

The public morals - public services tradeoff: Theory and evidence from Sharia regulations in Indonesia *

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Abstract

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. The first IV exploits village-level variation in the number of religious schools using a leave-out mean design. The second IV exploits district-level historical variation in religious intensity interacted with the country-wide increase in religiosity using a shift-share design. 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. The calibration of a formal voting model suggests that the total utility of the secular voters can decrease by as much as four times as the decrease in observed outcomes would justify. The evidence is consistent with the notion that politicians use divisive policies to strategically redistribute utility across voters while reducing the supply of material wellbeing.

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I Introduction

Democracies around the globe are experiencing the emphasis of politics 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.¹ Less is known about the supply side incentives of politicians to cater to these demands, and the impacts of such decisions.

I argue in this paper 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.²

I measure the impact of religious extremism on a range of government and 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.³ 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

¹Bentzen [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).

²The 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].

³Within 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.

for religious policies that rely on different identification assumptions. The first uses within-district, village level variation in preferences.⁴ 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.⁵

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 II 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 III and then in Section IV 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 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.⁶ I present the results both in fixed effect regression and in event study forms. I find that in election cycles where the incumbent is introducing 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

⁴Village (*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.

⁵I 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.

⁶According 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.

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.⁷ 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 that drive the religious turn.⁸ 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 policies 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 the assumption of district level parallel trends, but on an

⁷I use this indicator as the village level proxy for poverty, as poverty rate statistics are not available on the village level in PODES.

⁸There are examples both in the political science (Buehler 2008,2016) and in the economics literatures [Bazzi et al., 2018].

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. A strong first stage and an event study showing that increase in the number of schools predates the introduction of Sharia regulations confirms that this condition is met.

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.⁹ 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 which is potentially caused by omitted variables later on.¹⁰ 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 needs assuming 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.¹¹

Even though none of them individually eliminates all identification concerns, the results from the three

⁹The idea is similar to the shift share-type instruments discussed, see Bartik [1991].

¹⁰An analogue from the migration literature (which extensively 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.

¹¹Suharto 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.

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 IV 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 put formal structure on the decision to introduce Sharia policies in Section II. I build a simple model of elections which focuses on policy choices of an incumbent politician. This politician 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 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 hardliner 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.¹² The predictions are counterintuitive, but reflect the correlates of Sharia introduction described in Section III. 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

¹²When 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).

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 the moderate voters I can estimate this parameter directly. For the hardliner 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 that the hardliner voters have to like Sharia regulations ten times as much in absolute terms as the seculars 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.

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.

II Theoretical background and hypothesis

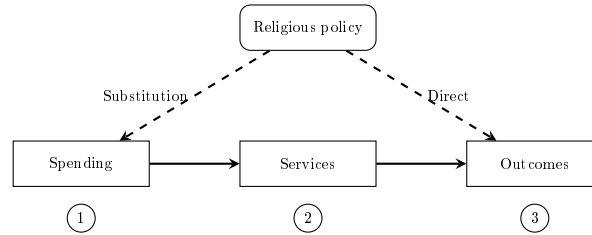
The hypothesis of the paper is that identity-based extreme policies serve as a cheaper alternative for politicians to supplying public services, or delivering material utility to voters directly. This creates an economic incentive to cater to a hardliner voter base. Whether this incentive becomes salient 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 Section III, in Section IV I identify these channels of impact of Sharia regulations, while Section V puts forward the mechanism formally in a way that allows for welfare calculations.

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).

III Context, data and stylized facts

III.A 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.¹³ 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.¹⁴

District executives (“regents” or “mayors”) have traditionally been the most important authorities over

¹³Laws 22/1999 and 25/1999

¹⁴Regencies (kabupaten) consist of mostly rural areas; cities (kota) are urban centers. The literature uses the “district” and “region” terms interchangeably.

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 at some random date between 1999 and 2004.¹⁵ Later, recognizing their *de facto* authority over policy, the central government introduced direct elections for district heads from 2005 onward.¹⁶ 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 recruitmental base for new politicians, 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.

Cyclicity 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

¹⁵This variation in the timing of local government democratization is used in previous literature [Martinez-Bravo et al., 2017, Martinez-Bravo, 2014].

¹⁶Again, 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).

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 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].¹⁷

III.B 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”.¹⁸ 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.¹⁹ 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.²⁰

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 for a decade, from the early work of Bush [2008] to the recent comprehensive account of Buehler [2016]. I use the list of regulations

¹⁷This 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]

¹⁸The 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].

¹⁹India, 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.

²⁰Hallaq [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.

compiled by these scholars as primary data source along with own data collection and a deeper analysis of the regulations’ actual content.²¹ 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.²² 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].

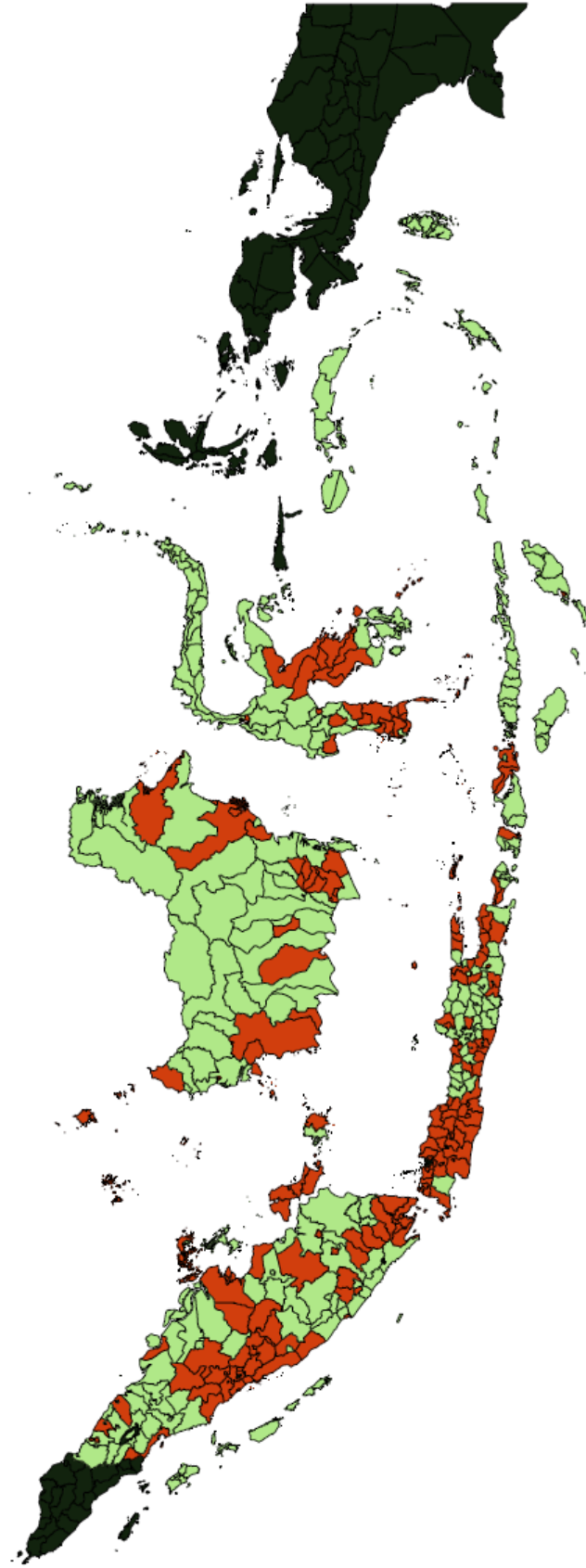
I categorize these pieces of legislation into two broad groups, which I call prohibitive and normative. Prohibitive regulations are the ones which severely constrain or ban altogether the selling, distributing and consuming alcohol; increase the crackdown on drugs and prostitution; enforce the retail 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 “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].

²¹I would like to thank Giannisa Novi Budiutami for the excellent research assistance she provided with this task.

²²Buehler (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.

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



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 shapefiles show borders in 2009.

