

Research Portfolio

Hector Bahamonde

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Research Statement

Within comparative politics, my research explores the intersection between inequality and political development, from a political economy perspective.

In “[Aiming Right at You: Group versus Individual Clientelistic Targeting in Brazil](#)” (Journal of Politics in Latin America) I used **matching techniques** for observational data. Do parties target individuals or groups? This is a question fundamental to understanding clientelism, yet the literature does not offer an answer. This paper argues that depending on certain conditions, brokers target individuals when they are identifiable and groups when brokers need to rely on the spillover effects of clientelism. Both identifiability and spillovers depend on individual poverty, group poverty, and political competition. Though the theory I outline focuses on targeting, the paper also argues that structural factors, such as the density of the poor, should be considered in the vote-buying literature. Structural factors are one of the few observables upon which brokers can base their decision regarding investment in clientelism. Using survey and census data from Brazil, the paper exploits variations in personal incomes within contexts of differing levels of poverty. I find that political parties engage in segmented or ad-hoc strategies, targeting individuals when identifiability is high, and groups when there are economies of scale. Importantly, non-poor individuals can also be offered clientelism.

In addition to that, and with the support of a generous grant, I designed in **Qualtrics** two **experiments** in the U.S. out of a series of experiments to be fielded in Latin America for further comparison. In “[Still for Sale: The Micro-Dynamics of Vote Selling in the United States, Evidence From a List Experiment](#)” (Acta Politica), I looked at the tipping points at which a sample of U.S. citizens ($N = 1,479$) prefer a monetary incentive rather than keeping their right to choose whom to vote for. In nineteenth-century United States politics, vote buying was commonplace. Nowadays, vote buying seems to have declined. The quantitative empirical literature emphasizes vote buying, ignoring the micro-dynamics of vote selling. We seem to know that vote buyers can no longer afford this strategy; however, we do not know what American voters would do if offered the chance to sell their vote. Would they sell, and at what price, or would they consistently opt out of vote selling? A novel experimental dataset representative at the national level comprises 1,479 U.S. voters who participated in an online list experiment in 2016, and the results are striking: Approximately 25% would sell their vote for a minimum payment of \$418. Democrats and Liberals are more likely to sell, while education or income levels do not seem to impact the likelihood of vote selling.

The second experiment is a **conjoint experiment** and it is being analyzed in a separate paper entitled “[Vote Selling in the United States: Introducing Support Vector Machine Methods to Analyzing Conjoint Experimental Data](#)” (*in preparation*). My coauthor and myself are developing a new methodology to analyzing conjoint experiments via **machine learning** techniques, particularly, **support vector machines**. Democracy has traditionally been theorized as a multidimensional concept (Dahl, 1971). Yet, the quantitative

study of clientelism—as a democratic failure—has been mostly studied from a unidimensional perspective. We argue that to better understand clientelism, quantitative studies should situate the phenomena within the multidimensionality of democracy to understand which specific subdimension should fail to explain vote selling. By leveraging a conjoint experiment on hypothetical vote selling in the United States—a traditionally considered consolidated democracy—this paper makes both methodological and substantive contributions to the literature. Using machine learning techniques, the paper identifies that among the five conjoint attributes considered in this study, the one related to the co-dependence of the Presidency on Congress is the only one that explains individual levels of vote selling. These findings are particularly relevant to the study of American democracy given the fact that the data were collected during the campaign period where Donald Trump was elected.

Finally, and within the same project, we are currently designing an **economic experiment** in the lab. This paper explains that the literature asserts that Chilean parties no longer buy votes. While those are good news, the bad news are that we are rather ignorants about a number of other interesting, and yet, unanswered questions. First and foremost, the approach used by most quantitative scholars focuses exclusively on vote-buying. That is, parties offering to buy votes, completely ignoring the ones who sell their votes (i.e. voters). This is a rather important distinction. What would voters do if offered the chance to sell their votes? Would voters still sell their votes to their own party of preference? We developed an experiment within the experimental economics tradition. Exploiting these novel data we shed some light on these questions.

My book manuscript entitled “*Structural Transformations in Latin America: State Building and Elite Competition 1850-2010*” explains that the economic structural transformation in Latin America—the secular decline of agriculture and substantial expansion of manufacturing—imposed tight constraints on the way politics was run by the incumbent landowning class. This was a major change due to the advantage the landed elites enjoyed since colonial times. Where the expansion of the industrial sector was weak, post-colonial norms persisted due to institutional inertia, perpetuating the advantaged position of the agricultural class. Leveraging economic sectoral outputs dating back to 1900 until 2010, for a sample of Latin American countries, I use **panel data methods** (particularly, Cox-proportional hazard models), time series analyses (VAR models, impulse response functions, and Granger-causality tests), and fine-grained qualitative data to support my argument. **Another major contribution of the book** is in measurement. One of the biggest gaps in the literature is the lack of a measurement of state capacities able to capture variations of *stateness* over time. Using a novel dataset, the book proposes measuring state capacities using earthquake data. The rationale is very intuitive: the capacity to enforce quake-sensitive building codes throughout the territory is a *reflection* of the overall (*in*)capacity of states of solving both logistic and political limitations at the subnational level. *Why does a 7.0 magnitude earthquake flatten Haiti, leaving at least 100,000 deaths, while a 8.8 earthquake in Chile in the same year leaves just 525 deaths?* By exploiting variations on earthquake death-tolls and local population to weight the number of deaths since 1900, I measure state capacities over time. Keeping magnitudes constant, casualty differentials should be attributed to the *lack of state capacities*. The paper version of the book is entitled “[Income Taxation and State Capacity in Chile: Measuring Institutional Development Using Historical Earthquake Data](#)” (*in preparation*).

In one of the **sections of the book**, I explain that the structural transformation required both sectors to grow in a *balanced* fashion, *leveling both elites in their relative political, economic, and military capacities*. Leveraging the dual sector model of economic growth, I sketch a theory of political and economic development that stresses the structural economic dependence of both sectors. The agricultural sector supplied labor and cheap foodstuff—which the industrial sector demanded, promoting balanced economic development of both sectors. The political consequence of balanced economic growth was the mutual political dependence and the need of inter-elite compromises that fostered both economic and political development in the long-run. The paper version of the chapter (currently *in preparation*) can be downloaded [here](#).

Summary and Future Research In summary, my book manuscript, job market paper and working papers on vote buying and experiments on vote selling work toward exploring the effects of inequality in political development from a comparative perspective. I use a broad methodological approach, historical comparisons, time series analyses and experimental and quasi-experimental methodologies. My goal is to use this toolkit to keep asking “big” questions that are fundamental to our discipline.

My future research will seek to study the connection between elite competition and democratic regimes, exploring the connection between state building and democratic institutions. The first paper within this wave of publications is entitled “[Inclusive Institutions, Unequal Outcomes: Democracy, State Capacity and Income Inequality](#)” ([Inclusive Institutions, Unequal Outcomes: Democracy, State Capacity and Income Inequality](#). *European Journal of Political Economy*, forthcoming.). This study argues that democratic rule

combined with high state capacity produce higher levels of income inequality over time. This relationship operates through the positive effect of high-capacity democratic context on foreign direct investment and financial development. The second paper is entitled “[Skyrocketed Inequality and \(Un\)lockdown Political Elites in Chile: Aerodrome Usage during Pandemic Times](#)” (in preparation). This paper explores a novel dataset on daily arrivals and departures of a small aerodrome in Santiago de Chile, one of the most unequal countries in the world. It particularly shows how elites were able to bypass lockdown policies aimed to restrict mobility during the pandemic using a small aerodrome located in the heart of the richest municipalities in Santiago. While most Chileans were under heavy lockdown policies, economic elites were able to travel during weekends to their vacation houses and cottages located in other parts of the country. While the Chilean state deployed with some degree of success both the army and police members throughout its territory, it was not able to control air traffic (strictly used by the elites). These findings go in line with the recent “politics of weakness” literature. In fact, this paper shows how local authorities faced high political pressures to systematically ignore overseeing traffic control policies during the pandemic. The paper exploits a novel dataset, time-series methods, regression discontinuity designs and key informant interviews.

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Aiming Right at You: Group versus Individual Clientelistic Targeting in Brazil

Héctor Bahamonde

Abstract: Do parties target individuals or groups? Although this question is fundamental to understanding clientelism, the literature does not offer an answer. This paper argues that, depending on certain conditions, brokers target individuals when they are identifiable, and groups when brokers need to rely on the spillover effects of clientelism. Both identifiability and spillovers depend on individual poverty, group poverty, and political competition. Though the theory I outline focuses on targeting, I also argue that structural factors, such as the density of the poor, should be considered in the vote-buying literature. Structural factors are one of the few observables upon which brokers can base their decision regarding investing in clientelism. Using survey and census data from Brazil, the paper exploits variations in personal incomes within contexts of differing levels of municipal poverty. I find that political parties engage in segmented or ad-hoc strategies, targeting individuals when identifiability is high, and groups when there are economies of scale. Importantly, non-poor individuals can also be offered clientelism.

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There is no agreement on when, how, and why parties choose to aim clientelist practices at individuals or groups.¹ The distributive politics and vote-buying literatures have traditionally pursued one of two approaches. The former has mostly focused on group targeting, usually districts or provinces (Dixit and Londregan 1996; Khemani 2015; and Calvo and Murillo 2004), showing that incumbent parties deliver public-sector jobs or construction projects contingent on the support of groups of people. The latter has typically focused on individuals and their characteristics, such as their socio-economic or electoral profiles. Substantively, however, it is not clear when or why clientelist brokers use either strategy.

In fact, the decision to investigate group-based and/or individual-based targeting seems to be attributable to distinct research designs and agendas, rather than theory. For example, ethnographers generally focus on individuals, while others have traditionally focused on groups (Scott 1972; Auyero 2000; Szwarcberg 2013; Weitz-Shapiro 2012; and González-Ocantos et al. 2012).²

What is most concerning, however, is that it is relatively assumed or implied that individual and group clientelist targeting strategies are interchangeable, when they are clearly not. Individuals pertaining to groups and individuals by themselves have different incentives to defect to the incumbent. For instance, individuals belonging to larger groups have greater incentives to defect (Stokes 2005), while individuals who are personally targeted have fewer incentives to defect (Auyero 2000). Anticipating this, brokers adjust their strategies accordingly. In the first instance, brokers deal with low-informational environments that increase principal-agent problems. In the second instance, brokers – who know their clients better – are able to leverage this knowledge, reducing the probability of defection. However, these differences have not been sys-

1 I am grateful to Robert Kaufman, Daniel Kelemen, Richard Lau, Paul Poast, Geoffrey Wallace, Douglas Jones, Ezequiel González-Ocantos, Juan Pablo Luna, Jorge Bravo, Eric Davis, Adam Cohon, Edwin Camp, Luciana Oliveira Ramos, Giancarlo Visconti, William Young, Johannes Karreth, and the reviewers and editor of *JPLA*. I also thank participants of the Latin American Studies Association 2014 conference, the Southern Political Science Association 2015 meeting, the Western Political Science Association 2015 meeting, and the 2014 Graduate Conference at the Political Science Department, Rutgers University. Any errors that remain, of course, are my responsibility. This work was partially funded by the Center for Latin American Studies at Rutgers University. I am grateful to the School of Arts and Sciences and the Department of Political Science for their travel grants.

2 I wish to thank Ezequiel González-Ocantos for this suggestion.

tematized in the literature. In the present paper, I propose a framework that explains when it is more efficient to target groups or individuals.

Particularly, by focusing on brokers, the paper advances an argument about the decision process regarding whom to target. The crux of the argument is that this decision is a function of three factors: individuals' discount factors explained by income levels, the incentives of clientelist brokers to rely on spillover effects caused by the nesting structure of individuals (that is, whether individuals are nested in poor or non-poor contexts), and brokers' incentives to engage in clientelism explained by higher electoral pressures and political competition.

Overall, I share Carlin and Moseley's (2015: 14) opinion that "[e]xisting research looks almost exclusively at individuals' socio-economic and, specially, electoral profiles [and] [y]et our knowledge of who parties target remains incomplete." The present paper seeks to contribute to this issue by incorporating both structural and individual factors that foster clientelism in the same theory. Analytically, the structure of the argument (and the empirics) allows for disentangling the effects of "being poor" and "living in a poor area." Another important implication of the argument is that I am able to suggest why parties that adopt clientelism as a strategy, target their resources to both poor and non-poor individuals, an empirical regularity that, to the best of my knowledge, has been unexplored so far.

Perhaps the area in which there is the most agreement among scholars is on the relationship between poverty and vote-buying (Calvo and Murillo 2004; Weitz-Shapiro 2012; Kitschelt 2000; and Kitschelt and Altamirano 2015). For example, Brusco, Nazareno, and Stokes (2004), Stokes et al. (2013), and Nazareno, Brusco, and Stokes (2008) explained that since the poor derive more utility from immediate transfers than the uncertain returns associated with future policy packages, clientelist political parties only target the poor. In fact, Weitz-Shapiro explained that "[a]lmost universally, scholars of clientelism treat and analyze [this] practice as an exchange between politicians and their poor clients" (Weitz-Shapiro 2014: 12; my emphasis).

However, this canonical predictor has recently been contested (Hicken 2007: 55). Szwarcberg (2013: 32) "challenges the assumption [that brokers] with access to material benefits will always distribute goods to low-income voters in exchange for electoral support," while González-Ocantos et al. (2012) and Holland and Palmer-Rubin (2015) found that income (measured at the individual level) had little or no effect on vote-buying. In fact, Figure 1 shows that non-poor individuals in Brazil did receive clientelist offerings. *Why would brokers target non-poor individuals?*

And relatedly, *why does contemporary scholarly work report null findings for poverty, traditionally the most important predictor of vote-buying?* I present an argument where individual income alone is not relevant (similarly, see Weitz-Shapiro 2012: 568). What matters is how noticeable individuals are. Wealthier individuals living in poor contexts and poor individuals living in non-poor contexts are more identifiable, increasing their respective probabilities of being targeted. I also contend in this article that, in low-information environments, brokers use these kinds of observables to reduce the probability of defection of their clientele.

Another often-considered contextual factor in the literature is the size of the community in which clientelism takes place. Large-sized communities impose severe principal-agent problems. Stokes (2005: 323) explained that the “community structure” mediates the incentives to defect. Large communities make voters more anonymous, increasing their probability of defection. In fact, Rueda (2017: 164) found that in Colombia vote buying is more effective in contexts of small polling places.

Figure 1. Individual Wealth and Vote-Buying in Brazil



Note: Following the advice of Córdova (2008) and Córdova and Seligson (2009, 2010), different socio-economic variables in The Latin American Public Opinion Project (2010) dataset were used to construct a relative wealth index (RWI). With this information, in addition to the frequency of clientelism question (*clien1*), the figure shows that clientelist brokers target individuals at all levels of income.

Several scholars have then argued that brokers prefer smaller groups because individuals nested in small communities should defect less (Brusco, Nazareno, and Stokes 2004; Kitschelt and Wilkinson 2006: 10; and Magaloni 2008: 67. See also Bratton 2008, for Nigeria, and Gingerich and Medina 2013: 456, for Brazil). Yet, even when brokers might prefer to target small communities (with fewer voters relative to large communities), it is not clear how political parties gain enough electoral returns, especially considering that clientelism is expensive.

Vote-buying is an expensive strategy (Zarazaga 2014: 35), and more so when clients are individually targeted.³ Stokes (2005: 317) argued that brokers develop skills that allow them to infer whether individual clients in small-sized communities voted for their party by looking at them in the eyes. Gay (1993, 1998) documented similar findings for the Brazilian case. This strategy requires brokers to sustain close relationships over time with their clients in a personal and individualized way. Knowing the clients' needs, delivering them benefits, monitoring their political behavior (and punishing them in case of defection), all in an individualized fashion, makes this strategy an extremely expensive choice – and it becomes even more expensive as more individuals are added to the broker's portfolio.

The cost of individual targeting increases linearly with the size of the targeted population (Hicken 2007: 56). This intuition is important because the brokers' production-possibility frontier cannot be shifted upwards either. Since the number of brokers is a depletable resource, at some point party machines run out of brokers, implying that monitoring capacities are bounded. In fact, Auyero (2000: 74) explained that the capacity brokers have to deliver benefits is "finite," and "only for a restricted number of people." However, and despite this constraint, brokers still have incentives to secure a large number of votes. Yet, the literature explains that clientelism should decrease in large communities. However, it is hard to conceive that brokers will stop being clientelist just because the size of the population is large. *A priori*, it seems a missed opportunity for brokers to let go of a large number of votes. In fact, survey data for the Brazilian case indicate that inhabitants of large, medium, and small municipalities are targeted in virtually the same proportion (Speck and Abramo 2001: 2). This article explains that when

3 Dixit and Londregan (1996: 1147) explained that brokers track "constituents' likes and dislikes, *compulsively* participating in a spectrum of events [such as] baptisms and bar mitzvahs, weddings and funerals [and even, holding] *daily* meetings with constituencies [even] *after* nine o'clock [hearing] what anyone wished to tell [them]" (My emphasis).

brokers need to secure large amounts of electoral support, especially when political competition is high, they turn to group-targeting strategies, relying on the spillover effects of clientelism. In these contexts, clientelism mobilizes electoral support from “actual” and “potential” beneficiaries, minimizing the costs of clientelist targeting while maximizing electoral benefits, a mechanism that I explain later on in the paper.

Civic associations might help solve some of the challenges large-sized groups present to brokers. As low-information environments prevent brokers from really observing individual electoral behavior (Zarazaga 2014: 35), they usually resort to alternative methods that allow them to make safer inferences. For example, Schaffer and Baker (2015: 1094) argued that clientelism is “socially multiplied” as party machines target individuals “who are opinion-leading epicenters” in informal situations, or “partisan networks” (Calvo and Murillo 2013), in what has been called “organization buying” (Stokes et al. 2013: 250–251).⁴ If parties buy “turnout” (Nichter 2008), then they will most probably target associations too, as “citizens immersed in clientelist networks [...] have a higher probability of voting than the rest” (Carreras and Castaneda-Angarita 2014: 7). I acknowledge the positive relationship between group membership and clientelism. However, what has not been explored yet is whether clientelism is explained by association membership itself, or by the fact that poor individuals usually address their problems as a *group*, since otherwise it would be too costly to solve them individually. If this is the case, group membership should be spuriously related to clientelism. While I find that group membership does have a positive effect on clientelism, I find that structural contexts that foster group-targeting have even more explanatory power.⁵

Moving forward, Weitz-Shapiro’s (2012) important paper found that in several Argentine municipalities, higher levels of political competition and low socioeconomic levels fostered higher levels of clientelism. In her paper, losses are conceptualized in terms of “moral costs.” Evidence for these types of costs has been presented in the literature very recently. For example, Carlin and Moseley (2015) argued that citizens

4 Holland and Palmer-Rubin (2015: 16) explained that when “parties lack their own brokerage networks [they seek] to capitalize on organizational networks instead.” Similarly, Rueda (2015: 13) argued that parties tend to target very specific civic associations of “seniors and associations of single mothers, organizing trips to recreational centers outside the city where all their expenses are covered.” Paradoxically, the stronger the civic society (that is, the more organized it is), the more clientelism there is.

5 These results are presented in Figure A4.

endowed with more democratic values feel more “moral repugnance” to clientelism. Vicente (2014) showed that vote-buying practices have an “immoral/illegal connotation,” while González-Ocantos et al. (2012) found that individuals wanting to avoid social stigma usually do not give truthful answers when asked directly about clientelism. Building on this literature, I contend that when political competition is high, clientelism will be higher in contexts where poor individuals live in poor economic contexts.

When Do Parties Target Individuals and When Groups?

Table 1 presents four ideal types in four quadrants; cases where individuals are highly identifiable; that is, non-poor individuals living in poor areas (Q1), and poor individuals living in non-poor areas (Q4). Identifiability in these cases reduces the cost of defection, permitting clientelist brokers to closely target individuals. While individual targeting is more expensive, it is also safer (compared to group targeting). The table also shows cases where individuals are hard to identify; that is, poor individuals living in poor areas (Q2), and non-poor individuals living in non-poor areas (Q3). In these cases, voters are more anonymous, making direct individual-based targeting and monitoring more costly. Since brokers still have incentives to seek electoral support, they engage in group targeting by relying on the spillover effects of clientelism. In these cases, the effects of vote-buying disseminates by mobilizing targeted voters and latent untargeted (but potential) clients. This form of targeting is cheaper but more uncertain.

Table 1. Strategy Set: Group versus Individual Targeting

	Non-Poor Individuals	Poor Individuals
High Competition	Poor Areas, identifiable, individual targeting.	Poor Areas, spillover effects, group targeting, cheap vote-buying.
Low Competition	Non-Poor Areas, group targeting, expensive vote-buying, lack of checks and balances, embezzlement.	Non-Poor Areas, identifiable, individual targeting.

Source: Author's compilation.

Individual Targeting

This is the safest bet a broker can make, but also the most expensive one, as it requires brokers to have sustained closed relationships with their clients. For instance, Zarazaga (2014: 26) stated that “brokers have detailed information about their neighborhood and clients’ needs.” Keeping track of every single client (and their respective needs) is an expensive strategy. After all, as Auyero (2000: 73) put it, brokers are “problem solvers.” Importantly, the kind of care given ranges from material needs to symbolic and immaterial necessities, making clientelism a relationship based on “trust, solidarity, reciprocity, caring, and hope.” Such broker-client symbiosis is both material and personal-intensive, making it very costly. As an investment, however, it pays off electorally. The same detailed information brokers have about their clients’ needs is then used to infer coercively (or know directly) the electoral behavior of their respective clientele, administering punishments or rewards accordingly (Stokes 2005: 317).

The transaction costs of clientelism are reduced by targeting identifiable clients. In 2009, Luna et al. (2011) made extensive participant observations in several campaigns, accompanying a number of candidates for several months in their campaigns for the legislative election in Santiago de Chile. With one incumbent, we spent considerable time on the ground, traveling in her district. On several times, as we drove throughout the district in her personal car, the candidate was able to recall who the head of household was (including his/her name), what her district office had contributed to solve their needs, and whether the household members were on good terms with her.⁶ Importantly, the economic diversity of the district provided a number of useful observables. In non-poor areas, poor houses with an unpainted wall, a rusty front yard fence, a two-story house with a bodega market on the first, a household with a broken window, or a junk diesel truck aground in the front yard, among others, provided distinctive points of reference. Identifiability, as an observable, made these receivers less anonymous, raising their cost of defection and making them more prone to cooperate. Table 1 portraits individuals living in these heterogenous contexts in Q4.

Households in Q4, being more noticeable, stand out in their respective contexts, making it easier for brokers to notice whether they need construction materials, whether there are wakes to which they could contribute flowers or birthday parties to which they could bring cakes. In

6 The actual gender of the candidate might have been changed for confidentiality purposes.

addition, it makes their possible defection more obvious and memorable for the brokers. In summary, higher levels of visibility supply brokers with good-quality information about their clients.⁷ In addition, when political contestation is low, the demand for votes is less astringent, shaping brokers' incentives to target in a more accurate, less massive fashion, identifiable and particularized individuals, not groups.

The capacity brokers have to identify potential clients not only comes from third-party sources, as the "organization buying" proponents explain (Holland and Palmer-Rubin 2015; Rueda 2015 and Stokes et al. 2013). In a similar account, others have pointed out that brokers are also "reliable neighbors" (Zarazaga 2014: 38); that is, members of the same community of targeted individuals. Acknowledging this approach, the argument presented in this article contends that brokers have incentives to expand their immediate local networks by colonizing visible targets outside of their own proximate neighborhood. By conceptualizing brokers as active political entrepreneurs who seek new supporters outside of their immediate context, the proposed framework complements other accounts, as presented in Szwarcberg (2013: 32) or Zarazaga (2016: 681), where brokers are neighborhood party agents. Clientelist entrepreneurship can be performed directly or indirectly. For instance, Auyero (2000: 65–66) described the situation of Cholo, a member of the inner circle of one of the brokers in Buenos Aires, Argentina, who visited "other poor neighborhoods of the area adjacent to" the place where the broker (and himself) lived, to spread news about some government plan, the governor, and the Peronist party, but importantly, also reporting to the broker any unattended material needs he had noticed. This illustrates how, via different channels, brokers expand their client portfolio outside of their immediate community.

An important implication is that individual poverty does not play a role by itself. Non-poor individuals living in poor areas (Q1) are also noticeable, and consequently, possible targets as well. Political competition shifts the demand for votes upwards. As elections become more contested, brokers need to secure even higher levels of electoral support. Since newly elected representatives are more likely to bring new people to their machines, brokers are also interested in seeing their candidates elected. Consequently, brokers will have even more incentives to engage in clientelism when political competition is high. In these cases, political competition is high enough to even mobilize non-poor individuals in a

7 Importantly, poor households do not need to be close to each other, but visible enough.

clientelist way. Since these votes are more expensive to purchase (given decreasing marginal utility from income, see Stokes 2005: 321), this strategy is less preferred. However, costly clientelism is worth the investment given the risk of losing the election.

Group Targeting

This is the least accurate targeting strategy, but also the cheapest one available to brokers. It leverages the spillover effects provided by larger concentrations of individuals who share the same socio-economic backgrounds. This strategy is less accurate because it mobilizes electoral support from “actual” clients (individuals who have actually been targeted), and “potential” clients (individuals who have not received benefits yet). It is preferred when poor individuals are nested in poor areas (Q2), or vice-versa (Q3). In these cases, individuals are masked by their environments, which means that identifiability is hard to achieve. As explained before, identifiability facilitates individual targeting, an important factor in reducing the probability of defection of targeted clients. When individuals are hard to identify, however, individual targeting becomes prohibitively expensive. Yet, brokers who still need to secure electoral support do not opt out of clientelism and instead turn to group targeting.

Auyero (2000: 65) described the case of Alfonsina in Argentina. Alfonsina was part of the broker’s inner circle and got a job as a cleaning lady in a public school. As the broker explained to her, before getting the job, Alfonsina had to be *patient* because as a member of “the circle,” she was in the pool of potential beneficiaries; it was only a “matter of time” until she could get the job. The idea of expectations and hope are important. Auyero explained that the

hope of a job serves as important glue within the inner circle. Although not everyone is employed at the municipality, the fact that someone gets [a] job has an important *demonstration* effect. (Auyero 2000: 65; my emphasis)

Building on this intuition, two ideal types are suggested: actual and potential beneficiaries. The former receive particularistic benefits “today” and vote for the broker’s candidate “tomorrow,” while the latter do not receive benefits “today” (in the expectation of receiving them in the future) but still vote for the broker’s candidate “tomorrow.”

Group targeting is cost-effective because it mobilizes two types of voters at the cost of investing in just one (i.e. the “actual”). Actual beneficiaries want to remain actual beneficiaries since they want to keep re-

ceiving benefits; thus, they keep supporting the broker's candidate. In turn, potential beneficiaries want to become actual beneficiaries, but are uncertain when that might happen; as a result, they also support the broker's candidate. In this sense, from the broker's perspective, this strategy reduces the sunk costs by half, multiplying the gross benefits by two. In other words, the broker's reputation of a "problem solver" disseminates twice as fast relative to individual targeting. It is in this sense that this is a massive (but less precise) form of clientelist targeting.

Given that potential clients support the broker's candidate in the absence of current inducements, brokers need to effectively calibrate the timing when potential beneficiaries become actual beneficiaries. In other words, brokers need to infer the discount factors of their potential clients, making it expensive for them to defect. Reputation, as a form of capital, is fundamental for brokers since "voters prefer to support [brokers] with a reputation for delivering because they are a more reliable source of future rewards" (Zarazaga 2014: 24). However, potential clients are also interested in investing in their reputation. From their perspective, they know that the flow of resources is dependent on the brokers' electoral success. Also, they do not know whether new brokers might have access to fewer resources or distribute them to other people. For them, the cost of switching brokers (or defecting) is very high since it also involves building relationships of confidence with another broker from scratch, which is costly. Hence, the incentives are for the broker to deliver benefits before it is too late, while the incentives for the potential client are to support the broker's candidate.

Since it does not matter what type an individual is, both actual and potential beneficiaries keep voting for the broker's candidate. While cost-effective, group targeting is less accurate since brokers hope to mobilize potential beneficiaries only indirectly; that is, by targeting actual beneficiaries. This makes this strategy a fragile one. However, besides the reputation costs described above, low-income voters have additional incentives to support the broker's candidate. This is described in Q2. Given that the poor are risk-averse, potential beneficiaries are better-off waiting (and voting for the broker's candidate) than defecting. In the same vein, but on a slightly different subject, Magaloni (2008: 20) posited that the Mexican PRI lasted as long as it did not because of electoral fraud but because voters supported the "known devil." Economic underdevelopment played a fundamental role in this equilibrium as well. Finally, higher levels of electoral contestation force brokers to engage in this less accurate, but massive form of clientelist targeting, leveraging (1) the incentive structure of potential clients to support the candidate even in the ab-

sence of current inducements, and (2) the higher levels of risk aversion poor individuals have.

Importantly, vote-buying is also targeted to non-poor individuals nested in non-poor groups (Q3). Vote-buying has decreasing returns to scale in non-poor individuals. That is, wealthier individuals derive fewer advantages from a bag of rice relative to poorer individuals (Kitschelt 2000). Anticipating this, brokers will not offer the same benefits to wealthy individuals, but will customize the type of offerings. This distinction is important, since most of the literature assumes that clientelist practices decrease when individual incomes rise. However, that approach does not explain the counterintuitive empirical regularity depicted in Figure 1; that is, non-poor individuals get targeted too. *Why are non-poor individuals targeted?* This article seeks to contribute to the literature by explaining that brokers make their offers more attractive to non-poor individuals by offering goods that are relatively more expensive. This is more likely when districts are wealthier.

While buying votes from non-poor individuals costs more, brokers in non-poor areas have more resources to spend. Along the same lines, Hicken (2007: 55) questioned the implicit assumption that the broker's vote buying funds remain fixed; stating that "a candidate's capacity to buy votes increases commensurate with increases in average incomes." In other words, higher levels of economic development not only raise personal incomes, but also shifts the broker's vote-buying capacities upwards. Similar evidence has been found in the Philippines (Schaffer 2004). The link between higher incomes and vote buying is particularly relevant for Brazil, since its electoral laws allows political parties to get *unlimited* funds (Abramo and Speck 2001: 14), enabling brokers greater capacities to buy more expensive votes.

Besides having more resources to spend, brokers in politically uncontested districts have fewer political constraints, facilitating the spending of expensive clientelism. In Q3 it is suggested that lower levels of political contestation allow brokers to spend on more expensive means of clientelism. Uncompetitive districts lack proper *de facto* mechanisms of checks and balances, giving local incumbents more "room to move," allowing them to divert local resources into more expensive means of targeting. I call this "embezzlement clientelism." Given these relatively more expensive costs, however, I expect this form of clientelism to be less frequent. In a dynamic similar to Q2, potential clients also support the broker's candidate, hoping to become actual beneficiaries. However – and unlike poor clients in Q2 – non-poor clients in Q3 (both actual and potential) have smaller discount factors. That is, non-poor individ-

als – given their relatively higher incomes – have more “patience.” This is especially important for brokers. In practice, potential clients’ timing constraints are more elastic, putting less pressure on brokers to deliver benefits in the short run.

Case Selection, Research Design, and Data Analyses

I. Data

This section empirically tests the theoretical proposition stated in Table 1 – that is, the combined effects of individual income, of being nested in poor/non-poor communities, and being exposed to different levels of political competition – on receiving clientelist benefits. Brazil is a good case because its poverty structure is such that it is possible to find low-income individuals nested in non-poor areas (and vice versa). This case is also interesting from an institutional perspective. The Brazilian electoral system incentivizes clientelism. Several factors such as multimember districts with open lists, and the institution of the *candidato nato*,⁸ “clearly [makes] Brazil one of the most personalistic systems of democratic governance” (Kitschelt and Altamirano 2015: 257), which might foster higher levels of clientelism. In fact, Gingerich (2014: 290) found that vote-buying drastically changed electoral results, concluding that “[v]ote brokerage can still pay electoral dividends in contemporary Brazil.”

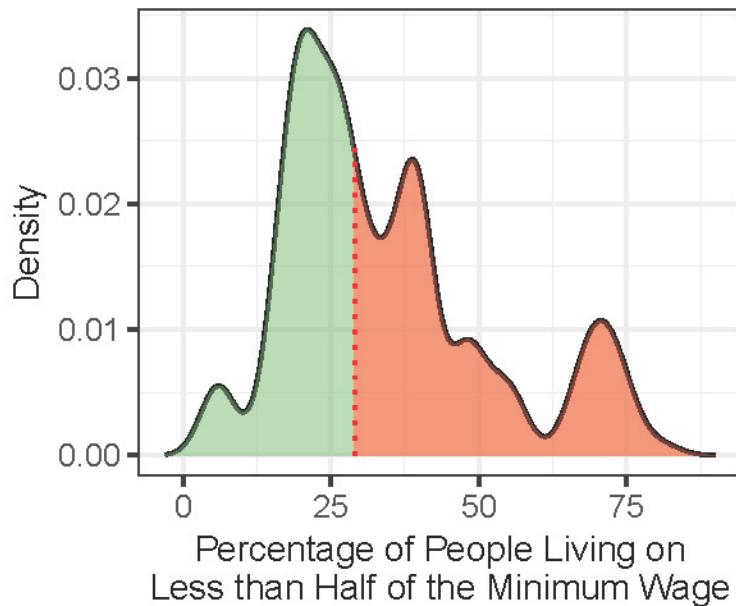
To test this hypothesis, I use survey data from 2010 from The Latin American Public Opinion Project (LAPOP) (2010).⁹ Though the LAPOP survey provides a question for income, people who are somewhat better off than their neighbors but live in poor areas may not “feel” poor. If this is the case, it could confound the results. Additionally, when answering the questioner, individuals might not want to reveal their true incomes (either because they are too low or too high). Following the advice of Córdova (2008) and Córdova and Seligson (2009, 2010), a

8 “[R]ule that removed parties’ control over the nominations process and let an electoral legislator decide to run on any party ticket.” See Kitschelt and Altamirano (2015: 257).

9 “I thank the Latin American Public Opinion Project (LAPOP) and its major supporters (the United States Agency for International Development, the United Nations Development Program, the Inter-American Development Bank, and Vanderbilt University) for making the data available.” The sample consists of five strata representing the five main geographical regions of Brazil. Each stratum was further sub-stratified by urban and rural areas.

relative wealth index (RWI) was constructed (see also Santos and Villatoro 2018). Using principal component analyses, the index measures wealth based on actual assets weighted by how common these assets are. Different indices were constructed for urban and rural contexts. Figure 1 plots the distribution of the index.

Figure 2. Distribution of the Density of the Poor



Note: Employing Brazilian census data from the IBGE (2010), the figure shows the percentage of individuals who live on less than half of the minimum wage in a given municipality. While individual income is measured using the relative wealth index (in Figure 1), the variable plotted here is used to measure economic development at the group level. Due to statistical reasons explained in this paper, the variable had to be dichotomized at its median (29 percent). However, in separate statistical analyses shown in Table A3 (weighted model), the variable is used without dichotomizing it, showing the same results.

II. Main Variables of Interest

To measure economic development at the group level, I constructed a variable that I call “the density of the poor” following a strategy similar to that of Weitz-Shapiro (2012). The variable, which is plotted in Figure 2, measures the degree of poverty at the municipal level. Using information from the 2010 Brazilian census,¹⁰ a semi-continuous variable was constructed to measure the percentage of individuals who live on less than half of the minimum wage in a given municipality. Given that the

¹⁰ Official data comes from the Bureau of Statistics of Brazil IBGE.

municipality of residence for each individual in the LAPOP survey is recorded, I was able to merge the census percentage with the LAPOP dataset. It is important to stress that the unit of analysis is the individual, and that this variable captures the economic context in which each individual lives. Just like other scholars in the past have tested the effect of being nested in rural areas,¹¹ this paper focuses on another class of contextual variable. Although the density of the poor group was originally a semi-continuous variable (that is, a percentage), it had to be dichotomized at the median (29 percent) to be able to construct a matched sample, which I justify and explain below. Figure 2 shows the continuous distribution dichotomized at the median (dotted line).

Finally, to measure political competition, I again follow Weitz-Shapiro (2012). Using official electoral data from the 2008 municipal elections,¹² I constructed a variable that measures the percentage of seats that are not controlled by the mayor's party in a given municipal council.

III. Matched Design

There is a built-in lack of relationship between “being poor” and “living in a poor municipality,” confirming that Brazil is in fact a good case to test this theory. Figure A1 in the Appendix shows that the unmatched/raw dataset already has embedded low levels of correlation between these two variables ($r = -0.44$).¹³

I was able to break this relationship down further using matching methods. Matching is a two-stage process. In the first stage, the analyst “preprocesses” the data, seeking to break any systematic relationship between, in this case, the density of the poor and the relative wealth index RWI (Ho et al. 2011). Matching does so by deleting observations for which similar observations cannot be found.¹⁴ The idea is to obtain a good covariate balance, as in Figure A3 (in the Appendix), to then estimate any appropriated statistical model.¹⁵ From a statistical standpoint,

11 See, for example, Brusco, Nazareno, and Stokes (2004) and Stokes (2005). Both studies used the log of population, which is a proxy for urban/rural.

12 Data from the Tribunal Superior Eleitoral.

13 The figure shows that, for both the matched and raw datasets, “being poor” and “living in a poor municipality” are not confounded, as it is possible to find poor individuals living in non-poor areas, and vice versa.

14 The final procedure matched 761 individuals living in the low-density poverty condition with 676 individuals living in the high-density poverty condition.

15 The idea is that the propensity of being exposed to the “high” density of the poor condition (or “propensity score”) has a similar distribution in both “treat-

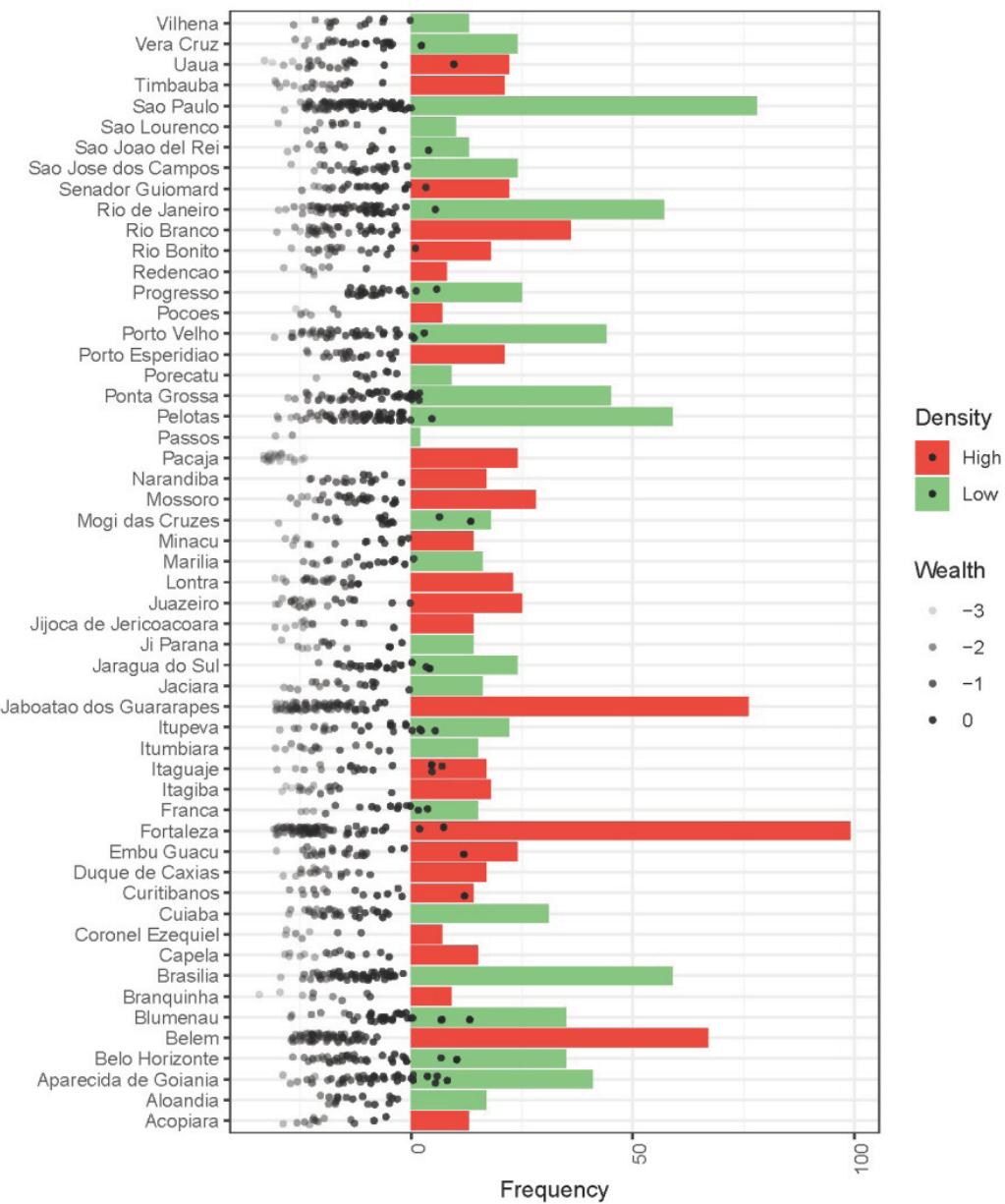
preprocessed datasets are less model-dependent (Ho et al. 2007),¹⁶ and prevent analysts from making extreme counterfactuals.¹⁷ The preprocessed data used in the matching approach has 54 municipalities, while the raw data used in the generalized propensity score (GPS) approach (which I explain below) also has 54. Figure 3 lists the municipalities and shows which ones are considered “high” or “low” in terms of the density of the poor after the dichotomization process. The figure also shows that there is considerable variance in income/RWI in both high- and low-poverty density conditions (bubbles).¹⁸

One could argue that dichotomizing the density of the poor variable at the median is an arbitrary decision. While there have been theoretical advances regarding general treatment effects regimes for continuous or semi-continuous response doses (Imai and Dyk 2004; and Hirano and Imbens 2004), algorithms with the ability to match on continuous treatment variables are not common. In order to obtain covariate balance in a non-parametric way (as matching does) but without dichotomizing the density of the poor, I also use the original (that is, continuous) density of the poor variable to construct a generalized propensity score GPS (Imbens 2004; Guardabascio and Ventura 2014; and Imai and Ratkovic 2014).¹⁹ The score is used to weight each observation in the model.²⁰

ed” and “control” groups. It is important to note that, despite the language, I do not claim any causal relationship in this paper.

- 16 Table A2 and Table A1 in the Appendix provide summary statistics for both the matched and raw datasets. Tables were generated using the *stargazer* R package. See Hlavac (2015).
- 17 King and Zeng (2005). The matching routine used was the *full* matching routine (Hansen 2004; Rosenbaum 2010), via the *MatchIt* R package (Ho et al. 2011).
- 18 Figure A2 in the Appendix shows the frequency of individuals by municipality in both raw and matched datasets.
- 19 To generate the weighting vector, I used the *CBPS* R package (Fong et al. 2018).
- 20 Besides matching on and weighting by the RWI index, I also included the following variables to match on/weighting by: municipal opposition, municipal population and individual involvement in civic associations.

Figure 3. Distribution of Observations by Municipality, Wealth Index and Density of the Poor



Note: The figure shows the municipalities in the analyses (matched set). For every municipality, the figure shows (1) the number of inhabitants (Y-axis), and (2) whether the municipality is considered having a high or low density of the poor. High-density municipalities have more than half of their inhabitants living on less than half of the minimum wage. The figure also shows (3) individual wealth indexes (bubbles).

IV. Model Specification

The dependent variable is clientelism. To measure it, I use the question that asks if a candidate or someone from a political party offered the respondent something, like a favor, food, or any other benefit or thing in return for her/his vote or support. Subjects could answer that this had happened often, sometimes, or never. Carreras and Irepoğlu (2013) and Holland and Palmer-Rubin (2015) used the same dataset and outcome variable. As they explained, the question did not ask whether respondents took the offer, hence it should not be an important source of social desirability bias (González-Ocantos et al. 2012). For statistical and substantive reasons, I dichotomized this variable, combining the alternatives often ($n = 91$) and sometimes ($n = 150$), leaving never ($n = 1,196$) unchanged.²¹

The following control variables were considered in the statistical analyses. Perception of corruption was included to hold constant the effect of respondents who declared clientelist activity when in reality they were referring to corruption scandals.²² Brokers usually target civic associations. Following Holland and Palmer-Rubin (2015: 28), an additive index to measure civic participation (Political Involvement) was created.²³ Some studies have also found group size to be important (Stokes et al. 2013). A variable to measure population size at the municipal level was constructed using Brazilian census data.

Following the convention in statistical studies of clientelism, an urban/rural dummy was also included. Some have argued that parties target their own supporters (Dixit and Londregan 1996, and Cox and McCubbins 1986), moderate opposers (Stokes 2005), or unmobilized supporters (Nichter 2008). To keep these effects constant, a variable to capture party identification (Political Id.) was included. Higher levels of democratic support should be negatively associated with clientelism. To control for that, a variable measuring democratic support was included. González-Ocantos, Kiewiet de Jonge, and Nickerson (2014) found that schooling plays a negative role on clientelism; hence, I control for education too.

21 These numbers come from the matched dataset.

22 I thank Cesar Zucco for this suggestion.

23 This variable was constructed by adding the frequency of attendance at religious meetings, community improvement meetings, and political party meetings (variables $\phi 6$, $\phi 8$ and $\phi 13$, respectively).

V. Functional Form

Observations are clustered on a number of important factors such as levels of municipal political competition, municipal poverty, and municipal population size. In order to account for these clustering effects, I use a “generalized estimating equations” approach. GEE were introduced by Liang and Zeger (1986) to fit clustered, repeated (that is, correlated), and panel data. This method is especially efficient when the data are binary (Hanley et al. 2003). GEE models are similar to random effects models (Gardiner, Luo, and Roman, 2009), in that they allow observations to be nested in hierarchical structures. This method requires analysts to parameterize the working correlation matrix. While Hedeker and Gibbons (2006: 139) stated that “the GEE is robust to misspecification of the correlation structure,”²⁴ Hardin and Hilbe (2013: 166) pointed out that “[i]f the observations are clustered (not collected over time), then [...] the exchangeable correlation structure” is the most appropriate working correlation matrix. Given that the data do not follow a panel but rather a clustered structure, the “exchangeable” correlation matrix was specified in all models.

While this method is very flexible, GEE estimates remain uninterpretable in practice (Carlin et al. 2001), making regression tables useless from a substantive standpoint. In this case, the problem is even more severe due to the interactive nature of the hypothesis being tested in this paper, which is a parameter for the multiplicative term between the variables wealth index, political competition, and density of the poor.²⁵ Methodologists agree about “not interpret[ing] the coefficients on the constitutive terms,” as they lack substantive meaning (Brambor, Clark, and Golder 2006: 77). These problems become more complex when it comes to generalized models, as a number of challenges arise.²⁶ Given that cross-partial derivatives are not advisable either, simulation methods

24 Carlin et al. (2001: 402) argued that “[r]elatively minor differences in estimates may arise depending on how the estimating equations are weighted, in particular within the generalized estimating equation (GEE) framework.” Westgate and Burchett (2017) and Gardiner, Luo, and Roman (2009, 227) made the same point.

25 Brambor, Clark, and Golder (2006: 74) offer the same advice.

26 As Ai and Norton (2003) explained, “(1) the interaction effect could be non-zero, even when the estimation says it is zero, (2) the statistical significance of the interaction effect cannot be tested with a simple t-test on the coefficient of the interaction term, (3) the interaction effect is conditional on the independent variables, [...] and (4) the interaction effect may have different signs for different values of covariates.”

are required (Zelner 2009). In particular, I follow the simulation approach introduced in King, Tomz, and Wittenberg (2000). This procedure samples via simulation from the point estimates, generating a new and larger distribution. In more detail, taking the single estimated parameters (that is, the regression coefficients), I constructed a distribution of estimated values for each coefficient. Relying on the central limit theorem, with enough sampling draws, the new simulated distribution is a transformation that approximates with a great degree of precision the (uninterpretable) coefficients. Subsequently, means and uncertainty measures can be constructed for each of these distributions. From a substantive standpoint, simulation methods also allow for the sampling of new distributions at different values of the independent variables. This will be important in simulating the expected value of clientelism for different “profiles,” such as non-poor individuals nested in high-poor dense municipalities in contexts of high political competition, among other profiles.

Since it is “impossible to evaluate conditional hypotheses using only the information provided in traditional results tables” (Brambor, Clark, and Golder 2006: 76), I have focused instead on the substantive results from the simulation methods. However, I still present the raw results in Table A3 in the Appendix.²⁷ Analogous to Table 1, in Figure 4 I simulate the predicted probabilities of being targeted using both the matched and weighted/GPS models. The horizontal panel depicts simulations for the “upper” (“non-poor,” 75 percent) and “lower” (“poor,” 25 percent) quartiles of the wealth index. In turn, the vertical panel shows the simulated values for the maximum (100 percent) and minimum (43 percent) values of the municipal opposition index. Each quadrant shows simulations for individuals nested in poor municipalities (high density of the poor), and non-poor municipalities (low density of the poor). Each profile shows two simulated probability distributions (with 95 percent confidence intervals): one for the matched sample, and one for the weighted/GPS model.²⁸ The idea is to show that the decision of dichotomizing the density of the poor variable at its median gives substantively

27 Table generated via the *texreg* R package. The first column shows the estimates for the matched dataset, while the second column shows the results for the GPS-weighted model. Virtually all coefficients have the same size and sign.

28 In the case of the weighted/GPS model, which does not use the dichotomized variable, I use the continuous version of the size of the poor variable, where “low density” represents the lower quartile while “high density” represents the upper quartile.

exact results than using the continuous version of that variable via the GPS analysis.

VI. Results

All quadrants in Figure 4, regardless of the approach used,²⁹ suggest that brokers engage in individual targeting when individuals are identifiable, and in group targeting when brokers need to rely on the spillover effects of clientelism.

In Q1, clientelism is more likely (with a 26 percent probability) in situations where non-poor individuals are nested in poor groups (i.e. where the density of the poor is “high”)³⁰ and living in electorally contested municipalities. As I have argued, these types of individuals are still targeted because they are more identifiable. For instance, a similar individual (same quadrant) who is nested in a non-poor group (“low” density of the poor), and consequently harder to identify, has a much lower probability of being targeted (7 percent). Similarly, individuals in Q4, such as poor individuals nested in non-poor areas (“low” density of the poor), and living in lowly contested municipalities, are more likely to be targeted (13 percent) relative to harder-to-identify individuals who live in poor areas (11 percent). In Q1, higher levels of electoral competition put heavier pressure on brokers to mobilize more expensive ways of clientelism. These pressures decay when incumbents face lower levels of electoral contestation (Q4).

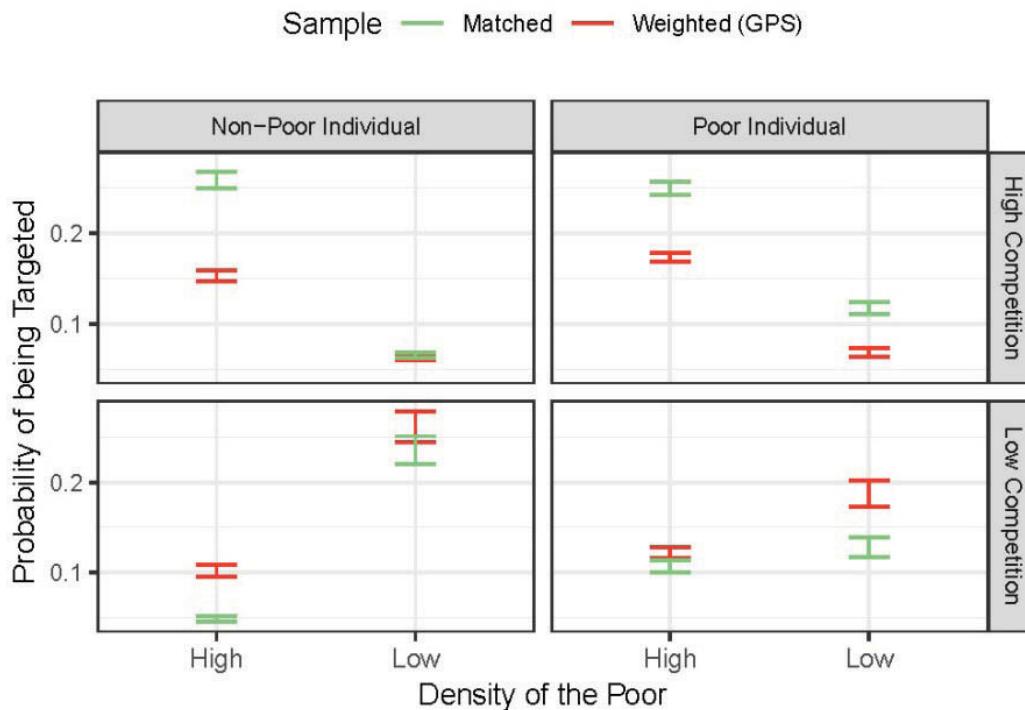
Figure 4 shows in Q2 that clientelism is more likely (25 percent) in situations where poor individuals are nested in poor groups (“high” density of the poor). As I have argued here, brokers will have incentives to engage in group targeting, taking advantage of the spillover effects of clientelism, leveraging the electoral support of potential clients by mobilizing actual clients. This is especially the case when the incumbent is seriously contested. Individuals that are similar (same quadrant), but nested in a non-poor group (“low” density of the poor), have a much lower probability of being targeted (12 percent). Individuals in Q3, who are non-poor individuals nested in non-poor areas (“low” density of the poor), and those living in lowly contested municipalities, are more likely to be targeted (24 percent) than similar individuals nested in non-poor areas (5 percent). Areas with higher levels of economic development also allow brokers to have more resources to distribute in what it was called

29 Although there are statistical differences, the differences across datasets are proportional.

30 Matched sample.

“embezzlement clientelism.” Lowly contested municipalities give brokers and political incumbents more room to allocate and distribute more expensive goods. However, and as theoretically expected, given that the net costs of this form of clientelism are higher, this is the least likely form of clientelism (reflected in the lower probabilities).

Figure 4. Simulated Expected Values of Clientelism



Note: After fitting the models shown in Table A3, this figure shows the predicted probabilities of being targeted under different scenarios, with 95 percent confidence intervals. Substantively, the figure emulates the theoretical predictions shown in Table 1. Clientelism is higher when non-poor individuals are nested in poor groups (“high” density of the poor) in highly contested municipalities (Q1), when non-poor individuals are nested in non-poor groups (“low” density of the poor) in scarcely contested municipalities (Q3), when poor individuals are nested in poor areas in highly contested municipalities (Q2), and when poor individuals are nested in non-poor areas in scarcely contested municipalities (Q4). For every quadrant, estimates from both the matched and weighted datasets are shown. The idea is to show that the decision to dichotomize the density of the poor variable at its median (as shown in Figure 2) gives substantively exact results than using the continuous version of that variable via the GPS analysis.

