.286**	-0.284**	-0.288**	-0.285**	-0.311**	-0.314**
	(0.135)	(0.125)	(0.130)	(0.120)	(0.139)	(0.130)
Electorate × RP	0.123**	0.131**	0.104*	0.109**	0.131**	0.130**
	(0.0556)	(0.0514)	(0.0532)	(0.0490)	(0.0597)	(0.0559)
Number of schools			1.749	0.0741		
			(15.78)	(15.97)		
Number of schools × RP			29.98	33.78		
			(35.56)	(34.79)		
Log schools					4417.9**	2766.2
					(1767.4)	(1932.8)
Log(schools+1) × RP					2312.7	5361.9
					(6172.5)	(5986.8)
Inc. has degree		14931.3**		16040.5**		15830.2**
		(6582.0)		(6488.3)		(6472.8)
Inc. is doctor		1905.7		2408.5		3685.7
		(6732.3)		(6678.3)		(6849.2)
Inc. is haji		22667.5***		21928.3***		20577.8***
		(6490.3)		(6533.5)		(6924.8)
Observations	530	530	530	530	530	530

The table shows OLS regressions of incumbent votes the size of the electorate (calculated from the population), the implicit employment variable (wage bill / minimum wage), different measures of penetration by Islamic schools, and their interactions with a dummy indicating if the incumbent introduced a religious policy in his or her cycle. Additional controls are characteristics of the incumbent that are identified from his or her full name: whether had any degree that is not a doctorate, whether had a doctorate, and whether he or she has completed the pilgrimage to Mecca (those who did can use the honorific title of Haji or Hajjah). Election data are hand-collected. Religious policy information is based on Buehler (2016). Demographics come from INDODAPOER, the number of Islamic schools are from PODES. Robust standard errors in parenthesis. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table D.3: Individual voting behavior

	(1) Pr(voted, district head)	(2) Pr(voted, village head)	(3) Pr(voted, legislature)
Public employee	0.0816***	-0.00818	0.0166
	(0.0228)	(0.0305)	(0.0233)
Devout	0.00819	-0.00679	-0.00517
	(0.0137)	(0.0151)	(0.0126)
Religious policy	-0.00988	0.0130	-0.00285
	(0.0182)	(0.0237)	(0.0162)
Public employee × RP	-0.0635*	-0.000896	-0.0536
	(0.0366)	(0.0569)	(0.0361)
Devout × RP	0.0705***	-0.0677	-0.000617
	(0.0269)	(0.0417)	(0.0225)
Observations	8958	7087	9141

The table shows OLS regressions of electoral participation on different characteristics of the voter. The data come from Wave 5 of the Indonesian Family Life Survey (2014). 50% of the respondents lived in a district which had a religious policy. The sample includes all Muslim respondents who lived in a district that did not have a religious policy in 2007. The outcome variable is a dummy indicating if the respondent says he or she voted during the last election for district head (Column 1), village head (Column 2), national legislature (Column 3). Additional controls include demographics (age, marital status, sex, years of schooling), province dummies, Islamic tradition dummies (Muhammadiyah and "Other", Nahdlatul Ulama is baseline), dummies indicating if the person moved between survey waves, whether voted in 2007, whether person lived in an urban area in 2013 and in 2007, and whether the person was a government worker in 2007. Standard errors are clustered at the district level. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Appendix E : Voter preferences

In Table E.4 I regress the dummy for Sharia regulations on crime indicators. In Columns 1-2 the unit of observation is a Muslim respondent in the IFLS in 2007 who lived in a district that did not have a religious policy.[131] The dependent variable is a dummy indicating if the district had a religious policy in 2013. In Columns 3-4 the same is regression is run on district averages. No significant coefficients are found, except for a negative one for property crimes in Column 2, indicating that if a person indicated that he or she suffered a property crime, such as theft or damaged property, it is less likely the same district would have a Sharia regulation by 2013. There is no evidence that districts that had higher crime in 2007 would end up having Sharia policies.

In Table E.5 I regress different measures of religious participation on a dummy indicating whether the person lived in a district that had a religious policy. In this case the sample is all Muslim respondents who lived in a district that had its first religious policy between 2007 and 2013. The controls include demographics, such as age, sex, marital status, a dummy for living in an urban area, years of schooling, province dummy, and Islamic tradition dummies. The outcome variables are sets of dummies, indicating whether 1) the respondent received a donation from a religious organization 2) if the respondent took part in a religious microfinancing group 3) if any general religious event occured in the village 4) if the respondent participated in that event. If districts which introduce Sharia policies substitute government production of public services with religious production, we would observe an increase in religious participation. If anything, there is week evidence that religious participation is weaker in these districts.

In Table Appendix E I regress a dummy indicating that the district had a Sharia regulation in 2013 on different levels of perceived corruption in 2007, for districts that had no religious policy that year. The units of observation are "informants" for the IFLS community survey (local authority figures who are not politicians or public administrators, so do not have a vested interest in a good result). There is one village in the community survey for every district, and (mostly) two informants for every village. The respondent asks whether corruption is present (Panel A) or corruption got worse since 2000 (Panel B) in different layers of public administration (columns). The only significant coefficients are for district heads and district parliaments (Columns 3 and 4) and the coefficients indicate that districts which report corruption in 2007 are less likely to have a religious policy in 2013. There is no evidence that people demand Sharia regulations because

[131] Only the 2007 IFLS wave had information on crime.

Table E.4: Crime perception in 2007 and probability of Sharia regulations in 2013

	(1)	(2)	(3)	(4)
Any crime	-0.0441		0.174	
	(0.0289)		(0.172)	
Violent crime		-0.0358		0.224
		(0.0432)		(0.199)
Property crime		-0.108**		0.162
		(0.0495)		(0.497)
Constant	0.345***	0.343***	0.239***	0.247***
	(0.0682)	(0.0683)	(0.0553)	(0.0458)
Observations	12712	12712	159	159
Unit	Person	Person	Region	Region

Table showls OLS regressions of the incidence of religious policies in 2013 in a district on different crime perception measures. Columns 1-2 use individual level data of respondents in the IFLS. The sample is all Muslim respondents who lived in 2007 in a district without a Sharia policy. The standard errors clustered at district borders in parenthesis for personal data; robust standard errors for district data. The question was whether the respondent or his or her family was a victim of crime. Columns 3-4 show responses from "informants" in the community facility survey. The sample is all districts that had no religious policy in 2007. Robust standard errors in parenthesis. The question is whether the respondent's village experienced incidence of the given crime. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table E.5: Religious participation and Sharia regulations

	(1) Pr(Donation)	(2) Pr(Arisan)	(3) Pr(Event occurence)	(4) Pr(Participation)
Religious policy	-0.00580*	-0.0179*	-0.0141	-0.00662
	(0.00348)	(0.0102)	(0.0192)	(0.0306)
Observations	13631	13667	13598	13598

The table contains OLS regressions of individual religious participation measures of respondents in the IFLS on a dummy indicating if they lived in a district with a religious policy in 2013. Sample: all Muslim respondents who lived in districts which had their first religious policy between 2007 and 2013.
Standard errors clustered at district borders in parenthesis. The outcomes are dummies indicating if the person received any donation from a religious organization (Column 1), participated in a religious microfinance group (Column 2), if religious events happened in the village (Column 3) and if the respondent participated (Column 4). *: significant at 10%; **: significant at 5%; ***: significant at 1%.

of corruption waves. This is in contrast with the results in Henderson and Kuncoro (2011) who find that this link existed in national elections in the 2000s.

