Elites, Financial Networks, and Commitment in Dictatorships: Evidence from the Panama Papers*

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Abstract

A large literature argues that dictatorships can achieve high levels of economic growth if dictators can commit to not expropriate elites. Extant research has focused on the role of formal institutions—legislatures and parties—in helping elites constrain dictators' predation. I complement this literature by documenting the role of an informal institution, elite financial networks, in constraining the dictator. I argue that dense financial ties among elites diffuse private information on the state of the economy, hence facilitating elites' monitoring—if the dictator reneges on his commitments to elites, informed elites are able to infer and punish his defection. Accordingly, I hypothesize that dictatorships with denser elite financial networks enjoy stronger property rights. To test my argument, I uncover networks of elites' co-ownership of offshore companies—a strong type of financial tie—using a large, untapped leak of private financial information, the Panama Papers. A thorough regression analysis of almost all dictatorships in the period 2002–2013 supports my theory: a one standard-deviation increase in financial network density predicts a half standard-deviation decrease in expropriation risk.

Keywords: dictatorships, property rights, elites, financial networks, tax havens

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1 Introduction

Research Question The importance of secure property rights to economic growth is one of the few consensus facts in economics (Barro, 1989; Acemoglu, Johnson and Robinson, 2001). A related consensus fact is that property rights protection requires limited government (North and Weingast, 1989; North, 1990). Most theories of limited government view it as product of elites' efforts to punish predatory behavior by the sovereign. Thus, at the core of the literature on economic growth, property rights, and limited government lies the question: how do elites constrain dictators?

Literature Scholars of authoritarian regimes[1] have focused on the role of political institutions.[2] For example, Boix and Svolik (2013) argue that interaction within legislatures enables elite monitoring of the dictator, hence deterring him from defecting on rent-sharing pacts. Similarly, Gehlbach and Keefer (2012) claim that authoritarian parties increase the observability of expropriation against party supporters, thereby tying the dictator's hands. In short, the literature sees political institutions as an antidote to dictator-elite information asymmetries and a catalyst for elite coordination.

Puzzle However, formal political institutions are only one medium through which elites can acquire, share, and act on private information regarding the dictator's predation. As Boix and Svolik (2013) acknowledge, "several institutions may serve to reduce asymmetries of information between the ruler and his allies [...] less formal, idiosyncratic, or traditional institutions may perform this function" (p. 307). Informal institutions may matter most where formal ones are ineffective or controlled by the dictator; in "personalist" or "established" regimes (Geddes, 1999; Svolik, 2009).[3] Therefore, by studying elites' interaction within informal institutions we can advance our understanding of how elites constrain dictators.

Argument In this article, I explore the role of one informal institution, financial networks. I argue that the structure of elites' financial networks affects the diffusion of private information on the true size of the regime's rents. This information allows some elites to infer whether they receive low rents because the dictator reneges on his commitment to share rents or because of a negative economic shock. Informed elites do not have to threaten to punish the dictator whenever he delivers low rents—a threat that is non-credible, as elites incur a cost for reallocating their capital. Instead, informed elites can credibly threaten to punish the dictator *only* when rents are high but he does not share them. Crucially, when more elites learn the true state of the regime's rents—through their financial ties to other elites—the credible threat posed to the dictator increases, and he is further deterred from predation. Hence, I hypothesize that, in countries where elites' financial network enables larger diffusion of private information about the economy, economic predation by the dictator is lower.

[1] I use the terms "authoritarian regime", "dictatorship", "autocracy", and "non-democracy" interchangeably. The same holds for the terms "dictator" and "autocrat".

[2] See Pepinsky (2014) for a general critique of the institutionalist approach to the study of dictatorships.

[3] Crucially, Geddes, Wright and Frantz (2014) show that the share of all dictatorships that are personalist has been increasing near-steadily since 1950.

Data To test this hypothesis, I operationalize the diffusion of information within a network as the network's density: the number of observed ties divided by the number of possible ties (the latter is a function of the network's size). To measure network density, I tap into the largest public source of private financial information to date, the leaked Panama Papers (International Consortium of Investigative Journalists, 2016). This unique, previously unused data includes information on offshore companies belonging to tens of thousands of individuals from all countries during the period 1990-2015.[4] Since this information was leaked from a firm that charges large fees for its services and holds "special expertise in creating tax shelters for wealthy global elite", I assume that the individuals in its records are, indeed, economic elites (Graham, 2016). Thus, for each dictatorial country-year since 1990, I treat the names associated with country k in year t as its elites, and I code elites i and j from k, t as financially tied if they are both associated with one or more of the same offshore companies. Through this method, I construct the elite financial networks needed to measure network density for a large sample of dictatorships and years.

Empirics To test my hypothesis, I estimate the effect of network density on the price of insurance against expropriation. I obtain the latter from the annual country ratings of the leading political risk insurance agency. My sample includes 51 dictatorships in the period 2002–2013. My analysis shows a statistically significant and substantively strong association in the hypothesized direction: all else equal, a one standard-deviation increase in elite financial network density is associated with as much as a half standard-deviation decrease in expropriation risk—equivalent to the difference between capital-controlling China and the capital-friendly United Arab Emirates. Other variables mostly have an inconsistent or statistically insignificant effect. My main finding is robust to a barrage of controls, unobservable effects, temporal dynamics, and statistical irregularities, and to using alternative dependent variables, independent variables, and rules for coding the underlying networks.

Contributions This study contributes to several literatures. The first literature is that from political economy and economic history, on the emergence of property rights and contract enforcement vis-à-vis limited government (North and Weingast, 1989; North, 1990). The economic interdependence of elites and sovereign has featured heavily in that literature; I expose how elites' economic interdependence affects their interaction with the sovereign. A second literature this study builds on is that from comparative politics, on power-sharing and elite politics in dictatorships (Bueno De Mesquita et al., 2005; Gandhi, 2008; Svolik, 2012; Geddes, Wright and Frantz, Forthcoming). Though elites' bargains with dictators have been thoroughly examined—along with the role of private information in those bargains—this study explores how networks condition the effect of private information on elites' position. A third literature related to this article is the international political economy literature on foreign direct investment and expropriation (Jensen, 2008; Jensen, Malesky and Weymouth, 2014). That literature looks at how political institutions—dictatorial or democratic—protect investors from economic predation. Instead, I focus on a non-institutional safeguard against

[4]To my knowledge, only one other research paper has used this information to date, to study the effects of offshoring on firm valuation (O'Donovan, Wagner and Zeume, 2016).

predation, financial networks. A related body of work is the literature on *de facto* property rights that arise through the action of guilds, business associations, and supply chains (Greif, Milgrom and Weingast, 1994; Doner and Schneider, 2000; Johns and Wellhausen, 2016). These studies take the role of informal institutions in creating property rights seriously, but they do not examine networks.[5] Methodologically, my approach belongs to a large literature in sociology, political science, and economics, which studies the effects of various types of networks on individual-level and network-level outcomes (see Jackson (2008) for a review). In terms of subject matter, a relevant literature is that on the causes and effects of tax haven and shell company usage (Johannesen and Zucman, 2014; Findley, Nielson and Sharman, 2014). Finally, from a policy perspective my findings suggest that, though dense financial ties between elites have negative normative connotations, they counter a more negative force, economic predation by the dictator.

Roadmap The remainder of this study proceeds as follows. Section 2 briefly reviews the literatures relevant to my research question. Section 3 lays out my argument in the form of a model sketch and derives its main empirical implication. Section 4 introduces my financial network data. Section 5 presents my empirical approach, the rest of my data, the results of my analysis, and robustness checks. Section 6 discusses alternative interpretations of my findings and limitations of my analysis. Section 7 summarizes and points to directions for future research.

2 Literature

A number of literatures relate to this study: the literature on property rights, contract enforcement, and limited government; the literature on foreign investment and expropriation; the literature on business associations and lobbies; the literature on power-sharing and elite politics in dictatorships.

Common Structure Reviewing these literatures is a gargantuan task. Fortunately, all share a common theoretical structure: an agent in a position of authority, A, interacts with a group of agents in a position of lesser authority, B, in a sometimes competitive, sometimes co-operative manner. A is usually the state, government, ruler, elected leader, or dictator, and members of B are elites, domestic or international firms, or investors. When cooperative, their relationship involves A protecting B's property rights, committing to announced policies, or sharing rents and power. A cooperative relationship also involves B supporting A's rule, by lending capital to A, not aiding coup/rebellions against A, or not defecting to a challenger. When their relationship becomes competitive, A and B engage in the opposite actions from the above.

Examples For example, the property rights theory of North and Weingast (1989) features the English crown and the landed gentry. The crown first expropriates the gentry, then commits to protecting its property—by increasing the powers of parliament and judiciary—in order to secure loans. In Svolik (2009) and Boix and Svolik (2013), the dictator interacts

[5] One exception is Razo (2009) and related pieces.

with a coalition of allies; the dictator shares or withholds rents from allies, and allies support or rebel against the dictator. Similarly, in Guriev and Sonin (2009), the players are a ruler and oligarchs, with the former having the power to expropriate the latter and the latter having the power to replace the former. Examining a very different setting, Johns and Wellhausen (2016) study the interaction of a government with foreign firms, where the government can honor or break contracts, and firms invest in protecting themselves from expropriation. A number of other studies from international political economy adopt a similar theoretical framework (Jensen, 2008; Jensen, Malesky and Weymouth, 2014; Wilson and Wright, 2017; Graham, Johnston and Kingsley, Forthcoming).

Intervening Factor Within the above unifying framework, there is often a role for an intervening force that conditions the relationship between A and B: for example, an economic shock, conflict, or technological change. However, this force might also operate through changing the way actors *within* B interact, which, in turn, affects B's interaction with A. A prominent example in the literature is political institutions. In Boix and Svolik (2013), institutionalized power-sharing allows allies in the dictator's coalition to monitor his compliance with their rent-sharing pact. Similarly, in Gehlbach and Keefer (2012), the creation of a ruling party enables the dictator's supporters to observe expropriations against party members. In both theories, institutions facilitate coordination among the actors in B to change the behavior of A—in a welfare-improving manner. However, institutions are only one among a myriad of factors that can alter the interaction of the actors in B.

Networks An understudied factor that affects the way firms, investors, or elites interact are *networks*—the ties that bind these actors. A large literature in sociology and economics shows that networks impact how rational actors coordinate joint actions, commit to reciprocate in certain actions, and build social capital (Jackson, 2008). Actors can have different ties (e.g. ethnicity, religion, nationality, education), both cooperative and competitive. For firms, investors, and elites, arguably the most important are *financial ties*—after all, these are primarily economic actors. A key function that financial ties serve is to diffuse valuable private information on the economy. This is particularly the case in dictatorships, where policy-making is more arbitrary and political connections especially valuable (Fisman, 2001). As such, one way to advance our understanding of how firms, investors, or elites coordinate with each other in interacting with dictators, is to study financial networks among these actors.

3 Theory

Elites I focus on the network of actors most likely to hold, share, and act on private information about the economy, elites. Elites are also the group most likely to benefit from constraining the dictator—they are the main target of predation.

Setup I present my argument in the form of a sketch model; an infinitely repeated game between a dictator and a set of N elites.[6] Elites form a given financial network—a set of

[6]I borrow some non-network-related elements from the formal models in Svolik (2009) and Boix and Svolik

bilateral financial ties—that determine the communication of information (more below). In the initial time period, dictator and elites form an agreement to share a fraction $0 < \beta < 1$ of total rents from their joint rule in each subsequent period. In all subsequent periods, the following sequence of plays takes place. First, the total value of rents, r, is determined by stochastic economic forces: $r = 1$ with probability π (good times), and $r = 0$ with probability $1 - \pi$ (bad times). Crucially, only the dictator observes r, along with a randomly chosen elite—label her i.[7] In the next step, informed elite i decides to which uninformed elites, if any, to reveal r (costlessly). Then, the dictator chooses how much to share with each elite (including i): β/N or 0. Subsequently, elites observe their share of rents and decide whether or not to punish the dictator.

Punishment & Cost Punishment can take the form of capital flight or any type of capital reallocation that might hurt the economy and, hence, create negative externalities for the dictator.[8] However, elites incur a cost c for reallocating their capital.[9] Therefore, elites prefer not to punish the dictator unless the expected gain from punishment—the rents they will earn discounted by the probability that the dictator will award them rents due to the threat of punishment—exceeds c. Unfortunately, for uninformed elites that probability is zero because the dictator knows that their threat is non-credible.