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.²³
- 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.²⁴
- 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.²⁵
- Regulation 2008/1 of the same district sets up compulsory extracurricular Islamic educational schemes for pupils learning in secular elementary schools.²⁶

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.

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].

III.C 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,

²³<http://www.jdih.setjen.kemendagri.go.id/download.php?KPUU=13882>

²⁴<http://www.jdih.setjen.kemendagri.go.id/download.php?KPUU=16542>

²⁵<http://www.jdih.setjen.kemendagri.go.id/download.php?KPUU=6686>

²⁶<http://www.jdih.setjen.kemendagri.go.id/download.php?KPUU=7057>

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 2013.²⁷ 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. This data is collected simultaneously with the population census, or other universal data collection efforts, such as the agricultural and economic censuses. The data that I use includes 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.²⁸ 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 elected or appointed), and a standardized index that I construct from the hundreds of variables on local service availability and infrastructure quality.²⁹

We hand-collected data on district heads, candidates and election outcomes from online news sources and regional statistical offices.³⁰ 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 ongoing 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

²⁷Such 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)

²⁸The size of the successfully linked sample is similar to that in Martinez-Bravo [2014]

²⁹See section §Appendix A on more information of the government services index.

³⁰For helping in the collection of this data I owe gratitude to Giannisa Novi Budiutami and Zsolt Hegyesi.

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.

III.D 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.³¹ 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”).

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.³² 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 V 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

³¹ This mostly means the year 2000, when only 3% of districts had a religious policy.

³² Dana Alokasi Umum, or DAU for short.

incentive to introduce Sharia policies.

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 is 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.

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 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 be introduced.

In the model presented in Section V 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 *kyai*) 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 receive religious education without entering the grassroots Islamist movement.³³

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 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.

³³In 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.

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.³⁴ 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 V (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 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.

³⁴INDODAPOER compiles data from several government agencies, so the original data coverage varies a lot across variables.

Table 2: Predictors of religious policies

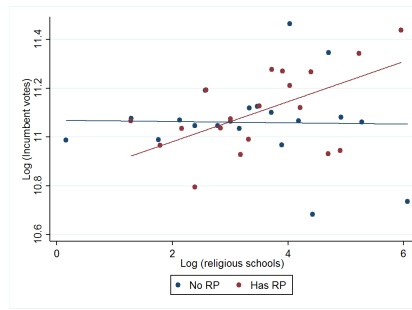
Panel A: $P(\text{religious policy}_{rt}) = \beta_1 \text{Log}(\text{schools} + 1) + \beta_2 X_{rt} + \alpha_r + \lambda_t$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log(schools+1)	0.00992*** (0.00214)	0.0148** (0.00685)	0.0142* (0.00740)	0.0174** (0.00680)	0.0150** (0.00736)	0.0173** (0.00781)	0.0148** (0.00681)	0.0203** (0.00854)
Log(GDP/pc)			0.000205 (0.0418)					0.0305 (0.0497)
Log(Block grants pc.)				-0.00844 (0.0128)				-0.0116 (0.0146)
Unemployment rate					0.00125 (0.00254)			0.00255 (0.00313)
Inequality						0.0492** (0.0196)		0.0371* (0.0194)
Poverty rate							-0.00420* (0.00231)	-0.00184 (0.00295)
Observations	3184	3184	2754	3084	2995	2786	3179	2143
Model	OLS	FE	FE	FE	FE	FE	FE	FE

Panel B: $P(\text{religious policy}_{rt}) = \beta_1 \text{Log}(\text{schools} + 1) + \beta_2 X_{rt} + \beta_3 \Delta X_{rt} + \alpha_r + \lambda_t$						
	(1)	(2)	(3)	(4)	(5)	(6)
Log(schools+1)	0.0143* (0.00742)	0.0189*** (0.00701)	0.0143 (0.00916)	0.0214*** (0.00769)	0.0162** (0.00691)	0.0216** (0.0106)
Log(GDP/pc)	0.0362 (0.0446)					0.0787 (0.0965)
Δ Log(GDP/pc)	-0.143 (0.0892)					-0.132 (0.131)
Log(Block grants pc.)		0.00287 (0.0153)				0.00106 (0.0119)
Δ Log(Block grants pc.)		-0.0117** (0.00576)				-0.00545 (0.00559)
Unemployment rate			0.00367 (0.00365)			0.00695 (0.00560)
Δ Unemployment rate			-0.00308 (0.00325)			-0.00410 (0.00417)
Inequality				0.0622** (0.0313)		0.0793** (0.0390)
Δ Inequality				-0.0228 (0.0205)		-0.0528* (0.0294)
Poverty rate					-0.00502** (0.00252)	-0.00598 (0.00365)
Δ poverty rate					0.00468 (0.00302)	0.0125 (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.

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.

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

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 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.

IV The impact of religious policies

IV.A 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.³⁵ The third outcome measures the ratio of implicit public employment to the population of the district.³⁶

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

³⁵The 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.

³⁶It 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.

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 SNP dataset) can be interpreted at least partly as a coverup for corruption. Unfortunately, there is no comprehensive dataset on explicit corruption cases that I am aware of, so directly documenting the impact of Sharia policies on corruption is beyond the scope of the current paper.

IV.B 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.³⁷ 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 as 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 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):

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

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 Potential 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 Potential 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 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.

³⁷These have an impact on how revenue redistribution across districts are calculated by the central government.

IV.B.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.³⁸ The set of control variables is the same as in the corresponding regressions.

IV.B.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 wagebill 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 wagebill 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.³⁹ 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

³⁸To 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.

³⁹Reported already in the Indonesian context by Sjahrir et al., 2013

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 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.

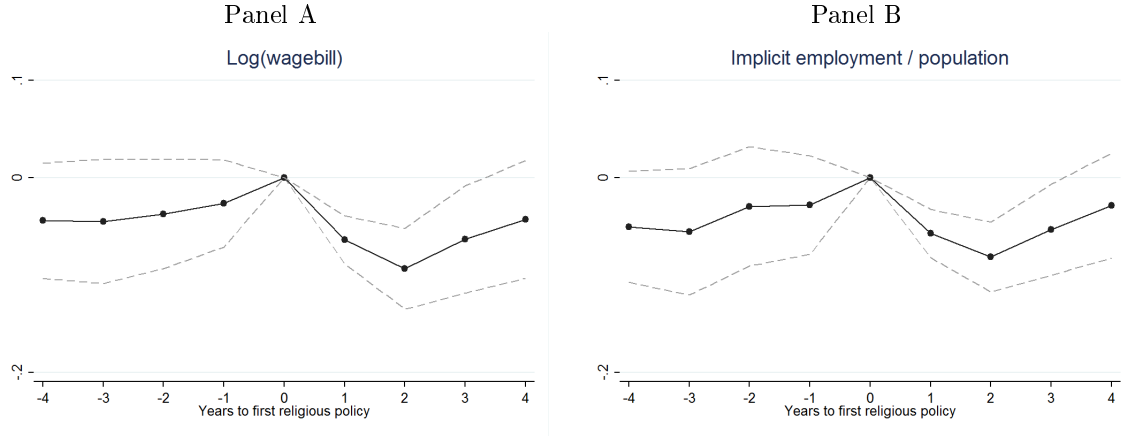
Table 3: Baseline specification

Panel A			
	(1)	(2)	(3)
	Log(wbill)	Log(imp. emp.)	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)	(2)	(3)
	Log(wbill)	Log(imp. emp.)	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



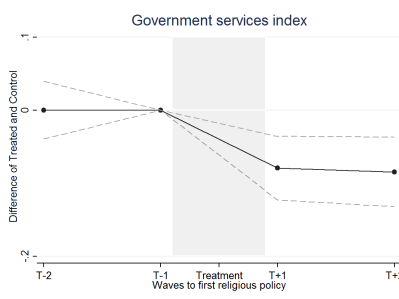
The figures shows event studies comparing different expenditure measures in districts which introduced a religious policy and districts that did not. The figure uses district-year observations. The events are defined as the start of the election cycle that saw the introduction of the religious policy. 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 wagebill. 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 section §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.⁴⁰ 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 village level data (see Panel A and B of Figure 6).

