# Secret Authoritarian Legacies, Transitional Justice, and the Quality of Representation

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**Abstract**

(150 words)

We formalize the classical "forward-looking" argument in support of personnel transitional justice (lustration). This argument states that by revealing information about former secret collaborators of the authoritarian regime among politicians, lustration prevents former authoritarian elites from extorting policy concessions from elected politicians. Absent lustration, former agents of the secret police could blackmail former collaborators-turned-politicians and threaten to reveal "skeletons in their closets" if politicians refused to implement policies these former agents desire. Hence, implementing lustration laws should allow all politicians to become responsive to their constituencies. We show that whether lustration indeed prevents such distortions in representation depends on its severity, the extent to which dissidents-turned politicians face consequences if their skeletons come out, and media freedom. Surprisingly, the ideological distance between the successor autocrats and former dissidents is not relevant. We test this theory with an original transitional justice dataset covering 43 countries that transitioned to democracy since 1945.

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# 1 Introduction

In February 2016, the widow of Czesław Kiszczak, former Chief of the Communist Secret Police in Poland, discovered a thick secret police file containing evidence that Lech Wałęsa, former Nobel Peace Prize Laureate and anti-communist dissident leader, had collaborated with the Communist Secret Police between 1970 and 1976. In November 1990, Wałęsa was elected president for a five year term, which ended before Poland implemented its lustration program, which would have vetted Wałęsa for connections to the communist secret police. Although Wałęsa's collaboration preceded his career as dissident and trade union organizer, Kiszczak might have pressured him to avoid implementing certain policies by threatening to release the compromising file. This led many people to question the quality of his presidency and the extent to which he represented interests of the electorate rather than those of the former secret police. It left others wondering about the prevalence of such acts of blackmail. How often did former authoritarian elites pressure their former spies into following their policy preferences instead of representing voters? To the extent that we consider representing voters' wishes a marker of high quality representative democracy, the revelation left many wondering just how representative Polish democracy was of its electorate.

With a few exceptions (Yarhi-Milo 2013, Felli & Hortala-Vallve 2015), the use of secrets and blackmail in effecting policy change had not received a lot of scrutiny from political scientists. Yet, in 2016, the term *kompromat* made front page news as it was used to describe Russia's attempts to intervene in US policy. Generally, *kompromat* (compromising materials) refers to either embarrassing information or evidence of a person's illegal activity, which could damage that person's career or open her up to prosecution should it be revealed. As Keith Darden points out, this kind of data was routinely collected by authoritarian secret services in the Soviet Era and used to control people through blackmail by threatening the "compromised" that the security services would release the damaging information to the public or—worse— to prosecutors (Darden 2001). Yet embarrassing or

damaging information, even if it was collected by authoritarian security forces, may also be put to use when the authoritarian regime itself is long gone.

Political scientists do not dispute that the power of former authoritarian elites extends beyond the life span of an authoritarian regime. This can happen for two reasons. First, autocrats are better positioned to capture state resources prior to the democratic transition, which they can then use in a clientelistic fashion to stay in power (Brun & Diamond 2014). The outgoing autocrats' access to resources can be cut off if they or their successors are voted out of power following the transition to democracy, but as various cases around the world demonstrate, this removal is frequently just temporary (Kitschelt 1999). Scholars have attributed the revival of successor authoritarian parties to the organizational advantage they hold over parties that are new to the party system. This organizational advantage allows them to make better use of state resources when they eventually do find themselves in government (Grzymala-Busse 2002). Second, autocrats may retain power after the transition, because the transition itself is endogenous to their strength. Some scholars have explained the advantage of former authoritarian elites following democratic transitions by arguing that they concede their power only in moments when they can expect to thrive under democratic conditions at least as well as they did under autocracy (Slater & Wong 2013).

Given these two sources of power the former autocrats have– one exogenous and the other endogenous to the transition itself— the only way to reduce this unfair advantage is Transitional Justice (TJ). Scholars of TJ have argued that these laws can undercut the privileged position of members of the former autocrats, their parties, enforcement apparatus as well as its collaborators (Stan & others 2009, David 2011).

TJ comprises of the "formal and informal procedures implemented by a group or institution of accepted legitimacy around the time of transition out of an oppressive or violent social order, for rendering justice to perpetrators, and their collaborators, as well as victims" (Kaminski, Nalepa & O'Neill 2006). The four pillars of transitional justice are

truth commissions, trials, victim compensation, and lustration. If the normative objective is barring former authoritarian elites from policy influence, then the transitional justice mechanism of paramount importance is lustration, which vets candidates for public office for ties to the former authoritarian secret police. It does this through opening archives of the secret police of the former authoritarian regime to uncover who among persons running for public and political office had worked for the secret police prior to the democratic transition. The proven collaborators are then either explicitly banned from holding office or information about their collaboration is released to the voters, who then can decide whether to still cast their votes on thus compromised politicians.

There are many retrospective or backward-looking arguments in support of lustration, ranging from victims of spying having the right to know who informed on their activities (Stan 2012) to the need to prevent former spies and their leading officers from playing key roles in public service (Nedelsky 2013). Yet some scholars dismiss transitional justice for this retroactive character and argue that "living well is the best form of revenge" (Halmai, Scheppele & McAdams 1997). Among the advocates of this so-called "Spanish Model of Transitional Justice" are Elster (1998) and Holmes (1994), who used Spain's most benign way of dealing with former authoritarian collaborators (sealing off the archives of Franco's secret police) to build their case that "doing nothing" is the best approach for new democracies to deal with past authoritarian regimes (Elster 2004). Extensive scholarship on personnel transitional justice corroborates this endorsement by showing misuses of lustration, de-communization, and de-ba'athification. One example is Iraq's policy aimed at purging new democratic institutions of former Ba'athists. De-ba'athification prevented 185 members of Saddam Hussein's party, mostly Sunnis, from running for the legislature in 2003. Despite its promise to promote societal reconciliation, it ignited ethnic tensions (David 2006). France's policy of *épuration* banning former Vichy collaborators from holding office following WWII had very similar effects (Kritz 1995, Elster 2004).

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However, although following the Spanish model and "doing nothing" need not produce immediate negative consequences, it may strengthen the power of authoritarian networks. If democracy survives, damaging information collected by the former authoritarian secret police for the benefit of authoritarian elites may turn elected politicians into clients of agents who threaten to reveal their "skeletons in the closet" (Nalepa 2010). Forgiving and forgetting may sabotage the capacity for elected politicians to represent voters, a phenomenon that is hard to pick up on for studies focusing on the immediate aftermath of transition. Thus, even if one agrees with the arguments of Elster and Holmes questioning the validity of the backward looking arguments for lustration, it may still be the case that lustration laws can, when appropriately designed and implemented, undercut the growth of authoritarian networks by exposing the files and secrets of the former authoritarian police. According to this argument, revealing evidence of human rights violations and collaboration with members of the ancién regime prevents former authoritarian elites from influencing policy in new democratic polities. Where lustration is lacking, former authoritarian elites can pressure politicians into policy concessions by threatening to reveal compromising information that could jeopardize the politicians' careers.