Panel A

Table E.6: Corruption incidence in 2007 and religious policies by 2013

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CorruptionPresent	0.100	-0.0625	-0.0934*	-0.113**	0.00226	-0.164**	-0.0584
	(0.0798)	(0.0743)	(0.0508)	(0.0492)	(0.0550)	(0.0659)	(0.0688)
Observations	331	331	331	331	331	331	331
definition	Village gov.	Subdist gov.	Dist. gov.	Dist. parl.	Dist. police	Healthcare	State schools

Robust standard errors in parenthesis.

Panel B

Table E.7: Corruption trends in 2007 and religious policies by 2013

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
GotWorse	0.0569	-0.0286	-0.111*	-0.186***	-0.0219	-0.00540	0.0310
	(0.160)	(0.156)	(0.0642)	(0.0560)	(0.0830)	(0.137)	(0.131)
Observations	331	331	331	331	331	331	331
definition	Village gov.	Subdist gov.	Dist. gov.	Dist. parl.	Dist. police	Healthcare	State schools

Robust standard errors in parenthesis.

The tables show OLS regression of the religious policy dummy in 2013 on Informant respondents in the IFLS community survey in 2007. The sample is all districts that had no religious policies in place in 2007. The explanatory variable in Panel A is a dummy indicating if the respondent said that corruption was present in a specific layer of government (each column corresponds to a different layer). The explanatory variable in Panel B is a dummy indicating if the respondent said that the situation in terms of corruption got worse since 2007. Robust standard errors in parenthesis. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

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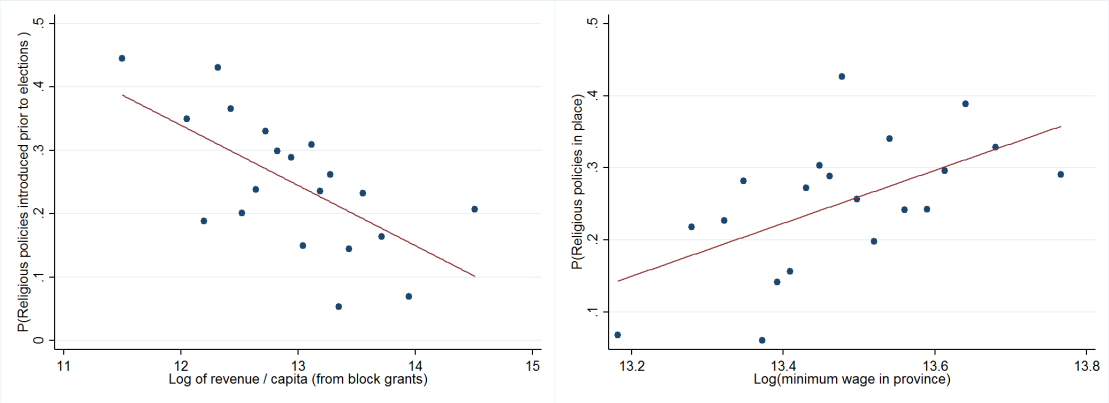
Appendix F : Additional figures and tables

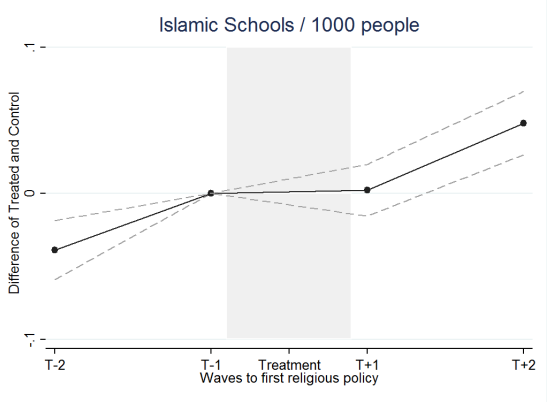
Stylized facts

Figure F.1: Determinants of Sharia regulations: model predictions



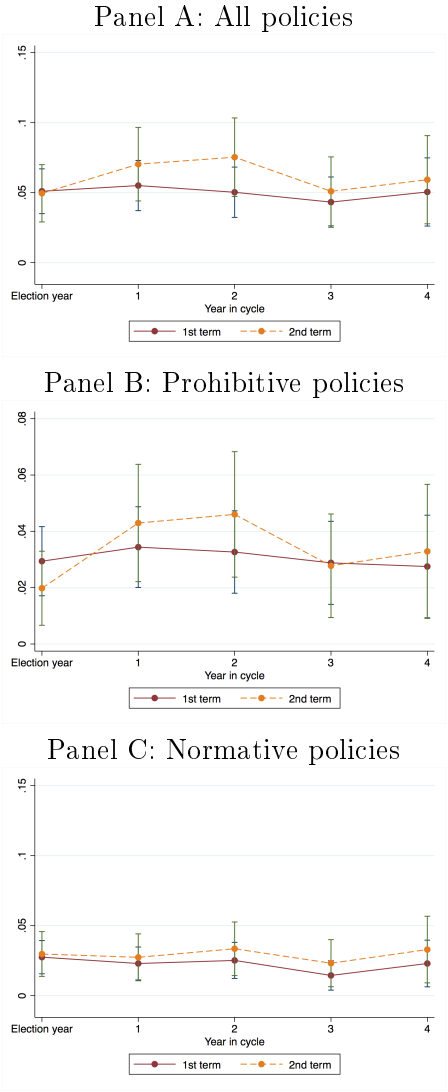
The figures show the occurrence of a religious policy (Y axis) against the revenues and the minimum wage in a binned scatterplot. The probability of religious policies is decreasing in reliable revenue streams ($\hat{\beta}$: -.10,t : -4.78), increases in wages ($\hat{\beta}$: .36,t : 2.85). Sample: district-election cycle observations, Controls: logs of population, GDP/cap, Block grant /cap (with wages only); year dummies

Figure F.2: Trends in school Islamic presence relative to religious policy introduction



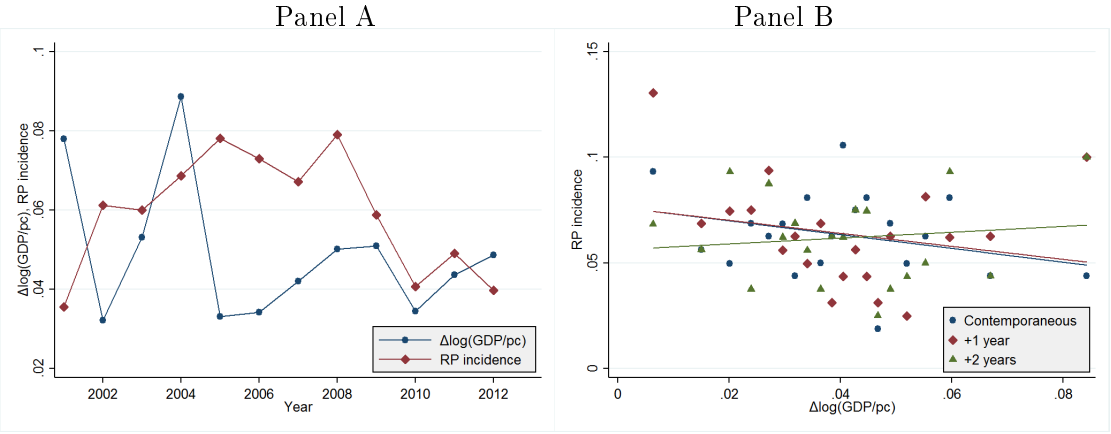
The figure shows an event study where the outcome variable is the number of Islamic schools per 1000 inhabitants in a village. Data come from the PODES survey. The figure uses village - survey wave observations. An event is defined as the village being in a district that introduced the first religious policy between the two survey waves. The survey takes place together with every national census, in every three years on average. The control variables are village fixed effects, calendar year dummies and log of population. The confidence interval is based on standard errors clustered at the district level. The figures show that villages that would introduce religious policies experienced an increasing trend in the number of religious policies in the village. 95% confidence bands included.