Predation Equilibrium To see how the dictator preys on uninformed elites, ignore informed elite i for now. Because uninformed elites do not observe the true size of rents, they can only adopt one strategy to deter the dictator from predation: punish him whenever they receive no rents. However, to execute this strategy uninformed elites have to punish the dictator *even when he has not reneged on their pact and times are simply bad.* The dictator knows this, and so the strategy contains a non-credible threat—uninformed elites are better off not punishing the dictator and foregoing cost c.[10] Thus, in equilibrium, the dictator is not deterred from predation, keeps all rents to himself (when there are any), and uninformed elites always get 0. In short, *when there is a complete information asymmetry between dictator and elites regarding the true size of rents (i.e. all elites are uninformed), the unique equilibrium involves complete predation.*[11]

Information The presence of informed elite i alters this equilibrium. Because i can condition her response on the economy's state, she avoids costly, unnecessary punishments—when times are bad i attributes low rents to stochastic forces and does not blame the dictator. Conversely, when times are good yet she receives low rents, i infers defection and seeks to punish the dictator. Thus, i's threat to punish the dictator if he defects is credible and

(2013).

[7] The theory's predictions are substantively similar if we allow more than one elite to observe r.

[8] Most theories of authoritarian survival argue that a strong economy bolsters the dictator's tenure. This effect can operate through rent-sharing with elites and the military or distributive politics to alleviate popular threats like revolutions and civil conflict.

[9] I think of c as the sum of brokerage fees, capital gains taxes, and regulatory compliance costs elites incur for reallocating capital, especially if moving it abroad.

[10] That is, uninformed elites prefer 0 (no rents) to $-c$ (no rents and costly punishment).

[11] This is similar to the no-power-sharing equilibrium in Boix and Svolik (2013).

incentivizes the dictator to share the agreed-upon rents with i (β/N). In other words, i's private information and the threat she conditions on that information secures her a *selective commitment* by the dictator (Haber, Maurer and Razo, 2003; Razo, 2009).[12] For the dictator to make additional selective commitments, though, more elites need to join i in making credible threats. This requires that i communicates her private information about the economy's true state, enabling additional elites to infer and punish defection.

Communication How does communication between elites occur? Recall that, after observing the true size of rents r, i can costlessly reveal r to other elites. However, i's incentive to *truthfully* reveal r to another elite will depend on their financial relationship—whether they are competitors or partners. Truthful communication between competitors is not possible. In particular, i has an incentive to lie to a competitor when times are bad and claim that the dictator defected; the competitor will then reallocate her capital, thinking that she is punishing the dictator, which can hurt her and benefit i. On the contrary, i has no incentive to lie to a financial partner, say j, when times are bad. If i lies, j's capital reallocation can adversely impact i; for example, capital might be moved out of a joint venture.[13] Thus, I limit my attention to communication between informed elites and their financial partners (if any).[14]

Network Structure Communication between informed elites and their partners depends on the financial network's structure: how many partners i has, how many partners they have, and so on and so forth. To illustrate the importance of network structure, I analyze two toy examples, depicted in Figure 1. Both networks have 7 elites, but vary significantly in how interconnected elites are.[15] The left network has 5 elites with no ties and 2 elites that are only tied to each other, while in the right network every elite is tied to at least 3 others. Given my assumptions about communication between elites, in the left network 1 or 2 elites learn the true size of rents, versus 4 or 7 elites in the right network (depending on which elite is endowed with the private information). Recalling that, in every period, one elite i randomly observes the true size of total rents r, we can derive the *average* number of elites to which i communicates r. This statistic is simply the sum of each elite's probability

[12] A richer implication would involve the dictator sharing rents equal to the punishment i can inflict on him—a form of "rent discrimination" by the dictator towards elites. In this manner, informed elites would be able to extract larger rent shares from the dictator the more capital they have. This feature can be incorporated without changing the core of the model but would needlessly complicate the analysis.

[13] In addition, if i lies, j will soon infer the lie, when i fails to punish the dictator herself. Note that, even though j will only observe i's lie in the next time period, j might punish i then, by terminating their financial tie—a tie that i presumably derives value from. And because financial partners play a repeated game, i should be deterred from lying to j. Another reason, though more difficult to insert in a rational choice model, might be trust between i and j. Indeed, much sociological research records higher levels of trust between connected nodes in different kinds of social networks (Cook and Hardin, 2001).

[14] An altogether different motivation for the assumption that only financial partners can communicate is informational: elites that are not fincancially tied might not even be aware of each other's existence. This assumption is more realistic for large, populous, and/or ethnically diverse countries, where there are many, heterogenous elites. Also, note that if i has no ties, the unique equilibrium is the same as with no communication: complete predation.

[15] Throughout, I model and measure ties as undirectional; that is, for any i and j, if i is tied to j, then j is tied to i.

of acquiring information $(1/N)$ times the number of elites she can communicate that information to (i.e. her number of ties). This equals $5 * \frac{1}{7} * 1 + 2 * \frac{1}{7} * 2 = \frac{9}{7} = 1.28$ elites in the left network and $6 * \frac{1}{7} * 4 + 1 * \frac{1}{7} * 7 = \frac{31}{7} = 4.43$ elites in the right network. This means that, on average, more than 3 additional elites learn r in the right network, and will infer and threaten to punish any defection by the dictator, thus securing selective rent-sharing commitments from the dictator.[16]

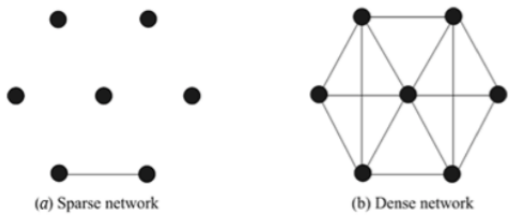

Figure 1: Two examples of elite networks

Network Density A well-known and readily operationalizable variable that reflects how network structure affects information diffusion is network density. Indeed, a large literature in sociology and economics links network density to important behaviors like cooperation, exchange, and conflict (see Jackson (2008) for a partial review). For an undirected network k in period t, density is calculated as $d_{k,t} = \frac{\sum_i^{N_{k,t}} x_j}{N_{k,t}(N_{k,t}-1)}$, where x_j denotes the number of financial ties of elite j and $N_{k,t}$ the number of elites in network k in period t (Kolaczyk, 2009).

Hypothesis Coupled with the concept of network density, the above stylized example brings us to the core of my theory and its key prediction: *where elites' financial network is more densely interconnected, the dictator is less likely to expropriate elites.* A dense financial network diffuses information about the true size of the regime's rents more widely, allowing more elites to infer whether the dictator reneged on his rent-sharing agreement and threaten to punish him. Credible threats of punishment by more elites, in turn, should act as a larger deterrent against predation, thereby producing selective commitments to protect more elites' property. In short, I hypothesize that, all else equal, *dictatorships with dense financial elite*

[16] A richer version of my sketch model would feature the dictator sharing all of β with only those elites that secure selective commitments from him; say, n elites, where $n < N$. Under this modification, an informed elite might have an incentive to withhold information from her uninformed connections, in order to keep n low and reap a larger β/n from the dictator (along with other informed elites that secure commitments). However, to dispel this incentive we must simply assume that an informed elite gains more from sharing information with a financial partner—through the various channels outlined above—than she does by increasing her share of rents from $\beta/(n+1)$ to β/n, especially when n is large, i.e. there are many informed elites. Moreover, we might imagine that informed elites face a public goods provision problem: every informed elite has an incentive to share information with her connections, thereby gaining from aligning her strategy with her financial partners, while free-riding on other informed elites' efforts to keep n low by withhold information from their own partners. This would create an equilibrium whereby no informed elite withholds information from her partners.

networks enjoy stronger property rights.

Selective vs Collective Commitments Before proceeding to the study's empirical component, I note an important distinction between my theory and the literature. Most theories of property rights formation in dictatorships focus on elites' collective action problem in constraining the dictator (North and Weingast, 1989; Svolik, 2009; Boix and Svolik, 2013). A common feature of these theories is that elites – the group of agents attempting cooperation – secure a *collective* commitment by the dictator to protect their property. Though collective commitments are undoubtedly a feature of dictator-elite interactions, so are *selective* commitments (Razo, 2009). Indeed, since authoritarian politics are more discretionary and relation-based than democratic politics, especially in less institutionalized regimes, the literature's near-exclusive focus on collective commitments seems unwarranted. Moreover, the study of selective commitment seems like a natural progression, given the literature's recognition of the fragility of formal institutions in dictatorships and the shifting focus to the study of informal institutions (Keller, 2014). In short, in terms of the literature, my theory is situated as one about how an informal institution enables elites to secure selective commitments from the dictator.

4 Data: Elites & Financial Networks

Network Data Unlike other independent variables used in statistical analyses, we cannot obtain network density from existing data; we must calculate it. To do so for a sample of K countries in T years, we first need to construct an equal number of networks of elites' financial ties. Collecting network data on elites' financial ties is far from straightforward, especially in dictatorships. It requires defining who the elites are and recording every elite's ties to every other elite. For a large country like China, this could mean hundreds of thousands of elites (nodes) and millions of potential financial ties (edges). One approach would be to use primary and secondary sources that identify economic elites and their financial ties in a sample of dictatorship-years. However, this would introduce bias, stemming from the disproportionate media coverage of prominent elites. Less prominent elites and ties between them are more likely to be omitted from news reports, which would result in networks that are smaller (fewer elites) and, possibly, sparser (fewer ties per elite) than reality. In addition, if we want to compare networks across countries and years, we need data on elites' financial ties that is recorded in the same way for all units. To see why this is crucial, imagine that elites' financial networks are identical in two dictatorships, k and k', yet k receives more coverage than k' due to its alliance with the US. In addition, imagine that the US's alliance with k also causes its dictator to expropriate less. The above data-collection approach will produce a network that is denser for k than k', which will lead us to spuriously attribute k's lower expropriation to its higher network density instead of its alliance to the US. Clearly, traditional techniques of network data-collection—observing or surveying the network's nodes (elites)—are of no use here (Wasserman and Faust, 1994). Equally blunt are the methods used to study other networks of political actors, such as co-sponsorship

networks in Congress (Fowler, 2006).[17]

Panama Papers To resolve obstacles in network data-collection, I exploit a new, unique, and untapped source of information on hidden financial ties. Constituting the largest leak of private financial information to date—11.5 million files adding up to 2.6TB of data—the so-called Panama Papers are the full body of documents of one of the largest provider of offshore legal services, Mossack Fonseca (Mossfon) (International Consortium of Investigative Journalists, 2016). These documents were leaked by an anonymous source to journalists in 2015. After more than a year of preparatory work by a team of 400 individuals from 100 news organizations in 80 countries, a reduced version of the documents were made available for public download.[18] In the words of the team behind the leak: "the real value of the database is that it strips away the secrecy that cloaks companies and trusts incorporated in tax havens and exposes the people behind them" (International Consortium of Investigative Journalists, 2016). This is the data I use to construct elites' financial networks in all dictatorships since 1990.[19]

Elites To use this data to record elites' financial ties, we must assume Mossfon's clients are elites. According to reports on the market for offshore services, Mossfon has "special expertise in creating tax shelters for the *wealthy global elite* [emphasis added]" (Graham, 2016). This is evident in the company's reported fees: depending on the service required, charges range from $\$1,500$ per year for setting up an offshore company in a not-so-costly jurisdiction to $\$17,500$ per year for providing a nominee director that acts on the beneficiary's behalf (Harding, 2016*a*). Though the data does not indicate which services were purchased by each elite, some of Mossfon's fees exceed the median per capita income of the vast majority of post-1990 dictatorships.[20] In other words, merely purchasing Mossfon's services is an expense that only the very wealth in dictatorships can incur.[21] Another piece of evidence on the elite

[17] Mahdavi (2016) proposes a method to construct affiliation networks of political elites via joint appearances in gala events, while Mahdavi and Ishiyama (2016) construct an affiliation network of N. Korean party elites via joint appearances in state-sponsored factory inspections. However, these methods are not scalable to a large number of countries.

[18] See https://offshoreleaks.icij.org/pages/about for further information on the data. The vast majority of Mossfon's leaked documents are excluded from the public dataset—email exchanges, bank account numbers, and financial transactions. However, for the purposes of this study, the excluded information is not necessary to record elites' financial ties (see Financial Ties paragraph). The public version of the data includes all clients' information on: their nationality, address, the companies they are associated with and their role in those companies, intermediaries used to establish the account (if any), and various dates relevant to the companies' status. In some cases, some of this information is missing, though there is no obvious pattern to this missingness.

