

# Personalist Property: Autocratic Leader Turnover and Property Rights

October 2024

## Abstract

When do property rights in modern autocracies survive beyond a leader's term in office? This paper proposes that in personalist autocracies, the turnover of the leader increases expropriation risks. Non-personalist autocracies have institutions among the ruling elite which stabilize successions. In personalist regimes, successors do not inherit constraints from the ruling group, allowing them to make radical changes to the distribution of property rights. Successors to personalist regimes may also expropriate resources to compensate supporters during transition crises. To isolate the causal impact of turnover, we use plausibly exogenous changes in leadership caused by leaders' terminal illnesses and accidents. In a panel analysis of expropriation events, we find that the death or incapacitation of a personalist autocrat increases the probability of expropriation by 10.8% to 15.6% in the two years following the leader's departure, with no significant effects observed in non-personalist regimes. Our results are robust to controlling for various factors such as democracy, regime type, ideology, and natural resource rents. These findings point to the benefits of executive constraints, even in autocracies.

*"We are surrounded by one-bullet regimes, and when the regime changes, it doesn't change a little; it can change 180 degrees. There is one exception — the Saudis. They have all those princes, and one can replace another without dramatic changes. They have this principle of the 'shura' — the council of royals who make deliberative, collective decisions. So the king may change but policy remains consistent."* - Anonymous Israeli MFA Official

## 1 Introduction

In the 2000s, Dutch firm Fondell began a partnership to refurbish a privatized aluminum refinery in Azerbaijan. To secure their assets, they built supply and ownership relations with politically connected domestic firms. The venture was jointly owned by the owner of Azerbaijan's largest oil company and the brother of the minister of economic development. Recent work on investor protections suggests that supply chain links to politically connected domestic firms should protect investments [Johns and Wellhausen, 2016].

However, Fondell's political connections did not survive long after Azerbaijani autocrat Heydar Aliyev died. Its Azerbaijani patrons quarreled with Aliyev's son and were purged from power. The same month of the purge, Fondell was frozen out of its assets in Azerbaijan, leading Fondell to sue for damages. This episode illuminates an understudied question in political economy: under what circumstances does leader turnover in autocracies compromise existing property rights?

We theorize that the effect of leader turnover on property rights depends on the extent of elite-level constraints on the leader. Autocracies vary widely in the extent to which senior supporters can operate autonomously and control the leaders. The most popular current autocracy regime typology characterizes regimes by the ruling groups' primary coordinating and constraining institutions, such as the monarchy, military, and party. Regimes with no strong constraining institution are called personalist to reflect the leader's greater power. Non-personalist have institutional mechanisms that allow supporters to coordinate with each other and credibly threaten to punish a leader who purges them or violates their collective will, such as a royal court, politburo, or military junta.

Our primary variable of interest is the strength of the ruling group's constraining institutions immediately prior to the leader's death or severe illness. We focus on pre-succession observables because they are policy-relevant<sup>1</sup> and to prevent post-treatment bias. Past studies have found that turnover in personalist regimes is associated with greater regime instability [Kendall-Taylor and Frantz, 2016] and that personalist regimes struggle to ensure continuity over successions.

The lack of constraining institutions gives a successor greater freedom to make drastic changes in economic policy. It is impossible to ensure that a successor will have preferences identical to his or her predecessor. In a constrained autocracy, policy results from a collective process in which key supporters exert influence or even a veto [Castaneda, 2001]. These veto players should dampen the effect of a shock to leader preferences. New leaders may be pro-autarky, desire to shift investment relationships to a different foreign power, or may simply grow impatient with an underperforming investor [Afrigue, 2014]. The 2005 Fondell case in Azerbaijan, described above, provides support for this theory.

Furthermore, leaders who do not inherit constraining institutions may employ expropriation to motivate their supporters during a chaotic interregnum. Autocrats are particularly vulnerable during their first years in office [Geddes et al., 2018]. Constraints help autocrats during crises because they allow the leader to make credible promises to share rents with supporters in the future [Meng, 2020]. Without constraints, supporters may not believe a leader's promise to retain them in the future [Meng, 2020]. For a non-credible autocrat, expropriation can provide fast cash and assets to reward supporters.<sup>2</sup> In 1997 in Congo (Kinshasa), Mobutu Sese Seko's regime collapsed during his treatment for terminal cancer. Former rebel Laurent Kabila filled the power vacuum but failed to build credibility with his supporters. He repeatedly violated investors' rights in order to supply his army and patronage network. We do not argue that this strategy is optimal because expropriating

---

<sup>1</sup>It would not be very helpful for investors to say "If the dictator dies AND there is a civil war, that could adversely affect your assets."

<sup>2</sup>This model is substantively similar to the credible commitment model in Meng [2020] based on Fearon [1995]. Meng proposes that when successors do not inherit constraints, there is a commitment problem. During their initial years in power, a new leader depends heavily on a broad coalition of supporters while facing severe financial constraints. In later periods, when the leader accumulates resources but requires less political support, promises of future compensation through permanent positions become less credible. Recognizing this time-inconsistency problem, supporters anticipate they won't be rewarded and choose to challenge the leader immediately. Meng argues that institutional constraints can resolve this commitment problem by shifting power to the early period. Similarly, expropriation serves as a mechanism to front-load payments to supporters, transferring resources from the future to the present and thereby averting early challenges to the regime.

has sharply diminishing returns (an unpaid soldier assassinated Kabila three years into his reign). Nonetheless, this strategy is attested by statements from Kabila’s financial advisors.

We test our theory on a panel dataset covering all major foreign direct investment (FDI) expropriations from 1950 to 2010. Using foreign direct investment as an outcome variable allows for better comparison across a diverse array of countries due to availability of cross-national data with good coverage [Kobrin, 1984, Minor, 1994, Hajzler, 2012] and because FDI is prevalent in both low-income and middle-income countries. We use a country-year measure from Geddes et al. [2017] to measure personalism. We measure expropriation using a binary event dataset collected by Kobrin [1984], Minor [1994], Hajzler [2012], and Tomz and Wright [2008]. Expropriation is defined as the government action of forcibly divesting the foreign asset owner of their assets, and we exclude minor changes in policy against investors’ interests (sometimes called creeping expropriations) as they are difficult to define [Kobrin, 1984] and more often relate to regulation in high-rule-of-law democracies [Pelc, 2017].

To prevent endogeneity between sources of turnover and property rights violations, we exploit plausibly random variations in turnover from terminal illnesses<sup>3</sup> and accidents. We include leaders who die in office from illness or accident, or who retire due to severe illness and die within two years<sup>4</sup>. Our data includes 86 exogenous leader turnovers from 1950 to 2010.

We find that expropriation increases during leadership transitions following the death or retirement of a leader in a personalist regime. Using a fixed-effects model, we estimate that the likelihood of expropriation increases by 10.8%-15.6% during the two years following a leader’s departure. We find null effects in non-personalist autocracies and confirm the difference with interaction terms.

This research contributes a practical analysis by providing a large-n analysis of rare political events with heterogeneous effect sizes. It also demonstrates the predictive value of theories of authoritarian politics for policy questions and investment decisions. By using pre-succession factors to determine expropriation likelihood, our model uses observables used by investors while making investment decisions<sup>5</sup>. This methodology improves upon existing empirical approaches that rely on observing changes after succession—information that emerges too late to guide investment decisions. We do include post-treatment controls like regime change as a robustness check and obtain similar results.

Our paper contributes to a growing literature on the differential behavior of personalist and non-personalist autocracies. Personalist regimes grant more monopolies to foreign investors, resulting in FDI being concentrated in the primary sector [Wright and Zhu, 2018]. Legislatures increase property rights and growth in non-personalist autocracies, but not in personalist regimes [Wilson and Wright, 2017]. Jones and Olken [2005] observe a larger effect of leader death on growth in unconstrained autocracies. Coups in personalist regimes are more likely to use violence [Chin et al., 2020] [Grundholm, 2020]. When personalist regimes collapse, they are less likely to become stable democracies [Geddes et al., 2018], but this effect is weaker when personalists create support parties [Frantz and Kendall-Taylor, 2017].

---

<sup>3</sup>There is no reason to believe that cancer or strokes are correlated with plausible determinants of political risk like commodity shocks. Following Jones and Olken [2005], this ensures that “the timing of the transfer from one leader to the next was essentially random” relative to changes in economic and political variables.

<sup>4</sup>The decision to seek end-of-life care abroad is endogenous through expectations about post-death instability. We provide multiple specifications in the appendix with similar results.

<sup>5</sup>This is also the reason why we exclude leader fixed effects, as successor attributes are observed too late.

The past work that comes closest to our study is that of [Fails \[2014\]](#). Fails (2014) constructs a metric of autocrats’ “replacement risk” using the annual number of past turnovers in the country. Unlike [Fails \[2014\]](#), we explain heterogeneity in the years when expropriations occur rather than providing stable risk estimations. Moreover, by using exogenous turnovers we reduce the endogeneity problem arising from turnovers being correlated over time. Our work is also similar to [Albertus and Menaldo \[2012b\]](#) who finds that new autocrats are more likely to expropriate land held by private citizens and that doing so increases their tenure.

The paper proceeds as follows. Section 2 defines personalism and enumerates our theory. Section 1.1 describes recent work on our research topic. Section 3 describes the methodology and data sources. We provide empirical results in Section 4, with further robustness checks in Section 5. We present case studies from Azerbaijan and Congo (Kinshasa) in Section 6.

## 1.1 Literature Review

This paper contributes to a growing body of literature on how autocratic constraints impact property rights. Gehlbach and Keefer propose that investor protections in autocracies are dependent on constraints within the ruling group, unlike the democratic literature on protections that focuses on the citizen-to-ruler constraints [[Gehlbach and Keefer, 2011, 2012](#)]. They emphasize institutions that “allow ruling group members to act collectively against the ruler” (pp. 621) facilitate investment in autocracies. In [Gehlbach and Keefer \[2011\]](#), they present a formal model in which a subset of investors are ruling group members who can coordinate against violations from the ruler and show that allowing members to coordinate increases investment. [Gehlbach and Keefer \[2012\]](#) find that investment is positively associated with intra-elite constraints.

[Knutsen and Fjelde \[2013\]](#) find that monarchies, characterized by unusually stable regimes with longer time horizons, have much better investor protections and property rights as rated by the International Country Risk Guide, scoring similarly with democracies and well above monarchic and military regimes. [Jensen et al. \[2014\]](#) find that legislatures improve investor protections in non-personalist autocracies but not in personalist regimes where the leader has amassed enough power to sideline the legislature. [Wright and Zhu \[2018\]](#) find that personalist autocracies attract more fixed-asset investment, particularly in the primary production sector, despite the higher risk of expropriation and obsolescence. This sector have higher entry barriers, allowing foreign investors to extract monopoly rents more easily. Weaker vertical constraints in personalist autocracies may allow them to ignore the social costs of monopolies while charging investors a premium [[Wright and Zhu, 2018](#)].

The ability of institutionalized autocracies to attract greater investment could be due to expectations that current property rights will outlive the leader. This possibility has not yet been investigated ([Gehlbach and Keefer \[2012\]](#) do not investigate this possibility and instead try to control for expectations about future political instability). Successions in personalist regimes are associated with higher instability [[Kendall-Taylor and Frantz, 2016](#)]. In non-personalist regimes, natural leader deaths rarely produce severe political crises because the same institutions that constrain leaders can organize their smooth replacement [[Geddes et al., 2018, Kendall-Taylor and Frantz, 2016](#)]. Support-

ers understand that internal fighting over the top job would signal vulnerability and invite attacks from excluded groups. Although they may have preferences over candidates, their preference for staying in power is much greater. Thus, they avoid prolonged internal debates and prefer to quickly coalesce around a new successor and circle the wagons [Kendall-Taylor and Frantz, 2016]. Single-party and monarchic regimes survive leader deaths in 96% and 95% of cases, respectively. In the highly constrained Partido Revolucionaria Institucional regime in Mexico, where presidents were forced via term limit, presidential successors needed the approval of all major factions within the regime [Castaneda, 2001]. Personalist regimes only survive leader deaths in 78% of cases. Brossier [2024] argues that personalist leaders' relative lack of enforceable ruling institutions prevents them from bequeathing their positions to descendants, partly because threats have no power after death.

## 2 Theory: Personalist Successions and Property Rights

This section discusses the role of leader-constraining institutions in autocracies and personalist rule. We argue that successors from personalist regimes do not inherit viable constraining institutions. Without constraints, successors are free to implement bolder economic reforms. Moreover, a lack of constraints prevents leaders from promising future rewards to their supporters. Unconstrained successors may resort to unsustainable revenue generation strategies over shorter time horizons. In the following section on expropriation, We argue that FDI expropriation should be associated with both greater economic policy change and shorter time horizons.

Autocratic rule is based on a repeated exchange of services between a leader and a support group. The ruling group is a collection of specialized elites who contribute repressive capacity, financing, bureaucratic services, and political loyalty, among others [Svolik, 2012];[De Mesquita et al., 2005]. Members of the support group, a smaller segment of the population compared to those in democracies, receive access to rents, privileges, and policy influence in return.