Discussion

This paper has argued that when poor individuals live in poor areas, brokers engage in group targeting relying on the spillover effects of clientelism. This strategy mobilizes targeted and untargeted clients by disseminating the broker's reputation of delivering benefits among potential beneficiaries. In a similar way, non-poor individuals clustered in non-poor areas are also targeted. In these cases, higher levels of economic development not only raise personal incomes, but also shift the broker's vote-buying capacities upwards. Lower levels of political contestation allow these more expensive forms of clientelism. However, in heterogeneous areas, brokers adapt their strategies and execute clientelism in a different way, relying on how identifiable individuals are. Identifiability raises the cost of defection by making their households more memorable, making receivers more likely to cooperate.

Incentives to offer or take clientelist offerings are not guided solely by structural or individual factors. This paper has suggested that both are necessary to understand clientelism better. Clearly, pressures to partake in clientelism, an expensive and uncertain strategy, rise as political competition raises (from 18 percent to 25 percent).³¹ However, the outcomes of this strategy differ largely depending on whether brokers face homogeneous or heterogeneous groups of individuals. Each one provides a different cost/benefit structure for both clients and brokers. Finally, I hope that the literature considers that groups and individuals provide different incentives to both brokers and clients, and hence, this distinction should be incorporated to better understand clientelism.

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31 Grand mean considering the most likely scenarios only.

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Appendix

Table A1. Summary Statistics: Raw Sample

	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Clientelism	1,483	0.171	0.376	0	0	0	1
Wealth Index	1,483	-1.543	0.846	-3.05	-2.261	-0.843	0.899
Municipal Opposition	1,483	81.761	11.821	43	75	89	100
Density of the Poor	1,483	2.435	1.12	1	1	3	4
Municipal Population	1,483	5.393	2.841	1	3	8	10
Urban	1,483	0.86	0.347	0	1	1	1
Political Involvement Index	1,483	1.792	1.619	0	0	3	9
Support for Democracy	1,483	5.426	1.682	1	4	7	7
Party Id.	1,483	5.939	1.15	1	6	6	12
Perception of Corruption	1,483	2.027	1.003	0	1	3	3
Years of Education	1,483	9.398	3.857	1	6	12	18

Table A2. Summary Statistics: Matched Sample

	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Clientelism	1,437	0.168	0.374	0	0	0	1
Wealth Index	1,437	-1.557	0.811	-3.05	-2.261	-0.866	0.899
Municipal Opposition	1,437	81.912	11.749	43	75	89	100
High Density of the Poor	1,437	0.47	0.499	0	0	1	1
Municipal Population	1,437	5.384	2.792	1	3	8	10
Urban	1,437	0.86	0.347	0	1	1	1
Political Involvement Index	1,437	1.784	1.613	0	0	3	9
Support for Democracy	1,437	5.417	1.684	1	4	7	7
Party Id.	1,437	5.934	1.16	1	6	6	12
Perception of Corruption	1,437	2.029	1	0	1	3	3
Years of Education	1,437	9.359	3.843	1	6	12	18

Table A3. Generalized Estimating Logistic Equations: Clientelism

	Matched	Weighted
(Intercept)	1.404 (1.968)	2.958 (2.691)
Wealth Index	1.374 (0.990)	1.320 (1.209)
Municipal Opposition	-0.040 (0.025)	-0.061 (0.032)
High Poor Density	-6.550** (2.399)	
Municipal Population	-0.115* (0.048)	-0.101 (0.053)
Urban	-0.091 (0.401)	-0.077 (0.416)
Political Involvement	0.046 (0.055)	0.047 (0.055)
Support for Democracy	-0.056 (0.046)	-0.051 (0.048)
Party Id.	-0.082 (0.053)	-0.087 (0.052)
Perception of Corruption	0.240** (0.088)	0.267** (0.089)
Years of Education	0.051* (0.021)	0.054** (0.020)
Wealth Index * Municipal Opposition	-0.018 (0.013)	-0.013 (0.015)
Wealth Index * High Poor Density	-2.509 (1.319)	
Municipal Opposition * High Poor Density	0.085** (0.030)	
Wealth Index * Municipal Opposition * High Poor Density	0.029 (0.016)	
Density of the Poor		-1.992* (0.921)
Wealth Index * Density of the Poor		-0.555 (0.372)
Municipal Opposition * Density of the Poor		0.024* (0.011)
Wealth Index * Municipal Opposition * Density of the Poor		0.005 (0.004)
Num. obs.	1,437	1,483
Num. clust.	54	54

Note: *** p < 0.001, ** p < 0.01, * p < 0.05. Clustered standard errors at the municipality level. First column shows the estimates using the matched dataset. Second column shows the estimates of the weighted model (the generalized propensity score was omitted in the table). Both models are logit GEE.

Figure A1. Distribution of Pre- and Post-Matching Observations by Wealth Index and Density of the Poor

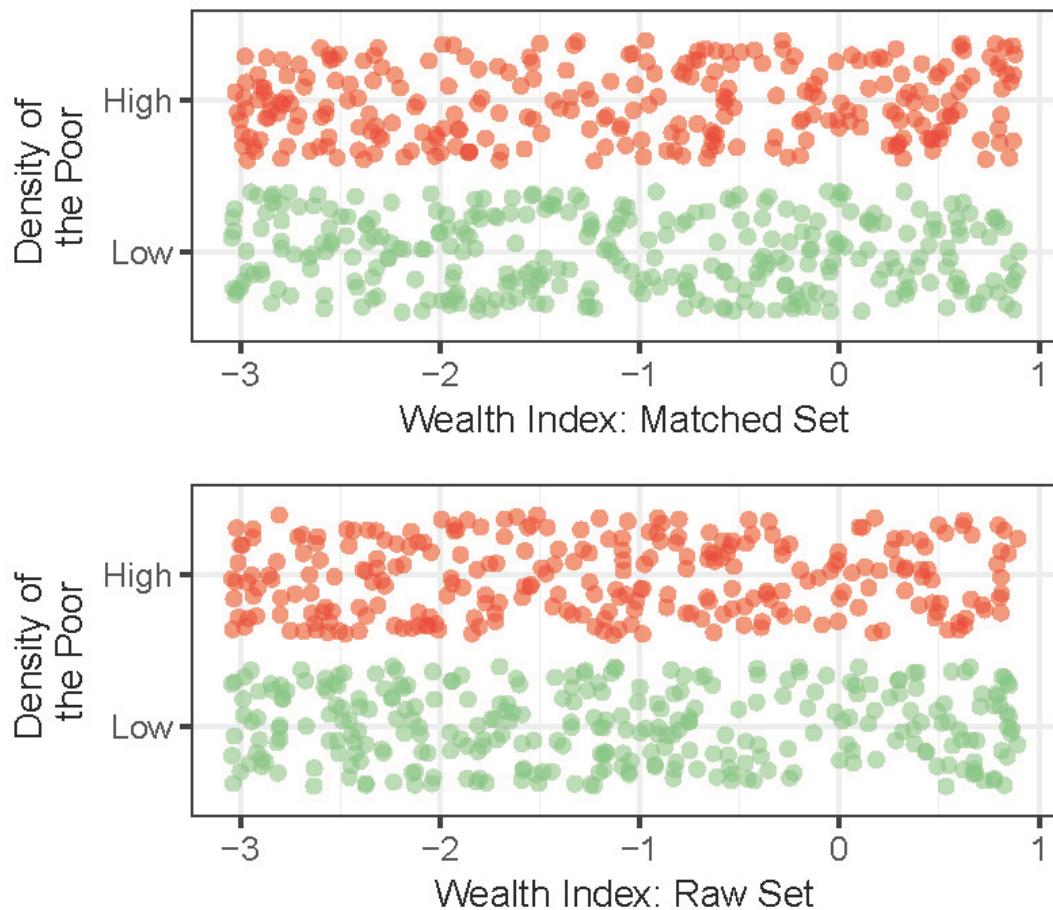


Figure A2. Frequency of Individuals by Municipality, Pre- and Post-Matching Deletion

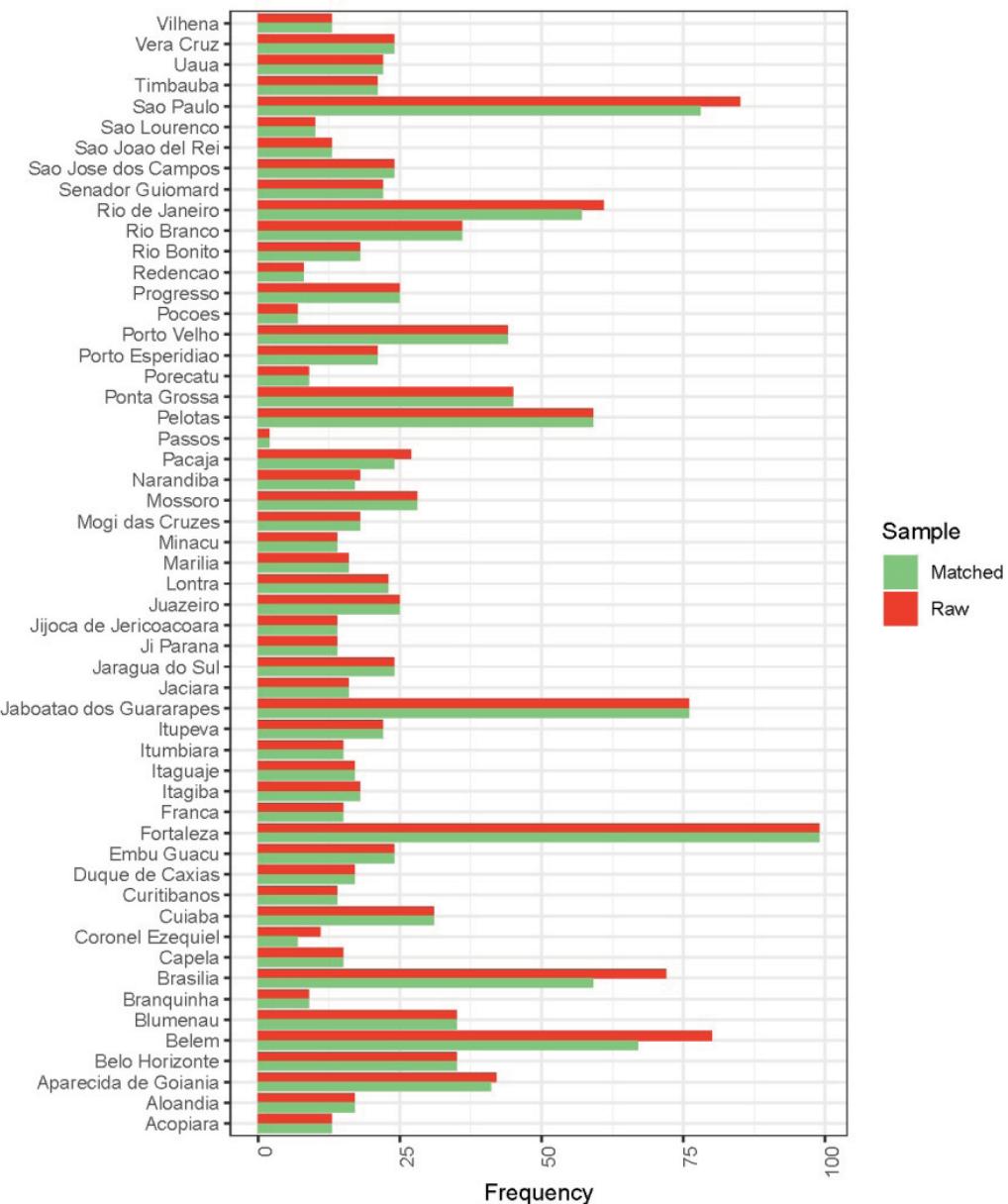


Figure A3. Pre- and Post-Matching Balance: Distribution of Propensity Scores

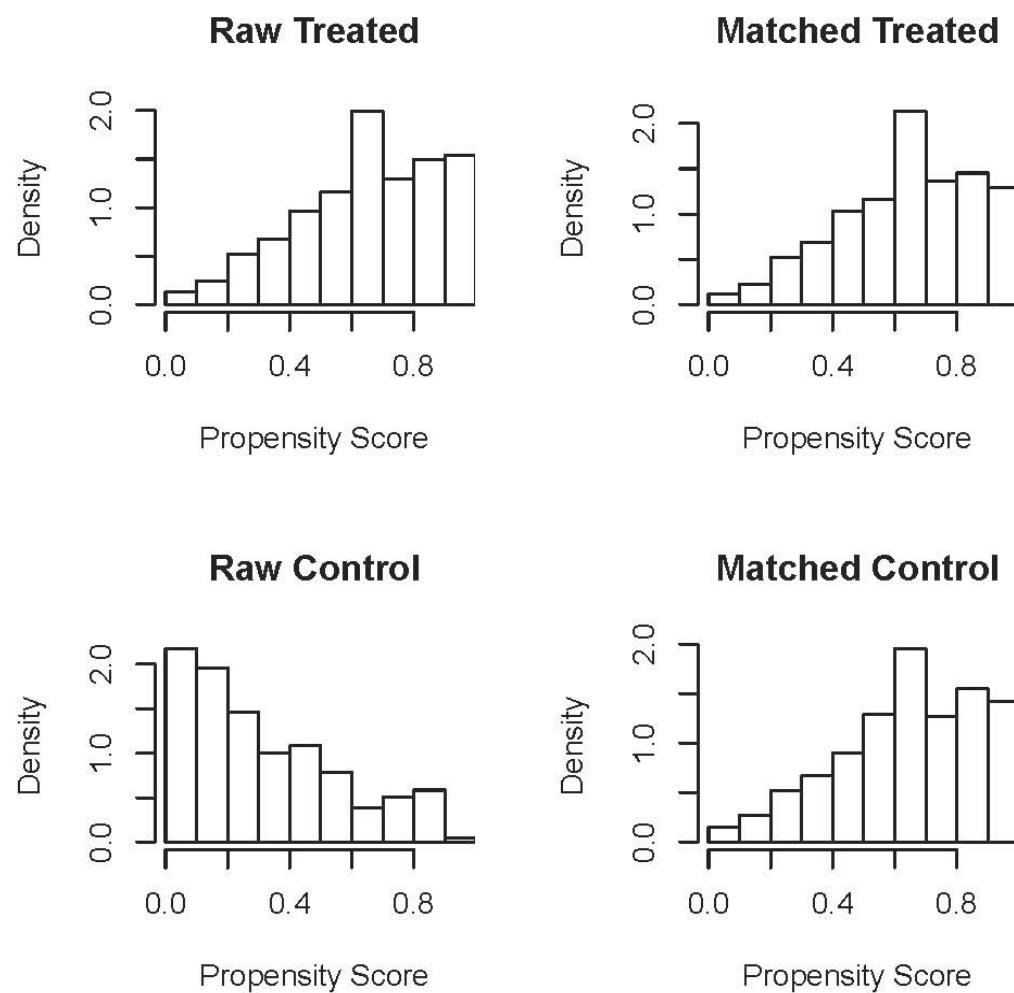
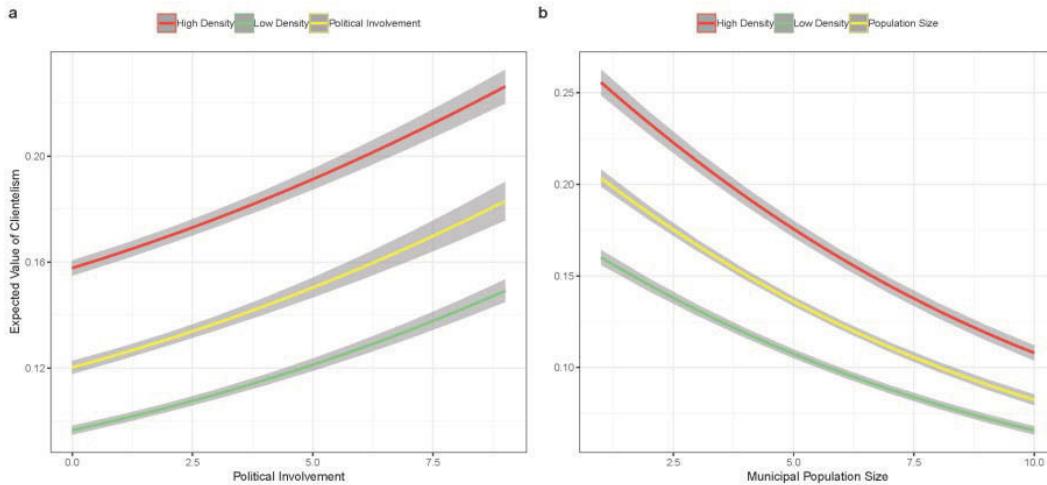


Figure A4. Simulated Expected Probability of Being Targeted: Political Involvement and Population Size



Note: Using the estimations in Table A3, the figure shows the probability of being targeted at different values of political involvement (**a**) and population size at the municipal level (**b**). The figure suggests that being nested in high-poor density areas contributes substantially more to explaining clientelism.

Figure A4 shows a plot divided in two panels. Panel **a** shows the simulated expected probabilities (with 95 percent confidence intervals) of being targeted at different levels of political involvement. As the blue lines suggests, individuals who participate in civic associations have higher probabilities of being targeted. This is in line with findings in previous research (Schaffer and Baker 2015; Carreras and Castaneda-Angarita 2014: 7; Calvo and Murillo 2013; Holland and Palmer-Rubin 2015: 16; and Rueda 2015). However, once I decompose these effects, being nested in high-poor density areas contributes substantially more to the model. These differences are statistically significant. Panel **b** shows the probability (with 95 percent confidence intervals) of being targeted at different increments of the size of the population. In line with the literature, I also see that this relationship is negative (Stokes 2005: 323; Kitschelt and Wilkinson 2006: 10; Magaloni 2008: 67; Rueda 2017; Bratton 2008; and Gingerich and Medina 2013: 456). However, the effect of being nested in high-poor density municipalities outperforms the effect of population size, suggesting spillover effects.

Apuntando Justo a Ti/Ustedes: Blancos Clientelares Grupales e Individuales en Brasil

Resumen: ¿Los partidos apuntan a grupos o individuos? Aunque esta pregunta es fundamental para entender el clientelismo, la literatura no ofrece una respuesta clara. Este trabajo argumenta que, dependiendo de ciertas condiciones, los compradores de votos apuntan a individuos cuando pueden identificar a sus blancos, y a grupos cuando necesitan utilizar los efectos indirectos que provee la lógica del clientelismo. Tanto la identificación individual como los efectos indirectos del clientelismo grupal, dependen de los niveles de pobreza individual, pobreza grupal, y los niveles de competencia partidista. Aunque la teoría de este trabajo se concentra en los blancos clientelares (grupales e individuales), también argumenta que factores estructurales, como la densidad de pobreza, deberían ser considerados en la literatura acerca de la venta de votos. Estos factores estructurales son de los pocos observables sobre los cuales los compradores de votos basan su decisión acerca de si invertir en clientelismo o no. Usando datos de opinión pública y censos de Brasil, el trabajo examina las variaciones en rentas individuales dentro de diferentes contextos de pobreza a nivel municipal. Los resultados sugieren que los partidos políticos emplean estrategias segmentadas o ad-hoc, apuntando a individuos cuando son altamente identificables, y a grupos cuando se presentan situaciones de economías de escala. Además, individuos que no están en situación de pobreza también pueden recibir ofertas clientelares.

Palabras clave: Brasil, clientelismo, venta de votos



Still for sale: the micro-dynamics of vote selling in the United States, evidence from a list experiment

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Abstract

In nineteenth-century United States politics, vote buying was commonplace. Nowadays, vote buying seems to have declined. The quantitative empirical literature emphasizes vote buying, ignoring the micro-dynamics of vote selling. We seem to know that vote buyers can no longer afford this strategy; however, we do not know what American voters would do if offered the chance to sell their vote. Would they sell, and at what price, or would they consistently opt out of vote selling? A novel experimental dataset representative at the national level comprises 1479 US voters who participated in an online list experiment in 2016, and the results are striking: Approximately 25% would sell their vote for a minimum payment of \$418. Democrats and Liberals are more likely to sell, while education or income levels do not seem to impact the likelihood of vote selling.

Keywords Vote buying · Vote selling · Clientelism · List experiments · United States

Vote sellers and vote buyers

Prior research on clientelism usually focuses on whether parties have attempted to buy votes (Vicente and Wantchekon 2009; Vicente 2014; Rueda 2015, 2017; Reynolds 1980; Nichter 2014; de Jonge 2015; Finan and Schechter 2012; González-Ocantos et al. 2014; Diaz-Cayeros et al. 2012; Brusco et al. 2004). Unfortunately, while this is an important question, it overlooks the conditions under which citizens would sell their vote. In fact, Nichter and Peress (2017) explain that studies continue to view clientelism typically as a top-down process, generally overlooking citizens'

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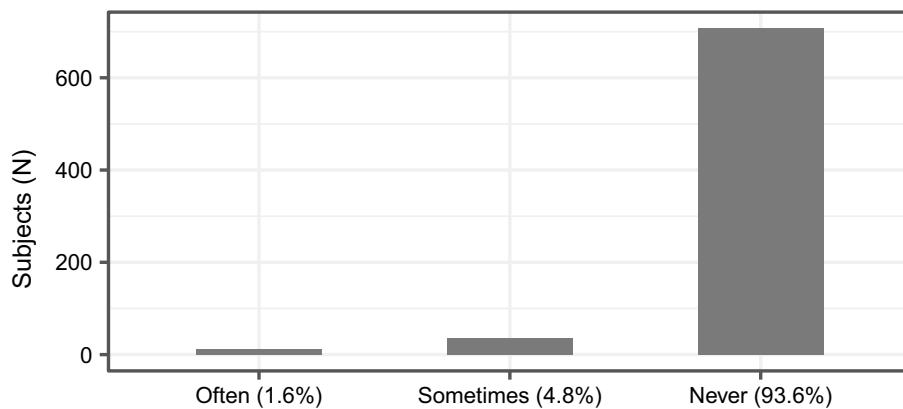


Fig. 1 Frequency of clientelism in the United States (2010). Note figure shows the frequency of survey respondents, $N = 755$. Source: LAPOP, 2010 wave for the United States. Question is clien1: “In recent years and thinking about election campaigns, has a candidate or someone from a political party offered you something, like a favor, food, or any other benefit or object in return for your vote or support? Has this happened often, sometimes, or never?”

demands. Since several questions pertaining to vote sellers remain unanswered, a bottom-up reconceptualization is necessary. For instance: *What would voters do if offered the chance to sell their vote? Would they sell it? And at what price?*¹

To illustrate the issue at hand, Fig. 1 shows responses of US citizens asked whether a candidate or a member of a political party has offered something in exchange for their vote, completely ignoring voters’ preferences. The figure begs the question of whether survey respondents who answered “never” *would* still be willing to sell their votes.

It seems that whether studies focus on vote buying or vote selling depends partly on methodological rather than theoretical decisions.² On the one hand, historical and/or ethnographically based contributions describe clientelist transactions from the point of view of voters, focusing on the conditions that make vote selling most likely (Posada-Carbó 1996; Sabato 2001; Auyero 2000; Szwarcberg 2013; Borges 2019). On the other hand, statistical, survey, and/or experimentally based work mostly explores issues related to vote buying. For example, using a field experiment in Benin, Wantchekon (2003) stresses the role of incumbency on vote buying. Jensen and Justesen (2014, p. 227) focus on the impact of “poverty on vote buying,” while Khemani (2015, p. 84) shows that “vote buying in poor democracies is associated with lower [public] investments.” Hence, and except for several important quantitative studies (Corstange 2012; Imai et al. 2015; Nicther and Peress 2017; Hicken et al. 2015, 2018; Michael and Thachil 2018), the emphasis of statistical studies remains on studying vote buying. Importantly, other statistically based studies have explored attitudes toward vote buying (Bratton 2008; Weitz-Shapiro 2012).

¹ It is important to note that clientelism as a practice involves more than just buying or selling votes. Other goods might be involved in the clientelist transaction—for instance, public jobs or public infrastructure, e.g., see for example Dixit and Londregan (1996), Calvo and Murillo (2004), and Khemani (2015). However, this paper’s focus is on just vote buying and vote selling.

² I thank one of the anonymous reviewers for this comment.



They suggest that a strong stigma is attached to vote buying, which might make voters unwilling to sell their vote. For instance, González-Ocantos et al. (2014, p. 208) designed a list experiment to study attitudes toward vote buying in Latin America. They conclude that most respondents find vote buying “unacceptable when provided with a hypothetical example.”

While the quantitative literature has advanced several important avenues of research, it has overlooked many important questions. The wording of the Latin American Public Opinion Project (LAPOP) question illustrates part of the issue. By focusing on vote buying, it gives the falsely optimistic impression that US voters systematically “oppose” vote buying, “thus” rarely engaging in clientelism (as Fig. 1 strongly suggests). Furthermore, most quantitative studies were conducted primarily in developing countries, seriously narrowing the scope of our inferences. In part, this is because the clientelism literature usually focuses on realized behaviors only—that is, actual clientelist transactions. Unfortunately, by ignoring attitudes of potential vote sellers, particularly when it comes to the willingness to sell, selection bias seriously threatens causal inferences.

This paper makes both methodological and substantive contributions to the literature by leveraging a list experiment on hypothetical vote selling in a consolidated democracy. We believe that studying hypothetical behaviors—such as the willingness to sell—is a valuable exercise. Geddes (1990, p. 131) explains the well-known selection issues of studying “only cases that have achieved the outcome of interest.” Hence, if we are interested in understanding the micro-dynamics of clientelism—particularly as a supply-and-demand issue—we should incorporate the preferences of both sellers and buyers, potential and/or actual. Since the focus of this paper is on the willingness to sell, we believe that we can also learn from *unrealized* clientelist transactions. Following the lead of González-Ocantos et al. (2014), this paper presents experimental evidence of hypothetical vote selling in the United States.

In 2016, a novel dataset representative at the national level was collected. A total of 1479 US voters participated in a list experiment between March 2 and March 6. This experiment made possible both the identification of the demographic factors that would make US voters more likely to sell their vote, and at what price, and the investigation of whether they would systematically lie about selling their vote. The results are striking. The data suggest that a sizable portion of US voters are willing to sell their vote (approximately 25%), would sell it for at least \$418, and would systematically lie about it (approximately 8%). Given that these data are representative at the national level (i.e., this is not a convenient sample), these findings are surprising. Democrats and Liberals are systematically more likely to sell than Republicans. Education and income levels do not seem to have a systematic impact on the willingness to sell.

While this paper essentially describes the phenomenon, it leaves for future research further consideration of the causes of hypothetical vote selling in the United States. Ultimately, this paper attempts to bring voters back into the quantitative study of clientelism, particularly by studying their willingness to sell.



The United States as a case

At first, many advanced democracies were clientelist political systems. For instance, Stokes et al. (2013, p. 200) explain that in the nineteenth-century United States, “vote buying was commonplace” and “the major urban political institution in the late nineteenth century” (Erie 1990, p. 2). In Chicago, New York City, Newark, and other large American cities, votes were exchanged for “cash, food, alcohol, health care, poverty relief, and myriad other benefits” (Stokes et al. 2013, p. 200). The street price of the right to vote freely was low. Bensel explains that “[voters] handed in a party ticket in return for a shot of whiskey, a pair of boots, or a small amount of money” (Stokes et al. 2013, p. 227). In general, students of American political development have analyzed vote buying in detail, confirming both its early development and its generalized practice (Bensel 2004; Campbell 2005).³

However, vote buying currently seems to have declined considerably, for two competing reasons. Stokes et al. (2013, p. 201) show that industrialization drove up the electorate’s median income, making vote buying more expensive for party machines. However, Kitschelt and Wilkinson (2006, p. 320) disregard the industrialization hypothesis, focusing on the lower levels of “[s]tate involvement in the public sector.”

Regardless, clientelist linkages are now rare. Figure 1 suggests that 93.6% of US respondents have never received a clientelist offer from a political party. While only a very small percentage (4.8%) report receiving such an offer from a political party, we do not know whether survey respondents *would* sell their votes. This paper presents systematic evidence that they would. Consequently, the counterintuitive results presented in this paper make our descriptive efforts worth pursuing. Representing the United States as a “crucial case,” both the narrative and the findings follow a “least-likely” design approach. As Levy (2008, p. 12) explains, “[i]nferential leverage from a least likely case is enhanced if our theoretical priors for the leading alternative explanation make it a most likely case for that theory.” The vote-buying literature mostly considers developing countries and describes vote sellers as poor (Weitz-Shapiro 2014, p. 12), uneducated (González-Ocantos et al. 2014), and undemocratic (Carlin and Moseley 2015). Thus, previous literature implies that the willingness to sell votes in the United States should be low, making it a difficult case study on vote selling.

The evidence that this paper presents may be associated with a probable erosion of American democracy.⁴ In a highly controversial pair of articles, Foa and Mounk (2016, p. 7) document a deep “crisis of democratic legitimacy [that] extends across a [...] wider set of indicators” in the United States. They find that 26% of millennials declare that it is “unimportant” in a democracy for people to “choose their leaders in free elections” (Foa and Mounk 2016, p. 10, and Foa and Mounk 2017). These findings raise many (unanswered) questions regarding the actual value that American electoral institutions hold for citizens, possibly undermining the legitimacy of

³ For the British case during the Victorian era see Kam (2017).

⁴ Relatedly, see Levitsky and Ziblatt (2018).



the integrity of voting. Is voting unimportant enough to lead US citizens to sell their votes if offered the possibility?

The next section gives a historical account of vote buying and vote selling in the United States. The section also attempts to situate both within a historical context. It particularly shows how vote buying and vote selling transitioned from their status as an important institution in American elections to a scarcely practiced electoral method. The following section explains the experimental design. Immediately thereafter, the paper presents the statistical analyses of the experimental data. The last section offers some working hypotheses and possible lines for future research.

Vote selling and patronage in the United States: a brief historical account

While all US states made bribery of voters illegal early in US history, these laws were purposely ignored. Well before the Gilded Age (1877–1896), several norms aimed to prohibit bribery, clientelism, and patronage. For instance, as early as 1725, the New Jersey legislature had already outlawed many electoral malpractices (Bensel 2004, p. 59). However, these restrictions were systematically bypassed. To circumvent property qualifications, for instance, office-seekers (and their supporters) commonly bought “freeholds for landless men in return for their vote” (Campbell 2005, p. 6), a practice known as “fagot voting.” Since it was a coercive bribe, after “the election, the land was simply returned to the original owner” (p. 6).

Weak institutions, poor bureaucracies, and bad-quality record-keeping helped to foster electoral malpractice.⁵ First, most states did not have actual registration laws, making voter eligibility difficult to determine (Argersinger 1985, p. 672). Historians frequently report that judges at polling places had a hard time determining not only the age of the potential voter,⁶ but also whether the prospective voter was a US citizen, especially in cases that involved newly naturalized immigrants with strong foreign accents (Bensel 2004, p. 20). Consequently, it was often up to the judge’s discretion whether to let prospective voters cast a ballot. Since judges were party appointees (Argersinger 1985, p. 672), their discretionary powers were systematically used to shape electoral outcomes.

Low literacy levels also helped to sustain vote selling in the United States. For example, in Kentucky and Missouri, the law required voters to verbally announce their choices at the polling places, instead of using party tickets (Bensel 2004, p. 54). Of course, the *viva voce* method was convenient for party workers who usually swarmed around the polling places. However, the ticket system eventually supplanted this method.

⁵ The U.S. Bureau of the Census did not exist. Consequently, it was relatively easy to invent names, “repeat,” or use any other subterfuge to “stuff the ballot box.” In fact, “a St. Louis politician admitted registry fraud but argued that there was no proof that the names he copied into the registry were of real people and, therefore, no crime had been committed” (Argersinger 1985, p. 680).

⁶ Judges used as a rough proxy whether the prospective voter had the ability to grow a beard (Bensel 2004, p. 20).



The “party strip” or “unofficial” ballot system also permitted all sorts of fraudulent election practices. The parties themselves produced party tickets. Since tickets varied by size and color, it made “the voter’s choice of party a public act and rendered voters susceptible to various forms of intimidation and influence while facilitating vote buying” (Argersinger 1985, p. 672). Similarly, Rusk (1970, p. 1221) explains that distinctive ticket colors and shapes “assured instant recognition of the ballot by the voters [and] party workers.” Reynolds and McCormick (1986, p. 836) present similar evidence. Consequently, party workers hired to monitor the voting window (Argersinger 1985, p. 672) had ample opportunity to punish or reward voters accordingly.

The ticket system required very strong party machines, which, in turn, required considerable economic resources to make the system work. However, political machines were oiled not only with money. On the one hand, many “ticket peddlers” (Argersinger 1985, p. 672) were volunteers (Bensel 2004, p. 17), saving some of the costs needed to maintain the machine. Most of these volunteers “enjoyed the patronage of elected party officials by holding government jobs, drawing public pensions, servicing government contracts, or enjoying special licensing privileges” (Bensel 2004, p. 17). On the other hand, political appointees “from janitor to secretary of state” and some corporations donated annually part of their salaries and revenues (Reynolds 1980, p. 197). Thus, parties amassed huge amounts of money.

With all these resources flooding the polls on election day, voting was truly an interesting spectacle. On that day, party agents would offer voters plenty of liquor as an incentive to vote the party ticket. Hence, “the street or square outside the voting window frequently became a kind of alcoholic festival in which many men were clearly and spectacularly drunk [to the point that] some could not remember whether or not they had voted” (Bensel 2004, p. 20). Even before the Gilded Age, American elections were engineered according to these “principles.” When running for the Virginia House, a young George Washington “spent nearly 40 pounds—a considerable sum for the day—on gallons of rum, wine, brandy, and beer; all used to win over the votes of his neighbors” (Campbell 2005, p. 5).⁷

The Australian ballot system significantly reduced the frequency of most of this malpractice (Rusk 1970, p. 1221). However, as vote selling and vote buying were so embedded in what was considered normal, the immediate effect of the Australian system was to reduce turnout (Reynolds and McCormick 1986, p. 851).

Today, the modus operandi of clientelism has changed, and both the frequency of vote buying/selling and the importance of party machines have declined. Scholars have pointed out that “party machines are a thing of the past” (Stokes et al. 2013, p. 230). However, some contemporary accounts remain of vote buying and selling in American elections. For instance, Campbell (2005, pp. 243–244) explains how a Democratic leader in Logan County, West Virginia, accepted \$35,000 in cash to support Senator Kennedy. As the Democratic leader explained, “this money was for one purpose: ‘We bought votes with it [...] that’s the way real politics works.’ ” Other examples are the famous primary election in March 1972 in Chicago (p. 262)

⁷ \$1250 in 2017 US dollars. Conversion based on Williamson (2018).



and the elections in the coal-rich Appalachian Mountains during the 1980s (p. 275). Similarly, nonacademic sources find that during the 2010 elections, “selling votes [was a] common type of election fraud” (Fahrenthold 2012). Others find that “[v]ote-buying is extremely common in *developed* [...] countries” (Leight et al. 2016, p. 1). If vote buying is “a thing of the past,” why do we still see it? How common is vote selling? The next two sections attempt to quantify—in an unbiased way—the willingness to sell votes among a representative sample of US voters.

Experimental design

The study of individual preferences depends on truthful answers. However, under certain circumstances, individuals might not want to answer truthfully, due to social pressure. For instance, to avoid having the interviewer judge them, individuals might not want to reveal having done something illegal, such as selling one’s vote. Failing to consider this systematic source of bias will pose threats to causal inference.

Since list experiments administer two lists of items (one to the control group, one to the treated group), list experiments are well suited to eliciting truthful answers (Blair 2015). Both lists look identical (e.g., each containing the same three items); however, the treatment list traditionally includes a fourth item, the sensitive item related to some socially condemned behavior. Respondents are asked how many items on the list they would endorse, not which ones. For instance, if an experimental subject answers “2,” the interviewer will not know whether that number includes the sensitive item. Consequently, if the survey respondent wants to endorse the sensitive item, the answer will be “masked” by the other items in the list. This concealment makes this technique suitable for studying socially condemned behaviors, such as vote buying (Corstange 2008; González-Ocantos et al. 2012; Corstange 2012; Blair and Imai 2012), drug use (Druckman et al. 2015), sexual preferences (LaBrie and Earleywine 2000), and attitudes toward race (Kuklinski et al. 1997; Redlawsk et al. 2010).

Given that both lists are assigned randomly, the mean number of nonsensitive activities that respondents endorse should be equal across the two lists. However, if there are any differences in means between the two groups, the differences should be attributed only to the presence of the sensitive item.

Blair and Imai (2012) and Imai et al. (2015) provide a statistical framework to analyze list data efficiently.⁸ They formalize two assumptions, namely, that there are (1) “no design effects” (i.e., the inclusion of a sensitive item has no effect on respondents’ answers to control items), and (2) “no liars” (i.e., respondents give truthful answers for the sensitive item). When the two assumptions hold and the item counts for types $y = 0$ and 4 are fully observed,⁹ experimental subjects with

⁸ While list experiments are common, researchers unfortunately “[utilize] only a difference in means estimator, and [do] not provide a measure of the sensitive item for each respondent” (Glynn 2013, p. 159).

⁹ For a hypothetical treatment list of four items.



item-count types $y = 1, 2$, and 3 can be inferred using multivariate techniques that allow for inferring who answered “yes” to the sensitive item. In addition, the statistical analyses permit studying the relationship between preferences over the sensitive item (i.e., vote selling) and an individual’s characteristics, such as income and party identification. Also, the design includes a “direct” question on the sensitive item, also making possible an estimation of the amount of social-desirability bias.

Collected in 2016, the data ($N = 1479$) are representative at the national level.¹⁰ Figure 6 shows the geographical distribution of survey respondents, grouped by party identification. The experiment was framed as a study about crime in the United States, not as a study about vote selling.¹¹ While pretesting the study, it was decided that the experiment needed to mask a very serious felony (selling one’s vote) among other equally serious felonies (such as stealing) and other less serious crimes (such as speeding or downloading music illegally from the Internet). Otherwise, the vote-selling item would have stood out among the other items, making it seem totally negative and undoable, and/or making the true purpose of the study obvious.

Before splitting the subject pool into the subjects’ respective experimental conditions, participants were asked to read an excerpt describing four illegal activities (including vote selling).¹² All were formatted as news pieces. The idea was to explain “vote selling” to “newsreaders.”

As Fig. 2 suggests, to prevent possible priming effects,¹³ the order in which experimental subjects answered the direct question¹⁴ and the list experiment were randomly assigned. To be sure, all subjects answered both the direct question and the list experiment. To further prevent the possibility of biased answers when asking the direct question to individuals in the treated group, the direct question stated that the hypothetical possibility of doing one of the illegal things mentioned previously in the excerpt would be randomly assigned. However, all participants were directly asked whether they would be interested in selling their vote. Direct answers were then used to estimate the proportion of “liars.”

As a follow-up, subjects answering “yes” to the direct question answered a pricing test that asked them to indirectly put a price on their votes. Following standard practice in marketing research, participants slid a handle indicating which price was considered “too cheap” for one’s vote. The slide ranged from \$0 to \$1000, in one-dollar increments. The idea was to capture the respondent’s willingness to sell. The pricing test particularly measures the lowest bound at which the participant would perceive the least economic benefit that was still enough to make selling his/her vote interesting. Moving forward, the list experiment contemplated one control and two

¹⁰ Research Now SSI collected the data between March 2 and March 6. Survey respondents belong to the online panel owned and administered by SSI. Notice of IRB Exemption Protocol #E16-292 is kept on file at the Office of Research and Regulatory Affairs of Rutgers University.

¹¹ To isolate the risks and costs associated with engaging in any illegal activity, the next phrase was included: “assuming you would not go to jail.”

¹² See “Appendix” for wording.

¹³ Blair and Imai (2012, p. 54) explain that asking the direct question to individuals in the treated group might bias the results.

¹⁴ See “Appendix” for wording.



possible treatments, each with different vote-selling prices. Since pricing a vote is difficult and based on several pretests, an arbitrarily low (\$100) or an arbitrarily high (\$500) price appeared. Participants randomly assigned to the treatment condition answered only one of these two treatments. More than hard-pricing tests, these two treatments account for possible different elasticities that might have interacted with individual socioeconomic backgrounds, mainly defined by income.¹⁵

Subjects randomly assigned to the control condition answered the following question:

Now, you will have to type HOW MANY, if any, of the following illegal activities you might engage in, assuming you would not go to jail.

(1) steal an iPod from a large department store
 (2) speed on the highway because you're late for work/school
 (3) download your favorite music from the internet illegally

Type in HOW MANY (NOT WHICH), if any, of these things you would do.

In turn, subjects randomly assigned to the treatment condition answered one of the following two treatments. Subjects assigned to the “low” price condition read the same vignette, but the next line was added in the third place: (3) sell your vote to a candidate for \$100. Those assigned to the “high” price condition read the following line: (3) sell your vote to a candidate for \$500.¹⁶

Figure 3 shows the distinct frequencies of participants declaring how many (if any) illegal things they would do. Notice that the figure shows the total number of items, not which ones. For instance, a frequency of “3” does not mean the frequency of the third item, but describes the total number of individuals answering that they would do three of the illegal activities described in the vignette.¹⁷ The order of the items was not randomized, to avoid violating the stable unit treatment value assumption (SUTVA).¹⁸

Showing that the probability of being assigned to any condition is not associated with individual covariates is important. Table 1 shows a multinomial logistic model. The dependent variable is the treatment condition (high treatment, low treatment, and control). The independent variables are observable characteristics captured

¹⁵ Holland and Palmer-Rubin (2015, p. 1189) explain that “the poor are thought to be more susceptible to vote buying.”

¹⁶ Since one of the two sentences was added, item (3) download your favorite music from the Internet illegally was moved to the fourth place.

¹⁷ The experimental design passes the standard tests for design effects (floor and ceiling effects). See Table 3.

¹⁸ Morton and Williams (2010, p. 98) explain that the treatment should be invariant or “stable.”



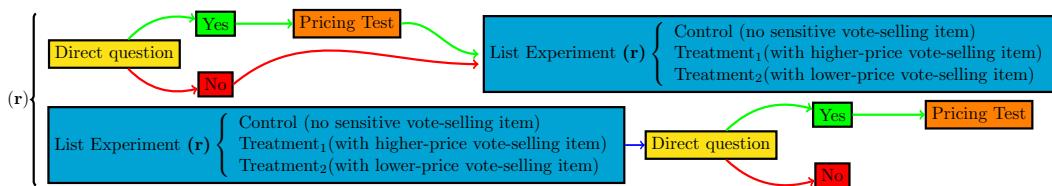


Fig. 2 Experimental flow of the list design. Note this figure shows the flow of the list experiment. Notice that (1) the order in which experimental subjects answered both the direct question and the list experiment was randomized; (2) there are two treatments, one with a selling price of \$100 (“low”) and one with a selling price of \$500 (“high”)

by a short questionnaire included in the study. Four variables were used: income, education, party identification, and political ideology. These were the same set of variables used when estimating likely vote sellers (below). Conveniently, the base category in the multinomial logistic regression is the control condition. The coefficients in the table are all zeros (and statistically nonsignificant). Consequently, these results show no observable differences between the “high” treatment condition and the control group. The same applies to the “low” condition.¹⁹

The paper acknowledges that considerable friction and transaction costs in the real world might mean that creating a market for vote selling would not be easy. For instance, party identification might increase (or decrease) the cost of selling one’s vote, presumably preventing (or fostering) the transaction. If the party of both sellers and buyers should match, fostering vote selling might represent a win-win situation for both. This experimental design does not consider blocking on party identification, as that might have increased considerably the number of cells.

Statistical analyses

Would US citizens sell their vote?

Table 2 shows a simple difference-in-means analysis between each treated group and the control group. On average, the control group would do 1.116 things on the list. Subjects treated under the “low” condition (\$100) would do 1.182 things on the list, while subjects in the “high” condition (\$500) would do 1.189 things.

Three important points characterize this bivariate analysis. First, the mean differences between treated groups (i.e., “low” and “high” treatments) are statistically zero, implying that neither treatment should introduce design bias into the experiment. Second, while treated subjects do have slightly higher means when compared to the control group (indicating some vote-selling propensity), these differences are not statistically significant. Third, while not statistically significant, $0.066 \times 100 = 6.6\%$ of subjects would sell their vote under the “low”

¹⁹ I thank the anonymous reviewer at *Acta Politica* for this suggestion.



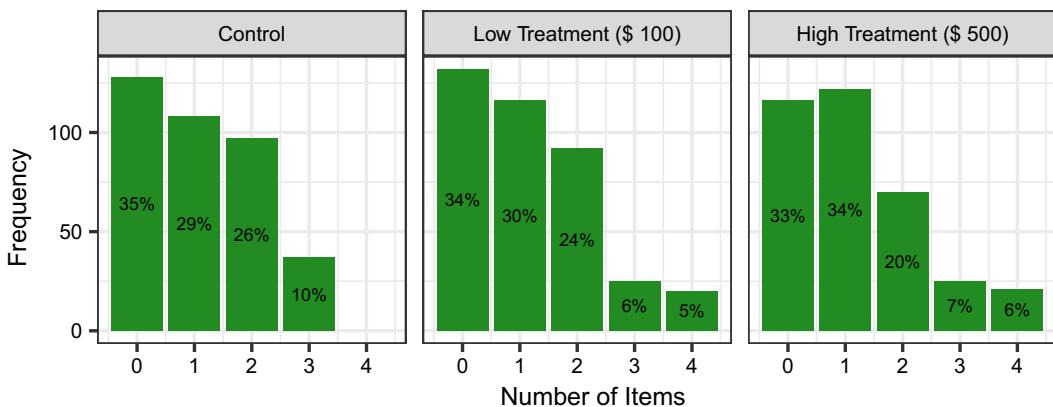


Fig. 3 Frequency and percentages of subjects declaring how many (if any) illegal things they would do. Note notice that the X-axis denotes the number of items, not which ones. Percentages show proportions per condition

Table 1 Covariate balance: multinomial logistic regression for both treatment conditions

	High	Low
Ideology	0.019 (0.068)	-0.031 (0.067)
Party Id.	-0.125 (0.083)	0.022 (0.080)
Income	-0.021 (0.022)	0.006 (0.021)
Education	0.049 (0.048)	-0.008 (0.047)
AIC	2449.471	2449.471
BIC	2499.583	2499.583
Log likelihood	-1214.736	-1214.736

The table shows a multinomial logistic regression. The dependent variable is the treatment condition (high, low, control). In both models, the base category is the control condition. The independent variables are observable characteristics captured by a short questionnaire included in the study. This set of covariates is the same as the one used in the statistical analyses of the list experiment. Since all estimated coefficients are close to zero and statistically nonsignificant, we can safely assume that the randomization mechanism worked as expected, i.e., there are no observable differences across the different treatment conditions. Reference category is control condition. Intercept was excluded from the table. $N = 1479$

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

condition, while $0.073 \times 100 = 7.3\%$ of subjects would sell their vote under the “high” condition. While these estimations score substantially under what is found through the multivariate approach used in this study, as shown below, they are also highly inefficient.

Bivariate calculations are statistically inefficient; hence, the data should be analyzed using multivariate techniques instead. Following the advice of Blair and Imai



Table 2 Differences in means between Treatments (high and low) and the Control group

Condition	Mean	Difference with control condition	Confidence intervals	<i>t</i>	df	<i>p</i> value
Low (\$100)	1.182	1.182–1.116 = 6.6%	[−9%, 22%]	0.846	748	0.398
High (\$500)	1.189	1.189–1.116 = 7.3%	[−8%, 23%]	0.913	700	0.361

The table shows two-tailed *t* tests between each experimental treated unit (“low” and “high” conditions) and the control group. The table shows that $0.066 \times 100 = 6.6\%$ of subjects would sell their vote under the “low” condition, while $0.073 \times 100 = 7.3\%$ of subjects would sell their vote under the “high” condition. Also, 95% confidence intervals are shown. It is evident that they are quite wide and not statistically significant

(2012) and Blair (2015), we took a statistical multivariate approach.²⁰ Exploiting the “low” and “high” treatments, we estimated two identical statistical models. In both models, the outcome variable is the item count of things that subjects would do. The idea is to estimate what we cannot observe (i.e., vote selling), using information that we do observe (i.e., socioeconomic and political variables captured by the questionnaire). The model considers the most common covariates studied in the vote-buying literature (Calvo and Murillo 2004; Stokes 2005; Kitschelt and Wilkinson 2006; Nazareno et al. 2008; Weitz-Shapiro 2012; González-Ocantos et al. 2014; Oliveros 2016; Bahamonde 2018)—that is, income, education, party identification, and political ideology.

Leveraging this multivariate approach makes estimating the proportion of hypothetical vote sellers possible. For both the “low” and “high” treatments, Fig. 4 shows the proportions of declared vote sellers (“Direct Question”), predicted vote sellers (“List Experiment”), and the difference between the two (“Social Desirability”).²¹ Substantively, the figure suggests that after combining the estimates of the “low” and “high” treatments, approximately 25% of the nationally representative sample would be willing to sell their vote.²² While a considerable proportion answered the direct question affirmatively (18%),²³ the analyses still suggest that survey respondents systematically underreported their true answers—that is, approximately 8% of the nationally representative sample would have lied.²⁴

The difference-in-means approach in Table 2 suggests that between 6.6 and 7.3% would be willing to sell their votes. However, the multivariate approach in Fig. 4 suggests that 25% would be willing to do so. While at first these differences might seem huge, they are not. As the literature suggests, multivariate approaches to analyzing list experiment data are far more efficient (Blair and Imai 2012; Blair 2015).

²⁰ The R package *list* was used (Blair 2015). The estimation method used was the “ml” and the maximum number of iterations was 200,000. The remaining arguments of the package were left at their default values.

²¹ Since the estimated quantities do not vary across the different treatments (“low” and “high”), it is reasonable to think that there are no specific concerns associated with the (arbitrarily) chosen prices.

²² This number was calculated averaging over the “high” (27%) and “low” (23%) estimates.

²³ This number was calculated averaging over the “high” (19%) and “low” (17%) estimates.

²⁴ This number was calculated averaging over the “high” (8%) and “low” (7%) estimates.



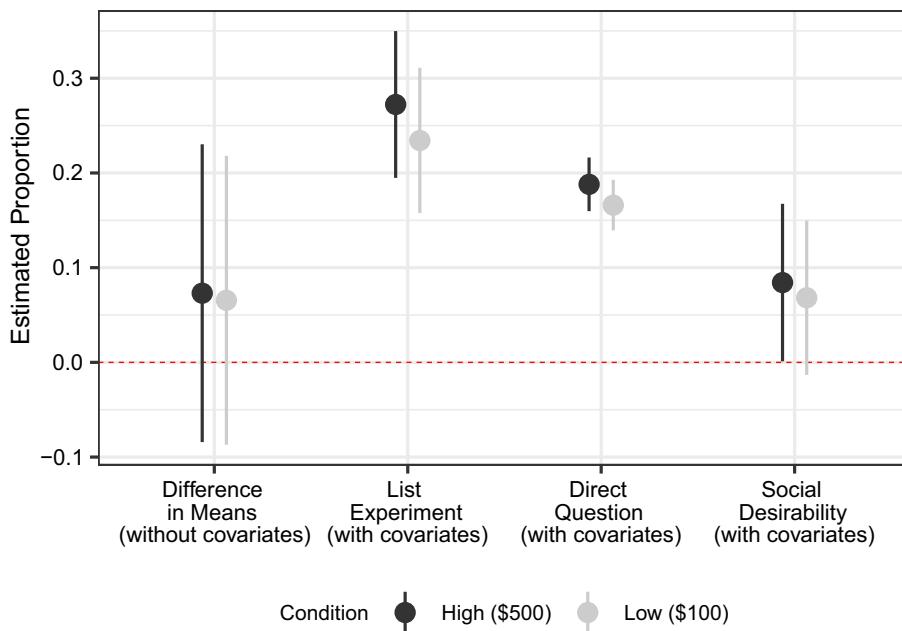


Fig. 4 List experiment data: declared and predicted vote sellers. Note figure summarizes Table 2 by showing the simple difference in means (without covariates). It also shows the proportion of declared (“Direct Question”) and predicted (“List Experiment”) hypothetical vote sellers, and the difference (“Social Desirability”). The three sets of main estimates were obtained via a multivariate procedure (including covariates). Combining both “low” and “high” treatments, 25% would be willing to sell their votes. And of those who answered affirmatively when asked directly (18%) an estimated additional 8% lied about it. “Liars” answer the direct question negatively, but they are likely sellers. The figure shows 95% confidence intervals. There are two arbitrarily “low” and “high” vote-selling prices. The reason for having both was to control for possible price elasticities. The figure suggests some small differences that are not statistically significant. Consequently, these arbitrary pricing decisions do not threaten the experimental design

Within the framework of regression analysis, the difference-in-means approach is just a bivariate lineal model.²⁵ Instead, the multivariate approach is also a lineal model, but it incorporates covariates. We claim that due to the multivariate’s greater efficiency than that of the difference-in-means approach, the former is a far better approach than the latter. One way of showing the efficiency of a statistical model is by examining its standard errors (King 1986, p. 676): the worse the data’s fit is, the greater the standard errors are, the more imprecise the model is, and the wider are the confidence intervals. Considering the statistical uncertainty of both methods (depicted in Fig. 4), it is easy to see that the multivariate approach is far more efficient than the difference-in-means approach. Since it uses more information when fitting the data (the covariates), it gives more precise estimates (narrower confidence intervals). Furthermore, going beyond efficiency issues, the estimates of both methods are statistically indistinguishable. Since the confidence intervals of

²⁵ With just a constant 1 on the right-hand side of the equation.



both approaches overlap, it is not possible to say that the estimated 7.3% and 6.6% are “smaller” than the estimated 25%.²⁶

Moving forward, the estimated proportion of vote sellers—“List Experiment” in Fig. 4—is calculated using information from subjects with fully observable preferences, i.e., subjects with an item count of 0 or 4. We know that the former would not do anything, and the latter would do all things mentioned in the list (including the sensitive item). Using the identified covariates (income, education, party identification, and political ideology), a model is fitted to predict all subjects with 0’s and 4’s on the left-hand side. Using this information makes obtaining individual-level vote-selling predictions possible, i.e., participants who would do 1, 2, or 3 things on the list (shown in Fig. 7 in the “[Appendix](#)”). Then, these individual-level predictions are compared with the direct question that all experimental subjects answered. If a subject is a predicted vote seller but answers the direct question negatively, it is inferred that due to concerns of social desirability, she might have chosen to lie.

What is the price for which US citizens would sell their vote?

Participants were also asked to declare which price they considered “too cheap” for their vote. The intention was to capture the respondent’s willingness to sell. The test measures the lowest bound at which participants would perceive the least possible economic benefit but enough to make them sell. Since it is the lowest threshold, the understanding is that a higher price will still be economically attractive.