Our paper seeks to examine in more detail this forward-looking argument for transitional justice. We develop a theoretical model reconstructing the blackmail mechanism that lustration is supposed to undercut. By making public the secret information that former autocrats could use to influence policy-making in the new democracy, lustration should improve its quality of representation. We use a game theoretic model of incomplete information to reconstruct the mechanism through which lustration laws prevent political participation of persons whose dependence on the former security apparatus interferes with their ability to faithfully represent policy preferences of the electorate.

Our first contribution is to uncover circumstances under which lustration will indeed improve the ability of politicians to represent preferences of their constituents as opposed to former authoritarian elites. The crux of our theory is the identification of conditions

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under which former secret police agents extract policy concessions from politicians even after lustration has been implemented. We identify (pooling and semi-pooling) equilibria under which lustration is compatible with bluffing and, consequently, lustration does not prevent departures from democratic representation. We also present an identity result, which shows that irrespective of the equilibrium being played, the formula describing misrepresentation of voters' preferences is the same across all types of equilibria.

Our second contribution to the literature on transitional justice is empirical. In order to test the implications of our theory we have created a novel dataset comprising of a time series cross-section of personnel transitional justice events in 43 post-authoritarian states. Despite a growing number of empirical studies examining the broad impact of lustration and transitional justice on democratic stability and peace (Olsen 2010, Van der Merwe, Baxter & Chapman 2009, Thoms, Ron & Paris 2008), there is a knowledge gap pertaining to the impact of personnel transitional justice policies, such as lustration, on the long term quality of democratic representation. The first generation of empirically oriented transitional justice research concentrated on explaining patterns and trajectories of implementing specific transitional justice measures (Stan & others 2009, Lundy & McGovern 2008, Mallinder 2008, Pettai & Pettai 2014). More recently, to understand the impact of transitional justice on the quality of democracy, scholars have begun to investigate transitional justice as an independent variable. Noteworthy because of their coverage of lustration policies are contributions by Horne (n.d.) and Olsen (2010).[1] Yet while Olsen (2010) aggregates all lustration activity into a dichotomous variable summarizing the presence or absence of lustration following the transition, Horne (n.d.) provides a rich description of lustration trajectories for as many as 12 countries, but only in Post Commu-

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[1]Horne examines the relationship between the nature of transitional justice mechanisms and trust, suggesting that revealing the notoriety with which citizens were spying on one another may decrease interpersonal trust in countries with a large network of covert collaborators. At the same time, her research indicates that a wide and compulsory lustration procedure may result in substantial bureaucratic turnover, thereby increasing political trust in governmental institutions. Horne's research was among the first to disaggregate trust in this manner; much of the previous literature assumed that transitional justice was essential for trust-building.

nist Europe.[2]

The danger of reducing a sometimes complex progression of a transitional justice proposal through the legislative process to a dummy variable is underscored by the fact that while in some countries the bulk of lustration activity occurs in the immediate aftermath of transition (as in Kenya and Ghana), in others, the process is far more protracted (as in most East European countries). Furthermore, once introduced transitional justice legislation seems to prevail in certain countries (as in the Czech Republic and for a long time in the Philippines), whereas in others they are accompanied by reversals (as in Indonesia).[3]

The next section introduces our formal model and our main (identity) result. It also contains the model's empirical implications. Section 3 presents the empirical strategy for evaluating two hypotheses about the quality of representation. Section 4 concludes. All proofs are relegated to the formal appendix.

## 2  The Formal Model

Formal modeling has scarcely been used to address questions involving transitional justice[4] and to the best of our knowledge has not been used in predicting the effect of personnel transitional justice mechanisms on the long term quality of democracy.

Can lustration limit the influence of former authoritarian elites? We approach this question by developing a formal theoretical model that reconstructs the mechanism through which former secret police officers influence politicians' policy-making decisions. Solving this model allows us to examine to what extent transitional justice indeed reduces the de-

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[2]Indeed, most contributions to the lustration literature suggest that this transitional justice policy is almost exclusively restricted to Post Communist Europe (Mayer-Rieckh & De Greiff 2007, Ellis 1996, Closa Montero 2010, Letki 2002, Volčič & Simić 2013, Stan 2013, Stan & Nedelsky 2015). This is however an artifact of restricting the search for personnel transitional justice to the term "lustration" and its derivatives, when in fact purging the state apparatus of members of the former authoritarian regime and their collaborators is called by different names in different parts of the world, from "vetting" and "purging" to "house-cleaning" as well as specific terms, such as "de-nazification," "de-communization," or "de-ba'athification."

[3]To see this most clearly, we refer to the readers to Figure 3 discussed in section 3

[4]Exceptions include Ritter & Wolford (2012), Nalepa (2010) and Casper & Tyson (2014)

pendence of politicians on their former leading officers. These former leading officers are referred to here as "agents."

Our model indicates that the degree of ideological polarization between the secret police agent and the politician is inconsequential for the quality of representation. This is surprising because it indicates that politicians do not give in to blackmail more frequently when they are asked to deviate less from their ideal point than when they are asked to deviate more. The model indicates that the quality of representation increases with the severity of lustration and that it is easier to achieve as the price of having skeletons in the closet revealed decreases. Thus, our model shows that, ironically, caring about former collaborators not being in politics increases the influence of authoritarian elites via this blackmail mechanism.

Our model implies also that when a country is capable of scrutinizing false information about skeletons in the closet (for example, by having a free media or a comprehensive and effective right to information law), blackmail occurs only in instances where evidence of collaboration does exist. In other words, transitional justice works as intended only when the damaging information can be objectively verified. The logic driving this finding is as follows: former collaborators-turned-politicians cannot know with certainty if files against them survived the transition, but former agents can bluff politicians into thinking such evidence exists if their bluff cannot be easily challenged. Hence, if such information cannot be properly scrutinized, former agents will be able to extort policy concessions from politicians even when they actually do not have skeletons to reveal (bluffing). In equilibria that allow for bluffing (pooling and semi-pooling equilibria), blackmail is effective regardless of lustration. These equilibria are contrasted with the separating equilibria, where blackmail is only effective insofar as lustration procedures left evidence behind.

A possible criticism of how well our model matches the empirical interpretation of blackmail with secret police files is that surely, if someone never consciously collaborated with the secret police, (s)he could be certain that no evidence of his or her collaboration

exists. Consequently, "bluffing with secret police files" would be limited to politicians who collaborated with the secret police and those who are innocent of such collaboration would not be affected. In response, we point to the extensive literature indicating that secret police agents routinely falsified evidence of collaboration in order to improve the appearance of their performance and to pocket compensation that was intended to be given to collaborators in exchange for information (Horne 2009, David 2003). Not infrequently were informal conversations recorded as "reports." The victim of such falsified evidence could never be therefore certain if evidence of the "collaboration" exists or not (Nalepa 2008). Thus, because of such instances of unintentional collaboration, we assume that theoretically, bluffing is always possible.