Ruling group members desire to survive in office. Autocratic governments tend to have a smaller selectorate and winning coalition than democracies, thereby enlarging the perks and influence accruing to elite supporters. Members also compete with one another for influence, yet they share a collective interest in restraining the leader from exploiting them [Myerson, 2015]. Qualitative work on authoritarian successions suggests that survival in office is supporters' primary goal during potentially dangerous interregna [Kendall-Taylor and Frantz, 2016].

Autocrats often wish to remove or replace some of their supporters [Svolik, 2012]. If supporters cannot be removed, the leader may struggle to punish corruption or motivate effort [Myerson, 2015]. The leader may have policy disagreements with the supporters and want to bring in ideologically aligned agents. Any new leader has a particularly strong reason to change supporters because their policy preferences and identities will differ slightly from the predecessor who chose the incumbent supporters.

A common solution to the supporters' dilemma is to establish leader-constraining institutions within the ruling group [Meng, 2020, Svolik, 2012], enabling them to remove a predatory leader. The form of constraining institutions varies widely, from a political party to a military junta to a royal court.

However, they share two key attributes. First, they must establish norms for when supporters can be removed, define how supporters are rewarded, and meet regularly to address supporters' interests. Second, they must have a credible threat to harm or remove a predating leader, often through connections with armed groups. For example, in Saudi Arabia from 1982 to 2005, the “Sudairi Seven” brothers constrained their foremost by distributing military and diplomatic power among themselves, with each of the defense, national guard, and intelligence forces held by a different brother and his descendants. Supporters who impose constraining institutions on leaders do survive in office for longer [Kroeger, 2020], measured by party strength and cabinet tenure.

Regimes that lack leader-constraining institutions are called personalist. Personalization occurs when the leader disrupts constraining institutions [Svolik, 2012, Geddes et al., 2018] through strategies such as purging supporters and replacing them with cronies, preventing supporters from communicating with each other or the military, or creating their own separate security forces to prevent a coup. When the probability of a supporter rebellion succeeding becomes sufficiently low, threats from supporters lose credibility, allowing the leader's power to dismiss or mistreat them to become extreme—illustrated by Stalin's and Saddam Hussein's treatment of their supporters.

Our key theoretical variable is whether the regime before the succession was personalist. In other words, does *the new leader inherit effective constraining institutions*, or does he not? We do not try to measure the strength of leader-constraining institutions immediately after a succession for several reasons. Firstly, that research question is not relevant to policy because investors cannot withdraw assets at that stage. Secondly, post-succession events are post-treatment and cannot be controlled for without biasing the estimates (although we do provide robustness checks with controls for regime change and civil war in Section 5.4). Thirdly, we are less confident that measures of regime attributes accurately capture the strategic situation the year after a succession.

Leaders who do not inherit constraints can implement more radical shifts in economic policy. A new leader is likely to differ in preferences from their predecessor more than the one leader  $t_2$  differs from herself in year  $t_1$ . Leadership turnover is a shock to the head of state's preferences over the economic structure and who should be permitted or allowed to own significant property. However, leaders who do not inherit constraints are uniquely positioned to implement larger policy changes. Well-coordinated ruling groups can use the threat of overthrow to keep leaders from deviating from their preferences [Svolik, 2012]. Consistent with these assumptions, past work has found that leader transitions in autocracies are associated with shocks to economic growth, monetary policy [Jones and Olken, 2005], foreign policy alignment [Li and Zha, 2024], and international trade policy [Gray and Kucik, 2017].

This mechanism assumes that successors will vary in their preferences from their predecessors and that new leaders who do not inherit constraining institutions are positioned to enact their preferences. It does not assume that successors from personalist regimes differ in preferences from their predecessors more than in institutionalized autocracies (but see [Kendall-Taylor and Frantz, 2016]). Successors in personalist regimes are more often drawn from outside the previous ruling group [Geddes et al., 2018]. It has been proposed that appointing a hereditary successor reduces elite fragmentation over successions [Brownlee, 2007]. However, Brossier [2024] finds that only 10% of personalist successors in Africa are relatives, as these regimes lack the institutional structures

|                      | Constrained leader (non-personalist) | Unconstrained leader (personalist) |
|----------------------|--------------------------------------|------------------------------------|
| Leader has not died  | Stable Property Rights               | Stable Property Rights             |
| Leader died recently | Stable Property Rights               | Unstable Property Rights           |

Figure 1: This diagram shows our key empirical predictions. In autocracies with leader-constraining institutions, the leader’s death should not affect investor protections. In autocracies with leader-constraining institutions, leader deaths should increase expropriation.

needed to build elite consensus around a hereditary successor.

New leaders will likely also differ in preferences over supporters [De Mesquita et al., 2005]. Because leader-constraining institutions are intended to protect supporters’ position from the leader [Geddes et al., 2013], we expect leaders who inherit no constraints will be more successful in changing their support base. Because interest groups vary in preferences over economic policy, we expect larger shifts in succession from personalism. In particular, the new support group may prefer to transfer assets from purged elites to themselves. Foreign multinationals often own ventures jointly with domestic political actors or rely on them for political protection. If ownership of a joint venture is reassigned, the foreign owners can become “collateral damage” to the domestic reshuffling, as occurred in the Azerbaijan case below.

The second mechanism is that expropriation may provide payments for political loyalty, solving a credible commitment problem during transitions. Autocrats tend to be more vulnerable to their supporters at the start of their tenure [De Mesquita et al., 2005, p. 100] [Albertus and Menaldo, 2012a] Meng [2020] [Kokkonen and Sundell, 2020] [Timoneda et al., 2024]. A new leader has not learned to use his coercive apparatus [Geddes et al., 2018]. Civil wars occur more frequently during autocrats’ initial years [Kokkonen and Sundell, 2020]. Coups are more likely to occur and succeed in a leaders first years Geddes et al. [2018].

A new autocrat must convince supporters to retain him and expend effort establishing his rule. A stable regime will produce enormous rents, but most of that revenue will come later after the crisis has passed [Meng, 2020]. With constraining institutions in place, the leader can credibly promise to share rents and influence with supporters in the future, once the initial succession crises have been resolved [Meng, 2020]. For that reason, new autocrats often create power-sharing institutions to motivate supporters during their first vulnerable years [Meng, 2020][Albertus and Menaldo, 2012a].

Alternatively, a leader may motivate supporters by expropriating assets to distribute as patronage. Whereas seizing and reallocating assets may harm future revenue by reducing investment, it can offer immediate access to cash or assets for distribution. In Section 6, we provide a case study from Laurent Kabila’s regime in Congo (Kinshasa) consistent with this mechanism.

We do not argue that this strategy is superior to power sharing. Raising revenue via expropriation is inefficient and temporary. The value of any investment depends on the credibility of the host government, which is damaged by the expropriation. Laurent Kabila was only temporarily able to raise funds. It is possible that Laurent Kabila adopted expropriation as a “second best” option because his reputation for assassinating allies prevented institutionalization.



### 3 Research Design

The previous sections proposed a theory that leadership successions cause higher expropriation in personalist autocracies than in other autocracy types. This section first proposes a research design that uses illnesses and accidents for exogenous variation in leadership successions.

Leader changes in autocracies do not occur randomly. Coups are the most common form of leader removal, triggered by purges, poor economic performance, or changes in the power of social groups. The effect of economic performance on expropriation is well-documented [Jensen et al., 2020]. Civil wars may induce a coup or overthrow and simultaneously compromise the state’s ability or willingness to protect property.

To avoid endogeneity, the study focuses on turnover due to accidents and natural illnesses. This strategy was first used by Jones and Olken [2005] to assess leader effects on growth and monetary policy. The methodology sidesteps the leader-strengthening effects of coups. Coming to power in a coup is a strong signal of support for a new leader, resulting in less frequent coups and more frequent purges in the early years of a leader’s tenure. Leaders that inherit via disease or accident of the previous leader (or predecessor) have no honeymoon period to bias the results. However, the methodology only observes variation in regimes where debilitating illnesses occur, and young, healthy leaders are out of the sample.

The effect of leader turnover on expropriation is identified in both personalist and non-personalist predecessor regimes, but this is insufficient to show that the heterogeneity in effect is caused by personalism. Endogeneity is possible if a third variable, such as natural resource rents, causes both personalism and vulnerability to turnover. For example, suppose that natural resource rents increase personalism in autocracies (there is some evidence of this [Fails, 2020]). Suppose that natural resource rents also increase the effect of leader turnover on takeover, but not through personalism. This would give us a false positive. We add controls for several possible confounders.

We show that natural resources do not drive our results by controlling for both rents and the interaction between rents and turnover. A linear fixed effects model is used to control for interactions of death with Polity Score, natural resource rents, and regime type, showing that the predecessor personalism effect is robust.

#### 3.1 Dependent Variable

Expropriation is a form of political risk in which a host country seizes a company’s assets and does not provide fair compensation. Expropriations may come through nationalizations, breaches of contract that cause the firm to cease operations, the state declining to protect the asset from seizure, or the forced sale of property [Esberg and Perlman, 2020]. Following Kobrin [1984], we define expropriation as the forced divestment of equity ownership of a foreign direct investor. The investment must entail international managerial control through equity ownership. While many national expropriations enter the dataset through minority foreign shareholders, the majority of national expropriations are not included. We include both formal expropriations (nationalizations) and covert expropriation through private actors, forced sales, and contract re-negotiations [Esberg and Perlman,



2020].

Our dependent variable is a data set of FDI expropriation events from 1950-2010. It was first compiled by [Kobrin \[1984\]](#), then updated by [Minor \[1994\]](#) and [Hajzler \[2012\]](#). We added several events identified by [Tomz and Wright \[2008\]](#) and two missing expropriation events in China and Cuba.

We focus on FDI expropriation for several reasons. Most importantly, it can be measured across countries more reliably than violations of domestic property rights. Cross-national datasets of domestic property rights are more recent and often include measures of corruption perceptions or stability that could be mis-measured during succession crises [[Beach and Miles, 2006](#)]. Foreign investors are also more likely to report an expropriation because they are less vulnerable to retaliation, face reporting requirements in their home jurisdiction, and have access to international investor-state arbitration. We did not use the popular International Country Risk Guide's (ICRG) scores for investor protections due to a lack of data. Exogenous turnovers occur only 86 times in the total sample but only 29 times in years with ICRG data<sup>6</sup>. Moreover, the ICRG scores are risk predictions as against our outcome of interest, which is an actual expropriation event. It is for this reason that we do not run our analysis on this outcome measure.

Unfortunately, we lack data on the value of the assets or the number of companies in the vast majority of events. In many cases there are no valuations recorded and when valuations are recorded the investors and host state tend to disagree on the asset value. Therefore, we use a simple binary outcome variable of 1 for any expropriation event, and 0 otherwise. To correct for country-years with no FDI we use both the FDI indicator from [Tomz and Wright \[2008\]](#) and an annual measure of FDI stocks from UNCTAD for years after 1970.

In the 1970's a wave of nationalizations in newly independent states occurred, followed by a collapse in expropriations in the 1980s and 90s caused by the exhaustion of seizable capital, low commodity prices, and desire to attract FDI [[Kobrin, 1984](#)] [[Minor, 1994](#)]. Since the 2000s, expropriation levels have risen due to increased FDI in developing countries [[Hajzler, 2012](#)], especially in the primary resources sector. These trends are represented graphically in Figure 2.

Despite "creeping expropriations", defined as transfer risk in which states gradually alter regulations to capture foreign investments, becoming more common since the 2000s [[Graham et al., 2018](#)], we do not include them in our analysis. Since leader deaths in office are rare events, datasets on creeping expropriation do not cover enough country-years to be assessed through our design. Regulatory changes are included only if they were severe enough to drive out foreign firms [[Hajzler, 2012](#)].

Our dependent variable may exclude smaller expropriations, especially where investors did not report or take legal action. It is more comprehensive on cases with international legal action. In other words, the dataset mainly concerns large expropriations where the state was eventually caught. Because these expropriations create the highest long-term reputational costs, they should be more affected by shocks to regime time horizons.

---

<sup>6</sup>For example, the ICRG scores exclude Turkmenistan during our entire sample period.