⁴⁰This peculiarity can be a consequence of the slope being very steep at the trend break when the policy is introduced.

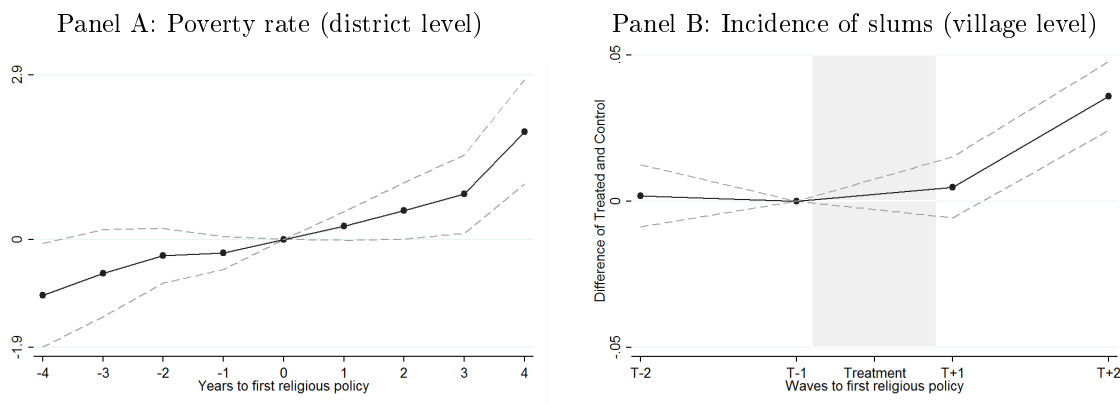
Table 5: Social effects main table

	(1)	(2)	(3)	(4)	(5)
	Poverty rate	Poverty gap	Poverty rate	Poverty gap	P(slum)
Religious policy	1.110*** (0.295)	0.224*** (0.0706)	-0.967 (0.707)	-0.272* (0.156)	0.0157** (0.00678)
Years after RP			0.179** (0.0849)	0.0414** (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.

Figure 6: Poverty event studies



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).

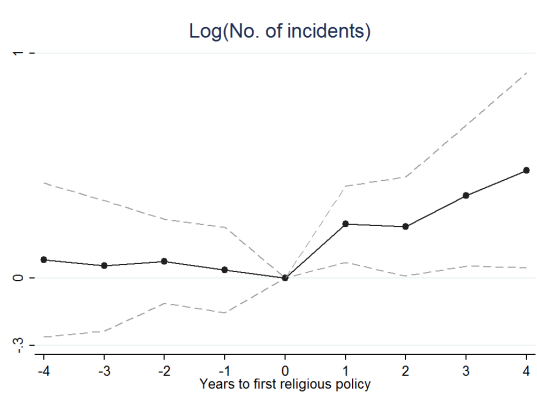
The results show that there are 23.33 more violent incidents on average, which translates to about a 28% increase. About 8 more people in every 100.000 falls victim to a violent crime. Figure 7 shows the same result in an event study and highlights that there were no significant differences in trends between adopters and non-adopters of religious policies previously.

Table 6: Sharia regulations as predictors of violence

	(1)	(2)	(3)
	No. of incidents	No. of incidents/100000 people	Log(No. of incidents)
Had RP (T-1)	23.33** (11.36)	7.946** (3.462)	0.279** (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 G.10 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 G.11 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 2016.⁴¹

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 atrophy 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.⁴²

IV.B.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

⁴¹Source: UNSDC/INTP/CTS statistics. The murder rate only considers intentional homicide, not killings during civil unrest.

⁴²The 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.

a control. This measure is the ratio between the average household expenditure and the average household expenditure of the lowest 20% (Table G.16). Second, I re-estimate the equation with lagged unemployment levels (Table G.17). 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 G.18). 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.⁴³ 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, 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,

⁴³The 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.

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 G.19 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 E.6 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 G.23 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’s 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 G.15 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.

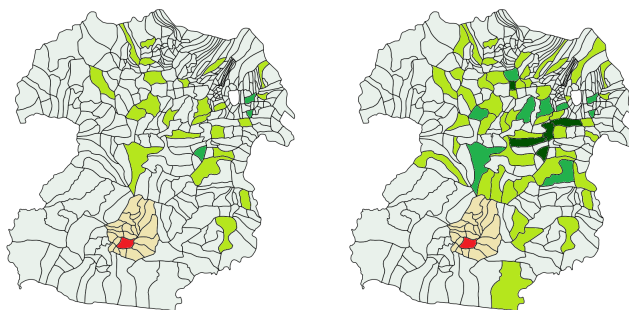
Remaining identification concerns. The difference-in-differences approach in any case assumes that the demand for Sharia regulations is exogenous to outcome variables, and parallel trends hold for introducing and not introducing districts in all variables except for demand for Sharia. This view is supported 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.

IV.C 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.

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 instrument 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.

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.⁴⁴ 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 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

⁴⁴To be precise, the number of *pesantren* and *madrasah diniyah*. There is no Islamic equivalent of a "state church", all educational institutions are private.

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.

IV.C.1 Identification assumptions.

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

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

again, RP is the dummy indicating the presence of a religious policy, X is the 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:

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

Throughout the paper, instrumental variables regressions are carried out using the softwares developed by Schaffer [2010].

IV.C.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.⁴⁵

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 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.

⁴⁵The Kleibergen-Paap Wald rk LM statistic is 14.95, the corresponding F statistic (justified due to using clustered SEs) is 19.09.

Table 7: IV regression: Government services

Panel A: OLS and IV regressions

	Baseline		+ Political vars.	
	(1) FE	(2) IV FE	(3) FE	(4) IV FE
Religious policy	-0.0855** (0.0425)	-0.444 (0.273)	-0.0861** (0.0423)	-0.446 (0.274)
Islamic schools / 1000 people	0.0188 (0.0170)	0.0316** (0.0155)	0.0186 (0.0170)	0.0313** (0.0155)
Administrative village			-0.00889 (0.0366)	-0.0130 (0.0371)
Remote village			0.0271 (0.0603)	0.0547 (0.0667)
Village has Hansip police			0.0689*** (0.0141)	0.0696*** (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*** (0.110)	0.358*** (0.110)	-0.160 (0.104)	-0.159 (0.103)
Islamic schools / 1000 people	-0.00173 (0.00286)	-0.00163 (0.00285)	0.0324** (0.0153)	0.0321** (0.0153)
Administrative village		-0.00337 (0.0228)		-0.0115 (0.0368)
Remote village		0.0661** (0.0275)		0.0252 (0.0609)
Village has Hansip police		0.00385 (0.00651)		0.0679*** (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 leaveout 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) FE	(2) IV FE	(3) FE	(4) IV FE
Religious policy	0.0160** (0.00687)	0.0780 (0.0642)	0.0159** (0.00688)	0.0797 (0.0650)
Islamic schools / 1000 people	0.00598** (0.00264)	0.00377** (0.00172)	0.00606** (0.00266)	0.00380** (0.00173)
Administrative village			0.0290*** (0.0104)	0.0297*** (0.0107)
Remote village			0.00686 (0.00880)	0.00197 (0.00984)
Village has Hansip police			0.00125 (0.00315)	0.00112 (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*** (0.110)	0.358*** (0.110)	0.0280 (0.0208)	0.0285 (0.0209)
Islamic schools / 1000 people	-0.00173 (0.00286)	-0.00163 (0.00285)	0.00364** (0.00171)	0.00367** (0.00171)
Administrative village		-0.00337 (0.0228)		0.0295*** (0.0104)
Remote village		0.0661** (0.0275)		0.00724 (0.00874)
Village has Hansip police		0.00385 (0.00651)		0.00143 (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 leaveout 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 8 and with the sample reduced to villages which never had a *pesantren*-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 (0.0427)	0.169 (0.376)	0.00386 (0.0426)	0.182 (0.383)
Islamic schools / 1000 people	0.0518*** (0.0157)	0.0505*** (0.0152)	0.0515*** (0.0156)	0.0502*** (0.0151)
Administrative village			0.0320 (0.0713)	0.0379 (0.0733)
Remote village			0.0292 (0.0864)	0.00934 (0.0922)
Village has Hansip police			0.0561* (0.0313)	0.0568* (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 (0.00858)	0.0746 (0.0897)	0.00858 (0.00859)	0.0792 (0.0921)
Islamic schools / 1000 people	0.00149 (0.00286)	0.00101 (0.00296)	0.00160 (0.00288)	0.00109 (0.00298)
Administrative village			0.0606*** (0.0212)	0.0629*** (0.0217)
Remote village			-0.00164 (0.0208)	-0.00949 (0.0213)
Village has Hansip police			-0.00639 (0.00806)	-0.00611 (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 *pesantren*-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%.