Figure F.3: When are religious policies passed?



The figures show the share of month-district observations within political cycles when Sharia regulations are introduced. 95% confidence bands included.

Figure F.4: Economic shocks and religious policy incidence



The first panel shows the average growth rate of the economy and the share of districts which introduced a religious policy each year. The vertical axis of the second panel shows yearly changes in per capita GDP, while the horizontal axis shows the probability of introducing Sharia policies in the same year, one year after, and two years after. The figures show that there is no obvious correlation between economic performance and the religious turn.

Effect heterogeneity

Figure F.5: Central vs peripheral villages in districts with RP

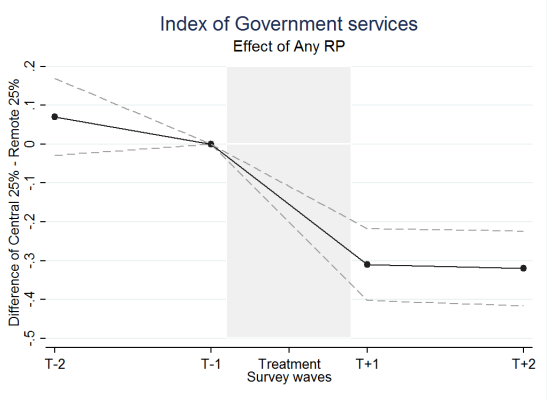
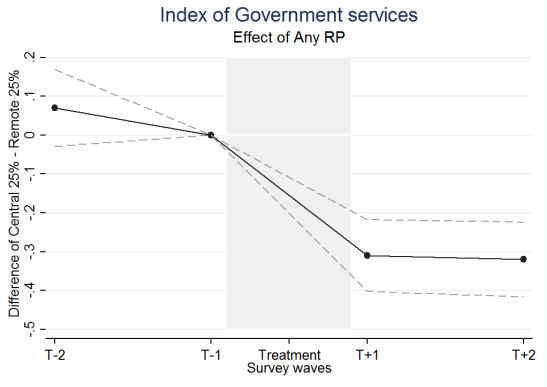
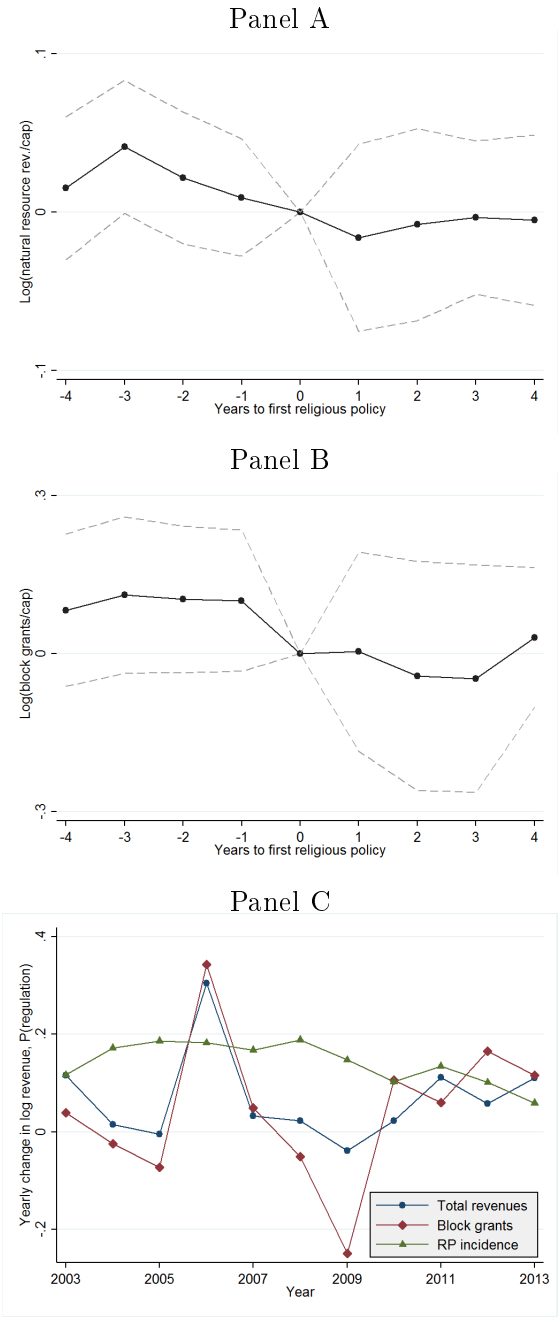


Figure F.6: Villages with and without local police in districts with RP



In this specification the sample is the villages in districts that had a religious policy between two survey waves. The event is defined as being in the quartile that is closest to the district center, while the control group is the most remote quartile in panel A. The event is defined as being in in a village that had a police unit which is supervised by the district government in panel B. The 95% confidence interval is based on standard errors clustered at the district level.

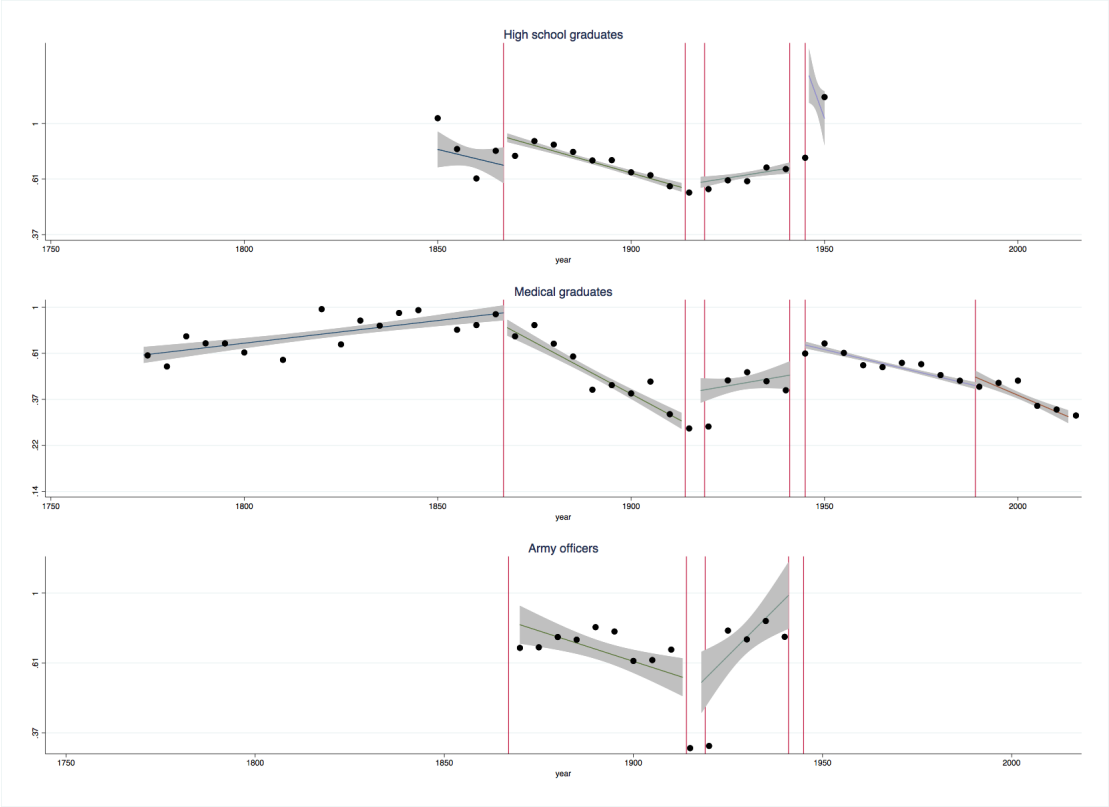
Figure F.7: Revenue event studies



Panel A and B replicate Figure 4 but the outcome variable is now revenue, not expenditure (revenue from natural resources and from bloc grants, respectively). 95% confidence intervals based on clustered standard errors (at district level) shown. Panel C plots the yearly change in different revenue sources and the yearly incidence of religious policies against time.