Our model features a former collaborator who is elected to public office following a transition to democracy and his agent who threatens to reveal embarrassing information against the former collaborator, unless the latter implements policies favored by the agent. The model assumes that, absent the pressure of blackmail, politicians would carry out the policies their voters' desire. The quality of representation suffers when the politician is vulnerable to extortion by the agent and implements the policy preferred by his former secret police officer, instead of the policy preferred by voters.

The key tension in our model comes from the fact that while the agent knows whether or not he possesses the evidence he threatens to release, the former collaborator does not. Notice, that the more stringent the lustration law, the less likely it is that such incriminating evidence is in possession of the former agent. Therefore, we model the severity of lustration as the probability that evidence of the politician's collaboration remains in the possession of the agent. Returning briefly to our example from the introduction, if harsh lustration had been implemented in Poland before Wałęsa served as President, the secret police chief would not have been able to use the files against Wałęsa.

## 2.1 Sequence of play

The game starts with a move of Nature, which with probability $p\epsilon(0,1)$ determines that evidence against the collaborator is in the possession of the agent and with probability $1-p$ that it is not. The politician, who we will call the $Dissident$ $(D)$, knows the value of $p$, but not its specific realization.[5] In the second stage of the game, the $Agent$ $(A)$, with an ideal point $a=0$, decides whether or not to make a policy demand towards the $Dissident$. For simplicity, we represent his decision by one of two exogenously given actions {Make Demand, Not Make Demand}. In the third stage, the $Dissident$ with ideal point $d\epsilon(0,1)$, who observes whether or not a demand on him has been placed, but not whether evidence against him exists (the realization of $p$) decides whether to make a policy concession in response to the Agent's demand and also how big of a concession to make. This is modeled as proposing a policy $x \in [a,d]$. If evidence against the Dissident does not exist, the game ends here. But if evidence does exist, the $Agent$ decides whether or not to reveal the evidence against the Dissident. The idea here is that if the concession is deemed insufficient by the Agent, he can exercise his threat and indeed reveal the compromising evidence against the Dissident.

The payoffs are a function of the Euclidean distance between the players' respective ideal points and implemented policy. We also assume that the $Agent$ pays a cost $c>0$ for making a demand when he does not have evidence in his possession. This makes sense, as trying to coax a politician into believing that the agent has evidence against him may require obtaining and disclosing some plausible information about the $Dissident$'s past which could be verified by the media, if the media has sufficient independence.

In addition, if evidence against him is revealed, the $Dissident$ pays the cost of being fired, $F$. The utilities, along with the entire game tree are presented in Figure 1 below.

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[5]We refer to the ex-collaborator turned politician as "dissident" to avoid confusing his ideal point ($d$) with the probability that secret files are in the possession of the agent. This nomenclature is also justified by the fact that in places where collaboration with the secret police was of clandestine nature, as was the case in PostCommunist Europe, the revelations are most damaging to former dissidents (Nalepa 2010)

Figure 1: Lustration Blackmail Game
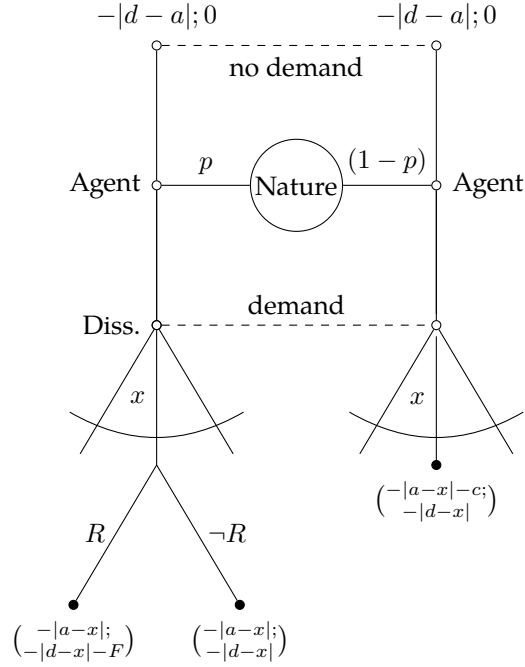


## 2.2 Strategies and beliefs

A strategy for the agent is a triple $s_A = (z^E, r, z^{\sim E})$, where $z^E \epsilon \{0, 1\}$ denotes $A$'s action when he is in possession of evidence against $D$ and $z^{\sim E} \epsilon \{0, 1\}$ denotes $A$'s action when he is not in possession of such evidence, with action $1$ denoting "make demand" and action $0$ denoting "do not make demand." Suppose that $r \epsilon \{Reveal. \sim Reveal\}$ denotes the action taken by $A$ following $D$'s counteroffer $x$. This part of $A$'s strategy can best be represented as a rejection region $R = \{x : x \in R \to A \text{ reveals evidence}\}$. A strategy for the dissident is a proposal $x \in [a, d]$ that $D$ makes in the event that $A$ makes a policy demand. Note that $D$ can simply ignore the demand if he choses $x = d$. [6]

This structure resembles that of a signaling game, in which $A$ is the sender of the message, while $D$ is the receiver. In line with these types of games, the dissident may have a chance to update his prior belief about the type of agent he is facing—whether it is an

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[6]Note that since $D$ does not observe whether evidence exists or not, but only observes whether a demand was placed $(1)$ or not $(0)$, we only need a single action do describe his strategy.

agent equipped in evidence of collaboration or not—after observing the agent's action. Depending on the relative magnitudes of $d, c$ and $F$, this might be a signaling game or a cheap talk game. We will assume, not without loss of generality, but in order to analyze the most interesting cases of this general setup, that $0 = a < d$. The solution concept we use is that of Perfect Bayesian Equilibrium (PBE). This means that in addition to strategies being best responses to each other, we also have to specify the $Dissident$'s beliefs and those beliefs must be consistent with what is observed in equilibrium play.
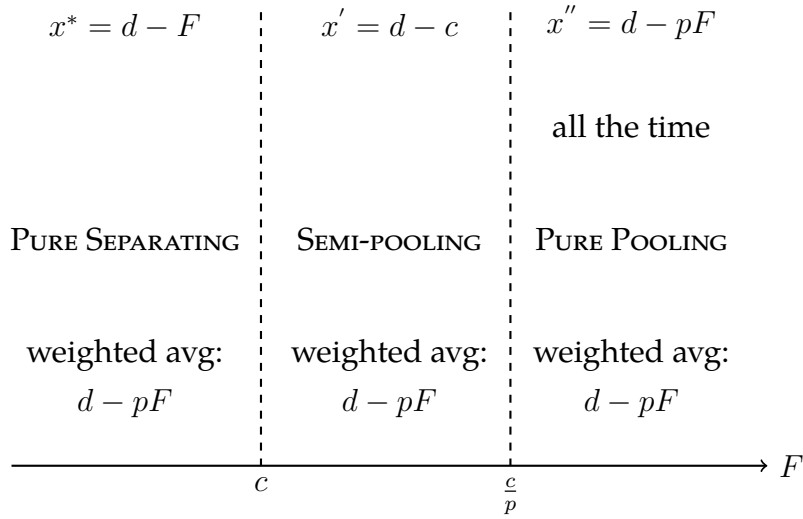
## 2.3 Results and Discussion

The three types of equilibria are presented in proposition 1 as well as in Figure 2 as a function of $F$, the dissident's cost of being fired. The set of conditions for the pooling equilibria is distinct from the set of conditions defining the separating and semi-pooling equilibria, with the critical element being the magnitude of $F$ relative to the cost of bluffing, $c$ and the probability that evidence against the dissident exists, $p$.