IV.C.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 G.20 shows the result, with no sign of such correlation. Table G.21 shows the same exercise with the slum variable as an outcome.⁴⁶

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 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.⁴⁷ Tables G.25 and G.26 are the corresponding outputs from this exercise, without any remarkable difference, so local spillovers are arguably not driving the results.

IV.D 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:⁴⁸

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

The first term of the product, $share_{r,0}$ is a measure of the pre-policy variation in the demand for Sharia-

⁴⁶Tables G.29 and G.30 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.

⁴⁷Subdistricts are called *kecamatan*, these are the administrative level between the village and the district.

⁴⁸See Bartik 1991 or Altonji and Card [1991].

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 1955.⁴⁹ The results are robust to the choice of the pre-policy Sharia demand variable.

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.

IV.D.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:⁵⁰

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

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.

⁴⁹Masyumi party. Vote shares are obtained from the <http://www.pemilu.asia> website; missing data is imputed from provincial averages reported in Feith [1955].

⁵⁰See Goldsmith-Pinkham et al., 2018, page 12, “Case 1”.

IV.D.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.

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.

IV.D.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.

Table 11: Shift-share instruments
Panel A: all years, non-survey year data imputed

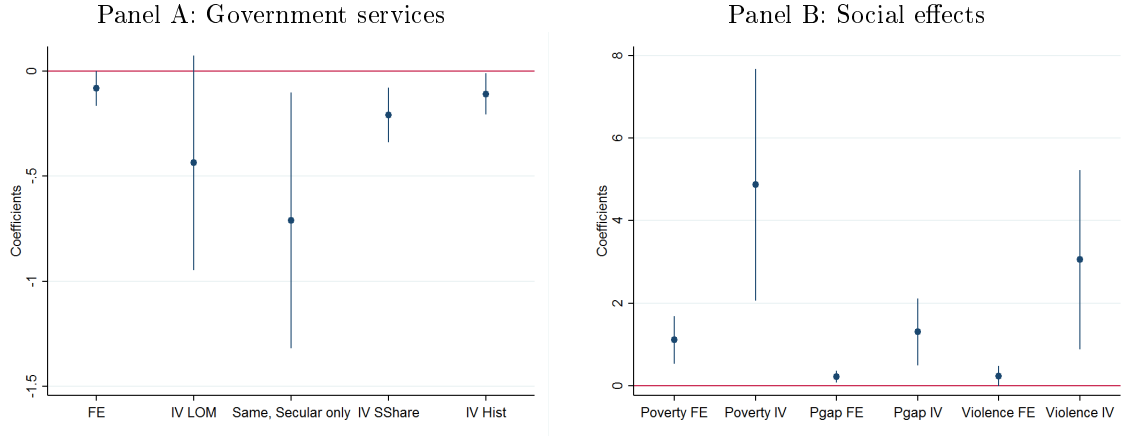
	(1)	(2)	(3)	(4)
	Government services	Poverty rate	Poverty gap	Log(No. of incidents)
Has RP	-0.0946** (0.0451)	4.870*** (1.434)	1.307*** (0.413)	3.055*** (1.109)
Observations	3291	3982	3982	1253

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

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

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.

IV.E Heterogeneity analysis.

IV.E.1 Short- and long-run effects

Now I analyze whether the impact of Sharia-regulations persists. In Table G.9 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.

IV.E.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.⁵¹ 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 heterogenous, 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 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 heterogenous across villages that have a district police and those that do not.

In table G.12 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

⁵¹The 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.

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.

IV.E.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.⁵² First, I re-estimate Equation equation (2) in the following form:

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

Then I estimate the same equations with the policy variable interacted with distance. The results can be found in table G.12 and table G.14, 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 G.14.

⁵²For a deeper discussion on the regulations and the classification please see Appendix A.

V Welfare analysis

V.A 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,

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

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.⁵³ 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 $\frac{\partial Pr(re-elected)}{\partial N_E} = \frac{\omega}{M}$.⁵⁴ Public employment in this context serves as a token for any relationship between the elected official and the voter that is contractable to some extent.

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.⁵⁵ This is a static setup; politicians and voters only care about the here and now. Incumbent votes are determined by equation

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

while challengers get votes according to

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

π s 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

⁵³I use the male pronoun because in the Indonesian example the vast majority of districts heads are male.

⁵⁴Relaxing this will result in lower reelection probabilities, and the incumbent will choose the corner solution for a larger part of the parameter space (see section §Appendix C), but does not change the essence of the analysis.

⁵⁵As if they, too, maximized $Pr(elected) \times M$, and entered the race until that value was over a certain fixed threshold.

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 hardliner 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 hardliner 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 microfoundation 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 ν_{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 each others 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:

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

And with introducing it:

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

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.

V.B 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 public morals - public services substitution. 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.

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

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.

V.C 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 hardliner voter. Total effect of divisive policies on average welfare then:

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

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.⁵⁶ As I discussed in Section III, 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 hardliner 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.

⁵⁶With 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.

V.D 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 hardliner 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 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 is cross sectional (from 2014), and does not cover the whole country.⁵⁷ Also, the respondents are asked if they voted in the past elections, but not

⁵⁷The 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 is 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.

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 is 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% 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_{NE}^S) 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 on 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).⁵⁸

Results. Table D.2 reports the estimation results. The coefficients β_3 and β_4 have the expected signs 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:

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

⁵⁸Other 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,⁵⁹ 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.⁶⁰ 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.⁶¹ 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.

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

⁵⁹The 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.

⁶⁰That 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 unreported robustness check I estimated the same model with both categories separately, and the results were qualitatively the same.

⁶¹Belief 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”

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).⁶²

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

V.E Setting up the welfare calculation

To do this, one should observe Equation 6 in the model, the formula for incumbent votes from Section II. 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 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:

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

⁶²What 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.

The implicit employment (wagebill 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 microfoundations (see section §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 the number of devout by establishing that $\hat{N}_R = \hat{\alpha}_2 \log(\text{schools}_{it})$.

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 $\hat{N}_E = \hat{\alpha}_1 \frac{\text{wagebill}_{it}}{\text{minimumwage}_{it}}$ and $\hat{N}_R = \hat{\alpha}_2 \log(\text{schools}_{it})$.

V.F 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 about 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.

VI 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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Appendix A : Data description

Coverage of violence data

Table A.1: Coverage of National Violence Monitoring System by provinces and years

	<i>Province/year</i>	<i>1997</i>	<i>1998-2004</i>	<i>2005-2010</i>	<i>2011</i>	<i>2012</i>	<i>2013</i>	<i>2014</i>
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].⁶³ 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)

⁶³I omit the variables related to healthcare, as over the study period Indonesia started experimenting with central government-financed healthcare schemes which grew steadily in coverage and funding, thus the data on healthcare infrastructure mostly reflects central, and not local government policies.

- Number of vocational education institutions
- Number of households that have access to electricity
- Number of households with landline phone subscriptions⁶⁴
- 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.