The results indicate that the average survey respondent would sell his/her vote for \$418 ($N = 189$), a very expensive price. These results are not unrealistic. While the selling price is very high, it matches what others have found. Bahamonde (2018, p. 52) finds that clientelist political parties in Brazil do target affluent voters at considerably higher prices. Part of the argument is that higher levels of economic development not only raise personal income, but also shift the broker’s vote-buying capacity upward.²⁷ That is, higher income does not necessarily stop vote buying; it just makes it more expensive.²⁸

Stokes et al. (2013) analyze the (im)possibility of expensive vote selling. Industrialization has driven up the median income of the electorate, increasing the selling price while turning vote buying into an increasingly expensive strategy for winning elections. Thus, from the demand-side (parties), vote buying is no longer an efficient mass strategy for party machines. Evidently, with the selling price so expensive, political parties cannot catch up with the supply-side, making vote buying in the United States a rare event (as Fig. 1 suggests). This situation has forced party machines to turn to other, less prohibitively costly alternatives. Thus, these results suggest that from the supply-side (i.e., voters), the vote is still up for sale, only for a very high price that party machines cannot afford.

²⁶ I thank the two anonymous reviewers of *Acta Politica* for stimulating this discussion.

²⁷ Similarly, see Abramo and Speck (2001, p. 14). For the Philippine case, see Schaffer (2004).

²⁸ In fact, there is some anecdotal evidence suggesting that a broker purchased one man’s vote for \$800 during the 2010 elections in eastern Kentucky (Shawn 2012, p. 6).



Since the pricing test is based on the direct question, its results require a word of caution. The list experiment does suggest that some respondents lied when directly asked if they would sell their vote. Consequently, we should expect the pricing test to be biased to some degree. Also, only a small proportion of respondents answered the direct question affirmatively. In addition, prices are the product of supply-and-demand dynamics. In this context, prices result from the interaction between parties (buyers) and voters (sellers). This research design observes only the sellers' side. Hence, we limit our inferences even more by thinking about these results as only suggestive of some willingness to sell. Hence, more than acting as definitive and final pricing tests, these findings do seem to suggest that the vote-selling price is high enough to deter political parties from engaging in vote selling. Finally, future research should design and conduct more complex studies where the design incorporates supply-and-demand dynamics.

Who are the most-likely vote sellers?

The proportion of likely vote sellers was estimated using a multivariate approach. The variables used were the most common explanatory factors studied in the clientelism literature. Ultimately, this procedure allows for profiling participants into likely vote sellers. Figure 5 shows estimated vote-selling probabilities at different levels of all variables used in the multivariate approach.

The analyses suggest that Democrats and Liberals are more likely to sell. These findings are in line with research that studies the different constitutive values of Liberals and Conservatives. Political psychologists have found that compared with Conservatives, Liberals construct their moral systems primarily upon narrower psychological foundations. Particularly, Liberals consider less important both the authority/respect and the purity/sanctity dyads (Graham et al. 2009, p. 1029). This might lead Liberals to engage more frequently in behaviors that might be considered “wrong,” such as vote selling. In fact, Gray et al. (2014, p. 7) explain that Conservatives “see impure violations as relatively more wrong.”

Unlike the conventional wisdom (Kitschelt 2000; Calvo and Murillo 2004; Weitz-Shapiro 2012; Carlin et al. 2015), Fig. 5 shows that education and income levels do not make vote selling more likely. Poverty has long been associated with vote selling. Brusco et al. (2004), Stokes et al. (2013), and Nazareno et al. (2008) explain that since the poor derive more utility from immediate transfers relative to returns associated with future (and uncertain) policy packages, clientelist political parties only target the poor. For instance, Weitz-Shapiro (2014, p. 12) explains that “[a]lmost *universally*, scholars of clientelism treat and analyze [this] practice as an exchange between politicians and their poor clients.”²⁹ The evidence presented in this paper aligns with that of others who have recently questioned the importance of this canonical predictor. Szwarcberg (2013) “challenges the assumption [that brokers] will always distribute goods to low-income voters in exchange for electoral

²⁹ My emphasis.



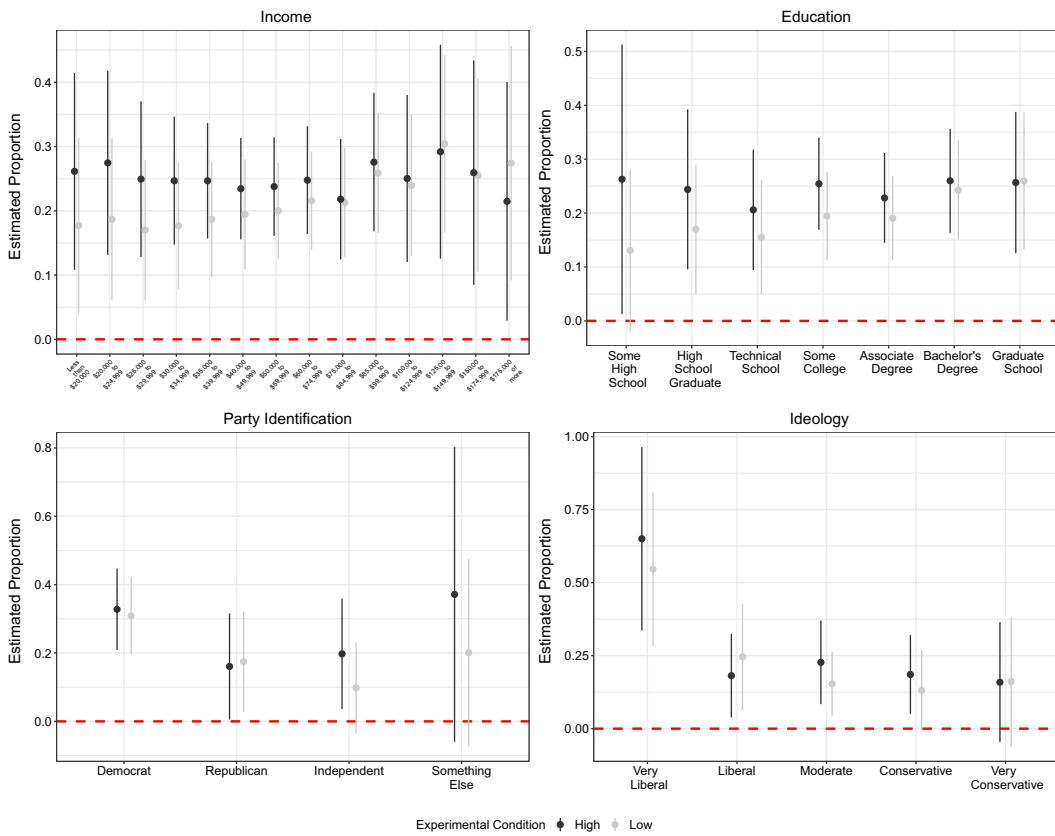


Fig. 5 List experiment: covariates used to estimate likely vote sellers. Note these variables were used in the multivariate statistical model to estimate individual-level probabilities of vote selling. The figure shows the predicted probabilities and their corresponding 95% confidence intervals for income, education, party identification, and ideology. Since the vote-selling prices were set arbitrarily, the reason for two experimental conditions (“low” and “high”) was to control for possible price elasticities. While there are some perceptible changes, they are not statistically significant. Consequently, these arbitrary decisions do not threaten the identification strategy

support,” while González-Ocantos et al. (2012) and Holland and Palmer-Rubin (2015) find that income had little or no effect on vote buying.³⁰ Notably, Bahamonde (2018) explains that brokers target individuals when they are identifiable and groups when brokers need to rely on the spillover effects of clientelism. Both mechanisms occur regardless of individual levels of income.

There do seem to be important substantive differences between the “low” and “high” vote-selling treatments. That is, factors that heavily determine economic status (income and education) seem to be more elastic to marginal increments in the buying price. As Fig. 5 shows, low-income and less-educated individuals are willing to sell their votes in a similar proportion to wealthier and more-educated respondents. However, poorer and uneducated individuals are more willing to sell their votes, conditional on higher prices. This might indicate that for them, behaving

³⁰ Relatedly, González-Ocantos et al. (2014, p. 205) and Corstange (2012, p. 494) also find very weak results for education in Peru and Nicaragua, and in Lebanon, respectively.



illegally is worthwhile but only when the payoff is “large enough.” These results are in line with those of experimental and applied economists who argue that “risk aversion decreases as one rises above the poverty level and decreases significantly for the very wealthy” (Riley and Chow 1992, p. 32). In other words, less-educated and low-income individuals, who are more fragile and precarious, tend to avoid risks and, hence, illegal activities. On the contrary, higher-income and more-educated individuals seem unaffected by the different stimuli and sell their vote in the same proportion, regardless of the price. For instance, highly educated individuals (graduate school level) sell their vote in the same proportion, under both the “low” (26%) and “high” (26%) conditions.

General discussion

Two conflicting pictures emerge. On the one hand, leaving aside concerns about social-desirability bias, we “know”—using nonexperimental data—that most people have never been offered the possibility to sell their vote (as per Fig. 1). On the other hand, the results presented here strongly suggest that they *would*. While buyers (e.g., parties) are not buying, a large proportion of latent vote sellers is willing to sell their vote.

While vote buying/selling in the United States was commonplace during the nineteenth century, higher median incomes have increased the cost of this strategy as a feasible tool to win elections, in turn, making vote buying rare in the United States. The paper confirms this hypothesis by suggesting that an important estimated proportion of US voters—25%—is very much willing to sell their vote, but for an estimated very expensive price—\$418. Overall, these results are striking, and the author is not aware of any other experimental design in which subjects in an industrialized democracy are asked whether they would sell their votes, and, moreover, which produces positive results. The paper began by establishing the tension between supply and demand sides within a clientelist relationship and noting that qualitative research usually focuses on vote selling, while quantitative studies usually focuses on vote buying. Furthermore, most of the literature concentrates its efforts on studying developing countries, mostly paying attention to realized clientelist transactions. As discussed, both aspects pose threats of selection bias to our inferences. This paper tries to fill these gaps by studying hypothetical vote selling via an experimental design implemented in an advanced democracy.

While the paper is rather descriptive, the author believes that the exercise was worth pursuing. The experimental evidence of a large critical mass willing to sell their votes in a developed country is novel. It is hoped that the paper sets the stage for future research and encourages other scholars to field the experimental design presented here in a comparative setting, to include both developed and developing countries. Future research should also consider different values placed on different offices.³¹ It is reasonable to think that presidential, Senate, House, state-legislature,

³¹ I owe this point to Christopher Chambers-Ju.



mayoral, and city-council elections produce different incentives and constraints regarding buying and selling votes. Also, future research should consider blocking party identification—for example, designing a more complex experiment, in which not only the price varies but also the vote-selling treatment is partisan.

Acknowledgements I thank Virginia Oliveros, Richard Lau, David Redlawsk, Christopher Chambers-Ju, Jessica Price, Maria Akchurin, the 2016 Experimental Research Group in Political Psychology at Rutgers University—New Brunswick, the Social Sciences Seminar at O’Higgins University, and the two anonymous reviewers at *Acta Politica* for their comments. This project was funded by the Center for the Experimental Study of Psychology and Politics at Rutgers University—New Brunswick. Bastián Garrido provided excellent research assistance. Usual caveats apply.

Appendix

Experimental manipulations and vignettes

Distractor paragraph. The next paragraph was used to distract subjects from the main purpose of the study, and also to define vote selling.

Washington, D.C.- A department store downtown had a robbery incident last week, reporting several missing iPods from their inventory. Authorities also inform that a group of local residents are trying to ``sell'' their votes to political candidates ahead of a local election for city council. Residents approached some of the candidates running for office and offered to vote for that candidate in return for monetary compensation. In a different subject matter, the local police station released a report on driving habits and behaviors in the Capitol district last week. Finally, cyber-crime has become an increasingly serious issue in the area in the past few years.

Direct Question. All subjects read the next paragraph, and then *all* answered the direct question:

Now you will be entered into a random lottery for the opportunity to do ONE of the illegal things you just read before. This means that you might be randomly offered to hypothetically do ANY of the activities mentioned before.



After a random assignment, you have been selected for the opportunity to hypothetically sell your vote. This means that you will have the hypothetical opportunity to accept money from a candidate for your vote. Would you be willing to accept the offer, assuming you would not go to jail? By selecting ``Yes,'' you could earn up to \$1,000.

Testing for design effects

See Table 3.

Table 3 Test for list experiment design effects

Respondent types	Low condition		High condition	
	Estimate	Standard error	Estimate	Standard error
(y = 0, t = 1)	0.0031	0.0346	0.0183	0.0351
(y = 1, t = 1)	-0.0063	0.0349	-0.0345	0.0353
(y = 2, t = 1)	0.0169	0.0226	0.0299	0.0237
(y = 3, t = 1)	0.0519	0.0113	0.0593	0.0126
(y = 0, t = 0)	0.3429	0.0242	0.3277	0.0249
(y = 1, t = 0)	0.2982	0.0347	0.3264	0.0351
(y = 2, t = 0)	0.2453	0.0299	0.2322	0.0307
(y = 3, t = 0)	0.0481	0.0193	0.0407	0.02

Since the Bonferroni-corrected p values of the *low* (0.8567) and *high* (0.3298) conditions are above the specified α (0.05), I fail to reject the null of no design effects

Geographical distribution of survey respondents

See Fig. 6.



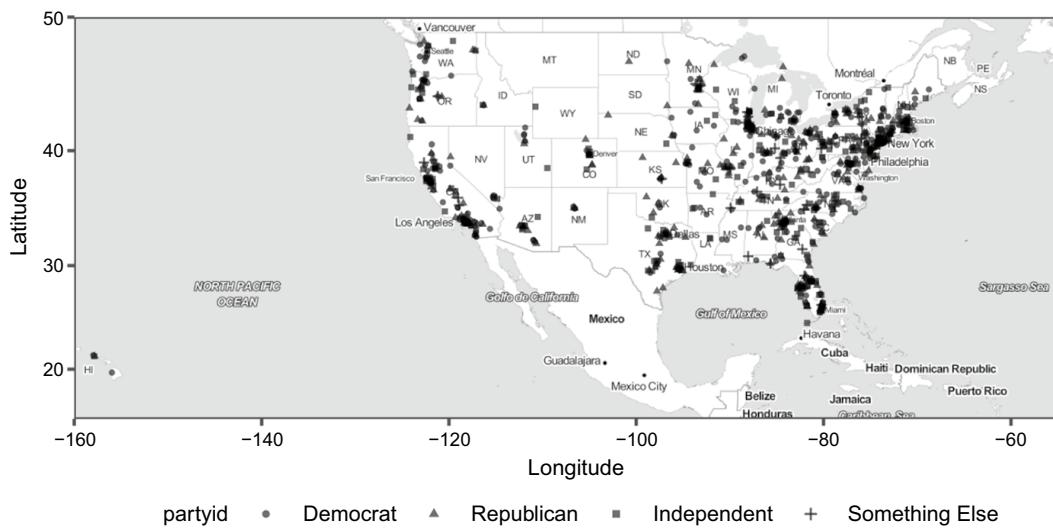


Fig. 6 Geographical distribution of survey respondents by party identification

Individual predictions

The vertical axis of Fig. 7 shows the estimated probabilities of the entire experimental sample, sorted across the horizontal axis. The figure is relevant as it openly shows the amount of uncertainty of the statistical estimates. Ultimately, these individual-specific predictions will be used to profile likely vote sellers.

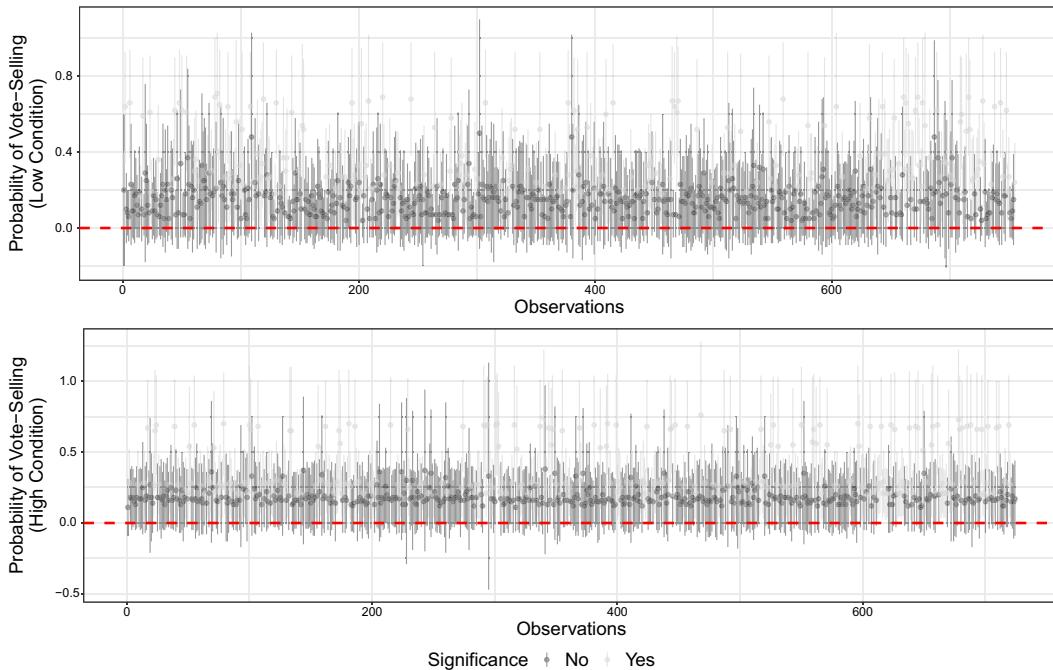


Fig. 7 Individual estimated probabilities of vote selling. Note figure shows the individual probabilities of vote selling ($N = 1479$) under the "low" and "high" conditions. After fitting the model, and following the advice of Blair and Imai (2012) and Imai et al. (2015), individual probabilities of vote selling under the "low" and "high" conditions were estimated. The figure also shows 95% confidence intervals



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Employment effects of COVID-19 across Chilean regions: An application of the translog cost function

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Abstract

Estimating an aggregated translog cost function for the period 2013–2018, and using alternative scenarios of product loss based on expert projections, this article provides a preliminary forecast of the regional employment effects of COVID-19 across Chilean regions. The total estimated loss in the average scenario was around 705,000 jobs (577,000 in the optimistic and 870,000 in the pessimistic scenarios). Relative impacts were spatially heterogeneous, ranging from 1.5% (Antofagasta Region) to 13.6% (Los Lagos Region) of total regional jobs in the average scenario. Estimated impacts may inform regionally-targeted social protection and economic stimulus policies at a time in which the virus has not fully spread and total regional employment impacts have not been realized. In any region and scenario, estimated losses were sizeable and call for rapid and spread implementation of job and production protection initiatives recently passed as well as others still being discussed in congress.

KEY WORDS

Chile, COVID-19, forecasting, regional employment, translog cost function

JEL CLASSIFICATION

I15; J23; R11



1 | INTRODUCTION

COVID-19 is considered the greatest threat to world health since the Spanish Flu of 1918,¹ and the economic impacts are expected to be comparable in magnitude only to those of the 1929 Great Depression (IMF, 2020). Strict policies have been implemented by most governments to contain the spread of the disease during what has been called the “Great Lockdown” (IMF, 2020). As a result, around half of the world population is currently under confinement.² Restrictions to production, consumption and trade start striking harshly the global and national economies. For instance, the US Federal reserve reported a 4.8% reduction in the GDP in the first quarter of 2020. China's GDP, shrank by 6.8%, an outcome that has not been seen in 40 years. In addition, Italy and France fell into a technical recession in the first months of 2020. International trade is expected to decrease between 13 to 32% in 2020 (WTO, 2020). Consequently, the global employment impacts of COVID-19 are expected to encompass around 195 million jobs lost (UN News, 2020).

Several studies have reported that economic recessions are usually characterized by high rates of job destruction (Darby, Haltiwanger, & Plant, 1985, 1986; Davis & Haltiwanger, 1990, 1992). However, the COVID-19 crisis is showing some peculiarities in the way that economic impacts unfold, with the risk of turning a transient shock into one with relatively permanent consequences. Guerrieri, Lorenzoni, Straub, and Werning (2020) observed that in the case of the COVID-19 crisis, the demand may overreact to the supply shock, since the shock can be amplified by mechanisms such as firms' closures and job destruction, thus aggravating the recession. In this context, standard fiscal stimuli may be less effective, which warrants the implementation of large-scale social protection policies. Because these market adjustment mechanisms may operate differently (or at least at a different pace) across regions, it is important to gather estimates of regional job losses to inform regional support strategies. However, the economic effects of the pandemic are currently ongoing and still very few estimations of its sub-national economic effects exist. This paper contributes to filling this gap in knowledge by estimating the employment effects of COVID-19 in Chile, an upper middle-income economy in Latin America with large regional inequalities.

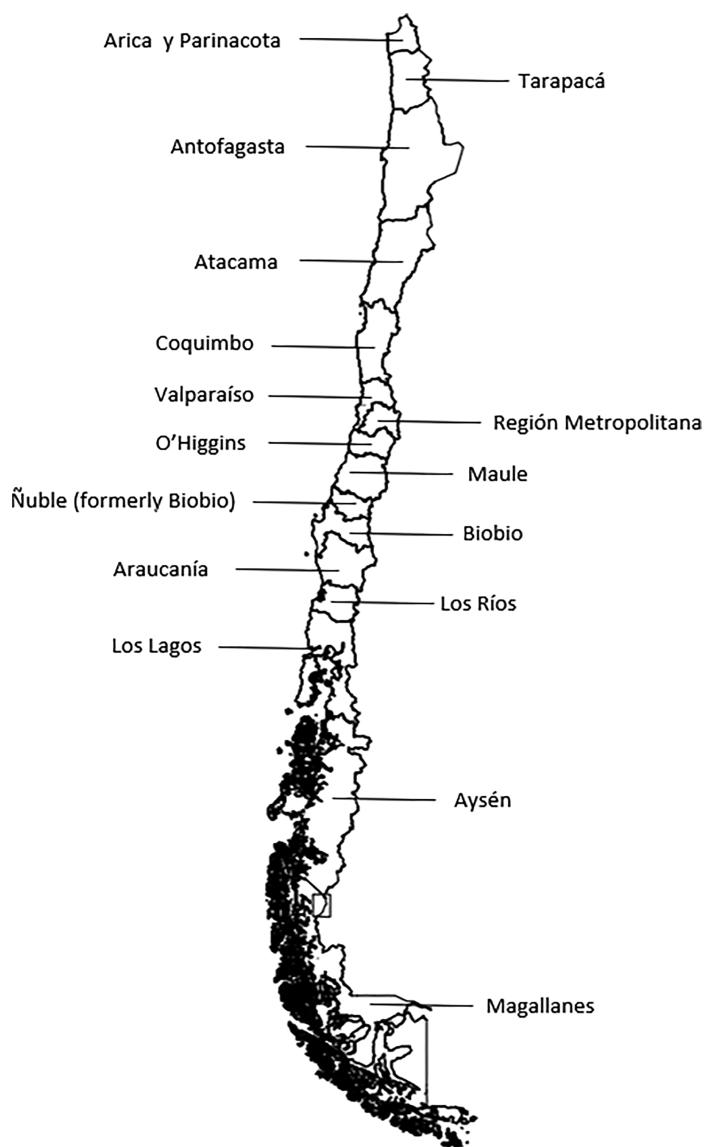
From a regional point of view, Chile is an interesting case to estimate the employment effects of COVID-19. Chile is currently organized administratively in 16 regions, 56 provinces and 346 municipalities (*comunas*). Chilean regions are arranged one on top of each other from north to south (Figure 1). In this paper, we consider the 15 regions that existed up to the end of 2018.³ The population of Chile is highly concentrated in the metropolitan region of Santiago, Chile's primate city (with around 40% of the national population) and the economic and political core of the country. However, Chile's regional labour markets are extremely heterogeneous. Although most jobs across all Chilean regions constitute retail and unsophisticated services, there are significant variations in the relative importance of the different industries, which are directly related to Chile's physical geography. In the sparsely populated regions of the north, for instance, a large percentage of employees work in mining and mining-related services. In the more densely populated regions in the centre and south of the country, in contrast, agriculture and the agri-food industry account for a relatively large share of the labour force (Olfert et al., 2014). The metropolitan region of Santiago is a largely urban economy, oriented to services and with a much more diverse industrial structure. We approached such heterogeneity by following a regional approach to the estimation of unemployment effects of COVID-19.

This estimation is based on fitting an aggregated cost function using data from the Chilean regions for the period of 2013–2018. We used a flexible functional form (translog) fitted using regional data from several official sources. Despite data limitations, the estimation results are largely consistent with theory. Estimated (region-specific)

¹See <https://www.telegraph.co.uk/news/2020/03/06/coronavirus-poses-serious-threat-public-health-since-spanish/>

²See: <https://www.euronews.com/2020/04/02/coronavirus-in-europe-spain-s-death-toll-hits-10-000-after-record-950-new-deaths-in-24-hou>

³The region of Ñuble was officially established in September 2018. In all calculations, Ñuble was treated as part of the former region of Biobio from which it originates.

**FIGURE 1** Chilean regions

elasticities were used to forecast the impact of Covid-19 on employment across Chilean regions due to product losses, using available expert forecasts to define three scenarios of national product loss. Results indicate a total loss of around 705,000 jobs in the average scenario. In the optimistic scenario, the estimated loss is around 577,000, and 870,000 is estimated in the pessimistic scenario. The approach followed here is based on dual production theory and is similar to the method used by Anríquez and López (2007) to study the employment and wage effects of agricultural expansion in Chile during the 1990s. This approach is grounded on well-established results in microeconomics, and is not particularly data-intensive. However, despite its strengths and applicability, we are unaware of its use for studying the regional employment impacts of COVID-19 (or other public health crises). Thus, this method may provide an econometric alternative to early estimations based on assessments of occupations at risk (Lund, Ellingrud, Hancock, Manyika, & Dua, 2020; Muro, Maxim, & Whiton, 2020).

As expected, the bulk (32%) of predicted job losses was estimated in the metropolitan region of Santiago. However, this is not the region with the largest estimated impacts in relative terms. The results indicated that the relative losses ranged from around 1.5% of regional jobs in the region of Antofagasta (a mining region in the north) to around 13.6% in the region of Los Lagos (a region in the south, mainly oriented to agriculture, aquaculture and the downstream agri-food industry). In any scenario and region, estimated impacts were considerable; therefore, the

implementation of determined and widespread policies for the protection of employment and production is well warranted.

The scope of this paper is admittedly limited since it does not provide explanations of the mechanisms by which COVID-19 impacts regional labour markets nor does it compare our forecasts against others obtained using alternative methods. However, this paper contributes to the body of literature on the economic impacts of COVID-19 in several ways. First, it proposes a theory-grounded, regression-based method to account for the employment effects of the disease, a method which is feasible to implement with regional data usually available. Second, it provides statistical inferences at the subnational level, which we think are useful for Chilean regional scientists and national and regional decision makers concerned with the labour impacts of the pandemic. Third, in assessing our estimations, we observed that in the first months of the pandemic in Chile, massive job losses coincided with many workers (many of them informal) becoming inactive. This phenomenon is itself intellectually interesting and politically relevant. While skilled workers are likely to cope better with the pandemic - for instance, by offering their services from home - unskilled workers lack what has been a traditional unemployment-buffering mechanism. This is an important phenomenon to monitor in the different regions of the country. To the best of our knowledge, we are unaware of previous work noting this adjustment in Chilean labour markets in the present COVID-19 context.

The following section presents the method used for estimating the employment effects of COVID-19 in Chilean regions. Section 3 presents the results, and the final section concludes this work.

2 | METHOD

The method for estimating the employment impacts of COVID-19 across Chilean regions is based on dual production theory (Chambers, 1988; Diewert, 1974). We fit a system of equations comprised of an aggregated cost function and the corresponding factor demand equations, using aggregated data from Chilean regions for the period of 2013–2018. Estimated output elasticities were used to calculate job losses in alternative scenarios, which were defined according to expert forecasts of product loss for the Chilean economy due to COVID-19. Product losses were allocated to regions using the initial sectorial impacts in the metropolitan region of Santiago and the regional shares in each sector.

The econometric model uses a translog specification, a flexible functional form which is a second-order approximation of an arbitrary cost function. The properties of the translog cost function have been previously described in the literature (e.g., Berndt, 1991; Christensen, Jorgenson, & Lau, 1971). The use of a flexible functional form has the advantage of, first, acknowledging the substitution relationships among production factors while imposing minimum restrictions to the underlying production technology. Second, it allows for imposing parametric restrictions which are consistent with standard production theory. While the econometric approach proposed here has several advantages, it also has some limitations. The translog cost function is based on a local approximation (i.e. within the neighbourhood of a point) and does not guarantee good “regional” (that is, in a wider region of the factor price space) approximations nor global satisfaction of the regularity conditions (Pollak, Sickles, & Wales, 1984). Moreover, at the cost of flexibility, many parameters must be estimated, which may reduce statistical power when performing inference in small samples like ours (Finch & Finch, 2017). However, we believe this method is functional for the problem at hand, particularly considering the still limited information available on the regional labour impacts of the pandemic.

The starting point is the following translog specification with technological change proposed by Diewert and Wales (1987):

$$\begin{aligned} \ln C(p, y, t) = & \alpha_0 + \sum_i \alpha_i \ln p_i + \alpha_y \ln y + \alpha_t (t - t^*) + \left(\frac{1}{2}\right) \sum_i \sum_j \alpha_{ij} \ln p_i \ln p_j + \sum_i \alpha_{iy} \ln p_i \ln y \\ & + \sum_i \alpha_{it} (t - t^*) \ln p_i + \alpha_{yt} (t - t^*) \ln y + \frac{1}{2} \alpha_{tt} (t - t^*)^2, \end{aligned} \quad (1)$$



where C is the production cost, i,j index factors, p is the factor's price, y is the output, t is the period and t^* is a reference year, such that $t-t^*$ is a measure of technological change. Homogeneity of degree one in factor prices is imposed through the following parametric restrictions in Equation 1 (Ryan & Wales, 2000):

$$\sum_i i = 1; \sum_i i_y = 0; \sum_i i_t = 0; \sum_j i_{ij} = 0 \text{ for the } i = 1, \dots, n \text{ factors.} \quad (2)$$

By Shepard's lemma, one arrives at the conditioned factor demands, as cost shares (s), differentiating 1 with respect to factor prices. For each factor i :

$$s_i(p, y, t) = i + \sum_j i_{ij} \ln p_j + i_{iy} \ln y + i_{it} (t - t^*), \quad \forall i. \quad (3)$$

The theoretical restriction of symmetry of the second-order derivatives of the cost function (Young's theorem) is imposed by the following restrictions to the cross-price coefficients in the n factor share equations 3:

$$i_{ij} = j_i, \quad \forall i, j. \quad (4)$$

Although Equation 1 contains all the parameters in 3, the system 1–3 (with parametric restrictions 2 and 4) is jointly estimated, in order to increase the statistical efficiency of the estimates (Hussain & Bernard, 2018). Considering the available data, our estimation includes three factors: labour, machinery, and construction. However, of the three share equations in 3, we only estimated the labour and machinery equations, to avoid a singularity problem. System 1–3 is estimated using Zellner's (1962) seemingly unrelated regressions in its iterative version (IT-SUR), which ensures that the arbitrary decision of which equation to exclude carries no consequences in the estimation results (Baum & Linz, 2009).

A check of global concavity of the cost function in each sample point was performed using the approach proposed by Diewert and Wales (1987), and implemented in the Stata software by Baum and Linz (2009). Diewert and Wales (1987) showed that the cost function will be (quasi)concave if the matrix M :

$$M = H - S^k + SS', \quad (5)$$

is negative (semi)definite. This will depend on all the eigenvalues () being (non-negative) positive. In 5, H is the Hessian matrix, S is the shares matrix and S^k is a diagonal matrix of shares, all of order $k \times k$, with k being the number of factors in the cost function.

Estimated coefficients of system 1–3 were used to calculate changes in total costs associated with changes in output. Total labour costs (average wages times the number of workers) (C_l) equals:

$$C_l = C * s_l. \quad (6)$$

Taking logarithms in 6 and differentiating both sides with respect to the output, after some manipulations, the change in total labour cost (dC_l) in region r as a function of estimated coefficients is:

$$dC_{lr} = C_{lr} * d\ln y_r (c_{yr} + s_{ly}/s_{lr}), \quad (7)$$

where $c_{yr} \equiv \left(\frac{d\ln C}{d\ln y} \right)_r$ is the output elasticity of total costs obtained from the estimation of 1, and $s_{ly} \equiv \frac{ds_l}{d\ln y} \equiv a_{ly}$ is the output semi-elasticity of the labour share in total costs. It worth noting that since it is dependent on the values of the covariates, the output elasticity of total cost (c_{yr}) in Equation 9 is specific for each observation, while the output

**TABLE 1** The data

Variable in the model	Empirical variable	Source
y (output)	Regional GDP (thousands of millions Ch.\$)	Central Bank of Chile
p_l (price of labour)	Average income of employed people in the region as an index (base 100 = metropolitan region in 2013)	National Institute of Statistics, national employment survey (ENE), (Nov.-Jan. moving quarter).
p_k (price of machinery)	Capital goods imports price index (national) (base 100 = 2013)	Central Bank of Chile
p_c (price of constructions)	Deflator of fixed capital consumption in the construction sector (national) (base 100 = 2013)	Central Bank of Chile
C_l (labour costs)	$C_l = p_l \times \text{number of employed people in the region (thousands of millions Ch.$)}$	Employed people: National Institute of Statistics, national employment survey (ENE), (Nov-Jan. moving quarter).
C_k (machinery costs)	Fixed capital consumption in machinery and equipment (thousands of millions Ch.\$). Allocated to regions using the share of each region in total capital.	Fixed capital consumption in machinery and equipment: Central Bank of Chile. Regional share in total capital estimated by Cerda (2018).
C_c (construction costs)	Fixed capital consumption in construction (housing plus rest of construction) (thousands of millions Ch.\$). Allocated to regions using the share of each region in total construction workers each year.	Fixed capital consumption in construction (housing plus rest of construction): Central Bank of Chile. Regional share o in total construction workers each year.: National Institute of Statistics, national employment survey (ENE), (Nov.-Jan. moving quarter).
C (total cost)	$C_l + C_k + C_c$	
s_i	Share of factor i in total cost: $s_i = C_i/C$	

elasticity of the labour share (η_{ly}) is constant across the entire sample space. From Equation 7, it is clear that the change in labour triggered by a change in output includes the effect of output changes on total regional cost (η_{cy}) and factor substitution effects due to changes in the production scale (η_{sy}).

Changes in total labour costs due to changes in output are then converted into changes in jobs (dl) using the average incomes of workers in each region (p_{lr}):

$$dl_r = \frac{dC_{lr}}{p_{lr}}. \quad (8)$$

The system 1–3 is fitted using annual data from the 15 Chilean regions existing up to 2018 and for the period 2013–2018. Regional GDP as chained volume (based on the 2013 Chilean input–output matrix) in thousands of millions Ch.\$ (variable y) is reported by the national accounts system managed by the Chilean Central Bank. Fixed capital consumption in machinery (in thousands of millions Ch.\$) is used as a proxy of machinery costs (C_k) and was retrieved from the same source. The fixed capital consumption in machinery was allocated to regions using regional shares in total capital calculated by Cerda (2018). We used the price index of capital goods imports (base 2013 = 100) as a proxy of the price of machinery (p_k). This price index is also reported by the Chilean Central Bank. This variable does not have regional variation, which can introduce some measurement errors. However, as argued by Anríquez and López (2007), the mobility of capital goods across regions is subject to few frictions due to a well-integrated capital market in Chile; thus, the price of capital should not have major regional differences. Fixed capital consumption in construction (housing

**TABLE 2** Expert forecasts of product growth for the Chilean economy in 2020

Source	Forecast end of 2019 (% of GDP) (1)	Forecast in June 2020 (% of GDP) (2)	Impact of COVID-19 (% of GDP) (1)–(2)
World Bank	2.5	−4.3	−6.8
OECD	2.4	−5.6	−8
OECD 2/1	2.4	−7.1	−9.5
ECLAC	1	−5.3	−6.3
Average			−7.7

Note: /1 OECD refers to the forecast with only one virus outbreak and OECD2 with a second.

plus the rest of construction, in thousands of millions of Ch.\$) was used as the construction costs variable (C_c), and was also taken from the national accounts system. The capital consumption in construction was allocated to regions using the share of each region in total construction workers each year, with the share calculated using the National Employment Survey (ENE, mobile quarter November–January) run by the Chilean National Institute of Statistics (INE). For the price of construction (p_c), we used the deflator of fixed capital consumption in the construction sector in the national accounts system (base 100 = 2013), also unavailable for regions. This is another source of measurement error, which is possibly more important due to, for instance, regional differences in land prices. These are differences that we cannot control with the data at hand. The average income of workers in each region each year was obtained from the INE's Supplementary Survey of Incomes (ESI), an extra module of the ENE survey added each year in the mobile quarter October–December. The workers' average income was multiplied by the regional number of workers to calculate the regional labour costs (C_l) each year. The workers' mean income was turned into an index (base 100 = the metropolitan region of Santiago in 2013) to use in the regressions as the labour price variable (p_l). Total regional costs were obtained adding labour, machinery and construction costs; subsequently, the share of each factor (s_{lr}, s_{kr}, s_{cr}) was computed. A summary of the variables used in the econometric estimation can be found in Table 1.⁴

We defined three scenarios of product loss following the most recent expert forecasts for the Chilean economy (World Bank, OECD and United Nations' ECLAC). The product impact of COVID-19 was defined as the difference between the national product change forecasted before the outbreak of the virus (December 2019–January 2020) and after the outbreak (first days of June 2020). The *average* scenario was defined as an impact equal to the average of the four forecasts and amounts to 7.7%. The *pessimistic* scenario was defined according to the largest impact, the OECD forecast with a second COVID-19 outbreak, which amounts to a 9.5% reduction. The *optimistic* scenario corresponds to the mildest product impact, by UN's ECLAC, which indicates a shrinkage of around 6.3%. The scenarios and the simulation parameters are summarized in Table 2.

Using the expert forecasts, the forecasted product impact in each scenario was allocated to regions using regional weights (w_r), such that Equation 8 now reads as:

$$dl_r = C_{lr}/p_{lr} * dy * \gamma_r / (y * w_r) (C_{yr} + s_{ly}/s_{lr}), \quad (9)$$

where y is the national product in 2019 and dy is the national product loss (in levels) for 2020 in each scenario. γ_r is a regional parameter used for allocating the forecasted national product loss in 2020 among the (former) fifteen regions, and w_r is the regional share in total product (in 2018, last year available).⁵

The γ_r parameter was calculated as:

⁴Data complied by the Chilean Central Bank was retrieved from its statistics website: <https://www.bcentral.cl/areas/estadisticas>. Data reported by INE was obtained from INE.stat (<https://stat.ine.cl/>) and INE databank (<http://bancodatosene.ine.cl/>) websites.

⁵Total here means the sum of the 15 regions, which is not equal to the national product, as the national GDP includes items which are not suitable for regional allocation.

**TABLE 3** System (1)–(3) estimation results

Variable	C	sl	sk
lnpl	0.940*** (0.098)	-0.065*** (0.017)	0.015 (0.020)
lnpl*lnpl	-0.065*** (0.017)		
lnpk	0.060 (0.098)	0.015 (0.020)	-0.000*** (0.000)
lnpk*lnpk	-0.000*** (0.000)		
lnpc	0.000*** (0.000)	-0.052** (0.024)	-0.000 (0.000)
lnpc*lnpc	0.056*** (0.020)	-0.006 (0.026)	0.000*** (0.000)
lny	0.489** (0.202)	-0.001 (0.005)	0.005 (0.006)
lny*lny	0.020* (0.011)		
lnpl*lnpk	0.015 (0.020)		
lnpl*lnpc	-0.006 (0.026)		
lnpl*lny	-0.001 (0.005)		
lnpk*lnpc	-0.000 (0.000)		
lnpk*lny	0.005 (0.006)		
lnpc*lny	-0.005 (0.005)		
t-t*	0.110 (0.108)	0.000** (0.000)	0.000*** (0.000)
(t-t*)2	-0.013** (0.006)		
lnpl*(t-t*)	0.000*** (0.000)		
lnpk*(t-t*)	0.000*** (0.000)		
lnpc*(t-t*)	0.000*** (0.000)		
lny*(t-t*)	0.009 (0.010)		
Constant	-2.889*** (0.985)	0.940*** (0.098)	0.060 (0.098)
Observations	88	88	88
chi2 p-value	0.0000	0.0000	0.4725
Breusch-Pagan test of errors independence (p-value)	0.0000		

Note: significant at *10%, **5%, ***1%. Standard errors in parenthesis.

$$r = \sum_s w_s^{RM} * w_{sr}. \quad (10)$$

w_s^{RM} is the contribution of each economic sector ($s = 1, \dots, 21$) to total employment change observed in the metropolitan region of Santiago between February–April 2020 and February–April 2019 (same quarter to avoid seasonality problems), and w_{sr} is the share of region r in total workers in the s sector. Thus, r considers the likely sectoral differences in the employment effects of the pandemic and the importance of each sector in each regional economy.⁶

The use of employment changes in the metropolitan region to calculate the r parameter is grounded in the following reasons. In the period between the first draft of this paper and the revised version, the National Institute of Statistics released the results of the National Employment Survey (NES) for the moving quarter February–April 2020, which allows for assessing the initial employment effects of the virus outbreak. Currently (the first days of June 2020), the Chilean National Government has opted for a strategy of “dynamic lockdowns,” in which specific areas (municipalities or specific areas within a municipality) are locked down and released based on the epidemiologic situation, an assessment which is largely based on the spatial concentration of detected COVID-19 cases. By the end of April 2020, the metropolitan region of Santiago accounted for around 80% of total cases (twice its share in the national population) and was the only Chilean region where a sizeable population went into a prolonged (more than

⁶This way of regionally allocating the impacts of COVID-19 was motivated by the comments of an anonymous reviewer to whom we are very grateful.

**TABLE 4** Output elasticities of total costs in 2018 for Chilean regions

Region	c_{yr}	Standard error	P-value	95% Confidence interval
Arica y Parinacota	0.838	0.057	0.000	0.726 0.950
Tarapacá	0.884	0.046	0.000	0.794 0.973
Antofagasta	0.942	0.050	0.000	0.845 1.039
Atacama	0.882	0.046	0.000	0.792 0.972
Coquimbo	0.892	0.045	0.000	0.804 0.980
Valparaíso	0.934	0.048	0.000	0.840 1.028
Metropolitana	0.991	0.068	0.000	0.858 1.124
O'Higgins	0.911	0.045	0.000	0.823 0.998
Maule	0.899	0.044	0.000	0.812 0.986
Biobio	0.930	0.047	0.000	0.838 1.023
Araucanía	0.889	0.045	0.000	0.801 0.978
Los Ríos	0.861	0.050	0.000	0.762 0.959
Los Lagos	0.896	0.045	0.000	0.809 0.984
Aysén	0.828	0.061	0.000	0.708 0.948
Magallanes	0.854	0.052	0.000	0.752 0.956

a few weeks) lockdown. For instance, a large section of the municipality of Santiago, where the city's main business and public administration district is located, entered into lockdown on 26 March 2020, along with six other municipalities, amounting to around 1.3 million people in lockdown. By the first days of June 2020, the municipality of Santiago had not been released yet; on the contrary, the strategy of dynamic lockdowns had shifted to locking down the entire greater Santiago City (34 municipalities) and some surrounding municipalities. The Santiago metropolitan region is the core and largest region, constituting around 40% of the national population, and has the highest sectoral diversity, with all economic sectors well represented. Thus, the results of the February–April round of the NES survey for the metropolitan region of Santiago provide what is arguably the best snapshot currently available of the sectorial employment effects in Chile in the likely event of the spread of the disease within the entire country and the subsequent implementation of total regional lockdowns.

To estimate the employment effects of COVID-19 in each region, total labour costs (C_{lr}), average regional workers' income (p_{lr}), the output elasticity of total costs (c_{yr}) and the share of labour in total costs (s_{lr}) were all evaluated for each region in 2018, which was the last year with all the data available. Confidence intervals for the impacts calculated with equation 9 were obtained using the delta method.

3 | RESULTS

Table 3 summarizes the estimation results of system 1–3 for a filtered sample of 88 observations.⁷ The results are largely consistent with the theory. The total marginal effects indicate that costs are non-decreasing in output and factor prices. Factors shares decrease as their prices increase, and point estimates suggest substitution between

⁷The system 1–3 was initially estimated using the full sample of 90 observations (15 regions × 6 years). Estimated elasticities were used to make retrodictions of employment changes in the 2008–2013 period, using Equations 7 and 8. An analysis of the standardized residuals (i.e., the standardized difference between observed and retrodicted employment changes) revealed two clear outliers where the rule-of-thumb criterion of a standardized residual greater than two (in absolute terms) was exceeded. Such outliers were excluded in the final estimations. We are grateful to an anonymous reviewer for motivating this validation exercise.

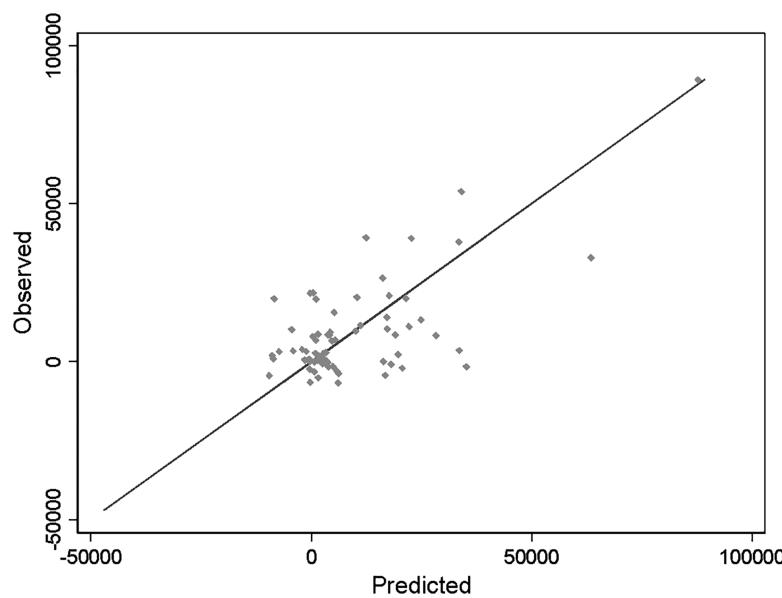


FIGURE 2 Model validation – predicted vs observed regional employment changes for the 2013–2018 period

machinery and constructions. In contrast, there is no clear substitution relationship between labour and the other production factors, although this is probably related to the small sample size and the lack of regional variation of capital and construction prices. Concavity was verified in approximately 76% of the sample. The Breusch-Pagan test confirmed the lack of independence of the errors across equations, which is expected since all derive from the same underlying technology (implicit in the cost minimization problem). Altogether, the results provide confidence in that in spite of considerable data limitations, the estimated translog cost function provides a reasonable representation of the aggregated regional production technology.

Output elasticities of total costs (ζ_{Cyr}) in 2018 and their standard errors are reported for each region in Table 4. They range from 0.828 (Aysén, south end of the country) to 0.991 (metropolitan region of Santiago). At a 95% confidence, in 11 out of the 15 regions, total costs responded less than proportionally to changes in output. In particular, the northernmost region (Arica y Parinacota) and the two southernmost (Aysén and Magallanes) are estimated as the less responsive to output changes. The estimated output elasticity of the labour share (constant across the sample space) indicates, however, that the share of labour in total costs is largely invariant to the scale of production ($s_{ly} = -0.0005$, not significant). This indicates that the impact of COVID-19 on employment would be mostly due to the reduction in output and would not be substantially buffered (nor amplified) by a substitution of factors triggered by the large product shrinkage. Again, we cannot exclude the possibility that the lack of significance of this second elasticity is due to the lack of regional variation in factor prices and the small sample size. As a simple validation exercise, Figure 2 displays observed against predicted employment changes for the period of 2013–2018. Predictions were obtained using the estimated elasticities in Equations 7 and 8. A 45-degree line was added to ease visualization. A simple regression indicated a R^2 of 0.57 and a slope coefficient of 0.74.⁸ Overall, the model seems to capture well the fundamental underlying economic relationships, and the proposed method has been demonstrated to be useful for estimating regional employment effects of COVID-19 in Chile.

Table 5 summarizes the regional parameters used in the calculation of labour impacts with Equation 9, showing the large share (40%) of the Santiago metropolitan region in the national impact (.). The calculated share is, nevertheless, lower than its share in the total product (46%). The region of Antofagasta, the largest mining region in the country, has the lowest calculated share in the national impact (2.4%) relative to its share in the total product (11%). Conversely, Biobio, a region with the third largest urban agglomeration in the country (Concepción), has a high calculated share (12%) in the national impact given its share in the total product (around

⁸The standardized residuals were all below the rule of thumb value of two in absolute terms.

**TABLE 5** Regional parameters for the employment impact calculations

Region	Regional GDP 2018 (thousands of millions Ch. \$)	Regional weight (w_r)	Regional share in total product 2018 (w_r)	Total labour costs 2018 (thousands of million Ch.\$)	Mean workers annual wage 2018 (thousands Ch.\$)	Share of labour in total costs 2018 (%)
Arica y Parinacota	1,111.9	0.009	0.008	436.8	5,819	62.3
Tarapacá	3,433.1	0.016	0.025	1,031.6	6,138	62.8
Antofagasta	14,787.8	0.024	0.106	2,422.4	8,310	59.3
Atacama	3,323.5	0.013	0.024	942.4	6,592	63.4
Coquimbo	4,251.4	0.043	0.030	2,093.7	5,542	67.4
Valparaíso	12,135.3	0.098	0.087	5,584.0	6,589	67.4
Metropolitana	65,031.3	0.404	0.465	27,255.7	8,036	72.5
O'Higgins	6,733.1	0.052	0.048	2,659.4	5,862	70.8
Maula	4,999.9	0.067	0.036	2,692.9	5,287	66.0
BioBio	11,018.5	0.116	0.079	5,361.9	5,550	68.3
Araucanía	3,951.6	0.053	0.028	2,512.0	5,333	64.2
Los Ríos	1,947.4	0.027	0.014	1,122.5	5,868	66.3
Los Lagos	4,706.1	0.060	0.034	2,686.9	6,047	68.9
Aysén	856.1	0.008	0.006	468.3	7,494	67.7
Magallanes	1,654.8	0.010	0.012	890.7	10,134	77.2

Notes: /1 dy = - 11,961.1 (th. MM Ch. \$) in the average, (th. MM Ch. \$) -14,757.2 in the pessimistic and -9,786.4 (th. MM Ch. \$) in the optimistic scenario. s_{ly} = - 0.0005 for all regions.

Cyr in Table 4./2 "Total" costs are the sum of labour, machinery and constructions costs.

**TABLE 6** Estimated impacts of COVID-19 on regional employment

Region	Average scenario			Optimistic scenario			Pessimistic scenario		
	Estimate	95% confidence interval		Estimate	95% confidence interval		Estimate	95% confidence interval	
Arica y Parinacota	-5,468	-6,208	-4,727	-4,474	-5,079	-3,868	-6,746	-7,659	-5,832
Tarapacá	-7,535	-8,313	-6,758	-6,165	-6,802	-5,529	-9,297	-10,257	-8,337
Antofagasta	-4,753	-5,255	-4,251	-3,889	-4,299	-3,478	-5,864	-6,483	-5,245
Atacama	-5,303	-5,854	-4,752	-4,339	-4,789	-3,888	-6,542	-7,222	-5,863
Coquimbo	-36,561	-40,213	-32,910	-29,914	-32,901	-26,926	-45,108	-49,613	-40,603
Valparaíso	-68,895	-75,910	-61,879	-56,368	-62,109	-50,628	-85,000	-93,656	-76,344
Metropolitana	-224,874	-255,276	-194,471	-183,987	-208,862	-159,113	-277,441	-314,951	-239,932
O'Higgins	-34,320	-37,663	-30,976	-28,080	-30,816	-25,344	-42,343	-46,468	-38,217
Maule	-65,848	-72,327	-59,369	-53,876	-59,176	-48,575	-81,241	-89,234	-73,248
BioBio	-102,136	-112,385	-91,887	-83,566	-91,951	-75,180	-126,012	-138,656	-113,367
Araucanía	-59,980	-66,027	-53,934	-49,075	-54,022	-44,128	-74,002	-81,462	-66,542
Los Ríos	-24,500	-27,340	-21,659	-20,045	-22,369	-17,721	-30,227	-33,731	-26,723
Los Lagos	-54,964	-60,405	-49,523	-44,970	-49,422	-40,519	-67,812	-74,525	-61,100
Aysén	-5,279	-6,052	-4,506	-4,319	-4,952	-3,687	-6,513	-7,467	-5,559
Magallanes	-5,013	-5,623	-4,403	-4,102	-4,601	-3,603	-6,185	-6,937	-5,433
Total	-705,428			-577,168			-870,333		



8%). This is due to the specific regional employment structure, which is characterized by a relatively large weight of agriculture and manufacturing, two sectors showing large initial employment impacts. According to the February–April ENE survey, retail, lodging and restaurants and manufacturing are the sectors that have been most affected initially by the COVID-19 shock, while others sectors such as health, education or mining are far less. While high-risk sectors such as lodging and restaurants correspond to the sectors at the greatest risk in the US (Muro et al., 2020), others like mining (described as a high-risk sector in the US) are not among the industries with the highest initial job losses according to the ENE survey. This finding suggests some particularities of the sectorial employment effects of COVID-19 in Chile.