[19] In 11/2017, a related dataset, called the Paradise Papers, was leaked to the same media that made the Panama Papers publicly available. The Paradise Papers consist of files leaked from Appleby, a competitor of Mossfon. The Paradise Papers' size is 1.4TB of data, a little more than half that of the Panama Papers. Though the Paradise Papers have not been made publicy available as of 11/14/2017, media coverage suggests that Appleby's clients are mostly multinational corporations and wealthy Westerners. As such, it is unlikely that the Paradise Papers will illuminate financial ties among elites in dictatorships to the extent that the Panama Papers do.

[20] Based on author's calculations using data from the Quality of Government (QoG) dataset (Teorell et al., 2011).

[21] A simple back-of-the-envelope calculation adds further weight to my assumption—particularly for poorer

status of Mossfon clients is offered by Alstadsæter, Johannesen and Zucman (2017*a*), who are able to match Norwegian and Swedish account holders to their tax returns. The authors find that only the top 0.1% household earners from two of the world's richest countries own Mossfon accounts. Thus, I safely assume that Mossfon's clients are, indeed, economic elites, and I use the data leaked from Mossfon to record elites' financial ties. Note a direct benefit of using the Panama Papers to identify elites: we do not have to identify elites individually, for a large number of countries. Their wealth, as implicitly revealed through purchasing Mossfon's services, renders them elites, and no researcher discretion is needed to classify them as such.[22]

Financial Ties To lend itself to testing my hypothesis, the Panama Papers must also measure elites' financial *ties*; that is, meaningful and cooperative financial relations. The data includes information on the companies associated with each client—the companies' names, jurisdictions (which countries they are registered in), incorporation dates, and inactivation dates (if applicable)—and clients' positions in those companies (e.g. director, beneficiary, shareholder, secretary, etc).[23] I use this information to record financial ties between clients (elites). Namely, I code elites i and j from country k[24] as tied in year t if they are both associated with the same company, m, and m is active in t.[25] Given that clients associated with a company have veto power over its activity, I assume that i and j have a strong, cooperative financial tie if they both have a substantive position in the same company.[26]

dictatorships. Assume there is a 1% probability that, in any year, client i will lose a particular asset—through expropriation, imprisonment, etc—if she does not register it in an offshore company. Further assume that the annual fee Mossfon charges for incorporating i's asset in offshore company is $\$2,000$. This implies that, for i to be making a rational decision in seeking Mossfon's service, her asset must be worth at least $\$200,000$. And, given that i is highly unlikely to have a single asset and no source of income, i's net worth must arguably be in the top percentile of the distribution in most post-1990 dictatorships.

[22]Due to the desire of Mossfon's clients' for anonymity, in a large fraction of companies it is not possible to discern the ultimate beneficiaries. Anonymity is usually achieved through the use of bearer shares, which award beneficiary status to their physical holder. Accounts that use bearer shares show "The Bearer"—or some variation of that title—as the account name. I do not use information from such accounts in constructing my networks, but there does not seem to be a pattern in the use of bearer shares.

[23]Though a key feature of offshore companies is their jurisdiction, it does not play a role in constructing my elite networks. I code elites' ties based on their own nationality or country of residence, not the country where their companies are registered. In this manner, elites i and j that are nationals of country k will appear in k's network, independent of where their companies are registered.

[24]Note that there can be other elites that are associated with company m but are not nationals of country k—indeed, this occurs frequently in the data. Though, these elites are financially tied to elites i and j—via their joint association with company m—I do not include them in the network for country k, but in the network for their own country.

[25]For a company to be active in year t, its incorporation date must precede t and its inactivation date (if applicable) must be later than t. Also, note that two elites can be tied through more than one company. I plan to account for this feature of the data in future versions of the analysis, by allowing the strength of elites' ties to vary: the more companies elites i and j are jointly associated with, the stronger their tie.

[26]There is a large number of positions a client can have in an offshore company, and some positions do not grant real power over the company. For example, if a name appears as the company's "nominee director" (or a variation of that title), that name is not the true beneficiary of the company, but usually an employee of Mossfon that acts on the beneficiary's account. As such, there is no reason to code that name as an elite and code its ties to other elites. To ensure that I record only substantive ties between Mossfon's clients (elites), I researched the legal powers of every position, and I retain only the positions that grant real powers over companies.

This allows me to construct a financial network through elites' joint association with one or more offshore companies. Again, note that no researcher discretion is needed to code elites' financial ties—they are revealed to us through elites' own actions, and these actions are recorded by an agent with no incentive to misreport them (Mossfon). Another advantage of this approach is that elites' ties are recorded in a uniform way across countries and years, because a single agent records these ties (Mossfon), using the same standard across countries.

Ties Example The following is an example of a typical entry in the raw data: New Russia Venture Partners Inc is an entity incorporated in the British Virgin Islands on 5/23/2003, and whose "officers" (i.e. the individuals associated the company) are Russian nationals Anna Baskakova, Igor Kubanov, and Sergey Vykhodtsev. I code these names as three of the nodes (elites) in the Russia 2003 network, and I add 3 edges (financial ties) between them. To complete the Russia 2003 network, I repeat this process for all other Russian nationals in the data that are associated with companies incorporated in 2003 or earlier. To complete the Russian network for my whole period of study, I repeat this process for all other years during 1990-2015 that Russia was a dictatorship. The same process is applied for all other dictatorship-year observations.[27]

Networks Example Figure 2 shows the financial networks for two countries in the year 2015, Chad (left) and Botswana (right). Note the richer structure of Botswana's network. At the graph's 9 o'clock position there is a very large clique—a group of nodes that are all tied to each other directly—formed by a single company of many officers. Also, in the graph's center there is a very large component—a group of nodes that are all tied (in)directly—formed by several companies with overlapping members. On the contrary, roughly 80% of Chad's elites have no ties, while connected elites only have 1 or 3 ties. These differences are reflected in a range of statistics, aside from the network size (48 nodes in Chad vs. 96 in Botswana). Most relevant to testing my hypothesis is their difference in density: Chad's elite network has a density of 0.008, a tenth of the respective density of Botswana (0.080).[28] Moreover, density is associated with property rights protection in Chad and Bostwana as predicted by my theory—Botswana is hailed as an institutional miracle, while Chad's property rights are among the world's worst (Acemoglu, Johnson and Robinson, 2002).

Mossfon Coverage How encompassing are the offshore networks in the Panama Papers? Obtaining estimates of Mossfon's market share in offshore legal services is difficult, due to the obscure nature of that market. The only available estimates place the firm's share at $5-10\%$ (The Economist, 2016*b*), with one report labeling Mossfon the "industry leader" (The Economist, 2012), and others calling it the "fourth largest" provider (Harding, 2016*b*). Clearly, the Panama Papers by no means capture the universe of elites' offshore financial ties—there are elites that do not use Mossfon for their offshore financial activities, as there are elites that do use Mossfon but might have other offshore financial ties that are not es-

[27] I discuss the use of alternative coding rules for constructing my elite networks in Section 5.3.

[28] The difference between the two countries becomes much larger if we allow for indirect diffusion of information, due to the large component in the Botswanan network. That is, if elite i can communicate with j via their mutual tie with l, whenever an elite within the component acquires private information, it will diffuse to the whole component, which constitutes half the Botswanan network.

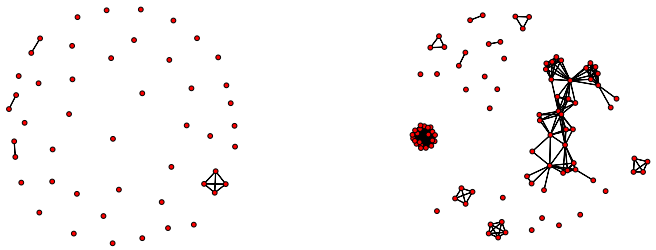

Figure 2: Elite financial network in Chad 2015 (left) vs. Botswana 2015 (right)

NOTES: Each node represents an elite; each edge represents at least one financial tie between the corresponding nodes. See text for data methodology.

tablished through Mossfon. That said, the data includes information on roughly 136,000 individuals and 310,000 offshore companies from all 101 countries that had one or more dictatorial spells during the period 1990-2015.[29] Moreover, there is no reason to expect that the pattern of elites' non-Mossfon-established offshore ties is systematically different from that appearing in the Panama Papers.

Offshore vs Onshore Ties Another concern with using the Panama Papers to capture financial ties between elites is whether offshore ties are meaningful. One might argue that *onshore* ties are more important, since most individuals hold a larger fraction of their assets onshore and onshore ties are more transparent.[30] In the case of elites in dictatorships, this argument is problematic for three reasons. First, elites in dictatorships most likely hold the majority of their wealth offshore. Using estimates from Alstadsæter, Johannesen and Zucman (2017*b*), I find that the average country in my sample holds around 17% of its GDP offshore, with that figure reaching over 50% for Russia, Saudi Arabia, Venezuela, and the UAE. And given the progressive nature of taxation in most countries coupled with the transaction costs of holding assets offshore, we can assume that elites in these countries hold a higher percentage of their wealth offshore than the average income-earner. Indeed, simple calculations show that even in countries with a relatively low share of their GDP offshore,

[29] These statistics are produced after cleaning the original data to include only individuals and their offshore companies from countries that had dictatorial spells during the period 1990-2015. The original data includes many more individuals and their companies from democracies and/or from earlier years, as well as many observations that cannot be attributed to specific countries. I use an extended version of the Geddes-Wright-Frantz data to identify which countries are dictatorships during 1990-2015 (Geddes, Wright and Frantz, 2014).

[30] I use "onshore ties" to refer to shared financial interests between elites in one or more assets that are located in elites' country of residence. I use "offshore ties" to refer to shared financial interests in assets that are either (i) located in a country different from elites' country of residence, or (ii) located in elites' country of residence but incorporated in a company that is registered in a *different* country (e.g. an apartment in Russia owned by Russian elites but incorporated in a Bahamas-registered company).

elites are more likely than not to hold more than half their wealth offshore.[31] The second advantage of using offshore ties to measure elites' financial connections is that they are unobservable to the dictator.[32] On the contrary, the dictator can observe onshore ties, thereby possibly deterring connected elites that want to conceal their connection from establishing onshore ties. Thus, because of their legal obscurity offshore ties should be more accurate than onshore ties in capturing connections between elites in dictatorships. A third, related argument in favor of using offshore ties is that they limit spillovers for elites. Due to the threat of predation, financial ties formed through onshore assets expose connected elites to negative externalities from their connection. For example, if elites i and j form a joint venture through an onshore company that gets expropriated because the dictator wants to prey on i, j will incur a negative spillover. Crucially, the dictator cannot prey on offshore assets, while onshore assets that are incorporated in offshore companies are harder to prey on than those incorporated in onshore companies. Therefore, offshore ties provide elites in dictatorships greater protection against indirect predation than onshore ties. For these reasons, using data on offshore ties to capture elite financial networks actually strengthens my research design.

Network Representativeness Putting aside the arguments in favor of using offshore ties, it is useful to assess the representativeness of the offshore networks in my data. In particular, we might want to know whether my networks are atypically dense/sparse. Ideally, to make that assessment I would use information on the observed elites that predicts elites' propensity to form financial ties. One feature that might predict tie formation is wealth. If the distribution of wealth among elites in the data is similar to that among elites in dictatorships at large, we would have one less reason to doubt the data's validity. Unfortunately, the Panama Papers provide no substantive information on the observed elites, while the sheer number of elites in the data renders an exhaustive merge with external data sources unfeasible. Thus, I restrict my attention to the most wealthy and public elites, billionaires, and calculate what percentage of the world's known billionaires from dictatorships are observed in my data. I use the most comprehensive source on billionaires, Forbes' *The World's Billionaires* list (Kroll, Miller and Serafin, 2010). For 2010, 11.5% of the list's billionaires in dictatorships appear in my data.[33] Recalling that Mossfon's share of the whole market for offshore legal services

[31] To verify this claim, recall that the average country holds 17% of its GDP offshore. Assume that the top 1% of income earners, call them elites, own 20% of GDP and the remainder is owned by the bottom 99%—a relatively unequal distribution for the countries in my sample. If the bottom 99% hold all of their assets onshore, this implies that elites hold 85% of their assets offshore. Note that the percentage of elites' assets held offshore increases the larger the percentage of the country's GDP held offshore and the lower the percentage of GDP held by elites (i.e. the more equitable the income distribution). For the average country in my sample, as long as elites own less than 34% of GDP—roughly the figure of the US, one of the most unequal countries as measured by Gini—my calculations imply that elites hold more than half of their assets offshore. In short, for the majority of the countries in my sample, it is likely that the bulk of elites' assets are offshore.