⁶⁴Though 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:

$$E(\text{share}_{r0}\epsilon_{rt} \mid z_{rt}) = 0).$$

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 IV 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:

$$schools_{rt} = schools_{r,0} \prod_{t=1..T} (\delta_t + \delta_{rt}), \quad (10)$$

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

$$schools_{rt} = schools_{r,0} \prod_{k=1..t} \delta_k + schools_{r,0} \left[\sum_{l=2..t} \delta_{r,l-1} \prod_{m=1..l-1} \delta_m \prod_{n=l..t-1} (\delta_{n+1} + \delta_{r,n+1}) \right]$$

The first term $schools_{r,0} \prod_{k=1..t} \delta_k$ is the shift share. The second term contains all terms from Equation 10 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

$$V = Pr(reelected) \times M - \omega N_E - s \cdot c \quad (11)$$

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

$$Votes_I = (1-s) \times \underbrace{(\Delta\pi_E N_E + \pi_{NE} N)}_{\text{Inc. votes without Sharia}} + s \times \underbrace{(N_R + \Delta\pi_E^S N_E + \pi_{NE}^S (N - N_R))}_{\text{Inc. votes with Sharia}} + e_I, \quad (12)$$

$$Votes_j = (1-s) \times \underbrace{(\Delta\pi_B N_B + \pi_{NB} N)}_{\text{Chall. votes without Sharia}} + s \times \underbrace{(\Delta\pi_B N_B + \pi_{NB} (N - N_R))}_{\text{Chall. votes if inc. does Sharia}} + e_j. \quad (13)$$

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 then 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 11:

$$\frac{\partial V}{N_E} : \frac{\partial P(\text{reelected})}{\partial N_E} = \frac{\omega}{M}$$

The conditional logit formula for the winning probability is

$$P(\text{reelected}) = P_r = \frac{\exp(\Delta\pi_E(s)N_E + \pi_{NE}(s)(N - s \cdot N_R) + s \cdot N_R)}{1 + \exp(\Delta\pi_E(s)N_E + \pi_{NE}(s)N) + J\exp(\Delta\pi_B \frac{\bar{M}}{p} + \pi_{NB}(N - s \cdot N_R))} \quad (14)$$

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

$$P_r^*(s) = \frac{1}{2} \pm \frac{1}{2} \sqrt{1 - 4 \frac{\omega}{M \cdot \Delta\pi_E(s)}} \quad (15)$$

Where the second derivative test reveals that the smaller root is a local minimum, and the larger root is a local maximum.⁶⁵ 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:⁶⁶

$$\Delta\pi_E(s)N_E = \log\left(\frac{P_r^*(s)}{1 - P_r^*(s)}\right) - s \cdot (1 + \pi_{NB} - \pi_{NE}(s)) \cdot N_R + (\pi_{NB} - \pi_{NE}(s))N + \log J + \Delta\pi_B \frac{\bar{M}}{p} \quad (16)$$

⁶⁵There 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 $\pi_{NE}(s)N + e$.

⁶⁶With the approximation that the approximation that $\log(1 + x) \simeq \log(x)$ for high enough x .

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 $\log\left(\frac{P_r^*(s)}{1-P_r^*(s)}\right) - s \cdot (1 + \pi_{NB} - \pi_{NE}(s)) \cdot N_R$.⁶⁷ 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.
- $\log J + \Delta\pi_B \frac{\bar{M}}{p}$ 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 hardliner 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 write up the definition of ΔN_E in terms of ex post clientele sizes observed in Equation 16:

$$\Delta N_E = \frac{1}{\Delta\pi_E(s=1)} (\Delta\pi_E(s=1)N_E(s=1) - \Delta\pi_E(s=0)N_E(s=0)) - \frac{\Delta_s}{\Delta\pi_E(s=1)} N_E(s=0)$$

Equation 16 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 hardliner channel. These describe the competing forces at work in choosing optimal employment

⁶⁷The term $\log\left(\frac{P_r^*(s)}{1-P_r^*(s)}\right)$ is monotonically increasing in P_r^* , so it will be smaller if $s=1$.

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 hardliner 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 hardliner vote with certainty.

The population channel $C_N = -(\pi_{NE}(s=1) - \pi_{NE}(s=0))N$ 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.

$$\Delta N_E = \frac{1}{\Delta \pi_E(s=1)} (C_P + C_N + C_R - \Delta_s N_E(s=0))$$

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 hardliner 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 hardliner community exceeds a certain threshold $N_R > \underline{N_R}$

$$\underline{N_R} = \rho \left((C_P + C_N - \Delta_s N_E(s=0)) - \frac{\Delta \pi_E(s=1)}{\omega} M\Delta P_r^* + c \right) \quad (17)$$

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 17 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 proposition amounts to showing that $\partial N_R / \partial M > 0$. There are three terms in Equation 17 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 of 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 $\log\left(\frac{P_r^*(s)}{1-P_r^*(s)}\right) = f(M \cdot \Delta\pi_E(s))$ 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 hardliner 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 $\frac{1}{\omega} \left(-\Delta P_r^* - \frac{\partial \Delta P_r^*}{\partial M} M\right) \cdot -\Delta P_r^*$ 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 15 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

$$-\frac{\partial \Delta P_r^*}{\partial M} = \frac{\omega}{2M^2} \left(\frac{1}{\Delta \pi_E(s=0) \sqrt{1 - \frac{4\omega}{\Delta \pi_E(s=0)M}}} - \frac{1}{\Delta \pi_E(s=1) \sqrt{1 - \frac{4\omega}{\Delta \pi_E(s=1)M}}} \right)$$

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 65) 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 17 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 : Evidence on political behavior

Table D.2: Total incumbent votes and incumbent decisions

	Incumbent votes					
	(1)	(2)	(3)	(4)	(5)	(6)
RP before election	19429.5 (13746.7)	11729.0 (13591.1)	22129.6 (14460.3)	14621.1 (14287.7)	9838.9 (14263.7)	-4099.4 (13841.6)
Electorate size	0.175*** (0.0243)	0.164*** (0.0231)	0.174*** (0.0234)	0.164*** (0.0223)	0.159*** (0.0249)	0.154*** (0.0242)
Implicit employment	0.157*** (0.0545)	0.171*** (0.0528)	0.158*** (0.0514)	0.170*** (0.0494)	0.158*** (0.0529)	0.171*** (0.0520)
Implicit employment \times RP	-0.286** (0.135)	-0.284** (0.125)	-0.288** (0.130)	-0.285** (0.120)	-0.311** (0.139)	-0.314** (0.130)
Electorate \times RP	0.123** (0.0556)	0.131** (0.0514)	0.104* (0.0532)	0.109** (0.0490)	0.131** (0.0597)	0.130** (0.0559)
Number of schools			1.749 (15.78)	0.0741 (15.97)		
Number of schools \times RP			29.98 (35.56)	33.78 (34.79)		
Log schools					4417.9** (1767.4)	2766.2 (1932.8)
Log(schools+1) \times RP					2312.7 (6172.5)	5361.9 (5986.8)
Inc. has degree		14931.3** (6582.0)		16040.5** (6488.3)		15830.2** (6472.8)
Inc. is doctor		1905.7 (6732.3)		2408.5 (6678.3)		3685.7 (6849.2)
Inc. is haji		22667.5*** (6490.3)		21928.3*** (6533.5)		20577.8*** (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 (wagebill / 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 is 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)	(2)	(3)
	Pr(voted, district head)	Pr(voted, village head)	Pr(voted, legislature)
Public employee	0.0816*** (0.0228)	-0.00818 (0.0305)	0.0166 (0.0233)
Devout	0.00819 (0.0137)	-0.00679 (0.0151)	-0.00517 (0.0126)
Religious policy	-0.00988 (0.0182)	0.0130 (0.0237)	-0.00285 (0.0162)
Public employee \times RP	-0.0635* (0.0366)	-0.000896 (0.0569)	-0.0536 (0.0361)
Devout \times RP	0.0705*** (0.0269)	-0.0677 (0.0417)	-0.000617 (0.0225)
Observations	8958	7087	9141

The table shows OLS regressions of electoral participation on different characteristics of the voter. The data comes 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 : Survey evidence on 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.⁶⁸ The dependent variable is a dummy indicating if the district had a religious policy in 2013. In Columns 3-4 the same 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 occurred 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 weak evidence that religious participation is weaker in these districts.