Table 6 summarizes the estimated employment losses in each region and their 95% confidence intervals and Figure 3 maps these losses as a percentage of total regional jobs. Overall, total job losses were more or less proportional to the expert forecasts of product shrinkage, since regional output elasticities were close to unity and the estimated output elasticity of the labour share in total costs was negligible. Total job losses amounted to around 705,000 in the average scenario. In the pessimistic scenario, job losses amount to nearly 870,000 and to 577,000 in the optimistic scenario. Examining the regional variation of impacts, almost a third of the national variation (nearly 225,000 jobs) was estimated in the metropolitan region of Santiago, although this figure was considerably less than its share in national employment (44%). The least impacted region in relative terms would be the region of Antofagasta, a region where mining accounts to around 54% of the regional GDP. In the average scenario, job

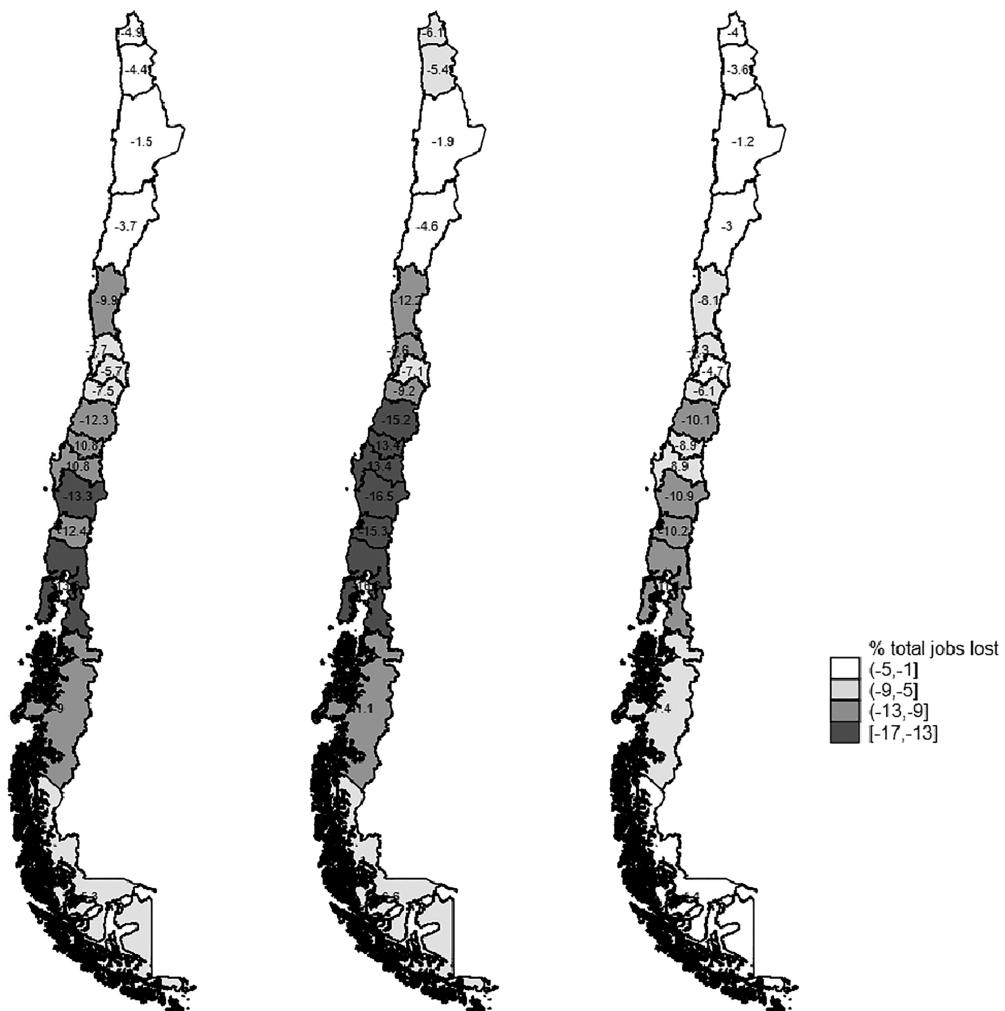


FIGURE 3 Regional employment impacts as a percentage of total jobs. Left: Average scenario. Centre: Pessimistic scenario. Right: Optimistic scenario



losses would be of around 4.800, which means around 0.7% of the national job loss. This is proportionally less than its share in the national employment (3.6%), and only a 1.5% of total regional jobs. While the expenditures in production factors have been relatively sensitive to output variations (Table 4) in Antofagasta compared to other regions, the regional employment structure is highly specialized in mining, a sector that has not been one of the most affected by the disease based on the evidence so far. Antofagasta's economy is highly sensitive to the copper price cycle (Atienza & Modrego, 2019), and the slow recovery in copper-buying countries (mainly China) will likely help mitigate the employment effects of the pandemic. In contrast, the region of Los Lagos is the most affected region in relative terms (13.6% of total regional employment in the average scenario, Figure 3). Los Lagos is a region located in the south of the country, oriented to the production of primary natural resources and its downstream agri-food industry. Agriculture, livestock, forestry, fishing and aquaculture alone account for around 12% of the regional product and manufacturing adds another 26%. Although the region's factors expenditure had an approximately average sensitivity to output changes (Table 4), its specialization in agriculture and (agri-food) manufacturing (two of the sectors most initially affected by COVID-19) has resulted in the large forecasted employment impacts in relative terms. More generally, Figure 3 illustrates how mining regions in the north of the country (Tarapacá, Antofagasta and Atacama) are predicted to have lower relative job losses compared to agriculture and agri-food-oriented regions in the centre and centre-south (Maule, Araucanía, Los Ríos and Los Lagos).

Overall, in all regions and scenarios, the estimated regional employment effects of COVID-19 are sizeable and call for determined actions to protect employment and production within the entire country. Since the pandemic is currently ongoing, and forecasts of economic impacts for the Chilean economy may still change, these estimations should be considered to be a preliminary approximation.

4 | CONCLUSIONS

This paper presents preliminary forecasts of employment losses in Chile at the regional level resulting from the COVID-19 pandemic. Following a methodological approach based on the estimation of an aggregated cost function for the period of 2013–2018 and on dual production theory, the employment losses were calculated for different scenarios of national product loss. The estimations suggest sizeable impacts. In the average scenario, the estimated impact was around 705,000 jobs country-wide, which amounts to 870,000 and 577,000 in the pessimistic and optimistic scenarios, respectively. In regional terms, impacts are heterogeneous across regions, with agriculture and agri-food oriented regions in the centre-south more affected than mining regions in the north.

These estimates are preliminary, and should be taken with caution. A first limitation of this study is based on the data, particularly the lack of regional data on consumption and prices of several production factors. Producing better estimates would be possible with more complete regional data, particularly data on consumption and prices of production factors. A second limitation is that we did not explicitly model the spatial spread of the disease nor the output impacts of COVID-19 in each region. Instead, we relied on expert forecasts for the national economy, which are regionally allocated in a more or less *ad-hoc* way, and implicitly assuming a uniform spread of the virus throughout the country. Finally, statistical relationships were estimated using data for a period of moderate growth of the national economy. However, estimated elasticities were used to forecast job losses in a situation of product loss not seeing during the period for which such elasticities were estimated. It is unclear whether employment adjustments in the COVID-19 context will be directly proportional to those seen in the 2013–2018 period. Indeed, the results of the ENE survey for the moving quarter February–April 2020 suggest that adjustment mechanisms that have not been previously observed, at least in the last decades, are currently taking place in the Chilean labour market. Traditionally, self-employment and informality have been powerful buffer mechanisms containing unemployment outbreaks during previous crises. During the COVID-19 crisis, an unusual rise in the number of inactive people (around 720 thousand people) has accompanied an increase in unemployment of only 0.9 percentage points between January–March and February–April 2020. At



the same time, there was a large reduction in informal jobs (around 415,000). Both these observations suggest that many jobs lost are due to informal and self-employed workers exiting the labour market, a pattern not observed at such a large scale during the 2013–2018 period. This phenomenon is important itself and deserves further research and monitoring by national and regional authorities.

Based on these considerations, total job losses at the end of 2020 may exceed the present forecasts. Indeed, between the last two moving quarters, a period capturing only the initial employment effects of COVID-19, the National Employment Survey (ENE) reported a fall of around 706 thousand in the number of employed individuals already, very close to what we forecasted for the average scenario. The inactive people and the unemployed people behind these figures may amplify the recession in the sense of Guerrieri et al. (2020). If that is the case, the expert forecasts of product losses we are using may also fall short, as the downwards corrections in most expert projections between April and June suggest.

Keeping these caveats in mind, the extent of estimated impacts is large (around 700 to 870,000 jobs potentially lost), calling for resolute actions to protect employment and production in the entire country. As a response to the virus outbreak, sizeable support packages have been implemented in Chile and others are currently being discussed. Actions already implemented include modifications to the national employment insurance to be used in situations of temporary job suspensions (thus avoiding firings), insurance supported by supplementary public funds, state-guaranteed loans with near-zero real interest rates for firms and additional cash and in-kind transfers to vulnerable households. Regarding the reformed unemployment insurance, the last ENE survey indicated an increase of around 365,000 “absent employees” (people with a job that was nevertheless not performed on-site last week), a change of a magnitude which is partly explained by the reformed employment insurance.

Regional authorities should be prepared for a rapid and effective implementation of these support initiatives, maintaining close monitoring of their communities and collaborating in targeting and implementing national support initiatives. Employers can also contribute to this effort by making a correct use of the employment protection insurance, limiting its use to cases when it is truly needed, and rapidly processing the requests to activate the insurance. Financial institutions can also play an important role in helping mitigate the economic consequences of COVID-19, by rapidly and effectively issuing state-guaranteed loans to firms that need them the most, particularly the small and medium-sized firms.

A direct extension of this work is the replication of the analysis for regional industries, which can be carried out to assess the sectoral impacts in each region, thus informing regional protection and recovery strategies. Likewise, a better understanding of the different types of contractual arrangements that are predominant in each productive sector as well as the sensitivity of jobs to economic shocks under these different contractual arrangements, can shed light on more specific employment protection policies for this crisis and other future crises. Finally, this work has not taken into consideration regional interactions which could significantly alter the estimated regional distribution of the impacts of COVID-19. These interactions may stem, for instance, from physical and human capital externalities and regional growth spillover effects (Valdez, 2019), and could be analyzed using spatial econometric methods, inter-regional input-output analysis and/or regional computable general equilibrium models. Global VAR (GVAR) models (Pesaran, Schuermann, & Weiner, 2004), which have been applied to forecasts of regional unemployment in Germany (Schanne, Wapler, & Weyh, 2010), are also a particularly appealing alternative for forecasting and simulating regional impacts of COVID-19. Further studies along these lines can substantially expand our understanding of the mechanisms by which the effects of the pandemic may diffuse across regions.

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Inclusive Institutions, Unequal Outcomes: Democracy, State Capacity, and Income Inequality

Abstract

Although the relationship between democratic rule and income inequality has received important attention in recent literature, the evidence has been far from conclusive. In this paper, we explore whether the redistributive effect of democratic rule is conditional on state capacity. Previous literature has outlined that pre-existing state capacity may be necessary for inequality-reducing policies under democratic rule. In contrast to that intuitive view, this study argues that democratic rule and high state capacity combined produce higher levels of income inequality over time. This relationship operates through the positive effect of high-capacity democratic context on foreign direct investment and financial development. By making use of a novel measure of state capacity based on cumulative census administration, we find empirical support for these claims using fixed-effects panel regressions with the data from 126 industrial and developing countries between 1970 and 2013.

1. Introduction

Median voter and selectorate theories posit electoral democracy as fundamentally equalizing (Acemoglu and Robinson, 2006; Boix, 2003; Bueno de Mesquita et al., 2003; Meltzer and Richard, 1981).¹ However, these redistributive propositions have not received support in recent, more empirically-minded literature (Acemoglu et al., 2015; Remington, 2011; Scheve and Stasavage, 2017; Timmons, 2010; Wong, 2016). The skeptics of the inequality-reducing effects of democratic institutions have noted that deficiencies in mechanisms of responsiveness and accountability, clientelism, interest group capture, and the institutional legacies of authoritarianism may pose serious obstacles to redistributive policies under democratic rule (Albertus and Menaldo, 2018). It has also been suggested that such effects might be heavily context dependent (Dorsch and Maarek, 2019; Soifer, 2013).

Looking at context-conditionality could be a new way forward to clarify both the theoretical and empirical relationship between democracy and inequality. In this paper, we explore whether democracy's impact on inequality is conditioned by state capacity. It might be expected that pre-existing state capacity, in the form of functioning bureaucracies and territorial penetration, would be necessary for redistributive policies under democratic rule (Ziblatt, 2008).² For example, Soifer (2013) focused on the effect of inequality on democratization and argued that inequality-induced redistributive conflict only ensues in contexts of considerable levels of state capacity, which allows the implementation of redistributive taxation and transfer policies.

This study found no empirical support for these intuitive claims. Using fixed-effects panel regression models with data from 126 industrial and developing countries from 1970 to 2013, we show that democratic rule combined with high state capacity leads to increasing income inequality over time. This study's theoretical argument centers on the idea that democracy and high state capacity combined provide a context of optimal property rights protection and contract security. We argue that the high-quality investment environment backed by democratic and high-capacity state institutions increases income inequality through two transmission channels: the higher inflow of foreign direct investment (FDI) and the development of sophisticated financial sectors, which have been associated in recent literature with increasing income inequality. While income concentration in high-capacity democratic environments occurs through market inequality, we contend that fiscal policy in

¹ We both thank the two anonymous reviewers, Martin Dimitrov, Patrick Egan, Robert Kaufman, Victor Menaldo, Thomas Oatley, Andres Sandoval, Xander Slaski, Hillel Soifer, and the participants of the XVI meeting of The Italian Association for the History of Economic Thought and the "Social Sciences Seminar" at Universidad de O'Higgins for their insightful comments. We also thank Jonathan Hanson for sharing the data on census administration. Javiera Tobar provided excellent research assistance. The usual caveats apply.

² We define "state capacity" as state institutions' ability to collect and manage information and to effectively execute policies in different areas, notably including market regulation and contract enforcement ("legal capacity") and resource extraction ("fiscal capacity") (Besley and Persson, 2009: 1219).

these contexts is not able to offset these changes. Multinational corporations and transnational elites become more relevant actors in national politics with increasing FDI flows and financial development, which allows them to exert downward pressure on labor-protecting regulations, redistributive taxation, and transfers.

Evaluating the effect of regime type on inequality at different levels of state capacity poses significant empirical challenges. Both regime type and state capacity tend to be endogenous to inequality levels and other socio-economic variables associated with economic development. Most importantly, democratic rule might create incentives to increase state capacity in order to levy more tax revenue and provide more public goods to citizens. We mitigate these concerns via a careful construction of a state capacity measure based on cumulative census administration, which is unlikely to reflect government policy priorities that are endogenous to democracy levels. We also use instrumental variables to relieve endogeneity concerns by instrumenting regime type with regional democratic diffusion. Our results, based on annual panel data, are robust to the system generalized method-of-moments estimator (GMM) analysis, alternative measurements of democracy, various sub-sample restrictions, such as excluding industrial and former Warsaw Pact countries from the analysis, and different lag structure specifications.

This paper joins several recent contributions in stressing the importance of conditional factors in the regime-inequality relationship. Our contribution is parallel to that of Dorsch and Maarek (2019), who argue that the effect of democracy on inequality is conditioned by the initial level of inequality. According to their argument, democratization tends to bring initial high or low levels of inequality to the “middle ground,” through either redistributive social policy or market reforms. We point to another factor that conditions the effect of democratic rule on inequality: state capacity, which principally affects inequality in the market income phase. This might well illuminate the institutional underpinnings of increasing within-country inequality in the last four decades in many parts of the world.³ The institutionalist literature has implicitly assumed that “inclusive institutions” do not only promote development but also more equal income distribution, at least in the long term (Acemoglu et al., 2001). In this paper, we provide evidence that this conclusion may not be warranted and that “inclusive” institutions—captured in the combination of democratic regime type and high-capacity state institutions—might well lead to a trend of steady increases in income inequality through different policy mechanisms.

The paper is structured as follows. In Section 2, we give an overview of the recent literature on democracy and inequality. In Section 3, we present our theoretical argument on the interactive relationship between democratic rule, state capacity, and inequality. Then, in Section 4, we present our research design and address issues of the measurement of key

³ There have been major exceptions to that trend, especially in Latin America, where inequality has declined since the end of 1990s, albeit very slowly.

variables. In Section 5, we present our results from fixed-effects panel models and the corresponding robustness checks. In Section 6, we test the transmission channels behind this relationship. In Section 7, we provide concluding remarks.

2. Democracy and Inequality

Democratic institutions have been conceptualized as a major source of responsiveness and accountability in the political economy literature, providing electoral incentives to redistribute income. Leaders in democratic nations need widespread support to achieve and sustain power and are, therefore, more likely to move beyond their narrow set of personal interests by appealing to a wider public through public policies (Meltzer and Richard, 1981). Compared to authoritarian polities, widespread enfranchisement in democracies is likely to result in higher public goods provision, which may help the poor to benefit from economic growth via investments in human capital (Baum and Lake, 2003; Lindert, 2004; Morgan and Kelly, 2013). These policies are expected to produce more equal income distribution over time.

Despite these plausible theoretical mechanisms, empirical evidence has not offered solid support for the inequality-reducing effects of democracy. Several empirical studies incorporating various regions of the developing world find that democracy does not induce lower income inequality (Acemoglu et al., 2015; Dorsch and Maarek, 2019; Gradstein and Milanovic, 2004; Timmons, 2010; Wong, 2016), more progressive taxation (Scheve and Stasavage, 2014), or pro-poor social policies (Mulligan, Gil, and Sala-i-Martin, 2004; Pagalayan, 2020; Ross, 2006). The causes of this “democratic unresponsiveness” have constituted a major puzzle for researchers. At the same time, some democracies might affect inequality more than others, and a focus on the social and institutional contexts in which democracies operate could offer a new way forward for fruitful theorizing.

In this paper, we concentrate on the question of whether democracy’s effect on inequality is conditioned on state capacity. We define “state capacity” as state institutions’ ability to collect and manage information and to effectively execute policies in different areas, notably including market regulation and contract enforcement (“legal capacity”) and resource extraction (“fiscal capacity”) (Besley and Persson, 2009: 1219). High state capacity implies a monopoly on violence over a territory and a cohesive and competent civil service and courts operating on the basis of well-established rules and routines. State capacity has received important attention recently as an explanatory variable in determining development outcomes (Hanson, 2015; Knutsen, 2013).

The previous literature has hinted that inequality reduction is more likely when both the political-electoral incentives stemming from regime characteristics and the state capacity to

redistribute exist. In low-capacity states, democratization should not matter for redistributive outcomes given their inability to collect taxes and implement social policy. Revenue extraction and policy implementation—both crucial for income redistribution—are dependent on the state's ability to penetrate its territory and implement decisions (Ziblatt, 2008). In low-capacity states, elites are able to escape taxation, lowering the state's ability to provide public goods and transfers (Scott, 1988). For example, income taxation requires identifying individual incomes both within the national territory and offshore, assessing value, and collecting payments. The implementation of redistributive policies, such as basic education, healthcare, social assistance, and insurance policies, is also likely to be dependent on the pre-existing capacity of the state institutions (Ziblatt, 2008).

3. Democracy, State Capacity, and Investor Confidence

In contrast to that intuitive account, we present a more nuanced understanding of the relationship between democracy, state capacity, and income inequality. Counterintuitively, we argue that democratic rule in the context of high state capacity is associated with increases in income inequality. It is plausible to think that democracy and high state capacity provide the context for optimal property rights and contract security, which favors investor confidence through lower-risk capital investments. The high-quality investment climate in a democratic, high-capacity setting increases inequality through two policy channels, financial development and larger FDI flows, that affect market income inequality. For several reasons—which we further introduce below—we believe that fiscal redistribution is not able to offset these inequality-concentrating mechanisms.

Under low state capacity, we would expect neither democratic nor authoritarian regime types to make much difference in terms of distributive outcomes, given that the state lacks the ability to undertake both redistributive policies and the provision of contract and property rights security. We also anticipate that in autocratic regimes, the level of state capacity does not matter for inequality. This is because different sub-types of authoritarian regimes are inherently diverse and have very different policy priorities in terms of property rights, contract security, and redistribution (Dorsch and Maarek, 2019). For instance, communist regimes in Eastern Europe and Asia led to extremely egalitarian outcomes over time, while many right-wing dictatorships in Latin America and Sub-Saharan Africa presided over the most unequal distributive outcomes in modern history. Our interactive theory therefore makes only the modest prediction that democratic rule is associated with increasing inequality in the context of high preexisting state capacity.

Democratic regimes have been widely portrayed as more likely to respect private property rights and provide greater rule of law, incentivizing capitalist investor confidence (North and Weingast, 1989; Olson, 2000). An influential argument has connected democracy with higher

FDI inflows precisely because of greater investment security (Busse and Hefeker, 2007; Jensen 2003, 2008). At the same time, democracy alone is not enough to secure investor confidence. Contract enforcement—based on state capacity to enforce the rule of law among private agents—is likely to be crucial for business confidence and attracting foreign investors, along with the protection of private property from arbitrary government involvement. Pre-existing state capacity clearly underlies this positive contractual environment (Besley and Persson, 2009). The “watchman” capacities of the state—Weberian-like central- and local-level bureaucracies, impartial courts, uniform weights and measures, and effective law enforcement institutions—are crucial for reducing uncertainty and transaction costs (Coase, 1960; Williamson, 1985). Therefore, it could be hypothesized that nations combining high state capacity with democratic rule achieve the highest FDI inflows (Li and Resnick, 2003).

In addition to fomenting FDI inflows, democratic high-capacity contexts offer an especially nurturing context for financial development. The checks and balances inherent to a democratic system reduce the government’s leverage in both expropriating assets and threatening property rights in the financial sector (Haber et al., 2008; Menaldo and Yoo, 2015). Yet these positive effects might not be achieved without pre-existing state capacity reducing important market failures that might otherwise result from information asymmetries and obstruct contract enforcement. For example, the creation of accurate property registers by the state allows banks to know who owns which assets, which facilitates the creation of contracts (Haber et al., 2008). The enforcement of modern bankruptcy law and the diffusion of the modern accounting standards underlying credit expansion may depend on the quality of bureaucracy and its ability to penetrate the reaches of the state territory. At the same time, stock market expansion is likely to depend on stronger corporate governance and the capacity to enforce bankruptcy laws (Becerra et al., 2012; Menaldo, 2016).

Thus far, we have argued that democratic and high-capacity state institutions are more likely to attract more FDI and help to develop sophisticated financial sectors. The second step of our argument connects these two variables with increasing income inequality. First, considerable recent evidence has pointed out that FDI flows may increase income inequality in both the developed and developing worlds (Basu and Guariglia, 2007; Jaumotte et al., 2013; Reuveny and Li, 2003). FDI inflows lead to an increased demand for skilled workers, associated with growing wage differentials between skilled and unskilled jobs, which is likely to increase income inequality (Decreuse and Maarek, 2015; Egan and Bogliaccini, 2017; Feenstra and Hanson, 1997; Kratou and Goaied, 2016). For example, investment by multinational corporations often creates a small sector of high wage earners and a large low-wage backward sector (Nafziger, 1997).⁴

⁴ Decreuse and Maarek (2015) show that FDI stock is negatively associated with the labor share in the host countries, though this effect is non-linear.

The development of a sophisticated financial system is another transmission channel by which investor confidence in high-state-capacity democracies produces higher income inequality. On the one hand, scholars have long recognized the growth-promoting and poverty-reducing effects of financial development through incentivizing and channeling savings (Beck et al., 2008). According to this view, financial development is likely to happen in the “extensive margin,” which is likely to be associated with more equal income distribution. On the other hand, financial development could be produced in the “intensive margin” through improvements in the quality and range of financial services available to those who already enjoy access to the financial system, which has an important potential to widen inequality and perpetuate intergenerational differences in economic opportunity (Greenwood and Jovanovic, 1990). Financial instruments, such as bonds and stocks, are likely to provide higher rates of returns to pre-existing capital, providing a basis for the concentration of financial assets (Piketty, 2014).

In addition, the un-equalizing effects of the financial system could work through a labor income channel. Financial sector employees are strongly concentrated at the top of the income distribution, and their earnings exceed those of employees with similar profiles (in terms of age, gender, and education) in other sectors. Asymmetric compensation schemes for bank managers may especially contribute to this un-equalizing dynamic (Denk and Cournède, 2015).⁵ Empirically, recent literature has provided evidence for both positive and negative associations between financial development and inequality. At least six recent papers find a positive association between financial sector size—usually proxied by private credit as a percentage of GDP—and an increase in income inequality, both in cross-national and subnational contexts (Dabla-Norris, et al., 2015; Denk and Cournède, 2015; Haan and Sturm, 2017; Jauch and Watzka, 2016; Jaumotte et al., 2013; Li and Yu, 2014). Other studies find that countries with higher levels of financial development have less income inequality (Hamori and Hashiguchi, 2012; Kunieda et al., 2014; Naceur and Zhang, 2016).

It might be expected that fiscal policy would offset the increase in market inequality in democratic high-capacity settings in the post-redistribution stage. However, while the context of high state capacity in democracies establishes preconditions for progressive taxation or social policy, the redistributive capacity does not automatically translate into policy outcomes. Inequality-increasing market processes also put pressure on fiscal policy, making it difficult to increase redistribution via taxes and transfers (Egan, 2010). With increasing FDI flows and more developed financial sectors, domestic and international corporate and financial elites become more relevant actors in national politics and are likely to exert

⁵ In addition, large financial sectors contribute to moral hazard problems. Given bailout expectations by the government, sophisticated financial instruments encourage the pursuit of high returns through risk-taking behaviors, benefiting members of the financial elite compared to other sectors of the economy (Korinek and Kreamer, 2014).

downward pressures on labor-protecting regulations and redistributive taxation and transfers (Wong, 2016).

A high concentration of income at the top increases potential resources for elite lobbying activities, augmenting their already disproportionate influence on policy making even in countries where considerable redistributive capacity exists (Acemoglu and Robinson, 2006). Starting in the mid-1970s, most industrial nations have experienced considerable reductions in marginal tax rates on income, which has contributed to higher inequality in the disposable income phase (Atkinson, 2015; Bartels, 2008; Gilens and Page, 2014). Egan (2010) shows that, in the Latin American context, accumulated FDI levels are associated with a greater likelihood of market economic reforms, such as lower tax burdens and domestic financial liberalization. Although further work needs to be done in this domain, it is likely that similar patterns of reinforcing elite dominance could be at play in other parts of the developing world, where economic elites enjoy similar political opportunities to concentrate capital.

To summarize, our theoretical propositions have the following empirical implications. Our main hypothesis is that democratic rule in a high-state-capacity context increases both market and post-redistribution inequality over time. We also posit that a democratic, high-capacity context is associated with larger annual FDI inflows and faster growth of the financial sector. For these reasons, we do not expect high-capacity democracies to experience larger fiscal transfers or redistribution, holding all else equal. Lastly, we also expect a positive association between FDI stock, financial development, and income inequality.

4. Research Design, Methods, and Data

We use annual fixed-effects panel regression models to test our propositions. We use unit fixed effects because we are particularly interested in changes within individual countries over time.⁶ Country-fixed effects allow us to account for country-specific omitted factors that are stable over time. The inclusion of a lagged dependent variable controls for autocorrelation. The model takes the following form:

$$\begin{aligned} Inequality_{i,t} = & \alpha_0 + \beta_0 Inequality_{i,t-1} + \beta_1 Democracy_{i,t-1} + \beta_2 State\ Capacity_{i,t-1} \\ & + \beta_3 Democracy * State\ Capacity_{i,t-1} + Controls_{i,t-1} + \gamma_i + \lambda_t + \mu_{i,t} \end{aligned}$$

Our main theoretical interest is the interaction term between the lagged values of democracy and the lagged values of cumulative state capacity; (β_3). γ_i and λ_t are the country- and year-fixed effects, respectively, while $\mu_{i,t}$ is the estimated residuals.

Variables and Measurement

⁶ List of countries is provided in the Appendix in Table A2.

Inequality: Our outcome variable is income inequality as measured by the Gini index. The Gini index ranges from 0 (perfect equality) to 100 (one person has all the income). We use the Standardized World Income Inequality Database (SWIID) (Solt, 2016) for our inequality measure. Using the Luxembourg Income Study (LIS) as the methodological standard for comparability, the SWIID incorporates data from various sources. The SWIID uses “model-based multiple imputation estimates of the many missing observations in the LIS series” (Solt, 2016, p. 1271), maximizing both comparability and sample size. Incomparability is reflected in the standard errors of the SWIID estimates, where the Gini estimates and their associated uncertainty are represented by 100 draws from the posterior distribution. The data set provides 100 imputations for each country-year observation (*ibidem*).⁷ The drawback of the SWIID data is therefore the reliance on estimation to fill in missing data points.

The SWIID is composed of four indicators—disposable income inequality (post-tax and -transfer), market income inequality (pre-tax and -transfer), absolute redistribution (the difference between the market income and disposable income Gini indexes), and relative redistribution (the percentage by which market income inequality is reduced). We expect a democratic high-capacity context to affect both market and disposable (net) income inequality. While we anticipate the inequality-increasing processes to work mostly through market income concentration, they also put a strain on fiscal redistribution, as we have argued above. Therefore, we present results with both net and market income inequality in our empirical analysis.

Democracy: We adopt the Boix et al. (2013) and Polity indicators as our main democracy measures. Boix et al.’s (2013) measure is based upon two principal components: 1) the use of elections to choose the legislature and, directly or indirectly, the chief executive, and 2) a minimum threshold of participation rights. The Polity democracy index consists of six component measures that record key qualities of executive recruitment, constraints on executive authority, and political competition (Marshall et al., 2017). It provides an ordinal ranking of political regimes on a scale of 10 to -10 (democracy to authoritarian regimes). Both of these measures offer almost universal country coverage over time. We further test the robustness of our results with the democracy indicators of Cheibub et al. (2010) and Dorsch and Maarek (2019) (based on the democracy measure initially developed by Papaioannou and Siourounis [2008] and Acemoglu et al. [2019]).

⁷ We make use of multiple imputation (MI) regression tools provided by Stata, as recommended by Solt (2016). We perform our main regressions over each of the 100 imputations in order to provide a reliable estimate of the coefficients, taking into account the standard errors across the 100 imputations. This allows the uncertainty of the SWIID to be reflected in MI regression estimates. Given that the MI estimation is computationally intensive, some MI regression tools are not available (e.g., 2LS2); we therefore chose to present the majority of our models with non-imputed estimates, calculating the mean of imputed series for each country-year, and performed the regressions on that single point estimate.

Given that inequality is likely to affect the prospects of democratic consolidation in different nations, issues of endogeneity must be discussed. Indeed, the level of inequality has figured as a crucial explanatory variable in previous studies of democratization (Acemoglu and Robinson, 2006; Ansell and Samuels, 2014; Boix, 2003). To mitigate reverse causality concerns, we make use of an instrumental variable strategy. Relying on previous work (Acemoglu et al., 2019; Dorsch and Maarek, 2019), we use regional waves of democratization as a source of exogenous variation in domestic democracy (Dorsch and Maarek, 2019). It is very unlikely that within-country inequality or other domestic economic and political variables could have an influence on the timing of regional democratization processes, while democratization waves clearly affect domestic democratization (Acemoglu et al., 2019; Huntington, 1991). It is implausible that democratic or autocratic waves have a direct effect on inequality in a particular country except through their effect on domestic political institutions. This instrument allows us to plausibly isolate an exogenous variation in democratic institutions.

We construct our instrument through the following strategy. For our binary indicator of democracy (Boix et al., 2013), we calculate the fraction of countries with democratic institutions in the region that shared the same regime type at the beginning of the panel. For instance, for country i , we add up the number of countries sharing regime type in the same region that are democratic at the time, excluding country i . For our continuous Polity indicator, we calculate the average democracy score in a region to instrument Polity scores in a given year, excluding the country itself.⁸

A possible violation of the exclusion restriction is that democratic transitions in neighboring countries affect domestic economic growth rates, which could affect economic variables domestically—especially if regional economies are integrated—which in turn affects both inequality and the likelihood of domestic democratic transition (Acemoglu et al., 2019). To mitigate these concerns, we control for log of GDP per capita in all models.

State Capacity: Operationalizing state capacity in the context of our analysis is a complicated task. Similar to regime type, state capacity tends to be endogenous to inequality levels and other socio-economic variables associated with economic development. In addition, democratization might affect state capacity by creating incentives to gather more tax revenue and provide more public goods and services to citizens (Acemoglu et al., 2011). Most existing measures used in the literature—based on fiscal capacity or levels of public goods provision—reflect the policy preferences of governments and are likely to be directly endogenous to regime type and inequality levels (Bockstette, 2002). In addition, as we explain below, expert survey-based indicators, such as the Bureaucratic Quality Index of the International Country Risk Guide (ICRG) or the World Bank (WB) Worldwide Governance

⁸ Following the definition of Dorsch and Maarek (2019), we define the regions as follows: Africa, Central Asia, Eastern Europe, Europe/U.S., Middle East, South-East Asia, South/Central America.

Indicators, are likely to be affected by expert biases of different types (Kurtz and Schrank, 2012).

We make use of a novel measure of state capacity that is less vulnerable to these problems: the regular ability to conduct national population censuses (Hanson, 2015; Soifer, 2013). The capacity to undertake periodic censuses captures the ability of the central state to gather information about its subjects, proxying well for functioning central and local bureaucracies and effective law enforcement institutions (Mann, 1984). In addition, censuses also provide the state the necessary information for the construction of tax registers, cadastral maps, and other forms of systematization (Soifer, 2013). Census administration therefore captures the capacity to collect and manage information and effectively execute policies in different areas, including market regulation, property rights protection, and contract enforcement (“legal capacity”) and the ability to extract resources (“fiscal capacity”) across national territory (Besley and Persson, 2009; Knutsen, 2013).

Even if nations have incentives to manipulate the timing, reach, and coverage of the censuses—concerns which we discuss below—there are no major political incentives to avoid them altogether. They are infrequent in time—conducted usually every five or 10 years—and take up relatively few resources compared to the implementation of welfare policies or infrastructure programs. However, they serve multiple purposes for both democratic and autocratic nations of different development levels (Christopher, 2008). Censuses not only provide information to identify subjects for taxation, military conscription, and government programs but have also figured as crucial nation-building devices for nations in the developing world while contributing to social control and surveillance for authoritarian regimes (Anderson, 1991; Lieberman and Singh, 2017; Taylor, 2019).⁹ It is therefore plausible to believe that the absence of a census acts as a direct signal of extreme state weakness, while its presence indicates meeting a minimal threshold of state organization. Where states cannot conduct censuses regularly, they are surely unable to undertake property rights and contract enforcement, even if they have political incentives to do so (Centeno, 2002).

We use data from the United Nations Social and Housing Statistics Section database on national population censuses,¹⁰ which documents information on the presence or absence of a standard national census for every country-year during the period 1945–2015, which covers

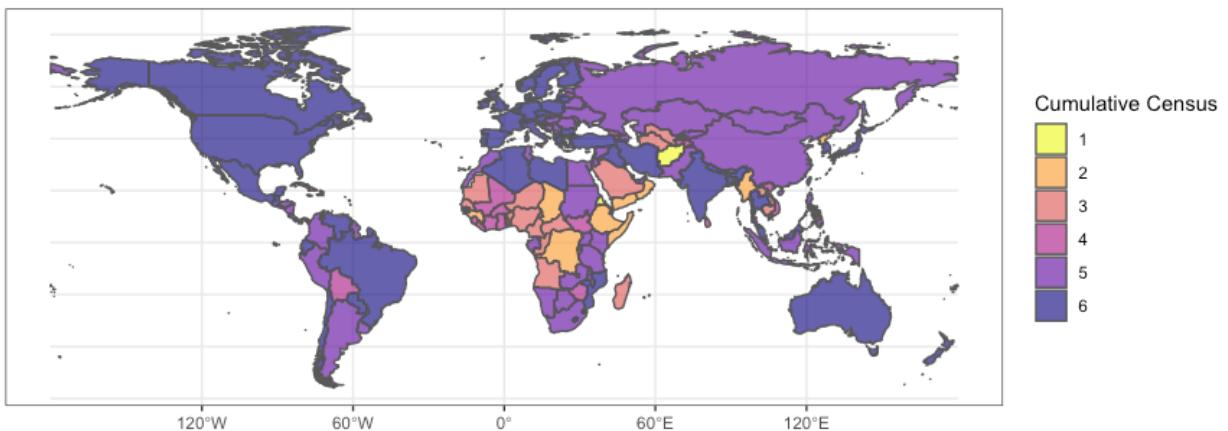
⁹ For instance, colonial independence movements, initially concerned with the censuses’ surveillance role, coopted them as a means of promoting national identity through the definition of a national population, akin to the definition of a national territory (Anderson, 1991).

¹⁰ The United Nations Social and Housing Statistics Section database excludes all censuses defined as “urban,” “administrative,” or “sample,” as well as all those described only as “scheduled,” since these censuses do not provide the government with systematic information about its entire population. This leaves two types of censuses in the sample: the standard census, as carried out in most countries, and the rolling census, carried out on an annual basis for a portion of the population in a small set of countries, including Iceland, Sweden, and Denmark (Hanson, 2015; Soifer, 2013).

13,466 country-years, as compiled by Hanson (2015).¹¹ An intuitive approach would be to create a lagged indicator measuring whether nations conducted a census in the past five or 10 years (Hanson, 2015; Soifer, 2013). Yet this measure is more likely to be endogenous to contemporary socio-economic situation and regime type, and the occurrence and timing of censuses could be manipulated by governments according to different policy priorities. For instance, it is possible that governments might determine the timing of censuses according to their electoral calendar to influence the boundaries of electoral districts (e.g., to exclude or include some particular ethnic and regional groups) or to show favorable population sizes in order to achieve more development aid from donors (Lieberman and Singh, 2017).

To mitigate these concerns, we construct a simple continuous indicator that counts the cumulative number of decades in which countries have conducted periodic censuses since 1950 for every country-year. The national censuses are conducted at either 10- or five-year intervals, and the absence of a census in a decade is likely to signal considerable weakness of the central government due to a lack of control over sub-national areas, absent bureaucracies, and inability to control national territory to its full extent. For instance, if a government were unable to conduct censuses in the 1950s and 1960s but able to do so in the 1970s and 1980s, the country receives a score of 2 for the whole decade of 1980–1990. The indicator has a global mean of 3.40 and standard deviation of 1.36. Figure 1 displays the cumulative census scores in 2010. In 2010, industrial countries have unanimously maximum values (6) on this indicator, while Somalia, Eritrea, Chad, Yemen, and Afghanistan possess the lowest values with only 1–2 census iterations.

Figure 1. Cumulative Census Variable in 2010



¹¹ The data for 1990–2020 is available in the United Nations Social and Housing Statistics Section <https://unstats.un.org/unsd/demographic-social/census/censusdates/>. The data for the period 1950–1990 is available at the United Nations Social and Housing Statistics Section. 2003. Ethnicity: A Review of Data Collection and Dissemination. Demographic and Social Statistics Branch, United Nations Statistics Division. At <http://unstats.un.org/unsd/demographic/sconcerns/popchar/Ethnicitypaper.pdf>.

This procedure—while rather blunt—creates an indicator that is largely unaffected by both expert coding bias and the policy priorities of governments, relieving inherent endogeneity bias (Soifer, 2013). This long-term measure—which captures the effect of censuses conducted in previous decades—is likely to “wash out” all temporary shocks resulting from the timing of elections, foreign aid priorities, or other time-variant policy agendas. These political incentives might explain why censuses are conducted in one particular year versus another but are unlikely to affect whether the census was conducted over a long time frame such as several decades. It is important to note that our measure captures all censuses conducted in the national territory since 1950 by any state—even those that were conducted under colonial administrations and nations that formed part of other countries. In this way, our measure accounts for pre-statehood state capacity. We demonstrate this point in the Appendix in Table A3., which lists all censuses in our sample that were undertaken under colonial administrations and parts of other countries.¹²

However, several potential criticisms of our measure merit discussion. First, our census indicator does not allow us to capture more subtle differences between countries that do not miss censuses, nor does it take into account more gradual increases or declines in state capacity occurring yearly.¹³ Despite these potential shortcomings, we argue that our indicator is considerably less susceptible to measurement error than the existing measurements based on expert evaluations, such as the Bureaucratic Quality Index (BQI) from the ICRG dataset and the WB World Governance Indicators, which are able to capture more subtle country-differences within regions.¹⁴ The “expert scores” are indeed likely to meaningfully reflect differences between countries in the same region (say between Sweden, Italy, and Albania in Europe).¹⁵

Yet these evaluations start to face enormous measurement validity issues—stemming from expert biases possibly inducing considerable measurement error—when comparing the aforementioned nations with countries outside of their political and cultural regions. Given

¹² Table A3. in the Appendix shows that states that emerged from other states with high capacity (had regular census administration) also had high capacity post-independence (i.e., had achieved a high cumulative census score by 2010), given that they had accumulated a high census score. As demonstrated by this table, the Soviet Union, Yugoslavia, and Czechoslovakia gave their high-capacity levels to successor states, given that all these censuses are captured by our measure. The table also shows that censuses conducted under colonial empires (British, French, and Portuguese) in other parts of the developing world are accounted for by our census measure as well.

¹³ We thank one of the anonymous reviewers for this comment.

¹⁴ For instance, our indicator does not allow us to capture any meaningful variation between industrial countries in Western Europe and North America, given that they have not missed censuses in any decade since the 1950s.

¹⁵ For example, Kurtz and Schrank (2012: 542) explain that measurements that rely on surveys, particularly of foreign investors or domestic firms, wrongly assume that “the interests of investors [...] and the interest of the state institutions are essentially coterminous.” In some instances where the state is strong and able to levy taxes and impose regulations, for example, the state will most likely “be judged ‘burdensome’ and ‘growth-inhibiting’ by many businesspersons” (Kurtz and Schrank, 2012: 542).

that what “high-quality bureaucracy” means in different countries is regionally and culturally specific, we cannot expect that quantitative gradations in expert scores for various dimensions meaningfully reflect differences in state strength (Kurtz and Schrank, 2012). A great advantage of our measure—besides its wide availability—is the fact that census occurrence is based on “hard” institutional data, which is not vulnerable to coding biases of the type that stem from expert evaluations (Knutsen, 2013; Kurtz and Schrank, 2012). Our simple measure is largely free of these measurement problems, as national censuses always include enumeration of the whole population (as defined by the UN), although their quality might vary considerably. The absence of a census gives a powerful indication of the weakness of state institutions in the developing world, despite the measure’s bluntness.

Second, another potential disadvantage of our measure, which sums up censuses over decades cumulatively, is that it cannot decline over time. To mitigate that concern, we have devised another cumulative census variable that introduces a penalty to the cumulative census score when countries miss the census in a decade. It is very plausible that when countries miss a census, state capacity is likely to decline. We believe that missing a census is likely to be a sign of inherent state fragility, which leads to a decline in state capacity. To demonstrate that our penalized measure is robust to different sizes of penalty, we create two versions of that variable. For every version, we subtract either 1 or 0.5 points from the accumulated state capacity variable when countries miss a census in a decade and find our results identical to the simple cumulative variable (Table 4).

Third, while all nations could potentially have a minimum of zero and a maximum of six censuses, colonial legacies could influence census administration and state capacity, confounding our results. Scholars have argued that former British colonies inherited higher institutional capacity and human capital stock at the time of independence compared to nations under French, Portuguese, and Belgian colonial rule (Cogneau and Moradi, 2014; La Porta et al., 1998; Landes, 1998).¹⁶ While former French, Belgian, and Portuguese colonies tend to have lower cumulative census scores, we show that these varying colonial legacies do not drive our results in any meaningful way. We capture the effects of different colonial legacies on different countries through sample restrictions and present evidence that our results are not driven by these trends in Table A6. in the Appendix.¹⁷

¹⁶ For instance, the British established a system of indirect rule—enabling local autonomy and self-governance—while the French relied on direct colonial rule accompanied by repression (Landes, 1998). Others have contrasted liberal British policies regarding missionary schooling to restrictive systems of state education in French dependencies, which led to higher human capital outcomes in British colonies (Cogneau and Moradi, 2014).

¹⁷ Table A.3 I shows that British colonies (with a cumulative census score of 4.5 in 2010) seemed to conduct more censuses than the French (3.38 in 2010), Belgian (3 in 2010) and Portuguese (4 in 2010) ones in the 1950s, and also have a higher cumulative census score in 2010. Yet, as we show in Table A6, varying colonial legacies do not confound our results.

Lastly, it might be that civil wars confound the relationship between cumulative census administration, democracy, and inequality. Civil wars are indeed the principal reason why nations are not able to take on censuses. Yet, while having a dreadful short-term effect on state capacity, not all civil wars lead to a deterioration of state capacity in the long term, which our measure intends to capture. As suggested by Pagalayan (2020), civil wars might incentivize central states to cater more public goods to sub-national regions that had been neglected by central governments before the conflict. In some cases, states have indeed started to conduct censuses rather quickly after civil wars with the aim of gathering information on citizens to provide better public services to them (Verpoorten, 2012). This discussion prescribes that we should not expect a clear relationship between civil wars and census administration proxying for state capacity. We account for these concerns by introducing control variables for civil conflict and ethnic fractionalization, variables commonly connected to domestic conflicts.

Our cumulative census measure is correlated to a reasonable degree with other proxies of state capacity. It has a .55 correlation with GDP per capita, a .33 correlation with tax revenue (as a percentage of GDP), and a .45 correlation with school enrollment. This suggests that the cumulative census variable is a reasonable proxy for “fiscal” state capacity. Our indicator has a slightly weaker association with “legal capacity”—property rights protection and contract enforcement. Our measure has a .39 correlation with Fraser Institute’s Economic Freedom Index, and .35 and .40 correlations with Property Rights Protection and Enforcement of Legal Contracts indicators in the same data base, respectively. Our indicator is not correlated with the Gini index (.03), which relieves the concern that censuses might be especially likely to be absent in low- or high-inequality nations.

Control Variables: Besides country- and year-fixed effects, we add a series of control variables to account for alternative factors that might be associated with inequality changes (in our baseline models). We add the log of GDP per capita to control for the level of economic development. We include trade openness as an indicator of economic openness, measured as imports and exports as a percent of GDP (Reuveny and Li, 2003). Inflation captures the macroeconomic situation of the country. Finally, we include the urban share of the population to account for the structure of the economy. Our control variables come from the World Development Indicators (WDI) of the World Bank. Descriptive statistics for these control variables are presented in the Appendix (Table A1.) We discuss the measurement of FDI, financial development, and other control variables in Section 6.

5. Results

We start the presentation of our results with models without the interaction term (Table 1). Our results directly replicate previous studies of inequality (Dorsch and Maarek, 2019;

Gradstein and Milanovic, 2004; Timmons, 2010; Wong, 2016). Models 1 and 2—using the MI approach, as suggested by Solt (2016)—show a lack of association between the Boix (Model 1) and Polity (Model 2) indicators of democracy and net inequality, respectively, while controlling for covariates typically used in the literature and considering country- and year-fixed effects.

In order to capture the conditional effect—and following Dorsch and Maarek (2019)—we first add an interaction between our binary Boix democracy indicator and the cumulative census score prior to democratization, therefore using a fixed state capacity variable for these interaction terms (Models 3 and 5).¹⁸ Model 3 presents results with MI estimation, Model 5 with imputed series. The results from columns 3 and 5 directly support our counterintuitive theoretical contentions: democratization and state capacity interact positively in producing higher inequality levels.¹⁹ Very similar coefficients are produced using the means of the imputed series.²⁰

These effects are substantively meaningful. The dichotomous democracy indicator allows us to calculate the long-run effect of democratic transitions under different levels of state capacity. The shift from democratic to authoritarian under the highest value of state capacity (six censuses) results in an increase in future inequality of 3.7 Gini points (Model 3), holding all other variables constant at their means. We follow the advice of Berry, Golder, and Milton (2012) and present the conditional effect of democracy at different levels of state capacity graphically, using the results from Model 5 (Table 1). Figure 2 shows that democracy has a positive effect on income inequality when state capacity is high at the moment of democratization (approximately two census iterations). To get a better sense of these results, Table 1 also provides the marginal impacts of the interaction term.

We obtain a similar result using the Polity continuous indicator as our democracy indicator (Models 4 and 6). While the continuous indicator does not allow us to calculate long-run effects, it allows us to evaluate the marginal impact of more gradual shifts in regime type, conditional on lagged levels of cumulative censuses (lagged in one period). Under the maximum level of state capacity (six censuses), the shift from full authoritarianism to full

¹⁸ The advantage of this approach is that it allows us to calculate the long-run effects of shifts from autocracy to democracy. Following Dorsch and Maarek (2019, eq. 4) and Brambor, Clark, and Golder (2006), we define the marginal effect of democracy when we include the interaction term as $\partial\text{Gini}\partial\text{Democracy} = 1 + 3*\text{State Capacity}$, for which the long-run effect is given by $1 + 3*\text{State Capacity}(\text{Max, Med}) 1 - 0$.

¹⁹ For Models 5 and 6, the stationarity of residuals were tested using conventional panel unit root tests. Thus, Fisher's tests with both the Augmented Dickey Fuller and the Phillips-Perron specifications were implemented. We report these tests in Table A4 in the Appendix. All variables in the main models were tested (models 5 and 6 in Table 1) i.e., dependent independent ones. All tests also included parameters to test for trend and drift. All tests included one lag, which is the same lag structure used in the estimation procedures. These tests were implemented using the “*xtfisher*” routine in Stata (v. 15). As the table suggests, most tests indicate stationary residuals at conventional levels of statistical significance.

²⁰ We prefer the median imputed series specification, as it is not as computationally intensive as MI models, while results are identical.

democracy (from -9 [10th percentile] to 10 [90th percentile] in Polity scores) would result in a 4.5-Gini-point increase in inequality in 10 years, holding all other variables constant at their means. The marginal effects of Polity are presented in Figure 3.

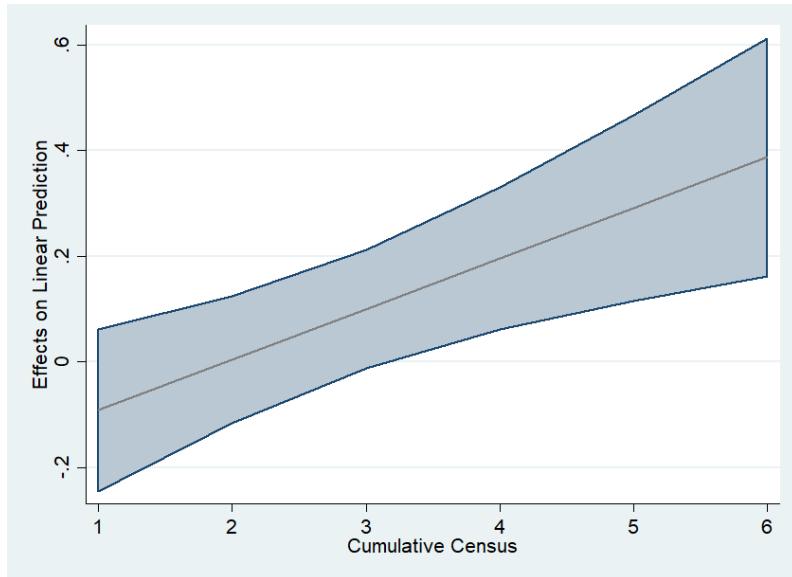
Table 1. Effect of Democracy and State Capacity on the Net Gini Index

	Multiple Imputation			Mean Imputed series		
	(1) Boix	(2) Polity	(3) Boix	(4) Polity	(5) Boix	(6) Polity
Gini Lagged	0.919*** (0.008)	0.920*** (0.008)	0.917*** (0.008)	0.921*** (0.008)	0.937*** (0.006)	0.941*** (0.006)
Boix Democracy	-0.065 (0.065)		-0.337*** (0.124)		-0.187* (0.102)	
Polity Democracy		-0.004 (0.006)		-0.021** (0.009)		-0.022*** (0.007)
Cumulative Census			-0.036 (0.048)	0.004 (0.086)	0.088** (0.040)	-0.043 (0.064)
Boix Democracy*Cumulative Census			0.107*** (0.037)		0.096*** (0.031)	
Polity*Cumulative Census				0.007** (0.003)		0.008*** (0.002)
GDP (log)	0.279** (0.134)	0.278** (0.137)	0.246* (0.129)	0.259* (0.133)	0.382*** (0.085)	0.426*** (0.083)
Inflation	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000* (0.000)	0.000* (0.000)
Urban Population	-0.013 (0.008)	-0.013 (0.008)	-0.008 (0.007)	-0.010 (0.008)	0.002 (0.005)	-0.001 (0.005)
Trade	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	0.001 (0.001)	0.000 (0.001)
LR effect at Census=6			3.67		6.17	
LR effect at Census=3			-0.19		1.6	
Marg. impact at Census=6			0.305	0.021	0.389	0.026
Marg. impact at Census=3			-0.016	0.000	0.101	0.002
Country and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,190	4,006	4,019	3,962	3,778	3,962
R-squared	0.99	0.99	0.99	0.99	0.99	0.99

Notes: The dependent variable is the net Gini coefficient and the main explanatory variables are one-period-lagged democratic scores from Boix et al. (2013) and Polity IV by Marshall et al. (2016) interacted with the

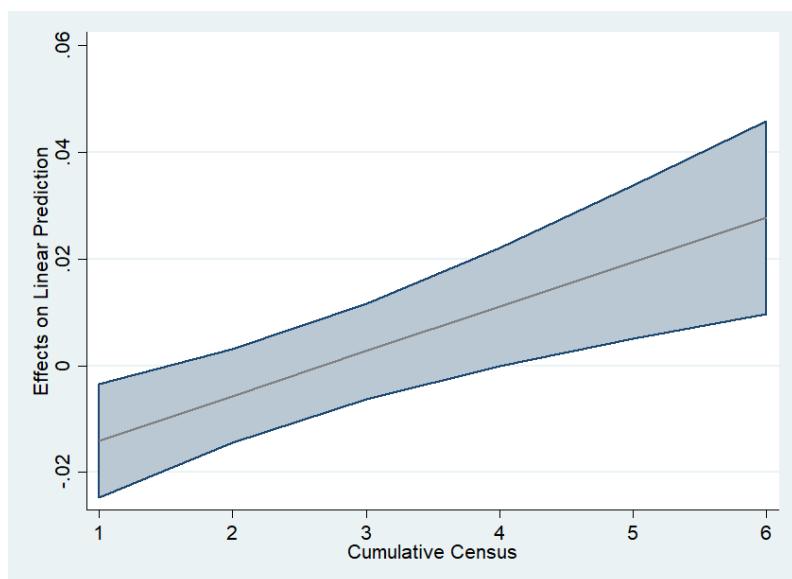
cumulative census variable. The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. All independent variables are lagged one year. Standard errors are clustered at the country level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 2. Marginal Effect of Boix Democracy Indicator on Gini Index for Different Pre-Democracy Cumulative Census Values



Note: Conditional Effect of Democracy and State Capacity on Inequality. The panel shows the predicted change in the Gini index from democratic transitions using Boix et al.'s (2013) democracy index at different pre-democracy cumulative census scores according to the estimates in Table 1, Model 5. The blue lines represent the 95% confidence intervals.

Figure 3. Marginal Effect of Polity Democracy Indicator on Gini Index for Different Lagged Cumulative Census Values



Note: Conditional Effect of Democracy and State Capacity on Inequality. The panel shows the predicted change in the Gini index from a one-unit change in democracy (Polity) at different lagged cumulative census values according to the estimates in Table 1, Model 6. The blue lines represent the 95% confidence intervals.

2SLS and GMM Estimations

Table 2 (Models 1–4) presents results from 2SLS instrumental variable regressions for both of our democracy indicators (Boix and Polity) with the means of imputed series. We consider both democracy and its interaction term with cumulative census values as endogenous and instrument for them with regional democracy share/scores and an interaction of the latter with the cumulative census indicator. We perform these analyses for our two democracy variables. We present the first stage's results in the Appendix (Table A5), where we demonstrate a positive association between our regional instruments, our democracy indicators, and second-stage results, with required statistics, in Table 2. In order to have an over-identified specification, as a third excluded instrument, we also use the regional wave measure from five years before our one-year lagged democratization/democracy score regressor (the sixth lag of the share of a country's region that is democratically governed, in the case of the Boix indicator).