**Proposition 1**

1. Let $x^* \equiv d - F$ and $R^* \equiv (-d + F, d - F)$. For $F < c$ and the posterior beliefs of $D$ be described by $Pr(E|0) = 0, Pr(\sim E|0) = 1, Pr(E|1) = 1, Pr(\sim E|1) = 0$, $(1, R, 0; x^*)$ is a (pure separating) PBE.

2. Let $x'' \equiv d - pF$ and $R'' \equiv (-d + pF, d - pF)$. For $F \geq \frac{c}{p}$ and the posterior beliefs of $D$ be described by $Pr(E|1) = p, Pr(\sim E|1) = 1 - p$ $(1, R'', 1; x'')$ is a (pure pooling) PBE

3. Let $x' \equiv d - c$, $R' \equiv (-d + c, d - c)$ and $\lambda^* \equiv \frac{p(F-c)}{(1-p)c}$. For $c \leq F < \frac{c}{p}$ and the posterior beliefs of $D$ be described by $Pr(E|0) = 0, Pr(\sim E|0) = \lambda^*, Pr(E|1) = 1, Pr(\sim E|1) = 1 - \lambda^*$, $(1, R', \lambda; x')$ is a (semi-pooling) PBE.

Informally, Proposition 1 states first, that for very low costs of revealing skeletons in the closet relative to costs of bluffing, there is a separating equilibrium in which the agent only makes a demand if evidence exists and the dissident makes a counteroffer $x^* = d - F$. Second, it states that if the costs of revealing skeletons are very high relative to the costs of bluffing, there is a pooling equilibrium in which the agent always makes a demand,

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Figure 2: Equilibria

$$x^* = d - F \qquad x^{'} = d - c \qquad x^{''} = d - pF$$

all the time

| PURE SEPARATING | SEMI-POOLING | PURE POOLING |

weighted avg: $\qquad$ weighted avg: $\qquad$ weighted avg:
$d - pF$ $\qquad\qquad$ $d - pF$ $\qquad\qquad$ $d - pF$

$$\xrightarrow{\hspace{8cm}} F$$

$c \qquad\qquad \frac{c}{p}$

irrespective of whether evidence exists or not and the dissident makes a counteroffer $x^{''} = d - pF$. Third, for moderate costs of revealing skeletons in the closet, there is a semi-pooling equilibrium, where the agent sometimes bluffs (i.e., makes a demand even when he has no evidence) and the dissident responds with a counteroffer $x^{'} = d - c$. In all equilibria, the counter-offer, when made, is accepted.

The derivation of pure separating, semi-pooling, and pooling equilibria is provided in the Formal Appendix. Below we also present an identity result characterizing the PBE outcome, followed by a discussion of this result.

Recall, that our model assumes that absent pressure from the agent, the dissident would carry out the policies the voters desire, which correspond to his ideal point. Ultimately, what we are most interested in across all equilibria is the quality of representation: the extent to which the dissident can withstand pressures from the agent to abandon his ideal point, $d$. In addition to the values of $x^*$, $x^{"}$, and $x'$, which correspond to the counteroffer proposed by the dissident (in the the separating, pooling and semi-pooling equilibria, respectively), we present the average levels of mis-representation. We interpret the quality of representation as resistance to the agent's blackmail. Conversely, misrepresentation

is how we interpret the departures from $d$, the Dissident's ideal point.

In order to derive the expected level of misrepresentation we weigh the PBE outcome in each equilibrium by the frequency of its occurrence. In the case of the pure separating equilibrium, it is simply $p(d - F) + (1 - p)d$, as the agent only proposes $x^* = d - F$, when evidence exists, which is $p$ of the time. The remaining $1 - p$ of the time, he reverts to $d$. In the case of the pure pooling equilibrium, the average policy is implemented at $1 * (d - pF)$, as the agent always places a demand and the dissident always responds with $x" = d - pF$. In the case of the semi-pooling equilibrium, the calculation of the policy implemented is somewhat more complex, because the agent places a demand when evidence exists, $p$, and $\lambda$ of the time when it does not exist. Hence the total frequency of placing a demand is given by $p + (1 - p)\frac{p(F - c)}{(1 - p)c}$. The dissident responds to this demand with $x'$, bringing the expected policy outcome to $(d - c)(p + (1 - p)\frac{p(F - c)}{(1 - p)c} + (1 - p)(1 - \frac{p(F - c)}{(1 - p)c})d$, which as in the previous two cases reduces to $d - pF$. These expected policy outcomes are presented as a function of $d$ and $p$ in Figure 2 and described below in Proposition 2, which follows directly from our derivation above.

**Proposition 2** The PBE outcome, interpreted as the expected level of misrepresentation, is given by the same formula across all three equilibria: $d - pF$.

A few implications follow from proposition 2. First, departures from perfect representation are constant for all values of $d$. This means that the effectiveness of blackmail does not depend on how far apart the ideal points of the agent and dissident are located. This is somewhat surprising as intuitively, we would expect the ideological proximity of ideal points to matter. [7] Second, it is clear that the quality of representation is decreasing in $p$. This is very intuitive as the probability that evidence does not exist induces the dissident to make a counteroffer closer to his ideal point. Third, quality of representation is highest under the pure separating equilibrium, followed by the semi-pooling equilibrium, followed by the pure pooling equilibrium, indicating that it decreases with the cost

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[7]As a robustness check, we solve the same model but using quadratic as a opposed to linear spacial preferences and for two of the equilibria, the effect of $d$ was negative

of firing.

Finally, although misrepresentation is the same under the separating equilibrium as under the semi-pooling equilibrium, and especially under the pooling equilibrium, transitional justice does not prevent blackmail according to the logic its proponents would like it to. In other words, the same quality of representation is achieved for the "wrong reasons." This can be explained as follows. Under the pure separating equilibrium, bluffing never occurs and the amount of misrepresentation tracks the amount of evidence left in the agent's possession (so it is directly responsive to the severity of lustration). Thus, under the separating equilibrium lustration works exactly as advocated by the proponents of the forward-looking argument for lustration. This equilibrium becomes easier to achieve as the costs of bluffing increase, because for higher values of $c$, it is easier for $F$ to be low enough to satisfy the $c < F$ constraint.

In the semi-pooling equilibrium $(0, R', \lambda^*; x')$, the Agent always makes a demand whenever the evidence exists and also when evidence does not exist with probability $\lambda^*$. The key feature of the semi-pooling equilibrium is that when it is played, the agent extracts policy concessions, undermining representation, even when evidence does not exist with probability $\lambda^*$. Misrepresentation does not exactly track transitional justice, but at least in $1 - \lambda^*$ of the cases when evidence has been swept by the transitional justice process, bluffing does not take place. This equilibrium occurs when $c < F < cp$.

