Economic Channels for Influence Over Governments

by

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Duke University

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Dissertation submitted in partial fulfillment of the requirements for the degree of
Doctor of Philosophy in the Department of Political Science
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Abstract

This dissertation focuses on how economic markets provide channels for influence over government policy. Specifically, I examine three levels of analysis: the household, the financial security, and the foreign state. Economic constraints on government policy are particularly salient in today's financialized economy. Understanding these dynamics helps us forecast what will happen in the future. Getting these forecasts right is important because taxpayers, governments, and investors all have skin in the game of effective use of government resources. To paint a picture of these constraints, my dissertation contains three papers. The first argues that individuals with access to economic insurance are less likely to protest than those without. Using macroeconomic and survey data, I find evidence supporting my theoretical expectations. The second paper turns from household economics to the financial markets for government debt securities. Although the literature shows how governments make certain choices in debt issuance and the pricing dynamics of government bonds, it remains unclear how the ownership structure of debt affects yields. I argue that government bonds with more concentrated ownership structures have higher price volatility, which should incur volatility risk premium as a result. I find evidence supporting my theoretical expectations. This paper speaks to the relationship between debt ownership and power; it matters because governments with more concentrated debt ownership could see higher debt service payments over time. The third paper considers how state actors can use foreign investment as a policy tool. I argue that

Chinese actors increase investment in target countries when future policy is more uncertain because investments act as a hedge against the possibility of unfavorable future policy. This runs counter to the traditional narrative, which suggests that foreign investment is more likely when policy is stable. Using a novel cross-national, high-frequency, machine-coded event data set, I find evidence supporting my expectations. My dissertation paints a picture of the breadth of ways that economic markets influence government policy. Governments contend with the economic interests of constituents who can demonstrate publicly, investors who can affect the price of their debt, and other states that can use investment to secure influence over future policy.

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1

Introduction

This dissertation focuses on how various economic markets provide channels that enable influence over government policy. In particular, I focus on three levels of analysis: the household, the financial market, and the foreign state.

Political scientists are very familiar with how individuals relate to and interact with the government as well as how governments act. But, as a discipline, political science spends less time focusing on how governments fund themselves and the effects of economic constraints on government policy.

These constraints are particularly salient in today's financialized economy. For instance: in 2021, the United States (US) federal government spent $6.82T but took in only $4.05T in taxes (United States Department of the Treasury, 2022). The US government does not operate profitable state-owned enterprises or sell natural resources, so the almost $3T difference is borrowed, mostly via debt securities on financial markets. It's a huge amount of money, to be sure. This trend has only grown over time, especially because of fiscal stimulus during COVID.

Furthermore, almost three-quarters of 2021 US government spending went towards social benefits including income security, social security, healthcare, medicare,

commerce and housing credits, education and other social services, and veterans benefits (United States Department of the Treasury, 2022). The way that the government funds itself has direct policy implications for these expenditures, and the ways that individuals experience economic turmoil affects what they demand of government spending.

So, given this salience, understanding economic channels for influence over governments is important. Knowing the effects that household finances and financial market movements have on government finances helps us forecast what will happen in the future. Getting these forecasts right is important because taxpayers, governments, and investors all have skin in the game of effective use of government resources.

Making predictions about the policy effects of these economic constraints is not an idle academic exercise. On the contrary, it is more crucial than ever. Individuals, investors, and governments allocate large amounts of public and private money based on their belief that certain facts are true. The costs of those facts not being true are huge. So, to paint a picture of these constraints, my dissertation contains three papers that consider different angles of economic channels for influence over governments.

1.1 Household Economics and Protest

The first paper starts with the frame of analysis that many political scientists understand most intuitively: the individual. Research in political science and economics has noted a connection betweeen economic conditions and political participation. This relationship can take a variety of forms, most of which rely on voting as a method of political participation: political business cycles, clientelism, even corruption. A smaller subset of this literature considers the relationship between economic circumstances and a different kind of political participation: protest. But the eco-

nomics and protest literature so far only considers a vague notion of "bad economic times," without defining clearly what that means.

Yet, an individual's economic circumstances are determined by a multitude of things. First, and most obviously, it is determined by their income. It is with good reason that most of the literature on protest and economics focuses on the way that income affects protest. But income is not the only aspect of household finance. Privately accrued wealth is important also because it has the potential to be used to smooth consumption in the case of income loss. Moreover, governments provide various kinds of social insurance that play a role in people's economic circumstances. Together, I consider private wealth and social insurance to be kinds of "insurance," a pool of funds designed smooth consumption during an income shock.

To the extent that protests occur in reaction to income shocks, the presence of insurance should moderate the effect. By disaggregating insurance from income, this paper contributes a nuanced understanding of individual's economic circumstances to the literature on economics and protests. Moreover, this paper expands on the social insurance literature, which so far has only examined the effect of economic insurance on voting.

1.2 Government Bond Ownership Concentration, Volatility, and Yield

The second paper turns from household economics to financial markets, specifically the market for government debt securities. This paper was motivated by a thought experiment about the effects of being too dependent on one creditor for financing: two governments who borrow the same amount of money on the bond market could experience debt very differently based only on the number of creditors who buy their debt. While economics and political science literature shows how governments make certain choices about the maturity profile, currency distribution, amount, and yield structure of their debt issuance, and finance literature examines pricing dynamics of

government bonds, it remains unclear how the ownership structure of debt affects yields.

I argue that government bonds with more concentrated ownership structures have higher price volatility because they are more subject to price movements when large holders buy and sell. As a result, these securities are likely to have a volatility risk premium; in other words, investors should be willing to pay less for bonds whose prices vary more.

But inherent in this discussion is the possibility of endogeneity: a debt security's issuer's "riskiness" could simultaneously affect yield, by charging a higher premium for riskier debt, and ownership concentration, by affecting the kinds of investors who are likely to accrue large positions. However, I argue that riskiness likely does not affect ownership concentration because the process that drives an investor to enter the market for a security is different from the process that drives the size of a position they take once they enter the market. The position size is likely affected by things other than risk, including available capital, investment strategy, kind of investment vehicle (pension fund, mutual fund, insurance company, etc.), and behavioral and demographic characteristics of the investor.

So far, political economy literature has clearly shown ways that financial markets affect governments and vice versa. But so far, the literature has not been able to connect the ownership structure of particular securities to particular implications for governments. This is my contribution in this paper: I show the pricing implications of ownership structures in government debt markets. This matters because my results suggest that governments with more concentrated debt ownership could see higher debt service payments up over time, contributing to poor fiscal balance.