Additional Figures to Chapter 2

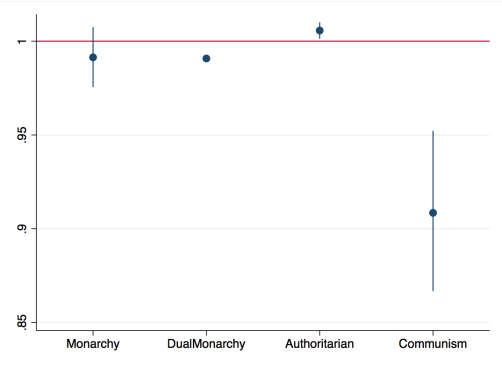
Figure F.8: Mean social status (reference group: "-i names")



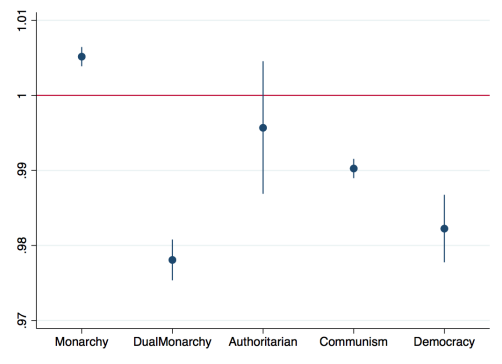
The figure plots the natural logarithm of status advantage against time and fits a line for each regime according to 13. The slopes are the estimated $ln(\hat{\beta})$ coefficients. The implied $\hat{\beta}$s are shown in Figure 23. 95% confidence bands included.

Figure F.9: Comparison of $\hat{\beta}$ coefficients (reference group: population)

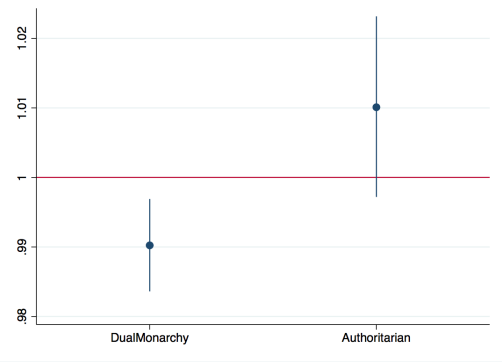
Panel A: High school graduates



Panel B: Medical doctors

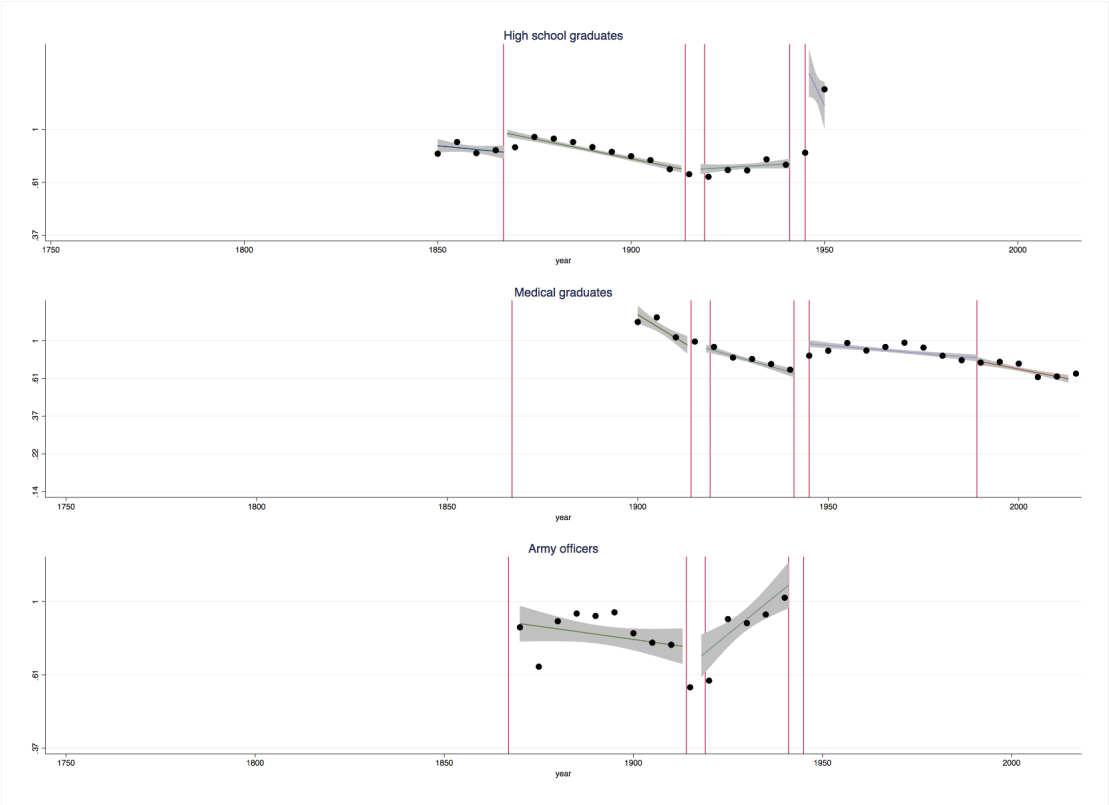


Panel C: Officers



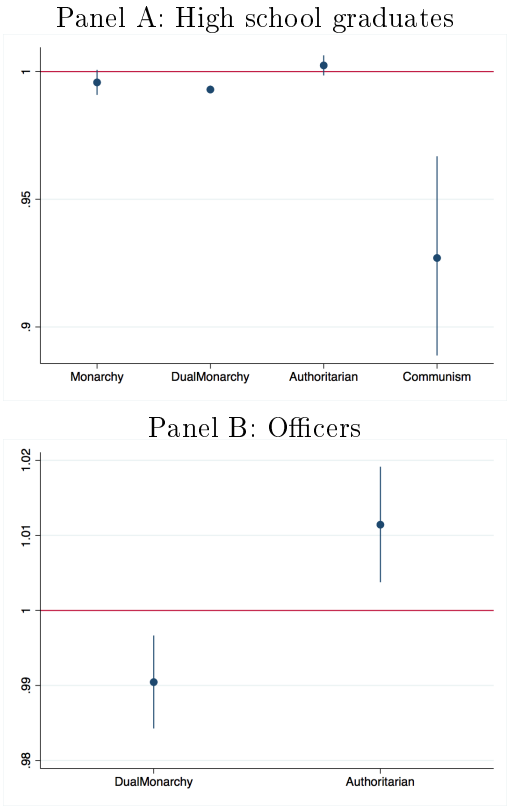
The figures show estimated $\hat{\beta}$ coefficients. The horizontal line corresponds $\hat{\beta}$=1. $\hat{\beta} > 1$ means that group status diverges from the mean; $\hat{\beta}$<1 mean sthat it regresses to the population mean. 95% confidence bands included.