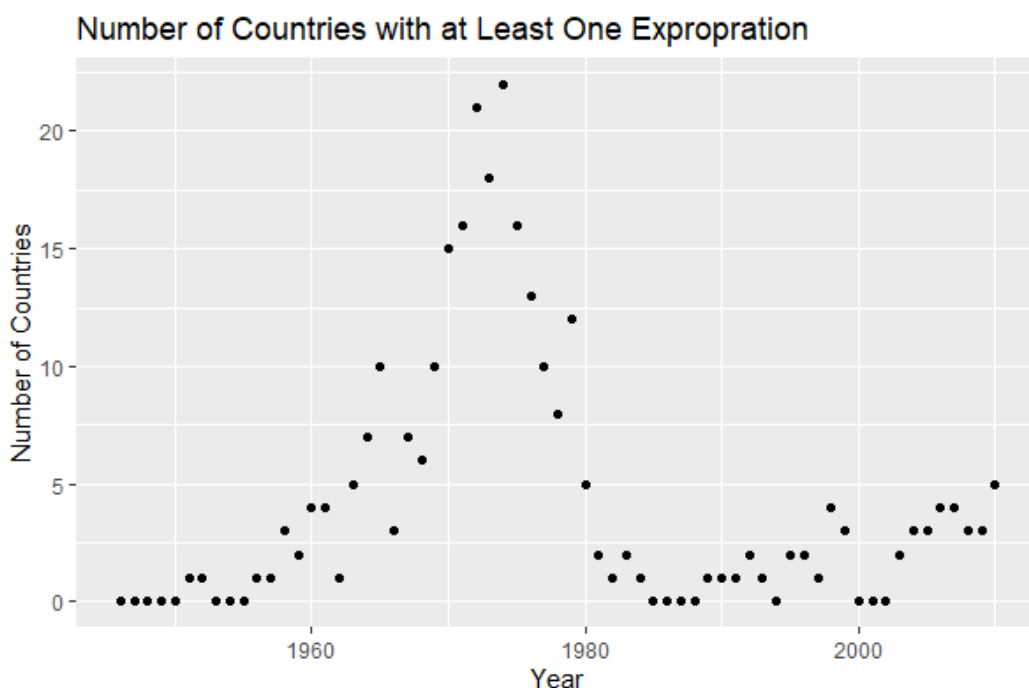


Figure 2: The figure indicates a significant increase in expropriations during the 1970s, followed by a gradual resurgence in the 2000s, coinciding with increased FDI in developing countries. However, the number of countries with expropriation cases per dollar of FDI is lower in the latter period, likely due to reduced FDI stock and stronger constraints from international organizations. Due to the sparse dependent variable and resulting high variance between years, we opted for decade fixed effects rather than year fixed effects.

### 3.2 Illness-and-Accident-Induced Transitions of Power

Our variable of interest is leaders' departure from office due to terminal or debilitating illnesses, but coding such departures can be difficult due to regimes hiding health information and supporters defecting when they realize death is imminent.

However, an illness may cause departure from office without causing immediate death. When King Fahd suffered a debilitating stroke in 1995, the royal family passed effective power to his son. However, he did not die for another 10 years. In regimes with the least instability upon succession, leaders are more likely to retire due to an illness than die in their boots. After personalist leader Abdelaziz Bouteflika suffered a stroke in 2013, his health was so compromised that he did not meet with his ministers during his final year in office. But he did not voluntarily give up power until popular demonstrations forced his hands. Bouteflika was right to cling to power; months after he left office, his former supporters imprisoned his family members to secure themselves and retaliate against him. Because leaders retire more easily when they expect no instability, excluding retirements would bias results upward.

Even worse, leaders flee when their illness creates sufficient instability. When supporters' positions depend on their personal relationships with the leader, a terminal illness dramatically decreases the value of said relationship [Mesquita and Smith, 2018]. A dead leader cannot reward, and an unconstrained successor may change the ruling coalition. As supporters learn about the leader's illness, the returns to loyalty rapidly decrease, and former supporters often cease to support the leader, sometimes going as far as joining the opposition. As a result, chronic, observable illnesses cause a spike in removals by supporters and challengers, even relative to leaders equally close to

death from observable natural causes [Mesquita and Smith, 2018]. Mobuto Sese Seko, Ferdinand Marcos, and the Shah of Iran were all abandoned by their supporters during chronic illnesses, fled, and shortly died in exile of chronic illnesses. Unsurprisingly, this effect is larger in strong-leader or personalist autocracies [Mesquita and Smith, 2018].

Therefore, when a terminal illness produces a severe crisis, it tends to be miscoded as a coup or a revolution. We solve this by including all instances in which the leader dies of an *observable* chronic illness immediately after departure. Therefore, we use two datasets for illnesses. We use Mesquita and Smith [2018]’s coding of chronic illnesses faced by autocrats. This dummy variable is positive if the leader died of a long-term terminal illness between their loss of power and the second following December 31<sup>st</sup>. The coding does not include sudden, unpredictable illnesses. Most cases are a result of cancer. We exclude cases where leaders retired due to debilitating illness but did not die within two years.

We use Archigos’ coding of natural deaths and retirement due to ill health [Goemans et al., 2009]. The downside to the Archigos measure is that it follows the official reason for the succession, often coded by the successors themselves. Archigos, therefore, excludes instances in which supporters observe a terminal illness and abandon their leader. Archigos does re-code retirements as coups when the leader’s career continues afterward (autocratic governments sometimes intentionally label coups as retirements to project unity).

Our main coding is the combination of both measures. Effectively, we include leaders who:

- Died of natural causes in their boots (in office)
- Announced a retirement<sup>7</sup> due to ill health and did not have subsequent political careers<sup>8</sup>
- Died of observable, chronic illnesses shortly after leaving office.

The variable *Transition Year* includes all successions by both codings. The variable *Transition Year (Arch)* includes only official successions by the Archigos database. The variable *Transition Year (Chronic)* includes only successions in which the leader died from a chronic, observable illness. To capture some variation in the severity of expropriation, we include the two years following a terminal illness. This allows expropriations that last longer to receive a higher weight. We reproduce our main results with these variables in Appendix A.

### 3.3 Personalism of the Regime Entering the Succession

Studies of personalist autocracies have traditionally coded multiple dummy variables dividing autocracies into different types: personalist, military, party, monarchy [Geddes, 1999]. We rejected this coding for several reasons. Personalism is a continuous aspect that can be observed across different regime types. Most importantly, the hand coding of regime types might be biased by observed succession crises as a sign of personalism. Moreover, hand-coding of regime types may be biased

<sup>7</sup>We do not attribute Ahmadou Ahidjo’s 1981 retirement from Cameroon to health reasons because Ahidjo’s career continued for 7 years afterwards, including two coup attempts.

<sup>8</sup>Archigos checked their subsequent careers.

by expropriations, with states having weak property rights more likely to be classified as personalist [Knutsen and Fjelde, 2013].

To address these issues, we use a continuous, annual measure of personalism developed by Geddes et al. [2017]. This measure is constructed using item-response theory and eight dummy variables coded for January 1 of each year. These variables capture the balance of power between the leader and supporters and are applicable to all autocracies.

The personalism score, measured in standard deviations, is not a measure of democracy and does not represent non-elite constraints on the leader or democratic institutions. We highlight that the correlation between personalism and polity score in our sample is low at 0.05. Our primary variable of interest is the personalism score of the leader who dies, which we call Predecessor Personalism (*Pred Pers*). This variable is recorded for January 1 of the year the leader left office. A score of 0 represents the average personalism for all country years, with observations having personalism scores of 1 being one standard deviation higher and observations with scores of -1 being one standard deviation lower.

### 3.4 Control Variables

The presence of foreign investments is a necessary condition for expropriating FDI. Our FDI measure was initially coded by tomz'sovereign'2008. We expanded it by filling in the missing information for years back to 1970 and adding in the UNCTAD FDI counts available for all countries after 1980. We also checked all the missing years after 1960 and incorporated any observations with FDI presence worth more than \$1 million.

To measure gross domestic product (GDP) per capita, we used World Development Indicators data supplemented by the Penn World Tables for missing years by Graham and Tucker [2019]. The variable *Natural Resource Rents* is the total rent percentage of GDP, as gathered by the World Development Indicators. We employed the combined polity score from the *Polity IV Combined Score* to capture democracies, and our measure of regime type duration came from the same Geddes et al. [2017] dataset as the personalism variable.

Table 1 displays descriptive statistics of expropriation in transition years, covering January 1<sup>st</sup> of the year after a natural death or health retirement. We discovered 86 such events in autocracies using Archigos data. As expected, transition years showed significantly lower personalism than average because of the new leaders' lack of experience and appointments. Transition years have higher resource rents than the average, which we controlled for in subsequent analysis. Their GDP per capita, polity scores, and years were similar to the average for autocracies in the sample.

## 4 Results

In this section, we empirically test our hypothesis that leader transitions (called *Transition Years*) increase expropriation only in personalist autocracies. The intuition behind this is reflected in the event study plots in Figure 3, which show a sharp increase in expropriations in leadership transition

Table 1: Autocracy Characteristics by Transition Period

|                         | (1)<br>Transition<br>Years |        |       | (2)<br>Non-Transition<br>Years |        |       |
|-------------------------|----------------------------|--------|-------|--------------------------------|--------|-------|
|                         | Mean                       | Median | SD    | Mean                           | Median | SD    |
| Expropriation           | 0.06                       | 0.00   | 0.25  | 0.06                           | 0.00   | 0.24  |
| Predecessor Personalism | -0.00                      | 0.05   | 0.85  | -0.24                          | -0.20  | 0.83  |
| Lag Personalism         | -0.24                      | -0.16  | 0.82  | 0.01                           | 0.04   | 0.87  |
| FDI                     | 0.98                       | 1.00   | 0.15  | 0.98                           | 1.00   | 0.14  |
| Log of GDPPC            | 23.89                      | 23.74  | 1.77  | 23.48                          | 23.31  | 1.71  |
| Natural Resource Rents  | 14.83                      | 9.38   | 15.72 | 11.39                          | 7.01   | 12.98 |
| Log of Population       | 16.19                      | 16.23  | 1.53  | 16.04                          | 16.00  | 1.36  |
| Leader Duration         | 1.96                       | 1.00   | 2.14  | 10.41                          | 8.00   | 9.15  |
| Polity IV Score         | -5.27                      | -7.00  | 4.45  | -4.96                          | -7.00  | 4.45  |
| Observations            | 170                        |        |       | 4421                           |        |       |

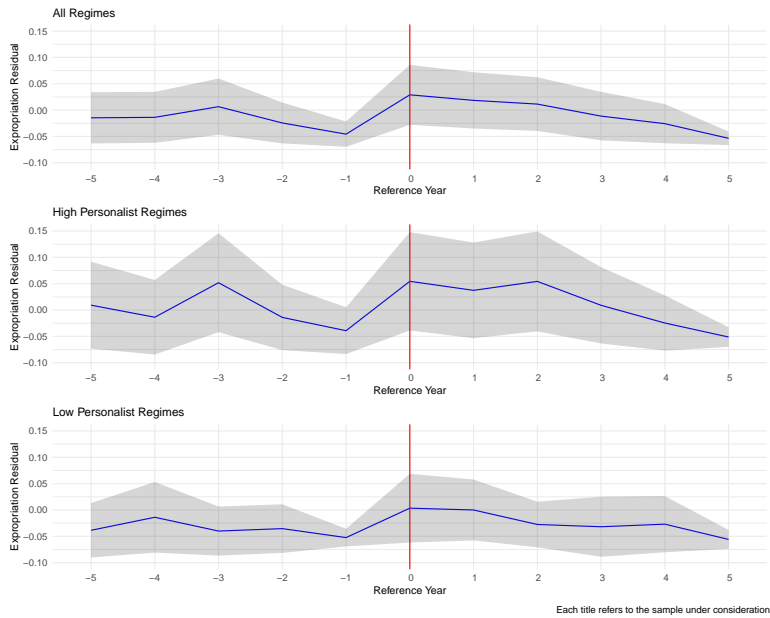


Figure 3: We plot the number of expropriations, adjusted by decade fixed effects, across all regimes before and after the year of turnover (which is labelled as 0 and is represented by the red line). The top plot includes all country-years. The downward trend over time is due to the spike in events in the 1970s and greater enforcement afterward. The second includes only regimes that are more personalist than the average. The final plot plots these residuals for regimes that are less personalist than average.

years when the predecessor was highly personalist. Therefore, we expect a positive interaction coefficient for the predecessor’s personalism and the transition year,  $Transition\ Year \times Pred\ Pers$ .

We test this hypothesis by running a fixed effects regression model specified in Equation 1:

$$Y_{i,t} = \beta TransitionYear_{i,t} + \gamma PredPers_{i,g(t)} + \delta(TransitionYear_{i,t} \times PredPers_{i,g(t)}) + \mathbb{X}_{i,t}\nu + \alpha_{d(t)} + \mu_i + \epsilon_{i,t} \quad (1)$$

where  $Y_{i,t}$  is a dummy for whether or not there was expropriation of FDI in country  $i$  in year  $t$ .  $g(t)$  refers to the Predecessor’s personalism in the year before their death.  $\mathbb{X}_{i,t}$  is the vector of controls added in the regression specification. We add country and decade fixed effects, which are  $\alpha$  and  $\mu$ , respectively. All models include decade fixed effects to account for large temporal patterns in expropriation<sup>9</sup> [Hajzler, 2012], and  $d(t)$  refers to the decade corresponding to year  $t$ . Finally,  $\epsilon_{i,t}$

<sup>9</sup>Expropriation events are concentrated in some years, and therefore decadal FE would allow us to capture this variation better. For example, year fixed effects would drop all observations from 1984 where no expropriations occurred despite a similar international context to 1983 and 1985 (see Figure 2).

corresponds to the error term. It is important to note that, unless specified otherwise, in all the regressions where country fixed effects are added, we cluster the standard errors at the country level. Table 2 summarizes our findings from a series of fixed effects regression models using Equation 1 and sequentially adding control variables.