1.3 China's Foreign Investment: Hedging Against Policy Uncertainty

The third paper generalizes one step further, considering how state actors can use economic markets as a strategic policy tool. More specifically, because of the tight connection between its government and economy, I focus on China's outbound investment.

Traditionally, the realm of economic policy making has been constrained to areas such as trade policy, currency policy, and government-financed development assistance. The academic literature has examined China's behavior in these markets very thoroughly and come to complex, often conflicting conclusions. For example, there is debate in the literature about the extent to which China's outbound development aid is motivated by its own interests, regardless of the recipient's credit. Other research has recently shown that Chinese loans come with political conditions attached such as requiring that the recipient country does not harm any Chinese entity present in that country.

I expect that similar political ends undergird China's participation in other international economic markets. In particular, because of the tight connection between its government and economy, Chinese actors are able to use investment in foreign countries as a hedge against policy uncertainty there. This runs counter to the traditional narrative of foreign investment, which suggests that investment is more likely when policy is stable and property rights are guaranteed. I propose that Chinese entities treat foreign investment more like financial market investors do, where uncertainty about the target country's future policy presents a good opportunity to solidify policy influence in the target country through the influence channels that accompany owning an asset.

This paper makes several contributions. First, I show conclusively that China does in fact conduct influence operations abroad, providing evidence against its

stated non-interference policy. Second, I show that outbound investments in particular serve as a hedge against policy uncertainty. This runs counter to the traditional narrative and provides evidence that the unique connection between the Chinese government and economy means that its investment takes a unique form for states. Third, I expand the discussion of China's presence in international economic markets by finding evidence that state-affiliated actors use public economic markets for strategic political purposes.

1.4 My Contribution

Each chapter of this dissertation alone contributes a substantive expansion in the state of knowledge on its topic. But taken together, my dissertation paints a picture of the breadth of ways that economic markets influence government policy. Today's financialized, globalized economy provides many opportunities for growth, but it also provides channels to influence governments. Chapters 2 and 4 show scenarios in which these channels could influence policy itself, but policy does not have to be the target of the influence. Chapter 3 shows a channel through which financial markets can influence government finances.

Generally, these essays illuminate ways that economic markets confer political power or weakness. At the household, financial security, and inter-state level, governments are subject to a variety of policy influences. More concretely, governments have to contend with the economic interests of constituents who can demonstrate publicly, investors who can affect the price of their debt, and other states that can use investment as a way to secure influence over future policy.

Chapter 2 shows that the literature's existing understanding of how household economic circumstances affect protest participation is incomplete. These findings have implications for the way scholars view political participation.

The theory and findings in Chapter 3 have implications for the democratic peace

literature and markets peace literature through the lens of the relationship between power and concentration of debt holding. For example, is there something called a debt peace? Does China have power over the US because it holds a large amount of US government bonds? Or does the US instead have control over a portion of the Chinese balance sheet? Chapter 3 speaks to a general problem about whether structure of debt ownership confers power or weakness.

Chapter 4 has major implications for the study of foreign direct investment (FDI), suggesting that China is a significant exception to the scholarly consensus that FDI is higher in stable circumstances. In doing so, Chapter 4 shows that the literature's traditional understanding of the different ways that countries and financial market actors handle risk may require more nuance.

Taken as a whole, this dissertation contributes a detailed picture of different ways economic markets can serve as channels for influence over governments.

2

Household Economic Insurance and Protest Mobilization

2.1 Introduction

Protests emerge in response to economic issues as varied as increasing prices of goods, financial crises, and poor employment prospects. Protest dynamics are well studied in the literature, but we still know little about why protests occur in response to some economic conditions and not others. For example, protests erupted from government removal of subsidies in Indonesia in 1998 and 2018, Bolivia in 2011, and Ecuador in 2019 (Maduz, 2011; Varagur, 2018; Romero, 2011; Valencia, 2019). Each of these protest events were substantial enough to force the government to reinstate the subsidy or even topple the regime. However, the removal of subsidies did not result in substantial protests in China in 2019, New Zealand in the 1980s, India in 2018, or even in Indonesia again in 2015 (AFP, 2019; Vitalis, 2007; Ross and Edwards, 2012; Saberin, 2018; Owen, 2015). If protests aim to resolve an unfavorable policy, why did protests emerge in response to some of these subsidy removals and not to others?

This paper seeks to answer the more general question: why do individuals protest under some economic conditions and not others? Research on protests provides a broad assessment that "bad economic times" are associated with more protest. However, the literature is quite unclear about what "bad economic times" actually are. I leverage the academic literature on government-funded social insurance to provide a richer account of why certain economic circumstances lead to protests and not others. The social insurance literature shows that individuals can compensate for a loss of labor market income with funds from elsewhere, allowing them to smooth consumption. I apply this logic to protest by arguing that when individuals cannot smooth consumption, they turn to an immediately available form of political participation: protest.

Before the mid-2000s, protests were limited to public demonstrations. But subsequent technological advances have enabled protests to arise on digital platforms (Kuran and Romero, 2018). For several reasons, I limit my conception of protest to the old-fashioned public demonstration. First, such protests are not constrained by exogenous timelines like voting is. Second, public demonstrations are more costly than social media posts. Third, such conception allows theoretical generalizability to both pre- and post-social media eras. Fourth, public demonstrations, however costly, are still possible in political environments that constrict digital expression.

To some readers, a connection between individual economic circumstances and protest might not be surprising. An abundant literature studies the relationship between economics and protest. However, this literature conceives of individual economic circumstances as only one-dimensional, consisting of only income; for many people, economic well-being is dependent on accumulated assets or support from the government in addition to income. This intuitive concept remains an assumption that has not been tested. I address it in this paper.

I argue that individuals gain utility separately from income and what I will call

"insurance", which can be either private (wealth) or public (social insurance). I define insurance to be a stock of funds that can be used as income when income suddenly drops. Although wealth and social insurance are not governed by an insurance policy, in general terms they both function similarly to car insurance: a stock of available funds grows by contributing money incrementally – albeit by personal savings or tax contributions instead of monthly premia – and can be liquidated and used as income if needed. If one's available insurance is negatively shocked, so is one's ability to smooth consumption. If one cannot adequately mitigate the shock via consumption smoothing, one can try to improve one's circumstances through political mobilization. Although protests may emerge from negative shocks to either income or insurance, I argue that the salience of an income shock to an individual is contingent upon her available insurance. If she has enough insurance to smooth consumption after an income shock, she is less likely to resort to protest.