Figure F.10: Mean social status (reference group: top20 names)



The figure plots the natural logarithm of status advantage against time and fits a line for each regime according to 13. The slopes are the estimated $ln(\hat{\beta})$ coefficients. The implied $\hat{\beta}$s are shown in Figure 23. Note: no Top20 names among doctors prior to WWI, and very few after 1945. 95% confidence bands included.

Figure F.11: Comparison of $\hat{\beta}$ coefficients (reference group: population)



The figures show estimated $\hat{\beta}$ coefficients. The horizontal line corresponds $\hat{\beta}$=1. $\hat{\beta} > 1$ means that group status diverges from the mean; $\hat{\beta}$<1 mean sthat it regresses to the population mean. Note: no Top20 names among doctors prior to WWI, and very few after 1945. 95% confidence bands included.

Heterogeneity analysis

Table F.8: Impact of religious policies in current and later cycles

Panel A

	(1) Log(wbill)	(2) Log(imp. emp.)	(3) Imp. emp. / pop.
Has RP	0.00496	0.00857	0.00340
	(0.0188)	(0.0179)	(0.0337)
Has RP and RP in current cycle	-0.0951***	-0.0955***	-0.120***
	(0.0159)	(0.0150)	(0.0274)
Observations	3383	3511	3511
MeanY	25.53	12.82	1.689

Panel B

	(1) Poverty rate	(2) Poverty gap	(3) Log(No. of incidents)
Has RP	1.161***	0.222***	0.242*
	(0.302)	(0.0694)	(0.135)
Has RP and RP in current cycle	-0.910***	-0.204***	-0.142
	(0.260)	(0.0628)	(0.0990)
Observations	3914	3914	889
MeanY	15.07	2.618	3.458

The main independent variables are an indicator dummy if the region has a Sharia regulation, and another indicating if it happened in the current cycle (both lagged).
Panel A shows a Fixed Effect regressions of different expenditure measures of the district government on the independent variables. Regional and calendar year fixed effects, income, GDP (lagged values of GDP/capita and its growth rate), election dummies, population (in logs, twice lagged) are included as controls. Panel B shows Fixed Effect regressions of different social outcomes on a dummy indicating if the district had a religious policy the year before the outcome was observed. The poverty rate is the % of people living under the poverty line, the poverty gap is an index measure showing how far is the averasge poor person is living from the poverty line (both come from the INDODAPOER data). The specifications have regional fixed effects, income, GDP (lagged values of GDP/capita and its growth rate), calendar year dummies, election dummies, population (in logs, twice lagged) are included as controls. Log(event count) data come from the national violence monitoring system; coverage is smaller than with the INDODAPOER data. Controls in this case include fixed effects, lagged poverty and lagged inequality data. Standard errors are clustered at the district level in all models. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table F.9: Perpetrator types

Panel A: P(perpetrator)

	(1) Religious group	(2) Government	(3) Other political
Had RP (T-1)	0.194**	0.0369	-0.0346
	(0.0888)	(0.0686)	(0.0775)

Panel B: Number of incidents by perpetrator

	(1) Religious group	(2) Government	(3) Other political
Had RP (T-1)	0.451***	1.351	4.330
	(0.159)	(1.167)	(2.817)

Panel C: Number of victims / 100000 people by perpetrator

	(1) Religious group	(2) Government	(3) Other political
Had RP (T-1)	0.0874*	1.688	2.315
	(0.0477)	(1.422)	(2.110)
Observations	921	921	921

The table shows a similar specification to Table 6, but instead of the incidence of violence, the outcome variables are grouped by perpetrators. In Panel A the outcomes of dummies indicating if the district had a violent act by the given perpetrator. Panel B shows the number of incidents by the specific petpetrator. Panel C shows the number of victims by 100.000 inhabitants by perpetrator. Note: not all perpetrators belong to these groups or any group, so the numbers do not have to add up to the coefficients from the previous table. District and time fixed effects included. Standard errors clustered at district borders. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table F.10: Incident types

Total number of victims by incident type

	(1) Killed	(2) Injured	(3) Kidnapped	(4) Sexually assaulted
Had RP (T-1)	3.317***	20.57*	0.423**	4.759
	(0.835)	(10.56)	(0.211)	(3.000)

Victims / 100000 people by incident type

	(1) Killed	(2) Injured	(3) Kidnapped	(4) Sexually assaulted
Had RP (T-1)	0.631***	6.752*	0.109	1.743***
	(0.192)	(3.944)	(0.0838)	(0.654)
Observations	921	921	921	921

The table shows a similar specification to Table 6, but instead of the incidence of violence, the outcome variable is the number of victims by incident types in Panel A, and number of victims per 100.000 inhabitants in Panel B. District and time fixed effects included. Standard errors clustered at district borders. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Village level heterogeneity in the effect of religious policies

Table F.11: Distance interactions

	(1) Gov. services	(2) P(slum)
Religious policy	-0.260***	0.0259***
	(0.0477)	(0.00906)
Any RP X Dist(100km)	0.579***	-0.0340
	(0.119)	(0.0212)
Observations	273432	273432
distance	0.449	0.760
distanceSE	0.0860	0.356

Standard errors clustered at 1996 regional borders in parantheses.
Controls:Village+Time FE, Log(Islamic schools),Log(Pop)

This table shows the same regression as Table 4 but also includes the distance from the district center (in 100 kms) interacted with the religious policy dummy. Standard errors are clustered at the district level. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table F.12: Content heterogeneity

	(1) infra	(2) Gov. services	(3) P(slum)
General RP	-0.180***	-0.151***	0.0103
	(0.0692)	(0.0516)	(0.0110)
Prohibitive RP	0.0511	-0.00199	0.0136*
	(0.0548)	(0.0456)	(0.00749)
Observations	273450	273450	273450

Standard errors clustered at 1996 regional borders in parantheses.
Controls:Village+Time FE, Log(Islamic schools),Log(Pop)

This table shows the same regression as Table 4 but instead of a single dummy on the religious policies, two separate dummies are used for the presence of Normative and Prohibitive regulations. Standard errors are clustered at the district level. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table F.13: Content heterogeneity + distance interaction

	(1) Gov. services	(2) P(slum)
General RP	-0.165***	0.0210
	(0.0621)	(0.0145)
General RP X Dist(100km)	0.0170	-0.0335
	(0.159)	(0.0306)
Prohibitive RP	-0.185***	0.0155*
	(0.0558)	(0.00916)
Prohibitive RP X Dist(100km)	0.623***	-0.00708
	(0.144)	(0.0210)
Observations	273432	273432
distance1	9.714	0.627
distanceSE1	88.67	0.443
distance2	0.297	2.184
distanceSE2	0.0745	5.845

Standard errors clustered at 1996 regional borders in parantheses.
Controls:Village+Time FE, Log(Islamic schools),Log(Pop)

This table shows the same regression as Table 4 but uses regulation type dummies instead of a single regulation dummy, and their interactions with the distance from the district center (in 100 kms) . Standard errors are clustered at the district level. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Assessing alternative causal mechanisms in the DID

Candidate quality

Table F.14: Titles of incumbents

	Introducing	Non-introducing	Difference
N	129	693	
Pilgrim share	.76	.60	.16
s.e.			(.0462)
Doctor share	.43	.35	.08
s.e.			(.0168)
Other honorific title	.56	.46	.10
s.e.			(.0478)

This table compares titles of incumbents who introduce Sharia policies to titles of incumbents who do not. The data are limited to incumbents for whom I was able to collect election data. The first row corresponds to the share of incumbents who completed the pilgrimage to Mecca and thus earned the title "Haji" or "Hajjah". The second row corresponds to share of incumbents with a doctoral title. The third row shows the share of incumbents who had any other title (Bachelor's or Master's Degree, clerical titles etc.).