[32] Unobservability does not only hold for ties formed through assets that are located offshore, but for ties formed through assets located onshore but incorporated in offshore companies. In fact, because of this unobservability, offshore companies are labeled "shells"—they are often used to obscure the physical person(s) that ultimately own the assets.

[33] The Forbes list includes all billionaires with estimated net wealth above 2 billion US dollars. The methodology for estimating billionaires' wealth is described in Kroll, Miller and Serafin (2010). I use the year 2010, as it

could not have exceeded 15% that year, it seems that the ultra-wealthy are neither over- nor under-represented in the Panama Papers. Though this is clearly a limited assessment of the representativeness of observed elites' wealth, let alone of other features that might affect the structure of the observed financial networks, it is reassuring that one test of the data's comprehensiveness produces no concerns.

Network Cross-Checks Another way to assess whether the networks in the Panama Papers accurately reflect the broader set of ties between elites is to compare my data to other sources. As hinted earlier, there are no other cross-national datasets of elite networks that cover *all* dictatorships. Hence, I resort to the assessments of two region specialists. Haddad (2011), an authoritative source on Middle Eastern elite networks, argues that Syria's elite network is particularly dense. Additionally, Cárdenas (2015) conducts an analysis of interlocking corporate directorates in Latin America and finds that the Chilean business network is denser than the Argentine one. And using a similar method, Cárdenas and Robles-Rivera (2017) find that Panama's elites are more densely interconnected than Costa Rica's, which, in turn, are more densely interconnected than El Salvador's. Indeed, in my data the Syrian network is the densest among all Middle Eastern ones, Chile's network is more dense than Argentina's, and Panama's is more dense than Costa Rica's and El Salvador's (though the latter is denser than the former).[34] Again, although these cross-checks do not provide conclusive proof of my data's validity, they do not raise any red flags for my data. Therefore, I proceed to use the Panama Papers as a unique source of information on elites' financial ties, with respectable coverage and no documented concerns of non-representativeness.

5 Analysis

In this section, I describe the approach used in my statistical analysis and present and discuss my baseline results and robustness checks.

5.1 Approach

Statistical Model My data is structured as a cross-sectional time-series (maximum of 101 countries and 26 years). The regressions I estimate for country k and year t are generally of the form

$$y_{k,t} = \alpha + \beta d_{k,t-1} + \boldsymbol{x}_{k,t-1}\boldsymbol{\gamma} + \epsilon_{kt} \tag{1}$$

where d is network density, as described in Section 3, and $\boldsymbol{x}$ a row-vector of controls. I begin by estimating a simple model with few control variables, then progress to more sophisticated models and add more controls. In all of my models, I lag time-varying predictors

was the latest year for which I could acquire comprehensive data.

[34] Furthermore, Haddad (2011) and many others have emphasized the importance of Rami Mahklouf, a cousin of Bashar al-Assad, among Syrian elites; he is one of the wealthiest Syrian businessmen and controls several state-owned companies. In line with his perceived centrality in the business world, in my data Mahklouf appears as the most central node in the Syrian network. Note that the Latin American countries I mention here were overwhelmingly democratic during the period I study, so they are not included in my later analysis.

by one year to mitigate some concerns of endogeneity, and I log predictors when it transforms their distribution to one that better approximates the Normal.[35] All of my models use country-clustered standard errors to account for within-country cross-year correlation of unobservables.

Dependent Variable Features My theory predicts a negative effect of elite network density on dictators' predation of elites. Unfortunately, predatory economic policies are hard to observe, and any proxy variable will be imperfect. That said, a good proxy variable should exhibit at least the four features. First, it should cover an adequately large sample of dictatorships and years, in order to provide my regressions statistical power and external validity; second, it should be measured in a consistent way across dictatorships and years, to reduce the risk of non-random measurement error; third, it should be non-binary (ordinal, discrete, or continuous), so as to allow statistical models to discriminate between varying levels of predation in different dictatorship-years; fourth, it should capture *actual*, not *perceived*, property rights. Unfortunately, most variables employed in the property rights literature fail the last criterion, as they are indexes based on expert opinion surveys.[36] These indexes face well-known critiques (e.g. Treisman (2007)): experts base their answers to property rights survey questions on proxies for property rights (e.g. capital flows), thereby creating spurious correlations between these proxies and property rights.

Dependent Variable A variable that is not based on perceptions and features in several political science studies is the ratings scale of political risk insurance agency Credendo Group (formerly ONDD) (Credendo Group, 2017).[37] Credendo is a leader in the political risk insurance market, and insures companies operating in any country against various political risks. One of Credendo's insurance products is closely related to the behavior I am studying: the *Expropriation Risk* product protects companies against direct expropriation or breach of contract by the government. Crucially, insurance agencies and their clients are rational profit-maximizers, thus we can be relatively certain that Credendo's premia accurately reflect the risks it insures against.[38] Moreover, as documented in interviews conducted by Jensen (2008), Credendo is a price-maker in the political risk insurance market, and insurance brokers exhibit price convergence the premia they charge. Therefore, Credendo's premia can also be considered representative.[39] The premia charged to each client are confidential, and the

[35] I address the issue of endogeneity in Section 6.

[36] In Section 5.3 I discuss the use of some of these indexes in my analysis.

[37] Credendo Group insurance premia were first used by Jensen (2008) and have since been used by Jensen, Malesky and Weymouth (2014), Wilson and Wright (2017), and Graham, Johnston and Kingsley (Forthcoming), among others. The version of the data I use is from the replication files of Wilson and Wright (2017).

[38] In addition, expropriation risk insurance seems to be the main revenue source for political risk insurance agencies. From 1991–2004, 84% of settlement claims received by the Overseas Private Investment Corporation, a major US government agency that insures against political risk, were for expropriations (Jensen, 2008). This further emphasizes Credendo's incentive to price expropriation risk correctly.

[39] To verify that Credendo ratings capture actual expropriation and other political risks affecting business, Graham, Johnston and Kingsley (Forthcoming) use the ratings to predict *de facto* measures of these risks, like restrictions on capital flows. Using a series of statistical models, they find that the Credendo ratings closely predict actual measures of political risks to business.

methodology for pricing the premia is proprietary. Fortunately, Credendo publicly releases an annual rating for each country, which is based on the underlying insurance contracts it sells to clients operating in that country. Like all insurance products, larger risks command higher premia, and so the higher the rating a country receives on Credendo's scale, the greater the expropriation risk in that country. That scale ranges from 1–7 and is comparable across countries and years.[40] Though Credendo's ratings for expropriation risk only cover the period 2002–2013, they include 70 out of 75 GWF dictatorships observed in that period, resulting in 666 data points. In short, the Credendo expropriation risk ratings satisfy all four criteria for my dependent variable: they are based on actual expropriation, are reasonably fine-grained, comparable across observations, and provide adequate coverage.[41]

Independent Variable As described in Section 4, my independent variable is the density of elite financial networks, as observed in the Panama Papers.[42] That density exhibits a strong right skew, with a of 0.09, median of 0.045, and a standard deviation of 0.12. Figure 3 shows the geographic distribution of density for the sample used in my regressions (2002–2013), as a map of dictatorships' average densities in that period. Like in many other spatial distributions, there is no unique interpretation. Yet, two patterns do stand out. The first is the low network density of dictatorships with former communist regimes. This might be owed to the relatively young nature of the economic elite in those countries, which were born through the haphazard transition from command to market economies. The second visible pattern is the high network density of countries in Southern Africa, which could be attributed to the ethnically cohesive nature of (clusters of) elites from the colonial era (e.g. British in Botswana and Zimbabwe, British and Germans in Namibia, and Arabs in Mozambique).

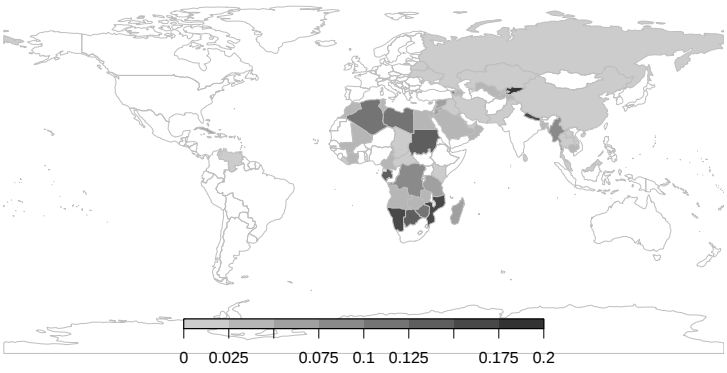

Figure 3: Global distribution of elite financial network density

NOTES: Average country density for 2002–2013 (years with non-missing dependent variable). Only 54 GWF dictatorships graphed (sample of interest); excludes 16 outliers (values 0/1). See text for data methodology.

[40] Expropriation risk ratings (for GWF dictatorships) are distributed relatively symmetrically, with a mean of 4.12, median of 4, and standard deviation of 1.55. In my regressions, I standardize the variable for interpretability.

[41] In Section 5.3 I discuss the use of alternative measures of expropriation and property rights in my analysis.

[42] I discuss the use of alternative independent variables in Section 5.3.

IV & DV Since my data has a temporal dimension, it is informative to visualize the dependent and independent variables over time. Figure 4 graphs the mean value of both variables across all years for which data is available.[43] There are two main takeaways from the plot. The first is that average financial network density steadily decreases after 1995, from a high of 0.21 to a low of 0.03.[44] This drop might seem to counter the global trend of increasing financial ties between countries during this period. Yet, recall that the data captures financial ties among elites *within countries*. Moreover, there might be a tradeoff in elite ties, such that stronger between-country ties are associated with weaker within-country ties. Indeed, one of the conventional wisdoms on global financial openness is that it disrupts entrenched elite networks within countries (The Economist, 2016*a*). The second takeaway is that expropriation risk (standardized) increases near-steadily after 2005, from 0.17 standard deviations below its mean, to 0.24 standard deviations above it. A corollary of this pattern is that, as elite network density decreases during 2005–2013, expropriation risk increases. In fact, it is this trend that underpins the negative association between network density and expropriation risk reported in my regressions.

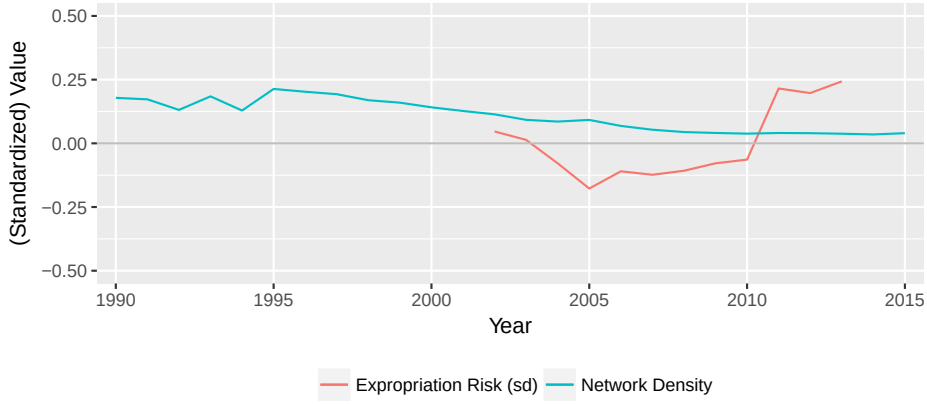

Figure 4: Dependent and independent variable means across time

NOTES: Expropriation risk values standardized for interpretability; data only exists for 2002–2013; mean for each year calculated using 36-42 countries. Density values in original scale (0–1); mean for each year calculated using 8-43 countries. See text for data sources and methodology.

Control Variables To control for spurious correlations between expropriation risk and density of elite financial ties I include several controls in my regressions:[45] first, the logged number of *nodes* (elites) in each network (country-year), since the number of nodes enters the formula for calculating density, and it might also affect the dictator's predation directly

[43] Means are calculated using a different number and list of countries in each year, since countries phase in and out of my sample (transitions to and from dictatorship to democracy). I do not include confidence intervals for the two series, since they vastly reduce the graph's interpretability.

[44] Before 1995, a maximum of 16 countries appear in the Panama Papers, so the mean density calculated for years up to 1995 is very noisy.