I perform two tests of my hypothesis. In the first, set in Europe, I use a Panel Vector Error Correction model to show that increases in macroeconomic wealth indicators decrease the extent to which income shocks correlate with aggregated protest counts. In the second, I conduct two survey analyses, set in Europe and the United States (US), to show that income shocks do indeed motivate different protest behavior in individuals with different levels of insurance. Both tests find evidence consistent with two notions: first, that individuals protest when under economic stress, and second, that private wealth can compensate for lost income. My results help shed light on why protests occur in response to changes in some economic indicators and not to others: both insurance and income can affect protests independently, but each attenuates the effect of the other. I broaden the literature on economics and protest by showing that protests relate to wealth as well as social insurance and income. I also connect the concept of insurance to a different method of political participation than currently studied in the literature: protest rather than voting.

The next section of this paper will describe existing scholarship on economics and protest and explain its shortcomings. The third section will describe my theoretical framework in more detail and explain how it helps address these shortcomings. The fourth section will describe my empirical approach and the fifth will discuss results and robustness. The final section will conclude.

2.2 Background

Under a utility-maximizing framework, rational, self-interested individuals may have incentives to engage in costly political action (Lohmann, 1993). Therefore, much of the literature examining the relationship between economics and protest assumes that individuals use protest as a tool to remedy economic hardship. In support, it finds evidence correlating "bad economic times" and increased protest. However, there is one major issue with this literature's imprecise definition of "bad economic times": it usually ignores the role of other economic assets available to an individual. To capture economic well-being, work relying on survey data usually measures economic circumstances by respondent-reported income (Brady, Verba and Schlozman, 1995) or perception of economic circumstances (Muñoz and Tormos, 2015; Rüdig and Karyotis, 2014) while other research only discusses income with no mention of accumulated assets (Solt, 2008, 2015). This shortcoming muddies this literature's conclusions: no observer of contemporary politics would suggest that private wealth cannot fund political mobilization or that, holding income constant, protests cannot emerge in response to sudden drops in financial markets that torpedo savings or investment accounts. Because it ignores wealth, the literature also generally ignores the relationship between protest and an individual's ability to smooth consumption. It does not answer, for example, whether two individuals of the same income but different wealth endowments might assess differently the decision to protest upon being fired from their jobs.

The idea that "bad economic times" lead to protest has featured prominently in the social psychological literature as Relative Deprivation Theory (RDT). RDT anticipates mobilization to be most likely when expectations of advancement exceed actual experience or when development gains are sharply reversed (Gurr, 1970).[1] However, empirical adjudication of RDT has come to varying conclusions. While Snyder and Tilly (1972) find no evidence of correlation between mass discontent and collective violence in France between 1830 and 1960,[2] Opp (1988) finds that "grievances have a causal effect on [social movement participation] but not a direct cross-lagged effect on protest." Still others find evidence that high income inequality makes citizens less likely to engage in politics (Solt, 2008). In an attempt to reconcile these seemingly conflicting findings, Kurer et al. (2019) find that structural economic disadvantages demobilize individuals, but deterioration of economic prospects increases political activity. This hodgepodge of findings fails to explain why protests arise in response to downturns in some economic conditions and not others. Perhaps RDT is hindered by a theoretical framework that fails to distinguish income from wealth.

Of the major schools of thought connecting economics and protest, Resource Mobilization Theory (RMT) comes the closest to meaningfully engaging with the concept of wealth. McCarthy and Zald (1977), early proponents of RMT, argue that social movements must compete for resources with individuals' other needs such as food, shelter, and savings. However, the authors make no further mention of individual wealth beyond this and instead focus on characteristics of social organizations. A later modification of RMT called Civic Voluntarism Theory argues that individuals participate in politics if they have the resources and opportunity to do so; in

[1] Davies (1962) applies a similar theory, J-curve theory, to revolutions; it is also applicable to protests.

[2] As cited in Kuran (1991).

particular, Brady, Verba and Schlozman (1995) argue that an individual can draw upon both time and money to participate in politics. Except for a passing reference to money's ability to be saved, the authors do not discuss the concepts of wealth or consumption smoothing; they define "money" as income and move on.

By deciding to protest, an individual forgoes other potential benefits that she could have accrued during the time spent protesting. Numerous scholars have shown that education, wages, and employment are sources of opportunity cost (Jenkins, Jacobs and Agnone, 2003; Kimeldorf, 2013; Collier and Hoeffler, 2004; Dahlum, 2019). The concept of "biographical availability" sheds some light on the relationship between opportunity cost and protest. McAdam (1986) defines biographical availability as "the absence of personal constraints that may increase the costs and risks of movement participation, such as full-time employment, marriage, and family responsibilities." Hurst and O'Brien (2002) and Wallace and Weiss (2015) find evidence for protests emerging among biographically available individuals in China; McAdam (1986) and Amenta and Zylan (1991) find evidence for the same concept in the United States. Brady, Verba and Schlozman (1995) argue that while biographical availability depends on attributes such as age or gender, it is orthogonal to income or education. The authors find evidence that those with free time have higher civic participation than those without. Taken together, this scholarship suggests that upon losing one's job, even though it might depend on demographic characteristics, one's opportunity cost to protest decreases regardless of what job one has lost or how much money one made beforehand.

The above are all important contributions to understanding a phenomenon as complex as protest. However, they gloss over the important distinction between income and wealth. It is important to disentangle wealth and income because the pain of an income shock can be lessened by liquidating wealth to compensate for lost income (i.e. smoothing consumption). One nuanced take on this distinction

stands alone: Ozarow (2014) shows that consumption smoothing, particularly via the possession of capital assets, matters to how the poor weathered one particular economic crisis in Argentina. Specifically, Ozarow mentions that the "corralito" policy in Argentina prevented savings withdrawals in an economic downturn and thereby restricted consumption smoothing. He further shows that the poor who possessed other forms of capital (e.g., physical, human, social, or non-savings financial) had greater flexibility during the early 2000s economic crisis.

In addition to neglecting the concept of wealth, the scholarship examining economics and protest also fails to apply lessons from literature examining the role that economic insurance plays in individual decision-making. Kuran and Romero (2018) omit reference to insurance affecting protest in their otherwise thorough review of the topic. Likewise, Aytaç and Stokes (2019), despite a thorough theoretical treatment of the determinants of political participation, omit economic insurance from their answer to the question "Why Bother?". Perhaps the reason for its omission from these major reviews is that the major schools of thought on the topic do not explicitly link economic insurance to protest activity.