The variable of interest for our hypothesis is *Transition Year X Pred Pers*, which shows a positive and significant (at 1% level of significance) effect on expropriation. A one standard deviation increase in the predecessor’s level of personalism raises the likelihood of FDI expropriation by an average of 10.8% to 15.6% during transition years compared to non-transition years.

| Table 2: Treatment Effects on Expropriation |                            |                     |                      |                      |
|---|----------------------------|---------------------|----------------------|----------------------|
|   | <i>Dependent variable:</i> |                     |                      |                      |
|   | Expropriation (Binary)     |                     |                      |                      |
|   | (1)                        | (2)                 | (3)                  | (4)                  |
| Transition Year                             | 0.029<br>(0.021)           | 0.028<br>(0.025)    | 0.016<br>(0.025)     | 0.034<br>(0.031)     |
| Pred Pers                                   | 0.001<br>(0.006)           | −0.002<br>(0.007)   | −0.003<br>(0.007)    | 0.016<br>(0.013)     |
| Transition Year X Pred Pers                 | 0.108***<br>(0.025)        | 0.156***<br>(0.030) | 0.152***<br>(0.030)  | 0.154**<br>(0.066)   |
| Lag Personalism                             | −0.009<br>(0.005)          | −0.008<br>(0.006)   | 0.001<br>(0.007)     | −0.017<br>(0.016)    |
| FDI Dummy                                   | 0.067**<br>(0.033)         | 0.053<br>(0.045)    | 0.051<br>(0.045)     | 0.056**<br>(0.027)   |
| Log of GDPPC                                |                            | 0.006<br>(0.004)    | 0.008*<br>(0.004)    | 0.034<br>(0.027)     |
| Natural Resource Rents                      |                            | 0.001<br>(0.0004)   | 0.001<br>(0.0004)    | −0.0001<br>(0.001)   |
| Log of Population                           |                            | 0.004<br>(0.004)    | 0.004<br>(0.004)     | −0.149***<br>(0.054) |
| Leader Tenure                               |                            |                     | −0.002***<br>(0.001) | 0.002**<br>(0.001)   |
| Polity IV Combined Score                    |                            |                     | 0.002<br>(0.001)     | 0.002<br>(0.003)     |
| Decade FE                                   | Yes                        | Yes                 | Yes                  | Yes                  |
| Country FE                                  | No                         | No                  | No                   | Yes                  |
| Observations                                | 2,751                      | 2,150               | 2,147                | 2,147                |

*Note:* \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

We confirm the robustness of our results in Appendix A by changing the coding of departures<sup>10</sup> and in Appendix B by correcting for autocorrelation using a Cochrane-Orcutt adjustment.

<sup>10</sup>We test the following codings and find that our results remain consistent: restricting transition years to only those that were caused by observable chronic illnesses (the Mesquita and Smith [2018] coding), to the inclusion of transitions caused only by leader accidents and retirements (which excludes departures for medical care), and to the inclusion of only one year since transition in leadership (as opposed to the main specification which allows for up to two years).

## 4.1 Testing for Heterogeneity in Treatment Effects

Our results above may not fully capture the differential impacts of a predecessor’s degree of power during transition years, as there may be heterogeneity in expropriation driven by whether the predecessor was highly personalist. To account for this, we reproduced the test with a split interaction term following Equation 2.

$$\begin{aligned}
 Y_{i,t} = & \beta_1(TransitionYear_{i,t} \times HighPredPers_{i,g(t)}) \\
 & + \beta_2(TransitionYear_{i,t} \times LowPredPers_{i,g(t)}) \\
 & + \delta HighPredPers_{i,g(t)} + \mathbb{X}_{i,t}\nu + \alpha_{d(t)} + \mu_i + \epsilon_{i,t}
 \end{aligned} \tag{2}$$

In this specification, we replaced the *Pred Pers* variable with a dummy variable, *High Pred Pers*, which is equal to one when *Pred Pers* is above its median value of 0.045, and 0 otherwise. The interaction terms, *Transition Year x High Pred Pers* and *Transition Year x Low Pred Pers*, are dummies for transition years when the predecessor’s personalism was high and low, respectively. The other covariates remain unchanged. We chose the median as the threshold for defining high and low predecessor personalism to avoid p-hacking concerns.

Table 3 shows that, in line with our hypothesis, high predecessor personalism during transition years is associated with an increased likelihood of expropriation, while there are no significant effects of low predecessor personalism during transition years.

We also tested alternative cutoff values to identify the threshold at which the effects begin, including 0 (equivalent to splitting our sample at the mean), 0.5, and 1 standard deviation (Table 4). The results show that the effect size increases with higher cutoff values, which supports our interaction term result that higher predecessor personalism is associated with a larger effect of turnover.



Table 3: Heterogenous Treatment Effects on Expropriation

|                                  | <i>Dependent variable:</i> |                     |                      |                     |
|----------------------------------|----------------------------|---------------------|----------------------|---------------------|
|                                  | Expropriation (Binary)     |                     |                      |                     |
|                                  | (1)                        | (2)                 | (3)                  | (4)                 |
| Transition Year x High Pred Pers | 0.083***<br>(0.031)        | 0.114***<br>(0.036) | 0.098***<br>(0.036)  | 0.106<br>(0.073)    |
| Transition Year x Low Pred Pers  | −0.019<br>(0.027)          | −0.026<br>(0.034)   | −0.037<br>(0.034)    | −0.010<br>(0.031)   |
| High Pred Pers                   | 0.011<br>(0.010)           | 0.002<br>(0.011)    | −0.002<br>(0.011)    | 0.013<br>(0.028)    |
| Lag Personalism                  | −0.008<br>(0.005)          | −0.007<br>(0.006)   | 0.003<br>(0.007)     | −0.015<br>(0.016)   |
| FDI Dummy                        | 0.070**<br>(0.033)         | 0.050<br>(0.046)    | 0.050<br>(0.045)     | 0.053*<br>(0.027)   |
| Log of GDPPC                     |                            | 0.006<br>(0.004)    | 0.008*<br>(0.004)    | 0.037<br>(0.028)    |
| Natural Resource Rents           |                            | 0.001<br>(0.0004)   | 0.001<br>(0.0004)    | 0.0001<br>(0.001)   |
| Log of Population                |                            | 0.004<br>(0.004)    | 0.004<br>(0.004)     | −0.131**<br>(0.056) |
| Leader Tenure                    |                            |                     | −0.002***<br>(0.001) | 0.001<br>(0.001)    |
| Polity IV Combined Score         |                            |                     | 0.001<br>(0.001)     | 0.001<br>(0.003)    |
| Decade FE                        | Yes                        | Yes                 | Yes                  | Yes                 |
| Country FE                       | No                         | No                  | No                   | Yes                 |
| Observations                     | 2,751                      | 2,150               | 2,147                | 2,147               |

*Note:* \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table 4: Treatment Effects on Expropriation (Different Thresholds)

|                                  | <i>Dependent variable:</i> |                             |                      |
|----------------------------------|----------------------------|-----------------------------|----------------------|
|                                  | Expropriation (Binary)     |                             |                      |
|                                  | Split at Mean              | Split at .5 sigma           | Split at 1 sigma     |
|                                  | (1)                        | (2)                         | (3)                  |
| Transition Year x High Pred Pers | 0.073<br>(0.058)           | 0.240**<br>(0.110)          | 0.497***<br>(0.144)  |
| Transition Year x Low Pred Pers  | 0.0002<br>(0.040)          | −0.022<br>(0.023)           | −0.025<br>(0.020)    |
| High Pred Pers                   | 0.007<br>(0.027)           | 0.006<br>(0.027)            | 0.013<br>(0.045)     |
| Lag Personalism                  | −0.014<br>(0.016)          | −0.015<br>(0.016)           | −0.015<br>(0.017)    |
| FDI Dummy                        | 0.053*<br>(0.027)          | 0.053*<br>(0.027)           | 0.053*<br>(0.027)    |
| Log of GDPPC                     | 0.0001<br>(0.001)          | −0.0001<br>(0.001)          | −0.0002<br>(0.001)   |
| Natural Resource Rents           | −0.133**<br>(0.055)        | −0.136***<br>(0.052)        | −0.149***<br>(0.054) |
| Log of Population                | 0.001<br>(0.001)           | 0.001<br>(0.001)            | 0.001<br>(0.001)     |
| Leader Tenure                    | 0.001<br>(0.003)           | 0.002<br>(0.003)            | 0.002<br>(0.003)     |
| Polity IV Combined Score         | 0.039<br>(0.027)           | 0.033<br>(0.026)            | 0.033<br>(0.026)     |
| Decade FE                        | Yes                        | Yes                         | Yes                  |
| Country FE                       | Yes                        | Yes                         | Yes                  |
| Observations                     | 2,147                      | 2,147                       | 2,147                |
| <i>Note:</i>                     |                            | *p<0.1; **p<0.05; ***p<0.01 |                      |

4.2 Does Predecessor Personalism Cause Treatment Effect Heterogeneity Through Alternative Channels?

The previous section indicates that personalist autocracies experience higher levels of expropriation during turnovers. However, personalism may be correlated with other variables that also affect turnover instability. For instance, personalist leaders may make appointments based on personal connections rather than merit and incentivize pro-regime loyalty through personal rewards, raising the political class’s cost of removing them. [Jones and Olken, 2005]; [Svolik, 2012]; [Geddes et al., 2018]. This may create a larger power vacuum when they turnover.

Further, it is also possible that personalism and predecessor’s personalism correlate with other variables such as natural resource rents [Fails, 2020]. Resource rents could therefore be a confounding variable, especially if rentier states expropriate more during succession crises. Jensen and Johnston [2011] argue that rentier states may face weaker reputational costs from expropriation and therefore respond more to a succession crisis.

Alternatively, democratic institutions might affect succession crisis severity. Autocracies also vary slightly in their democratic institutions [Gandhi and Przeworski, 2007], although variance in de jure parliaments and elections is low [Meng, 2020]. Vertical constraints have a major effect on expropriation in frequency and type [Graham et al., 2018]. Personalism has a correlation of -0.24 with the polity score. That is, the heterogeneity might be driven by regime type rather than personalism. Monarchies tend to have lower personalism scores, while military and single-party states have higher scores by 0.2 standard deviations on average across all country years. Each regime type has different succession mechanisms, most obviously in the case of monarchies.

We test the robustness of the predecessor-personalism-expropriation relationship to these alternative controls in Table 12 in Appendix C. To test for the heterogeneity in the treatment effect, we include each variable interacted with the transition variable. Model 1 adds an interaction with resource rents. Model 2 adds Polity IV combined Scores. Model 3 adds Wahman et al. [2013]’s coding of regimes by type into monarchy, military, single-party, and multi-party. We did not use the Geddes et al. [2014b] Personalism-Military-Party coding to avoid contamination via coding with the dependent variable, expropriation. In Model 4 we include all the interaction terms. As expected, the *Transition Year X Monarch* term is not significant, suggesting that there is a lack of evidence to support this channel.

Our results might alternatively be driven by ideology. One possibility is that because personalist regimes collapse more frequently upon leader deaths, they are replaced by populist left-wing governments. If such governments prefer to expropriate foreign investment, they are likely to enact such a policy in their early years. This is a possible mechanism behind our effect.

Even worse, we could get spurious results if predecessor personalism is correlated with left ideology and if left ideology causes expropriation, especially during turnover years. Leftist regimes might expropriate after a turnover to enact their policy agenda or to signal their ideological commitment. However, the Pearson correlation between predecessor personalism and leftism in our sample is -0.12<sup>11</sup> (presumably because the Soviet communist parties were effective leader-constraining institutions).

We test both mechanisms using political orientation data from Scartascini et al. [2018]. We selected it for its comprehensiveness; it covers 180 countries from 1975 onward. Using this data, we construct *Left Executive*, a dummy that takes a value of 1 for every country year in which the ruling party is “communist, socialist, social democratic, or left-wing”, and 0 otherwise. To mirror our predecessor personalism variable, the variable *Transition X Left Executive* extends the predecessor’s ideology to two years if they experience an exogenous turnover.

Table 13 gives the results of the model. The inclusion of ideology variables makes no substantive difference in the results. Most importantly, *Transition X Left Executive* is not significant suggesting that we lack the evidence to suggest that the successor’s ideology could be driving the variation in expropriation during transition years. Moreover, the sign on the coefficient is negative and the magnitude (in absolute terms) of the coefficient is seven times smaller than the coefficient on the interaction variable of predecessor personalism and transition year. This suggests that it is indeed the predecessor’s personalism that is increasing the likelihood of expropriation during the transition

<sup>11</sup>The Pearson correlation between personalism and leftism is similar and stands at -0.11.

years.

We considered using the predecessor’s ideology instead, however, in the entire dataset, there is only one case where the ideology changed over an exogenous succession, rendering the exercise statistically under-powered.

## 5 Robustness Checks

In addition to the checks mentioned in the previous sections, we include some more robustness checks to validate our results.