[45] I include essentially all of the control variables that appear in previous regressions of the Credendo expropriation risk ratings (e.g. Jensen (2008)), while adding some futher controls. I discuss the use of even more control variables in Section 5.3.

(in larger networks there are more elites to constrain the dictator);[46] second, *GDP per capita* (logged, in current US dollars), as it might strengthen elite ties through increasing financial transactions, while decreasing dictators' incentive to prey due to collecting more in taxes; third, the *growth* rate (of GDP per capita in current US dollars), because a faster growing economy might also propel tie-generation, while making the dictator hesitant to disrupt a well-performing economic system; fourth, *inflation* (annual percentage change in consumer price index), as it might raise transaction costs for elites, hence reducing their financial ties, while tempting the dictator to seize control of companies in order to halt rising prices; fifth, *government net lending* (as a percentage of GDP), seeing as in countries where the government is a net lender to the private sector elites might have more resources to transact with, and the government will have less need to seize assets from the private sector; sixth, *trade* (import and export value as a percentage of GDP, logged), because elites from countries with more international transactions might also transact more domestically, while dictators in such countries might be disciplined by trade flows if they expropriate; seventh, *oil rents* (production value in current US dollars, logged), since oil wealth typically crowds-out the private economy, thereby reducing opportunities for elite transactions, while dictators often expropriate oil assets (Mahdavi, 2014); eight, whether there is a *legislature* (dummy variable), given that elites often belong to and interact within legislatures, and legislatures are thought to constrain dictators (Wright, 2008); ninth, whether the country was a *British colony* (dummy variable), because the British colonial elite was particularly cohesive, while institutions endowed by the British are linked to stronger property rights (Acemoglu, Johnson and Robinson, 2001); tenth, *ethnolinguistic fractionalization* (0–1 index), since diversity might fractionalize elite networks, while also preventing elites from coordinating to discipline the dictator; eleventh, *regime duration* (years), because elites might become more integrated as time progresses, while regime survival might undermine the dictator's urge to prey on the economy;[47] twelfth, *authoritarian regime-type* (GWF typology, 4 dummy variables), because the organizational structures associated with different regime types can affect elites' interaction as well as the constraints placed on the dictator (Geddes, Wright and Frantz, Forthcoming).[48] Furthermore, I include 5 regional indicators, to control for time-invariant unobservables that operate at the region-level and might affect both network density and expropriation risk (e.g. culture, geography). Finally, I include a lagged value of the dependent variable (LDV), due to the strong autocorrelation revealed by statistical tests.[49]

[46] Also, note that nodes directly enter the formula for density in the denominator and the upper limit of the summation.

[47] Following a similar rationale, instead of the regime's duration, in alternative specifications I control for the leader's tenure or the leader's age.

[48] GDP, growth, and trade data are from the World Bank's World Development Indicators (World Bank, 2016). Inflation and government net lending data are from the IMF's World Economic Outlook (International Monetary Fund, 2014). Oil data are from Ross (2013). Legislature data are from the World Bank's Database of Political Institutions (Beck et al., 2001). British colony data and regional indicators (Asia, Latin America, Middle East and North Africa, Sub-Saharan Africa, and ex-USSR) are from Hadenius and Teorell (2007). Ethnolinguistic fractionalization data are from Fearon and Laitin (2003). Regime duration data and regime type indicators are from Geddes, Wright and Frantz (2014). All preceding variables were obtained – though some were transformed – through the QoG dataset (Teorell et al., 2011).

[49] Durbin-Watson, Breusch-Godfrey, and Wooldridge tests on the residuals of regressions without LDVs reveal substantial serial correlation, thereby calling for the inclusion of an LDV.

Sample Though the number of observations in my regressions varies depending on which controls I include, my sample generally consists of 293–476 observations from 47–51 countries and 11–12 years of data (2002–2013).

5.2 Baseline Results

Density Table 1 displays the results from several variations of my baseline regression (Equation 1). In line with my hypothesis, across all specifications there is a statistically significant negative association between network density and expropriation risk. Density's significance ranges from above the 1.5% level to below the 0.01% level, and its coefficient ranges from roughly -0.035 (Model 2) to -0.160 (Model 1). I describe substantive effects for density using Model 1's coefficient because, though Model 1 is unsophisticated and lacks many controls, the coefficient it produces is actually closer to that of more robust models I present later (Table 2). Recalling that the dependent variable is standardized, density's substantive effect in Model 1 is moderate: a doubling of network density (roughly an increase of 1 log unit) is associated with a decrease of 0.16 standard deviations in expropriation risk the next year.[50] Alternatively, a 1 standard deviation increase in density (1.61 log units) is associated with a 0.26 standard deviation decrease in expropriation risk. A difference of 0.26 standard deviations in expropriation risk is substantial, as it is similar to that between Mozambique and the United Arab Emirates. Yet another way to describe density's substantive effect is the following: an increase in density from its observed minimum to its observed maximum (-8.67 to 0.50 in log units) is associated with an decrease of 1.47 standard deviations in expropriation risk, similar to the difference between Azerbaijan and Singapore.[51]

[50] All right-hand-side variables are lagged by one year, but I refrain from giving a temporal interpretation to the reported effects in order to conserve space. A doubling of network density is not a particularly large increase, given that mean density in the sample used in the regressions is 0.06. Such an increase can be achieved by holding the number of nodes (elites) fixed and doubling the number of ties between them, or by holding the number of ties fixed and decreasing the number of nodes by roughly 28%. The exact decrease in nodes needed to double density depends on the initial number of nodes and initial density, due to the non-linear relationship between nodes and density.

[51] I also rerun my regressions after adding density's squared term, in order to capture any curvilinear effect density might have on expropriation risk. Density remains negative and statistically significant and its coefficient strengthens, while the squared term is also negative and not always significant. This suggests density does not have a U-shaped effect on expropriation. Thus, I present the regressions without the squared term, which are also easier to interpet.

Table 1: Expropriation Risk – Baseline Models

	(1)	(2)	(3)	(4)
Log Network density$_{t-1}$	-0.159^{**}	-0.035^{**}	-0.037^{***}	-0.046^{***}
	(0.063)	(0.015)	(0.012)	(0.012)
Log Network size$_{t-1}$	-0.244^{***}	-0.022	-0.023^{*}	-0.025^{*}
	(0.053)	(0.016)	(0.013)	(0.014)
Log GDP/capita$_{t-1}$		-0.009	-0.009	-0.016
		(0.015)	(0.016)	(0.014)
GDP/capita growth$_{t-1}$		-0.004	-0.004	-0.005
		(0.003)	(0.003)	(0.003)
Inflation$_{t-1}$		0.001	0.001	0.001
		(0.001)	(0.001)	(0.001)
Gov. net lending$_{t-1}$		-0.002	-0.002	-0.001
		(0.002)	(0.002)	(0.002)
Log Trade/GDP$_{t-1}$		-0.024	-0.021	-0.002
		(0.031)	(0.030)	(0.029)
Log Oil production$_{t-1}$		0.002	0.002	0.003^{**}
		(0.001)	(0.001)	(0.001)
Legislature$_{t-1}$			0.036	0.012
			(0.041)	(0.041)
British colony			-0.012	-0.029
			(0.021)	(0.019)
Ethnolinguistic frac.			-0.054	-0.055
			(0.053)	(0.055)
Regime duration$_{t}$			0.000	0.000
			(0.000)	(0.000)
Military$_{t-1}$				0.088^{**}
				(0.036)
Monarchy$_{t-1}$				-0.021
				(0.040)
Personalist$_{t-1}$				-0.098^{***}
				(0.025)
Expropriation risk$_{t-1}$		0.919^{***}	0.920^{***}	0.919^{***}
		(0.023)	(0.024)	(0.022)
Constant	0.368^{***}	0.106	0.072	0.112
	(0.110)	(0.153)	(0.153)	(0.143)
Region Dummies	N	Y	Y	Y
N	476	355	355	355
n	51	47	47	47
T	12	11	11	11
Adj-R^2	0.07	0.91	0.91	0.91

NOTES: $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$. All models are OLS with country-clustered standard errors. Dependent variable is standardized. Variables without time indexes are time-invariant. Regime-type reference category is Party. Coefficients on region dummies suppressed to save space. See text for data sources.

Nodes The only controls that are statistically significant are nodes, oil rents, and two regime type indicators.[52] Interestingly, the number of elites is negatively associated with expropriation risk. This implies that, all else equal, dictatorships with a larger number of elites expropriate less. However, I hesitate to give weight to this finding, as it is not particularly robust: the coefficient on nodes is insignificant in Model 2, only significant at the 10% level in Models 3 and 4, and insignificant in most robustness checks I discuss in Section 5.3. Moreover, including an interaction term between density and nodes renders the coefficients on both nodes and the interaction positive and/or insignificant, while preserving the sign and significance of density's coefficient.[53] At the risk of over-interpreting the results from the regression with the interaction term, they suggest that, all else equal, a larger elite exacerbates expropriation (positive coefficient on nodes), especially if densely connected (positive coefficient on interaction). Alternatively, we can interpret the negative coefficient on density coupled with the positive interaction term as suggesting that, only when they are small do dense elite networks constrains the dictator. In any case, the inconsistent sign and statistical significance of the coefficient on nodes renders further discussion fruitless.

Regime Type A more puzzling finding than the (inconsistent) negative effect of nodes on expropriation risk is that personalist regimes have the lowest expropriation risk among GWF regime types. This contradicts the literature, which argues that personalist regimes score the highest on expropriation risk (Wright, 2008). Curiously, in one of their models Wilson and Wright (2017) also find that personalist regimes are associated with lower expropriation risk. However, when they probe that result further, the authors find that it is not robust. Similarly, if I interact the personalist regime-type dummy with density, the coefficient on personalism becomes insignificant, while the interaction term becomes positive, though not always significant. Again, at the risk of over-interpreting a non-robust finding, it suggests that, all else equal, dense elite networks constrain dictators' predation, but less so against personalists. The weaker effect of elite network density in personalist regimes might be owed to personalist dictators' concentration of power; already facing few constraints, powerful dictators might be able to prey on subjugated elites more easily when these elites are interconnected (Svolik, 2009). Where in other regime types financial ties between elites foster information-sharing and monitoring, against a personalist dictator the same ties might exacerbate the dictator's predation through elites' financial contagion. Another way to interpret the estimates from the regression with the density–personalism interaction is the following: the adverse effect of personalism on property rights only holds in dictatorships where the elite is more densely connected. Regardless of the interpretation, the frailty of this finding limits the returns from discussing it further. Moreover, other coefficients on regime types indicators show the expected signs. Monarchies face statistically similar expropriation risk

[52] Model 4 corroborates the deleterious effect of oil wealth on property rights documented in the literature. The dummy coefficient for Latin America (not reported) is also statistically signifcant, and its positive sign reflects the region's poor record of property protection. Unsurprisingly, the coefficient on the LDV is also positive and significant, as in every regression I will present.

[53] I do not present the results of these regressions because an interaction with two logged variables (density and nodes) is particularly hard to interpet. I also rerun my regressions after adding the squared term of nodes, in order to capture any curvilinear effect nodes might have on expropriation risk. The subsequent regressions produce insignificant coefficients for both nodes and nodes squared.

as single-party regimes (reference category). Finally, military regimes pose the largest risk for expropriation. This could be attributed to the military's generally myopic policy outlook and attempts to seize control of assets deemed strategically important.

Substantive Effects Comparison To better grasp the substantive effect of density on expropriation, it is useful to benchmark it against the effect of regime type. Figure 5 shows the mean predicted value of expropriation risk for all levels of density across different regime types. The maximum effect of regime-type – the mean predicted difference in (standardized) expropriation risk between personalist and military regimes – is small; less than 0.19 standard deviations. On the other hand, the maximum effect of density – resulting from a change from a network consisting of isolated elites to one where all elites are connected to each other (0 to 1) – is almost double (0.38 standard deviations).[54] In short, elite network density seems to have a statistically significant and sizable effect on expropriation, much more so than other variables featured in the literature.