The political economy literature examining social insurance provides a more systematic approach to understanding the relationship between income and wealth. Moene and Wallerstein (2001) and Iversen and Soskice (2001) distinguish between income and some source of consumption smoothing. In the authors' theories, individuals treat government welfare spending as insurance against losing labor market income.[3] This is the distinction missing from the literature on economics and protest: individuals may have other sources of economic prosperity beyond labor market in-

[3] Moene and Wallerstein (2003: p. 486) give a clear analogy of the important conceptual difference between redistribution and social insurance. "[A]ll insurance policies are redistributive in the sense that fire insurance redistributes resources from those lucky enough to never experience a fire in their house to those who have the misfortune of experiencing such. Nevertheless, fire insurance is not redistributive ex ante. We do not expect fire insurance to be more popular among the poor than among the rich."

come such as social insurance, private wealth, or a partner's income. Although the social insurance literature does not explicitly state that individuals use wealth to smooth consumption, it makes one assumption that points in that direction. By stating that the rich have less need for government welfare spending, Moene and Wallerstein (2001) implicitly assume that the rich smooth their own consumption when faced with an employment shock. Zimmerman and Carter (2003) find evidence for exactly this: when faced with income loss, the rich usually liquidate assets to smooth consumption while the poor more likely sacrifice consumption to conserve assets.

Although it considers political participation via voting instead of protest, the social insurance literature helps clarify the relationship between shocks to income and public fiscal safety nets. Moene and Wallerstein (2001) seek to explain why government welfare spending is higher in some unequal countries and not others. If voters view welfare spending as redistribution from rich to poor, more unequal societies should have higher redistribution (Meltzer and Richard, 1981). However, if voters view welfare spending as social insurance, more unequal societies should demand less of it. The authors find that when welfare benefits are targeted only at the unemployed, welfare spending behaves like social insurance against income loss rather than redistribution from the rich to the poor. This could be because employed voters who aren't eligible for benefits decrease their support for this kind of welfare spending policy. In other words, only individuals who cannot adequately insure themselves privately against shocks seek policy redress. The authors clarify this finding in their 2003 paper, finding evidence that demand for social insurance rises with the risk of a negative shock to income (Moene and Wallerstein, 2003). Other authors have found corresponding empirical support for this concept as well. Rueda (2005: p. 64-65) finds survey evidence that individuals at higher risk of unemployment (a negative income shock) generally support more robust unemployment insurance and are willing to

pay more taxes for it.[4]

These findings in the social insurance literature point toward an important analytical distinction between income and other sources of economic well-being that can be used as insurance. This distinction is important for the relationship between economics and protest because it suggests economic crises can trigger income shocks, wealth shocks, or both. This difference might help explain why protest happens in some places at some times and not others.

Other literatures on political economy, sociology, and behavioral psychology explain the process of how individuals form preferences over social insurance. Iversen and Soskice (2001) and Ansell (2014) find that personal wealth affects preferences over social insurance. Carnes and Mares (2013) find, similarly, that these preferences stem from the level of satisfaction with the financial performance of existing social insurance systems. The same literature has also found that these preferences manifest in the political realm via participation in protests. People protest if their economic circumstances are uncertain, especially when they are subject to a less generous social insurance scheme (Dodson, 2016; Schmalz, Sommer and Xu, 2017). This collective action is motivated by a fear that results from economic uncertainty (Shi, 2019).

However, none of these papers note the conceptual similarity between an individual's available social insurance and her private wealth. Because of this, they leave unanswered the general relationship between broadly-construed "insurance" and protest. While this literature illuminates a variety of different correlates of protest, it begs the question: why is the interaction between insurance and income important to an individual's likelihood to protest?

[4] For other survey evidence, see also Boeri, Börsch-Supan and Tabellini (2001), as cited in Rueda (2005).

2.3 Argument

The aforementioned literature shows a relationship between an income shock or perceptions of "bad economic situations" and spikes in protest. I further develop this strand of literature by arguing that when individuals cannot smooth consumption, they turn to an immediately available form of political participation: protest. I argue that individuals gain utility separately from income and "insurance", which can be either private (wealth) or public (social insurance). Either kind of insurance can be used as income when income drops. If one's available insurance is negatively shocked, so is one's ability to smooth consumption to compensate for income loss. If one cannot adequately mitigate the income shock via consumption smoothing, one can turn to political mobilization to try to improve one's circumstances. Although protests may emerge from negative shocks to either income or insurance, I argue that the salience of an income shock to an individual is contingent upon her available insurance. If she has enough insurance to smooth consumption after an income shock, she is less likely to resort to protest. I develop the literature in two ways. First, I add nuance to the conception of "bad economic times" and its relationship with protest by showing that protests connect to wealth as well as social insurance and income. Second, I connect the concept of social insurance to a different method of political participation: protest rather than voting.

Explanations for social phenomena can be structural or individualistic. I offer an individualistic explanation of behavior in situations that can arise due to structural factors. To grasp this clearly, consider two hypothetical coworkers who lose their jobs because of layoffs induced by industrial automation. The two workers have the same income, but one has large savings and the other does not. Although the cause of their job loss is structural, I expect that the one without savings is more likely to protest because of her individual economic situation. I do not offer an explanation for when

protests occur in general. Rather, my model offers a more restricted prediction: the distribution of protest across a broad swath of people affected by some economic shock.

Two fundamental premises underlie this argument. First, individuals are rational actors gaining utility from income and wealth. Second, individuals calculate their own utility before engaging in collective action. In addition to these two premises, this paper will borrow one concept and one assumption from the social insurance literature. I will refer to income and wealth similarly to the way this literature distinguishes between income and social insurance. In line with the findings of Ansell (2014), Iversen and Soskice (2001), and Zimmerman and Carter (2003), I will assume that the rich have no need for publicly financed insurance because they can use wealth to smooth consumption during an income shock.

I will then build upon this literature by explaining how wealth plays a similar role to social insurance in smoothing consumption. Similarly to the social insurance literature, I will argue that individuals express preferences about government intervention in the economy via political means. There are four key differences between my argument and the social insurance literature. First, I focus on protest instead of voting. Second, I focus on policy outcomes that can include any form of government intervention in the economy, not just fiscal spending on social insurance. Third, I expand the pool of available funds for consumption smoothing to include private wealth as well as social insurance benefits. Fourth, I argue that protesters are motivated by income and wealth shocks instead of by mechanisms in the existing social insurance literature such as income inequality and skill specificity.