Government expenditure and clientele regressions

Table F.15: Inequality control

Panel A: spending, all years

	(1) Log(wbill)	(2) Log(imp. emp.)	(3) Imp. emp. / pop.
Religious policy in cycle	-0.0348*	-0.0384**	-0.0365
	(0.0189)	(0.0182)	(0.0319)
Observations	3165	3185	3185
MeanY	25.53	12.82	1.689

Panel B: spending, direct elections

	(1) Log(wbill)	(2) Log(imp. emp.)	(3) Imp. emp. / pop.
Religious policy in cycle	-0.105***	-0.110***	-0.162***
	(0.0293)	(0.0289)	(0.0440)
Observations	2061	2081	2081
MeanY	25.53	12.82	1.689

Panel C: poverty indicators

	(1) Poverty rate	(2) Poverty gap	(3) Poverty rate	(4) Poverty gap
Religious policy	0.948***	0.177***	-1.833***	-0.472***
	(0.295)	(0.0676)	(0.699)	(0.144)
Years after RP			0.338***	0.0795***
			(0.0819)	(0.0178)
Observations	3186	3186	3145	3145
MeanY	15.07	2.618	15.07	2.618

This is a version of Table 3 where I also control for lagged measures of inequality. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table F.16: Unemployment rate control

Panel A: spending, all years

	(1) Log(wbill)	(2) Log(imp. emp.)	(3) Imp. emp. / pop.
Religious policy in cycle	-0.0415**	-0.0448***	-0.0568*
	(0.0177)	(0.0170)	(0.0293)
Observations	3050	3067	3067
MeanY	25.53	12.82	1.689

Panel B: spending, direct elections

	(1) Log(wbill)	(2) Log(imp. emp.)	(3) Imp. emp. / pop.
Religious policy in cycle	-0.118***	-0.122***	-0.193***
	(0.0303)	(0.0299)	(0.0454)
Observations	2242	2259	2259
MeanY	25.53	12.82	1.689

Panel C: poverty indicators

	(1) Poverty rate	(2) Poverty gap	(3) Poverty rate	(4) Poverty gap
Religious policy	0.916***	0.188**	-2.197***	-0.517***
	(0.339)	(0.0792)	(0.817)	(0.189)
Years after RP			0.360***	0.0826***
			(0.0867)	(0.0207)
Observations	3063	3063	3023	3023
MeanY	15.07	2.618	15.07	2.618

This is a version of Table 3 where I also control for lagged measures of the unemployment rate. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table F.17: Sectoral change control

Panel A: spending, all years

	(1) Log(wbill)	(2) Log(imp. emp.)	(3) Imp. emp. / pop.
Religious policy in cycle	-0.0356**	-0.0379**	-0.0417
	(0.0180)	(0.0173)	(0.0307)
Observations	3661	3682	3682
MeanY	25.53	12.82	1.689

Panel B: spending, direct elections

	(1) Log(wbill)	(2) Log(imp. emp.)	(3) Imp. emp. / pop.
Religious policy in cycle	-0.119***	-0.117***	-0.180***
	(0.0300)	(0.0297)	(0.0449)
Observations	2502	2523	2523
MeanY	25.53	12.82	1.689

Panel C: poverty indicators

	(1) Poverty rate	(2) Poverty gap	(3) Poverty rate	(4) Poverty gap
Religious policy	1.052***	0.192***	-1.746**	-0.443***
	(0.299)	(0.0684)	(0.756)	(0.159)
Years after RP			0.334***	0.0762***
			(0.0856)	(0.0188)
Observations	3702	3702	3652	3652
MeanY	15.07	2.618	15.07	2.618

This is a version of Table 3 where I also control for lagged measures of change in the sectoral composition of the GDP. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table F.18: Does violence predict religious policies?

	(1)	(2)	(3)	(4)	(5)	(6)
No. of incidents (T-1)	-0.000117					
	(0.000106)					
Δ No. of incidents (T-1)		0.000104				
		(0.000156)				
No. of incidents/100000 people (T-1)			-0.000331			
			(0.000346)			
Δ No. of incidents/100000 people (T-1)				-0.000122		
				(0.000276)		
Log(No. of incidents) (T-1)					-0.0202	
					(0.0127)	
Δ Log(No. of incidents) (T-1)						-0.00411
						(0.00733)
Observations	921	799	919	796	921	799

The table shows fixed effect regressions of a dummy indicating if the district had a Sharia policy in the year, regressed on different lagged measures of violence: the number of all incidents in the previous year, the incident number / 1000 people, the natural log of the number of incidents, and changes in these variables. The source of the data is the SNPK dataset, and the sample includes all districts that were covered by the SNPK. (See Figure A.1 for details). The specifications include district and time fixed effects. Standard errors are clustered at district borders. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Robustness of village level regressions

Leave-out-mean placebo test

Table F.19: Placebo: Government services

	Cross-section IV			1996-2000 panel
	(1)	(2)	(3)	(4)
	RP	IV	RP+IV	IV
Ever RP	-0.0170		-0.0156	
	(0.0363)		(0.0388)	
Average of schools in region (log)		-0.0155	-0.00940	0.399
		(0.0589)	(0.0631)	(0.316)
Observations	45556	45556	45556	91043

This table show a placebo experiment to Table 7 . Columns 1 shows a pre-policy regression of the Government Services index on a dummy indicating if the district would have a religious policy by 2013. Column 2 shows the pre-policy regression of the outcome variable on the instrument. Column 3 includes both variables. Column 3 shows a regression of the outcome on the instrument in a panel setting that includes 1996 and 2000 only. All standard errors are clustered on the district level. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table F.20: Placebo: Prevalence of slums

	Cross-section IV			1996-2000 panel
	(1)	(2)	(3)	(4)
	RP	IV	RP+IV	IV
Ever RP	0.0138		0.0161	
	(0.00984)		(0.0101)	
Average of schools in region (log)		-0.00908	-0.0154	-0.282***
		(0.0124)	(0.0127)	(0.0925)
Observations	45556	45556	45556	91043

This table show a placebo experiment to Table 8 . Columns 1 shows a pre-policy regression of the slum dummy on a dummy indicating if the district would have a religious policy by 2013. Column 2 shows the pre-policy regression of the outcome variable on the instrument. Column 3 includes both variables. Column 3 shows a regression of the outcome on the instrument in a panel setting that includes 1996 and 2000 only. All standard errors are clustered on the district level. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table F.21: Effect of religious policies on the Infrastructure Index

	(1)	(2)	(3)	(4)
Religious policy	-0.0622	-0.155***		
	(0.0520)	(0.0591)		
Any RP X Dist(100km)		0.309***		
		(0.118)		
General RP			-0.180***	-0.214***
			(0.0692)	(0.0814)
General RP X Dist(100km)				0.0963
				(0.170)
Prohibitive RP			0.0511	-0.0356
			(0.0548)	(0.0637)
Prohibitive RP X Dist(100km)				0.295**
				(0.128)
Observations	273450	273432	273450	273432
distance		0.502		
distanceSE		0.195		
distance1				2.227
distanceSE1				3.551
distance2				0.121
distanceSE2				0.194