In addition, we present F-statistics of excluded instruments for the first-stage regressions in Table 1. Cragg–Donald F-statistics give evidence that the set of instruments is strong (above the rule of thumb of 10). The large p-values in the Hansen J statistics also confirm that the excluded instruments are exogenous. The results from the 2SLS procedure are similar to those in Table 1, with coefficients of interaction term larger in size. The shift from democratic to authoritarian rule under the highest value of state capacity results in an 8-Gini-point increase in inequality in the long run, holding all other control variables constant at their means (Model 1).

Table 2. Effect of Democracy and State Capacity on the Net Gini Index—2SLS and GMM

	2SLS			GMM	
	(1) Boix	(2) Polity	(3) Boix	(4) Polity	(5) Boix
Lagged Gini	0.937*** (0.006)	0.944*** (0.006)	0.944*** (0.006)	0.941*** (0.009)	0.995*** (0.001)
Boix Democracy	-0.117 (0.245)		-0.096 (0.243)		-0.216** (0.087)
Boix*Cumulative Census	0.108** (0.053)		0.085* (0.055)		0.039* (0.024)
Polity Democracy		-0.055*** (0.015)		-0.062** (0.031)	-0.020*** (0.007)
Polity*Cumulative Census		0.007* (0.004)		0.011** (0.003)	0.005** (0.002)
Cumulative census	0.097* (0.056)	-0.082 (0.064)	0.133** (0.057)	0.012 (0.018)	0.001 (0.017)
					0.012 (0.018)

GDP (log)	0.392***	0.034	0.298**	0.017	0.201**	0.032*
	(0.088)	(0.045)	(0.093)	(0.021)	(0.179)	(0.017)
Inflation	0.000*	0.000	0.000	0.000	0.000***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Urban Population	0.001	0.006	0.002	0.006	-0.001*	-0.002**
	(0.006)	(0.006)	(0.006)	(0.009)	(0.001)	(0.001)
Trade	0.001	0.002	0.002*	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)		
Ethnic Diversity			8.099	1.613		
			(6.531)	(2.836)		
Civil Conflict			-0.091	-0.117*		
			(0.060)	(0.068)		
ODA (log)			-0.006	-0.007*		
			(0.004)	(0.004)		
Left			0.008	0.099*		
			(0.041)	(0.057)		
Growth			-0.604*	-0.069		
			(0.350)	(0.438)		
Fraser Index			0.013	0.015		
			(0.013)	(0.014)		
LR effect at Census=6	8.43		7.39		3.6	
LR effect at Census=3	3.29		2.84		-19.8	
Marg. impact at Census=6	0.531		0.414		0.018	
Mar. impact at Census=3	0.207		0.159		-0.099	
C-D F stat on excl. IVs	84.26	58.56	73.14	56.78		
Hansen J-stat p-value	0.11	0.14	0.12	0.26	0.38	0.9
Excluded Instruments	3	3	3	3		
Number of Instruments					98	124
AR(1)					0.000	0.000
AR(2)					0.888	0.640
Observations	4,034	4,020	3,999	3,894	3,160	3,042
R-squared	0.741	0.742	0.837	0.769	0.769	0.818

Notes: The dependent variable is the net Gini coefficient and the main explanatory variables are one-period-lagged democratic scores from Boix et al. (2013) and Polity IV by Marshall et al. (2016) interacted with the cumulative census variable. The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. All independent variables are lagged one year. Standard errors are clustered at the country level. *** p<0.01, ** p<0.05, * p<0.1.

In Models 3 and 4, we further probe the validity of our instrumental variable strategy by including various control variables that might confound the conditional effect of democracy and state capacity on inequality. First, given that civil conflicts are likely to affect both census administration and inequality, we control for lagged events of civil conflict by including an indicator from the Uppsala Conflict Data Program and the Peace Research Institute (UCDP/PRI) Armed Conflict Dataset (Gleditsch et al., 2002). Second, left governments are more likely to engage in redistributive policies and investments in state capacity. We grasp this effect through the left partisanship indicator from the Database of Political Institutions (Beck et al., 2001). Third, we also control for Official Development Aid (ODA) as a percentage of GDP to account for its possible effects on census administration. Fourth, ethnic diversity could hinder redistributive policies (Pfetzing and Sturm, 2020), which we account

for by controlling for the Herfindahl index from the Ethnic Power Relations (EPR) Core Dataset (Cederman, Wimmer, and Min, 2010). Lastly, we control for the Fraser Institute Index of Economic Freedom, a summary index constructed from five components (size of government, legal system and property rights, sound money, freedom to trade internationally, and regulation) (Krieger and Meierrieks, 2016). In both models, our results remain unaltered, for both the Boix et al. (2013) and Polity measures.

To further bolster the robustness of our conclusions, we also provide results using the system method-of-moments estimator (GMM) introduced by Arellano and Bond (1991) and Arellano and Bover (1995) (Models 5 and 6). The GMM system uses lagged explanatory variables in levels and differences as instruments. AR(1) and AR(2) in Table 2 report the p-values for first- and second-order autocorrelated disturbances in the first differences equations, where the null denotes no correlation. The first-order serial correlation AR(1) is expected since we are including lags as instruments. However, a correlation at higher orders than 1 would lead to an inconsistent estimator. Hence, the null should not be rejected for AR(2). Our results are robust to GMM estimation.

Robustness Tests

In Table 3, we present further robustness tests considering some intuitive sample restrictions. In Models 1 and 2, we replicate our results with 5-year panels. The effect of political variables on distributive outcomes is usually slow-moving, and democracy might take time to produce results, so longer panel lengths may capture more substantive variation in the variables between each observation (Dorsch and Maarek, 2019). We take the variables' values in the first year of each five-year time period, starting from 1970 (independent variables are lagged by one panel period). We demonstrate that identical results to annual panels are obtained using 5-year panels, interacting Boix (Model 1) and Polity IV (Model 2) variables with our cumulative census indicator.²¹ A shift from autocracy to democracy under the highest cumulative census value (6) results in a 7-Gini-point increase in inequality in the long term.

In Models 3 and 4, we replicate our main results, excluding industrial countries from the sample. We replicate our results with both democracy measures. Therefore, we are certain that our results are not driven by an increasing inequality trend in the industrial world since the 1970s but can be generalized more widely to other regions (Atkinson, 2015; Piketty, 2014). In Models 3 and 4, we find an identical effect when excluding former Warsaw Pact nations, where inequality increased after democratization in a relatively high-capacity context. Both long-run and marginal effects look similar to those of previous models. *Further robustness.* The colonial origins of the countries could also affect our results. In Table A6.,

²¹ Lagged variables are thus lagged by one panel period.

we exclude French and British developing world colonies from our sample, and replicate our results. This gives evidence that different colonial legacies affecting state capacity do not affect our results.

Table 3. Effect of Democracy and State Capacity on the Net Gini Index: Robustness

	Mean Imputed series					
	5-year Panels		OECD Excluded		Warsaw Pact Excluded	
	(1) Boix	(2) Polity	(3) Boix	(4) Polity	(5) Boix	(6) Polity
Gini Lagged	0.714*** (0.032)	0.689*** (0.034)	0.942*** (0.007)	0.935*** (0.008)	0.937*** (0.006)	0.936*** (0.007)
Boix Democracy	-1.177** (0.555)		-0.112 (0.135)		-0.175* (0.105)	
Polity Democracy		-0.082** (0.041)		-0.024*** (0.008)		-0.003 (0.010)
Cumulative Census	0.024 (0.237)	0.448 (0.352)	0.153*** (0.049)	-0.068 (0.077)	0.092** (0.042)	-0.081 (0.075)
Boix *Cumulative Census	0.515*** (0.170)		0.080** (0.040)		0.093*** (0.032)	
Polity*Cumulative Census		0.041*** (0.013)		0.003* (0.002)		0.009*** (0.003)
Inflation	0.000 (0.000)	0.001 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000** (0.000)
Urban Population	-0.006 (0.028)	0.001 (0.029)	0.011* (0.007)	0.007 (0.007)	0.001 (0.005)	-0.003 (0.006)
Trade	0.013** (0.005)	0.002 (0.005)	0.002** (0.001)	0.000 (0.001)	0.001 (0.001)	0.000 (0.001)
GDP (log)	0.501*** (0.101)	1.692*** (0.481)	0.486*** (0.103)	0.540*** (0.101)	0.383*** (0.089)	0.460*** (0.103)
LR effect at Census=6	6.69		6.34		6.08	
LR effect at Census=3	1.29		2.21		1.65	
Marg. impact at Census=6	1.913	0.450	0.368	0.01	0.383	0.05
Marg. impact at Census=3	0.368	0.198	0.128	-0.02	0.104	0.02
Observations	692	665	3,170	2,987	3,782	3,469
R-squared	0.967	0.969	0.984	0.985	0.990	0.990

Notes: The dependent variable is the net Gini coefficient and the main explanatory variables are lagged democratic scores from Boix et al. (2013) and Polity IV by Marshall et al. (2016) interacted with the cumulative census variable. Models 1 and 2 consider 5-year panels, while for Models 3–6, the unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. Standard errors are clustered at the country level. *** p<0.01, ** p<0.05, * p<0.1.

We also test the robustness of our results using a variable which introduces a penalty to the cumulative census score when countries miss the census in a decade in Table 4. In Models 1 and 2 we subtract 1 and Models 3 and 4 0.5 points from the accumulated state capacity variable when countries miss a census, interacting these variables with our Boix et al. and Polity democracy indicators. These interaction terms remain significant and the substantive effects are similar to our baseline models in Table 1. In addition, to probe the robustness of our results to other democracy indicators, we show identical results to Table 1 with the democracy variables developed by Cheibub et al. (2010) and Dorsch and Maarek (2019) (based on Papaioannou and Siourounis [2008] and Acemoglu et al. [2015, 2019]; see Appendix [Table A7]).

Table 4. Effect of Democracy and State Capacity (Penalized Variables) on the Net Gini Index.

	Mean Imputed series			
	(1) Boix Penalized 1	(2) Polity Penalized 1	(3) Boix Penalized 0.5	(4) Polity Penalized 0.5
Lagged Gini	0.937*** (0.006)	0.941*** (0.006)	0.938*** (0.006)	0.940*** (0.006)
Boix Democracy	-0.209** (0.092)		-0.184* (0.105)	
Polity Democracy		-0.023*** (0.007)		-0.024*** (0.008)
Cumulative Census	-0.011 (0.037)	-0.100*** (0.038)	0.048 (0.041)	-0.026 (0.046)
Boix Democracy*Cumulative Census	0.069*** (0.025)		0.060** (0.027)	
Polity*Cumulative Census		0.007*** (0.002)		0.007*** (0.002)
GDP (log)	0.441*** (0.084)	0.429*** (0.082)	0.443*** (0.084)	0.435*** (0.082)
Inflation	0.000* (0.000)	0.000** (0.000)	0.000* (0.000)	0.000** (0.000)
Urban Population	-0.003 (0.005)	-0.001 (0.005)	-0.003 (0.005)	-0.002 (0.005)
Trade	0.001 (0.001)	0.000 (0.001)	0.001 (0.001)	0.000 (0.001)
Constant	-1.846** (0.901)	-1.870** (0.878)	-1.930** (0.900)	-1.958** (0.879)
Long-run effect at Census=6	3.25	0.32	2.84	0.3
Long-run effect at Census=3	-0.03	-0.03	-0.06	-0.05
Marginal impact at Census=6	0.205	0.019	0.176	0.018
Marginal impact at Census=3	-0.002	-0.002	-0.004	-0.003
Observations	4,034	3,977	4,034	3,977

R-squared	0.990	0.991	0.991	0.991
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Notes: The dependent variable is the net Gini coefficient and the main explanatory variables are one-period-lagged democratic scores from Boix et al. (2013) and Polity IV by Marshall et al. (2016) interacted with the penalized cumulative census variable. The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. All independent variables are lagged one year. Standard errors are clustered at the country level. *** p<0.01, ** p<0.05, * p<0.1.

Lastly, we also explore if the inequality-increasing effect of democracy under high capacity works primarily through market (gross) inequality, as we have hypothesized in Section 3, rather than redistribution. In Models 1 and 2 (Table 5), we document a positive interactive effect of Boix et al. (2013) and Polity variables and the cumulative census variable on market inequality. In Models 3 and 4, we find no interactive effect of democracy variables and state capacity on fiscal redistribution (measured as an absolute difference between market and net inequality). This provides evidence that the impact of high-capacity democracy on the net Gini mostly occurs through changes in market income distribution rather than redistribution. Fiscal redistribution is not greater in high-capacity democracies, showing that it is not likely to offset inequality-increasing changes occurring in the market phase. Next, we turn to a test of concrete policy channels through which high-capacity democracies promote higher inequality.

Table 5. Effect of Democracy and State Capacity on the Market Gini Index and Redistribution

	Mean Imputed series			
	(1) Boix	(2) Polity	(3) Boix	(4) Polity
	DV: Market Gini	DV: Market Gini	DV: Redistribution	DV: Redistribution
Gini Lagged	0.985*** (0.003)	0.986*** (0.003)		
Redistribution Lagged			0.867*** (0.009)	0.871*** (0.008)
Boix Democracy	-0.224*** (0.035)		-0.242** (0.099)	
Polity Democracy		-0.023*** (0.002)		-0.013* (0.007)
Cumulative Census	-0.004 (0.014)	0.021 (0.022)	-0.099** (0.039)	0.091 (0.062)
Boix*Cumulative Census	0.066*** (0.010)		0.028 (0.030)	
Polity*Cumulative Census		0.007*** (0.001)		0.001 (0.002)
GDP (log)	0.025 (0.028)	0.021 (0.028)	-0.385*** (0.081)	-0.433*** (0.079)
Inflation	0.000*** (0.000)	0.000*** (0.000)	-0.000 (0.000)	-0.000 (0.000)
Urban Population	-0.005*** (0.005)	-0.004** (0.004)	-0.016*** (0.016)	-0.013*** (0.013)

	(0.002)	(0.002)	(0.005)	(0.005)
Trade	-0.000	-0.001**	-0.002**	-0.002*
	(0.000)	(0.000)	(0.001)	(0.001)
Long-run effect at Census=6	11.47		-0.07	
Long-run effect at Census=3	-1.73		-0.16	
Marginal impact at Census=6	0.17	0.019	-0.07	-0.007
Marginal impact at Census=3	-0.026	-0.002	-0.16	-0.01
Observations	4,019	3,962	4,019	3,962
R-squared	0.998	0.998	0.988	0.988

Notes: The dependent variable for Models 1 and 2 is the market Gini coefficient and, for Models 3 and 4, redistribution. The main explanatory variables are one-period-lagged democratic scores from Boix et al. (2013) and Polity IV by Marshall et al. (2016) interacted with the cumulative census variable. The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. All independent variables are lagged one year. Standard errors are clustered at the country level. *** p<0.01, ** p<0.05, * p<0.1.

6. Mechanisms

In this section we empirically test the causal mechanisms underlying our theory. We expect democratic rule to be positively associated with FDI inflows and the size of the financial sector only when state capacity exceeds a minimum level. We do not expect democratic rule to have an effect on these variables under low capacity, given the state's inability to provide contract and property rights security. For reasons outlined in Section 3, we do not anticipate a positive effect of democracy on fiscal redistribution and government spending in high-capacity contexts. Lastly, we also expect to see a positive association between FDI stock and financial development and inequality.

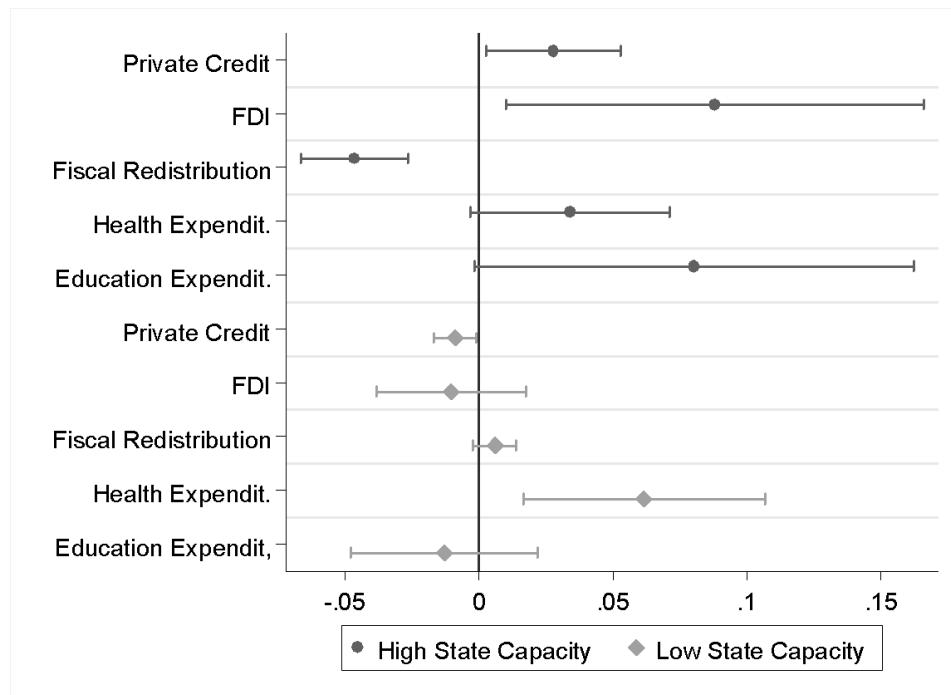
We measure financial development with an indicator commonly used in studies of financial development: private credit by deposit money banks and other financial institutions to GDP. This measurement captures the ratio of claims on the private sector by deposit money banks and other financial institutions to GDP (Beck et al., 2010). FDI is measured by annual FDI inflows to a country as a percentage of GDP, retrieved from WDI. We proxy fiscal policy through the fiscal redistribution measure introduced above and public goods provision through healthcare and education spending (as a percentage of GDP) and education levels through secondary school enrollment.

To investigate these policy mechanisms driving our theory, we present a series of split-sample regressions. For each policy area, we split the sample with respect to the level of state capacity at which the estimated impact of the Polity index on the Gini coefficient switches from positive to negative (at 3 from Model 6 of Table 1). We test these relationships using similar fixed-effects regressions as in the main analysis while controlling for GDP per capita, GDP annual change, and country- and year-fixed effects. To facilitate exposition, we have plotted the Polity coefficients for each of these 10 regressions in Figure 4. Lines around the point estimates represent 95% confidence intervals. Tables A8 and A9 in the Appendix

present the fixed-effects panel regressions that underlie the coefficient plots presented in Figure 4.

Figure 4 provides evidence that democratic rule favors FDI inflows and financial sophistication only in contexts where minimal state capacity has been met. Under low values of state infrastructural power, democracy lacks a relationship with these variables. This suggests that a minimal level of state capacity is necessary for democratic rule to improve the investment climate. By contrast, we find little evidence that democratic rule promotes fiscal redistribution and public goods provision in high-infrastructural power contexts. Democratic rule has a positive effect on health expenditure only in low-capacity contexts. As Knutsen (2013) and Hanson (2015) have argued, democratic rule may operate as a substitute for a capable state in providing better public goods in low-capacity contexts. This is explained by the special propensity of dictatorial rulers to choose non-welfare-promoting policies under low state capacity (Wintrobe, 1998). According to this rationale, a shift from dictatorship to democracy produces greater investments in redistributive social policy under low state capacity compared to democratization under high state capacity.

Figure 4. Policy Channels of Democratic Effect on Inequality at Different Levels of State Capacity



Note: This figure shows the estimated marginal effect of Polity on a series of policy areas for the subsamples with high state capacity (black dots) and low state capacity (gray diamonds), where the subsample cutoff is a cumulative census score of 3. The lines around the point estimates represent 95% confidence intervals.

Lastly, we expect to see a positive association between FDI inflows and financial development and inequality. To test this relationship, we use our baseline model (Model 3 in Table 1) and add lagged private credit and FDI stock (as percentage of GDP) as independent variables, while excluding democracy and state capacity variables (Table 6). Using net inequality as the outcome variable, Model 1 demonstrates a statistically significant relationship with Private Credit, while Model 2 displays a significant association between FDI stock and the Gini index. Models 3 and 4 produce similar results using system GMM analysis.

Table 6. Effect of Private Credit and FDI Stock on Net and Market Gini Indexes

	Mean Imputed Series			
	(1)	(2)	(3) GMM	(4) GMM
Lagged Gini	0.937*** (0.006)	0.934*** (0.007)	0.996*** (0.002)	0.996*** (0.002)
GDP per capita (log)	0.371*** (0.092)	0.377*** (0.097)	-0.007 (0.017)	0.009 (0.015)
Urban Population	-0.005 (0.005)	-0.004 (0.005)	-0.001 (0.001)	-0.001 (0.001)
Inflation	0.000*** (0.000)	0.000*** (0.000)	0.001** (0.000)	0.000** (0.000)
Trade	0.001 (0.001)	0.001 (0.001)	0.000 (0.000)	0.000 (0.000)
Private credit	0.003*** (0.001)		0.001* (0.001)	
FDI stock		0.044** (0.022)		0.029** (0.012)
Constant	-1.417 (0.978)	-0.782 (1.071)	0.054 (0.180)	-0.063 (0.185)
Hansen J-stat p-value			0.23	0.11
Number of Instruments			120	78
AR(1)			0.00	0.00
AR(2)			0.173	0.037
Observations	3,840	3,898	3,013	2,964
R-squared	0.991	0.991	0.991	0.991

Notes: The dependent variable is the net Gini coefficient. The main explanatory variables are one-period-lagged Private Credit and FDI stock. Models 3 and 4 present results from system GMM analysis. The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. All independent variables are lagged one year. Standard errors are clustered at the country level. *** p<0.01, ** p<0.05, * p<0.1.

7. Conclusion

In this paper, we have explored whether the effect of democratic rule on income inequality is conditional on state capacity. Counterintuitively, we argue that democratization and democratic rule in the context of high state infrastructural power is associated with increases in income inequality. Larger financial sectors and FDI inflows favor income concentration through market incomes. To test our hypothesis, we introduced a novel measure of state capacity based on cumulative census administration. Our empirical results are robust to instrumental variable and GMM estimation and various alternative measures of democracy, and they apply beyond the context of the industrial world, a high-capacity democratic context where inequality has increased sharply in recent decades. In addition, we also test the mechanisms of our theory, finding consistent support for our claim that the interactive effect of democratic rule and infrastructural power posited in our main analysis might operate through financial development and FDI.

We join the recent literature in exploring the conditional relationship between democracy and inequality. Our contribution is parallel and complementary to that of Dorsch and Maarek (2019), who argue that the effect of democracy on inequality is conditioned by the level of inequality at the moment of democratization. Consistently with our findings, they show that inequality tends to increase after democratic transitions in autocratic nations that had developed a strong state to deliver public goods to the poor, given that their policies tend to move towards the “middle ground” after democratization. By contrast, in regimes that democratize under low capacity, inequality is usually high, which leads to greater catering to demands by median voters for larger redistribution, resulting in a decrease in income inequality after democratization.

Albertus and Menaldo (2018) stress another set of factors—authoritarian constitutions and other institutional legacies—that pose obstacles to fiscal redistribution after democratization and affect inequality that way. In this paper, we stress the inequality-increasing mechanisms associated with democratic rule in high-state-capacity contexts. We believe our conclusion speaks directly to recent scholarship on increasing inequality in the developed world and many regions of the developing world, reflecting the natural tendency of well-functioning capitalism to produce higher income concentration (Piketty, 2014). Institutional literature has implicitly assumed that “inclusive institutions” promote not only development but also more equal income distribution, at least in the long term (Acemoglu, Johnson, and Robinson, 2001). In this paper, we have provided evidence that this conclusion may not be warranted and “inclusive” institutions—captured in the combination of democratic regime type and high-capacity state institutions—might well lead to a trend of steady increases in income inequality, which we argue happens through financial development and FDI inflows. Further research should clarify the additional pathways through which high-capacity state institutions and democratic regime type affect inequality.

Appendix

Table A1. Descriptive Statistics

Variable	Obs.	Mean	Std. Dev.	Min	Max
Net Gini	4,636	38.59	8.70	20.43	60.88
Market Gini	4,622	45.56	6.41	22.23	70.54
Redistribution	4,622	6.99	7.39	-13.86	25.43
Cumulative Census	4,449	3.42	1.36	0	6
Polity	4,445	3.80	6.59	-10	10
Boix Democracy	3,843	0.58	0.49	0	1
GDP per capita	4,225	12167.37	16971.26	182.71	111968.30
Urban Population share	4,464	53.72	23.51	4.99	100.00
Trade	7,144	70.59	47.25	0.02	441.60
Inflation	4,344	41.62	410.73	-98.70	15444.38
Private Credit	4,127	40.42	36.20	0.85	262.46
FDI Inflows	4,089	3.64	13.48	-58.32	451.72
Health Expenditure	2,616	3.53	2.05	0.27	10.05
Education Expenditure	2,528	4.39	1.66	0	13.21957
Ethnic Diversity	4,479	0.42	0.25	0.01	0.93
Civil Conflict	4,636	0.22	0.42	0	1
ODA	2,969	5.20	8.50	-0.68	181.10
Left	4,636	0.56	0.50	0	1
Fraser Index	2,105	6.59	1.07	2.47	8.88
Democracy Cheibub (2010)	3,928	0.59	0.49	0	1
Democracy Dorsch and Maarek (2019)	4,374	0.66	0.47	0	1

Table A2. List of Countries

Afghanistan, Albania, Argentina, Armenia, Australia, Austria, Bahamas, Bangladesh, Barbados, Belarus, Belgium, Benin, Bhutan, Bolivia, Bosnia, Botswana, Brazil, Bulgaria, Burkina Faso, Cambodia, Cameroon, Canada, Chad, Chile, China, Colombia, Costa Rica, Croatia, Cyprus, Czech Rep., Denmark, Djibouti, Ecuador, Egypt, El Salvador, Estonia, Ethiopia, Finland, France, Gambia, Georgia, Germany, Ghana, Greece, Guatemala, Guinea, Haiti, Honduras, Hungary, Iceland, India, Indonesia, Iran, Ireland, Israel, Italy, Japan, Jordan, Kazakhstan, Korea, Rep., Latvia, Lebanon, Lesotho, Liberia, Lithuania, Luxembourg, Macedonia, Madagascar, Malawi, Malaysia, Maldives, Malta, Mauritania, Mauritius, Mexico, Moldova, Mongolia, Morocco, Myanmar, Namibia, Nepal, Netherlands, New Zealand, Nicaragua, Niger, Nigeria, Norway, Pakistan, Panama, Paraguay, Peru, Philippines, Poland, Portugal, Qatar, Romania, Russia, Rwanda, Senegal, Seychelles, Sierra Leone, Singapore, Slovakia, Slovenia, South Africa, Spain, Sri Lanka, Sweden, Switzerland, Taiwan, Tajikistan, Tanzania, Thailand, Togo, Tunisia, Turkey,

Uganda, Ukraine, United Kingdom, United States, Uruguay, Vanuatu, Venezuela, Zambia, Zimbabwe.

Table A3.

Country	Independence year	Prior ruling country/empire	Censuses under other countries
Armenia	1991	Soviet Union	1959, 1970, 1979, 1989
Bangladesh	1971	Pakistan	1951, 1961
Belarus	1991	Soviet Union	1959, 1970, 1979, 1989
Benin	1960	France	-
Bosnia	1992	Yugoslavia	1953, 1961, 1971, 1981, 1991
Botswana	1966	Britain	1946, 1964
Burkina Faso	1960	France	-
Cameroon	1960	France	-
Chad	1960	France	-
Croatia	1991	Yugoslavia	1953, 1961, 1971, 1981, 1991
Cyprus	1960	Britain	1960
Czech Rep.	1992	Czechoslovakia	1950, 1961, 1970, 1980, 1991
Djibouti	1977	France	1967
Eritrea	1993	Ethiopia	1984
Estonia	1991	Soviet Union	1959, 1970, 1979, 1989
Gambia	1965	Britain	1963
Georgia	1991	Soviet Union	1959, 1970, 1979, 1989
Ghana	1957	Britain	-
Guinea	1958	France	-
Kazakhstan	1991	Soviet Union	1959, 1970, 1979, 1989
Latvia	1991	Soviet Union	1959, 1970, 1979, 1989
Lesotho	1966	Britain	1956
Lithuania	1991	Soviet Union	1959, 1970, 1979, 1989
Macedonia	1991	Yugoslavia	1953, 1961, 1971, 1981, 1991
Madagascar	1960	France	-
Malawi	1964	Britain	-
Malaysia	1963	Britain	1951, 1960
Mauritania	1960	France	-
Morocco	1956	France	-
Namibia	1990	South Africa	1960, 1970, 1981
Niger	1960	France	-
Nigeria	1960	Britain	-
Russia	1991	Soviet Union	1959, 1970, 1979, 1989
Rwanda	1962	Belgium	-
Senegal	1960	France	-
Seychelles	1976	Britain	1960, 1971
Sierra Leone	1961	Britain	-
Singapore	1965	Malaysia	1957
Slovakia	1992	Czechoslovakia	1950, 1961, 1970, 1980, 1991
Slovenia	1991	Yugoslavia	1953, 1961, 1971, 1981, 1991
Tajikistan	1991	Soviet Union	1959, 1970, 1979, 1989
Tanzania	1961	Britain	1948, 1957
Togo	1960	France	-
Tunisia	1956	France	-
Uganda	1962	Britain	1948, 1959
Ukraine	1991	Soviet Union	1959, 1970, 1979, 1989

Vanuatu	1980	France	1967
Vietnam	1976	France	-
Zambia	1964	Britain	1950, 1963
Zimbabwe	1980	Britain	1962, 1969

Table A4. Unit Root Tests for Main Models (5) and (6) in Table 1

Variable	Fisher			Phillips-Perron	Conclusion
	No trend/drift	Trend	Drift		
Gini	0.6670	0.1793	0.0000	0.0000	Stationarity
Boix Democracy	1.0000	1.0000	0.0000	1.0000	Unit root
Polity Democracy	0.0063	0.0000	0.0000	0.0000	Stationarity
Cumulative Census	1.0000	0.0000	0.0000	1.0000	Stationarity
GDP (log)	0.9702	0.0978	0.0000	0.0860	Stationarity
Inflation	0.0000	0.0000	0.0000	0.0000	Stationarity
Urban Population	0.0000	0.0000	0.0000	0.0000	Stationarity
Trade	0.0007	0.0000	0.0000	0.0000	Stationarity

Note: Fisher's tests with both the Augmented Dickey Fuller and the Phillips-Perron specifications were implemented. The values in the table are combined p-values from different independent unit root tests (one per panel). All variables in the main models were tested (models 5 and 6 in Table 1). All tests also included parameters to test for trend and drift. All tests included one lag (same lag structure as the estimation strategy). These tests were implemented using the “xtfisher” routine in Stata (v. 15). The alternative hypothesis is stationarity. As the table suggests, most tests indicate stationary residuals at conventional levels of statistical significance.

Table A5. First-Stage Results

	(1)	(2)	(3)	(4)	(5)	(6)
DV:	DV:	DV:	DV:	DV:	De-	DV:
Democra	Democra	Boix*Cumula	Democra	mocracy	Polity*	Cumul
cy (Boix)	y (Boix)	tive Census	y (Polity)	(Polity)	Cumulative	Census
L. Boix Regional share	0.934*** (0.045)	1.130*** (0.048)	0.067 (0.142)			
L6. Boix Regional share	0.030 (0.036)	0.085** (0.037)	-0.007 (0.109)			
Boix Regional share *Cumulative Census		-0.101*** (0.007)	0.754*** (0.022)			
L. Polity Region Average				1.223*** (0.120)	1.375*** (0.126)	1.349*** (0.358)

L6. Polity Region Average			1.451***	0.128	0.124
			(0.312)	(0.317)	(0.380)
Polity Region Average				-0.029***	0.765***
*Cumulative Census				(0.009)	(0.027)
GDP (log)	- 0.124*** (0.013)	-0.078*** (0.014)	0.002 (0.042)	-2.738*** (0.200)	-2.578*** (0.205)
Inflation	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	0.000*** (0.000)	0.000*** (0.000)
Urban Population	0.001 (0.001)	-0.003*** (0.001)	-0.019*** (0.003)	0.100*** (0.012)	0.088*** (0.013)
Trade	0.001*** (0.000)	0.001*** (0.000)	0.001* (0.001)	0.014*** (0.003)	0.019*** (0.003)
Observations	6,543	6,139	6,039	5,676	5,589
R-squared	0.70	0.56	0.67	0.53	0.71
					0.49

Notes: The dependent variables are Boix et al. (2013) (Models 1 and 2) and Polity IV by Marshall et al. (2016) (Models 4 and 5) and their interaction with cumulative census values (Models 3 and 6). The key independent variables include lags of regional democracy share/average indicators and their interaction with cumulative census values. The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. Standard errors are clustered at the country level. *** p<0.01, ** p<0.05, * p<0.1.

Table A6. Effect of Democracy and State Capacity on the Net Gini Index: Sample restrictions

	Mean Imputed Series			
	(1) Boix French Colonies Excluded	(2) Polity French Colonies Excluded	(3) Boix British Colonies Excluded	(4) Polity British Colonies Excluded
Gini Lagged	0.946*** (0.006)	0.945*** (0.006)	0.953*** (0.006)	0.959*** (0.006)
Boix Democracy	-0.240** (0.108)		-0.384*** (0.101)	
Polity Democracy		-0.021*** (0.007)		-0.027*** (0.007)
Cumulative Census	0.096** (0.041)	-0.037 (0.073)	0.036 (0.039)	0.015 (0.064)
Boix Democracy*Cumulative Census	0.107*** (0.032)		0.146*** (0.030)	
Polity Democracy*Cumulative Census		0.007*** (0.002)		0.009*** (0.002)
Inflation	0.000 (0.000)	0.000* (0.000)	0.000 (0.000)	0.000** (0.000)

Urban Population	0.004 (0.005)	-0.002 (0.005)	0.006 (0.005)	0.001 (0.005)
Trade	0.002** (0.001)	0.001 (0.001)	0.003*** (0.001)	0.001 (0.001)
GDP (log)	0.388*** (0.081)	0.412*** (0.090)	0.314*** (0.076)	0.329*** (0.080)
Constant	1.584*** (0.522)	-1.878** (0.946)	1.391*** (0.483)	-1.747** (0.846)
Long-run effect at Census=6	7.44	0.38	10.47	0.66
Long-run effect at Census=3	1.5	0	1.15	0
Marginal impact at Census=6	0.402	0.021	0.492	0.027
Marginal impact at Census=3	0.081	0	0.054	0
Observations	3,775	3,602	3,372	3,205
R-squared	0.991	0.991	0.993	0.993

Notes: The dependent variable is the net Gini coefficient and the main explanatory variables are one-period-lagged democratic scores from Cheibub et al. (2010) and Dorsch and Maarek (2019) interacted with the cumulative census variable. The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. All independent variables are lagged one year. Standard errors are clustered at the country level. *** p<0.01, ** p<0.05, * p<0.1.

Table A7. Effect of Democracy and State Capacity on the Net Gini Index: Alternative Measures of Democracy

	Mean Imputed Series	
	(1) Cheibub	(2) Dorsch and Maarek
Lagged Gini	0.943*** (0.020)	0.935*** (0.018)
Democracy Cheibub	-0.251 (0.154)	
Democracy Dorsch and Maarek		-0.183 (0.169)
Cumulative Census	0.050 (0.067)	0.035 (0.079)
Democracy Cheibub*Cumulative Census	0.127*** (0.047)	
Democracy Dorsch & Maarek (2019)*Cumulative Census		0.096* (0.055)
GDP (log)	0.427** (0.189)	0.409** (0.190)
Inflation	0.000 (0.000)	0.000 (0.000)
Urban Population	0.004 (0.008)	-0.002 (0.008)
Trade	0.000 (0.002)	-0.000 (0.002)
LR effect at Census=6	8.96	6.05
LR effect at Census=3	2.28	1.62
Marginal impact at Census=6	0.51	0.39
Marginal impact at Census=3	0.13	0.105

Observations	3,445	3,905
R-squared	0.993	0.991

Notes: The dependent variable is the net Gini coefficient and the main explanatory variables are one-period-lagged democratic scores from Cheibub et al. (2010) and Dorsch and Maarek (2019) interacted with the cumulative census variable. The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. All independent variables are lagged one year. Standard errors are clustered at the country level. *** p<0.01, ** p<0.05, * p<0.1.

Table A8. The Effect of Democracy on FDI and Financial Development

	(1)	(2)	(3)	(4)
	DV: Private Credit	DV: Private Credit	DV: FDI inflows	DV: FDI inflows
	High Capacity	Low Capacity	High Capacity	Low Capacity
Lagged DV	0.883*** (0.008)	0.957*** (0.008)	-0.117*** (0.023)	0.319*** (0.019)
Polity	0.028* (0.015)	-0.009* (0.005)	0.088* (0.047)	-0.010 (0.017)
GDP per capita	0.123*** (0.024)	0.080*** (0.014)	0.086 (0.078)	0.171*** (0.058)
% Change GDP	-0.603*** (0.089)	-0.227*** (0.076)	0.762** (0.310)	0.113 (0.271)
Constant	-0.199 (0.167)	-0.048 (0.033)	0.140 (0.566)	-0.383*** (0.134)
Wald p-value	0.06	0.06	0.05	0.54
Observations	1,950	3,096	2,105	2,802
R-squared	0.987	0.971	0.378	0.367

Notes: The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. All independent variables are lagged one year. Standard errors are clustered at the country level. *** p<0.01, ** p<0.05, * p<0.1.

Table A9. The Effect of Democracy on Fiscal Redistribution

	(1)	(2)	(3)	(4)	(5)	(6)
	DV: Redistributio n	DV: Redistribution	DV: Health Expenditur e	DV: Health Expenditur e	DV: Education Expenditure	DV: Education Expenditure
	High Capacity	Low Capacity	High Capacity	Low Capacity	High Capacity	Low Capacity
Lagged DV	0.871*** (0.015)	0.830*** (0.012)	0.762*** (0.016)	0.709*** (0.026)	0.612*** (0.035)	0.805*** (0.020)
Polity	-0.046*** (0.012)	0.006 (0.005)	0.034 (0.023)	0.062** (0.027)	0.080 (0.050)	-0.013 (0.021)
GDP per capita	-0.012	0.005	0.089**	0.235	-0.008	0.118**

	(0.019)	(0.017)	(0.044)	(0.146)	(0.107)	(0.058)
% Change GDP	-0.172** (0.082)	0.049 (0.099)	-0.624*** (0.115)	-0.514 (0.335)	-0.344 (0.239)	-0.556** (0.245)
Constant	0.159*** (0.053)	0.084 (0.076)	0.170 (0.117)	0.750*** (0.103)	0.124 (0.268)	0.030 (0.118)
Wald p-value	0.00	0.23	0.13	0.02	0.11	0.54
Observations	2,020	2,056	1,770	874	612	1,099
R-squared	0.989	0.989	0.972	0.904	0.935	0.935

Notes: The unit of analysis is country-year. All specifications include a full set of country- and year-fixed effects. All independent variables are lagged one year. Standard errors are clustered at the country level. *** p<0.01, ** p<0.05, * p<0.1.

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Recreating Market Conditions for Vote-Selling and Vote-Buying in the Lab: The Chilean Case

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Abstract

The literature asserts that Chilean parties no longer buy votes. While those are good news, the bad news are that we are rather ignorant about a number of other interesting, and yet, unanswered questions. First and foremost, the approach used by most scholars focuses exclusively on vote-buying. That is, parties offering to buy votes, completely ignoring the ones who sell their votes (i.e. voters). This is a rather important distinction. What would voters do if offered the chance to sell their votes? Would they sell them? And if so, at what price? Would voters still sell their votes to their own party of preference, or would they sell it to the opposing party? Do voters set a higher selling price if selling to the opposing party, while lowering the price if selling to the party they would have supported anyways? Another important question is who political parties target: party supporters, opposers, or swing voters? By recreating market conditions that exist between vote-buyers and vote-sellers implemented in the lab, the paper sheds light on these issues.

Please consider downloading the last version of the paper [here](#).

Rough draft PLEASE DO NOT CIRCULATE.

Keywords— clientelism; vote-buying; vote-selling; experimental economics; formal modeling.

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I. INTRODUCTION

Scholars mostly agree on the positive correlation between poverty and clientelism (Calvo and Murillo 2004; Weitz-Shapiro 2012; Kitschelt 2000; Kitschelt and Altamirano 2015).¹ Since the poor derive more utility from immediate transfers than the uncertain returns associated with future policy packages, clientelist political parties only target the poor (Brusco, Nazareno, and Stokes (2004) and Stokes et al. (2013)). Indeed, Weitz-Shapiro (2014, p. 12) explained that “[a]lmost universally, scholars of clientelism treat and analyze [this] practice as an exchange between politicians and their poor clients.”

This agreement has recently been challenged (Hicken 2007, p. 55). González-Ocantos et al. (2012) and Holland and Palmer-Rubin (2015) found that income had little or no effect on vote-buying. For instance, Szwarcberg (2013, p. 32) “challenges the assumption [that brokers] with access to material benefits will always distribute goods to low-income voters in exchange for electoral support.” In fact, Bahamonde (2018) explains that non-poor individuals can be targeted when they are sufficiently noticeable, increasing compliance. He explains that wealthy houses in very poor neighborhoods in Brazil can be targeted too.

While there are important agreements, the literature has failed to provide a convincing answer to the following question: *Who do parties target? Swing or core voters? Why?* These questions have historically haunted the literature. In fact, Carlin and Moseley (2015, p. 14) states that “our knowledge of who parties target remains incomplete.” On the one hand, Cox and McCubbins (1986) explain that since constituencies are well-known, they allocate resources to core voters. On the other hand, Stokes (2005) argues that since allocating resources to individuals who *ex-ante* vote for the party is a waste, parties target swing voters (similarly, see Zarazaga (2016) and Gallego (2014)). Yet, Zarazaga (2016, p7) asserts that both “[q]ualitative and quantitative evidence mainly shows that party machines reward their own supporters,” not swing voters.

Instead, the clientelism literature has seen a proliferation of tangential answers. While all of them are contributions, they do not really tackle the afore mentioned question. Dixit and Londregan (1996) explain that parties both swing and core voters, but that depends on a number of factors. . Nicther (2008) (turnout-buying) is another very important contribution. Unfortunately, it deviates from the question by increasing the complexity on the varieties of clientelism. Similarly, Zarazaga

add something

¹Following Nicther (2014, p. 316), clientelist vote-buying is defined as “the distribution of rewards to individuals or small groups during elections in contingent exchange for vote choices.”

(2016, p. 7) introduces yet another category (conditional supporters) who “will vote for the party machine only as long as unexpected events do not persuade them to do otherwise”

The paper seeks to contribute to this issue by incorporating both structural and individual factors that foster clientelism in the same theory.

II. THE MODEL

We consider an electorate of n voters. Voters vote for a leader to implement a common policy γ from the set $\Gamma = \{1, 2, \dots, 100\}$. Each citizen i has an ideal point x_i which is an *iid* draw from an uniform distribution Γ . When policy γ is implemented, payoffs of citizen i are given by $u(D, x_i, \gamma) = D - |x_i - \gamma|$, where D represents **completar acá**. This payoff can be incremented by transferences from both parties to voter i .

In this election, there are two candidates. One “left-wing” party and one “right-wing” party. The left-wing (right-wing) candidate represents a policy γ_L (γ_R) which is an *iid* draw from an uniform distribution over $\{1, \dots, 50\}$ ($\{51, \dots, 100\}$). The location of this policy give us the number of voters n_L leaning towards the left-wing candidate, while the number of voters leaning towards the right-wing party is given by $n_L + n_R = n$. While we consider that voters are attached to an ideological continuum, we do so with the sole purpose of modeling preferences—both formally and experimentally.²

Moving forward, both parties negotiate with only one of these n voters. That voter is randomly selected from the total population n . Observe that the higher the n , the lower the representation in the election of this voter. That is, a larger n necessarily implies that every individual electoral choice matters less. However, if n is small, negotiating with this voter may be more attractive to political parties. This is because negotiating with a large number of voters is costly. We assume that each party has a budget (B) that they can use to buy votes. If a party decides not to negotiate with the voter (or the voter does no accept the offer), the party keeps this budget. The profits of party i is given by,

²Ultimately, experimental subjects are not told anything about ideology. They only observe that there are a number of “points” associated with the victory of party A or party B. In this sense, voters lean (“ideologically”) towards the party that gives them more points.

$$\pi_i(W, e_i, s_i) = W \cdot e_i + (1 - s_i \cdot a_j) \cdot B$$

where W ($W \geq B$) is a constant that represents how much each party values winning the election, $e_i = 1$ if party i wins the election, 0 otherwise, s_i is the fraction of B that the party offers to voter j who can accept the offer ($a_j = 1$) or not ($a_j = 0$). We study two versions of this party-voter interaction. One is where both parties make simultaneous offers to the voter, and she decides whether to accept the offer (vote-buying case). Another one is where the voter can make private offers to both parties, and then the party decides if to pay or not for that voter's vote (vote-selling case).

The timing of the game is as follows: at the beginning of the game n voters and two political parties are randomly located on their respective ideal points: voters along Γ , the “left-wing” candidate along $\{1, \dots, 50\}$, and the “right-wing” candidate on $\{51, \dots, 100\}$. All locations are public information, as well as every party’s budget B , the total number of voters (n) and the number of supporters of each party (n_L and n_R). What follows then, depends on the specific game. On the vote-buying case, each party simultaneously decides if making an offer to the voter. If a party decides to negotiate with the voter, privately offers him to buy his vote (i.e. accept the offer and vote for the party). Then the voter decides if to take the offer, or which one accept if he receives two offers. If he accepts an offer, he should vote for that candidate.³ On the vote-selling case, the voter may privately propose a certain amount to each party in exchange for her vote. Then the parties decide if to pay or not the offer. The voter then decides which one to accept, if any. In this case, the voter offers to one or both parties, and each proposed amount might be different.

I. Equilibrium in Vote-Buying Case

In this case, both parties can offer certain amount in exchange for electoral support. Note that parties only have incentives to negotiate with a voter if he is the pivotal voter. That means that $|n_L - n_R| \leq 1$, and that voter i supports the ex-ante winner of the election ($i \in \max\{n_L, n_R\}$). The voter prefers the party closer to her ideal point. If both parties are located at the same distance, the

³It is important to consider that to simplify the game (and the experiment), accepting the offer necessarily implies compliance. That is, accepting the offer means voting for the party the voter accepted the offer from. We leave for future research the case where the voter may defect.

voter is indifferent. Denote by $i^* \in \{L, R\}$ the preferred party of the voter, and $-i^*$ the other party.

Note that, naturally, both parties want to make different offers. If the voter is pivotal, the less preferred party has incentives to offer him a certain amount m_{-i^*} such that he perceives more utility voting for that party rather than voting for the opposite party, that is:

$$\begin{aligned} m_{-i^*} &\geq (D - |x_{i^*} - \gamma_{i^*}|) - (D - |x_{i^*} - \gamma_{-i^*}|) \\ &= |x_{i^*} - \gamma_{-i^*}| - |x_{i^*} - \gamma_{i^*}|. \end{aligned}$$

Parties expect winning the election but have limited budgets. Hence, they want to win the election at a minimum cost. If party $-i^*$ offers the voter $m_{-i^*} = |x_{i^*} - \gamma_{-i^*}| - |x_{i^*} - \gamma_{i^*}|$, he will be indifferent between voting for party i^* or party $-i^*$. Both offers $m_{i^*} = 0$ and $m_{-i^*} = |x_{i^*} - \gamma_{-i^*}| - |x_{i^*} - \gamma_{i^*}|$ are the minimum amount, but enough to make the pivotal voter indifferent between both political parties. Indifference gives the party some electoral advantage of winning of the election. Voter indifference gives two possible Nash equilibria. In one equilibrium the voter rejects the offer and votes for i^* . In the other equilibrium, the voter accepts the offer and the elected party is $-i^*$. If individuals are utility maximizers, they should be indifferent between these two equilibria.

II. Equilibrium in Vote-Selling Case

In the case that the voter can set the price of his vote, he may negotiate with one or both parties setting the price that he is willing to accept in exchanging of voting for that party. In this setting, the voter has incentives to set the highest price each party can pay. In our model this is given by B (which is public knowledge). When the voter is pivotal, he may swing towards party $-i^*$ only if the budget is big enough to compensate what he loses when voting for his less prefer policy ($B > |x_{i^*} - \gamma_{-i^*}| - |x_{i^*} - \gamma_{i^*}|$). When the voter decides to negotiate with both parties, and both accept to pay the price set by him, he chooses one offer, voting for his preferred political party i^* .

Since the parties-voter negotiation does not change the electoral outcome, vote-selling is not efficient to parties. When a party wins the election due to vote-selling, the party's payoff is $\pi_i(W, 1, 1) = W$, while the loser party obtains $\pi_i(W, 0, 0) = B$. If the pivotal voter decides to negotiate with both political forces, parties i^* and $-i^*$ have to decide if accept to pay B to the

voter. This strategic situation is represented as follows,⁴

		$-i^*$
	Accept	Reject
i^*	Accept	W, B
	Reject	B, W
		$W + B, B$

Thus, we can observe that there exists an unique equilibrium where both parties are willing to pay B to the voter.

III. EXPERIMENTAL DESIGN

Following our theoretical formalizations, a lab economic experiment was performed. The experiment was conducted at O'Higgins University and Centre for Experimental Social Sciences (CESS) of *Universidad de Santiago*, Chile. Subjects received a minimum of \$5,000 Chilean pesos. The maximum depended on the quality of individual decisions. [summary statistics here](#). The basic flow is depicted in [Figure 1](#).

The experiment has two parts, with four stages each. The first part is the vote-buying portion. During the first stage, participants are assigned a role at random. They can be either *party A*, *party B*, or *voter*. Voters are assigned at random an “ideological” position. That is, voters receive a certain amount of points (at random) depending on whether party A or B wins the election. For instance, if party A wins election, a voter might receive 2,400 points, whereas if party B wins the election, the voter might receive 200 points. It is in this sense that the voter is “ideologically” closer to party A. The substantive correlate is that voters perceive some utility when, for instance, their preferred fiscal policies are implemented. During the first stage, both parties receive different endowments too. The idea is to reflect the fact that some parties are wealthier than others. Note that voters receive zero endowments. The clientelism literature is consistent in that both poor and rich voters are prone to receive clientelist offerings ([Bahamonde 2018](#)).

⁴This situation is considering that, if both parties accept to pay the price set by the voter, he prefers the party i^* .

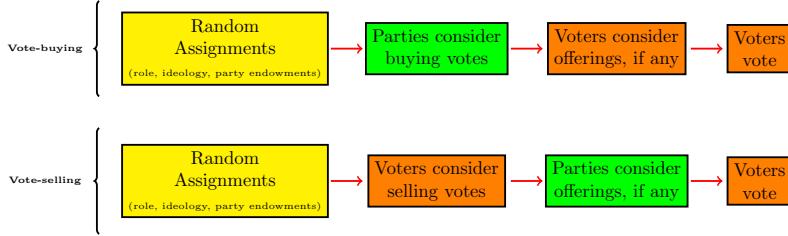


Figure 1: Experimental Flow.

Note: Note here.

During the second stage of the first part, parties decide whether to go out and buy votes by making clientelist offerings. Experimental subjects playing the party role enter an amount of points, which ranges from zero to the maximum assigned budget. They are told that offering zero means they do not want to buy votes. Importantly, both budgets (for party A and B) are the same. However, given that the voter-party distance is assigned at random, both parties have *relative* “different” budgets.

In the third stage voters evaluate whether to take that offer or not. If the party decided that it did not want to make an offer at that time, the voter is told that the party did not make an offer. Voters are told that accepting the offer necessarily implies voting for that party (no defecting in this experimental design). In this regard, the third and fourth stage are in reality one stage.

The second part is the vote-selling portion of the experiment. This part is run during the same experimental session, but loading a separate `Ztree` program. Right after the first part is completed, experimental subjects are then asked to continue with the study.

The second part is exactly the same, except that this time voters are first-players: they get to offer parties an amount of points, and then, parties get to decide whether to take or reject that offer. Note that the experimental currency are “points,” which later translated into actual money.⁵

I. Expected Comparative Statics

This experiment randomizes the voter’s and party’s “ideological” positions, party endowments,⁶, and whether the voter is pivotal or not (whether the voter represents $\frac{1}{3}$ or $\frac{1}{5}$ of the electorate). There is one static event, namely, the order of the experiment (the vote-buying part goes first, while the

⁵Particularly, Chilean pesos.

⁶As explained above, not directly, but by randomizing the ideological position of voters. By doing so, we are able to manipulate the *relative* purchasing power of every party.

vote-selling part goes second). This aspect is presented to all experimental subjects (both roles) before the second stage (both experimental parts). Exploiting this experimental data, we intend to shed light on the conditions that foster vote-buying/selling. Particularly, we are interested in analyzing the next aspects of a clientelist transaction.

Ideology. Since Downs (1957), spatial theorist have theorized for a long time about the role of political ideology on different electoral aspects (see Enelow and Hinich (1990) for a review). Unfortunately, one of the main criticisms of the Downsian paradigm, is its unidimensionality. That is, the big assumption of voters being concerned only about the spatial distance between their policy preferences and the ones of the available parties. Acknowledging this problem, Adams, III, and Grofman (2005, p. 20) introduce “non-policy” factors. Unfortunately, these factors are mainly socio-demographics traits, such as race, gender, income, among others. While it is true that these traits are not strictly policy-based, they are highly correlated with them. In this paper we try to advance the literature by incorporating clientelism, a non-policy issue (Kitschelt and Wilkinson 2006).

Our experimental design allows us to explore the tipping point at which voters stop caring about ideology. Since the voter-party spatial distance is randomized, our design sheds some light on the elasticity of ideology, clarifying *when* voters renounce to politics (and start selling their votes). Hence, by offering voters a non-policy choice (selling one’s vote), we complement the spatial literature, ultimately, by focusing on the question of democratic values too.

Competitiveness. The degree in which elections are contested or not has an important role in explaining clientelism. Competitive authoritarian regimes (Levitsky and Way 2010) survive not due to electoral fraud, but because of the incumbent’s capacity to mobilize a large mass of supporters, discouraging likely opposers (Magaloni 2008). Unfortunately, we still do not know at which point likely opposers feel discouraged, and abandon the electoral race. Since the experiment also randomizes the number of likely voters, we will be able to observe at which point is efficient to buy votes (when needed), and at which point is a waste.

Endowments. For the Brazilian case, Bahamonde (2018) explains that parties with access to more resources are also able to buy more expensive goods, even targeting the wealthy. However, Szwarcberg (2013, p. 32) finds that parties with access to material resources does not necessarily

imply clientelist targeting. The literature then has not been really able to explain the relationship between having resources and vote-buying: *Do parties with more resources engage in vote-buying?*

Targeting. *Who do political parties target? Swing voters or core supporters?*

Voter's Bargaining Power. By manipulating the relative importance of a voter, we will be able to answer the following question: *Does block voting (i.e. when unions or other civic groups vote coordinately for the same candidate) increase the selling price?*

Sequence. By considering two sequences, one where the party gets to be the first player, and one where the voter does, *Does the order matter?*

IV. STATISTICAL ANALYSES

pending.