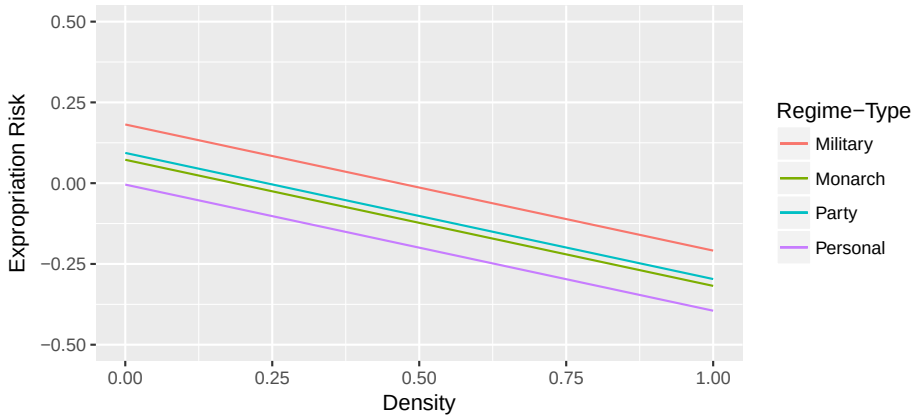

Figure 5: Marginal effect of elite network density on expropriation risk by regime-type.

NOTES: Regression lines based on coefficients from Table 1, Model 4. Expropriation risk standardized. Density logged in regression model but graphed as unlogged for interpretability. See text for data sources.

5.3 Robustness Checks

Though the models presented in Table 1 include a battery of controls, regional indicators, and an LDV, there might still exist unobserved confounders that bias the estimated relationship between network density and expropriation risk.

First-Differences & Fixed Effects The first threat to inference I try to guard against are country-specific, time-invariant unobservables, such as culture or geography. Indeed, Wooldridge, Lagrange Multiplier, and F- tests all reject the null hypothesis of no country effects. Hence, in Table 2, Models 1–2, I estimate first-difference (FD) and country-fixed effects (FE) regressions. Both models confirm the statistically significant negative association

[54]Note that this effect is almost a lower bound estimate, since the associated coefficient (Table 1, Model 4) is significantly smaller than that of other models.

between density and expropriation risk found in my baseline regressions. In addition, the coefficient on density is now stronger than the coefficients reported in Table 1. Comparing FD and FE models, the only differences are that the FD model finds a stronger effect for density than the FE model and a significant negative effect for nodes (null effect in FE model).

Two-Ways Effects Another unobservable confounder to guard against are year-specific shocks that affect countries homogeneously (e.g. global recessions). Indeed, LM- and F-tests both reject the null hypothesis of no two-ways effects (country and year). Thus, Model 3 estimates a two-ways FE model. Again, the coefficient on density is negative, significant, and stronger than the baseline regression coefficients, while the coefficient on nodes loses significance.[55]

Arellano–Bond/GMM Including an LDV in either FD or FE model would make my estimates inconsistent, thus Models 1–3 omit the LDV.[56] However, as noted above, statistical tests suggest that an LDV might be necessary to remove the strong autocorrelation in expropriation risk. Hence, I employ an Arellano–Bond (A–B) estimator, which uses Generalized Methods of Moments (GMM) estimation within a country-FE model to instrument the lags of the dependent variable with higher-order lags.[57] Crucially, as evident in Model 4, the coefficient on density is still negative and highly significant, while its (absolute) size is almost double that of other models.[58]

[55] One curious finding in Model 3 is the negative coefficient on inflation, suggesting that dictatorships wher prices increase faster experience *less* expropriation. However, I hesitate to interpet this result further, due to the coefficient's minute size and significance at only the 10% level.

[56] In particular, an LDV makes the FD estimator inconsistent because the first-differenced LDV predictor $\Delta y_{i,t-1}$ is correlated with the first-differenced error $\Delta\epsilon_{i,t}$. Similarly, an LDV makes FE estimates inconsistent because the demeaned LDV predictor $y_{i,t-1} - \bar{y}_i$ is correlated with the demeaned error $\epsilon_{i,t} - \bar{\epsilon}_i$. The latter bias (Nickell bias) is a concern in "short" panels (low T/n), like my own. Note that, even though a *maximum* of 12 years of data are used in my regressions, the average T is around 9, due to the unbalanced structure of my panel and countries transitioning in/out of dictatorship at different durations.

[57] I instrument the first lag of the dependent variable with its second lag. The results are substantively unchanged if I use both second and third lag as instruments, while the model passes the Sargan test of valid instruments. Using even more instruments is possible but results in (further) reductions of degrees of freedom. I also fit an A–B/GMM model with two-ways effects (Model 4 plus year effects), and the results are substantively unchanged. The only differences I observe versus Model 4 is that the coefficient on density is 0.25, the coefficient on government lending becomes insignificant, while the coefficient on oil production becomes significant.

[58] Model 4 also reports significant negative coefficients for nodes and net government lending (the latter only siginificant at the 10% level).

Table 2: Expropriation Risk – Additional Models

	FD	FE	2-way FE	A–B/GMM
	(1)	(2)	(3)	(4)
Log Network density$_{t-1}$	−0.192***	−0.168**	−0.175**	−0.329***
	(0.065)	(0.076)	(0.072)	(0.109)
Log Network size$_{t-1}$	−0.250**	−0.004	0.077	−0.372**
	(0.100)	(0.108)	(0.128)	(0.157)
Log GDP/capita$_{t-1}$	−0.006	−0.083	−0.129	−0.046
	(0.125)	(0.145)	(0.164)	(0.185)
GDP/capita growth$_{t-1}$	−0.002	−0.001	0.000	0.004
	(0.005)	(0.006)	(0.007)	(0.008)
Inflation$_{t-1}$	0.000	0.000	−0.001*	0.000
	(0.000)	(0.000)	(0.000)	(0.001)
Gov. net lending$_{t-1}$	−0.003	−0.010	−0.007	−0.007*
	(0.004)	(0.006)	(0.006)	(0.004)
Log Trade/GDP$_{t-1}$	0.002	−0.088	−0.028	0.144
	(0.122)	(0.160)	(0.188)	(0.146)
Log Oil production$_{t-1}$	0.060	−0.090	−0.102	0.186
	(0.095)	(0.100)	(0.114)	(0.149)
Expropriation risk$_{t-1}$				0.591***
				(0.222)
N	339	387	387	293
n	48	48	48	48
T	12	12	12	12
R^2	0.03	0.09	0.07	0.17
Robust Wald-test p-val	0.02	0.00	0.01	0.00
Autocorr. test p-val	0.03	0.00	0.00	0.88

NOTES: *p < .1; **p < .05; ***p < .01. All models linear and include country-clustered standard errors. Dependent variable is standardized. Model 1 estimated in first differences. Model 2 includes country fixed effects. Model 3 includes country and year fixed effects. Model 4 includes country fixed effects and the first lag of the dependent variable, instrumented by its second lag. The model is estimated through Generalized Method of Moments (Arellano–Bond estimator). See text for data sources.

Model Choice Choosing which model to rely on for inference in the absence of a tight mapping between theory and empirics or a well-defined body of empirical work is, to some extent, a matter of preferences.[59] On the one hand, the FD and A–B estimators are better suited to removing the strong autocorrelation in expropriation risk by removing medium- and long-term trends in the data, but only identify coefficients using variation from short-term

[59]Note that in all Table 2 models I am forced to drop institutional and demographic controls (legislature, British colony, regime duration, ethnolinguistic fractionalization), as well as regime-type and regional dummies, as they are either time-invariant or exhibit so little within-country variation over time (e.g. legislature, regime type) that their coefficients are non-identified.

trends.[60] On the other hand, the FE models allow for trends around the mean to inform their estimates, as they merely de-mean the data, but might provide inconsistent estimates in the face of strong autocorrelation. Indeed, autocorrelation is a serious concern here, and the FE models' residuals do not pass the Durbin-Watson test of first-order autocorrelation, whereas the FD model's (differenced) residuals do. This suggests that first-differencing adequately removes autocorrelation. However, as shown in the last line of Table 2, even the FD model fails the Breusch–Godfrey test of no serial correlation, while the A–B model is the only one that passes it. Moreover, Model 4 exhibits stronger fit than Models 1–3 in terms of R^2, though I should add the usual caveats about assessing fit through R^2.[61] Overall, the tests and diagnostics favor the A–B model. This verdict is in line with practitioners' advice that the A–B estimator is best applied to data with a large number of observations, few time periods, heteroskedasticity, serial correlation, a dynamic dependent variable, and dynamic and potentially endogenous predictors—all features of my data (Roodman et al., 2009).

Substantive Effects Revisited Table 2 shows that, apart from Model 1, the baseline regressions significantly underestimate the coefficient on density. The coefficient from the most appropriate model (A–B) suggests that, all else equal, a doubling of network density in country i in year $t-1$ (roughly an increase of 1 log unit) is associated with a decrease of 0.33 standard deviations in expropriation risk in year t. Alternatively, a 1 standard deviation increase in log density (1.61 log units) predicts a 0.53 standard deviation increase in expropriation risk, equivalent to the difference between China and the United Arab Emirates. It should be noted that "all else equal" in Model 4 involves controlling for network size and several economic variables, unobservable country-specific factors (country FE), and only leaving variation in the dependent variable between t and $t-1$ (due to the LDV). In other words, density has to clear a high bar in Model 4 to have such a significant and sizable effect on expropriation risk. Overall, after using a series of more sophisticated and robust models than those in Table 1, I find that network density retains its statistical significance, while displaying an even stronger negative effect on expropriation risk.

Additional Models To further probe the robustness of my findings, I estimate a series of additional specifications. Since expropriation risk ratings are measured on an 7-point scale, one could argue that an ordered choice model best describes the data-generating process behind my dependent variable and might also make my estimates more precise. However, the consensus in the literature is that the advantages of ordered choice over linear models are minimal, especially when the dependent variable has several values and does not deviate much from the normal distribution (Riedl and Geishecker, 2014).[62] Nevertheless, I rerun my baseline regressions using an ordered logit and find substantively similar results. Another robustness check I conduct is fitting my baseline regressions to cross-sectional data, which

[60] I estimate the FD model both with and without a linear time trend (intercept). The results are substantively identical, and the coefficient on density is statistically identical.

[61] R^2 and other measures of goodness of fit are not usually reported for A–B/GMM models, so I manually calculate R^2.

[62] Additionally, ordered choice models are inconsistent when combined with fixed effects, unstable when applied to panels with complex dynamics, and produce output that is significantly harder to interpret.

I create by calculating country-level means for all variables (dependent and predictor).[63] Again, my results remain substantively unchanged. Furthermore, I repeat my analysis using unlagged values of all predictors, and find that my main results hold. Similar findings emerge from lagging density by 1 year and other predictors by 2 years—a specification that might increase the odds that the former is exogenous to the latter. Furthermore, my results are robust to using alternative types of robust standard errors, like Beck-Katz, Huber-White, and Newey-West, as well as regular standard errors. I also substitute fixed with random effects—though the latter problematically assume that all predictors are uncorrelated with the error term—and obtain very similar results (compared to Table 2, Models 3–4).[64] Finally, I substitute the first with the second lag of the dependent variable in my baseline regressions and also rerun those regressions with both lags, which removes further autocorrelation but reduces my sample size. Overall, the findings from both Tables 1–2 are not sensitive to variations of the estimated models.

Alternative DVs: Expropriation Incidents As argued in Section 5.1, my dependent variable exhibits several desirable traits in testing my argument. That said, it is interesting to explore whether my findings hold when using three sets of alternative dependent variables. First, I explore two datasets of *actual incidents of expropriation*. Albertus (2015) codes all international instances of land expropriation in the period 1900–2009, while Hajzler (2012) records all international expropriation events against agriculture, mining, petroleum, and utility companies from 1960 to 2006.[65] Unfortunately, the rare frequency of expropriation (roughly 3% of cases for the country-years in my sample), coupled with missing data, leaves too little variation in the dependent variable to allow for robust estimation using these datasets. Another weakness of these datasets is that, though the incidents of expropriation that they record are clearly cases of the dictator's predation on elites, they ignore "softer" predatory behavior like targeted regulation, extortionary taxation, and transfer restrictions. Putting these limitations aside, I adapt my baseline specifications to logit models and find a significant negative effect for elite network density on the odds of expropriation, as measured by both datasets—though the effect is stronger for land expropriation.

Alternative DVs: Property Rights Indexes The second set of alternative dependent variables I use is *indexes of property rights*, whose limitations were touched upon in Section 5.1. I briefly describe some of these indexes.[66] The PRS Group International Country Risk Guide produces a 12-point "investment profile" rating, which is an aggregate of three components: contract viability/expropriation, profits repatriation, and payment delays (Howell, N.d.). The Heritage Foundation compiles the Index of Economic Freedom,

[63] I do not fit the models of Table 2 to the cross-sectional data, since those models require data with a temporal dimension.