### 5.1 Components of the Personalism Measure

Our primary independent variable is constructed using Item Response Theory (IRT) to aggregate eight indicators of leader power into a single scalar variable. We selected this index because the use of multiple indicators may provide a more accurate or sensitive measure. We were also concerned about the possibility of coder bias while considering a binary measure of personalism, because the coder’s knowledge of property rights could influence the coding of personalism in general. The components of the IRT personalism measure refer to specific institutional variables and should thereby reduce coder bias.

We decided to use all components of the personalism measure because any elite institution could be used to threaten leaders with removal and thereby constrain executives. While our theoretical arguments apply most obviously to the independence of the party executive committee, in different institutional configurations an independent military or a cabinet may also both constrain leaders and threaten successors.

In this section, we show similar results if the individual components of the IRT personalism measure are used. The components of the measure come from [Geddes et al., 2017] and are:

- “Does access to high office depend on personal loyalty to the regime leader?” (*officepers*)
- “Did the regime leader create a new support political party after seizing power” (*creatparty*)
- “Does the regime leader control appointments to the party executive committee?” (*partyex-com-pers*)
- “Is the party executive committee absent or simply a rubber stamp for the regime leader’s decisions” (*partyrbstmp*)
- “Does the regime leader personally control the security apparatus?” (*sectyapp-pers*)
- “Does the regime leader promote officers loyal to himself or from his ethnic, tribal, regional, or partisan group, or are there widespread forced retirement of officers from other groups?” (*milmerit-pers*)

- “Does the regime leader create paramilitary forces, a president’s guard, or new security force loyal to himself?” (*paramil\_pers*)
- “Does the regime leader imprison/kill officers from groups other than his own without a reasonably fair trial?” (*milnotrial*)

In Table 5 we present a series of regressions in which the predecessor personalism composite measure is replaced by each component of the personalism measure. For a leader with no autocratic predecessor (i.e., preceded by a state foundation, a democrat, or a foreign occupation), we replace each predecessor’s personalism component with the average value across the entire sample.

Our results by component are consistent with our general results. Only two of the eight components are not significant at the 1% level of significance in the individual covariate models: *officepers* and *sectyapp*. This could suggest that the party organization variables are most important to succession stability, but there is too little variation to draw any inferences.



Table 5: Treatment Effects on Expropriation of Predecessor’s Components of Personalism Index

|                     | <i>Dependent variable:</i> |                    |                    |                    |                    |                   |                   |                   |                     |
|---------------------|----------------------------|--------------------|--------------------|--------------------|--------------------|-------------------|-------------------|-------------------|---------------------|
|                     | Expropriation (Binary)     |                    |                    |                    |                    |                   |                   |                   |                     |
|                     | (1)                        | (2)                | (3)                | (4)                | (5)                | (6)               | (7)               | (8)               | (9)                 |
| Transition Year     | −0.831<br>(1.054)          | 0.135<br>(0.466)   | −0.679<br>(0.752)  | −0.664<br>(0.752)  | −1.249<br>(1.042)  | −0.445<br>(0.758) | −0.049<br>(0.797) | −0.255<br>(0.644) | −3.525**<br>(1.616) |
| Trans x officepers  | 1.761<br>(1.144)           |                    |                    |                    |                    |                   |                   |                   | −0.135<br>(1.485)   |
| Trans x createparty |                            | 2.127**<br>(0.998) |                    |                    |                    |                   |                   |                   | 0.905<br>(1.404)    |
| Trans x partyexcom  |                            |                    | 2.164**<br>(0.929) |                    |                    |                   |                   |                   | 0.606<br>(2.459)    |
| Trans x partyrbr    |                            |                    |                    | 2.114**<br>(0.928) |                    |                   |                   |                   | 2.298<br>(2.590)    |
| Trans x milmerit    |                            |                    |                    |                    | 2.628**<br>(1.142) |                   |                   |                   | 3.346**<br>(1.610)  |
| Trans x milnotrial  |                            |                    |                    |                    |                    | 1.499*<br>(0.908) |                   |                   | −0.189<br>(1.303)   |
| Trans x sectyapp    |                            |                    |                    |                    |                    |                   | 0.749<br>(0.922)  |                   | −0.840<br>(1.394)   |
| Trans x paramil     |                            |                    |                    |                    |                    |                   |                   | 1.485*<br>(0.835) | 1.153<br>(1.204)    |
| Observations        | 2,677                      | 2,677              | 2,677              | 2,677              | 2,677              | 2,677             | 2,677             | 2,677             | 2,677               |
| Akaike Inf. Crit.   | 1,137.276                  | 1,136.424          | 1,129.314          | 1,130.674          | 1,132.671          | 1,132.411         | 1,138.991         | 1,136.891         | 1,136.559           |

*Note:* \*p<0.1; \*\*p<0.05; \*\*\*p<0.01  
All regressions include controls for FDI, the log of gdp per capita, natural resource rents, and the log of population



## 5.2 Missing Observations

Our current analysis relies on list-wise deletion, which is a potential source of bias. The main source of deletion is the lack of FDI or GDP data in early years for low income countries. The variables GDP, population, and natural resource rents are missing most observations before 1975. Only 19% of country years before 1975 have natural resource rents data. Similarly, due to the improved UNCTAD dataset, we observe FDI presence in 96% of cases after 1975, compared to only 56% before then. These missing observations are likely to bias the sample toward more developed states and to the more recent period after the expropriation slump of 1980.

As a robustness check, we impute the missing values using Multiple Imputation by Chained Equations (MICE). Imputing the covariates will mainly affect the results through increased sample size because variations in natural resources and population play a modest role in expropriation. MICE substitutes missing values with predicted variables with similar mean and variance.

Table 14 and Table 15 in Appendix D show the results of our regression model once we have imputed the missing values using MICE. With a few minor changes in values, the original results continue to hold with the interaction term indicating that, on average, the effect on expropriation of FDI of a one standard deviation increase in predecessor's personalism is 7% more during transition years compared to non-transition years. The effect size, but not significance, is smaller compared to what we found in our main specification, but it might be driven by the change in sample size.

## 5.3 Logit Model

Since our outcome variable is binary, we also include a logit model in Table 16 and Table 17 in Appendix E, which delineate the results. We do not use logit models in our primary specification because linear probability models offer more tools to reduce bias from autocorrelation, which is a greater threat to inference.

## 5.4 Post-Treatment Variables

This section adds controls for political change and instability downstream of leader succession, such as regime change, civil war, and changes in support groups. These variables are potential mediators between succession and expropriation. "Regime change (3yrs)" is one if the regime is 3 or fewer years old and is compiled by [Geddes et al. \[2014a\]](#). "Major civil wars" is 1 if more than 1000 people died in civil war in that year and comes from the Uppsala Conflict Data Project [[Wallensteen, 2013](#)]. Source of Leader Support (SOLS) Change measures "which leadership changes within countries bring to power a new leader whose primary support is drawn from different societal groups than those who supported her predecessor" [[Mattes et al., 2016](#)].

These variables are not included to reduce bias but to suggest that the process is not mediated solely through regime collapse. Failure to include the mediators cannot produce bias because regime change, civil wars, or support group changes do not cause leader illnesses and accidents. The post-treatment variables introduce new potential sources of bias from collider-stratification if un-

observable variables affect both the mediators and the outcome variable [Dworschak, 2024].

To the extent that readers believe the collider stratification bias is modest, the results provide a measure of the portion of the treatment effect that does not go through each potential mediator. The consistent positive coefficients in Table 6 suggest that the effect of personalist successions is entirely mediated by regime change, civil war, or support group change. Results for the interaction of personalism with succession are significant at the 5% level of significance in all models except the last two, which are significant at the .1 level.

Table 6: Including Posttreatment Variables

|                        | <i>Dependent variable:</i> |                    |                    |                   |                   |
|------------------------|----------------------------|--------------------|--------------------|-------------------|-------------------|
|                        | Expropriation (Binary)     |                    |                    |                   |                   |
|                        | (1)                        | (2)                | (3)                | (4)               | (5)               |
| Transition X Pred Pers | 2.184***<br>(0.606)        | 1.669**<br>(0.740) | 1.688**<br>(0.760) | 1.449*<br>(0.767) | 1.401*<br>(0.792) |
| New Regime (3yrs)      | 1.132***<br>(0.260)        | 0.256<br>(0.347)   |                    |                   | 0.314<br>(0.356)  |
| Major Civil War        |                            |                    | 0.056<br>(0.571)   |                   | 0.056<br>(0.571)  |
| SOLS Change            |                            |                    |                    | 0.351<br>(0.542)  | 0.466<br>(0.545)  |
| Transition Year        | −0.466<br>(0.659)          | −0.142<br>(0.744)  | −0.156<br>(0.758)  | −0.061<br>(0.727) | −0.070<br>(0.726) |
| Pred Pers              | −0.036<br>(0.149)          | 0.388<br>(0.256)   | 0.372<br>(0.262)   | 0.474*<br>(0.262) | 0.398<br>(0.270)  |
| Decade FE              | Yes                        | Yes                | Yes                | Yes               | Yes               |
| Country FE             | No                         | Yes                | Yes                | Yes               | Yes               |
| Observations           | 2,150                      | 2,150              | 2,106              | 2,046             | 2,006             |
| Akaike Inf. Crit.      | 784.226                    | 774.931            | 757.389            | 746.868           | 733.794           |

*Note:* \*p<0.1; \*\*p<0.05; \*\*\*p<0.01  
Additional controls for lag(pesonalism), log GDPPC, FDI, resource rents.

## 6 Case Studies

### 6.1 Case 1: Fondel Chemicals, Azerbaijan, 2005

In 2005, Azerbaijan seized \$57 million in investments made by German company Fondel in an aluminum plant. The government stated that Fondel failed to invest sufficiently in the plant [Murdova and Abbasov, 2006b]. However, the timing of the expropriation, alongside the purge of its negotiator and the family of its Azerbaijani co-owner, suggests that it was related to purges following the succession of Ilham Aliyev.

In 2001, President of Azerbaijan Heydar Aliyev appointed Farhad Aliyev as Minister of Economic Development. Farhad Aliyev was a long-time member of the Ruling New Azerbaijan party (no relation to President Heydar Aliyev). During Farhad Aliyev's tenure in office, his brother, Rafiq Aliyev, president of Azerbaijan's largest oil company, became an owner in a joint venture with Dutch multinational Fondel to take over Azeral, an aluminum mining and refining interest in Azerbaijan.

Following Heydar Aliyev's death in 2003 and succession by his son Ilham, Farhad Aliyev's political position was damaged. A dispute began between a faction led by Farhad Aliyev and rival oligarchs Heydar Babiye and Kalmadin Heydarov [Ismayilov, 2005a], who also controlled Azerbaijani firms. Farhad Aliyev and his faction took a pro-privatization position and publicly accused his rivals of corruption and operating monopolies in the parliament [Ismayilov, 2005b]. In 2005, Ilham Aliyev began isolating Farhad by moving control of privatization out of his ministry [Ismayilov, 2005b]. In October of 2005, Farhad and his brother Rafiq were imprisoned on charges of collaborating with the opposition to remove Ilham. The opposition party in question, the Azadig Alliance, denied the truth of the allegations and called the purge "a culmination of the struggle between oligarchs" [Ismayilov, 2005b]. Heydar Babiye replaced Farhad Aliyev as minister of economic development on October 19, 2005.

Fondel alleges that in October 2005, the same month as Farhad and co-owner Rafik were arrested, the Azerbaijani government began interfering in their operations [Murdova and Abbasov, 2006a]. The company alleges that the state confiscated the company seal, required approval for all internal company decisions, and intimidated Fondel employees. In 2006 Fondel launched a suit against the government for violation of the contract. Fondel's director suggested that the motives behind the expropriation were political, pointing to the arrest of Farhad and Rafik [Murdova and Abbasov, 2006a].

Given that Fondel was expropriated the same month that Farhad and Rafik were imprisoned and when other holdings linked to Farhad Aliyev were seized, one plausible explanation is that Fondel was under Aliyev's protection. When Aliyev was removed from the ruling coalition, Fondel lost their protection and could be targeted.

However, the purge of Farhad and Rafik was itself downstream of the new president's different ideological positions on privatization and foreign investment. Heydar Babayev championed an anti-privatization agenda, which drove the original dispute with Farhad [Murdova and Abbasov, 2006a]. Because "personnel is policy," there is no easy way to distinguish the effect of the policy change

from the purge.

## 6.2 Case 2: Mineral Interests in Congo (Kinshasa) in 1998

By 1997, Mobutu Sese Seko had ruled the Congo for 26 years. The regime he constructed was personalized to the extent that he became infamous for publicly executing his elite supporters. But by 1997, Mobutu could no longer hide his prostate cancer. Sensing weakness, his supporters abandoned him, and rebels mobilized. With foreign backing, a new rebel coalition was formed under Laurent Kabila, Andre Kisase Ngandu, and Anselme Masasu Nindaga [[Roessler and Verhoeven, 2019](#)]. By mid-1997, Kabila and his allies were in the capital.