V. DISCUSSION

pending.

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Vote Selling in the United States: Introducing Support Vector Machine Methods to Analyzing Conjoint Experimental Data

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Abstract

Democracy has traditionally been theorized as a multidimensional concept (Dahl, 1971). Yet, the quantitative study of clientelism—as a democratic failure—has been mostly studied from a unidimensional perspective. We argue that to better understand clientelism, quantitative studies should situate the phenomena within the multidimensionality of democracy to understand which specific subdimension should fail to explain vote selling. By leveraging a conjoint experiment on hypothetical vote selling in the United States—a traditionally considered consolidated democracy—this paper makes both methodological and substantive contributions to the literature. Using machine learning techniques, the paper identifies that among the five conjoint attributes considered in this study, the one related to the co-dependence of the Presidency on Congress is the only one that explains individual levels of vote selling. These findings are particularly relevant to the study of American democracy given the fact that the data were collected during the campaign period where Donald Trump was elected.

Abstract length: 150 words.

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Keywords— conjoint designs; vector support machines; support for democracy; United States.

I. TOWARD A MULTIDIMENSIONAL STUDY OF CLIENTELISM

Democracy has been theorized as a multidimensional concept. Specifically referring to *polyarchies*, Dahl (1971, p. 3) explains that full democracies should satisfy a number of dimensions which speak to certain institutional guarantees that create opportunities to (1) formulate political and social preferences, (2) signify those preferences, and (3) have preferences weighted equally when conducting a government.

Yet, clientelism—as a democracy failure—has been studied almost exclusively from a unidimensional perspective. We believe that there exists a methodological and conceptual alignment—one that biases our inferences. On the one hand, qualitative, historical and/or ethnographically-based contributions describe clientelist transactions as complex and multidimensional. By employing qualitative techniques, researchers are able to provide “thick descriptions” (Goertz 1973) of the phenomena at hand (Posada-Carbó 1996; Sabato 2001; Auyero 2000; Szwarcberg 2013; Borges 2019). On the other hand, statistical, survey, and/or experimentally-based work mostly explores singular aspects related to clientelism—typically, the effect of a single variable (or treatment) on the probability of clientelism.¹ For example, using a field experiment in Benin, Wantchekon (2003) stresses the role of “incumbency” on vote buying, while Jensen and Justesen (2014, p. 227) focus on the impact of “poverty” on vote buying. While the quantitative literature on clientelism has advanced on a number of important questions, most studies concentrate their efforts on a single variable which (when possible) is manipulated in an experimental or quasi-experimental design (Corstange 2012; Imai, Bethany Park, and Kenneth Greene 2015; Nichter and Peress 2017; Hicken et al. 2015; Hicken et al. 2018; Michael and Thachil 2018; Bratton 2008; Weitz-Shapiro 2012; González-Ocantos,

¹Quantitative scholars then usually focus on the “effects of causes” rather than on the “causes of effects” (Pearl 2015).

Kiewiet de Jonge, and Nickerson 2014; Bahamonde 2018; Bahamonde 2020; Oliveros 2016). Since the approach (i.e. unicausal/multicausal) is correlated with the method (quantitative/qualitative), we believe this methodological and conceptual alignment represents an important gap in the literature.

Substantively, we argue that to better understand the motivations behind clientelism and the micro-dynamics that drive it, studies should situate the phenomena within the *multidimensionality of democracy*. In other words, What are the *causes* of clientelism? Which *dimensions* of democracy—as described by Dahl (1971)—should fail to produce clientelism? While qualitative researchers are better equipped to properly answer these questions, there are some quantitative techniques that might provide broader explanations for the causes of clientelism. We do not argue that these quantitative tools might give us the kind of rich explanations ethnographies provide. However, we hope this paper provides multidimensional answers to a multidimensional concept within the quantitative framework. Ultimately, this paper tries to provide a multidimensional explanation for clientelism within the “effects of causes” approach (Pearl 2015). Exploiting a novel conjoint dataset, this paper developed an experimental design which sought to answer *Which of the three democratic dimensions explained by Dahl (1971) should fail to produce clientelism in the United States?*

Since the vote-buying literature mostly considers developing countries and describes vote sellers as poor (Weitz-Shapiro 2014, p. 12), uneducated (González-Ocantos, Kiewiet de Jonge, and Nickerson 2014), and undemocratic (Carlin and Moseley 2015), the willingness to sell votes in the United States should be low, making it a difficult case study on vote selling.² And such, this study follows a “least-likely” design presenting the United States as a “crucial case.” As Levy (2008, p. 12) explains, “[i]nferential leverage from a least likely case is enhanced if our theoretical priors for the leading alternative explanation make it a most likely case for that theory.” However, the evidence that this paper presents may be associated with a probable erosion of American democracy (Levitsky and

²However see Bahamonde (2020).

Ziblatt 2018). Foa and Mounk (2016, p. 7) document a deep “crisis of democratic legitimacy [that] extends across a [...] wider set of indicators” in the United States. They find that 26% of millennials declare that it is “unimportant” in a democracy for people to “choose their leaders in free elections” (Foa and Mounk (2016, p. 10) and Foa and Mounk (2017)). Our study aims to contribute to this debate by presenting experimental evidence that links the democracy theory literature with the clientelism literature.

The methodological contribution of this paper is twofolds. First, this paper contributes to the literature by leveraging a conjoint experiment on hypothetical vote selling in the United States, a traditionally considered consolidated democracy. Most quantitative studies have been conducted in developing countries, seriously narrowing the scope of our inferences. In part, this is because the clientelism literature usually focuses on realized behaviors only—that is, actual clientelist transactions. Unfortunately, by ignoring attitudes of *potential* vote sellers, particularly when it comes to the willingness to sell, selection bias seriously threatens causal inferences. Geddes (1990, p. 131) explains the well-known selection issues of studying “only cases that have achieved the outcome of interest.” Thus, and following the lead of González-Ocantos, Kiewiet de Jonge, and Nickerson (2014) and Bahamonde (2020), this paper presents experimental evidence of hypothetical willingness to sell the vote in the United States.

Second, we introduce machine learning techniques, particularly support vector machine analyses (SVM) for analyzing conjoint datasets. SVMs rely on computational algorithms that solve classification problems in a data-driven fashion. One of the main advantages of SVM methods is that their optimal classification properties are robust even when working with multiple dimensions at the same time. Since this paper makes the case for a multidimensional approach to the study of clientelism, we claim the method is appropriate and relevant to the discipline. From a technical standpoint,

SVMs separate groups of observations in a hyperplane. A hyperplane is a geometrical space where observations—survey respondents, in this case—are located. As explained later, the three democracy dimensions described by Dahl (1971) were operationalized in five different subdimensions (i.e. conjoint attributes). This paper employed SVM methods to group most-similar observations according to their preferences toward the five conjoint attributes. After the classification problem was solved, standard regression techniques were used to study correlations between the preferences of survey participants toward the five conjoint attributes and their willingness to sell the vote—captured by a question asked during the same study.

We claim this set of techniques might provide new insights about the *specific* democracy dimensions (Dahl 1971) that should fail to make vote selling most likely. In fact, we find that among the five attributes, the only one that ought fail to make vote selling most likely, is the presence of presidential checks and balances. That is, individuals who systematically do not value accountable Presidents, thus preferring a fully autonomous President—one that governs *without* a Congress—are more likely to sell their vote. We believe that this contribution is novel. The authors are not aware of any study in the quantitative literature of clientelism that conceives democracy as a multidimensional concept. In fact, the literature usually assumes that clientelism emerges when “democracy”—as a whole—fails. This paper takes a step further explaining which *specific* democracy attribute ought to fail to make vote selling most likely.

The paper continue as follows. **First**, we explain the logic of conjoint analyses and their main contribution to political science. **Second**, we introduce a novel dataset of U.S. voters representative at the country level. In that section we **analyze** the conjoint dataset exploiting the traditional conjoint approach and explain its shortcomings. **Third**, we introduce the support vector machine approach to analyzing conjoint data, and proceed to explaining its main advantages and show our

main results. Finally, in the [Appendix](#) section we make available a [Python](#) ([Listing 1](#)) routine not only to replicate the data analyses in this study, but also to spread these methods among interested researchers, to hopefully move this area of research forward. The [final section](#) concludes.

II. TRADITIONAL CONJOINT ANALYSES

Conjoint designs ask respondents to choose from hypothetical profiles that combine multiple attributes, “enabling researchers to estimate the relative influence of each attribute value on the resulting choice or rating” (Hainmueller, Hopkins, and Yamamoto [2014](#), p. 2). Typically, survey participants are given a number of “tasks” where they have to make a number of choices between—usually—two set of profiles. It is generally accepted that Luce and Tukey ([1964](#)) started the conjoint design (Green and Srinivasan [1978](#); Franchino and Zucchini [2015](#)).

This methodology has been widely used in marketing research to measure “consumer trade-offs among multi-attributed products and services” (Lenk et al. [1996](#), p. 174). Typically, researchers in that field would assign arbitrary utilities to investigate “how much difference each attribute could make in the total utility of a product” (Orme [2010](#), p. 79). Utilities were assigned according to general expectations, for instance, a “respondent generally prefers higher gas mileage to lower gas mileage” (Green and Srinivasan [1978](#), p. 107). At the time this seemed particularly interesting given the impossibility to truly randomize the set of attributes. Hence, the analyst needed to set the utilities associated with every attribute in advance, usually building a small number of attribute profiles or “combinations” (Lenk et al. [1996](#), p. 175). Much research was done arguing how ranked attributes or attribute ratings were better than using assigned utilities (Carmone, Green, and Jain [1978](#), p. 301). For instance, Louviere, Flynn, and Carson ([2010](#), p. 60) criticize the use of arbitrary utilities assigned to every attribute, making traditional conjoint analyses incompatible with economic

theory. Since early conjoint methods exploited the “additive measurements” of the utilities associated with the respective attribute (Luce and Tukey 1964, p. 2), some times that led to non-accounted-for nonlinearities.

A number of topics have been studied, such as preferences for health care (Ryan 2000), vaccine decision making (Seanehia et al. 2017), preferences for energy-saving measures (Poortinga et al. 2003), preferences toward different food packagings (Silayoi and Speece 2007), consumer demand for fair trade (Hainmueller, Hiscox, and Sequeira 2015), evaluations of teaching performance (Kuzmanovic et al. 2013), roommate choice (Shafranek 2019) and renter behavior (Hankinson 2018).

Hainmueller, Hopkins, and Yamamoto (2014) “introduced conjoint analysis to political science as a survey experimental method for causal inference” (Horiuchi, Markovich, and Yamamoto 2020, p. 1), particularly making conjoint designs compatible with the potential outcomes framework of causal inference (Rubin 1974). Since then, a number of important studies have been published, making a very common tool for causal inference in political science (Cuesta, Egami, and Imai 2021). Just to name a few examples in political science, conjoint designs have been used to study attitudes toward immigrants (Hainmueller and Hopkins 2015), preferences toward political candidates (Franchino and Zucchini 2015; Horiuchi, Smith, and Yamamoto 2017; Horiuchi, Smith, and Yamamoto 2020; Mares and Visconti 2020), the role of candidate sex on voter choice (Ono and Burden 2019) and the role of the information environment in partisan voting (Peterson 2017). In part, this is due to the simplicity of the main quantity of interest developed by Hainmueller, Hopkins, and Yamamoto (2014, p. 3)—the *average marginal component effect* (AMCE).³ The quantity equals the counterfactual probability where a specific characteristic would be chosen if the value of that characteristic is absent

³Due to space concerns, we are not deriving the AMCE here. The AMCE has been well explained and widely used before. See Equation 5 in Hainmueller, Hopkins, and Yamamoto (2014, p. 11). In addition to that, see Egami and Imai (2019), who have introduced another quantity of interest, the average marginal interaction effect (AMIE).

(Hainmueller, Hopkins, and Yamamoto 2014, p. 11).⁴ Since the AMCE does not rely on arbitrary utility assignments nor does resort to functional form assumptions (Hainmueller, Hopkins, and Yamamoto 2014, p. 3), it has become a very common quantity of interest in political science, specially, because it also avoids “unnecessary statistical assumptions” at the same time that improves “internal validity than the more model-dependent procedures” (Hainmueller, Hopkins, and Yamamoto 2014, pp. 2–3).⁵ Importantly, they show that “when attribute levels are randomized independently from one another, the ordinary least squares (OLS) estimates of the coefficients from the linear regression of the choice indicator on the set of dummy variables for the levels of the attributes provide unbiased and consistent estimates of the AMCEs” (Horiuchi, Smith, and Yamamoto 2017, p. 14).⁶ Others have argued that when attribute levels are randomized, the design “reduces social desirability bias by providing many potential reasons for supporting or opposing a proposed [attribute]” (Hankinson 2018, p. 7),⁷ while others offer guidance regarding the number of attributes by developing a two-stage conjoint design (Bansak et al. 2019).

III. STUDYING CLIENTELISM MULTIDIMENSIONALLY VIA CONJOINT DESIGNS

Our multidimensional approach toward the study of vote selling is novel in the quantitative literature. Quantitative contributions on vote buying, vote selling and clientelism in general, are usually unidimensional. Survey experiments have been widely used to study this phenomena. For instance, Bahamonde (2020), González-Ocantos, Jonge, et al. (2012), González-Ocantos, Kiewiet de Jonge,

⁴Importantly, Leeper, Hobolt, and Tilley (2020, p. 6) explain that arbitrary choice of reference category when computing the AMCE might introduce “highly distorted descriptive interpretations of preferences among subgroups of respondents.”

⁵Yet, some necessary assumptions need to be made. For instance, in order to make statistical inferences, the AMCE depends on (clustered) standard errors, which in turn rely on the central limit theorem. See Hainmueller, Hopkins, and Yamamoto (2014, p. 17).

⁶Hainmueller, Hopkins, and Yamamoto (2014, p. 15) show that OLS estimators have “identical” properties to the subclassification estimators, and therefore this “implies that the linear regression estimator is fully nonparametric.”

⁷However, see Horiuchi, Markovich, and Yamamoto (2020).

and Nickerson (2014), and González-Ocantos, Kiewiet de Jonge, and Nickerson (2015) use list experiments to study the effect of selling prices or specific issues related to norms and legitimacy on the probability of vote selling. While these and other studies have advanced a number of important questions in the discipline, unfortunately, they are able to study one aspect at a time, mainly, by manipulating a word, a sentence, a framing or a price. As Hainmueller, Hopkins, and Yamamoto (2014, p. 2) point out, these designs “have an important limitation for analyzing multidimensional decision making.” We fill this gap by introducing a multidimensional conjoint-based approach to studying vote selling in the United States.

Our contribution builds directly on Carlin and Singer (2011), Carlin and Moseley (2015), and Carlin (2018). Using survey data, they build a series of multidimensional indexes to measure—in Dahlian terms—attitudes towards democracy. Particularly, using the Q-Method and cluster analyses, they account for the multifaceted views towards a democracy. Considering their operationalization strategy of Dahl’s conceptualization of democracy (Table 1) but also leveraging the Hainmueller, Hopkins, and Yamamoto (2014) approach to designing conjoint experiments, we implemented a conjoint design (Table 2) aimed to studying the multidimensionality of conditions that make vote selling most likely in the United States. Particularly, we are interested in specifying which of the three democracy dimensions of Dahl (1971) ought to fail to make vote selling most likely in the United States.

Conjoint designs are suitable tools to “determine which components of the manipulation produce the observed effect” (Hainmueller, Hopkins, and Yamamoto 2014, p. 2). Following Dahl (1971), Table 1 specifies three general dimensions that should be satisfied for a country to be considered democratic (first column). Every dimension has a number of requirements (second column). Based on Carlin and Singer (2011), Carlin and Moseley (2015), and Carlin (2018), we operationalized these

Dahl's Polyarchy Dimension	Dahl's Requirements for a Democracy	Experimental Operationalization for Conjoint Design
Formulate preferences	Freedom of expression	Media can confront the government
	Alternative sources of information	Media can confront the government
	Right of political leaders to compete for support	President cannot rule without Congress
	Right to vote	Citizens can vote in the next two elections
	Freedom to form and join organizations	Citizens can associate with others and form groups
	Freedom of expression	Media can confront the government
Signify preferences	Alternative sources of information	Media can confront the Government
	Right of political leaders to compete for support	President cannot rule without Congress
	Right to vote	Citizens can vote in the next two elections
	Free and fair elections	Citizens can vote in the next two elections
	Eligibility for public office	Citizens can run for office for the next two elections
	Freedom to form and join organizations	Citizens can associate with others and form groups
Preferences are weighted equally in conduct of government	Freedom of expression	Media can confront the government
	Alternative sources of information	Media can confront the Government
	Right of political leaders to compete for support/votes	President cannot rule without Congress
	Right to vote	Citizens can vote in the next two elections
	Free and fair elections	Citizens can vote in the next two elections
	Institutions for making government policies depend on votes and other expressions of preference	Citizens can vote in the next two elections
	Eligibility for public office	Citizens can run for office for the next two elections
	Freedom to form and join organizations	Citizens can associate with others and form groups

Table 1: Dimensions of Democracy (Dahl 1971) and Their Corresponding Experimental Operationalizations.

Note: Dahl (1971) specifies three general dimensions that should be satisfied for a country to be considered democratic (first column). Every dimension has a number of requirements (second column). Based on Carlin and Singer (2011), Carlin and Moseley (2015), and Carlin (2018), we operationalized these requirements for the conjoint experiment by devising five attributes (third column). As Table 2 shows, all participants were asked to choose between hypothetical candidates that either supported or rejected each of these five attributes.

requirements for the conjoint experiment by devising five attributes (third column): (1) *media can confront the government*, (2) *president cannot rule without congress*, (3) *citizens can vote in the next two elections*, (4) *citizens can run for office for the next two elections* and (5) *citizens can associate with others and form groups*.

Given that conjoint designs are able “to identify the causal effects of various components of a

In the next section you will see 10 different candidates presented in pairs. Each candidate supports different policies. Some candidates might or might not share some similarities/differences. You might not like any of them, but we want to know which candidate represents the lesser of the two evils for you. You might want to focus your attention on the issues that you care about the most.

Candidate 1	Candidate 2
Media CAN confront the government	Media CANNOT confront the government
President CANNOT rule without Congress	President CAN rule without Congress
Citizens CANNOT vote in the next two elections	Citizens CANNOT vote in the next two elections
Citizens CAN run for office for the next two elections	Citizens CAN run for office for the next two elections
Citizens CAN associate with others and form groups	Citizens CANNOT associate with others and form groups

Which of these candidates represents the lesser of the two evils for you?

Candidate 1 <input type="checkbox"/>	Candidate 2 <input type="checkbox"/>
--------------------------------------	--------------------------------------

Table 2: A Multidimensional Approach to Studying Clientelism: A Conjoint Design (example).

*Note: Participants were asked to choose between two hypothetical candidates (**Candidate 1** and **Candidate 2**). Every entry was filled at random according to the five different attributes explained in [Table 1](#). In practice, every subject chose between two unique hypothetical candidates. Note that in order to highlight the differences between the two candidates, the can and cannot were capitalized. The idea was to minimize experimental fatigue.*

treatment in survey experiments” (Hainmueller, Hopkins, and Yamamoto [2014](#), p. 2), we claim that this is an appropriate tool to shed some light on the multi-causal study of clientelism. To study which democratic dimension(s) should fail to produce clientelism, we presented subjects (as in [Table 2](#)) two hypothetical candidates that supported (or not) every policy (attribute)—as operationalized in [Table 1](#). We recognize that the resulting candidate profiles are highly unlikely. Unlikely profiles (such as doctors with no education) have been a big concern in the conjoint literature. So far the suggestion has been to delete them before hand by restricting randomization

of certain unlikely profiles (Hainmueller, Hopkins, and Yamamoto 2014) or by marginalizing “factors over the target population distribution” via the population AMCE (Cuesta, Egami, and Imai 2021, p. 12). While acknowledging the advantages of both approaches, our goal is identifying a set of democratic attributes that, when absent, make clientelism more likely. In fact, external validity seems to be the trade-off when building a case study according to the *least*-likely case design (Levy 2008). In addition, one of the methodological contributions of this paper is to overcome selection bias by studying *hypothetical* behaviors, specially the ones where the outcome of interest has not been produced (Geddes 1990). And finally, there are several survey experiments that have fielded hypothetical questions, mostly putting respondents in experimental conditions that do not necessarily mimic reality. For instance, Bahamonde (2020) finds that a big portion of U.S. voters would be willing to sell their vote to an hypothetical candidate in exchange for money, while Ballard-Rosa, Martin, and Scheve (2017) examine a number of tax proposals “that are infeasible in the real world politics” (Cuesta, Egami, and Imai 2021, p. 5).

Table 2 shows one possible realization of the experiment. It is important to note that every attribute was randomly assigned, and consequently, every participant in practice chose between two unique hypothetical candidates. Also, in order to minimize experimental fatigue, the CAN and CANNOT were capitalized.

The study considered a *direct question* about the intention to sell the vote. As a whole, the conjoint experiment was framed as a study about crime in the United States, not as a study about clientelism. Participants were asked to read *an excerpt* mentioning a number of crimes. All were formatted as news pieces. The idea was to explain “vote selling” to “newsreaders.” To further prevent bias, the direct question stated that there was the hypothetical possibility of doing one of the illegal things mentioned in the excerpt. And that this possibility would be randomly assigned.

However, all participants were directly asked whether they would be interested in selling their vote. Following Bahamonde (2020), to capture the willingness to sell without the potential costs, participants were asked whether they would be willing to accept the offer, assuming they would not go to jail. After answering the conjoint portion of the study, participants were asked to answer a battery of socio-demographic and political questions.

Ultimately, our design will allow a series of hypotheses tests between every of the five democracy attributes and the vote selling question. Typical conjoint analyses offer descriptive associations between hypothetical attributes. While these analyses have advanced a number of important research avenues, they do not permit statistical associations between the selected profiles and the respondents attitudes or preferences. By introducing support vector machine techniques to analyzing conjoint experiments, we are able to do so. As we explain later, this approach improves our causal inferences by permitting statistical correlations between the selected conjoint profiles (democracy) and the respondents attitudes (vote selling). We also are able to control for other observables (the socio-demographic battery). Before presenting the machine learning approach, we first present our novel dataset and analyzes it first by using the traditional AMCE-based conjoint approach.

I. Classic Conjoint Data Approach

Collected in 2016, the data ($N=1,108$) are representative at the national level.⁸ Figure 1 shows the geographical distribution of survey respondents grouped by party identification. Following Hainmueller, Hopkins, and Yamamoto (2014), we computed the AMCE via the OLS estimator using clustered standard errors. In this section we present a classic conjoint analysis. Particularly, we

⁸1,108 respondents, everyone answering 5 tasks with 2 candidates each. *Research Now SSI* collected the data between March 2 and March 6 2016. Survey respondents belong to the online panel owned and administered by SSI. Notice of IRB exemption Protocol #E16-292 is kept on file at the Office of Research and Regulatory Affairs of [REDACTED] University.

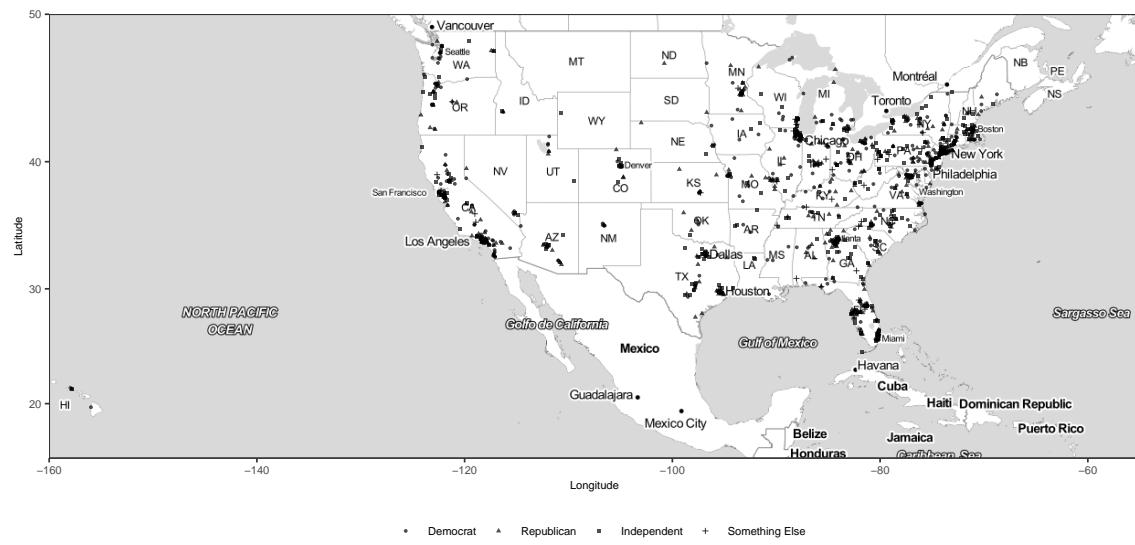


Figure 1: Geographical Distribution of Survey Respondents by Party Identification.

Note: The data ($N=1,108$) were collected by Research Now SSI between March 2 and March 6 2016 and are representative at the national level. Survey respondents belong to the online panel owned and administered by SSI.

show the hypothetical candidates' attributes that were selected by survey respondents.

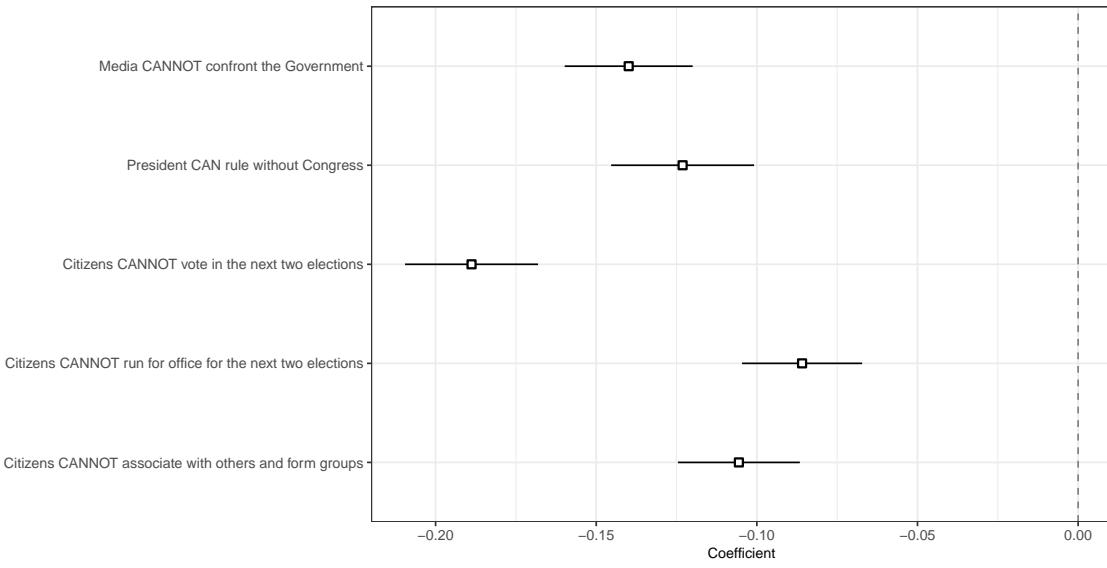


Figure 2: Classic AMCE Analysis: Candidate Selection and Dahl's Democratic Dimensions.

Note: Following Hainmueller, Hopkins, and Yamamoto (2014), the figure shows the corresponding AMCEs for every of the attributes explained in Table 1. All attributes are based on Dahl (1971). All reference categories were omitted—all of them are at the 0 vertical line and represent the opposite of the attribute shown in the plot. For substantive reasons, all categories displayed in the figure represent the non-democratic side of the attributes. The figure strongly suggests that respondents systematically preferred hypothetical candidates who supported democratic policies.

Figure 2 suggests that respondents systematically preferred hypothetical candidates who supported democratic policies. Authoritarian candidates that can rule without Congress, or political systems in which there is controlled mass media, or where citizens are not allowed to vote, run for office or associate with others, are systematically rejected by the nationally represented pool of respondents. These analyses are not surprising as they conform with our theoretical priors, i.e. the United States has (traditionally) been considered a strong democracy.

While classic conjoint analyses provide consistent causal estimates, they unfortunately overlook respondent's preferences. As Figure 2 suggests, analysts can *describe* aggregate behaviors but cannot establish relationships between the respondent's preferences and her attribute choices. In other words, analysts are not able to tell a story of *why* respondents choose what they chose. By

implementing support vector machine methods we are able to introduce additional covariates—such as respondent socio-economic and political preferences—and explore the reasons that make vote selling more likely in the United States. Why we still believe description is “good science” (Gerring 2012), the descriptive nature of the classic conjoint design (for instance, via the AMCE) might (wrongly) suggest that democratic values scored high in the United States. After all, Figure 2 strongly indicates that non-democratic candidate attributes are systematically rejected (i.e. all coefficients are negative). And as a consequence, that might imply that the intention to sell the vote should be low. The ability to introduce covariates, such as the intention to sell the vote, might shed some light about how healthy or broken democratic values were at the time were the data were collected (which coincides with the campaign period that gave Donald Trump the U.S. presidency). We claim this is an exceptional opportunity to study democracy, and particularly, the democratic attitudes that, when broken, explain vote selling. Next section introduces support vector machine methods that allow analyzing conjoint data considering subject preferences and/or attitudes.

IV. INTRODUCING SUPPORT VECTOR MACHINES METHODS TO ANALYZING CONJOINT DATA

Support vector machine methods are a subclass of machine learning techniques, which in turn are a branch of the artificial intelligence field. The main goal of these methods is to use computational algorithms to learn from data (Miyamoto et al. 2015). For instance, in agricultural sciences machine learning methods are used to identify between crops and grass using satellite images as data. There are mainly three branches of machine learning techniques: supervised learning, unsupervised learning and reinforcement learning. Based on a subset of the complete dataset (the “training” dataset), the goal of supervised learning is to identify a mathematical function able to map a vector (“input”)

to another vector (“output”) based on input-output data pairs which are used as examples.⁹ For instance, the analyst can define how crops and grass look like (output). Once the mathematical function is defined, supervised learning techniques should be able to automatically recognize newly presented images of crops and grass (input) and classify them (map) into their respective categories (output)—i.e. “grass” or “crops.” Unsupervised learning is similar to supervised learning, but the training dataset have only the input component. Thus, the computational algorithms in this case should be able to find patterns in the dataset without having prior information. Finally, reinforcement learning was inspired by behavioral psychology, and its main goal is to determine what actions an algorithm should perform in order to maximize some notion of “accumulated reward.”

Support vector machines (SVMs) are a set of techniques of supervised learning which might be extended to classification applications and/or standard regression techniques (Maimon and Rokach 2005, Ch. 12). Supervised learning techniques have been used in political science before (D’Orazio et al. 2014; Kevin Greene, Baekkwan Park, and Colaresi 2019; Cantú and Saiegh 2011). In this paper we are interested in applying SVM methods to—first—solve a classification problem, and then in a second stage, apply OLS methods. The first stage is a data-driven process which consists of defining a full set of democratic attitudes, and then finding a classification rule (i.e. a mathematical function) that takes survey participants and classify them along the constructed democratic attitudes spectrum. In particular, we are interested in learning about the political preferences of survey participants by using the information participants provided when making choices between the two hypothetical candidates and the policies those candidates endorsed (as exemplified in Table 2). Since the focus is to account for the multi-dimensionality of democracy, we perform these analyses for all five democracy attributes. In the second stage we use standard regression methods to study correlations between the five democracy attributes (obtained in the first stage) and respondent’s

⁹A further demonstration is in the Appendix.

socio-political attitudes, particularly, the willingness to sell the vote. In sum, we exploit the conjoint dataset to first classify all participants within a common space for all five attributes. Then we fit five separate OLS regression models between those five spaces and the willingness to sell the vote (controlling for relevant covariates).

Stating the problem Consider the following classification task: estimate a function $f : \mathbb{R}^J \rightarrow \{-1, 1\}$ using a system of input-output training data pairs. The f function should be able to map every participant attitudes toward the five democracy attributes and classify those attitudes within a common numeric space.

As exemplified in [Table 2](#), all hypothetical political candidates took stances in favor (or against) the five democracy policies. For an input $\mathbf{x} \in \{0, 1\}^5$, which represents the range of possible political stances of a candidate, the function f maps survey participants with all democracy attributes, linking both in what could be described as the “decision” made by the survey participant. For instance, hypothetical candidates 1 and 2 in [Table 2](#) are represented by vectors $(1, 1, 0, 1, 1)$ and $(0, 0, 0, 1, 0)$, respectively. Then, the function f connects the respondent’s attribute choices (embodied by hypothetical candidates 1 or 2) with the respondent herself (i.e. her “decision”).

Constructing the vector weight \mathbf{w}^i One way to motivate our approach is via the latent variable and the standard maximization utility approaches. For simplicity, we assume that participant preferences can be modeled by an additive linear utility function, and that the decision function f takes a utility sign $\{+, -\}$ depending on the attribute chosen—more details are provided below. Note that coding sign is completely irrelevant to the substantive policies endorsed by the candidates. If the data can be partitioned by an hyperplane (which is the case in most randomized conjoint designs), such functions can be defined for every participant i .

Survey participants evaluate between two hypothetical candidates and choose one. They do so for all five tasks. To clarify, for a survey participant i , we have the following conjoint data,

$$(\mathbf{x}_1^{1,i}, \mathbf{x}_1^{2,i}; y_1^i), (\mathbf{x}_2^{1,i}, \mathbf{x}_2^{2,i}; y_2^i), \dots (\mathbf{x}_5^{1,i}, \mathbf{x}_5^{2,i}; y_5^i) \quad (1)$$

where $\mathbf{x}_k^{1,i}$ represents the attributes of candidate 1 shown to citizen i during task k . Similarly, $\mathbf{x}_k^{2,i}$ represents the attributes of candidate 2 shown to citizen i during task k . The corresponding y_k is the selected candidate. We coded $y_k^i = 1$ when survey participant i selected candidate 1, and $y_k^i = -1$ when survey participant i selected candidate 2. Since we are trying to characterize a linear function for every i , survey participants can be mapped by a vector weight $\mathbf{w}^i \in \mathbb{R}^5$ and an intercept b^i . Indeed, for a candidate \mathbf{x} , the function,

$$u_i(\mathbf{x}) = \mathbf{w}^i \cdot \mathbf{x} + b, \quad (2)$$

models the utility function of every survey participant i . For consistency, we identify weights $\mathbf{w}^i \in \mathbb{R}^5$ and intercept $b \in \mathbb{R}$ such that,

$$\mathbf{w}^i \cdot (\mathbf{x}_k^{1,i} - \mathbf{x}_k^{2,i}) > 0 \Leftrightarrow y_k^i = 1, \quad (3)$$

and

$$\mathbf{w}^i \cdot (\mathbf{x}_k^{1,i} - \mathbf{x}_k^{2,i}) < 0 \Leftrightarrow y_k^i = -1, \quad (4)$$

implying that whenever two candidates $\mathbf{x}_1^{1,i}$ and $\mathbf{x}_1^{2,i}$ are presented, the survey respondent will choose the one that provides him with a larger utility.

Optimization strategy Note that, within this framework, it is sufficient to consider the differences between the democracy attributes among the two hypothetical candidates. In fact, the selected hypothetical candidate and their corresponding policy stands are observed quantities. Therefore, and from a theoretical standpoint, unlike u_i which cannot be directly constructed, \mathbf{w}^i is the only observed quantity of interest which is accessed within the space of differences between candidates.

We define then the centered coordinates $\mathbf{z}_k^i \in \{-1, 0, 1\}$ as,

$$\mathbf{z}_k^i = \mathbf{x}_k^{1,i} - \mathbf{x}_k^{2,i}, \quad (5)$$

hence, from now on, the intercept b will be ignored because a function of the type $f_i(\mathbf{z}_k) = \text{sign}(\mathbf{w}_i \cdot \mathbf{z}_k^i)$ is mathematically sufficient. Indeed, equations [Equation 3](#) and [Equation 4](#) become,

$$y_k^i (\mathbf{w}^i \cdot \mathbf{z}_k^i) > 0. \quad (6)$$

It should be clear by now that the challenge is to identify weights \mathbf{w}^i for every survey participant i . Under the data separability assumption, it has been shown by Vapnik and Chervonenkis ([1991](#)) that it suffices to focus on the margin, defined as the minimal distance of a sample to the decision surface. For notation simplicity, the dependence of y^i and \mathbf{z}_k^i on i will be dropped. By rescaling \mathbf{w}_i we know that the closest points to the hyperplane must satisfy,

$$|\mathbf{w}^i \cdot \mathbf{z}_k| = 1, \quad (7)$$

and if two observations \mathbf{z}_k and \mathbf{z}_m belong to different classes (i.e. the selected candidate), then the margin is defined as the distance of these two points to the hyperplane such that,

$$\frac{\mathbf{w}^i}{\|\mathbf{w}^i\|} \cdot (\mathbf{z}_k - \mathbf{z}_m) = \frac{2}{\|\mathbf{w}^i\|}. \quad (8)$$

It should be clear by now that for each survey participant i , the optimal hyperplane is the solution to the following optimization problem,

$$\begin{aligned} & \min_{\mathbf{w}^i} \frac{1}{2} \|\mathbf{w}^i\|^2 \\ & \text{subject to } y_k (\mathbf{w}^i \cdot \mathbf{z}_k) \geq 1, \quad k = 1, 2, 3, 4, 5. \end{aligned} \quad (9)$$

Regarding the actual dataset analyzed in this study, it is unknown if these data can *a priori* be separated by an hyperplane. To allow for bad classification issues, Cortes and Vapnik (1995) introduced the concept of “slack variables” ξ_i that relax the optimization problem restrictions (Equation 9):

$$y_k (\mathbf{w}^i \cdot \mathbf{z}_k) \geq 1 - \xi_k, \quad \xi_k \geq 0, \quad k = 1, 2, 3, 4, 5. \quad (10)$$

All in all, this allows controlling for both the classification strength of $\|\mathbf{w}^i\|$ (or the capacity of the algorithm to correctly classify the data), and the sum of the slack variables $\sum_{k=1}^5 \xi_k$ which account for possible errors in classification. Since we allow the learning algorithm some degree of deviations during the classification process, we need to account for this error in the optimization problem. By doing so, we will find a new \mathbf{w} that might not be the unique solution to Equation 9—that in the non-linearly separable case does not exist—but still is good enough in the sense that the number of training data-pairs misclassified is small. By doing so the following trade-off problem is encountered: finding a \mathbf{w} with small norm that classifies properly a large proportion of the data. A widely used solution to that trade-off is the C -SVM or “soft margin classifying,” which is based in

the minimization of the following objective function,

$$\min_{\mathbf{w}^i, \xi} \frac{1}{2} \|\mathbf{w}^i\|^2 + C \sum_{k=1}^5 \xi_k, \quad (11)$$

where the regularization constant $C > 0$ determines the trade-off between the empirical error and the complexity term.

We solved the optimization problem with the Python library “*sklearn*” (see Appendix section).¹⁰

I. Analyzing The Conjoint Data via Support Vector Machines

The SVM approach has been used to solve a classification problem. Via a data-driven process described in the previous section, five \mathbf{w}_i variables were constructed, one per democracy attribute (as shown in third column of [Table 1](#)). These five vectors conform five different dependent variables.

In this section we estimate five OLS multivariate lineal models (see [Table A2](#)), where the covariate of interest is the declared willingness to sell the vote (“`sell vote`”). Distributions of the five dependent variables are plotted in [Figure 3](#). Given that they represent different attributes of the same concept—i.e., “support for democracy”—it is not a surprise that these distributions look very similar. From a substantive standpoint, the interest is on finding statistically significant predictors (if any) across all/some (or none of) the five democracy attributes. Following the literature on clientelism,¹¹ the following covariates were included: `woman`, `party id.`, `ideology`, `education`, `political knowledge`, `registered to vote`, `trust in Federal Gov.`, `income` and `sell vote`

¹⁰An *R* routine is also available upon request. This algorithm solves the problem directly using the steepest descent approach. Due to computational complexity and possible convergence issues, this routine is not recommendable when the number of training-data pairs is large. However, since the optimization problem is convex, algorithms converge to a unique solution but the convergence time might be long.

¹¹See particularly Auyero (2000), Kitschelt (2000), Brusco, Nazareno, and Stokes (2004), Calvo and Murillo (2004), Stokes (2005), Nazareno, Brusco, and Stokes (2008), Nichter (2008), González-Ocantos, Jonge, et al. (2012), Weitz-Shapiro (2012), Szwarcberg (2013), Vicente (2014), González-Ocantos, Kiewiet de Jonge, and Nickerson (2014), Rueda (2015), Zarazaga (2014), González-Ocantos, Kiewiet de Jonge, and Nickerson (2015), Schaffer and Baker (2015), Holland and Palmer-Rubin (2015), Zarazaga (2016), Rueda (2017), Oliveros (2016), Bahamonde (2018), Bahamonde (2020), and Murillo, Oliveros, and Zarazaga (2021).

(which captures the willingness to sell the vote).¹²

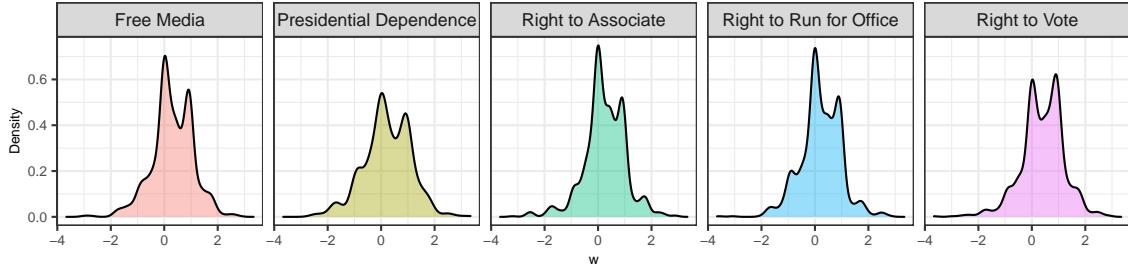


Figure 3: Support Vector Machine Analyses and the Constructed w_i : Five Democracy Attributes.

Note: Leveraging a data-driven process (particularly, support vector machines) and following Dahl (1971), five individual-level scores of support-for-democracy w_i were constructed. Each w_i distribution represents a specific democracy dimension which will be used as a dependent variable to study possible correlations with the willingness to sell the vote. Five different multivariate OLS models were estimated having these distributions as dependent variables—they are shown in Table A2 in the Appendix section and summarized in Figure 4.

Figure 4 shows the point estimates and their estimated uncertainty for all five models. Most importantly, the figure shows that the willingness to sell the vote is negatively correlated with the democracy attribute that speaks to the idea that the President of the United States should not govern without a Congress (“Presidential Dependence”). We believe that individuals who are willing to sell their freedom to vote for money, value strong leaderships that can govern in an unaccounted-for way, that is, without the participation of the American Congress. The other dimensions are not correlated with the willingness to sell.

¹²Table A1 shows summary statistics.

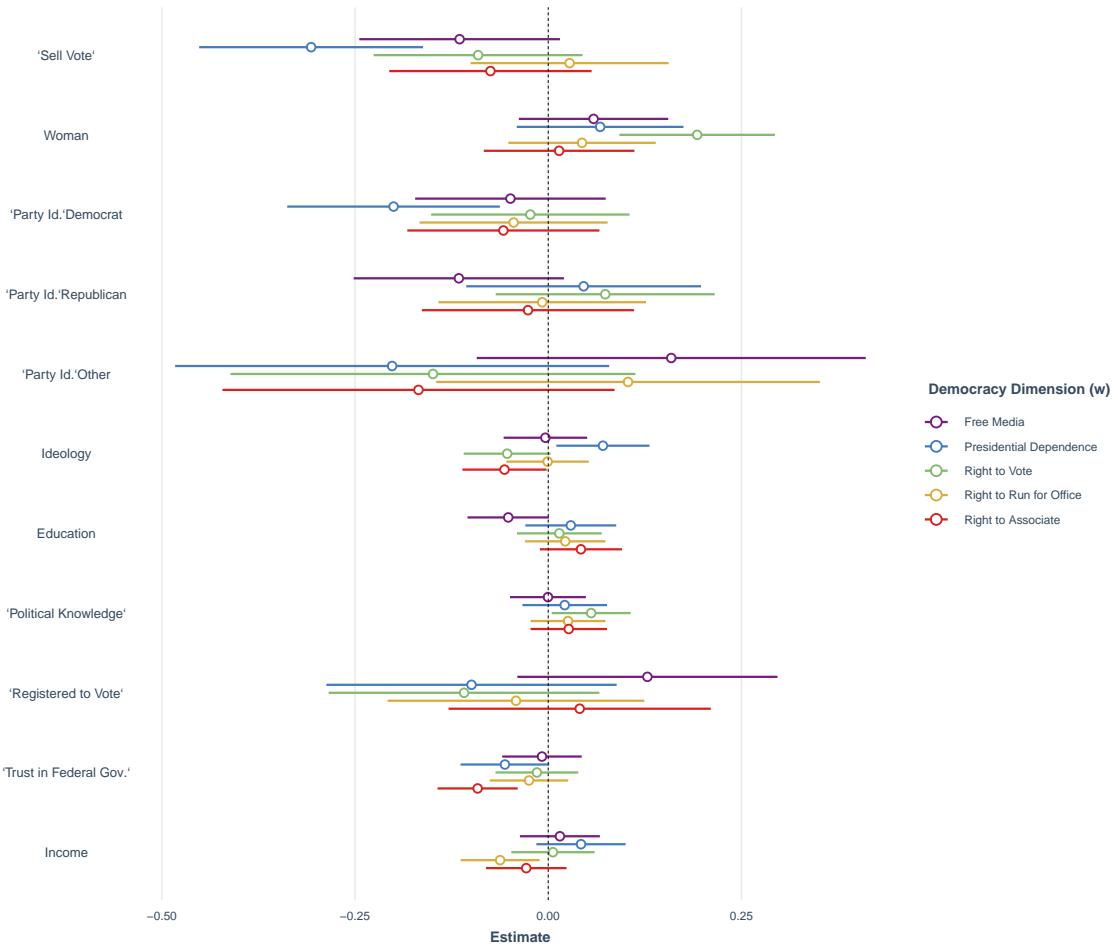


Figure 4: Multivariate Analysis: A Multidimensional Approach to Study Vote Selling and Support for Democracy.

Note: The figure shows correlations between the willingness to sell the vote (*Sell Vote*) across all five democracy dimensions (w_i). All dimensions were constructed following Dahl (1971), operationalized in Table 1 and collected in a conjoint experiment in the United States during 2016 (see Table 2). In this figure, all estimates are OLS (see regression table in Table A2). From a substantive standpoint, the figure shows that from the five democracy attributes, the only one that ought to fail is the belief that the President of the United States may not rule without a Congress (“Presidential Dependence”). In other words, vote sellers systematically believe that the President can govern on his/her own. Importantly, the other democracy attributes are not relevant to explain vote selling.

We believe this finding is relevant. The clientelism literature usually frames the act of selling (or buying) votes as “a democracy” failure. Unfortunately, as we have argued, that explanation, while

important, it is too general. This paper explains why it is important to approach democracy from a multidimensional perspective—using the framework provided by Dahl (1971). The immediate consequence is to identify the particular democracy dimension associated with clientelism, particularly, vote selling. While this paper identifies such dimension, it leaves for future research theorizing about why that particular dimension is associated with vote selling. However, we encourage researchers, particularly political and social psychologists, to investigate the relationship between authoritarian personalities with strong traits of obedience to authority (who would likely let a president govern without a congress) and vote selling.

V. CONCLUSION

Exploiting a novel conjoint dataset representative at the national level and machine learning methods, this paper finds that U.S. voters who score low on the belief that the President should govern with a Congress are more likely to sell the vote. Following the multidimensional approach to study democracy proposed by Dahl (1971), the other dimensions are not relevant to explain vote selling. We believe these findings are relevant to the clientelism literature since most explanations of vote selling see the phenomena as a general democratic failure.

In addition to that, these findings go in line with recent studies pointing out to a possible erosion of American democracy—see Bahamonde (2020), but particularly Levitsky and Ziblatt (2018). The data analyzed in this paper were collected while the political campaign that led Donald Trump to the U.S. Presidency took place. From that perspective, survey respondents not only participated in this study, but also (some of them) voted for Donald Trump. This is particularly important given the events occurred during January 2021 where Trump incited the storming of the U.S. Capitol. Following the lead of Levitsky and Ziblatt (2018), we encourage comparative politics scholars to

incorporate the United States (as case) to the pool of cases usually studied while studying clientelism, which are mostly developing countries.

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.....Word count: 6,533

VI. APPENDIX

I. Experimental Manipulations and Vignettes

Distractor Paragraph. The next paragraph was used to distract subjects from the main purpose of the study, and also to define vote selling. After reading the excerpt, participants were told about the hypothetical possibility of doing one of the illegal things mentioned in the excerpt. And that this possibility would be randomly assigned. However, all participants were directly asked whether they would be interested in selling their vote (as seen on **Direct Question**).

Washington , D.C.— A department store downtown had a robbery incident last week , reporting several missing iPods from their inventory . Authorities also inform that a group of local residents are trying to `` sell '' their votes to political candidates ahead of a local election for city council . Residents approached some of the candidates running for office and offered to vote for that candidate in return for monetary compensation . In a different subject matter , the local police station released a report on driving habits and behaviors in the Capitol district last week . Finally , cyber—crime has become an increasingly serious issue in the area in the past few year .

Direct Question. All subjects read the next paragraph, and then *all* answered the direct question:

Now you will be entered into a random lottery for the opportunity to do ONE of the illegal things you just read before. This means that you might be randomly offered to hypothetically do ANY of the activities mentioned before.

After a random assignment , you have been selected for the opportunity to hypothetically sell your vote. This means that you will have the hypothetical opportunity to accept money from a candidate for your vote. Would you be willing to accept the offer , assuming you would not go to jail? By selecting ``Yes, '' you could earn up to \$ 1,000.

II. Summary Statistics

Table A1

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Right to Run for Office	1,005	0.212	0.745	-3.575	-0.133	0.759	2.801
Right to Associate	1,005	0.247	0.768	-3.194	-0.064	0.793	2.890
Free Media	1,005	0.308	0.759	-3.018	-0.009	0.905	2.795
Presidential Dependence	1,005	0.228	0.870	-2.609	-0.302	0.926	3.344
Right to Vote	1,005	0.399	0.795	-3.664	0.000	0.944	2.863
Woman	1,005	1.567	0.496	1	1	2	2
Ideology	1,005	2.940	1.107	1	2	4	5
Registered to Vote	1,005	1.093	0.290	1	1	1	2
Trust in Federal Gov.	1,005	2.612	1.125	1	2	3	5
Income	1,005	7.021	3.805	1	4	10	14
Education	1,005	4.811	1.707	1	4	6	7
Political Knowledge	1,005	0.663	0.372	0.000	0.402	0.871	2.432
Sell Vote	1,005	0.161	0.368	0	0	0	1

III. Algorithm

```
1 # svm.py
2 import numpy as np # for handling multi-dimensional array operation
3 import pandas as pd # for reading data from csv
4 from sklearn.svm import LinearSVC # for classification problem
5 from sklearn.pipeline import make_pipeline # create pipeline
6 from sklearn.preprocessing import StandardScaler # scaling data
7
8 # but following work good enough
9 reg_strength = 10000 # regularization strength
10 learning_rate = 0.000001
11 #init()
12 data = pd.read_csv('./dataMergedConjoint.csv')
13 # SVM only accepts numerical values.
14 # Therefore, we will transform the categories into
15 # values 1 and 0.
16
17 # at.run
18 cancannot_map = {'Citizens CANNOT run for office for the next two elections':0, '
19     'Citizens CAN run for office for the next two elections':1}
20 data['at.run'] = data['at.run'].map(cancannot_map)
21 # at.asso
22 cancannot_map = {'Citizens CANNOT associate with others and form groups':0, '
23     'Citizens CAN associate with others and form groups':1}
24 data['at.asso'] = data['at.asso'].map(cancannot_map)
25 # at.press
26 cancannot_map = {'Media CANNOT confront the Government':0, 'Media CAN confront the
27     Government':1}
```

```

25 data['at.press'] = data['at.press'].map(cancannot_map)
26 # at.presaut
27 cancannot_map = {'President CANNOT rule without Congress':1, 'President CAN rule
28 without Congress':0}
29 data['at.presaut'] = data['at.presaut'].map(cancannot_map)
30 # at.vote
31 cancannot_map = {'Citizens CANNOT vote in the next two elections':0, 'Citizens CAN
32 vote in the next two elections':1}
33 data['at.vote'] = data['at.vote'].map(cancannot_map)
34
35 # drop last column (extra column added by pd)
36 # and unnecessary first column (id)
37 # data.drop(data.columns[[-1, 0]], axis=1, inplace=True)
38 # put features & outputs in different DataFrames for convenience
39 Y = data.loc[:, 'selected'] # all rows of 'diagnosis'
40 X_c1 = data.iloc[range(0,11080,2),[5,6,7,8,9]] # all feature rows candidate 1
41 X_c2 = data.iloc[range(1,11080,2),[5,6,7,8,9]] # all feature rows candidate 1
42 X = X_c1.values-X_c2.values
43 X = pd.DataFrame(X)
44 Y_c1 = Y.iloc[range(0,11080,2)] # all feature rows candidate 1
45 Y_c2 = Y.iloc[range(1,11080,2)] # all feature rows candidate 1
46 Y = Y_c1.values-Y_c2.values
47 Y = pd.DataFrame(Y)
48 W = pd.DataFrame(data=None,columns=['k','w.at.run','w.at.asso','w.at.press','w.at.
49 presaut','w.at.vote','selected','at.run','at.asso','at.press','at.presaut','at.
vote'])
50
51 print("training started...")
52 for i in list(range(int(len(Y)/5))):
```

```

50     print(i)
51
52     X_train = X.iloc[5*i:5*(i+1),:]
53
54     #X_train = [X_train.iloc[0,:],X_train.iloc[1,:],X_train.iloc[2,:],X_train.iloc
55
56     [3,:],X_train.iloc[4,:]]
57
58     y_train = Y.iloc[5*i:5*(i+1)]
59
60     if (-1 in np.array(y_train)) and (1 in np.array(y_train)):
61
62         #clf = make_pipeline(StandardScaler(),LinearSVC(random_state=0, tol=1e-5,
63
64             fit_intercept=False))
65
66         clf = LinearSVC(random_state=0, tol=1e-5, fit_intercept=False, C = 10, max_
67
68             iter = 2000)
69
70         clf.fit(X_train, y_train.values.ravel())
71
72         #print(clf.decision_function(np.eye(5)))
73
74         w=list(clf.decision_function(np.eye(5)))
75
76
77         w = [i+1]+w
78
79         w = pd.DataFrame({ 'k':[w[0],w[0],w[0],w[0],w[0]] ,
80
81             'w.at.run':[w[1],w[1],w[1],w[1],w[1]] ,
82
83             'w.at.asso':[w[2],w[2],w[2],w[2],w[2]] ,
84
85             'w.at.press':[w[3],w[3],w[3],w[3],w[3]] ,
86
87             'w.at.presaut':[w[4],w[4],w[4],w[4],w[4]] ,
88
89             'w.at.vote':[w[5],w[5],w[5],w[5],w[5]] })
90
91
92         #aux=pd.DataFrame(np.ones((5,1))*w)
93
94         w['selected']=y_train.values
95
96         w['at.run']=X_train[0].values
97
98         w['at.asso']=X_train[1].values
99
100        w['at.press']=X_train[2].values
101
102        w['at.presaut']=X_train[3].values
103
104        w['at.vote']=X_train[4].values
105
106        W = pd.concat([W,w])

```

76

77 pd.DataFrame(W).to_excel(r'./File_Name.xlsx', index = False)

svm_script.py

IV. Regression Table: OLS Analyses using the SVM Approach to Analyzing Conjoint Data

	Free Media	Presidential Dependence	Right to Vote	Right to Run for Office	Right to Associate
‘Sell Vote’	-0.11 (0.07)	-0.31*** (0.07)	-0.09 (0.07)	0.03 (0.07)	-0.07 (0.07)
Woman	0.06 (0.05)	0.07 (0.05)	0.19*** (0.05)	0.04 (0.05)	0.01 (0.05)
‘Party Id.’ Democrat	-0.05 (0.06)	-0.20** (0.07)	-0.02 (0.07)	-0.04 (0.06)	-0.06 (0.06)
‘Party Id.’ Republican	-0.12 (0.07)	0.05 (0.08)	0.07 (0.07)	-0.01 (0.07)	-0.03 (0.07)
‘Party Id.’ Other	0.16 (0.13)	-0.20 (0.14)	-0.15 (0.13)	0.10 (0.13)	-0.17 (0.13)
Ideology	-0.00 (0.02)	0.06* (0.03)	-0.05 (0.03)	-0.00 (0.02)	-0.05* (0.03)
Education	-0.03 (0.02)	0.02 (0.02)	0.01 (0.02)	0.01 (0.02)	0.02 (0.02)
‘Political Knowledge’	-0.00 (0.07)	0.06 (0.07)	0.15* (0.07)	0.07 (0.07)	0.07 (0.07)
‘Registered to Vote’	0.13 (0.09)	-0.10 (0.10)	-0.11 (0.09)	-0.04 (0.08)	0.04 (0.09)
‘Trust in Federal Gov.’	-0.01 (0.02)	-0.05 (0.03)	-0.01 (0.02)	-0.02 (0.02)	-0.08*** (0.02)
Income	0.00 (0.01)	0.01 (0.01)	0.00 (0.01)	-0.02* (0.01)	-0.01 (0.01)
R ²	0.02	0.07	0.03	0.01	0.03
Adj. R ²	0.01	0.06	0.02	0.00	0.02
Num. obs.	1005	1005	1005	1005	1005

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$. Every column represents each of Dahl (1971) democracy dimensions. All models OLS. Intercept omitted.