Standard errors clustered at 1996 regional borders in parantheses.
Controls:Village+Time FE, Log(Islamic schools),Log(Pop)

This is a version of Tables 4 to F.13 where I use a version of the Government services which only has the source variables on infrastructure. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table F.22: Infrastructure Index - IV

	Baseline		+ Political vars.	
	(1)	(2)	(3)	(4)
	FE	IV FE	FE	IV FE
Religious policy	-0.0596	-0.250	-0.0589	-0.250
	(0.0526)	(0.347)	(0.0524)	(0.349)
Islamic schools (log)				
Administrative village			-0.0349	-0.0371
			(0.0462)	(0.0468)
Remote village			-0.0536	-0.0390
			(0.0647)	(0.0681)
Village has Hansip police			-0.0120	-0.0116
			(0.0134)	(0.0136)
Observations	269552	269552	269201	269201

This is a version of Table 7 where I use a version of the Government services which only has the source variables on infrastructure. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table F.23: Infrastructure Index - First stage and reduced form

	First stage		Reduced Form	
	(1)	(2)	(3)	(4)
Islamic schools / 1000 people (other villages)	0.360***	0.358***	-0.0899	-0.0895
	(0.110)	(0.110)	(0.131)	(0.131)
Islamic schools (log)				
Administrative village		-0.00337		-0.0363
		(0.0228)		(0.0463)
Remote village		0.0661**		-0.0555
		(0.0275)		(0.0648)
Village has Hansip police		0.00385		-0.0126
		(0.00651)		(0.0134)
Observations	269552	269201	269552	269201

This is a version of Table 7 where I use a version of the Government services which only has the source variables on infrastructure. *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Specification test: villages in same subdistrict left out from leave-out-mean

Table F.24: IV regression: Government services

	Baseline		+ Political vars.	
	(1)	(2)	(3)	(4)
	FE	IV FE	FE	IV FE
Religious policy	-0.0855**	-0.876***	-0.0861**	-0.885***
	(0.0425)	(0.308)	(0.0423)	(0.310)
Islamic schools / 1000 people	0.0188	0.0470***	0.0186	0.0469***
	(0.0170)	(0.0179)	(0.0170)	(0.0179)
Administrative village			-0.00889	-0.0180
			(0.0366)	(0.0401)
Remote village			0.0271	0.0884
			(0.0603)	(0.0696)
Village has Hansip police			0.0689***	0.0700***
			(0.0141)	(0.0157)
Observations	269552	269549	269201	269198

This is a version of Table 7 where the leave-out mean is constructed with the omission of immediate neighbors (villages within the same subdistrict). *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table F.25: First stage and Reduced form

	First stage		Reduced Form	
	(1)	(2)	(3)	(4)
Average of schools in dift. subdist.	0.0818***	0.0813***	-0.0717***	-0.0719***
	(0.0208)	(0.0208)	(0.0255)	(0.0255)
Islamic schools / 1000 people	0.00200	0.00212	0.0452***	0.0450***
	(0.00597)	(0.00596)	(0.0166)	(0.0165)
Administrative village		-0.0105		-0.00874
		(0.0234)		(0.0370)
Remote village		0.0595**		0.0357
		(0.0287)		(0.0602)
Village has Hansip police		0.00239		0.0678***
		(0.00654)		(0.0141)
Observations	269549	269198	269549	269198

This is a version of Table 7 where the leave-out mean is constructed with the omission of immediate neighbors (villages within the same subdistrict). *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table F.26: IV regressions: Prevalence of slums

	Baseline		+ Political vars.	
	(1)	(2)	(3)	(4)
	FE	IV FE	FE	IV FE
Religious policy	0.0160**	0.182**	0.0159**	0.183**
	(0.00687)	(0.0767)	(0.00688)	(0.0770)
Islamic schools / 1000 people	0.00598**	0.0000832	0.00606**	0.000147
	(0.00264)	(0.00208)	(0.00266)	(0.00209)
Administrative village			0.0290***	0.0309***
			(0.0104)	(0.0115)
Remote village			0.00686	-0.00593
			(0.00880)	(0.0114)
Village has Hansip police			0.00125	0.000913
			(0.00315)	(0.00328)
Observations	269552	269549	269201	269198

This is a version of Table 8 where the leave-out mean is constructed with the omission of immediate neighbors (villages within the same subdistrict). *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table F.27: First stage and Reduced form

	First stage		Reduced Form	
	(1)	(2)	(3)	(4)
Average of schools in dift. subdist.	0.0818***	0.0813***	0.0149***	0.0148***
	(0.0208)	(0.0208)	(0.00448)	(0.00445)
Islamic schools / 1000 people	0.00200	0.00212	0.000446	0.000533
	(0.00597)	(0.00596)	(0.00184)	(0.00184)
Administrative village		-0.0105		0.0290***
		(0.0234)		(0.0104)
Remote village		0.0595**		0.00494
		(0.0287)		(0.00877)
Village has Hansip police		0.00239		0.00135
		(0.00654)		(0.00316)
Observations	269549	269198	269549	269198

This is a version of Table 8 where the leave-out mean is constructed with the omission of immediate neighbors (villages within the same subdistrict). *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table F.28: Placebo: Government services

	Cross-section IV			1996-2000 panel
	(1)	(2)	(3)	(4)
	RP	IV	RP+IV	IV
Ever RP	-0.0170		-0.0154	
	(0.0363)		(0.0388)	
Average of schools in dift. subdistr.(log)		-0.0151	-0.00927	0.217
		(0.0575)	(0.0615)	(0.191)
Observations	45556	45553	45553	91040

This is a version of Table F.19 where the leave-out mean is constructed with the omission of immediate neighbors (villages within the same subdistrict). *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table F.29: Placebo: Prevalence of slums

	Cross-section IV			1996-2000 panel
	(1)	(2)	(3)	(4)
	RP	IV	RP+IV	IV
Ever RP	0.0138		0.0161	
	(0.00984)		(0.0101)	
Average of schools in dift. subdistr.(log)		-0.00880	-0.0149	-0.176***
		(0.0122)	(0.0125)	(0.0380)
Observations	45556	45553	45553	91040

This is a version of Table F.20 where the leave-out mean is constructed with the omission of immediate neighbors (villages within the same subdistrict). *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Additional Tables to Chapter 2

Table F.30: Average population shares of name groups over decades

decade starting at	-y share in population	share of Roma names (int)	Low educ. (500)	Low educ. (1000)
1770	0.0199	0.0024	0.0416	0.0127
1780	0.0209	0.0023	0.0409	0.0129
1790	0.0218	0.0022	0.0402	0.0131
1800	0.0228	0.0021	0.0395	0.0133
1810	0.0238	0.0020	0.0388	0.0135
1820	0.0247	0.0019	0.0381	0.0137
1830	0.0257	0.0018	0.0374	0.0139
1840	0.0267	0.0018	0.0367	0.0141
1850	0.0276	0.0017	0.036	0.0143
1860	0.0286	0.0016	0.0353	0.0144
1870	0.0296	0.0015	0.0346	0.0146
1880	0.0306	0.0014	0.0339	0.0148
1890	0.0324	0.0015	0.0336	0.0152
1900	0.0334	0.0018	0.0335	0.0154
1910	0.0370	0.0024	0.0310	0.0145
1920	0.0357	0.0020	0.0330	0.0154
1930	0.0320	0.0027	0.0390	0.0178
1940	0.0259	0.0033	0.0525	0.0252
1950	0.0227	0.0043	0.0562	0.0275
1960	0.0218	0.0057	0.0578	0.0284
1970	0.0208	0.0070	0.0593	0.0293
1980	0.0199	0.0084	0.0608	0.0301
1990	0.0190	0.0097	0.0624	0.0310
2000	0.0184	0.0110	0.0635	0.0319
2010	0.0182	0.0120	0.0642	0.0327