Kabila inherited no real constraining institutions from his predecessor, and his new government had problems motivating his supporters. Rather than attempt to build constraints, as predicted by [Meng \[2020\]](#), Kabila resorted to repressing his senior supporters as Mobutu had done. His rebel ally Kisase died under suspicious circumstances while marching to the capital; he was most likely killed with Kabila's consent. Once Kabila arrived in office, he shrank his support coalition by betraying his Rwandan and Ugandan benefactors, who then supported an insurgency in the eastern Congo (Kinshasa). Next, Kabila came to distrust his remaining ally Masasu Nindanga, and had him executed. In response, troops recruited by Masasu left the front lines, further deteriorating the war situation [[Zajtmán and Rabaud, 2011](#)].

These purges left Kabila confronting a civil war with a significantly reduced fighting force compared to what he initially commanded. Having repeatedly purged his supporters, Kabila could hardly promise future rewards to his remaining supporters. Kabila would have to pay up front, for which he turned to expropriation.

In 2000, Kabila sold a three-year monopoly on diamond exports to an Israeli firm, forcing all other trading firms out of the market. Kabila's investment advisor Nkere Ntanda said of the move "The war was still raging. The equipment had to be paid for, the soldiers had to be paid. New ways of obtaining funds had to be found and this monopoly was a way of achieving it" [[Zajtmán and Rabaud, 2011](#)]. Ntanda's comments imply that these cash-generating schemes were intended to pay for the war.

Expropriation and extortion did not work out for Kabila, as his worsening reputation with investors rapidly decreased demand for his concessions and monopolies. In 2000, the diamond export monopoly was sold for just \$20 million [[Zajtmán and Rabaud, 2011](#)]. A few months later Kabila was killed by his bodyguards, who were paid poorly and irregularly, after much of his army had deserted him [[Prunier, 2009](#)].

We do not argue that the path Laurent Kabila chose was optimal or a superior alternative to building a non-personalist regime ([Meng \[2020\]](#) suggests that path is more common). Perhaps Kabila's suspected killing of close allies in 1997 prevented him from building trust with his supporters, or he simply preferred to keep the regime personalist for idiosyncratic reasons. However, the case suggests that Kabila's "inheritance" from Mobutu was critical to his decision to adopt expropriation. Leaders who inherit functioning constraining institutions would have been able to credibly promise

future shares of power in a way that Kabila could not.

## 7 Conclusion

This paper finds that terminal illnesses of autocrats are associated with an increased likelihood of expropriation of FDI only in personalist regimes. During transitions, weaker support institutions increase volatility in policy, rent distribution, and external threats. Because personalist regimes have a high concentration of power and lack strong support institutions, they suffer from greater instability and policy volatility during transitions.

We find that a one standard deviation increase in the predecessor's personalism is associated with a 10.8% to 15.6% increase in the likelihood of expropriation of FDI during transition years, compared to non-transition years. Moreover, a look at heterogeneous treatment effects suggests that above-median predecessor personalism during the transition year increases the likelihood of expropriation by 8.30 percentage points. We do not find any evidence of a similar relationship between personalism and FDI expropriations for transitions with below-median predecessor personalism.

Our results also have practical implications for allocating foreign investment and pricing insurance contracts. Terminal illness of leaders should not affect pricing in non-personalist regimes, while personalist regimes are less reliable clients, despite what within-leader analyses might suggest.

## References

- Jeune Afrique. Le Togo nationalise l'hôtel 2 février - Jeune Afrique.com, November 2014. URL <https://www.jeuneafrique.com/5272/economie-entreprises/le-togo-nationalise-l-h-tel-2-f-vrier/>.
- Michael Albertus and Victor Menaldo. Dictators as Founding Fathers? The Role of Constitutions Under Autocracy. *Economics & Politics*, 24(3):279–306, 2012a. ISSN 1468-0343. doi: 10.1111/j.1468-0343.2012.00402.x. URL <https://onlinelibrary.wiley.com/doi/abs/10.1111/j.1468-0343.2012.00402.x>. eprint: <https://onlinelibrary.wiley.com/doi/pdf/10.1111/j.1468-0343.2012.00402.x>.
- Michael Albertus and Victor Menaldo. If You're Against Them You're With Us: The Effect of Expropriation on Autocratic Survival. *Comparative Political Studies*, 45(8):973–1003, August 2012b. ISSN 0010-4140, 1552-3829. doi: 10.1177/0010414011428593. URL <http://journals.sagepub.com/doi/10.1177/0010414011428593>.
- William W. Beach and Marc A. Miles. Explaining the factors of the index of economic freedom. *2006 Index of Economic Freedom*, pages 55–76, 2006. URL <https://citeseerx.ist.psu.edu/document?repid=rep1&type=pdf&doi=43ce606c68255e05f95ab9fcc3a7f4823f4f2ae4#page=73>. Publisher: Citeseer.
- Marie Brossier. Succession in Personalist Regimes in Africa. *Personalism and Personalist Regimes*, page 231, 2024. URL [https://books.google.com/books?hl=en&lr=&id=9CcHEQAAQBAJ&oi=fnd&pg=PA231&dq=brossier+succession&ots=LKF\\_KWafVb&sig=rpqpFDFAPnZoaV-EP21W0dnvdj8](https://books.google.com/books?hl=en&lr=&id=9CcHEQAAQBAJ&oi=fnd&pg=PA231&dq=brossier+succession&ots=LKF_KWafVb&sig=rpqpFDFAPnZoaV-EP21W0dnvdj8). Publisher: Oxford University Press.
- Jason Brownlee. Hereditary succession in modern autocracies. *World politics*, 59(4):595–628, 2007. URL <https://www.cambridge.org/core/journals/world-politics/article/hereditary-succession-in-modern-autocracies/BBE26B84497FEF15B1D98D85B3C07AC4>. Publisher: Cambridge University Press.
- Jorge G. Castaneda. *Perpetuating Power: How Mexican Presidents Were Chosen*. New Press, November 2001. ISBN 978-1-56584-708-8.
- John Chin, Abel Escribà-Folch, Wonjun Song, and Joseph Wright. Reshaping the Threat Environment: Personalism, Coups, and Assassinations. Technical report, Working Paper, 2020.
- Bruce Bueno De Mesquita, Alastair Smith, Randolph M. Siverson, and James D. Morrow. *The logic of political survival*. MIT press, 2005.
- Christoph Dworschak. Bias mitigation in empirical peace and conflict studies: A short primer on posttreatment variables. *Journal of Peace Research*, 61(3):462–476, May 2024. ISSN 0022-3433, 1460-3578. doi: 10.1177/00223433221145531. URL <https://journals.sagepub.com/doi/10.1177/00223433221145531>.
- Jane Esberg and Rebecca Perlman. Covert Confiscation: How Governments Differ in Their Strategies of Expropriation. *Princeton University*, 2020.
- Matthew D. Fails. Leader Turnover, Volatility, and Political Risk. *Politics & Policy*, 42(3):369–399, 2014. ISSN 1747-1346. doi: 10.1111/polp.12078. URL



- <https://onlinelibrary.wiley.com/doi/abs/10.1111/polp.12078>. eprint: <https://onlinelibrary.wiley.com/doi/pdf/10.1111/polp.12078>.
- Matthew D. Fails. Oil income and the personalization of autocratic politics. *Political Science Research and Methods*, 8(4):772–779, October 2020. ISSN 2049-8470, 2049-8489. doi: 10.1017/psrm.2019.14. URL <http://www.cambridge.org/core/journals/political-science-research-and-methods/article/oil-income-and-the-personalization-of-autocratic-politics/55DC4E21E3206EF09EEB69E63BF298D1>. Publisher: Cambridge University Press.
- James D. Fearon. Rationalist explanations for war. *International organization*, 49(3):379–414, 1995. Publisher: Cambridge University Press.
- Erica Frantz and Andrea Kendall-Taylor. Pathways to democratization in personalist dictatorships. *Democratization*, 24(1):20–40, January 2017. ISSN 1351-0347, 1743-890X. doi: 10.1080/13510347.2015.1131977. URL <https://www.tandfonline.com/doi/full/10.1080/13510347.2015.1131977>.
- Jennifer Gandhi and Adam Przeworski. Authoritarian Institutions and the Survival of Autocrats. *Comparative Political Studies*, 40(11):1279–1301, November 2007. ISSN 0010-4140. doi: 10.1177/0010414007305817. URL <https://doi.org/10.1177/0010414007305817>. Publisher: SAGE Publications Inc.
- Barbara Geddes. What do we know about democratization after twenty years? *Annual review of political science*, 2(1):115–144, 1999. Publisher: Annual Reviews 4139 El Camino Way, PO Box 10139, Palo Alto, CA 94303-0139, USA.
- Barbara Geddes, Joseph Wright, and Erica Frantz. Autocratic regimes codebook. *Available from: dictators. la. psu. edu*, 2013.
- Barbara Geddes, Joseph Wright, and Erica Frantz. Autocratic Breakdown and Regime Transitions: A New Data Set. *Perspectives on Politics*, 12(2):313–331, June 2014a. ISSN 1537-5927, 1541-0986. doi: 10.1017/S1537592714000851. URL [https://www.cambridge.org/core/product/identifier/S1537592714000851/type/journal\\_article](https://www.cambridge.org/core/product/identifier/S1537592714000851/type/journal_article).
- Barbara Geddes, Joseph Wright, and Erica Frantz. New data set: Autocratic breakdown and regime transitions. *Perspectives on Politics*, 12(2):313–331, 2014b.
- Barbara Geddes, Joseph Wright, and Erica Frantz. A measure of personalism in dictatorships. page 17, October 2017.
- Barbara Geddes, Joseph George Wright, Joseph Wright, and Erica Frantz. *How dictatorships work: Power, personalization, and collapse*. Cambridge University Press, 2018.
- Scott Gehlbach and Philip Keefer. Investment without democracy: Ruling-party institutionalization and credible commitment in autocracies. *Journal of Comparative Economics*, 39(2):123–139, June 2011. ISSN 0147-5967. doi: 10.1016/j.jce.2011.04.002. URL <https://www.sciencedirect.com/science/article/pii/S0147596711000242>.
- Scott Gehlbach and Philip Keefer. Private Investment and the Institutionalization of Collective Action in Autocracies: Ruling Parties and Legislatures. *The Journal of Politics*, 74(2):621–635,

- April 2012. ISSN 0022-3816, 1468-2508. doi: 10.1017/S0022381611001952. URL <https://www.journals.uchicago.edu/doi/10.1017/S0022381611001952>.
- Henk E. Goemans, Kristian Skrede Gleditsch, and Giacomo Chiozza. Introducing Archigos: A Dataset of Political Leaders. *Journal of Peace Research*, 46(2):269–283, March 2009. ISSN 0022-3433, 1460-3578. doi: 10.1177/0022343308100719. URL <http://journals.sagepub.com/doi/10.1177/0022343308100719>.
- Benjamin A. T. Graham, Noel P. Johnston, and Allison F. Kingsley. Even Constrained Governments Take: The Domestic Politics of Transfer and Expropriation Risks. *Journal of Conflict Resolution*, 62(8):1784–1813, September 2018. ISSN 0022-0027. doi: 10.1177/0022002717701181. URL <https://doi.org/10.1177/0022002717701181>. Publisher: SAGE Publications Inc.
- Benjamin AT Graham and Jacob R. Tucker. The international political economy data resource. *The Review of International Organizations*, 14(1):149–161, 2019. Publisher: Springer.
- Julia Gray and Jeffrey Kucik. Leadership Turnover and the Durability of International Trade Commitments. *Comparative Political Studies*, 50(14):1941–1972, December 2017. ISSN 0010-4140. doi: 10.1177/0010414017695330. URL <https://doi.org/10.1177/0010414017695330>. Publisher: SAGE Publications Inc.
- Alexander Taaning Grundholm. Taking it personal? Investigating regime personalization as an autocratic survival strategy. *Democratization*, 27(5):797–815, July 2020. ISSN 1351-0347, 1743-890X. doi: 10.1080/13510347.2020.1737677. URL <https://www.tandfonline.com/doi/full/10.1080/13510347.2020.1737677>.
- Christopher Hajzler. Expropriation of foreign direct investments: sectoral patterns from 1993 to 2006. *Review of World Economics*, 148(1):119–149, 2012. Publisher: Springer.
- Rovshan Ismayilov. Arrests of Entrepreneurs in Azerbaijan Likely Linked to Political Re-Shuffle | Eurasianet, December 2005a. URL <https://eurasianet.org/arrests-of-entrepreneurs-in-azerbaijan-likely-linked-to-political-re-shuffle>.
- Rovshan Ismayilov. Azerbaijani Minister Fired, Allegedly Arrested for Coup Attempt | Eurasianet, October 2005b. URL <https://eurasianet.org/azerbaijani-minister-fired-allegedly-arrested-for-coup-attempt>.
- Nathan M. Jensen and Noel P. Johnston. Political Risk, Reputation, and the Resource Curse. *Comparative Political Studies*, 44(6):662–688, June 2011. ISSN 0010-4140. doi: 10.1177/0010414011401208. URL <https://doi.org/10.1177/0010414011401208>. Publisher: SAGE Publications Inc.
- Nathan M. Jensen, Edmund Malesky, and Stephen Weymouth. Unbundling the relationship between authoritarian legislatures and political risk. *British Journal of Political Science*, pages 655–684, 2014. Publisher: JSTOR.
- Nathan M. Jensen, Noel P. Johnston, Chia-yi Lee, and Hadi Sahin. Crisis and contract breach: The domestic and international determinants of expropriation. *The Review of International Organizations*, 15(4):869–898, October 2020. ISSN 1559-7431, 1559-744X. doi: 10.1007/s11558-019-09363-z. URL <http://link.springer.com/10.1007/s11558-019-09363-z>.