Table A2: Statistical models

V. Data-pairs

Consider the following set of training data-pairs $\{(\mathbf{z}_k, y_k)\}_{k=1}^n$ with $\mathbf{z}_k \in \mathbb{R}^2$ and $y_k \in \{-1, 1\}$. We look for an hyperplane going through the origin such that,

$$\mathbf{w} \cdot \mathbf{z}_k > 0 \Leftrightarrow y_k = 1 \quad \text{and} \quad \mathbf{w} \cdot \mathbf{z}_k < 0 \Leftrightarrow y_k = -1, \quad (12)$$

in the case of $z_k \in \mathbb{R}^2$ we have that such hyperplane is simply a straight line with a 0 intercept,

$$L(\mathbf{z}) := L((z_1, z_2)) = w_1 z_1 + w_2 z_2. \quad (13)$$

Now, if $z_k = (z_{k,1}, z_{k,2})$, then we look for \mathbf{w} such that,

$$y_k (w_1 z_{k,1} + w_2 z_{k,2}) > 0, \quad \forall k = 1, \dots, n. \quad (14)$$

Since $n \in \mathbb{N}$ is fixed, there must be some $\delta = \delta(\mathbf{w})$ such that,

$$\delta = \min_{k=1, \dots, n} y_k (w_1 z_{k,1} + w_2 z_{k,2}) > 0, \quad (15)$$

therefore, by redefining $\mathbf{w} = \mathbf{w}/\delta$, equation [Equation 14](#) cab be rewritten as,

$$y_k (w_1 z_{k,1} + w_2 z_{k,2}) > 1 \quad (16)$$

moreover, the points \mathbf{z}_k closest to $L(\mathbf{z})$ can be defined such that,

$$y_k (w_1 z_{k,1} + w_2 z_{k,2}) = 1. \quad (17)$$

Recall that the distance between a point $\mathbf{z} = (z_1, z_2) \in \mathbb{R}^2$ to the line L is given by,

$$\text{distance}(\mathbf{z}, L) = \frac{|w_1 z_1 + w_2 z_2|}{\sqrt{w_1^2 + w_2^2}}. \quad (18)$$

Let $(z_{k,1}, z_{k,2})$ a training data-pair belonging to the class labeled $+1$, and $(z_{m,1}, z_{m,2})$ a training data-pair belonging to the class labeled -1 . Assume furthermore that $(z_{k,1}, z_{k,2})$ is one of the points in the $+1$ class that is closest to the optimal hyperplane. Similarly, assume that $(z_{m,1}, z_{m,2})$ is one of the points in the -1 class that is closest to the optimal hyperplane. The margin is defined as the sum of the distances between $(z_{k,1}, z_{k,2})$ and $(z_{m,1}, z_{m,2})$ to the optimal hyperplane. That is,

$$\text{margin} = \frac{|w_1 z_{k,1} + w_2 z_{k,2}|}{\sqrt{w_1^2 + w_2^2}} + \frac{|w_1 z_{m,1} + w_2 z_{m,2}|}{\sqrt{w_1^2 + w_2^2}} \quad (19)$$

since $y_k = 1$ then we have that,

$$y_k (w_1 z_{k,1} + w_2 z_{k,2}) = w_1 z_{k,1} + w_2 z_{k,2} = 1 > 0 \quad (20)$$

and

$$y_m (w_1 z_{m,1} + w_2 z_{m,2}) = -(w_1 z_{m,1} + w_2 z_{m,2}) = -1 < 0 \quad (21)$$

therefore, we can rewrite the margin as,

$$\text{margin} = \frac{w_1 z_{k,1} + w_2 z_{k,2}}{\sqrt{w_1^2 + w_2^2}} - \frac{w_1 z_{m,1} + w_2 z_{m,2}}{\sqrt{w_1^2 + w_2^2}} = \frac{(w_1, w_2) \cdot (z_{k,1} - z_{m,1}, z_{k,2} - z_{m,2})}{\sqrt{w_1^2 + w_2^2}}, \quad (22)$$

or simply by $\frac{\mathbf{w}}{\|\mathbf{w}\|} \cdot (\mathbf{z}_k - \mathbf{z}_m) = \frac{2}{\|\mathbf{w}\|}$. To find the optimal hyperplane is, in this example, to find

X

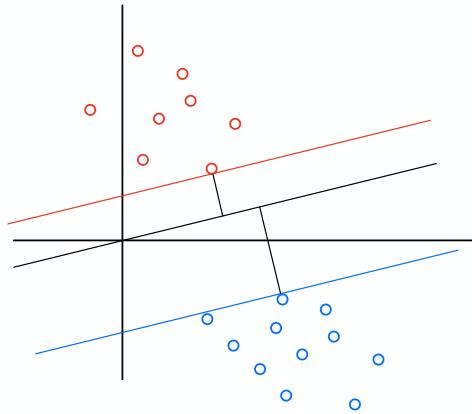


Figure A1: A two dimensional example of an optimal hyperplane. Red dots correspond to the training data-points labeled -1, blue dots correspond to the training data-points labeled as +1. The margin is defined as the sum of the distance between points in different classes to the hyperplane.

the values of w_1 and w_2 such that the margin is the largest possible. This is achieved with the vector \mathbf{w} with smallest norm, and we find out the optimization problem stated in the main text,

$$\min_{\mathbf{w}} \frac{1}{2} \|\mathbf{w}\|^2 \quad (23)$$

subject to $y_k (\mathbf{w} \cdot \mathbf{z}_k) \geq 1, k = 1, \dots, n.$

Not Just Guns or Butter, but What Came First—Guns or Butter? Introducing GVAR to International Relations

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Abstract

Power remains a crucial concept in international relations. In recent decades a prevailing notion became present in the literature explaining that economic power is a prerequisite for all other forms of power (military, political and cultural). Yet, such an assumption has never been properly tested. To test this assumption, the paper introduces a new time-series method to political science—Global Vector Auto-regression (GVAR). While the method is widely used in economics, it has not been employed in political science. The method should be appealing to scholars in political science since it enables big-N and big-T hypotheses tests. We also present Granger-causality tests within the context of GVAR and test if economic power is a prerequisite for military power. Our results suggest that the role of the economy has changed through history. Namely, in 19th century it was the military power that drove (Granger-caused) the economy; yet, since 1955 the roles are reversed.

Keywords— time series; IPE; GVAR; IR

Work in progress. Please don't cite. Download the last version of the paper [here](#).

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POWER: ASSUMED BUT UNTESTED

“Power is the ultimate aphrodisiac”

Kissinger (1973)

Since the dawn of social and political science, *power* has been a key concept of multiple research projects (Lasswell and Kaplan 1950). In recent decades a prevailing notion became present in international relations, which sometimes takes the shape of a latent assumption (Kennedy 1989; Luttwak 1990; Fukuyama 1992; Nau 1995; Kirshner 1997; Gilpin 2001; Brooks 2005; Ikenberry 2011). The argument is neatly summed by Organski, who claimed that economic power is a prerequisite for all other power capabilities: without a strong economy a dominant state stagnates and declines (Organski 1958, 299-306). However, this claim has never been properly tested. For instance, Mearsheimer (2001, Ch. 3) dedicates a whole chapter to explain how latent power matters to military power,¹. Yet his explanation remains untested.

We believe that if the literature offers assumptions about the nature of power while proposing causal mechanisms between power factors—military, economic, political and cultural (Mann 1986)—then it should also provide empirical evidence about those. We focus one of these assumptions, particularly, that economic power is a *prerequisite* of all other power factors.

This paper contributes to the literature by introducing Global Vector Autoregression (GVAR) methods to international relations. While panel-data methods handle well numerous panels (countries) within relatively short timespan, and time-series methods do the same but for longer timespan but for small panels (usually just one), GVAR perform statistically well with both big N and long T . In particular, we show how Granger-causality tests (C. W. J. Granger 1969) within the framework of GVAR methods should shed some light on the power prerequisite assumption. In this paper we focus solely on the (Granger) causal relation between *economic* and *military* powers. We hypothesize that pre-WWI it was military power that Granger-caused economic power—greater military led to greater economic power. Conversely, we expect that post WWII economic power Granger-caused military power.

The paper proceeds as follows. First, section I discusses the concept of power by particularly focusing on the relationship between economic and military power. In section II we present actual time-series data on economic and military power for the maximum possible number of available

assumed?
yet, we are
using prox-
ies. I think
we should
change op-
tics here.
That’s not
the problem
we’re tack-
ling. What
we try to
do is to ex-
amine the
“origins” of
power.

panels. The section also shows Granger-causality tests within the GVAR framework. We also show some simulations. Finally in section VI we propose future research avenues regarding this scientific inquiry.

I. CONCEPTUALIZING POWER

II. INTRODUCING THE GVAR METHODOLOGY

GVAR was introduced in M. Pesaran, Schuermann, and S. M. Weiner (2004). It “was developed in the aftermath of the 1997 Asian financial crisis to quantify the effects of macroeconomic developments on the losses of major financial institutions” (Mauro and Pesaran 2013, 1). The main feature of GVAR is that it “take[s] into account the various interlinkages in the global economy in the context of a truly multicountry setting” (H. Pesaran, Schuermann, and S. Weiner 2004, 139) by incorporating a large number of panels (“big N ”) for long time-spans (“big T ”) which are weighted by exogenous factors such as bilateral trade.

While GVAR has been widely used in economics (Favero 2013; Mauro, Filippo, and Pesaran 2013; Chudik and Pesaran 2016; Eickmeier and Ng 2015) its characteristics are also appealing for political scientists. Yet, to our knowledge, the GVAR method has not been used or introduced in political science so far. We do so in this paper and test relations between economic and military power in a truly global setting. In the rest of the paper we rephrase the famous dilemma about whether governments should allocate resources to finance “guns” (military power) or “butter” (economic power or economic development in general).

Global vector auto-regressive models (GVAR) are a special category of vector auto-regressive models (VAR). Following Box-Steffensmeier et al. (2014, 164), define a VAR model as follows,

$$\mathbf{x}_{it} = \boldsymbol{\alpha}_i + \Phi_i \mathbf{x}_{i,t-p} + \mathbf{u}_{it} \quad (1)$$

where \mathbf{x}_{it} is a $k_i \times 1$ vector of endogenous variables which are lagged p times on the right-hand side, and where $E(\mathbf{u}_{it}) = 0$. Now, following Mauro and Pesaran (2013, 14), define a GVAR model with p lags for country i as follows,

$$\mathbf{x}_{it} = \boldsymbol{\alpha}_i + \boldsymbol{\Phi}_i \mathbf{x}_{i,t-p} + \boldsymbol{\Lambda}_{1i} \mathbf{W}_{x,t} + \boldsymbol{\Lambda}_{2i} \mathbf{W}_{x,t-p} + \mathbf{u}_{it} \quad (2)$$

where \mathbf{x}_{it} is a $k_i \times 1$ vector of domestic (i.e. endogenous) variables, \mathbf{W}_t is a $k_i \times 1$ vector of weakly-exogenous foreign variables, and \mathbf{u}_{it} is a serially uncorrelated and cross-sectionally weakly dependent process. Typically, the choice of the lag orders of every GVAR system is selected according to the Akaike information criterion (Mauro and Pesaran 2013, 19). Also individual country-specific models of the form of [Equation 2](#) “allow for cointegration among domestic variables as well as between domestic and country-specific cross-section averages of foreign variables” (Chudik and Pesaran 2016, 179).

As it becomes apparent, the inclusion of foreign variables \mathbf{W}_t in [Equation 2](#) is one of the main characteristics of the GVAR approach, and the main difference with the VAR equation described in [Equation 1](#). As explained by others “[t]hrough the use of foreign variables, the GVAR is able to account for bilateral inter-relationships amongst countries, and therefore control for spillovers on the basis of cross-country exposure.”²

We detail in [section III](#) that to construct \mathbf{W}_t we employ bilateral trade—but the particular choice of exogenous variable depends on the empirical application. In simple, the Global vector autoregressive GVAR model in [Equation 2](#) explains $\mathbf{x}_{i,t}$ as a function of past values $\mathbf{x}_{i,t-p}$ lagged p times, at the same time that it weights these dynamics by weakly-exogenous foreign variables \mathbf{W}_{t-p} (weights which are captured by parameters $\boldsymbol{\Lambda}_{ni}$).

Granger-causality tests Since we are substantively interested in whether “guns” (Granger) *cause* “butter” or the other way around, in this paper we estimate country-specific bivariate Granger-causality tests within the GVAR framework.³ The Granger-causality method was introduced by C. Granger (1969) and seeks to investigate if some variable X (Granger) “causes” another variable Y , or the other way around. A variable X is said to “Granger-cause” Y if predictions of Y based on lagged values of Y and lagged values of X perform better than explaining Y just with its own past values.

Since Granger-causality tests are usually estimated via VAR equations ([Equation 1](#)) we now derive the Granger-causality test within the GVAR framework. The substantive advantage of GVAR Granger-causality test over regular Granger-causality test is that estimates are weighted by the global economy (\mathbf{W}_t in [Equation 2](#)), situating the domestic dynamics within the global context.

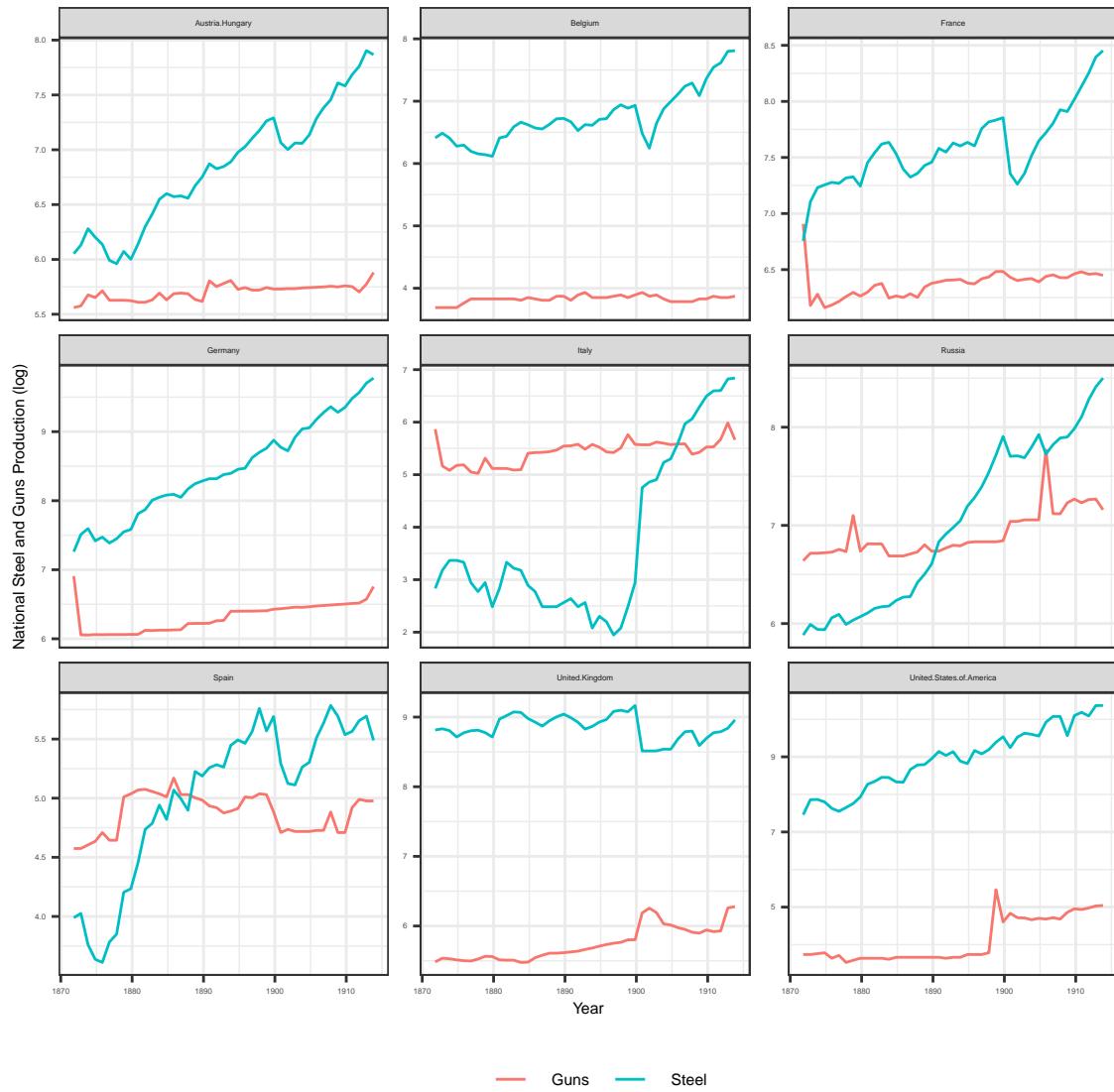


Figure 1: National Steel and Guns Production (log), 1871-1913.

Note: Variables are “milper” and “irst.” Both were obtained from Singer, Bremer, and Stuckey (1972).

More formally, we estimate a GVAR Granger-causality system for every country i with p lags as shown in [Equation 3](#):

$$\begin{aligned}\mathbf{x}_{it} &= \boldsymbol{\alpha}_i + \boldsymbol{\Phi}_{1i}\mathbf{y}_{i,t-p} + \boldsymbol{\Phi}_{2i}\mathbf{x}_{i,t-p} + \boldsymbol{\Lambda}_{1i}\mathbf{W}_{x,t} + \boldsymbol{\Lambda}_{2i}\mathbf{W}_{x,t-p} + \boldsymbol{\Lambda}_{3i}\mathbf{W}_{y,t} + \boldsymbol{\Lambda}_{4i}\mathbf{W}_{y,t-p} + \mathbf{u}_{it} \\ \mathbf{y}_{it} &= \boldsymbol{\alpha}_i + \boldsymbol{\Phi}_{1i}\mathbf{x}_{i,t-p} + \boldsymbol{\Phi}_{2i}\mathbf{y}_{i,t-p} + \boldsymbol{\Lambda}_{1i}\mathbf{W}_{y,t} + \boldsymbol{\Lambda}_{2i}\mathbf{W}_{y,t-p} + \boldsymbol{\Lambda}_{3i}\mathbf{W}_{x,t} + \boldsymbol{\Lambda}_{4i}\mathbf{W}_{x,t-p} + \mathbf{u}_{it}\end{aligned}\quad (3)$$

where the only added complexity is the introduction of a second variable (y), but the lag and weight structure remains the same as in [Equation 2](#).

III. DATA: THE ART OF POSSIBLE

We operationalize military and economic power using variables from the Correlates of War Project, particularly, the National Material Capabilities dataset (Singer, Bremer, and Stuckey [1972](#)).⁴ The dataset covers all countries in the world between 1816-2012. Since we are interested in analyzing the longest T and the widest N possible, using the COW dataset seemed the most obvious choice. To proxy military power we use the “military personnel” variable and to proxy economic power we use the “iron and steel production” variable. Let military power be \mathbf{x}_{it} and economic power be \mathbf{y}_{it} in [Equation 3](#).

In order to maximize the extension of the dataset, it was necessary to split it in two. Since wars typically not only redraw country borders but also end, merge or split countries, the most efficient way of having the least missing data possible was partitioning the dataset in two. The first period goes between 1871 and 1913 ([Figure 1](#)). The second period goes between 1955 and 2012 ([Figure 2](#)).

We believe that while GVAR methods offer interesting properties for political scientists and for international relations scholars in particular, there is the inevitable issue of missingness: not all countries remain observed, especially during long periods of times. The biggest concern is that the ones that do remain observed are systematically different from the ones that do not. For instance, it is reasonable to expect that countries that cease to exist were weaker economically, politically and militarily. We believe these issues, while inconvenient, do not threaten our statistical inference with selection bias. This paper does not address country survival. In fact, it seeks to shed light on military and economic dynamics of *all* countries who actually survive for the longest timespan possible. Finally, missing data is not a new problem in applied research. There are several ways

justify both,
but mostly
steel

to cope with this issue, for instance, multiple imputation. However we leave possible solutions for future research.

cite something

Moving forward, there is a considerable gap between 1913 and 1955. At the risk of sounding repetitive, it is worth emphasizing that a country needs to remain available for the *whole* time. Even if just one year is missing then that particular country cannot be considered. While we believe this limitation is an important one, balanced panels are not just a GVAR requirement but rather a general statistical need in most TS-PD models. We encourage analysts to deal with this issue by also finding substantive reasons for splitting the data, as we do next.

While the need for splitting the dataset in two was empirical, the decision was complemented with both theoretical and historical reasons. The hope is to take theoretical advantage of an empirical problem. The first timespan seeks to capture the international dynamics of the German unification and the beginning of World War I. The second time span corresponds to the post World War II international scenario. In order to maximize our analytical leverage respect to the partitioned dataset, we conform the next two working hypotheses:

check

Hypothesis 1. *Between 1871 and 1913 military power Granger-causes economic power.*

Hypothesis 2. *Between 1955 and 2012 economic power Granger-causes military power.*

The main added value of GVAR methods is the inclusion of a foreign or global variable which acts as a weight **W** in [Equation 2](#) and [Equation 3](#). The idea is to account for shock transmissions across economies. In this application we have opted for dyadic trade data. This variable was constructed by the same project but in the Trade dataset (Barbieri, Keshk, and Pollins [2009](#); Barbieri and Keshk [2016](#)).⁵

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Foreign variables are by definition (weakly) exogenous (Chudik and Pesaran [2016](#), 170). However, in many applications, especially in political science and international relations, there might be economic or military superpowers so influential to other countries that cannot be considered exogenous. More on the contrary, it should be quite natural to find that some economies based on asymmetric inter-dependence or post-colonial influence are highly correlated with the economy of the hegemon. For instance, di Mauro and Smith ([Mauro and Pesaran 2013](#), 236) and Chudik and Pesaran ([2016](#)) run separate GVAR analyses on the United States. We encounter similar non-exogeneity issues with China, Russia and the United States for the 1955-2012 period. Substantively that means that the role of these three countries is influential in other smaller economies breaking the (weakly)

term correct?

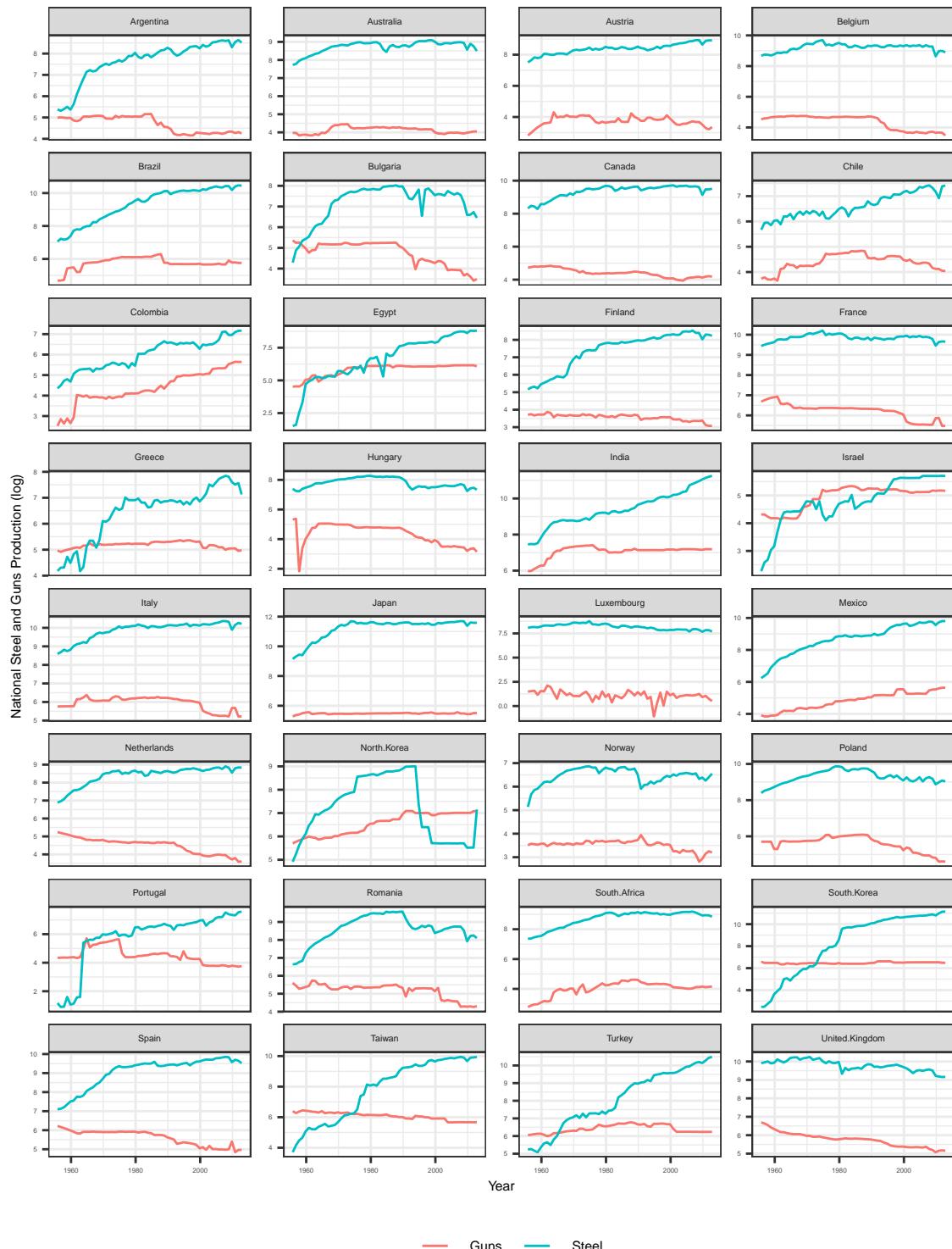


Figure 2: National Steel and Guns Production⁷ (log), 1955-2012.

Note: Variables are “milper” and “irst.” Both were obtained from Singer, Bremer, and Stuckey (1972).

exogeneity assumption. Empirically, due to multicollinearity and matrix non-invertibility issues, the GVAR models analyzed in this paper were mathematically impossible to perform having China, Russia and the United States along with the rest of the economies. For these reasons these three economic and military superpowers were analyzed separately.

Moving forward, the weight variable \mathbf{W} is a square matrix which has all \mathbf{K} countries in both its columns and rows with zeros as diagonal elements. The matrix represents bilateral trade among two countries measured by the *flow1* and the *flow2* variables. The former measures imports from a country (*importer1*) to another country (*importer2*) and the latter measures the reverse dyad, i.e. imports from *importer2* to *importer1*. In addition to that, \mathbf{W} contains \mathbf{T} sub-matrices, one sub-matrix per every year t . Each sub-matrix t has dimensions $k \times k$ for a total of \mathbf{K} countries such that,

$$\mathbf{W}_t = \begin{bmatrix} & \mathbf{i}_1 & \mathbf{i}_2 & \mathbf{i}_3 & \dots & \mathbf{i}_K \\ \mathbf{i}_1 & 0 & f_{21} & f_{31} & \dots & i_{K1} \\ \mathbf{i}_2 & f_{12} & 0 & f_{32} & \dots & i_{K2} \\ \mathbf{i}_3 & f_{13} & f_{23} & 0 & \dots & i_{K3} \\ \vdots & \vdots & \vdots & \vdots & \ddots & \vdots \\ \mathbf{i}_K & f_{1K} & f_{2K} & f_{3K} & \dots & i_{KK} \end{bmatrix}$$

Every W_t matrix weights all \mathbf{K} country-specific Granger regressions described in [Equation 3](#). Every k system is weighted by the other $\mathbf{K} - 1$ sub-matrices of dyadic trade. As [Equation 3](#) shows, the GVAR methodology also considers p lags of the \mathbf{W} matrix.

Following the literature on Granger-causality tests we focus our attention model-specific f-tests (one per country) which tests if all variables in the model are jointly significant. Then null is that there is no Granger causality.⁶

IV. RESULTS: GVAR GRANGER-CAUSALITY TESTS

The GVAR method handles a large number of panels. While this is an advantage of this approach it imposes restrictions in how to communicate results effectively. We considered that showing a table containing all the $\mathbf{K} \times 2$ f-tests (one f-test per each country-specific Granger-causality relationship as in [Equation 3](#)) would be inefficient. In this application it is more informative getting a general sense of the possible causal mechanisms among all countries rather than analyzing each Granger

equation individually. Hence in [Figure 3](#) we show country-specific p-values of all Granger-causality f-tests obtained when fitting [Equation 3](#).⁷ [Figure 3](#) is organized in panels. Columns represent the time period and rows represent the Granger-causality relationship as shown in [Equation 3](#). The figure conveys in colors the statistical significances of every country-specific f-test. In gray are shown the p-values that are larger than .1. While China, Russia and the United States GVAR Granger-causality tests were performed separately, they are shown with the rest of the countries.

From a substantive point of view, [Figure 3](#) shows that during the 1871 – 1913 period, in 44% of the countries economic power Granger-caused military power. Yet, interestingly, this percentage drops to 38% and 33% for the influential economies during the 1955 – 2012 period. On the other hand, in the same second period in 46% of the countries military power Granger-caused economic power.

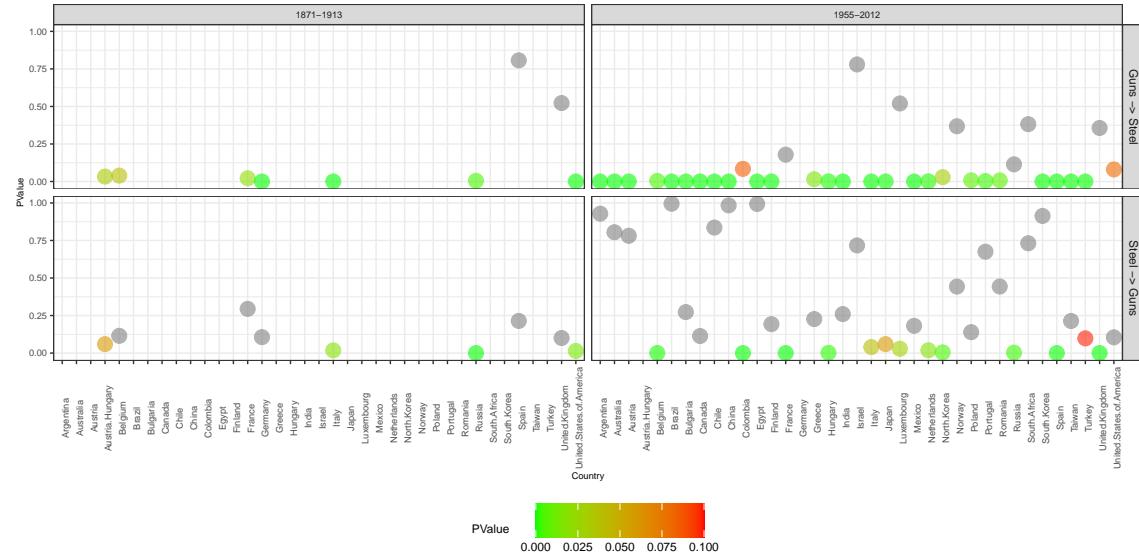


Figure 3: P-Values of the Country-specific Granger-causality F-Tests, 1871–2012.

Note: Plot shows country-specific p-values of the Granger-causality f-tests obtained when fitting [Equation 3](#) (detailed results shown in [Table A1](#), [Table A2](#) and [Table A3](#)). The plot shows that during the 1871-1913 period, in 44% of the countries, steel Granger-caused guns. This percentage changes to 38% and to 33% for the hegemonic countries during the 1955-2012 period.

briefly analyze both Hypoth

V. SIMULATIONS

In this section we compare different methods for analyzing big-N and big-T datasets. First, we take the VAR example provided by Enders (2014, 286),

$$\begin{aligned}\mathbf{x}_t &= \boldsymbol{\alpha}_{10} + \boldsymbol{\beta}_{11}\mathbf{x}_{t-1} + \boldsymbol{\beta}_{12}\mathbf{y}_{t-1} + \mathbf{u}_{1,t} \\ \mathbf{y}_t &= \boldsymbol{\alpha}_{20} + \boldsymbol{\beta}_{21}\mathbf{y}_{t-1} + \boldsymbol{\beta}_{22}\mathbf{x}_{t-1} + \mathbf{u}_{2,t}\end{aligned}\tag{4}$$

and simulate a VAR data-generating process by setting $\alpha_{10} = \alpha_{20} = 0$, $\beta_{11} = \beta_{22} = 0.7$ and $\beta_{12} = \beta_{21} = 0.2$. Next, we derive the panel representation of Equation 4 generating a dataset of 30 countries following the same VAR process.

VI. DISCUSSION

The present research project makes two contributions to the broader International Relations literature. First, we test the general assumption that the economic power is the prerequisite for all other power factors. Second, we introduce to Political Science a new time series method—GVAR.

Our preliminary results indicate that the assumption of economic power dominance, which is so often and with ease made by scholars, is at least dubious, and flawed at best. In fact, the military power Granger-causes economic power in both periods—19th and 20th century.

However, future iterations of this project are necessary to be fully confident in such a result and conclusion. First, we need to enhance our model to also include military expenditures and energy consumption. Second, robustness checks using different time frames and imputation needs to be performed as well. Third, a comparison of results using GVAR with PVAR method would be beneficial. Namely, the amount of countries whose data is available for 19th century would still be classified as a small N. Thus, PVAR would also be appropriate method to be used for this period with small N. Consequently, we would expect that for 19th century conclusions from PVAR and GVAR would be the same, whereas for 20th century the conclusions would differ due to the big N in that period.



Figure 4: Simulated VAR Processes: 250 years, 120 countries.

Note: The figure shows 120 simulated VAR processes (countries). Each process lasts for 250 years. Following Enders (2014, 286), we set in Equation 4 $\alpha_{10} = \alpha_{20} = 0$, $\beta_{11} = \beta_{22} = 0.7$ and $\beta_{12} = \beta_{21} = 0.2$.

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VII. APPENDIX

I. Appendix

Table A1: *Bivariate Global Granger Causality Tests of the World Political Economy, 1871-1913*

	Granger Relationship	F-Test	P-Value	DF	Adjusted R-sq	Lags
Austria-Hungary	butter → guns	2.16	0.06	8,31	0.192	1
	guns → butter	2.48	0.03	8,31	0.233	
Belgium	butter → guns	1.781	0.11	16,19	0.263	5
	guns → butter	2.339	0.04	16,19	0.38	
France	butter → guns	1.27	0.29	8,31	0.052	1
	guns → butter	2.72	0.02	8,31	0.261	
Germany	butter → guns	1.844	0.11	8,31	0.148	1
	guns → butter	7.891	0	8,31	0.586	
Italy	butter → guns	2.777	0.02	16,19	0.448	5
	guns → butter	6.801	0	16,19	0.726	
Russia	butter → guns	8.725	0	16,19	0.779	5
	guns → butter	3.595	0	16,19	0.543	
Spain	butter → guns	1.454	0.21	8,31	0.085	1
	guns → butter	0.554	0.81	8,31	-0.101	
United Kingdom	butter → guns	1.85	0.1	16,19	0.28	5
	guns → butter	0.966	0.52	16,19	-0.016	
United States	butter → guns	2.942	0.01	8,31	0.285	1
	guns → butter	8.608	0	8,31	0.609	

Table shows country-specific Granger-causality F-tests. Last column shows number of domestic lags used per every country-specific Granger model. Number of lags for endogenous variables = 2. Number of lags for foreign variables = 2. Max number of lags for estimating the country-specific VAR model = 5.

Information criteria for optimal lag length = AIC. Deterministic variables: Trend and constant.

Table A2: *Bivariate Global Granger Causality Tests of the World Political Economy, 1955-2012*

	Granger Relationship	F-Test	P-Value	DF	Adjusted R-sq	Lags
Argentina	butter → guns	0.422	0.93	10,44	-0.12	1
	guns → butter	7.765	0	10,44	0.556	
Australia	butter → guns	0.629	0.8	12,41	-0.092	2
	guns → butter	5.618	0	12,41	0.511	
Austria	butter → guns	0.656	0.78	12,41	-0.084	2
	guns → butter	7.305	0	12,41	0.588	
Belgium	butter → guns	3.842	0	12,41	0.392	2
	guns → butter	3.001	0	12,41	0.312	
Brazil	butter → guns	0.237	0.99	12,41	-0.209	2
	guns → butter	9.498	0	12,41	0.658	
Bulgaria	butter → guns	1.269	0.27	12,41	0.057	2
	guns → butter	4.286	0	12,41	0.427	
Canada	butter → guns	1.69	0.11	10,44	0.113	1
	guns → butter	5.055	0	10,44	0.429	
Chile	butter → guns	0.561	0.84	10,44	-0.088	1
	guns → butter	7.313	0	10,44	0.539	
Colombia	butter → guns	4.855	0	18,32	0.581	5
	guns → butter	1.737	0.08	18,32	0.21	
Egypt	butter → guns	0.219	0.99	10,44	-0.169	1
	guns → butter	5.471	0	10,44	0.453	
Finland	butter → guns	1.428	0.19	12,41	0.088	2
	guns → butter	5.041	0	12,41	0.478	
France	butter → guns	8.235	0	16,35	0.694	4
	guns → butter	1.442	0.18	16,35	0.122	
Greece	butter → guns	1.344	0.23	18,32	0.11	5
	guns → butter	2.39	0.02	18,32	0.334	
Hungary	butter → guns	3.363	0	12,41	0.349	2
	guns → butter	3.711	0	12,41	0.38	
India	butter → guns	1.287	0.26	14,38	0.072	3
	guns → butter	6.048	0	14,38	0.576	
Israel	butter → guns	0.702	0.72	10,44	-0.058	1
	guns → butter	0.63	0.78	10,44	-0.073	
Italy	butter → guns	2.151	0.04	10,44	0.176	1
	guns → butter	11.365	0	10,44	0.657	
Japan	butter → guns	1.876	0.06	16,35	0.216	4
	guns → butter	4.079	0	16,35	0.491	
Luxembourg	butter → guns	2.296	0.03	10,44	0.194	1
	guns → butter	0.925	0.52	10,44	-0.014	
Mexico	butter → guns	1.436	0.18	16,35	0.12	4
	guns → butter	8.525	0	16,35	0.702	
Netherlands	butter → guns	2.483	0.02	10,44	0.216	1
	guns → butter	3.73	0	10,44	0.336	
North Korea	butter → guns	2.875	0	18,32	0.403	5
	guns → butter	2.143	0.03	18,32	0.292	
Norway	butter → guns	1.02	0.44	10,44	0.004	1
	guns → butter	1.122	0.37	10,44	0.022	
Poland	butter → guns	1.57	0.14	12,41	0.114	2
	guns → butter	2.745	0.01	12,41	0.283	
Portugal	butter → guns	0.749	0.68	10,44	-0.049	1
	guns → butter	3.265	0	10,44	0.296	
Romania	butter → guns	1.019	0.44	10,44	0.004	1
	guns → butter	2.878	0.01	10,44	0.258	
South Africa	butter → guns	0.686	0.73	10,44	-0.062	1
	guns → butter	1.102	0.38	10,44	0.019	
South Korea	butter → guns	0.448	0.91	10,44	-0.114	1
	guns → butter	9.019	0	10,44	0.598	
Spain	butter → guns	4.66	0	10,44	0.404	1
	guns → butter	4.92	0	10,44	0.421	
Taiwan	butter → guns	1.382	0.21	12,41	0.08	2
	guns → butter	5.667	0	12,41	0.514	
Turkey	butter → guns	1.696	0.1	14,38	0.158	3
	guns → butter	25.426	0	14,38	0.868	
United Kingdom	butter → guns	9.085	0	10,44	0.6	1
	guns → butter	1.139	0.36	10,44	0.025	

Table shows country-specific Granger-causality F-tests. Last column shows number of domestic lags used per every country-specific Granger model. Number of lags for endogenous variables = 3. Number of lags for foreign variables = 3. Max number of lags for estimating the country-specific VAR model = 5. Information criteria for optimal lag length = AIC. Deterministic variables: Trend.

Table A3: *Bivariate Global Granger Causality Tests of Influential Economies, 1955-2012*

	Granger Relationship	F-Test	P-Value	DF	Adjusted R-sq	Lags
China	butter → guns	0.252	0.98	9,45	-0.142	2
	guns → butter	83.191	0	9,45	0.932	
Russia	butter → guns	3.663	0	7,48	0.253	1
	guns → butter	1.775	0.11	7,48	0.09	
United States	butter → guns	1.821	0.1	7,48	0.095	1
	guns → butter	1.961	0.08	7,48	0.109	

Table shows country-specific Granger-causality F-tests. Last column shows number of domestic lags used per every country-specific Granger model. Number of lags for endogenous variables = 2. Number of lags for foreign variables = 2. Max number of lags for estimating the country-specific VAR model = 5. Information criteria for optimal lag length = AIC. Deterministic variables: NA.

NOTES

1. They described it as perhaps the most fundamental power, whereas Elster (1976, 245-70, 249) is even more assertive arguing that power is the most important single idea in political theory.
2. International Monetary Fund (2016, 17).
3. We acknowledge that without proper experimentation and randomization there cannot be proper causation. Consequently, and following the Granger methodology, we employ a rather lose definition of “causation” and explore if lagged values of a variable *forecast* another variable.
4. Version 5.0.
5. Version 4.0.
6. All GVAR Granger-causality tests were done using the **GVARX** R package, version 1.3 and implemented by Tsung-wu (2020). The package performs estimations and inferences of Global Vector Autoregression models based on H. Pesaran, Schuermann, and S. Weiner (2004) and Dees et al. (2007).
7. We owe this point to Tsung-wu Ho. Country-specific results are shown in the Appendix section, particularly Table A1, Table A2 and Table A3.

Income Taxation and State Capacity in Chile: Measuring Institutional Development Using Historical Earthquake Data

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Abstract

Income taxation fostered via spillover effects increases in state-consolidation over time in Chile. The paper contributes in two ways. First, it studies the relationship between taxation and state building outside Europe. Second, the paper tests the theory using a novel approach. Exploiting the exogeneity of earthquake shocks, I create a novel hand-collected longitudinal dataset on Chilean earthquake death tolls. Under reasonable assumptions, the capacity for enforcing and monitoring building codes throughout the territory is a reflection of a state's overall capacities. Using a Bayesian Poisson regression the paper shows that death tolls decrease (state capacity increases) once the income tax law was implemented in 1924. To explore the causal mechanisms, I discuss the Chilean case since the 1920s.

Keywords— state-building; fiscal sociology; development; Latin America; time-series and panel data econometrics.

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Levi (1989, 1) famously explained, “the history of state revenue production is the history of the evolution of the state.” However, even when we have gained considerable knowledge of fiscal expansion in European cases, the study of public finance within a context of state consolidation in the developing world is lacking, especially in the presence of “new leading [economic] sectors” (Schneider 2012, 2). In fact, while there are a number of theories about state capacity in Latin America, domestic explanations centered on the role of economic structural transformation and taxation have been overlooked.¹ Besides this theoretical gap, there is also an empirical deficit. As Soifer (2012, 586) points out, most “scholarship on state capacity [...] lack[s] a satisfying conceptualization and measurement scheme for this concept.” Moreover, while most state formation theories are situated during pre-colonial (Mahoney 2010) or early (Kurtz 2013; Soifer 2015), independent Latin America, we lack a measurement that corresponds *temporally* to the theories we have. In other words, most explanations of state-making are *historical* in nature, yet, in practice, available measurements capture *contemporary* levels of *stateness*.

This paper contributes to the literature from a substantive perspective by explaining the positive relationship between adopting the income tax and sectoral conflicts. The historical evidence suggests that since the income tax law was an agreement among the elites, the institution had quasi-voluntary compliance. In turn, the argument outlines how income taxation had positive spillover effects over state institutions, increasing levels of state consolidation over time. The mechanisms evidence how the presence of tax assessors and collectors throughout the territory increased norms of enforcement of state regulations more generally, fostering overall state capacity over time.

The paper also contributes to the literature from an empirical perspective. Exploiting the exogeneity of earthquake shocks, I leverage a novel hand-collected dataset on Chilean earthquake death tolls between 1900 and 2010. Earthquakes are time-invariant, and importantly, orthogonal to economic development and regime type. Under reasonable assumptions, if the state’s capacity for enforcing and monitoring building codes throughout the territory is a reflection of overall state capacity, then death-toll differentials should be mainly associated with state capacity. Exploiting this variation via a Bayesian Poisson model I find that death tolls associated with earthquakes systematically decreased (that is, *state capacities increased*) after the income tax law was implemented in 1924. Importantly, these changes are not correlated with economic growth or industrialization levels. The empirical section also provides an in-depth qualitative case study.

The paper argues, in more detail, that (a) the emergence of the industrial sector caused higher

levels of sectoral conflicts, triggering the implementation of the income tax, which in turn, (b) fostered state development over time via spillover effects (Figure 1). The article empirically tests both hypotheses, while providing a short Chilean case study.

The paper not only builds on the fiscal sociology literature (Musgrave 1992), but on the sectoral politics approach, too (B. Moore 1966; Stephens, Rueschemeyer, and Stephens 1992; Ansell and Samuels 2014; Boix 2003; Acemoglu and Robinson 2009). In particular, it argues that elites whose assets are allocated in different sectors of the economy have different preferences over direct taxation, and consequently, state centralization (Acemoglu and Robinson (2009, 289), Best (1976, 50), Mamalakis (1971, 109)). The framework follows Mamalakis (1969, 1971), who introduced the sectoral conflict approach for the Latin American cases but also Hechter and Brustein (1980, 1085) who explain, “state formation will be most likely to the degree that powerful individual actors form two groups on the basis of divergent economic and political interests.”

As such, this article is an attempt to provide an alternative explanation of state development to the bellicist approach (Tilly 1992; Dincecco and Onorato 2016) and extended to the Latin American case by Thies (2005), Thies, Chyzh, and Nieman (2016), and Kurtz (2006)—but see Centeno (2002). And while Kurtz (2009, 2013) and Soifer (2015) situate the relevant state-building critical juncture at the end of the colonial period, before the class compromises that this paper identifies, the argument pursued in this paper explains that the implementation of the income tax was an important building block in this process.

I. SECTORAL CONFLICTS, AND INCOME TAXATION

The landed Latin American elites were an hegemonic economic group protected by norms and institutions that originated during colonial times (Keller 1931, 13). Not only that, the post-colonial institutional and economic orders were designed to give an unfair advantage to the agricultural sector. Mamalakis (1971, 99) is well known for describing how an agriculture-government coalition was formed in Chile, beginning with the colonial period. Several historians point out that “[i]n those areas where the government did interfere in the countryside, the effect was to strengthen the position of the landowning class” (Bauer 2008, 118). In fact, the little public infrastructure that existed benefited mostly the agricultural sector (Rippy 1971; Marichal 1989; Zeitlin 1984; Bauer 2008). By extension, the landowning class controlled most of the politics, too (Wright (1975, 45-46), Zeitlin

(1984, 13), Bauer (2008, 45), Baland and Robinson (2008, 1748), Best (1976, 56), Rippy (1971) and Marichal (1989)). For example, Collier and Collier (2002, 106) explain that the Chilean “national government was dominated by the central part of the country, with owners of large agricultural holdings playing a predominant role.”²

However, when the “structural transformation” happened—a process by which there is a “secular decline of agriculture and substantial expansion of manufacturing” (Johnston and Mellor 1961, 567)—it imposed tight constraints on the way politics was run by the incumbent landowning class. Given the foundational advantage of the landed elites, the secular emergence of the industrial sector generated political, economic, and military threats to the landed elites.³ For instance, before the Civil War, *salitreras* (nitrate towns) in northern Chile were so prominent that they were considered “a state within the state” (Barros 1970, 500). Industrial bosses had to approve decisions on whether public employees could be fired, whether public works could be developed, and on whether politicians could give public speeches. *Salitrera* industries also coined their own currency and had their own particular local laws. All in all, these sets of practices posited credible threats to the agricultural political and economic orders.

The preferences over fiscal policy of the expanding industrial sector clashed with the ones of the agricultural class. On the one hand, land fixity increased the risk premium of the landed elite’s main asset (Robinson 2006, 512), so they systematically resisted taxation. However, as capital could be reinvested in nontaxable sectors (Hirschman (1970); R. Rogowski in Drake and McCubbins (1998, ch. 4); however, see Bates and Lien (1985, 15)), industrialists’ preferences toward taxation were more elastic.

The emergence of a strong industrial class led to heavier pressures for the implementation of the income tax law.⁴ As industrialists depended more on infrastructure implemented at the local level—such as roads, railroads, and bridges—the industrial classes in Latin America “[preferred] to shoulder a higher tax burden through progressive direct taxation” (Beramendi, Dincecco, and Rogers 2016, 18).

In fact, in 1924 Chilean industrial elites accepted income taxes by agriculturalist incumbents *in exchange* for having more state services and being included in state politics. As others have explained, the non-agricultural sector “accepted taxation, while demanding state services and expecting to influence how tax revenues were spent [...] Consultation and cooperation were relatively institutionalised between the two sides” (Carmenza Gallo, in Brautigam, Fjeldstad, and Moore

(2008, 165)).⁵ Since both elites agreed on implementing the income tax, compliance was high.

Institutionalist economists find that optimal institutional choices result from political settings where all involved actors “had a voice in the choice of institutions” (Aghion, Alesina, and Trebbi 2004, 566), essentially contributing to an equilibrium of quasi-voluntary compliance (Levi 1989).

II. POSITIVE EXTERNALITIES OF INCOME TAXATION ON STATE CENTRALIZATION

Indirect taxes were easier to levy and hence this kind of revenue is generally considered “unearned income” (M. Moore 2004a, 304) or “easy-to-collect source of revenues” (Coatsworth and Williamson 2002, 10). Since customs administrations have always been concentrated in a few critical locations—especially ports—tariffs and customs duties oftentimes did not require an elaborate fiscal structure (Bertola and Ocampo 2012, 132). Given the relatively lower costs states have to incur to collect them, indirect taxes had a very low impact on state building (M. Moore 2004b, 14). In fact, when early Latin American states depended heavily on trade taxes, the state apparatus tended to be less developed (Campbell 1993, 177).

Fiscal sociologists, however, explain that direct taxation fostered state consolidation. Musgrave (1992, 98-99) and M. Moore (2004a, 298) explain that transforming private incomes into public property fostered state expansion. Particularly, since taxation (especially on incomes) requires such a high degree of state penetration, the study of public finances also offers a theory of state building.

The mechanism through which the positive spillover effects of taxation on state-building occur is based on the endogenous accumulation—and transmission—of knowledge. Particularly, the mechanism considers situations where the stock of know-how accumulated in the revenue service spread to other state institutions such as institutions related to the counting of individuals, policing, and the enforcement of public security, contracts, building codes, zoning laws, among others. The state-building literature has also considered these types of mechanisms before. For instance, Soifer (2013, 2012) and Bahamonde and Trasberg (2018) proxy state capacity by considering a cumulative count of censuses taken in a particular country. The understanding is the same: as early states learn to count their inhabitants (or tax them, in my account), they also learn—at marginally lower costs—to perform other state tasks.

This paper explains that direct taxation via a learning-by-doing process produced technical

complementarities⁶ between the state’s existing stock of know-how and fiscal capacities. In short, implementing the tax lowered the marginal costs of adding an additional layer of *stateness* onto the territory. Krasner (1985, 46) explains that “tariffs and export taxes are easier to obtain than direct taxes, which require high levels of bureaucratic skill and voluntary compliance.” This paper argues that it is this bureaucratic effort (contingent on elite-led fiscal pacts) that was what fostered state consolidation over time. For instance, it was necessary to send official emissaries to check on accounting books of the refinery in the north, the winery in the central valley, and the *hacienda* in the south. Eventually, these delegations became more complex—and at lower marginal costs—increasing the density of state presence in the territory.