Protests are a tool to maximize one's utility under the constraints of current economic conditions.[5] By protesting, an individual aims to pressure the government into

[5] I will use the Oxford English Dictionary's definition 6c: "Of a (large) number of people: to express collective disapproval or dissent publicly, typically by means of an organized demonstration;

implementing policy that increases her economic well-being. Formal social insurance need not be the kind of policy protesters seek to be implemented: changes in subsidies, price levels, taxes, or bailouts can satisfy protest demands. Indeed, protests have arisen under various kinds of economic dissatisfaction. In 2011, protesters rallied against policies reducing income like austerity measures in Greece and the removal of a fuel subsidy in Bolivia (Donadio and Kitsantonis, 2011; Romero, 2011). Protests emerged in Thailand and Indonesia in response to dropping financial markets during the Asian Financial Crisis of 1997 (Maduz, 2011). These protests were consequential: although the Greek protests failed to persuade the government to dial back austerity, the Bolivian protests succeeded in pressuring the government to maintain the fuel subsidy and the Thai and Indonesian protests both resulted in government transitions.

The variety of circumstances under which protests arise hints that the individuals participating in them might have different reasons for doing so. When deciding to participate in a protest, individuals weigh costs and benefits. There are two kinds of benefits individuals could receive from protesting. First, some social benefit such as social recognition or catharsis could arise merely from participating. Second is the chance of some policy benefit that alleviates the economic shock, which inherently includes the possibility, however small, that one's participation will influence the policy outcome at the margin.[6]

For a given circumstance, individuals have idiosyncratic thresholds for how much effort is appropriate to attempt to shift policy via protest. This threshold is the cost of protesting that individual will tolerate. To protest, one must forego other activities, travel to and participate in the protest, and submit to any consequences

to engage in a mass protest, usually against a government policy or legal decision" OED. (2020).

[6] It is also possible that the government could react to a protest by implementing a policy that harms rather than helps the protester, in which case the policy benefit becomes a cost.

of doing so. The cost of protesting takes two forms: transaction costs and opportunity costs. Each type of cost can vary across individuals by income, employment status, and location. For example, research indicates that the opportunity cost of protesting decreases with unemployment (Brady, Verba and Schlozman, 1995; Jenkins, Jacobs and Agnone, 2003; Collier and Hoeffler, 2004; Thyne, 2006; Costa, 2011; Kimeldorf, 2013; Dahlum, 2019). The transaction costs of protest vary based on location, technology availability, regime type, and repression of the protest (Rüdig and Karyotis, 2014; Wallace and Weiss, 2015; Chenoweth et al., 2017). These costs also vary systemically across countries. For example, transport costs of attending a protest are most likely lower in a dense city such as Hong Kong than in a rural country such as Afghanistan. Strong authoritarian central governments are more likely to repress protests than governments with weak capacity or governments of countries with strong protections on individual rights of expression. The cost of protesting bears on which individuals protest and consequently on the overall scale of protest movements.

These idiosyncratic cost thresholds imply that individuals gain economic utility from multiple sources. I identify two main sources: income and insurance.

An individual's real income is the purchasing power of her current nominal income. Nominal income consists of labor market income minus tax. Two constraints operate on real income: the nominal value of income and the prices of goods and services in the economy. In the immediate term, nominal income itself is constrained by wages and employment. Rising nominal wages, all else equal, result in higher real income. On the other hand, real income falls when nominal wage growth is outpaced by rising prices in goods or services. In the extreme, it falls to zero under unemployment.

Insurance is a stock of assets that, when exchanged, function as income at some point in the future. Insurance can take two forms: private and public.

Private insurance, also called wealth, is an individual's stock of accumulated assets. It has value because it can be exchanged for cash, goods, or services in the future. Wealth derives this value from two important properties: the real value of accumulated assets and the liquidity of the market to exchange those assets for cash, goods, or services. An individual can become more (less) wealthy by holding a constant stock of assets that increase (decrease) in value or by increasing (decreasing) holdings of assets that maintain a constant value. Her ability to exchange assets for cash, goods, or services is higher in liquid markets. This ability to exchange wealth at some point in the future could take the form of smoothing consumption: relying on liquidated wealth to purchase goods and services instead of uncertain or decreasing income.

Wealth is an important source of insurance for many individuals. Furthermore, its accumulation over time is psychologically salient (Davies, 1962; Shi, 2019). A sudden drop in the value of one's savings is traumatizing and can have long-term effects on one's financial position. Although a negative wealth shock may not affect one's ability to eat, it detracts from wealth's ability to serve the purpose for which it was accumulated: to spend at a later date, donate to charity, or pass on to one's heirs.

Public insurance is funded by the government but fills the same role as private insurance: funds that become available in the case of income loss. I will follow the convention of much of the literature in referring to it as social insurance. Although social insurance differs from wealth in its source and distribution across society, it has the same ability to smooth consumption under an income shock for those who receive it.

Although both income and insurance contribute to an individual's economic well-being, the two are theoretically distinct. Income is a flow while insurance is a stock. Income, private wealth, and social insurance are functions of different inputs: real

income is a function of nominal income and price levels, wealth is a function of financial market valuation and liquidity, and social insurance is a function of government policy. Furthermore, each individual treats income and insurance differently because she depends on them differently from her neighbor. Even those who may not have any wealth still likely have income. Income is therefore highly salient for those who depend on it exclusively, who number far more than those who depend on wealth but not income. Someone with little wealth relies heavily on income while someone unreliant on income places more importance on the value of wealth. While real income affects one's capacity to eat this month, insurance affects one's medium-term capacity to insure one's family against shocks.

To illustrate this, Figures 2.1 and 2.2 show several circumstances where income and wealth might not move together. First, price increases reduce real income (all else equal) but might not affect the value of non-cash wealth tied to land or other asset prices: for example, as shown in Figure 2.1 inflation rose in tandem with the ATX stock market in Austria between 2015 and 2020. In this situation, a household's net worth could have increased because of the rise in the ATX while its real income decreased because of inflation. Second, shrinking liquidity in financial markets need not contribute to shrinking wages or higher unemployment. From 2016 to 2020, the Federal Reserve increased U.S. interest rates slowly, while unemployment decreased and wages rose. The better labor market likely increased the real income of many households, while increasing interest rates made borrowing more difficult.[7]

Although insurance and income are distinct, the two are related. Insurance becomes income when it is liquidated for cash that can be exchanged for goods or services. For example, an individual who loses her job can smooth consumption by enrolling in unemployment insurance. If one has wealth, one can smooth consumption by selling wealth for cash and using that cash to buy goods or services. At

[7] All data from TradingEconomics (2020).