- Leslie Johns and Rachel L. Wellhausen. Under One Roof: Supply Chains and the Protection of Foreign Investment. *American Political Science Review*, 110(1):31–51, February 2016. ISSN 0003-0554, 1537-5943. doi: 10.1017/S000305541500057X. URL [https://www.cambridge.org/core/product/identifier/S000305541500057X/type/journal\\_article](https://www.cambridge.org/core/product/identifier/S000305541500057X/type/journal_article).
- Benjamin F. Jones and Benjamin A. Olken. Do Leaders Matter? National Leadership and Growth Since World War II\*. *The Quarterly Journal of Economics*, 120(3):835–864, August 2005. ISSN 0033-5533. doi: 10.1093/qje/120.3.835. URL <https://doi.org/10.1093/qje/120.3.835>.
- Andrea Kendall-Taylor and Erica Frantz. When dictators die. *Journal of Democracy*, 27(4):159–171, 2016. Publisher: Johns Hopkins University Press.
- Carl Henrik Knutsen and Hanne Fjelde. Property rights in dictatorships: kings protect property better than generals or party bosses. *Contemporary Politics*, 19(1):94–114, 2013. Publisher: Taylor & Francis.
- Stephen J. Kobrin. Expropriation as an attempt to control foreign firms in LDCs: trends from 1960 to 1979. *International Studies Quarterly*, 28(3):329–348, 1984. Publisher: Blackwell Publishing Ltd Oxford, UK.
- Andrej Kokkonen and Anders Sundell. Leader Succession and Civil War. *Comparative Political Studies*, 53(3-4):434–468, March 2020. ISSN 0010-4140, 1552-3829. doi: 10.1177/0010414019852712. URL <https://journals.sagepub.com/doi/10.1177/0010414019852712>.
- Alex M. Kroeger. Dominant party rule, elections, and cabinet instability in African autocracies. *British Journal of Political Science*, 50(1):79–101, 2020. Publisher: Cambridge University Press.
- Andrew X. Li and Wen Zha. Leadership turnover, winning coalition size and foreign policy realignment. *Asian Review of Political Economy*, 3(1):2, February 2024. ISSN 2731-5835. doi: 10.1007/s44216-024-00022-8. URL <https://link.springer.com/10.1007/s44216-024-00022-8>.
- Michaela Mattes, Brett Ashley Leeds, and Naoko Matsumura. Measuring change in source of leader support: The CHISOLS dataset. *Journal of Peace Research*, 53(2):259–267, March 2016. ISSN 0022-3433. doi: 10.1177/0022343315625760. URL <https://doi.org/10.1177/0022343315625760>. Publisher: SAGE Publications Ltd.
- Anne Meng. *Constraining Dictatorship: From Personalized Rule to Institutionalized Regimes*. Cambridge University Press, August 2020. ISBN 978-1-108-89214-8. Google-Books-ID: d732DwAAQBAJ.
- Bruce Bueno de Mesquita and Alastair Smith. Political Loyalty and Leader Health. *Quarterly Journal of Political Science*, 13(4):333–361, October 2018. ISSN 1554-0626, 1554-0634. doi: 10.1561/100.00017123. URL <https://www.nowpublishers.com/article/Details/QJPS-17123>. Publisher: Now Publishers, Inc.
- Michael S. Minor. The demise of expropriation as an instrument of LDC policy 1980–1992. *Journal of International Business Studies*, 25(1):177–188, 1994. Publisher: Springer.

- Mina Murdova and Rufat Abbasov. Contract Dispute Disturbs Azerbaijan's Business Climate, March 2006a. URL <https://eurasianet.org/contract-dispute-disturbs-azerbaijans-business-climate>.
- Mina Murdova and Rufat Abbasov. Contract Dispute Disturbs Azerbaijan's Business Climate, March 2006b. URL <https://eurasianet.org/contract-dispute-disturbs-azerbaijans-business-climate>.
- Roger B. Myerson. Moral hazard in high office and the dynamics of aristocracy. *Econometrica*, 83 (6):2083–2126, 2015. Publisher: Wiley Online Library.
- Krzysztof J. Pelc. What Explains the Low Success Rate of Investor-State Disputes? *International Organization*, 71(3):559–583, 2017. ISSN 0020-8183, 1531-5088. doi: 10.1017/S0020818317000212. URL [https://www.cambridge.org/core/product/identifier/S0020818317000212/type/journal\\_article](https://www.cambridge.org/core/product/identifier/S0020818317000212/type/journal_article).
- Gérard Prunier. *From genocide to continental war: The 'Congolese' conflict and the crisis of contemporary Africa*. Hurst & Company, 2009.
- Philip Roessler and Harry Verhoeven. *Why comrades go to war: liberation politics and the outbreak of Africa's deadliest conflict*. Oxford University Press, USA, 2019.
- Carlos Scartascini, Cesi Cruz, and Philip Keefer. The Database of Political Institutions 2017 (DPI2017). Technical report, Inter-American Development Bank, March 2018. URL <http://publications.iadb.org/11319/8806>.
- Milan W. Svolik. *The Politics of Authoritarian Rule*. Cambridge University Press, September 2012. ISBN 978-1-139-56107-5. Google-Books-ID: 6fUgAwAAQBAJ.
- Joan C. Timoneda, Abel Escribà-Folch, and Kyle Haynes. The Paths to Personalization: How Dictators Accumulate Power Over Time. *SSRN Scholarly Paper*, January 2024. doi: 10.2139/ssrn.4706527. URL <https://papers.ssrn.com/abstract=4706527>.
- Michael Tomz and Mark L. J. Wright. Sovereign Theft: Theory And Evidence About Sovereign Default And Expropriation. In *University, Centre*, 2008.
- Michael Wahman, Jan Teorell, and Axel Hadenius. Authoritarian regime types revisited: updated data in comparative perspective. *Contemporary Politics*, 19(1):19–34, 2013. Publisher: Taylor & Francis.
- Peter Wallensteen. The Uppsala Conflict Data Program, 1978–2010: The story, the rationale and the programme1. In *Peace research*, pages 111–124. Routledge, 2013. URL <https://api.taylorfrancis.com/content/chapters/edit/download?identifierName=doi&identifierValue=10.4324/9780203808962-12&type=chapterpdf>.
- Matthew Charles Wilson and Joseph Wright. Autocratic legislatures and expropriation risk. *British Journal of Political Science*, 47(1):1–17, 2017. Publisher: Cambridge University Press.
- Joseph Wright and Boliang Zhu. Monopoly rents and foreign direct investment in fixed assets. *International Studies Quarterly*, 62(2):341–356, 2018. Publisher: Oxford University Press.
- Arnaud Zajtman and Marlene Rabaud. Murder in Kinshasa, June 2011. URL <https://www.youtube.com/watch?v=rDqTddLqo-w>.

## 8 Appendix

### A Decomposing Chronic Illnesses and Retirements or Accidents

Here we report our main results with alternative codings of exogenous turnovers.

*Transition Year (Chronic)* is one for all years in which a leader died of a chronic and observable disease while in office or in the subsequent calendar year. It is true for one or for two years after departure. We test this in Table 7. *Transition Year (Arch)* includes leaders who retired due to illness or died in an accident (and is true for up to two years after departure). We test this in Table 8. As a reminder, our main measure is a combination of both these measures because leaders may strategically decide not to retire. Specifically, if they anticipate instability they are more likely to hang on to power late into a severe illness, which (to a large extent) we correct for in our main specification.

Table 7: Treatment Effects on Expropriation (Considering only Chronic Illnesses)

|                                      | <i>Dependent variable:</i> |                     |                     |                     |
|--------------------------------------|----------------------------|---------------------|---------------------|---------------------|
|                                      | Expropriation (Binary)     |                     |                     |                     |
|                                      | (1)                        | (2)                 | (3)                 | (4)                 |
| Transition Year (Chronic)            | 0.040<br>(0.027)           | 0.008<br>(0.032)    | −0.004<br>(0.033)   | 0.018<br>(0.038)    |
| Pred Pers                            | 0.001<br>(0.006)           | −0.002<br>(0.007)   | −0.003<br>(0.007)   | 0.014<br>(0.013)    |
| Transition Year (Chronic) x PredPers | 0.192***<br>(0.032)        | 0.254***<br>(0.038) | 0.249***<br>(0.038) | 0.240***<br>(0.086) |
| Lag Personalism                      | −0.009*<br>(0.005)         | −0.009<br>(0.006)   | 0.001<br>(0.007)    | −0.018<br>(0.016)   |
| Decade FE                            | Yes                        | Yes                 | Yes                 | Yes                 |
| Country FE                           | No                         | No                  | No                  | Yes                 |
| Observations                         | 2,751                      | 2,150               | 2,147               | 2,147               |

Notes. Regressions include controls for FDI, Log of GDPPC, Natural resource rent, Log of population, Leader duration, and Polity IV Combined Score. \*p<0.1 \*\*p<0.05 \*\*\*p<0.01.

Table 8: Treatment Effects on Expropriation (Using only Retirement Data)

|                                    | <i>Dependent variable:</i> |                     |                     |                   |
|------------------------------------|----------------------------|---------------------|---------------------|-------------------|
|                                    | Expropriation (Binary)     |                     |                     |                   |
|                                    | (1)                        | (2)                 | (3)                 | (4)               |
| Transition Year (Arch)             | 0.013<br>(0.021)           | 0.010<br>(0.026)    | −0.003<br>(0.026)   | 0.022<br>(0.030)  |
| Pred Pers                          | 0.003<br>(0.006)           | 0.001<br>(0.007)    | −0.001<br>(0.007)   | 0.020<br>(0.014)  |
| Transition Year (Arch) X Pred Pers | 0.072***<br>(0.027)        | 0.115***<br>(0.034) | 0.110***<br>(0.034) | 0.113<br>(0.069)  |
| Lag Personalism                    | −0.008<br>(0.005)          | −0.008<br>(0.006)   | 0.003<br>(0.007)    | −0.015<br>(0.016) |
| Decade FE                          | Yes                        | Yes                 | Yes                 | Yes               |
| Country FE                         | No                         | No                  | No                  | Yes               |
| Observations                       | 2,751                      | 2,150               | 2,147               | 2,147             |

Notes. Regressions include controls for FDI, Log of GDPPC, Natural resource rent, Log of population, Leader duration, and Polity IV Combined Score. \*p<0.1 \*\*p<0.05 \*\*\*p<0.01.

In Table 9 we reproduce the primary result, using only the first year (January 1<sup>st</sup> - December 31<sup>st</sup>) after a leader died or retired due to ill health. (This is set against our main model specification where up to two years of transition are considered.)

Table 9: Treatment Effects on Expropriation (Using Archigos Data For One Transition Year)

|                                   | <i>Dependent variable:</i> |                     |                     |                     |
|-----------------------------------|----------------------------|---------------------|---------------------|---------------------|
|                                   | Expropriation (Binary)     |                     |                     |                     |
|                                   | (1)                        | (2)                 | (3)                 | (4)                 |
| (One) Transition Year             | 0.022<br>(0.028)           | 0.018<br>(0.034)    | 0.003<br>(0.034)    | 0.028<br>(0.038)    |
| Pred Pers                         | 0.003<br>(0.006)           | 0.001<br>(0.007)    | −0.001<br>(0.007)   | 0.019<br>(0.013)    |
| (One) Transition Year X Pred Pers | 0.126***<br>(0.035)        | 0.202***<br>(0.043) | 0.194***<br>(0.043) | 0.200***<br>(0.070) |
| Lag Personalism                   | −0.009*<br>(0.005)         | −0.009<br>(0.006)   | 0.001<br>(0.007)    | −0.018<br>(0.016)   |
| Decade FE                         | Yes                        | Yes                 | Yes                 | Yes                 |
| Country FE                        | No                         | No                  | No                  | Yes                 |
| Observations                      | 2,751                      | 2,150               | 2,147               | 2,147               |

Notes. Regressions include controls for FDI, Log of GDPPC, Natural resource rent, Log of population, Leader duration, and Polity IV Combined Score. \*p<0.1 \*\*p<0.05 \*\*\*p<0.01.

B    **Cochrane-Orcutt Adjustment for Autocorrelation**

Table 10 and Table 11 present the main and heterogeneous treatment specifications, respectively, with the Cochrane-Orcutt adjustment to correct for autocorrelation in our model. The results are nearly identical. The effect on expropriation of FDI of a one standard deviation increase in predecessor’s personalism is, on average, 10.3-14.7% more during transition years as compared to non-transition years (Table 10).