In the pooling equilibrium, bluffing occurs every time evidence does not exist regardless of how much evidence transitional justice has left in the hands of agents. Either the separating or the semi-pooling equilibrium is better from the normative point of view than the pooling equilibrium, where the frequency of blackmail is completely orthogonal to the actual evidence of collaboration. Under this equilibrium, the extent to which lustration has successfully eliminated evidence of collaboration has no bearing on the effectiveness of blackmail. Blackmailing or bluffing occurs frequently and lustration is completely dysfunctional. This last equilibrium occurs when the possibility of having skeletons in

the closet revealed by the former secret police is very damaging to the politician's career $(F > \frac{c}{p})$.

Consequently, although it is clear that raising $c$ has no effect on the total quality of representation, a higher $c$ makes the $F < c$ and $F < \frac{c}{p}$ constraints easier to satisfy. Thus, it makes the separating equilibrium more likely vis. a vis. the semi-pooling equilibrium or the semi-pooling equilibrium more likely vis. a vis. the pure pooling equilibrium. As a result, raising $c$ makes transitional justice work more like the advocates of the forward-looking argument for lustration want it to work.

## 3   Empirical Analysis

The model presented in this paper describes behavior of individual politicians and officers of former authoritarian regimes. Data on political preferences of individual officers and the quality of representation at the level of individual legislators is very difficult to obtain, so we will rely on slightly more aggregated information to evaluate our model. Namely, we test the implications of our model using a measure of party programaticness as an observable implication for high-quality representation. Programmaticness is defined in the literature as a situation where parties run on policy platforms that are recognizable to voters, voters support parties because of those platforms and can later hold them accountable for implementing those platforms (Kitschelt & Freeze 2010).

We test the empirical implications of the model using an original dataset of partisan programaticness in countries that transitioned from party-based authoritarian rule. Since our argument is that effective blackmail results in departures from policies that represent the electorate, the outcome of interest is the degree to which a party implements policies that correspond to the platform that voters elected it for. Hence, effective blackmail is observable as a decrease in a party's programaticness, whereas parties that are not subjected to blackmail by former authoritarian elites should, on average, be more programmatic.

Our dependent variable builds on the measure of programmaticness constructed by Herbert Kitschelt and his co-authors (Kitschelt, Freeze, Kolev & Wang 2009, Kitschelt & Freeze 2010). Their original measure was constructed from a large-scale survey of experts in 88 different countries answering questions about the cohesion, salience, and polarization of 506 parties. Programmaticness was conceptualized as building on these three aspects and an index was constructed based on responses to questions tapping into each of these components.[8] Given the conceptual proximity between their polarization and the distance in our model between ideal points of the dissident and agent, we chose to create a new measure that leaves out questions pertaining to polarization. The result is a score between $0$ and $1$ that comprises just the cohesion and salience dimensions of the original variable. Programmaticness is measured at the party level, as in Kitschelt et. al..

The first implication of our formal model is that the probability with which evidence against the dissident exists (parameter $p$ in the model) decreases the degree of programmatic representation. To operationalize $p$, we take advantage of lustration policies. As discussed throughout this article, lustration unearths existing evidence of former collaborators of the authoritarian regime, in an effort to prevent them from entering into politics. The extent to which lustration within a country has been successful corresponds to the probability that evidence of collaboration remains in the hands of some former agent. Thus, increases in the severity of lustration correspond to lower values of $p$.

Most cross-national empirical analyses of transitional justice rely on dichotomous variables to indicate whether lustration policies were implemented on a given country after transition.[9] However, relying on a dichotomous variable risks missing relevant variation: countries that implemented lustration share the same value regardless of the severity or depth of the specific laws and policies implemented.

To avoid lumping countries in only two categories (no lustration/lustration), we built

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[8]The Empirical Appendix provides full details on how this index was constructed

[9]See for example the *Transitional Justice Database Project*, or the *Post-conflict Justice and Sustainable Peace* projects.

our own dataset of chronologies of personnel transitional justice events. For each country, transitional justice events were coded as either positive or negative. Positive events are those aimed at introducing new legislation, or broadening the existing one. Examples of positive transitional events are submitting a lustration proposal to the floor of the legislature, passing or upholding lustration legislation, or overturning of a presidential veto against such legislation. Negative transitional justice events are events that attempt to constrain lustration, for example by voting down or vetoing lustration provisions by the constitutional court.[10] Figure 3 summarizes these data. Each panel follows the chronology of lustration events in a given country after transition. The horizontal axis indicates the calendar year and the vertical axis indicates the frequency of events in that year: red events are positive and blue events are negative.

Figure 3 shows that measuring lustration with an indicator variable misses the rich scope of personnel transitional justice. As shown, lustration in many countries was not merely a matter of implementation or no implementation. Rather, lustration bills were proposed, some succeeded but some others failed at some stage of the legislative process. Some bills were passed only to be vetoed by an executive, halted by an upper chamber, or challenged by a constitutional court. Personnel transitional justice was frequently amended and sometimes struck down entirely by successive governments. In this way, our data shows that relevant variation is not only between countries that implemented lustration and countries that did not, but also among countries who conducted some form of lustration.[11]

We capture the variation between negative and positive transitional events by constructing a score for the *severity* of lustration implemented in a given country $c$. This score is defined as the total number of positive transitional events divided by the total number

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[10]See the Appendix for a detailed description of the data gathering process.

[11]The figure also shows that broadening the conceptual and geographical scope of lustration is a promising avenue of research. The event collection, as described in the Appendix, used keyword searches broader than the term 'lustration,' showing a much richer scope of personnel transitional justice events. In addition, limiting the data collection to Post-Communist Europe is unwarranted: figure 3 shows that personnel transitional justice policies have been implemented in a variety of countries beyond Eastern Europe.