[64] A Hausman test rejects the null hypothesis that fixed and random effects estimates are similar. I cannot estimate two-ways random effects (country and year), due to the unbalanced nature of my panel.

[65] Albertus and Menaldo (2012) compile a more detailed record of expropriation – of not just land – in Latin America. Unfortunately, their dataset overlaps with my network data only in the period 1990–2002, during which there were too few years of dictatorship in Latin America to allow for a meaningful analysis.

[66] All indexes are annual and cross-national, with the exception of the monthly PRS Group ratings (I convert them to annual averages). Most of these variables are obtained through the QoG dataset (Teorell et al., 2011).

which includes a property rights component scoring from 0 to 100 "the degree to which a countrys laws protect private property rights and the degree to which its government enforces those laws" (Miller, Kim and Holmes, 2015, p. 353). The World Economic Forum releases the Global Competitiveness Report, which subsumes an 8-point rating of property rights protection (Schwab and Sala-i Martin, 2016). The World Bank issues the World Development Indicators, which provide a 7-point variable reflecting property and contract rights legislation and enforcement (World Bank, 2016). Finally, the Fraser Institute produces the Economic Freedom of the World dataset, which includes a 0 to 10 rating of "legal structure and security of property rights" (Gwartney et al., 2016).[67] Though the above indexes differ in their methodology, granularity, and geographic and temporal coverage, they all yield roughly the same conclusion in my regressions: dictatorships with more dense elite financial networks are associated with stronger property rights.

Alternative DVs: Property Rights Proxies The last group of alternative dependent variables that I analyze are three *proxies for property rights*. First, I follow Weymouth (2011) in using financial dollarization, the proportion of a country's bank deposits denominated in foreign currency, as a proxy for "indirect expropriation" and property rights (Yeyati, 2006). One concern with this proxy is that dollarization mostly reflects the average household's hedge against inflation and currency devaluation, not elites' response to predation by the dictator. The second proxy I use is net foreign direct investment (FDI) outflows as a percentage of GDP (World Bank, 2016). Though FDI certainly captures the decisions of holders of large amounts of capital (i.e. elites), using it as a property rights proxy raises a different concern: FDI decisions depend on many more factors than economic predation, some of which I cannot control for (e.g. relative return on foreign vs domestic assets). Thus, FDI is arguably a weak proxy for property rights. The final proxy I employ is an estimate of illicit financial flows, which measures the value of outflows "illegally earned, transferred, and/or utilized" (Solomon and Spanjers, 2017, p. 1). A concern with this variable is that illicit outflows might be funds the dictator *rewards* elites with, just as much as they might be funds elites try to hide from the dictator. Additionally, illicit outflows are often proceeds of crime, and crime's relationship to economic predation in dictatorships is unclear. Perhaps due to the above limitations, I find that the effect of elite network density is insignificant and/or has an inconsistent sign in regressions of all three property rights proxies.

Alternative Independent Variables Another robustness check I conduct involves using different network statistics as independent variables. Though I formulate my theory in terms of network *density*, I cannot rule out other measures of network structure affecting elite's coordination in constraining the dictator. One network statistic that might capture the dynamics described in my theory is the fraction of the network that is non-isolated (non-isolate fraction); that is, has at least one tie. This variable reflects a weak form of density, in that it does not discriminate between elites with one financial tie and elites with dozens of ties. This "loss of information" in the variable might be the reason why, despite displaying a neg-

[67] There are also several datasets with rule of law indexes, such as Freedom House's Freedom in the World report and the World Bank's Worldwide Governance Indicators (Freedom House, 2016; World Bank, 2014). I omit these indexes from my analysis because rule of law relates to the enforcement of a much broader set of rights than property rights.

ative and significant effect on expropriation risk in all of my models, non-isolate fraction's coefficient is roughly half that of density's. A different metric often used in network analysis is transitivity, otherwise known as clustering. A network's (global) clustering coefficient captures the ratio of observed triads (e.g. i-j, j-k, k-i) to potential triads (Wasserman and Faust, 1994). Since we might expect that transitivity in financial ties fosters elite coordination, I rerun my regressions with networks' clustering coefficient as the independent variable.[68] I find substantively similar results as in the tables presented above, with the exception of the fixed-effects regressions (null effect). Finally, I substitute density with another prominent network statistic, degree centralization. Loosely speaking, a network's degree centralization measures the influence – in terms of network ties – of the most centrally located node(s) (Freeman, 1979). One might imagine that, if one elite has significantly more ties than any other elite, she might be able to control the flow of resources and information through the network and act as a focal point for coordination to constrain the dictator. As such, dictatorships with elite networks with high degree centralization might experience stronger property rights. Indeed, I find that degree centralization has a significant negative effect on expropriation risk – and stronger than density – in all of my regressions other than the A-B model (null effect).[69]

Additional Controls: Economic A further robustness check I conduct entails using additional control variables. Due to the myriad of determinants of expropriation risk (DV) that might also affect elite network density (IV), coupled with the absence of a literature on my IV, I try to control for more potential confounders than is customary. In particular, I experiment with additional controls from three categories: economic, political, and demographic. Among economic variables, I begin by controlling for exchange rate restrictions with data from Ilzetzki, Reinhart and Rogoff (2017); it is possible that in dictatorships with such restrictions elites interact more to overcome them, while economic mismanagement stemming from the restrictions might lead the dictator to expropriate elites in order to gain control of key companies (e.g. Venezuela recently). A related variable I account for is capital controls, using data from Fernández et al. (2016). As with exchange rate restrictions, elites might interact more to overcome capital controls, while the economic burden controls cause might incentivize dictators to scapegoat elites and seize their assets (e.g. Venezuela again). I also control for government expenditure as a share of GDP (with data from International Monetary Fund (2014)), since a large government sector might crowd out the private sector and elite interactions within it, while large governments are generally more likely to prey on private firms (owned by elites). The next variable I control for is income inequality (using data from World Bank (2016)), because concentration of income might lead to the concentration of financial interactions between few elites, while also tempting the dictator

[68] To illustrate the potential role of network transitivity in elite coordination, imagine a scenario where elites i, j, and k gain by punishing D, but only succeed if all three coordinate. Assume an elite only knows the participation decision of elites she is tied to, and that i is tied to j and k but j and k are not tied to each other. Even though i knows that j and k want to punish D, and the vice-versa also holds, j (k) does not know that k (j) wants to punish D; thus the participation threshold of 3 is not met and coordination fails. In short, transitive financial ties among elites might foster coordination in constraining the dictator, and dictatorships with elite networks that have higher clustering coefficients might experience less predation.

[69] Density is correlated 42% with the non-isolate fraction, -1% with transitivity, and 71% with degree centrality in my sample.

to redistribute elites' income through expropriation. Overall, my results are substantively unchanged, though using these controls substantially reduces my sample.[70]

Additional Controls: Political The second group of additional controls I explore are political/institutional. First, I include an indicator of the presence of multiple parties from Cheibub, Gandhi and Vreeland (2010). Similar to the effect of legislatures, it is possible that the ties observed in my data are the result of elite interactions within parties, which are argued to constrain dictators (Gandhi, 2008). Furthermore, I control for executive constraints, as measured by Marshall, Jaggers and Gurr (2016), because an unconstrained executive might both limit elites' interactions and prey on them. I also control for corruption, using the Bayesian index of Standaert (2015); this is because corruption often involves elite cronyism, which should foster elite interactions, but also the misuse of public office for private gain, which result in economic predation. Finally, I control for the origins of countries' commercial law, due to their potentially joint effect on elites' financial transactions and property rights (La Porta et al., 1999). None of these four controls nullifies the effect of density on expropriation, though some weaken its coefficient and/or limit my sample.

Additional Controls: Demographic The last group of controls I account for are three demographic variables.[71] The first is (log) population, because it might be harder for elites to form ties within a large population, while populous countries imply larger, more valuable firms for dictators to expropriate. The second demographic control I include is (log) population density, since spatial clustering might imply network clustering for elites and also larger elite influence on the dictator due to proximity. For the same reason, I control for urbanization (% of total population). Overall, density's effect on expropriation risk remains negative and statistically significant in all models, and in some models (urban) population has a significant (negative) positive effect.

Alternative Network Codings The final set of robustness checks I carry out involve changing the way I construct my elite networks from the Panama Papers. I experiment with four variations of my coding rules; two affect the ties coded, another two the nodes coded. First, I drop ties formed through offshore companies that are dissolved. In other words, I assume that elites i and j are no longer tied in year t if company k, through which the tie was formed in the data, dissolves. This coding rule assumes – perhaps naively – that dissolving an offshore company has substantive bearing on the connections of the respective elites; that the dissolution is not due to accounting, legal, or financial reasons. The second coding rule I change relates to offshore companies with the same name. Since some company names appear many times in the data (e.g. New Ventures Ltd.), I merge companies with the same name when they are incorporated in the same country (e.g. Bahamas).[72] The third change I make to my coding scheme is analogous to the second one, but applied to officers

[70] One change in my results is that density is insignificant in the fixed effects regresions that control for exchange rate restrictions.

[71] All demographic variables are from World Bank (2016).

[72] Though every company has a unique identifying number in the data, it is not clear that every number identifies a unique company. I also experiment with "fuzzy" matches of company names (e.g. merging New Ventures Ltd. with New Ventures).

(i.e. the individuals associated with each company); in particular, I merge officers with the same name and nationality.[73] Note that merging two officers reduces the number of nodes in the network by 1 and aggregates their ties, thereby increasing network density. Finally, I use different techniques to identify and drop "nominee" directors from my networks—officers contracted by the real owners of offshore companies to act as frontpeople. Namely, I purge my networks from officers that are associated with a large number of companies (I vary that threshold) or list law firms as their address. None of the above alterations to my networks substantively changes my regression results, while some merges strengthen the coefficient on density.[74]

6 Discussion

In the previous section I documented a robust statistical association between my measure of elite financial network density and various measures of expropriation and property rights in dictatorships. However, the interpretation I have given to my findings might not be unique. In this section I explore five alternative interpretations of my findings, as well as four limitations of my empirical approach.

Elite Collective Action Several prominent theories of property rights formation and power-sharing in dictatorships focus on elites' collective action problem in constraining the sovereign / dictator (North and Weingast, 1989; Boix and Svolik, 2013). As such, it is important to evaluate my findings vis-à-vis collective action theory. According to the latter, all else equal, collective action is more likely to be achieved the smaller the group in question, the greater its ability to provide selective rewards and punishments to its members, and the more unequal the distribution of expected gains within the group.[75] Interestingly, my findings are not consistent with any of these predictions. First, as noted in Section 5.2, the number of nodes (elites) in the network has a negative effect on expropriation risk—though the effect is inconsistent in sign and statistical significance, especially when I interact nodes with density in my regressions. This result contradicts a collective action interpretation, whereby a larger network of elites should be less likely to constrain the dictator and should thus experience more predation (positive coefficient). The second piece of evidence against a collective action view is that the legislature dummy in my regressions has a positive (insignificant) effect on expropriation. Given that legislatures institutionalize the selective allocation of rewards among elites, we should expect them to facilitate collective action, thereby producing a negative effect on expropriation, instead.[76] Finally, assuming income inequality is a good proxy

[73] As with company merges, I experiment with "fuzzy" matches of officer names. I also use information on officers' listed addresses, though noisy and incomplete, to match officers with the same name, nationality, and address.

[74] Officer merges by name have a larger effect on the coding of networks from East Asian dictatorships (e.g. China, Singapore) because these countries have lower-than-average variation in names. Crucially, these countries also have above-average property rights protection, thus merging officers reenforces the negative relationship between network density and exprorpiation risk in my regressions.

[75] Ideally, one agent should value the public good to be produced under collective action enough to find it individually rational to provide it herself.

[76] Note that my finding is consistent with Jensen, Malesky and Weymouth (2014) and Wilson and Wright

for elites' unequal gains from curtailing expropriation – with the wealthiest elites gaining more from constraining the dictator – we should expect the Gini coefficient to have a negative effect on expropriation.[77] Once again, my regressions are inconsistent with a collective action mechanism underpinning the effect of elite networks, as income inequality has a positive insignificant effect on expropriation.