In Table E.6 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 of corruption waves. This is in contrast with the results in Henderson and Kuncoro [2011] who find that this

⁶⁸Only the 2007 IFLS wave had information on crime.

link existed in national elections in the 2000s.

Table E.4: Crime perception in 2007 and probability of Sharia regulations in 2013

	(1)	(2)	(3)	(4)
Any crime	-0.0441 (0.0289)		0.174 (0.172)	
Violent crime		-0.0358 (0.0432)		0.224 (0.199)
Property crime		-0.108** (0.0495)		0.162 (0.497)
Constant	0.345*** (0.0682)	0.343*** (0.0683)	0.239*** (0.0553)	0.247*** (0.0458)
Observations	12712	12712	159	159
Unit	Person	Person	Region	Region

Table shows 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)	(2)	(3)	(4)
	Pr(Donation)	Pr(Arisan)	Pr(Event occurrence)	Pr(Participation)
Religious policy	-0.00580* (0.00348)	-0.0179* (0.0102)	-0.0141 (0.0192)	-0.00662 (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%.

Table E.6: Corruption perception in 2007 and probability of Sharia regulations in 2013
Panel A

Table E.7: Corruption incidence in 2007 and religious policies by 2013							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CorruptionPresent	0.100 (0.0798)	-0.0625 (0.0743)	-0.0934* (0.0508)	-0.113** (0.0492)	0.00226 (0.0550)	-0.164** (0.0659)	-0.0584 (0.0688)
Observations definition	331 Village gov.	331 Subdist gov.	331 Dist. gov.	331 Dist. parl.	331 Dist. police	331 Healthcare	331 State schools
Robust standard errors in parenthesis.							

Panel B

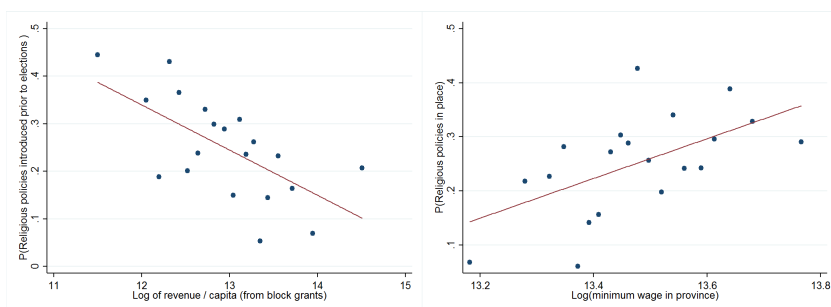
Table E.8: Corruption trends in 2007 and religious policies by 2013							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
GotWorse	0.0569 (0.160)	-0.0286 (0.156)	-0.111* (0.0642)	-0.186*** (0.0560)	-0.0219 (0.0830)	-0.00540 (0.137)	0.0310 (0.131)
Observations definition	331 Village gov.	331 Subdist gov.	331 Dist. gov.	331 Dist. parl.	331 Dist. police	331 Healthcare	331 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%.

Appendix F : Additional figures

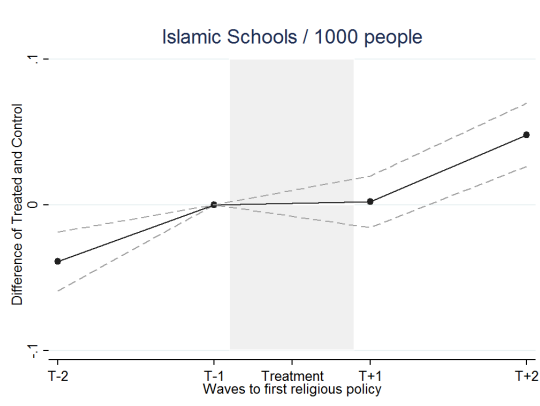
Stylized facts

Figure F.1: Determinants of Sharia regulations: model predictions



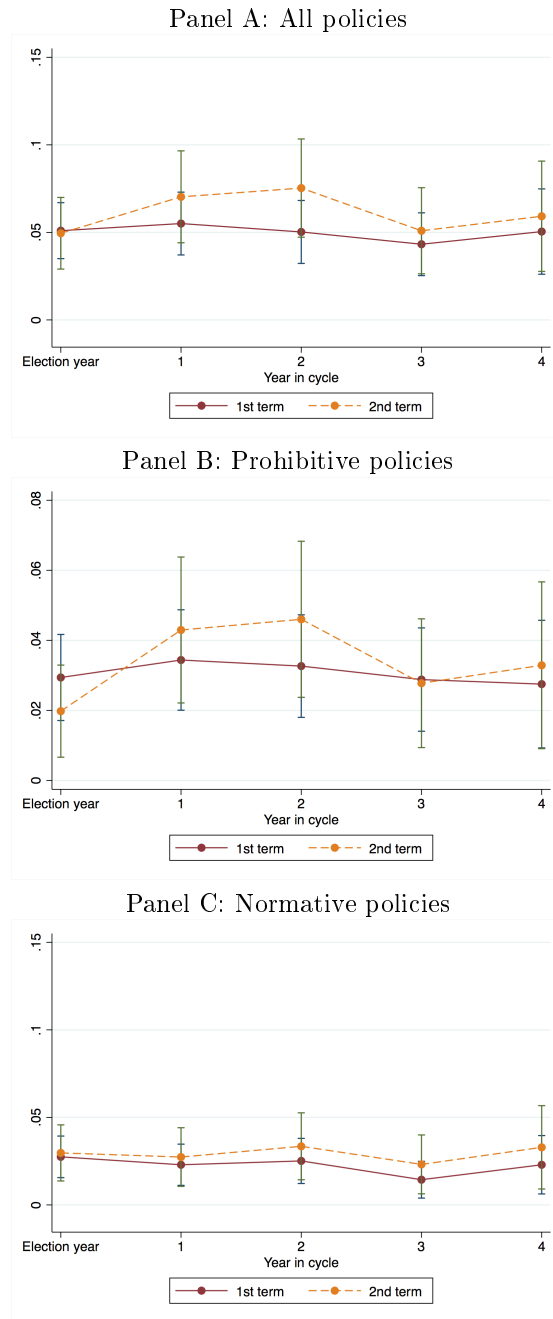
The figures show the occurrence of a religious policy (Y axis) against the revenues and the minimum wage in a binned scatterplot. The probability of religious policies is decreasing in reliable revenue streams ($\hat{\beta} : -.10, t : -4.78$), increases in wages ($\hat{\beta} : .36, t : 2.85$). Sample: 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



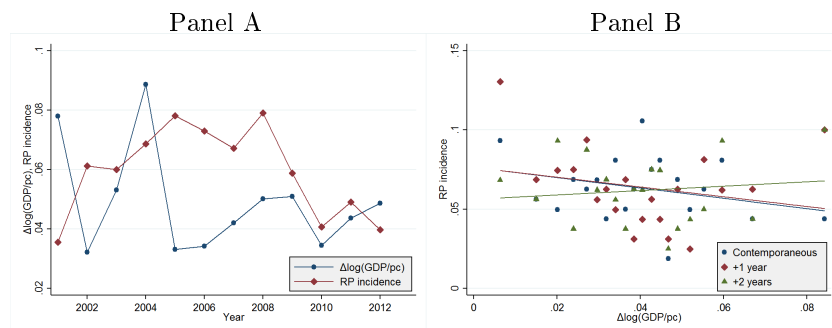
The figure shows an event study where the outcome variable is the number of Islamic schools per 1000 inhabitants in a village. Data comes from the PODES survey. The figure uses village - survey wave observations. An event is defined as the village being in a district that introduced the first religious 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.