Figure 3: Summary of Negative and Positive Events in Personnel Transitional Justice
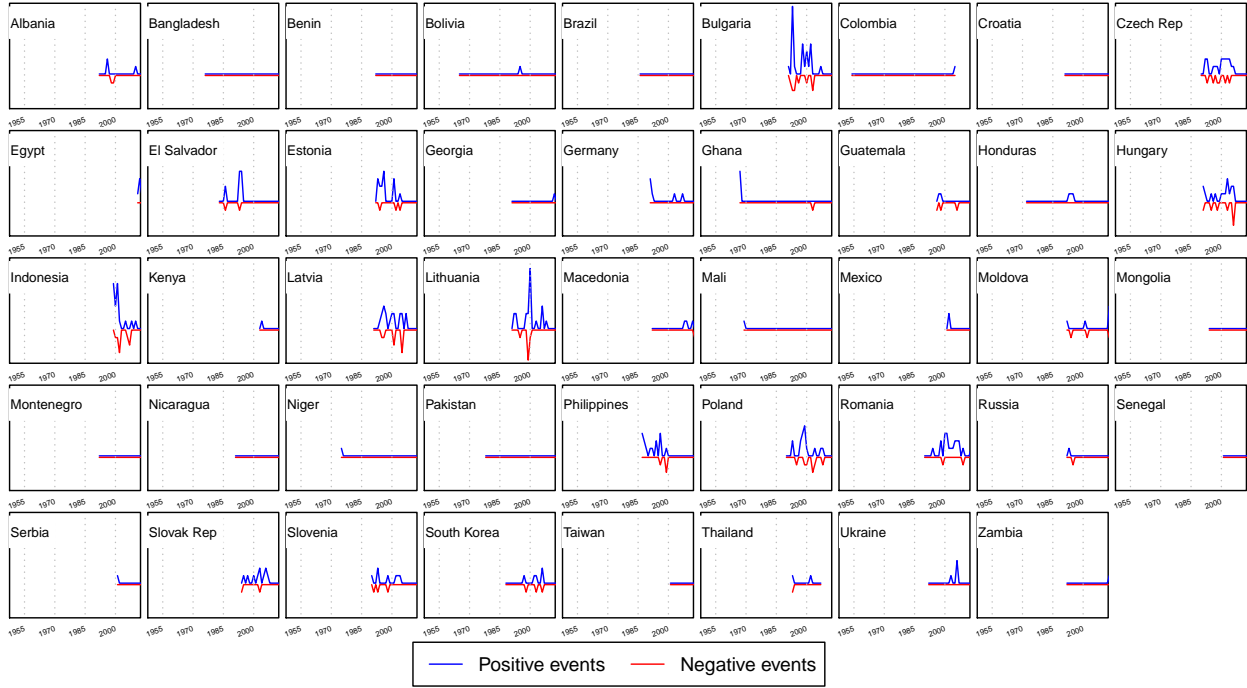


of transitional events plus one.[12] Thus, this variable takes on the value of 0 when country $c$ did not attempt any transitional event at all, and will approach 1 when the number of positive events is closer to the total number of events.[13] Note that, whereas the dependent variable, programmaticness, is measured at the party-level, $severity$ is measured at the country-level. The addition of 1 to the denominator of the measure ensures that it is well defined (in instances where a country has no transitional justice events, either positive or negative). Our first hypothesis can be stated in terms of thus defined severity as follows:

**Hypothesis 1** As the $severity$ of lustration legislation increases, all else kept constant, blackmail will be less successful and the quality of representation will increase.

A second empirical implication of the model is that as the costs of revealing the dissi-

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[12]$severity_c = \frac{Positive\_events_c}{Total\_events_c + 1}$

[13]The number of negative transitional events in country $c$ is strictly lower than the number of positive transitional events in that country. Thus, countries with at least one transitional justice will have a non-zero score.

dent's skeletons increase, the degree of programmatic representation should decrease. In the context of democracies recovering from authoritarian rule, revealing that a dissident collaborated in the past with the authoritarian regime can be damaging for his political reputation. Therefore, the costs of revealing skeletons, amount to the electoral costs of being fired (by the voters). However, voters may care to various degrees about revelations of evidence produced by the agent and their preference for a specific candidate may more or less directly translate into a candidate's chances of remaining in office. For instance, voters from younger democracies may care more about the authoritarian past compared to voters in older ones (Stan 2006, Bernhard & Kubik 2014, Wilde 1999, Cohen 1995, De Brito, Enríquez & Aguilar 2001). This claim is supported by literature on historical memory (Bernhard & Kubik 2014), but is also true by the mere fact that a larger share of voters in a young democracy will have experienced (and can remember) life under autocracy (Pop-Eleches 2007, Pop-Eleches & Tucker 2011, Pop-Eleches & Tucker 2012). In electoral systems with the personal vote (such as SMD or OLPR), these costs could be higher than in systems that do not give voters a say over which specific candidate gets elected into office (Carey & Shugart 1995). Thus the "personal vote" could serve as an alternative measure of the costs to the dissident of having embarrassing evidence revealed. A third way to operationalize this parameter would be to look at turnout in the first free elections following the transition to democracy. We offer a coarse operationalization for cost of firing: the time lapsed since the transition. We argue that, the more time has passed, voters might be less likely to care about revelations of skeletons in the closet. This is our second hypothesis:

**Hypothesis 2** As the time lapsed since the transition to democracy increases, all other things held constant, blackmail will be less effective and the degree of programmatic representation will increase.

Thirdly, a somewhat unexpected implication of our formal model is that the ideological distance between the dissident and his agent has no effect on the degree of programmatic representation. We operationalize this distance between ideal points of the dissident and the agent as the ideological distance in left-right placement according to DALP experts.

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For each non-authoritarian successor party, this distance is defined as the absolute difference in left-rght placements between a country's successor authoritarian party and each non-successor party. Our third hypothesis can be stated as:

**Hypothesis 3** Programmatic representation should be constant in the ideological distance between a country's successor authoritarian party and any non-successor parties.

## 3.1 Statistical Models

Our hypotheses can be translated to the following linear specification:

$$programaticness_{pc} = \beta_c + \beta_d * d\_party_{pc} + \beta_s * severity_c + \beta_n * n\_year_c + \beta_X \sum X_c + e_{pc} \quad (1)$$

Where $programaticness_{pc}$ is the measure of programaticness for party $p$ in country $c$, operationalized using the $cosal\_3$ score; $d\_party_{pc}$ is the ideological distance between party $p$ and the successor party in country $c$; $severity_c$ is the score of severity of lustration implemented in country $c$; and $n\_year_c$ is the number of years since transition in country $c$. The term $\sum X_c$ stands for other control variables by country $c$, and $e_{pc}$ is the error term. As implied by equation 1, the dependent variable, (programaticness), and one independent variable (ideological distance) are party-level attributes, but the remaining variables are measured at the country-level.

We estimate the model specified above using multilevel regression. In this case, estimation using ordinary least squares regression (OLS) would risk biased and inefficient results because characteristics of parties within the same country are unlikely to be independent of each other.[14] Multilevel regression can be thought of as a compromise between assuming all parties are independent from each other, and assuming all variation across parties

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[14]Traditional solutions for data with similar structures to ours (such as fixed effects or clustered standard errors) are inapplicable because some countries only have one party that is not successor authoritarian.

can be explained by the country that they belong to. A multilevel approach will structure the variation in the dependent variable according to *party*-level and *country*-level characteristics. Specifically, we will estimate the model above with a series of country-level coefficients ($\beta_c$) that will share an underlying distribution with a global mean ($\mu_{\beta_c}$) and standard deviation ($\sigma_{\beta_c}$).[15]

The results of our estimations are shown in table 1. Note that all variables have been linearly transformed to range between 0 and 1, for ease of interpretation. All models presented here include country-level intercepts (estimations for $\beta_c$), which are omitted from the table out of space considerations. The table shows the average effect of the variables of interest in a manner similar to OLS regression: we estimate the average change in the degree of programaticness of a non-successor party that is associated with an increase of one unit in the covariate of interest.