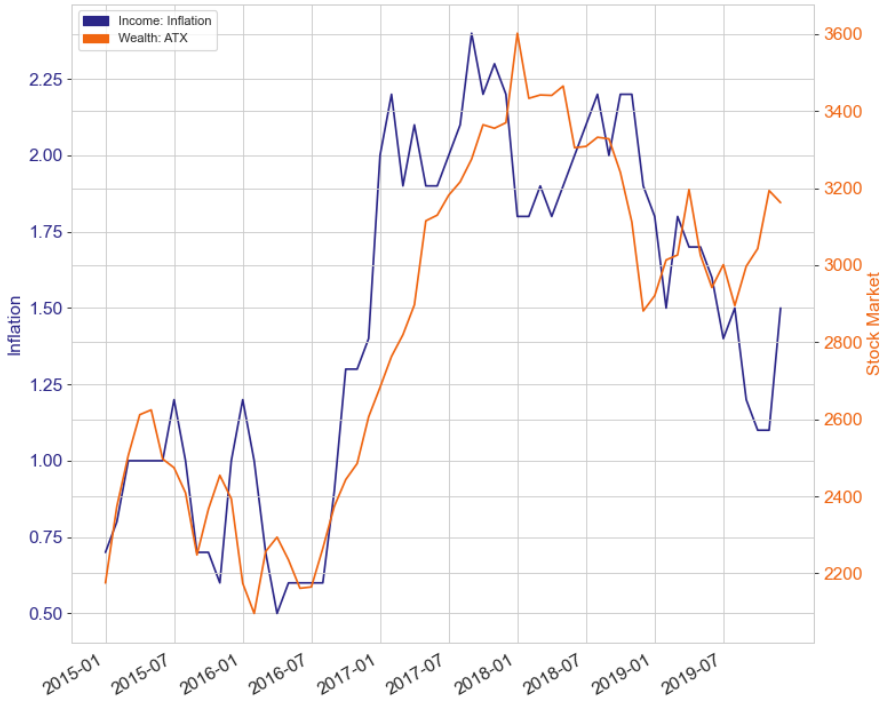


FIGURE 2.1: Income (inflation) and wealth (the stock market, ATX) moving in different directions in Austria.

this point, liquidated wealth or unemployment checks become constrained by price level, one of the constraints on real income. The income-shocked individual still has enough money to eat if her insurance provides enough to smooth consumption; her reaction to an income shock will be more intense if she cannot. Social insurance benefits and wealth are both sources of consumption smoothing for income loss.

Negative economic shocks cause shifts in the costs and benefits of protest. As noted in the literature, the opportunity cost of protesting decreases with a negative shock to income. A negative shock to wealth would most likely not affect the opportunity cost of protesting. On the benefit side, shocks to either income or wealth increase an individual's potential policy gain from a protest. Simultaneous shocks to wealth and income increase policy benefits even more than one shock alone because in addition to decreasing the opportunity cost of protest, the individual's reduced

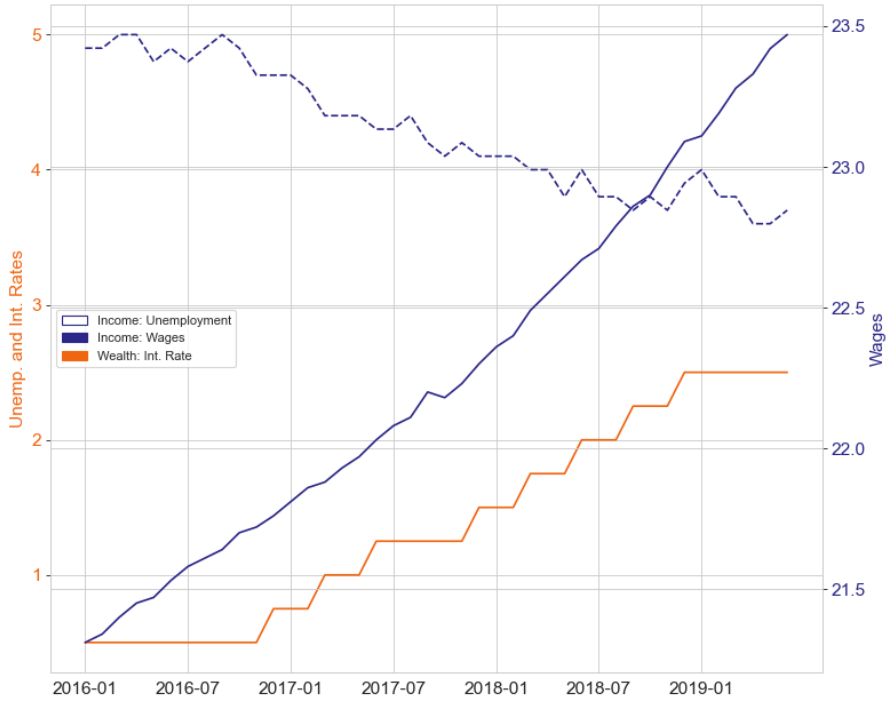


FIGURE 2.2: Income (unemployment and wages) and wealth (the interest rate) moving in different directions in the US.

ability to smooth consumption compounds her increased potential policy benefit. Moreover, social benefits could grow more salient as the community of affected citizens grows. These forces all push the individual in the direction of protest under economic shocks.

The salience of an income shock to an individual is contingent upon her available insurance, be it private or public. Even during normal times, individuals smooth consumption to adjust for small variations in income, a concept which becomes far more important when income drops suddenly. Smoothing consumption is much harder when a negative shock to asset valuation or market liquidity prevents wealth from being exchanged for cash, goods, or services. If an individual has enough wealth to smooth consumption after an income shock, she is less likely to resort to protest because her potential benefit from a policy intervention does not heavily

outweigh the costs of protest. If her government provides enough social insurance to smooth consumption, her policy benefit is likewise low. However, if her insurance is insufficient to smooth consumption, she has less confidence in her ability to make ends meet; her potential benefit from a policy intervention more likely exceeds the costs of protest.

Protest is more appealing than other political options to address economic issues for three reasons. First, individuals can protest immediately, while other forms of political participation such as voting operate on fixed long-term time schedules. Second, individuals are usually unable to marginally affect policy on their own but know that collective action can pressure the government to implement policy. Third, individuals gain social benefits from protesting (Kuran, 1991; Rüdig and Karyotis, 2014).

However, one individual's decision does not a protest make. Few political processes operate only at the individual level: most are more accurately characterized as an interaction between the individual and her social network (Campbell, 2013). This is especially true of protest (Siegel, 2011; Aytaç and Stokes, 2019). From this viewpoint, the protest literature explains why multiple individuals mobilize simultaneously. One of the most common ways to model collective action is a threshold model, commonly seen in the economic and sociological literature, where an individual's willingness to protest is related to her belief that others will mobilize (Schelling, 1971; Granovetter and Soong, 1988; Kuran, 1991; Braun, 1995; Yin, 1998; Siegel, 2011; Hollyer et al., 2015; Aytaç and Stokes, 2019). Such social determinants of protest as peer pressure come into effect logically after an individual decides she is dissatisfied with her situation, and only when she is deciding what to do about her dissatisfaction.[8] What conditions contribute to the formation of these thresholds?