And while there exists a tension between the intention to tax and the capacity to actually do it, it is important to remember that both elites wanted to implement a system of income taxation, fostering an equilibrium of quasi-voluntary compliance. The literature, in fact, finds that introducing the income tax has been associated with improvements in efficiency and expansion of the scope of a number of other state tasks. For instance, Kaldor explains that the revenue service is the “point of entry.” Once this institution is secured, securing the rest is marginally easier (in Brautigam, Fjeldstad, and Moore (2008, 15)). In turn, Besley, Ilzetzki, and Persson (2013, 208) explain that implementing the income tax law is “associated with investments in public administrative structures that support tax collection” in a number of countries, including Chile, while Dincecco and Troiano (2015, 3) find “a positive and significant relationship between the introduction of the income tax” and per capita (1) total expenditures, (2) education expenditures, and (3) health expenditures. Others have found that literacy levels in Chile rose from 40% in 1907 to 66% in 1925 (Engerman, Sokoloff and Mariscal, in Engerman and Sokoloff (2011, Ch. 5)), and the share of national revenue accounted for by income taxes after implementing the income tax in 1924 rose from 6% in 1920 to 23.7% in 1940 (Engerman, Sokoloff and Zolt, in Engerman and Sokoloff (2011, 178)). In turn, Humud (1969, p. 154) documents that the income tax was widely enforced, generating considerable resources for the Chilean treasury (in Bowman and Wallerstein (1982, 451-452)), and that the dependence on custom taxes decreased from 70.2% to 41.1% during those same years (Engerman, Sokoloff and Zolt, in Engerman and Sokoloff (2011, Ch. 6)).

III. FROM EARTHQUAKE DEATH-TOLLS TO STATE CAPACITY

Did the implementation of the income tax in Chile foster state development over time? While this section motivates the measurement approach pursued in this paper, it leaves for future research—presumably, in a “measurement paper”—further performance and sensitivity analyses regarding this measurement.

More than being blessed, the literature is in fact cursed due to an over-abundance of poor indicators of state consolidation (Soifer 2012, 589). In fact, its abundance “points to the poor state of empirical measures of the quality of states” (Fukuyama 2013, 347) mostly because most indices are conflated with analytical and conceptual problems (Ferreira 2017, 1292).

One notable example is “protection of the rule of law,” which is commonly used as a proxy for state capacity (Besley and Persson 2009, 1237). As Kurtz and Schrank (2007, 543) explain, this strategy is severely confounded “with policy preferences over the structure of private property rights.” On the one hand, this is problematic since the sources of these data are usually elite interviews (Fukuyama 2013, 349). To “the extent that public bureaucracies *are* effective in imposing taxes or regulatory demands [...] they are likely to be judged ‘burdensome’ and ‘growth-inhibiting’ by many businesspersons” (Kurtz and Schrank 2007, 542) thereby introducing systematic measurement error (Kurtz and Schrank 2012, 618). On the other hand, the problem is conceptual. As Soifer (2008, 247) puts it, there is a widely spread “problem of misalignment between dimension and indicator.” Kurtz and Schrank (2012, 619) recommend “explicitly avoid[ing] an emphasis on outputs that are at the center of political or policy debates, such as property rights.” For example, the U.S.S.R. had a strong state; however, it did not protect property rights.

Another iconic example of this misalignment problem is the use of fiscal extraction as a proxy of state capacity. Johnson and Koyama (2017, 3) explain that “[t]ax revenue per capita is a commonly used metric of fiscal capacity,” which in turn “speaks” to levels of state capacity. In fact, Thies (2015, 172) conceptualizes “fiscal capacity [...] in terms of tax revenue extracted from society.” This error is very common in the literature, and other examples are Besley, Ilzetzki, and Persson (2013, 224) and Besley and Persson (2014). Not only do tax shares reflect policy preferences (Soifer 2013, 9) but also, as Fukuyama (2013, 353) explains it, there “is a difference between extractive potential and actual extraction rates.” For instance, since American institutions were deliberately designed to limit the exercise of state power, the U.S. taxes very little (Fukuyama 2004, 6). However, it is not reasonable

to say that the U.S. is a “weak state.” Moreover, in late imperial China, “high taxes on peasants [...] were the result of rulers’ *lack* of power. Chinese rulers consistently attempted to limit officials’ excessive extractions from the masses, but were unable to do so” (Kiser and Tong 1992, 301).⁷

Finally, others have proxied state capacity with economic growth, which is also problematic (Fearon and Laitin 2003; Besley and Persson 2011). Interestingly, Mahoney (2010, 4, 6-7) pursues the same strategy. As Dargent, Feldmann, and Luna (2017) explain, state capacity and economic growth are causally distinct mechanisms. For instance, boom-led economic growth has left net state capacity low in Peru.

This paper identifies an additional limitation. Beyond conceptual and analytical problems, most available measurements are unable to capture temporal variations of state capacity. Since most explanations of state-making have a strong historical component, the lack of an indicator able to *travel in time* represents a huge deficit in the literature. Just to name a few examples, Soifer (2012, 585) “builds a new measure of state capacity for [...] contemporary Latin America [combining] multiple dimensions (extraction, security, and the administration of basic services).” Kurtz and Schrank (2012, 618-619) designed some list-experiments to study bureaucrats’ opinions, Dargent, Feldmann, and Luna (2017) “analyses the evolution of state capacity in Peru during the recent commodity boom,” while Luna and Toro (2014) and Luna and Soifer (2017) employ a survey-based design to measure contemporary subnational state capacities. While these measurements do overcome the conceptual and analytical problems mentioned above, they do not help us in studying state capacities in a historical setup.

Still, economic historians offer other alternatives to proxy levels of state capacity over time. Some examples are levels of investments in public goods such as infrastructure (Enriquez, Sybllis, and Centeno 2017), roads (Mann 1984, 2008; Acemoglu 2005; Saylor 2012; Thies 2009; Besley and Persson 2010), electrification (measured as light intensity per pixel, Huntington and Wibbels (2014)), and railroads (Saylor (2012, 302), Coatsworth (1974)).

Unfortunately, many of these measurements are debatable. For instance, Soifer (2012, 593) explains that “railroads were often constructed by private actors.” The same problem applies to other types of infrastructure.

There are other more appropriate strategies such as the opening of postal offices (Acemoglu, Moscona, and Robinson 2016), the administration of national censuses (Lee and Zhang 2017; Soifer 2013; Centeno 2002; Hanson and Sigman 2013; Hanson 2015). Another variation of this technique is

“age heaping” or vaccination (Soifer 2012).

While these measurements do capture historical variations of state capacity, some other problems arise. Censuses, for example, provide a non-continuous temporal measurement of state capacities. For instance, censuses are applied in Chile every ten years, so having just a few snapshots of state-capacity should compromise any statistical analysis. In turn, vaccines are usually targeted at primary and high school students. In practice, vaccines are administered by the schools themselves, both public and private. Private schools might be more efficient in doing so, inflating the average level of state capacity.

To solve some of these limitations, the paper proposes earthquake death tolls as an alternative to measure state capacity over time. In particular, the proposed measure is explained leveraging the Chilean case. Unlike censuses—*unfortunately*—earthquakes happen in Chile often. While “[e]arthquakes alone claim thousands of lives a year” (Anbarci, Escaleras, and Register 2005, 1908), they are not well studied in political science (Brancati 2007, 719). Building on Mann (1984, 113), the proposed measurement intends to capture the state’s *infrastructural* power, that is, “the capacity of the state [to] actually [...] penetrate civil society, and to implement logically political decisions throughout the realm.”

Natural hazards involve two kinds of processes, one natural and another human (Raschky 2008, 627). In the case of earthquakes, the natural component happens at random, and as a consequence, they are exogenous to the affected locality. For instance, Brancati (2007, 728) explains that “earthquakes constitute a natural experiment,” while Gignoux and Menéndez (2016, 27) also point out “that the occurrence of earthquakes can be viewed as random [allowing the analyses of] these events as a set of repeated social experiments.” In fact, earthquakes are orthogonal to levels of state capacity and economic development (Kahn (2005, 271) and Brancati (2007)).

Since the natural process associated with the realization of earthquakes is random, the only unexplained part that is left is the systematic human component, which is what the measurement captures. Consequently, keeping earthquake magnitudes constant at their means, (population-weighted) death counts should be attributed to the (*in*)capacity of the states to invest in preparedness and earthquake-mitigation institutions.

I focus on earthquakes and not on other natural disasters, such as “extreme temperature events, floods, landslides, and windstorms” (Kahn 2005, 280), because earthquakes cannot be foreseen and, as such, they put to the test the states’ capacity for having their preventive institutions *already* in

place and in good shape. In fact, Brancati (2007, 716) explains that “[e]arthquakes may provoke conflict more than any other type of natural disaster because they have rapid onsets [and] are not predictable.”⁸ State capacity consists of sustained proactive efforts of enforcing institutions in the territory and hence, short-term reactive actions should not be considered state-*making*.

Under reasonable assumptions, the capacity of deploying inspectors to enforce quake-sensitive zoning and building codes should be a reflection of the overall levels of state capacity. In fact, Ambraseys and Bilham (2011, 153) explain that “[e]arthquake-resistant construction depends on responsible governance,” while Raschky (2008, 628) argues that the effects of natural hazards depend on the region’s “institutional vulnerability.” Thus, state capacity acts as a scope condition undermining (facilitating) the implementation of construction norms. For example, Bilham (2013, 169) explains that “although engineering codes may exist[,] mechanisms to implement these codes are largely unavailable” in low-capacity states. For example, Anbarci, Escaleras, and Register (2005, 1910) explain that “while Iran has building codes [...] comparable to those existing in the United States, they tend to be enforced only in the country’s larger cities,” not in the countryside.

Only high-capacity states implement and enforce quake-sensitive regulations. The Chilean government started its efforts to ameliorate the impact of earthquakes after the great quake of 1928 in Talca. A first effort happened in 1929 when *Ley number 4563* was implemented. The law was among the first attempts to prohibit “construction, reconstruction or any other repairing or transformations [...] without a permit from the authorities.” Importantly, the law required that all blueprints had to be signed off by an expert before the construction started. By 1930, *Decreto number 4882* was adopted, but this time the rule made a number of technical prescriptions,⁹ determining what kinds of construction materials ought to be used, among other requirements. Critically, while the central government had retained the control of the supervision of the code since the promulgation of the *ley*, the *decreto* explicitly created the role of the *inspector* to supervise, enforce, and monitor these measures at the local level. Furthermore, *artículo 414* of the Chilean *Decreto 4882* granted inspectors “free access to the building” at any time during the construction process. The proposed measurement captures whether these good intentions achieved lower death tolls.

The proposed measurement has a number of advantages. Unlike non-experimental survey-based or purely policy-based measures, earthquake death tolls are an objective measurement of earthquake preparedness, an activity that any state must perform. For instance, Carlin, Love, and Zechmeister

(2014, 422) explain that “a basket of ‘minimal’ state functions [typically includes] primary education, public health, [the] rule of law, public finance management, and disaster relief.”

However, the measurement has a number of drawbacks. Obviously, the country needs to have earthquakes, possibly limiting the number of potential cases. Yet, good indicators of state capacity also suffer from the same problem (i.e. context-specificity). For instance, Soifer (2012, 593) and Slater (2008, 252) propose a measurement based on whether states are able to enforce voter registration “where voting is mandatory,” or conduct “state registration of marginal populations,” respectively, limiting the study of state-capacity to democratic countries only. This is not only a democracy-specific limitation but also a temporal one.

One advantage that actually mitigates some of these drawbacks is that most earthquakes occur at the various borders of the Pacific, Latin American, African, Arabic, Indian and Eurasian plates, allowing a number of potential cross-country comparisons (Keefer, Neumayer, and Plümper 2011, 1534). In fact, from a population size perspective, this measurement is also a convenient one. A “quarter of the world’s population inhabits [...] the northern edge of the Arabian and Indian Plates that are colliding with the southern margin of the Eurasian Plate” (Bilham and Gaur 2013, 618).

Additionally, there are countries like India or the United States where earthquakes happen only in certain regions. Presumably, mitigation policies in these places would need to be targeted to specific areas, possibly undermining the assumption that these kinds of policies should penetrate the entire territory. For instance, Dunbar, Bilham, and Laituri (2003, 164) explain that the Indian state implements targeted policies (that might not necessarily correspond to the administrative areas) based on isoseismal maps that define different zones of seismic hazard.

Another potential concern is that the ability to count the death toll might be a function of state capacity itself.¹⁰ However, in most cases, civic organizations, the Catholic Church, and, particularly, the press (national and local) have been the main entities who (willingly or not) have carried out the task of enumerating the deaths. Another potential issue is the measurement of the magnitudes. Before the instrumental period, magnitudes were obtained in an estimative way and, while there are methods to approximate historically-felt magnitudes to instrumental-like intensities (Szeliga et al. 2010), this strategy, unfortunately, adds more than one layer of complexity. All in all, this measurement offers a rough approximation of levels of state capacities over time.

IV. EMPIRICAL SECTION

The theory should pass two tests. First, the implementation of the income tax should be associated with the rising of the industrial sector. Given that all Latin American economies began by developing an agricultural sector first, the emergence of an industrial sector can be conceptualized as a sectoral conflict. In other words, the timing of the rising of the industrial sector should coincide with the implementation of the income tax. Second, we should observe that once the income tax is implemented, we should observe higher levels of state development.

I. Income Tax Adoption: Latin America

This section seeks to establish an empirical connection between sectoral conflict and the implementation of the income tax. All countries began as agricultural economies. Thus, sectoral conflict was proxied by measuring industrial development relative to agricultural sectoral growth rates, as presented in the [MOxLAD](#) data.¹¹ The dataset spans from 1900 to potentially 2010.¹² Since the idea is to capture the contribution of each individual sector, sectoral growths rates were not combined.

Using secondary information, [Table OA1](#) states when the income tax was implemented. [Figure 2](#) shows both sectoral outputs (independent variables), and the year when the income tax law was passed (dependent variable). Since population has been associated with the probability elites expand the franchise—and consequently the tax base, Engerman and Sokoloff ([2005](#), 892-893)—the total country-year population as a control variable was included.

[Table 1](#) shows 3 models.¹³ Following Aidt and Jensen ([2009](#)), Model 1 computes the lagged conditional hazard ratio of a country which has not yet adopted the income tax, adopts it in a given year, as a function of industrial and agricultural outputs.¹⁴ Countries drop out of the sample when they adopt the income tax. Lagging the independent variables should account for non-contemporaneous factors that might conflate the contribution of sectoral growths, such as prior state capacities, among others. Model 2 shows the estimated coefficients of a generalized estimating equation (GEE).¹⁵

Generalized estimating equations were introduced by Liang and Zeger ([1986](#)) to fit clustered, correlated, and panel data (Zorn [2006](#), 322). This method is especially well suited to analyze binary data (Hanley et al. [2003](#)).¹⁶

Substantively, GEE models provide an estimated marginal mean, or the weighted average of all

cluster-specific effects (or conditional means). Model 3 is a conditional logit model (“fixed effects”).¹⁷ One important advantage of this strategy is the ability to account for country-specific effects. For example, fiscal expansion could be a function of country-specific prior state-building capacities.¹⁸ A number of scholars rightly argue that post-colonial state-capacities are in part a function of pre-colonial state-capacities (Wimmer 2016, 1416, Mahoney 2010, Lange, Mahoney, and Hau 2006, 1426). Fixed-effects should be able to account for these and other unobserved or hard-to-measure covariates, which if left unaccounted for, would introduce omitted variable biases (Angrist and Pischke 2008).

All models suggest that the rise of a strong industrial sector largely accelerates the implementation of the income tax law. Moreover, a strong agricultural sector not only has zero impact on fiscal development, but a negative one. Using the estimations from Model 1, I follow Gandrud (2015) and King, Tomz, and Wittenberg (2000), and in Figure 3, simulate 10,000 times the hazard rate of implementing the income tax law, conditional on industrial and agricultural growth rates.¹⁹ Since the hazard rate “is the probability that a case will fail at time t ” (Licht 2011, 231), I take advantage of this quantity of interest which allows some dependency on both time *and* the covariates (Box-Steffensmeier and Jones 2004, 15). Figure 3 strongly suggests that the faster the agricultural sector develops, the less likely the implementation of the income tax. This relationship does not change at later stages of development, suggesting that polities with a strong agricultural elite are not associated with fiscal development. However, rapid industrial development is associated with an earlier implementation of the income tax law.

II. Positive Spillover Effects of Income Taxation: Chile

Fiscal sociologists, mostly focusing on the continental cases, have for a long time claimed that the capacity of taxing individuals’ incomes fosters overall state capacity. Unfortunately, there are no attempts to study this relationship for the Latin American cases. The results presented in this paper find support for this claim.

Previous section suggests a strong relationship between levels of sectoral conflict and the implementation of the income tax law in most Latin American countries. The stronger the industrial sector, the sooner the tax is implemented. This section seeks to establish an empirical link between the implementation of the tax, and higher levels of state development. Due to data availability, this section focuses on Chile only.

I constructed a novel hand-collected longitudinal dataset using the *Significant Earthquake Database* compiled by the National Centers for Environmental Information (NOAA) as a starting point (NGDC/WDS, National Geophysical Data Center / World Data Service 2015). The dataset “contains information on destructive earthquakes from 2150 B.C. to the present,” such as magnitude, date, latitude, longitude, number of deaths, among other variables. Tsunami casualties were excluded. Additionally, since “most of the damage in major earthquakes occurs within 30 km of the epicenter” (Dunbar, Bilham, and Laituri 2003, 172), earthquakes that did not happen on land were not dropped. While the epicenter might have been a few miles away from the shore, the consequences certainly reached the land.

Using archival census data from 1907 to 2012,²⁰ the NOAA dataset was complemented with local population measures at the municipal level where the quake hit. The local population was used to weight the death toll.²¹ Adding archival census data, I coded the main economic activity of the affected municipality²² and whether the municipality was urban or rural.²³ The death-tolls and magnitudes proportionated by the NOAA dataset were contrasted case by case with historical press archival information.²⁴ Magnitudes, in particular, were also compared with the International Seismological Centre.

Figure 4 plots the over time variation, while Figure 5 plots the geographical variation, and the dominant productive sector at the municipal level. Both figures suggest that Chile is a good case to study infrastructural state capacity using the earthquake framework: Chile has considerable variance regarding quake magnitudes, locations, and sectoral variation. The northern part of Chile has historically been an industrial region, while the southern part of Chile has traditionally been agricultural. Both have been affected by earthquakes in a similar fashion. Relatedly, both regions vary according to their climate (which correlates with agriculture). Furthermore, the distance from Santiago, which is located near latitude 33°, might impose some degree of difficulty for the central government to reach the farthest northern/southern parts of the territory (Foa and Nemirovskaya 2016, 418). However, the central part also has considerable earthquake activity. There is also variance considering longitude. Closeness to the Andean mountains (around longitude 70°) determines the ruggedness of the terrain, presumably making it harder for the state to penetrate these areas. In fact, Brancati (2007, 729) explains that “[e]arthquakes often occur in mountainous areas.” All things considered, earthquakes have affected the territory from coast to mountain, both north and south, close and far away from Santiago, and in both agricultural and industrial areas, solving potential

concerns about geographical sectoral self-selection. For instance, it would have been a problem that a specific sector, say the industrialists, were located *only* in the northern part, which is the most earthquake-prone area.

Following conventional wisdom, the unit of analysis is the earthquake (Kahn 2005, 273). As an event, each earthquake is associated with a death toll, a location, a magnitude, a local population, and an urban/rural setting. Following statistical convention, a count model was used to test the effect of implementing the income tax law on earthquake death tolls over time (Anbarci, Escaleras, and Register (2005, 1907), Kahn (2005, 276), Brancati (2007, 729), Escaleras, Anbarci, and Register (2007)), specifically a Bayesian Poisson regression was employed.

The main quantity of interest is an interaction term— β_3 in [Equation 1](#)—between earthquake magnitude and a binary variable that denotes whether the income tax had been implemented at the time of the event. The idea is to inspect whether the baseline propensity of the earthquake's magnitude of increasing the death toll is modified when combined with the introduction of the income tax. The null hypothesis is that the income tax does not alter this baseline propensity. The alternative, however, is that introducing the income tax decreases (increases) the propensity.²⁵

In addition, the local population was used to proxy for local economic development (Sokoloff and Engerman 2000). Since the idea is to account for the state's ability to enforce building codes and zoning laws, it is important to control for the capacity wealthier localities might have had to enforce those norms on their own—i.e. without the need of the state.

Latitude was included to control for the proximity to the Andean mountains, aiming to control for a built-in tectonic earthquake predisposition. Longitude seeks to control for climate and other unmeasured conditions that make agricultural development more difficult. In turn, both measurements serve as good proxies of terrain ruggedness and the difficulties the state faces in reaching these areas.

Undoubtedly, there are many more factors that might increase death tolls. Ambraseys and Bilham (2011, 154), for example, explain that the “number of fatalities depends on whether an earthquake happens at night or during the day, in the winter or in the summer, in a mountainous region or in a valley, after strong and protracted fore-shocks and with or without warning.” While the model has some of these factors accounted for, complete hourly data is lacking. However, Lomnitz (1970, 1309) explains that “some of the larger Chilean earthquakes which have caused deaths” between the 1900’s and the 1960’s have been afternoon quakes. Other factors such as “the

speed of tectonic movements [and] the degree to which the lower plate bends the upper plate” and the focal depth (Keefer, Neumayer, and Plümper 2011, 1534) could not be included due to the lack of complete data over time.

All in all, the next equation was fitted:²⁶

$$\begin{aligned}
 \text{Deaths} &\sim \text{Poisson}(\lambda_i) \\
 \log(\lambda_i) = &\mu + \beta_1 \text{Magnitude}_i + \beta_2 \text{Income Tax}_i + \\
 &\beta_3 \text{Magnitude}_i \times \text{Income Tax}_i + \\
 &\beta_4 \text{Population}_i + \beta_5 \text{Longitude}_i + \\
 &\beta_6 \text{Latitude}_i + \beta_{7,k} \text{Sector}_i
 \end{aligned} \tag{1}$$

where

$i_{1,\dots,I}$ and $I = 103$ events;

$k_{1,\dots,K}$ and $K = 3$ sectors.

While Table 2 shows the estimated results, the coefficient of the interaction term (i.e. β_3) remains uninterpretable from a substantive standpoint (Brambor, Clark, and Golder 2006, 74). The problem becomes more complex when it comes to generalized models (such as Poisson models), as a number of challenges arise. In an important paper, Ai and Norton (2003) explain that the interaction effect could be nonzero, even when the coefficient says it is zero; the statistical significance of the interaction effect cannot be tested with a simple t-test on the coefficient of the interaction term; the interaction effect is conditional on the independent variables; and that the interaction effect may have different signs for different values of covariates. In addition to all these challenges, and given that cross-partial derivatives are also not advisable, simulation methods are required (Zelner 2009; King, Tomz, and Wittenberg 2000). This procedure samples via simulation from the point estimates, generating a new and larger distribution. That is, taking the single estimated parameters (i.e. the regression coefficients), a new distribution of estimated values for each coefficient is constructed. Relying on the central limit theorem, the new simulated distribution is a transformation that approximates with a great degree of precision the (uninterpretable) coefficients with enough sampling draws.

Subsequently, means and uncertainty measures can be constructed for each of these distributions.

Fortunately, Bayesian methods have embedded a systematic and intuitive framework to solve these challenges. In particular, for every coefficient, I estimated five chains with 300,000 iterations per chain. Considering the Monte Carlo Markov Chain properties, the first 30,000 iterations of every chain were discarded. Following Brambor, Clark, and Golder (2006), [Figure 6](#) shows the conditional effect of earthquake magnitudes on implementing the income tax.²⁷

Substantively, [Figure 6](#) shows that the death toll systematically *decreases* over time—i.e. levels of state capacity systematically increase over time—*once the income tax law is implemented*. Particularly, by implementing the income tax, the baseline propensity of an earthquake increasing the death toll decreases from an estimated over time average of eighteen to an estimated over time average of five.

As argued before, a potential issue might be that sectors self-select into more/less earthquake-prone geographical locations. For instance, it might be argued that agricultural areas, being mostly rural, might have lower constructions and less populated areas, potentially having lower earthquake death tolls. Industrialists, in turn, being a more urban-oriented sector, might have highly populated municipalities and higher constructions (edifices), potentially showing higher death tolls.

To rule out this possibility, $\beta_{7,k}$ in [Equation 1](#) has a hierarchical structure which allows having three different intercepts, one per every k sector. In other words, this parameter shows whether earthquake death tolls are systematically higher in agricultural, industrial, or mixed municipalities. [Table 2](#) strongly suggests that industrial areas do not have higher earthquake death tolls when compared to agricultural areas. The posterior predicted means for the former are 41 deaths and for the latter, 43 deaths. Similarly, mixed municipalities have a predicted death toll of 44.

Finally, it could be argued that the idea of sectoral conflict is overstated and that they were not conflicting sectors. Some historians claim that there was just one economic and political elite, invested in both sectors. Since landowners also invested in industry (Kirsch (1977, 57, 95), Bauer (2008), Coatsworth and Williamson (2002, 23)), it could be argued that there was a blurry class division between the industrial and agricultural sectors (Bauer 2008, 30, 44, 94, 108). Perhaps the most cited reference regarding this issue is Veliz (1963, 231-247).

However, there are a number of stylized facts that strongly suggest that there was indeed a structural cleavage between the two sectors. For example, it was common that industrialists invested in real estate, yet, in many instances, they did so *just* to obtain credit. Kirsch (1977, 59) explains

that “in a *rural society* land offered one of the best guarantees for loans [since] loans could not be secured by equipment, machinery, or inventory. Only real estate was acceptable collateral.”²⁸ In fact, this practice shows how the credit system was oriented to give an unfair advantage to the landed elites. Unda (2017, 9), for instance, explains that in Mexico, industrial elites complied with the income tax in exchange for having a credit system more adequate for them. Similarly, Zeitlin (1984, 174) finds “the combined ownership of capital and landed property was a distinctive quality of *certain* [elites] actors,”²⁹ not *all* their members. There were also other instances where miners invested in banking, yet, Segall (1953) argues that Chilean bankers, after the crisis of the mining sector around the 1870s, had acquired a number of mineral deposits given as collateral years before. And finally, but for the Argentinean case, Hora (2002, 609) explains that “the image of an entrepreneurial elite with assets scattered throughout several spheres of investment does not appear entirely correct.”

There are also structural reasons to believe that cross-sectoral investments were not efficient. The “dual sector” model argues that the economy is divided into agriculture and industry (Jorgenson 1961, 311). One finding of this paradigm is that the “natural” structural role of the agricultural sector is to provide labor and cheap foodstuff to the industrial sector (Ranis and Fei (1964, 114), Reyes (2015, 129)). For instance, Dixit (1973, 326) argues that the “agricultural sector *must* fulfill [...] its dual *role* of supplier of labour to industry and of food for the industrial labour force.”³⁰ The rationale is that more efficient agricultural techniques make agricultural production less labor intensive, allowing landowners to free workers that the industrial sector can rely on (Johnston 1951, 498). A surplus of labor naturally leads to a reallocation of redundant workers into the industrial sector, which is the crux of economic development (Ranis and Fei (1964, 7), Leibenstein (1957, 51)). Nurkse (1953), in fact, argues that development means to employ the surplus labor. Similarly, Matsuyama (1991, 621-622) points out that “[i]ndustrialization [*consists of*] a shift of resources from agriculture to manufacturing.”

V. DISCUSSION

The paper sketched an argument about how higher levels of sectoral contestation increased state capacity over time. Particularly, it explained how the emergence of industrial elites posited credible threats to incumbent landowners, pushing agricultural and industrial elites to reach agreements that materialized in investments in state-making institutions (the income tax), which in turn fostered

higher levels of state consolidation over time.

The empirical analyses showed that earthquake death tolls decrease (i.e. state capacity increases) after the income tax law is implemented and that the emergence of the industrial sector accelerated the implementation of the income tax.

Enforcing quake-sensitive building codes embodies the most basic form of social contract that exists between the state and its subjects. Earthquake damage poses a major threat to commercial, official, and residential buildings, potentially triggering higher levels of looting and social unrest. Any kind of political leader should be interested in preventing looting and social unrest. Leaders not only care about their own survival but also about the legitimacy of the state. In the event of heavy social unrest, not only is the essential social Hobbesian-like contract broken, but the expectations of social peace are also questioned (Carlin, Love, and Zechmeister 2014, 419). The physical presence of the state literally crumbles when institutions of social coercion and discipline, such as state schools, prisons, and police stations, collapse. For example, when the magnitude 7.0 earthquake hit Haiti in 2010, the *Prison Civile de Port-au-Prince* had a population of 4,500 inmates. During the quake, five inmates died. As a prison guard describes it, “everyone escaped. Everyone. Except the dead.” This natural disaster exacerbated the already existent chaos, freeing “gang bosses, kidnappers, gunmen,” among others (Reed 2011; Laursen 2010), reducing the legitimacy of the state to zero.

Finally, income taxation did even more than just trigger other state capacities. Via a process of assimilation, it also helped in constructing the figure of the citizen, centered on the concept of the taxpayer. Regardless of an individual’s race, religion, culture, or any other kind of status, the state classifies its subjects according to their incomes and obliges them to pay, punishing whoever refuses to do so. From a sociological standpoint, this “generality makes taxation a crucial element in the development of the “imagined community” (Anderson 2006), of the modern nation-state [...] Taxation enmeshes us in the web of generalized reciprocity that constitutes modern society” (Martin et al., in Martin, Mehrotra, and Prasad 2009, 3).

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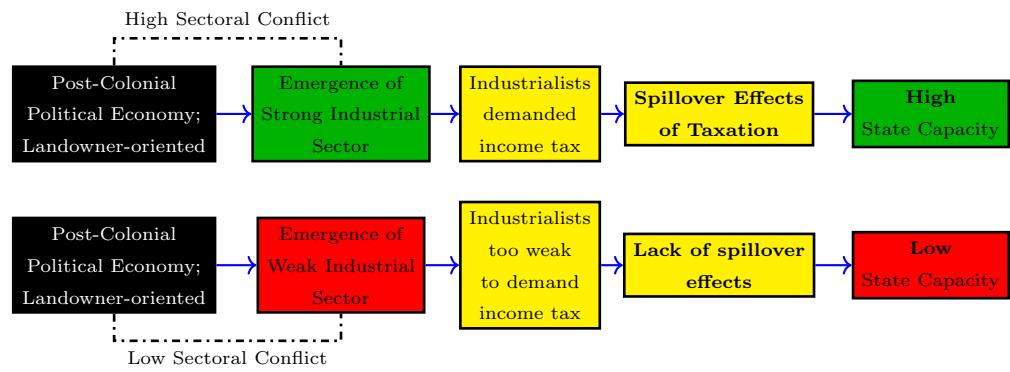


Figure 1: Causal Mechanism.

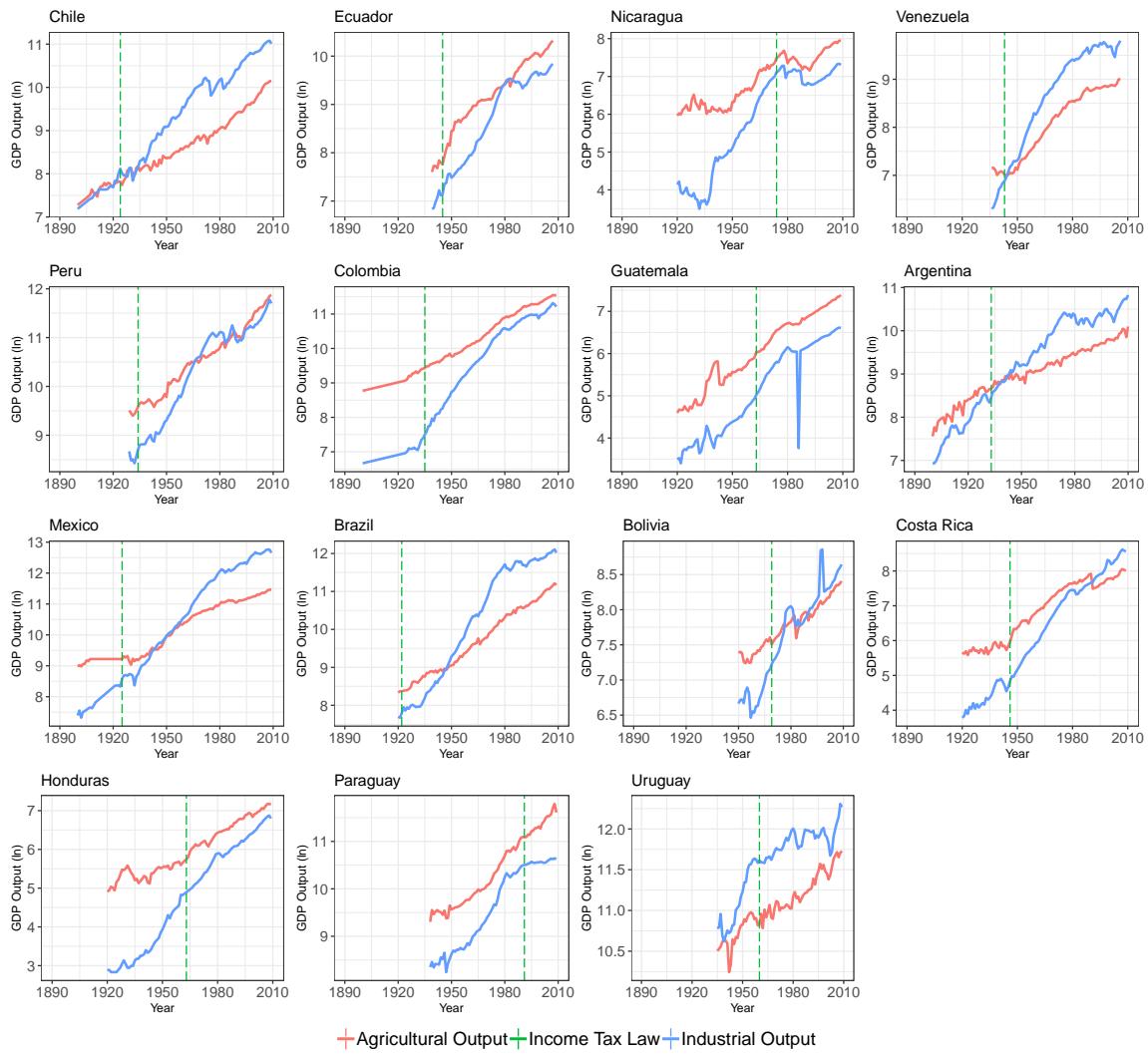


Figure 2: Industrial and Agricultural Outputs, and The Passage of the Income Tax Law in Latin America.

Note: Figure shows historical sectoral outputs, and year of the passage of the income tax law. Following convention, the figure shows logged values. Figure shows all countries for which there were complete data.

Source: *MOxLAD*, and other sources compiled by the author (see *Table OA1*).

	(1) Cox (1 lag)	(2) Logit GEE	(3) Conditional Logit (FE)
Manufacture Output _{t-1}	2.033*** (0.682)		
Agricultural Output _{t-1}	-2.438*** (0.857)		
Total Population	0.000* (0.000)		
Manufacture Output (ln)		1.384*** (0.357)	0.227** (0.095)
Agricultural Output (ln)		-1.827*** (0.421)	-0.330* (0.173)
Total Population (ln)		1.424*** (0.408)	0.983*** (0.225)
AIC	33.311		9043.609
R ²	0.049		0.223
Max. R ²	0.110		0.997
Num. events	15		1258
Num. obs.	415	1658	1658
Missings	0		0
PH test	0.073		
Num. clust.	20		

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Robust standard errors, models 1 and 2. Country fixed effects in 3. Intercept omitted.

Table 1: Sectoral Origins of Income Taxation: Income Tax Law and Industrial Development

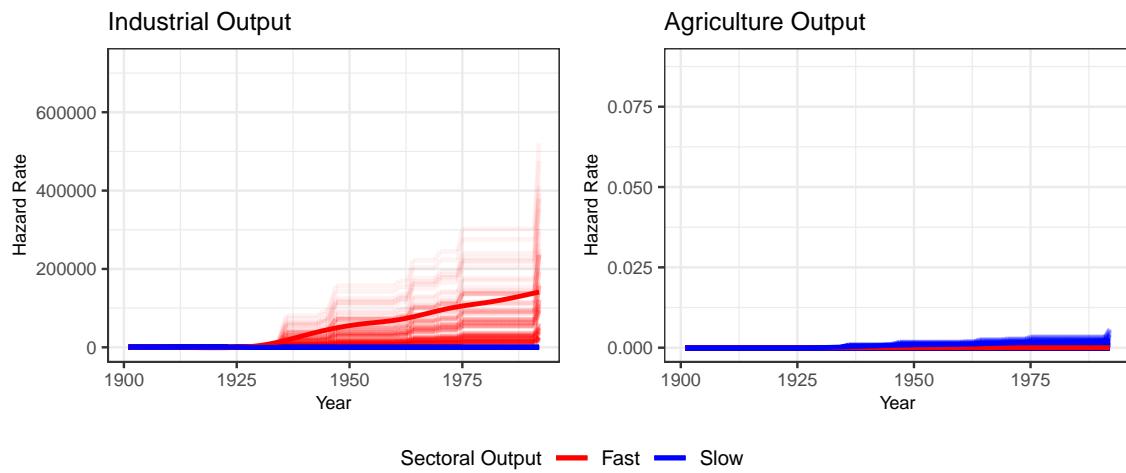


Figure 3: Hazard Rate of Implementing the Income Tax Law in Latin America.

Note: Using estimations of Model 1 in Table 1, figure shows 1,000 simulations with different sectoral growth speeds. ‘Slow’ is the minimum value, while ‘rapid’ is the maximum value for each sectoral output. The figure also shows the 95% confidence intervals.

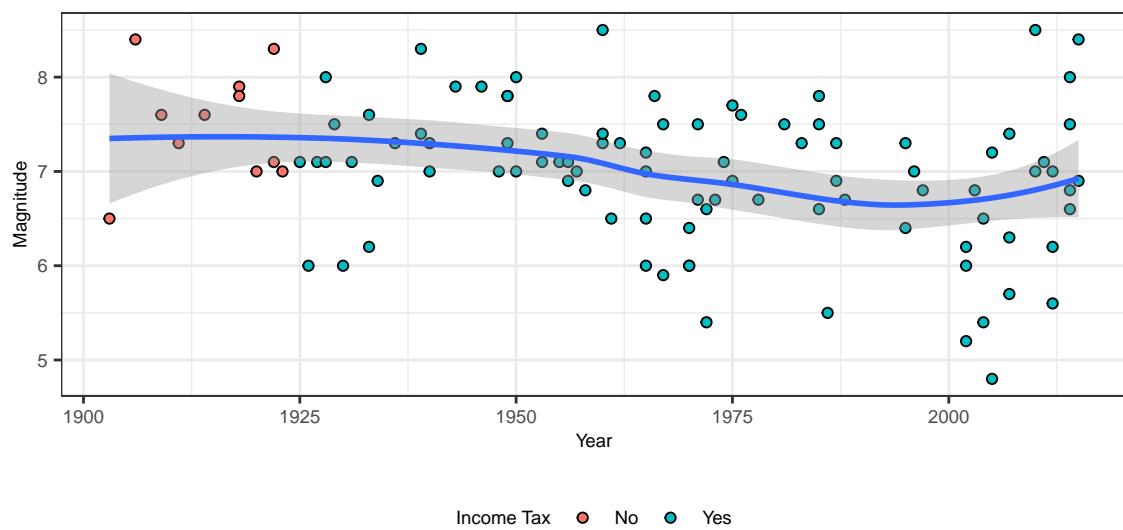


Figure 4: Earthquakes in Chile: 1903-2015.

Note: Figure shows earthquakes over time ($N=103$). Additionally, the figure shows earthquakes before and after the implementation of the income tax in 1924. A smoothing function was added to show that there are not statistically significant different decreases/increases in magnitudes over time.

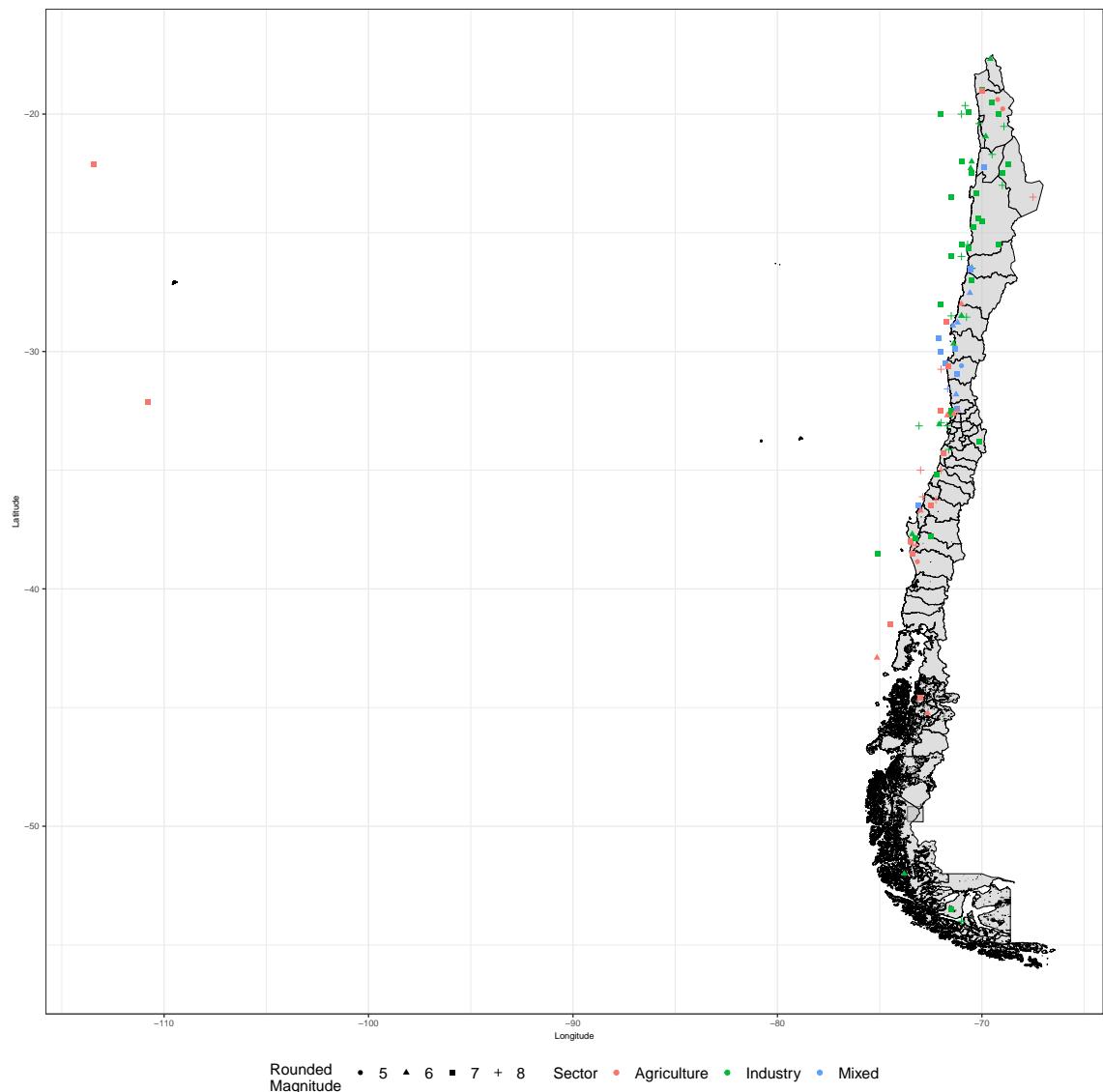


Figure 5: Data Used in the Analyses: Geographical Distribution of Earthquakes in Chile, 1903-2015.

Note: The figure shows a total of 103 earthquakes using a combination of archival information and external sources. Each quake was colorized according to the predominant economic sector at the municipal level. In total, there were 31 earthquakes that took place in agricultural localities, 56 in industrial, and 16 in mixed municipalities.

	Mean	SD	Lower	Upper	Pr.
Income Tax	108.43	3.47	101.67	115.09	1.00
Magnitude	17.91	0.42	17.09	18.71	1.00
Income Tax * Magnitude	-13.36	0.41	-14.14	-12.55	1.00
Latitude	-0.87	0.01	-0.90	-0.85	1.00
Longitude	4.40	0.03	4.34	4.46	1.00
Population	0.04	0.00	0.04	0.04	1.00
Sector[Agriculture]	43.48	55.26	-67.06	151.20	0.79
Sector[Industry]	40.53	55.26	-70.06	148.34	0.77
Sector[Mixed]	44.26	55.26	-66.26	152.00	0.79

Note: 300,000 iterations with a burn-in period of n = 30,000 iterations discarded.

95% credible intervals (upper/lower bounds). All R-Hat statistics are below critical levels.

Standard convergence diagnostics suggest good mixing and convergence.

A total of five chains were run.

Table 2: The Spillover Effects of Income Taxation in Chile: Simulated Posterior Predictions (Poisson Regression, [Equation 1](#)).

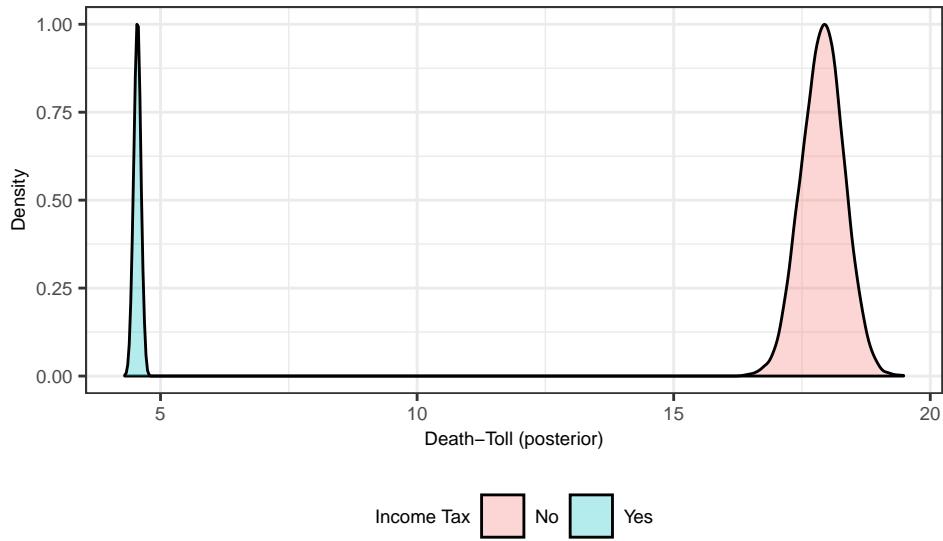


Figure 6: Conditional Effects of Earthquake Magnitudes on Implementing the Income Tax Over Time in Chile.

Note: Using the estimations from Table 2 (Equation 1), and following the advice of Brambor, Clark, and Golder (2006), the figure shows the conditional effect of earthquake magnitudes on implementing the income tax in Chile in 1924 ($\beta_1 + \beta_3 \times \text{Income Tax}_i$). Particularly, by implementing the income tax, the baseline propensity of the earthquake's magnitude of increasing the death toll decreases from an estimated overtime average of eighteen to an estimated overtime average of five. Hence, the figure suggests that implementing the income tax law had positive effects on state capacity over time. Both distributions were computed via a MCMC routine, particularly the iteration of five chains with 300,000 iterations per chain. Considering the Monte Carlo Markov Chain properties, the first 30,000 observations of every chain were discarded.

VI. ONLINE APPENDIX

Country	Available Data	Year Income Tax	Law	Source
Chile	1900 - 2009	1924	<i>Ley</i> 3996	Mamalakis (1976, 20) and LeyChile.Cl (official)
Peru	1929 - 2009	1934	<i>Ley</i> 7904	Gobierno del Perú (1934) (official)
Venezuela	1936 - 2006	1943	<i>Ley</i> 20851	<i>Gaceta Oficial</i> (official) and Ventura and Armas (2013, 27)
Colombia	1900 - 2009	1935	<i>Ley</i> 78	Figueroa (2008, 9)
Argentina	1900 - 2010	1933	<i>Ley</i> 11682	Infoleg.Gob.Ar (official)
Mexico	1900 - 2009	1925	<i>Ley de Impuesto sobre la Renta</i>	Unda (2017, 8)
Ecuador	1939 - 2007	1945	-	Aguilera and Vera (2013, 135)
Nicaragua	1920 - 2009	1974	<i>Ley</i> 662	Legislacion.Asamblea.Gob.Ni (official)
Guatemala	1920 - 2009	1963	<i>Decreto</i> 1559	Instituto Centroamericano de Estudios Fiscales (2007, 165)

Table OA1: Sample, Data Available, and Year the Income Tax was Implemented

NOTES

1. A few exceptions are Gallo (1991, 7-8), Beramendi, Dincecco, and Rogers (2016), and Saylor (2014, 8) who consider elite conflicts to study state-making and fiscal development in the developing world. Dargent, Feldmann, and Luna (2017) focus on a “‘challenger-based’ causal mechanism” of state formation in Peru; however, they concentrate their efforts on the role of exogenous economic shocks.
2. Similarly, McBride (1936, 15) explains that “Chile’s people live on the soil. Her life is agricultural to the core. *Her government has always been of farm owners. Her Congress is made up chiefly of rich landlords.* Social life is dominated by families whose proudest possession is the ancestral estate.” My emphases.
3. As Boix (2015) explains, lower levels of inter-elite economic inequality are tied to similar degrees of military capabilities. Under these circumstances, war is most likely to exhaust all existent assets without producing positive outcomes for either sector (Richard Salvucci, in Uribe-Uran 2001, 48), leading to heavier pressures to reach agreements instead of engaging in armed conflicts. In Chile, while initially both “antagonistic elites” (Keller 1931, 37-38) confronted each other in two bloody civil wars between a “large landed property [elite against a] productive capital[ist] [elite]” (Zeitlin 1984, 23), due to low levels of inequality, war was not sustainable over time. For instance, while *Balmacedistas* managed to secure the support of the army, *congresistas* (the anti-Balmaceda group) gathered support from the navy. Similarly, in the subsequent years of the civil war, there were a number of *aborted* coups, 1907, 1912, 1915, and 1919 (in Collier and Collier 2002, 109), suggesting an equilibrium where no elite was the leading elite.
4. Separate panel-data analyses, available upon request, also confirm this hypothesis.
5. My emphases.
6. These are situations in which “an increase in the output of [a] commodity [...] lowers the marginal costs of producing [other] commodity” (Hirschman 1958, 67).
7. My emphasis.
8. My emphasis.
9. See, especially, article 151.
10. I thank [] for this comment.
11. Particularly, the *agriculture value-added* and *manufacturing value-added* variables. Both are expressed in local currency at 1970 constant prices.
12. Since countries are “censored” once they implement the income tax law, they leave the sample (potentially) before 2010. According to Astorga, Berges, and Fitzgerald (2005, 790), these data provide extended comparable sectoral value-added series in constant purchasing power parity prices. These data, and similar strategies, have been employed before (see Thies 2005)
13. All tables were produced using the `texreg` package (Leifeld 2013). All Cox models were computed using

the **survival** R package (Therneau 2015). The GEE logistic regression was computed using the **geepack** package (Hojsgaard, Halekoh, and Yan 2016). The simulations were performed using the **simPH** R package (Gandrud 2015).

14. Following Box-Steffensmeier and Jones (2004, 49), the next equation was fitted:

$$h_i(t) = \exp(\beta_1 \text{Industrial Growth}_{i,t-1} + \beta_2 \text{Agricultural Growth}_{i,t-1} + \beta_3 \text{Total Population}_{i,t-1}) h_0(t) \quad (2)$$

for all countries i and years t .

15. Following Zorn (2006, 331), the next equation was fitted:

$$\pi_{i,t} = \Phi(\beta_1 \log(\text{Industrial Growth}_{i,t}) + \beta_2 \log(\text{Agricultural Growth}_{i,t}) + \beta_3 \log(\text{Total Population}_{i,t})) \quad (3)$$

where π is the logit link function, and Φ is as scale parameter (i.e. the cumulative distribution function), for all i countries, and t years.

16. GEE methods require analysts to parameterize the working correlation matrix. Though GEE are robust to misspecified correlation structures (Hedeker and Gibbons (2006, 139), Westgate and Burchett (2017), Gardiner, Luo, and Roman (2009, 227), Carlin et al. (2001, 402)). Zorn (2006, 338) explains that whereas the choice of estimator makes little or no difference, the unit on which the data are grouped makes a big difference. Hence, following Hardin and Hilbe (2013, 166) the “independence” working covariance structure was used, which also corrects for small-sized panel designs.

17. More formally,

$$\pi_{i,t} = \Phi(\beta_0 + \beta_1 \log(\text{Industrial Growth}_{i,t}) + \beta_2 \log(\text{Agricultural Growth}_{i,t}) + \beta_3 \log(\text{Total Population}_{i,t}) + \alpha_i) \quad (4)$$

where α are the country fixed effects for all countries i .

18. I thank [redacted] for this suggestion.

19. Box-Steffensmeier and Jones (2004, 15) explain that the hazard rate is the most common quantity of interest analysts focus on. Figure 3 shows 95% confidence intervals.

20. Particularly, censuses of 1907, 1920, 1930, 1940, 1952, 1960, 1970, 1982, 1992, 2002 and 2012. Some of them were kept at the *Biblioteca Nacional* and others at the *National Statistic Institute* historical library.

21. Around 90% of the time it was possible to recover the actual local population. For the rest, the population of the most concentrated area nearby was recovered. Consequently, population is used as a control, not to weight the dependent variable directly.

22. This variable was constructed by coding press and official sources (mainly censuses) of the main economic sector at the local level: *Agriculture* (n=31), *Industry* (n=56), *Mixed* (n=16).

23. Urban=85, rural=18. If more than 50% of the population lived in an urban setting, I assigned a 1 to that municipality, 0 otherwise. Urban concentrations are most likely to have vertical constructions rather than one-story buildings, increasing the potential number of casualties. Consequently, it is important to control for this source of variation. I thank [redacted] for this suggestion.

24. *El Mercurio* and *La Nación* newspapers, both kept at the *Archivo Nacional* of the *Biblioteca Nacional de Chile*.

25. While the main model does not include year fixed effects (due to multicollinearity issues), another model (not shown) including year fixed effects was fitted. The results are the same. The advantages of the fixed effects model is that it accounts for country-specific prior state capacity levels and other unmeasured yearly factors. I thank [redacted] for this suggestion.

26. All parameters $\beta \sim \mathcal{N}(0, 0.0001)$, all precisions $\tau \sim \mathcal{G}(1, 1)$, while $\mu \sim \mathcal{N}(0, 0.0001)$. Traceplots—not shown but available upon request—indicate that the model has good mixing.

27. Based on [Equation 1](#), the simulated matrix was constructed following the routine described above, iterating over the next generalized equation: $\beta_1 + \beta_3 \times \text{Income Tax}_i$.

28. My emphases.

29. My emphasis.

30. My emphases.