The resulting estimations are consistent with the implications of our theoretical model. Most importantly for our argument, they show that higher scores of *severity* of lustration are associated, on average, with more programatic parties even after controlling for country-specific effects. The effect is statistically significant at the 0.05% confidence level in all specifications, and its average effect is quite sizeable. The difference between a country that did not attempt have any lustration at all (*severity* score of 0) and a country with the highest lustration *severity* score (0.9) is on average, 0.22 points. Since programaticness ranges from 0 to 1, this effect is not minor.

The estimated coefficients for the ideological distance between the dissident and the successor party (*d_party*) are not statistically distinguishable from zero. This lack of effect is consistent with the somewhat surprising finding from the theoretical model that the level of programmaticness does not depend on the distance between the ideal points of

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[15]Assuming all parties are independent of each other would mean estimating a fully pooled model with one global intercept. Assuming total dependency on country characteristics would mean a traditional fixed-effects approach that would estimate one intercept per country (minus a base category) . The advantage of our multilevel model is that when there is very little observed variation at the country-level, the coefficient can be estimated to be close to the mean $\mu_{\beta_c}$.

Table 1: Explaining programaticness (country intercepts not shown)

| | Model 1 | Model 2 | Model 3 | Model 4 | Model 5 |
|---|---|---|---|---|---|
| *d_party* | −0.045 | −0.044 | −0.045 | −0.050 | −0.050 |
| | (0.043) | (0.043) | (0.043) | (0.043) | (0.043) |
| | | | | | |
| *severity* | | 0.252*** | 0.252*** | 0.209** | 0.222** |
| | | (0.090) | (0.089) | (0.089) | (0.091) |
| | | | | | |
| *n_year* | | | −0.210* | −0.201* | −0.160 |
| | | | (0.125) | (0.120) | (0.131) |
| | | | | | |
| *press_freedom* | | | | 0.453** | 0.405 |
| | | | | (0.230) | (0.248) |
| | | | | | |
| *opp_status* | | | | | 0.036 |
| | | | | | (0.054) |
| | | | | | |
| *no_succ* | | | | | −0.041 |
| | | | | | (0.064) |
| | | | | | |
| Constant | 0.363*** | 0.236*** | 0.350*** | 0.013 | 0.015 |
| | (0.031) | (0.054) | (0.086) | (0.190) | (0.211) |
| | | | | | |
| Observations | 201 | 201 | 201 | 201 | 201 |
| Log Likelihood | 114.694 | 116.833 | 117.076 | 118.421 | 114.975 |
| Akaike Inf. Crit. | −221.389 | −223.666 | −222.153 | −222.843 | −211.950 |
| Bayesian Inf. Crit. | −208.176 | −207.150 | −202.333 | −199.720 | −182.220 |

*Note:* $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01

the successor authoritarian party and the non-successor parties. Although we are conservative in the interpretation of this null finding, it is important to point out that the lack of statistical significance is robust across all models.

Of course, it could be that this null finding is driven by preexisting closeness between the opposition and the authoritarian parties. That is, it could be that parties that existed during the authoritarian period are, at the same time, ideologically closer to the authoritarian parties and more likely to be blackmailed. We control for this possible confounder using a measure of how active the opposition was during the authoritarian era. The data provided by Cheibub, Gandhi & Vreeland (2010) report whether the authoritarian regime had no legislature, a single party- or multiparty-legislature. In our sample, only two authoritarian regimes had no legislature at all, so we turned the variable $opp\_status$ into an indicator taking the value of 1 is there were multiple parties represented in the legislature, and 0 otherwise. As can be seen in Model 5, the inclusion of this variable does not change our results.

Models 3 and 4 include the variable $n\_year$, which captures time since transition to democracy as a proxy for the cost of firing ($F$). As can be seen, the coefficient is negatively associated with the programmaticness of a party, which supports the idea that the more time has passed since transition, the less voters care about alleged collaborators rendering blackmail ineffective. However, the coefficient is not statistically significant in either model, something that we attribute to the course measure used in our analysis.

Finally, models 4 and 5 include the variable $press\_freedom$, which is our way of operationalizing parameter $c$, the cost of bluffing. Recall that our model predicts that an increase in the costs of bluffing to the agent does not increase the degree of programmatic representation, but that all things held constant, it increases the chances of an equilibrium where transitional justice does not work as it ought to. Hence, there is no direct effect of the cost of bluffing on programmatic representation, though there is an effect on whether politicians are bluffed or blackmailed. We include press freedom, measured with the rat-

ing provided by Reporters Without Borders (Press Freedom)[16] as a way to evaluate the performance of the remaining variables controlling for "costs of bluffing." If our theoretical model is correct, we would not expect the inclusion of this variable to change our findings.

The intuition behind operationalizing $c$ with press freedom is as follows: When an agent tries to bluff the dissident into thinking that the agent is in possession of evidence against the politician, such a bluff can be easily exposed in an environment with free media. Indeed, a wide literature discusses how journalists are on par with state-sponsored archives (Laplante & Phenicie 2009, Pinto 2010, Chapman 2009) when it comes to revealing evidence of former secret police conduct. Although such revelation is not subject to the same level of scrutiny as state sponsored legislation, a high degree of media independence would lead to the bluff being called, whereas the media's failure to corroborate the claim that the dissident in question was a former collaborator would weaken the agent's bluff. Models 4 and 5 incorporate this variable and show that, although this variable is positively related to the degree of programaticness of a political party, it does not change the estimated effects of the rest of the variables. Note, however, that the constant term is no longer statistically significant when $press_freedom$ is included. We interpret this as a type of omitted variable bias: in models 1, 2, and 3 the average effect of a free press is encapsulated by this constant.

Finally, model 5 addresses the fact that not all countries have a successor authoritarian party. To avoid losing these cases, we substituted missing cases with the average distance between successor authoritarian parties and the rest of the parties. Model 5 includes an indicator variable ($no\_succ$) that takes the value of 1 if there is no missing successor, which allows us to verify if the estimate of the coefficient for $d\_party$ is biased because of our handling of missing data. The coefficient for this indicator is not statistically significant

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[16]To avoid confusion, the variable was linearly transformed so that lower rankings mean lower freedom of the press in a given country.

nor does it change the point estimates for the remaining variables.[17]

Figure 4 provides a visual representation of the main relationship of interest in this paper: the one between programaticness and *severity* of lustration. The figure shows the *severity* score on the horizontal axis, and the average country-effects estimated in model 1 on the vertical axis. Model 1 included only the regressor that was measured at the party level: ideological distance, and estimated a series of country-specific intercepts that capture the average country-level programaticness. Figure 4 suggests a positive relationship between these two variables. Furthermore, this pattern seems to be much clearer for the observations that fall in the right half of the panel: the countries that implemented more severe lustration policies. This latter point supports our decision to measure transitional justice as a process, rather than as a dichotomous measure of implementation or failure to implement lustration law.