[8] Being asked to protest and previous protest participation are the strongest predictors of protest (Schussman and Soule, 2005; Rüdig and Karyotis, 2014).

In the language of Kuran (1991: p. 18), the social threshold at which an individual will protest varies according to external circumstances that affect the relationship between the size of the opposition and the individual's external payoff for supporting the opposition. Economic circumstances could certainly change this threshold. For example, a large segment of the population simultaneously experiencing an income shock could increase both the number of individuals opposed to government economic policy and each individual's payoff for supporting this opposition. Such a shock would alter the utility calculations of many individuals simultaneously, coordinating many individual-level rational choice decisions. Some of these individuals decide upon the same reaction. This shock would thereby be a mechanism for overcoming the collective action problem typically associated with mass mobilization. Such economic crises are therefore a coordinating device for collective action. Thus, a story about individual utility calculations scales into a public protest demanding policy change.

In contrast to the existing literature on economics and protest, I argue that income and insurance are different inputs into an individual's utility. The severity of an individual's reaction to an income shock depends on her insurance. One implication of this story is that economic outcomes bearing on income and insurance likely affect protest. Because of its importance as an insurance mechanism and the difficulty accumulating it, I expect protests to arise from negative wealth shocks. As a result of income's immediate usefulness, salience, and likely relationship with opportunity cost of protest, and in agreement with the literature, I hypothesize that protests are likely under negative shocks to income. Most importantly, I also hypothesize that income shocks are more likely to lead to protest when individuals have less insurance.

This theory is in line with the social insurance literature. In that literature, the rich do not need public insurance because they have enough wealth to smooth consumption themselves. Middle-income and poor people cannot always do so: they require compensation for their lower insulation against future shocks. This com-

pensation is the policy benefit protesters seek. Furthermore, the literature shows that individuals support social insurance more when they make less money; in other words, they seek insurance when they are at risk of not being able to smooth consumption under an income shock. I argue the same: under an income shock, one smooths consumption by liquidating wealth if one can and protests for policy relief if one cannot.

Of course, there are a lot of contextual features that are likely to facilitate the dynamics in the economic environment. For example, the literature provides evidence that democracy facilitates collective action in protests (Bellinger Jr and Arce, 2011; Jo and Choi, 2019) and that regime suppression can neutralize protests up until a point, beyond which it incentivizes further micromobilization (Opp and Roehl, 1990). An individual's consumption threshold will be conditional on regime characteristics: the political and economic development of a country can affect the incidence of protest by making resources available to protesters (Dalton, Van Sickle and Weldon, 2010; Arce and Rice, 2009). My argument does not preclude protests by the middle class, a phenomenon visible in some protests led by students or the working class. Such a protest could result from a shock to a portion of the economy particularly salient to one section of the population, perhaps due to a resource endowment specific to that sub-population. In the empirical models below, I will include control variables to account for these explanations.

2.4 Research Design

I conduct a two-stage empirical analysis to evaluate these hypotheses. In the first stage I predict the count of protesters that demonstrate in a country during a particular month using macroeconomic indicators of wealth and income. During the time period in question, I set my analysis in 11 countries in Western Europe that encompass a broad variety of social movements and economic conditions. These countries

also are a good setting to test my hypotheses because of their history of social mobilization, variety of welfare states, and strong data reporting. The choice of these countries will further facilitate comparison with other social insurance literature that analyzes OECD countries (Moene and Wallerstein, 2001, 2003; Rueda, 2005).[9]

In the second stage, I use two sets of individual-level survey data to show that income shocks do indeed motivate different protest behavior in individuals with different levels of insurance. I predict an individual's protest participation by whether someone close to her has recently become unemployed and whether she has access to economic insurance via possession of material wealth.

2.4.1 Aggregate Data

Four categories of data contribute to the aggregate analysis: protest data, macroeconomic data, social insurance data, and data to allow for alternate explanations. This evaluation will cover the aforementioned 11 countries in Western Europe between 2005 and 2015, the latest time period for which accurate protest data is available.

Protest Data

I source protest data from the PolDem data set, which contains data for 31 European countries since 2005 (Kriesi et al., 2020). The dependent variable p is the weighted number of participants in all protests that occurred in country c during month t. PolDem obtains this count by collecting over five million English-language newswire reports, removing duplicate reports and reports on events in countries outside Europe, and applying a supervised document classifier to filter relevant reports

[9] Although this literature includes analysis of non-European OECD members (Australia, Canada, Japan, New Zealand, and the United States), I limit my analysis to Europe because of protest event data availability. I begin with the 18 OECD countries analyzed by Moene and Wallerstein (2001). I then exclude the five mentioned non-European countries. In addition to being excluded from the protest event data set, Australia and New Zealand have ceased reporting Money Supply M2, a key input into the *Savings* variable. I also exclude Belgium and the Netherlands because they do not report wage data. These exclusions result in an analysis covering Austria, Denmark, Finland, France, Germany, Italy, Norway, Portugal, Sweden, Switzerland, and the United Kingdom.

from irrelevant ones. They then once more discard duplicates and finally apply a supervised protest mention classifier. PolDem then weights the estimated count of participants in each event to compensate for sample selection, newswire selection, and country population size. These adjustments allow comparison between participant counts in events in large countries that are covered well in English-language newswires (like France) and events in small countries that are more neglected by English-language newswires (like Portugal). While the PolDem data set includes entries for each protest event, I aggregate to the monthly level to match the level of analysis of the economic data. Figure 2.3 shows the sum of the weighted protester counts over time aggregated across all countries and Figure 2.4 shows the same country-month counts as a density plot. It is clear in Figures 2.3 and 2.4 that counts of protesters increased through the mid-2000s and reached a steady state around the 2008 Financial Crisis.

Figures 2.4 and 2.5 provide further insights about the protester counts. Countries are prone to stay within a certain range of protest behavior. The vast majority of data points are well below 5,000 weighted protesters per month. High monthly protester counts (say, over 300,000 protesters) are not distributed evenly across all countries; they only occur in several countries (France, Portugal, and the United Kingdom). In Figure 2.5, the distribution of protesters across SI is also striking: there are far fewer protesters in the higher range of generosity than the lower range. The variation of SI within countries is also interesting: countries usually stay in a particular range of spending but do vary within that range.[10]

Economic Data

Modern economists have access to measurements of every conceivable facet of economies. Some of these indicators bear on real income and others on wealth, two of the key

[10] See Figure 2.6 for SI by country over time.