Figure F.3: When are religious policies passed?



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

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

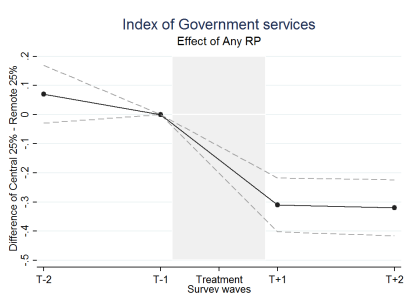
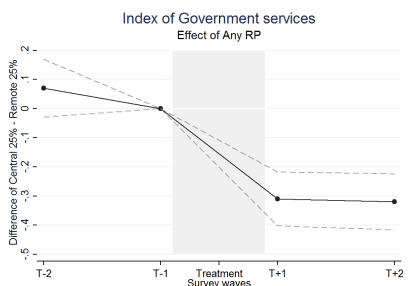
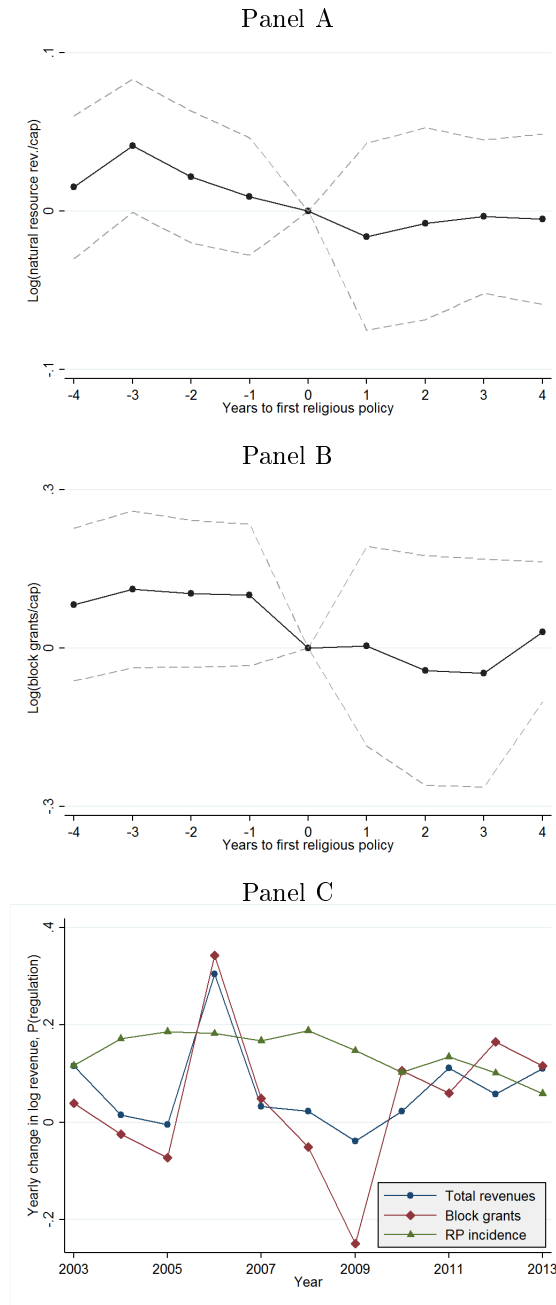


Figure F.6: Villages with and without local police in districts with RP



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 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.

Appendix G : Additional tables

Hererogeneity analysis

Table G.9: Impact of religious policies in current and later cycles

Panel A			
	(1)	(2)	(3)
	Log(wbill)	Log(imp. emp.)	Imp. emp. / pop.
Has RP	0.00496 (0.0188)	0.00857 (0.0179)	0.00340 (0.0337)
Has RP and RP in current cycle	-0.0951*** (0.0159)	-0.0955*** (0.0150)	-0.120*** (0.0274)
Observations	3383	3511	3511
MeanY	25.53	12.82	1.689
Panel B			
	(1)	(2)	(3)
	Poverty rate	Poverty gap	Log(No. of incidents)
Has RP	1.161*** (0.302)	0.222*** (0.0694)	0.242* (0.135)
Has RP and RP in current cycle	-0.910*** (0.260)	-0.204*** (0.0628)	-0.142 (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 average 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 comes 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%.

Violence and religious policies

Table G.10: Perpetrator types

Panel A: P(perpetrator)			
	(1)	(2)	(3)
	Religious group	Government	Other political
Had RP (T-1)	0.194** (0.0888)	0.0369 (0.0686)	-0.0346 (0.0775)
Panel B: Number of incidents by perpetrator			
	(1)	(2)	(3)
	Religious group	Government	Other political
Had RP (T-1)	0.451*** (0.159)	1.351 (1.167)	4.330 (2.817)
Panel C: Number of victims / 100000 people by perpetrator			
	(1)	(2)	(3)
	Religious group	Government	Other political
Had RP (T-1)	0.0874* (0.0477)	1.688 (1.422)	2.315 (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 perpetrator. 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 G.11: Incident types
Total number of victims by incident type

	(1)	(2)	(3)	(4)
	Killed	Injured	Kidnapped	Sexually assaulted
Had RP (T-1)	3.317*** (0.835)	20.57* (10.56)	0.423** (0.211)	4.759 (3.000)
Victims / 100000 people by incident type				
	(1)	(2)	(3)	(4)
	Killed	Injured	Kidnapped	Sexually assaulted
Had RP (T-1)	0.631*** (0.192)	6.752* (3.944)	0.109 (0.0838)	1.743*** (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 G.12: Distance interactions

	(1)	(2)
	Gov. services	P(slum)
Religious policy	-0.260*** (0.0477)	0.0259*** (0.00906)
Any RP X Dist(100km)	0.579*** (0.119)	-0.0340 (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 G.13: Content heterogeneity

	(1)	(2)	(3)
	infra	Gov. services	P(slum)
General RP	-0.180*** (0.0692)	-0.151*** (0.0516)	0.0103 (0.0110)
Prohibitive RP	0.0511 (0.0548)	-0.00199 (0.0456)	0.0136* (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 G.14: Content heterogeneity + distance interaction		
	(1)	(2)
	Gov. services	P(slum)
General RP	-0.165*** (0.0621)	0.0210 (0.0145)
General RP X Dist(100km)	0.0170 (0.159)	-0.0335 (0.0306)
Prohibitive RP	-0.185*** (0.0558)	0.0155* (0.00916)
Prohibitive RP X Dist(100km)	0.623*** (0.144)	-0.00708 (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 G.15: 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 is 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 G.16: Inequality control

Panel A: spending, all years

	(1)	(2)	(3)
	Log(wbill)	Log(imp. emp.)	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)	(2)	(3)
	Log(wbill)	Log(imp. emp.)	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)	(2)	(3)	(4)
	Poverty rate	Poverty gap	Poverty rate	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 G.17: Unemployment rate control

Panel A: spending, all years

	(1)	(2)	(3)
	Log(wbill)	Log(imp. emp.)	Imp. emp. / pop.
Religious policy in cycle	-0.0415** (0.0177)	-0.0448*** (0.0170)	-0.0568* (0.0293)
Observations	3050	3067	3067
MeanY	25.53	12.82	1.689

Panel B: spending, direct elections

	(1)	(2)	(3)
	Log(wbill)	Log(imp. emp.)	Imp. emp. / pop.
Religious policy in cycle	-0.118*** (0.0303)	-0.122*** (0.0299)	-0.193*** (0.0454)
Observations	2242	2259	2259
MeanY	25.53	12.82	1.689

Panel C: poverty indicators

	(1)	(2)	(3)	(4)
	Poverty rate	Poverty gap	Poverty rate	Poverty gap
Religious policy	0.916*** (0.339)	0.188** (0.0792)	-2.197*** (0.817)	-0.517*** (0.189)
Years after RP			0.360*** (0.0867)	0.0826*** (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 G.18: Sectoral change control