The evidence presented here does not support the existence of a relationship between the ideological distance between dissident and successor parties, and party programmaticness. Although consistent with the model, this is counterintuitive, as one would expect parties closer to the authoritarian successor to give into blackmail more willingly than parties that are ideologically distant from the successor autocrats. Yet we find no evidence of this intuition, as the coefficient of $d\_party$ is not statistically significant.

Secondly, the evidence shows that the severity of transitional justice is positively related to the degree of programaticness. This result is promising insofar as it suggests that lustration policies produce, on average, more programmatic parties after transition. However, the theoretical model also shows that lustration by itself does not prevent blackmail of former dissidents turned politicians. In order for the proper functioning of lustration policies, this must come accompanied by a free press that will prevent bluffing.

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[17]See the Appendix for alternative treatment of missing values of successor parties.

Figure 4: Country intercepts (Model 1) and country-level variables
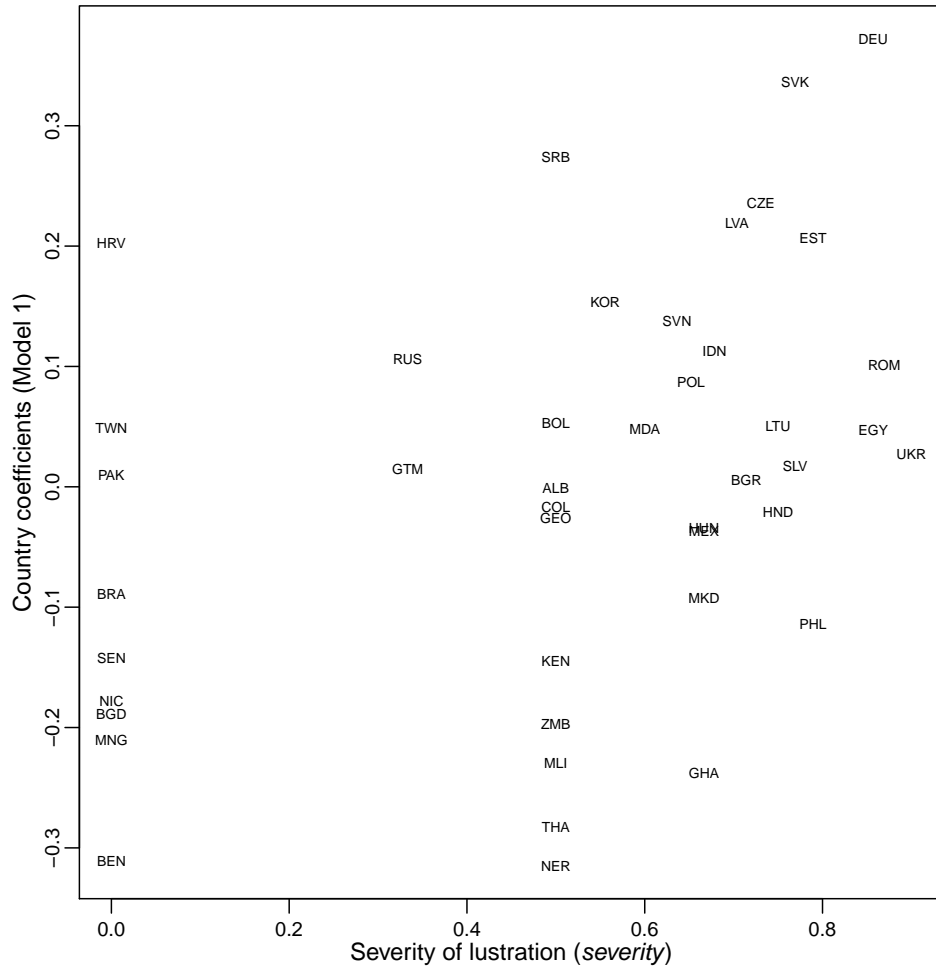


# 4 Conclusion

In this paper we investigated how lustration, understood as the policies that reveal information about prior collaboration with secret authoritarian elites, influence the quality representation. We used formal theory to model the situation in which agents of the former authoritarian regime use secret police files to blackmail democratic elites (dissidents). Such blackmail, in our model, is aimed at forcing the democratic elites to implement policies that are amenable to the former authoritarian elites, and not to the voters. Theoretically, we find that former authoritarian elites' ability to shape policy is higher when transitional justice is legislated less frequently and that their influence decreases with how much

the voters care for skeletons of the authoritarian past in their politicians' closets. Also, to our surprise and in contrast to what most of the literature has been positing, the ideological distance between the former autocrats and the former dissidents has no bearing on the ability of former autocrats to extract concessions via blackmail. Thus, the polarized policy recommendations found in the transitional justice literature recommending dealing with former autocrats either by banishing them from politics entirely or by integrating them into the political class are both theoretically and empirically unfounded.

This paper contributes to an understanding of the relationship between transitional justice, rule of law and the quality of representation in new democracies. Broadly, it does so by linking transitional justice to the secret authoritarian legacies. Specifically, it finds that lustration and personnel transitional justice policies more broadly can prevent blackmail of current politicians by former authoritarian elites, by decreasing the potentially embarrassing information that remains hidden and, thus,is prone to be used for blackmail. We test this implication empirically using an original dataset of all personnel transitional justice events in former authoritarian party-based regimes, and our empirical analysis confirmed this first theoretical point.

Furthermore, our paper shows that lustration does not prevent blackmail always: we also find that holding all else constant, lack of free press can sabotage transitional justice efforts. Allegations of collaboration need to be investigated and verified: in a liberal democracy this task is often performed by a free press. In the absence of independent media blackmail occurs through bluffing: threatening to accuse current politicians of having ties with the former authoritarian regime even when no actual evidence exists.

The implications from our theory allows us to reinterpret some dominant political narratives about the effects of delayed transitional justice on the quality of representation. In the past, scholars have criticized countries such as Poland and Hungary for being late comers to transitional justice. The initially implemented transitional justice policies in these two countries were extremely mild leading many transitional justice experts to believe

that "living well was the best revenge." However, both countries started off the democratization period with very little lustration activity, but have gradually acquired quite a substantial body of transitional justice legislation. Today, their party systems stand out as highly programmatic, particularly in Poland's case. In light of our paper, transitional justice policies implemented years after the transition might enable equilibria that lead to better representation than policies implemented right after transition. This is so because in the immediate aftermath of transition the costs to politicians' having skeletons revealed is the highest. In other words, right after the transition, agents of authoritarian regimes have the highest leverage to blackmail through bluffing. Paradoxically, then, transitional justice works better for democratic representation the less the public cares for skeletons in the closet because this lack of attention makes bluffing less frequent.

These contributions speak to the effects of transitional justice processes on the long-term quality of representation in new democracies, an area that has sparked relatively little scholarly attention. Few have examined the ability of former authoritarian elites to leverage the secret information assembled under their tenure to have enduring impact on the policies of new democracies (Darden 2001). Our paper shows that lustration matters for representation. Thus, from a normative point of view, shaming politicians who had a murky past might be desirable. Yet this paper suggests that there might be a trade-off between shaming politicians for collaborating with authoritarian regimes, and implementing a transitional justice policy that works as it should.

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