Table 10: Treatment Effects on Expropriation (Cochrane-Orcutt Adjustment)

|                             | <i>Dependent variable:</i> |                     |                     |                     |
|-----------------------------|----------------------------|---------------------|---------------------|---------------------|
|                             | Expropriation (Binary)     |                     |                     |                     |
|                             | (1)                        | (2)                 | (3)                 | (4)                 |
| Transition Year             | 0.027<br>(0.022)           | 0.026<br>(0.026)    | 0.015<br>(0.027)    | 0.033<br>(0.026)    |
| Pred Pers                   | 0.001<br>(0.007)           | −0.001<br>(0.008)   | −0.003<br>(0.008)   | 0.015<br>(0.011)    |
| Transition Year X Pred Pers | 0.103***<br>(0.026)        | 0.142***<br>(0.032) | 0.138***<br>(0.032) | 0.147***<br>(0.031) |
| Lag Personalism             | −0.010<br>(0.006)          | −0.010<br>(0.007)   | 0.0002<br>(0.008)   | −0.018*<br>(0.010)  |
| Decade FE                   | Yes                        | Yes                 | Yes                 | Yes                 |
| Country FE                  | No                         | No                  | No                  | Yes                 |
| Observations                | 2,751                      | 2,150               | 2,147               | 2,147               |

Notes. Regressions include controls for FDI, Log of GDPPC, Natural resource rent, Log of population, Leader duration, and Polity IV Combined Score. \*p<0.1 \*\*p<0.05 \*\*\*p<0.01.

Table 11: Heterogenous Treatment Effects on Expropriation (Cochrane-Orcutt Adjustment)

|                                  | <i>Dependent variable:</i> |                     |                    |                     |
|----------------------------------|----------------------------|---------------------|--------------------|---------------------|
|                                  | Expropriation (Binary)     |                     |                    |                     |
|                                  | (1)                        | (2)                 | (3)                | (4)                 |
| Transition Year x High Pred Pers | 0.095***<br>(0.034)        | 0.113***<br>(0.038) | 0.098**<br>(0.038) | 0.105***<br>(0.038) |
| Transition Year x Low Pred Pers  | −0.022<br>(0.030)          | −0.031<br>(0.036)   | −0.041<br>(0.036)  | −0.013<br>(0.035)   |
| High Pred Pers                   | 0.006<br>(0.012)           | 0.002<br>(0.013)    | −0.002<br>(0.013)  | 0.012<br>(0.019)    |
| Lag Personalism                  | −0.007<br>(0.006)          | −0.008<br>(0.007)   | 0.002<br>(0.008)   | −0.016<br>(0.010)   |
| Decade FE                        | Yes                        | Yes                 | Yes                | Yes                 |
| Country FE                       | No                         | No                  | No                 | Yes                 |
| Observations                     | 2,619                      | 2,150               | 2,147              | 2,147               |

Notes. Regressions include controls for FDI, Log of GDPPC, Natural resource rent, Log of population, Leader duration, and Polity IV Combined Score. \*p<0.1 \*\*p<0.05 \*\*\*p<0.01.

C    Testing Alternative Channels

In this section we present tables that test alternative channels that may be driving the treatment heterogeneity we find in our results. Section 4.2 explains how these tables help to rule out alternative explanations of our results.

Table 12: Treatment Effects on Expropriation (Multiple Interactions)

|  | <i>Dependent variable:</i> |                     |                     |                     |
|--|----------------------------|---------------------|---------------------|---------------------|
|  | Expropriation (Binary)     |                     |                     |                     |
|  | (1)                        | (2)                 | (3)                 | (4)                 |
| Transition Year                          | 0.383***<br>(0.140)        | 0.036<br>(0.034)    | 0.075<br>(0.051)    | 0.132<br>(0.090)    |
| Pred Pers                                | −0.001<br>(0.007)          | −0.001<br>(0.007)   | −0.001<br>(0.006)   | 0.014<br>(0.013)    |
| Transition Year X Pred Pers              | 0.146***<br>(0.031)        | 0.159***<br>(0.031) | 0.178***<br>(0.030) | 0.199***<br>(0.057) |
| Transition Year x Natural Resource rents |                            | 0.002<br>(0.005)    |                     | 0.002<br>(0.011)    |
| Transition Year x Polity IV              |                            |                     | −0.039<br>(0.070)   | −0.005<br>(0.126)   |
| Transition Year x Monarch                |                            |                     | 0.053<br>(0.066)    | −0.009<br>(0.102)   |
| Transition Year x Military               |                            |                     | −0.137**<br>(0.062) | −0.168<br>(0.113)   |
| Transition Year x One Party              |                            |                     | −0.065<br>(0.069)   | −0.080<br>(0.071)   |
| Transition Year x Multiple Parties       |                            |                     | −0.047**<br>(0.019) | 0.054*<br>(0.031)   |
| Transition Year x Log GDP                |                            |                     | 0.007<br>(0.012)    | 0.015<br>(0.032)    |
| Monarch                                  |                            |                     | −0.032**<br>(0.015) | −0.022<br>(0.028)   |
| Military                                 |                            |                     | −0.026**<br>(0.013) | −0.044**<br>(0.021) |
| One Party                                |                            | 0.002<br>(0.001)    |                     | 0.004<br>(0.003)    |
| Decade FE                                | Yes                        | Yes                 | Yes                 | Yes                 |
| Country FE                               | No                         | No                  | No                  | Yes                 |
| Observations                             | 2,150                      | 2,147               | 2,047               | 2,044               |

Note: \*p<0.1; \*\*p<0.05; \*\*\*p<0.01



Table 13: Treatment Effects on Expropriation (Including Ideology)

|                             | <i>Dependent variable:</i> |                    |
|-----------------------------|----------------------------|--------------------|
|                             | Expropriation (Binary)     |                    |
|                             | (1)                        | (2)                |
| Transition Year             | 0.027<br>(0.025)           | 0.020<br>(0.027)   |
| Pred Pers                   | 0.004<br>(0.006)           | 0.007<br>(0.009)   |
| Transition Year X Pred Pers | 0.143***<br>(0.027)        | 0.133**<br>(0.055) |
| Left Executive              | 0.011<br>(0.010)           | 0.032<br>(0.028)   |
| Transition X Left Executive | −0.027<br>(0.049)          | −0.032<br>(0.043)  |
| Lag Personalism             | 0.003<br>(0.005)           | −0.003<br>(0.012)  |
| Decade FE                   | Yes                        | Yes                |
| Country FE                  | No                         | Yes                |
| Observations                | 1,937                      | 1,937              |

Notes. Regressions include controls for FDI, Log of GDPPC, Natural resource rent, Log of population, Leader duration, and Polity IV Combined Score. \*p<0.1 \*\*p<0.05 \*\*\*p<0.01.

D Results from Imputation

Table 14: Treatment Effects on Expropriation (Using MICE)

|                             | <i>Dependent variable:</i> |                     |                     |                   |
|-----------------------------|----------------------------|---------------------|---------------------|-------------------|
|                             | Expropriation (Binary)     |                     |                     |                   |
|                             | (1)                        | (2)                 | (3)                 | (4)               |
| Transition Year             | 0.011<br>(0.019)           | 0.009<br>(0.019)    | −0.010<br>(0.019)   | 0.007<br>(0.021)  |
| Pred Pers                   | 0.004<br>(0.005)           | 0.002<br>(0.005)    | 0.0004<br>(0.005)   | 0.003<br>(0.007)  |
| Transition Year X Pred Pers | 0.070***<br>(0.022)        | 0.072***<br>(0.022) | 0.066***<br>(0.022) | 0.069*<br>(0.037) |
| Lag Personalism             | −0.003<br>(0.004)          | −0.004<br>(0.004)   | 0.007<br>(0.005)    | −0.004<br>(0.010) |
| Decade FE                   | Yes                        | Yes                 | Yes                 | Yes               |
| Country FE                  | No                         | No                  | No                  | Yes               |
| Observations                | 4,457                      | 4,457               | 4,457               | 4,457             |

Notes. Regressions include controls for FDI, Log of GDPPC, Natural resource rent, Log of population, Leader duration, and Polity IV Combined Score. \*p<0.1 \*\*p<0.05 \*\*\*p<0.01.

Table 15: Heterogenous Treatment Effects on Expropriation (Using MICE)

|                                  | <i>Dependent variable:</i> |                   |                   |                   |
|----------------------------------|----------------------------|-------------------|-------------------|-------------------|
|                                  | Expropriation (Binary)     |                   |                   |                   |
|                                  | (1)                        | (2)               | (3)               | (4)               |
| Transition Year x High Pred Pers | 0.049*<br>(0.028)          | 0.047*<br>(0.027) | 0.026<br>(0.028)  | 0.059<br>(0.042)  |
| Transition Year x Low Pred Pers  | −0.017<br>(0.025)          | −0.023<br>(0.025) | −0.039<br>(0.025) | −0.014<br>(0.019) |
| High Pred Pers                   | 0.010<br>(0.008)           | 0.008<br>(0.008)  | 0.004<br>(0.008)  | 0.003<br>(0.014)  |
| Lag Personalism                  | −0.002<br>(0.004)          | −0.004<br>(0.004) | 0.007<br>(0.005)  | −0.008<br>(0.009) |
| Decade FE                        | Yes                        | Yes               | Yes               | Yes               |
| Country FE                       | No                         | No                | No                | Yes               |
| Observations                     | 4,457                      | 4,457             | 4,457             | 4,457             |

Notes. Regressions include controls for FDI, Log of GDPPC, Natural resource rent, Log of population, Leader duration, and Polity IV Combined Score. \*p<0.1 \*\*p<0.05 \*\*\*p<0.01.

E Alternative Specifications

Table 16 and Table 17 give the results from a logistic model. The results are presented in log odds ratios, but the sign and significance are consistent with the above. Note that in model 4 we have not clustered the standard errors at the country level.

Table 16: Treatment Effects on Expropriation (Using Logit Model)

|                             | <i>Dependent variable:</i> |                     |                     |                    |
|-----------------------------|----------------------------|---------------------|---------------------|--------------------|
|                             | Expropriation (Binary)     |                     |                     |                    |
|                             | (1)                        | (2)                 | (3)                 | (4)                |
| Transition Year             | −0.300<br>(0.526)          | −0.584<br>(0.683)   | −0.947<br>(0.687)   | 0.044<br>(0.767)   |
| Pred Pers                   | 0.019<br>(0.125)           | −0.012<br>(0.147)   | −0.036<br>(0.151)   | 0.501*<br>(0.267)  |
| Transition Year X Pred Pers | 2.013***<br>(0.533)        | 2.443***<br>(0.617) | 2.210***<br>(0.614) | 1.752**<br>(0.743) |
| Lag Personalism             | −0.179<br>(0.111)          | −0.165<br>(0.126)   | 0.094<br>(0.144)    | −0.237<br>(0.234)  |
| Decade FE                   | Yes                        | Yes                 | Yes                 | Yes                |
| Country FE                  | No                         | No                  | No                  | Yes                |
| Observations                | 2,751                      | 2,150               | 2,147               | 2,147              |
| Akaike Inf. Crit.           | 1,038.639                  | 799.898             | 787.409             | 770.588            |

Notes. Regressions include controls for FDI, Log of GDPPC, Natural resource rent, Log of population, Leader duration, and Polity IV Combined Score. \*p<0.1 \*\*p<0.05 \*\*\*p<0.01.

Table 17: Heterogenous Treatment Effects on Expropriation (Using Logit Model)

|                                  | <i>Dependent variable:</i> |                     |                   |                    |
|----------------------------------|----------------------------|---------------------|-------------------|--------------------|
|                                  | Expropriation (Binary)     |                     |                   |                    |
|                                  | (1)                        | (2)                 | (3)               | (4)                |
| Transition Year x High Pred Pers | 1.059**<br>(0.447)         | 1.420***<br>(0.503) | 0.873*<br>(0.516) | 1.346**<br>(0.663) |
| Transition Year x Low Pred Pers  | −0.354<br>(0.616)          | −0.579<br>(0.765)   | −0.930<br>(0.769) | 0.187<br>(0.864)   |
| High Pred Pers                   | 0.261<br>(0.207)           | 0.118<br>(0.248)    | 0.038<br>(0.253)  | 0.632<br>(0.532)   |
| Lag Personalism                  | −0.184*<br>(0.107)         | −0.153<br>(0.122)   | 0.120<br>(0.139)  | −0.180<br>(0.229)  |
| Decade FE                        | Yes                        | Yes                 | Yes               | Yes                |
| Country FE                       | No                         | No                  | No                | Yes                |
| Observations                     | 2,751                      | 2,150               | 2,147             | 2,147              |
| Akaike Inf. Crit.                | 1,048.865                  | 812.891             | 798.629           | 780.529            |

Notes. Regressions include controls for FDI, Log of GDPPC, Natural resource rent, Log of population, Leader duration, and Polity IV Combined Score. \*p<0.1 \*\*p<0.05 \*\*\*p<0.01.