Panel A: spending, all years

	(1)	(2)	(3)
	Log(wbill)	Log(imp. emp.)	Imp. emp. / pop.
Religious policy in cycle	-0.0356** (0.0180)	-0.0379** (0.0173)	-0.0417 (0.0307)
Observations	3661	3682	3682
MeanY	25.53	12.82	1.689

Panel B: spending, direct elections

	(1)	(2)	(3)
	Log(wbill)	Log(imp. emp.)	Imp. emp. / pop.
Religious policy in cycle	-0.119*** (0.0300)	-0.117*** (0.0297)	-0.180*** (0.0449)
Observations	2502	2523	2523
MeanY	25.53	12.82	1.689

Panel C: poverty indicators

	(1)	(2)	(3)	(4)
	Poverty rate	Poverty gap	Poverty rate	Poverty gap
Religious policy	1.052*** (0.299)	0.192*** (0.0684)	-1.746** (0.756)	-0.443*** (0.159)
Years after RP			0.334*** (0.0856)	0.0762*** (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 G.19: 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 G.20: Placebo: Government services

	Cross-section IV		1996-2000 panel
	(1) RP	(2) IV	(3) RP+IV
Ever RP	-0.0170 (0.0363)		-0.0156 (0.0388)
Average of schools in region (log)		-0.0155 (0.0589)	-0.00940 (0.0631)
Observations	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 G.21: Placebo: Prevalence of slums

	Cross-section IV		1996-2000 panel	
	(1)	(2)	(3)	(4)
	RP	IV	RP+IV	IV
Ever RP	0.0138 (0.00984)		0.0161 (0.0101)	
Average of schools in region (log)		-0.00908 (0.0124)	-0.0154 (0.0127)	-0.282*** (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%.

Assessing the potential for public-private substitution

Table G.22: Effect of religious policies on the Infrastructure Index

	(1)	(2)	(3)	(4)
Religious policy	-0.0622 (0.0520)	-0.155*** (0.0591)		
Any RP X Dist(100km)		0.309*** (0.118)		
General RP			-0.180*** (0.0692)	-0.214*** (0.0814)
General RP X Dist(100km)				0.0963 (0.170)
Prohibitive RP			0.0511 (0.0548)	-0.0356 (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 G.14 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 G.23: Infrastructure Index - IV				
	Baseline		+ Political vars.	
	(1)	(2)	(3)	(4)
	FE	IV FE	FE	IV FE
Religious policy	-0.0596 (0.0526)	-0.250 (0.347)	-0.0589 (0.0524)	-0.250 (0.349)
Islamic schools (log)				
Administrative village			-0.0349 (0.0462)	-0.0371 (0.0468)
Remote village			-0.0536 (0.0647)	-0.0390 (0.0681)
Village has Hansip police			-0.0120 (0.0134)	-0.0116 (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 G.24: Infrastructure Index - First stage and reduced form				
	First stage		Reduced Form	
	(1)	(2)	(3)	(4)
Islamic schools / 1000 people (other villages)	0.360*** (0.110)	0.358*** (0.110)	-0.0899 (0.131)	-0.0895 (0.131)
Islamic schools (log)				
Administrative village		-0.00337 (0.0228)		-0.0363 (0.0463)
Remote village		0.0661** (0.0275)		-0.0555 (0.0648)
Village has Hansip police		0.00385 (0.00651)		-0.0126 (0.0134)
Observations	269552	269201	269552	269201

This is a version of Table ?? 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 G.25: IV regression: Government services

	Baseline		+ Political vars.	
	(1) FE	(2) IV FE	(3) FE	(4) IV FE
Religious policy	-0.0855** (0.0425)	-0.876*** (0.308)	-0.0861** (0.0423)	-0.885*** (0.310)
Islamic schools / 1000 people	0.0188 (0.0170)	0.0470*** (0.0179)	0.0186 (0.0170)	0.0469*** (0.0179)
Administrative village			-0.00889 (0.0366)	-0.0180 (0.0401)
Remote village			0.0271 (0.0603)	0.0884 (0.0696)
Village has Hansip police			0.0689*** (0.0141)	0.0700*** (0.0157)
Observations	269552	269549	269201	269198

This is a version of Table 7 where the leaveout mean is constructed with the omission of immediate neighbors (villages within the same subdistrict). *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table G.26: First stage and Reduced form

	First stage		Reduced Form	
	(1)	(2)	(3)	(4)
Average of schools in dift. subdist.	0.0818*** (0.0208)	0.0813*** (0.0208)	-0.0717*** (0.0255)	-0.0719*** (0.0255)
Islamic schools / 1000 people	0.00200 (0.00597)	0.00212 (0.00596)	0.0452*** (0.0166)	0.0450*** (0.0165)
Administrative village		-0.0105 (0.0234)		-0.00874 (0.0370)
Remote village		0.0595** (0.0287)		0.0357 (0.0602)
Village has Hansip police		0.00239 (0.00654)		0.0678*** (0.0141)
Observations	269549	269198	269549	269198

This is a version of Table ?? where the leaveout mean is constructed with the omission of immediate neighbors (villages within the same subdistrict). *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table G.27: IV regressions: Prevalence of slums

	Baseline		+ Political vars.	
	(1) FE	(2) IV FE	(3) FE	(4) IV FE
Religious policy	0.0160** (0.00687)	0.182** (0.0767)	0.0159** (0.00688)	0.183** (0.0770)
Islamic schools / 1000 people	0.00598** (0.00264)	0.0000832 (0.00208)	0.00606** (0.00266)	0.000147 (0.00209)
Administrative village			0.0290*** (0.0104)	0.0309*** (0.0115)
Remote village			0.00686 (0.00880)	-0.00593 (0.0114)
Village has Hansip police			0.00125 (0.00315)	0.000913 (0.00328)
Observations	269552	269549	269201	269198

This is a version of Table 8 where the leaveout mean is constructed with the omission of immediate neighbors (villages within the same subdistrict). *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table G.28: First stage and Reduced form

	First stage		Reduced Form	
	(1)	(2)	(3)	(4)
Average of schools in dift. subdist.	0.0818*** (0.0208)	0.0813*** (0.0208)	0.0149*** (0.00448)	0.0148*** (0.00445)
Islamic schools / 1000 people	0.00200 (0.00597)	0.00212 (0.00596)	0.000446 (0.00184)	0.000533 (0.00184)
Administrative village		-0.0105 (0.0234)		0.0290*** (0.0104)
Remote village		0.0595** (0.0287)		0.00494 (0.00877)
Village has Hansip police		0.00239 (0.00654)		0.00135 (0.00316)
Observations	269549	269198	269549	269198

This is a version of Table ?? where the leaveout mean is constructed with the omission of immediate neighbors (villages within the same subdistrict). *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table G.29: Placebo: Government services

	Cross-section IV			1996-2000 panel
	(1)	(2)	(3)	(4)
	RP	IV	RP+IV	IV
Ever RP	-0.0170 (0.0363)		-0.0154 (0.0388)	
Average of schools in dift. subdistr.(log)		-0.0151 (0.0575)	-0.00927 (0.0615)	0.217 (0.191)
Observations	45556	45553	45553	91040

This is a version of Table G.20 where the leaveout mean is constructed with the omission of immediate neighbors (villages within the same subdistrict). *: significant at 10%; **: significant at 5%; ***: significant at 1%.

Table G.30: Placebo: Prevalence of slums

	Cross-section IV			1996-2000 panel
	(1)	(2)	(3)	(4)
	RP	IV	RP+IV	IV
Ever RP	0.0138 (0.00984)		0.0161 (0.0101)	
Average of schools in dift. subdistr.(log)		-0.00880 (0.0122)	-0.0149 (0.0125)	-0.176*** (0.0380)
Observations	45556	45553	45553	91040

This is a version of Table G.21 where the leaveout mean is constructed with the omission of immediate neighbors (villages within the same subdistrict). *: significant at 10%; **: significant at 5%; ***: significant at 1%.