Dictator's Encompassing Interest My theory assumes that only financially connected elites share private information on the economy, in order to protect their shared economic interests from the dictator's predation—elites that are not financially tied might be competitors with an incentive to misinform each other. Under this view, a dense financial network reflects overlapping economic interests for elites. However, it is instead possible that a dense financial network reflects the dictator's encompassing interest in the economy (Olson, 1993). That is, financial ties among elites might be the result of the dictator distributing economic resources across elites in a manner that maximizes allocative efficiency. Under the Olsonian view of the dictator as a stationary bandit, when the dictator has a larger encompassing interest (stake) in the economy he chooses policies that maximize economic output and preys less. Though this theory might seem consistent with my analysis, it faces two obstacles. First, to argue that the dictator can foster ties between elites and that *those* ties increase his encompassing interest in the economy and reduce his predation, I would have to use data on ties that the dictator controls, not ties formed through offshore companies that the dictator is not even aware of. In other words, unless onshore financial ties are identical to offshore ones, we cannot assume that the dictator shapes the ties used in my analysis.[78] Second, if the dictator can shape (offshore) ties between elites, we should expect that personalist regimes, where the dictator yields more influence, to have denser elite networks. Interestingly, personalist regimes are robustly associated with lower network density. Coupled with my other finding that personalist regimes – surprisingly – experience lower expropriation risk, it is hard to treat dense offshore elite networks as a result of the dictator's encompassing interest in the economy. Similarly, I cannot attribute lower predation in countries with dense elite networks to the dictator's encompassing interest.

Intra-Elite Incentive Alignment Another potential interpretation of my findings is that the negative effect of dense elite networks on expropriation operates mostly through the incentive alignment that financial ties create between elites. Though I naturally assume that connected elites have shared economic interests – which allow information-sharing – aligned preferences should mostly affect *horizontal* expropriation; that is, elites' predation on each other. The more elites' economic interests overlap, the larger the cost for one elite to undermine another elite's interests, so the lower the horizontal expropriation. However,

(2017), who find that legislatures have a null effect on expropriation when pooling different regime types.

[77] Admittedly, this assumption is problematic when using the Gini coefficient as a measure of income inequality, because top income earners (elites) might have the same income and still create a high Gini—if their income is a large multiple of lower income earners.

[78] Another, econometric piece of evidence against the endogeneity of my estimate of density's effect is that, if density were endogenous, its coefficient should be similar across FD and FE estimates. In fact, the coefficient for (log) density has the same sign and sufficiently similar point estimates and standard errors in Table 2, Models 1–2.

it is unclear why shared interests *per se* should limit *vertical* expropriation—by the chief executive (dictator)—measured by my dependent variable. If aligned elite incentives reduce vertical expropriation, other determinants of shared elite interests should also have a negative effect on expropriation risk. Two potential proxies of these determinants are ethnolinguistic fractionalization (ELF index) and how concentrated industries are (Herfindahl-Hirschman index)—ethnolinguistically non-diverse elites might have shared interests due to shared culture, while elites in an economy with concentrated industries will experience less market competition. Nevertheless, my regressions show a null effect for both proxy variables, giving no support to a mechanism where dense elite networks limit vertical expropriation only through aligning elites' incentives.

Vertical vs Horizontal Expropriation At this stage, it is fruitful to take the distinction between vertical and horizontal expropriation more seriously. Jensen, Malesky and Weymouth (2014) argue that vertical expropriation is limited by stronger property rights, which govern the relationship between state (dictator) and private actors (elites), while horizontal expropriation is limited by contracting institutions, which govern the relationships among private actors (elites). How do dense elite networks affect each type of expropriation? My analysis shows a robust negative effect of dense elite ties on vertical expropriation. However, I find a negative effect for network density on indexes of investor protection and rule of law, like those used by Jensen, Malesky and Weymouth (2014) to measure horizontal expropriation. These findings suggest that it is possible to extend my theory to produce a dual effect of elite networks on expropriation. On the one hand, dense elite ties reduce vertical expropriation by diffusing private information that elites use to secure selective commitments from the dictator. On the other hand, financial ties reduce horizontal expropriation by aligning elites' interests and reducing their incentive to prey on each other. I leave such an extension of my theory to future work.

Regime Insiders vs Outsiders Another competing explanation of my findings rests on the observation that the dictator does not make decisions about which elites to prey on alone; members of his regime, inner circle, or ruling/winning coalition – i.e. insiders – also participate in economic decision-making. Crucially, insiders are relatively secure from predation, compared to elites excluded from decision-making, i.e. outsiders. As such, a less simplistic and more important distinction than dictator-vs-elites might be insiders-vs-outsiders. Moreover, an insiders-vs-outsiders approach might change the interpretation of my data, since some of the financial ties I observe might be between insiders and outsiders, not outsiders and other outsiders. In other words, it is possible that the financial networks I observe align the incentives of elites excluded from decision-making – potential prey – with elites included in decision-making – potential predators, and one could interpret the negative effect of elite network density on (vertical) expropriation against outsiders as the result of financial ties binding insides with outsiders. If this mechanism underlies my findings, we might expect two implications to hold. First, density should have a weaker effect in constraining expropriation in personalist regimes. Given that insiders – other than the dictator – are fewer and less powerful in personalist regimes, we should expect insider-outsider ties to do little to limit outsiders' expropriation. Yet, to the extent that there is a consistent pattern, density actually has a stronger effect in the subsample of personalist regimes versus that of all other

regime types. The second implication we might expect if insider-outsider ties underpin my findings is for density to matter more where there are fewer elites (after controlling for population). Since, all else equal, a smaller elite implies higher chances that denser ties bring outsiders closer to insiders, we might expect a negative effect from the interaction of density with nodes. However, as noted in Section 5.2, the nodes-density interaction term has an inconsistent sign and significance across specifications. In short, there is no evidence that the negative effect of elite network density on expropriation operates through insider-outsider ties, as opposed to information diffusion among outsiders.

Reverse Causality Having explored several competing interpretations of my findings, I move on to consider four potential limitations of my empirical approach. First, I address the question of reverse causality. One might argue that lower expropriation risk cultivates trust between dictator and elites, thereby encouraging stronger financial ties between elites. There are two issues with this criticism. First, it is unclear why trust between dictator and elites would foster elite-to-elite ties, instead of dictator-to-elite ties. In other words, reverse causality would be a natural concern if my independent variable measured the density of ties in a dictator-centric network, not in a network with only elites. The second problem with the reverse causality claim is that, as argued in Section 4, the elite ties in my data are formed through offshore companies, which are unobservable to the dictator and hold assets that are harder to prey on. Thus, even if lower expropriation by the dictator cultivates trust between elites, that should be reflected in denser *onshore* ties, due to onshore assets' visibility and their vulnerability to predation. Since my independent variable does not measure onshore ties, though, it is not obvious how reverse causality can account for the negative association between dense offshore elite ties and expropriation.

Network Measurement Error The second criticism of my approach that I address is non-random measurement error in my independent variable. Given the incentives for substituting onshore with offshore financial ties outlined in Section 4, it follows that the worse the property rights are in a dictatorship the larger the share of elites' ties that will be offshore. And since my data only captures offshore ties, this implies that my networks are more representative of elite ties in dictatorships with higher expropriation risk. In other words, if the "latent" independent variable I am trying to capture is density of *aggregate* elite ties, it is possible that my "proxy" independent variable, density of *offshore* ties, is more accurately measured in countries with high values of the dependent variable, expropriation risk. Can this non-random noise account for my findings? Recall that my analysis associates higher expropriation risk with sparser elite networks. For this association to be the result of non-random noise, it must be the case that more accurate measurements of the latent independent variable produce *sparser* networks; that is, getting a clearer picture of all elite ties must produce a network with fewer ties per node. However, if anything, the opposite is likely to be true: more accurately measured elite ties should produce *denser* networks. Thus, to the extent that removing measurement error would alter my analysis, it would probably be in the direction of a stronger negative effect of elite network density on expropriation.

Offshore Assets & Ability to Punish The third potential issue with my empirical approach also relates to the use of data on offshore ties. Since the elites in the Panama Papers

have offshore company accounts, my data might be unsuitable for testing my theory's main implication. In particular, because I argue that elites extract selective commitments from the dictator by threatening to reallocate their capital, it might seem that my theory rests on elites holding most of their assets onshore—otherwise their potential economic harm to the dictator would not be sufficient to deter him from predation. This criticism has two limitations. First, as noted in Section 4, having an offshore company account does not mean that one's assets are located offshore; for example, one can own real estate in his country of residence through a company registered offshore.[79] Relatedly, even if an elite's offshore company owns assets located offshore, this does not mean that said elite does not also own assets onshore. In short, most elites in the Panama Papers are likely to have some onshore assets that they can use to threaten the dictator through punitive capital reallocation. The second issue with this criticism is that it ignores the two-way nature of my theory: elites can also extract selective commitments by promising to *reward* the dictator through their *offshore* capital. Elites can deter the dictator from preying on them by committing themselves to repatriating their offshore capital and using it to boost the economy and/or aid the dictator: investments to increase employment, public-private partnerships to develop infrastructure, participation in distributive politics, or simply transfers to the dictator. Moreover, combining the two issues raised above, if I were to use data on elites that have only onshore assets, I would risk producing a weaker test of my theory. Since they do not have offshore assets, these elites would have to incur a cost to set up offshore companies to reallocate their onshore assets, and would thus have reduced capacity to punish the dictator. Furthermore, without having offshore capital, these elites would have limited capacity to reward the dictator through capital repatriation. Overall, it is clear that my theory does not rest on elites having solely onshore assets. On the contrary, my analysis becomes more powerful vis-à-vis my theory when using data on elites that have a mix of onshore and offshore assets, like the ones in the Panama Papers.

Predation: Elites vs Foreign Firms The final potential limitation of my analysis concerns my dependent variables; namely, whether the measures I use capture predation against domestic elites or foreign companies. My main dependent variable, the price of insurance against expropriation, factors in the risk of expropriation against foreign firms, not just domestic ones. Domestic elites might have no stake in these foreign firms, and so my dependent variable might not be measuring predation against the actors I am interested in. Furthermore, as noted in Section 5.3, the property rights indexes I use are based on the judgement of mostly foreign experts, while the incidents of expropriation variables also include incidents against foreign firms. Though none of my dependent variables perfectly captures predation against domestic elites, it is reassuring that my main finding holds across all of these variables. Each variable is likely to capture a different degree and type of predation against domestic elites. Interpreting the negative effect of network density on all of these variables *collectively*, it seems reasonable to take the beneficial effect of dense networks on domestic elites seriously.

[79] Indeed, this seems to be a common use for offshore companies, according to anecdotes from classified reports on some of the Panama Papers accounts.

7 Conclusion

Recap I have argued that an informal institution—financial networks—aids elites in constraining dictators, much like formal political institutions do. The strong, cooperative ties elites form within financial networks allow them to share private information on the true state of the economy and monitor the dictator's compliance with their rent-sharing agreement. Where financial networks are denser, private information diffuses to more elites, which present the dictator with a larger credible threat if he preys on their shared rents. As such, dense elite networks deter economic predation and constrain dictators, thus strengthening property rights. To test my argument, I uncovered the structure of one type of elite financial network, ties in offshore companies, in all dictatorships post 1990, using the largest leak of information on offshore finance, the Panama Papers. After constructing these networks, I derived measures of elite density, which I used as the independent variable in my regression analysis. Controlling for a host of confounders, country- and year-level effects, temporal dynamics, and statistical irregularities, as well as using alternative dependent variables, independent variables, and rules for constructing the underlying networks, I found that dictatorships with denser elite offshore financial networks are associated with significantly lower expropriation risk. I interpret these results as evidence that, though dense financial ties between elites are usually associated with corruption, nepotism, and patronage, in dictatorships, where the main threat to property rights are unchecked dictators, a densely financial connected elite is the lesser of two evils—it counters dictators' predation.

Future Research Future research could revolve around the broader role of networks in dictatorships. Since most networks of political importance are endogenous to politics, one could develop an integrated theory on how networks' role varies, depending on such factors as the dictator's strength, institutional environment, and level of development. Furthermore, in addition to financial ties among economic elites, one could study the role of familial, ethnic, religious, or other ties. Similarly, one could extend the focus to networks among other key players in dictatorships, such as party elites in regimes with dominant parties, ruling families in monarchies, and officers in military regimes. In all of these cases, the main empirical challenge is capturing ties among the actors of interest—in a comparable way within and across countries and/or time. Thus, to test the role of networks in dictatorships researchers will have to combine innovative data collection and contextual knowledge of the cases under study with careful research design. I leave these tasks to other scholars.

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