Table F.31: High school student group shares

decade starting at	"-y" name share	Relative rep.	Roma name share	Relative rep.
1850	0.1417	5.130	0.0015	0.8785
1860	0.1343	4.6945	0.0008	0.5302
1870	0.1495	5.0549	0.0007	0.4840
1880	0.1162	3.8048	0.0008	0.5941
1890	0.1098	3.3974	0.0010	0.6177
1900	0.0985	2.9529	0.0007	0.4096
1910	0.0971	2.6383	0.0010	0.4417
1920	0.0940	2.7380	0.0010	0.5564
1930	0.0863	2.7846	0.0008	0.3416
1940	0.0759	3.1762	0.0021	0.5073
1950	0.0655	2.8301	0.0023	0.6223

Table F.32: Medical doctor group shares

decade starting at	"-y" name share	Relative rep.	Roma name share	Relative rep.	Frequent (500), low status	Relative rep.
1770	0.0689	3.1127	0	0	0	0
1780	0.0694	3.0180	0	0	0	0
1790	0.0882	4.1201	0	0	0	0
1800	0.0621	3.0515	0	0	0	0
1810	0.1597	6.3590	0	0	0	0
1820	0.1216	4.8343	0	0	0	0
1830	0.1129	4.3382	0	0	0	0
1840	0.1406	5.0275	0	0	0	0
1850	0.0856	3.1926	0	0	0	0
1860	0.1672	5.8398	0	0	0	0
1870	0.1703	5.6574	0	0	0	0
1880	0.1168	3.8938	0	0	0.0010	0.0296
1890	0.0816	2.4333	0	0	0	0
1900	0.07510	2.3517	0	0	0	0
1910	0.0635	1.7827	0.0002	0.0416	0.0025	0.0820
1920	0.1428	4.0450	0.0019	0.9760	0.0195	0.6100
1930	0.1438	4.4201	0.0004	0.1915	0.0248	0.6205
1940	0.1274	5.1369	0.0019	0.5839	0.0293	0.5593
1950	0.1121	4.9107	0.0024	0.5510	0.0383	0.6840
1960	0.0963	4.3755	0.0018	0.3205	0.0434	0.7454
1970	0.0813	3.9179	0.0028	0.4044	0.0377	0.6420
1980	0.0582	2.9186	0.0033	0.3802	0.0426	0.7045
1990	0.0606	3.1204	0.0017	0.1915	0.0437	0.6863
2000	0.04081	2.3036	0.0021	0.1876	0.0359	0.5710
2010	0.0348	1.9795	0.0019	0.1493	0.0362	0.5615

Table F.33: Army officer group shares

decade starting at	"-y" share	Relative rep.	Roma name share	Relative rep.	Frequent (500), low status	Relative rep
1870	0.1399	4.7289	0.0008	0.5217	0.0090	0.2616
1880	0.1502	4.9197	0.0004	0.3104	0.0088	0.2598
1890	0.114	3.52821	0.0004	0.2345	0.0131	0.3901
1900	0.1174	3.5169	0.0007	0.4560	0.0143	0.4278
1910	0.0967	2.6225	0.0008	0.2808	0.0195	0.6358
1920	0.2767	7.7773	0.0031	1.4085	0.0201	0.6012
1930	0.2825	8.8519	0.0022	0.9484	0.0184	0.4740
1940	0.3299	10.7656	0.0002	0.0551	0.0153	0.3049

Table F.34: Main figure regressions

Panel A - High school graduates

	(1) Monarchy	(2) Dual Monarchy	(3) WW I	(4) Authoritarian	(5) WW II	(6) Communism
Ln(β)	-0.00588*	-0.00882***	0.0328	0.00433	0.0627*	-0.0925**
	(0.00319)	(0.000722)	(0.0341)	(0.00294)	(0.0290)	(0.0223)
N	17	47	6	22	6	5

Panel B - Medical doctors

	(1) Monarchy	(2) Dual Monarchy	(3) WW I	(4) Authoritarian	(5) WW II	(6) Communism	(7) Democracy
Ln(β)	0.00432***	-0.0260***	0.108	0.00405	0.0593**	-0.0104***	-0.0250***
	(0.00122)	(0.00178)	(0.0843)	(0.00489)	(0.0125)	(0.000739)	(0.00282)
N	69	47	6	19	5	44	25

Panel C - Officers

	(1) Dual Monarchy	(2) WW I	(3) Authoritarian	(4) WW II
Ln(β)	-0.00960***	0.0389	0.0114***	0.133***
	(0.00319)	(0.0412)	(0.00388)	(0.0124)
N	45	6	19	4

Robust standard errors in parentheses. $*p < 0.10$, $**$ $p < 0.05$, $***p < 0.01$.

Table F.35: Main figure regressions - "-i" names as reference group

Panel A - High school graduates

	(1) Monarchy	(2) Dual Monarchy	(3) WW I	(4) Authoritarian	(5) WW II	(6) Communism
Ln(β)	-0.00864	-0.00925***	0.0392	0.00567**	0.0586*	-0.0960**
	(0.00823)	(0.00102)	(0.0218)	(0.00222)	(0.0219)	(0.0240)
N	17	47	6	22	6	5

Panel B - Medical doctors

	(1) Monarchy	(2) Dual Monarchy	(3) WW I	(4) Authoritarian	(5) WW II	(6) Communism	(7) Democracy
Ln(β)	0.00515***	-0.0222***	0.0629	-0.00433	0.0905***	-0.00979***	-0.0179***
	(0.000651)	(0.00142)	(0.0536)	(0.00453)	(0.00880)	(0.000661)	(0.00234)
N	65	47	6	19	5	44	25

Panel C - Officers

	(1) Dual Monarchy	(2) WW I	(3) Authoritarian	(4) WW II
Ln(β)	-0.00981***	0.00732	0.0100	0.108**
	(0.00342)	(0.0832)	(0.00656)	(0.0227)
N	45	6	19	4

Robust standard errors in parentheses. $*p < 0.10$, $**$ $p < 0.05$, $***p < 0.01$.

Table F.36: Main figure regressions - Top 20 names as reference group

Panel A - High school graduates

	(1) Monarchy	(2) Dual Monarchy	(3) WW I	(4) Authoritarian	(5) WW II	(6) Communism
Ln(β)	-0.00426	-0.00706***	0.0255	0.00240	0.0646**	-0.0758**
	(0.00251)	(0.000786)	(0.0246)	(0.00201)	(0.0223)	(0.0215)
N	17	47	6	22	6	5

Panel B - Officers

	(1) Dual Monarchy	(2) WW I	(3) Authoritarian	(4) WW II
Ln(β)	-0.00431	0.00348	0.0131**	0.106**
	(0.00281)	(0.0369)	(0.00616)	(0.0132)
N	45	6	19	4

Robust standard errors in parentheses. $*p < 0.10$, $**$ $p < 0.05$, $***p < 0.01$.

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