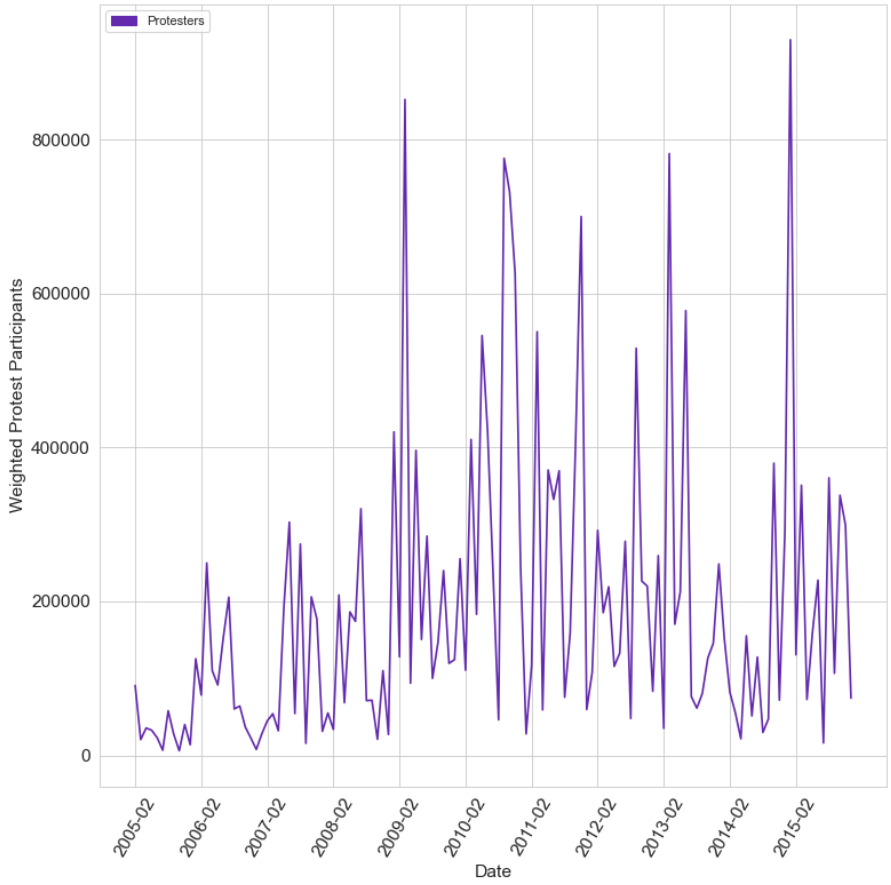


FIGURE 2.3: Aggregate Monthly Protester Counts. The weighted count of protesters increases in early 2009.

theoretical concepts to identify for this study. Such economic indicators, sourced from TradingEconomics (TE) at the country-month level, are my independent variables in the aggregate analysis. TE is an economic reporting platform that sources a wide variety of economic indicators from every country across the world over modern history. Each country reports a subset of all possible economic indicators that varies over time. Naturally, the reported indicators vary by country: for example, the United States reports more indicators than Mali does. The frequency at which a particular indicator is reported also varies between countries. A strength of this data vis-a-vis existing protest literature is its high frequency. To the extent that

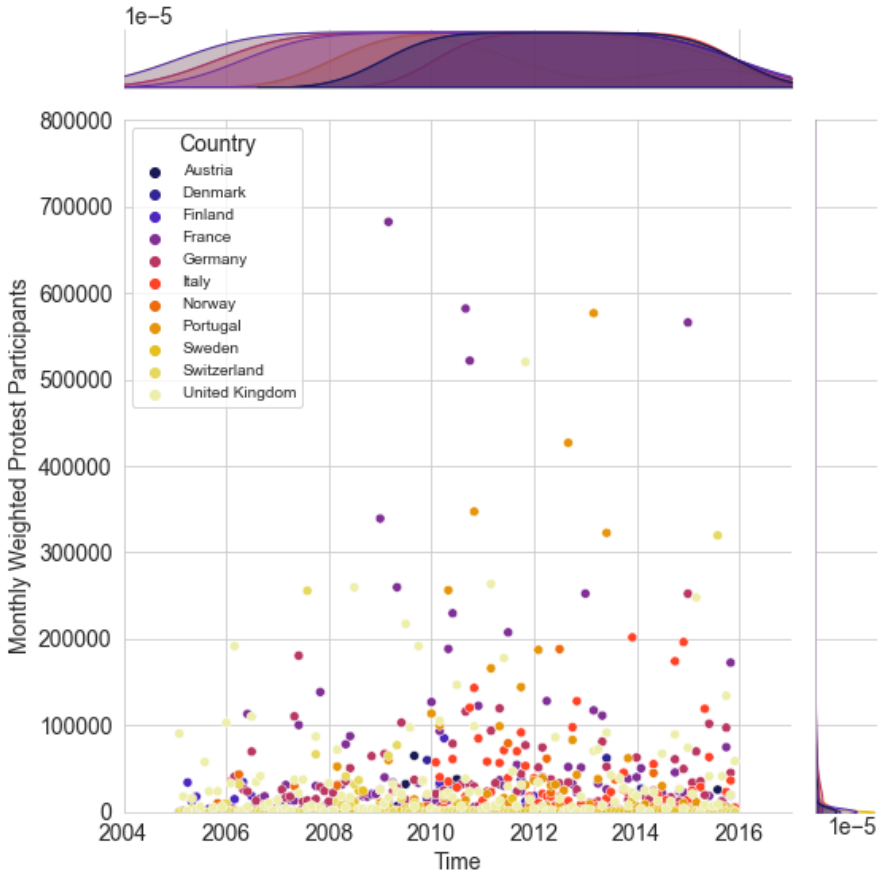


FIGURE 2.4: Monthly Protesters Over Time by Country. High monthly protester counts only occur in France, Portugal, and the United Kingdom.

people's lives happen in real time and not in annual increments, data more frequent than annual aggregations allows closer pinpointing of relationships.

The independent variables of interest are two vectors of TE indicators for country c in month t. The first is composed of those indicators j bearing on individual real income and is denoted by $Y_{ct} = (y_{1ct}, y_{1ct}, \ldots, y_{jct})$. The second is comprised of those indicators k bearing on wealth and is denoted by $W_{ct} = (w_{1ct}, w_{1ct}, \ldots, w_{kct})$. The theoretical distinctions between income and wealth inform choices of w_{kct} and y_{jct}.

Measures such as GDP per capita, often used as a stand-in for economic well-being, can be aggregated both in time and location to the country-year level. These

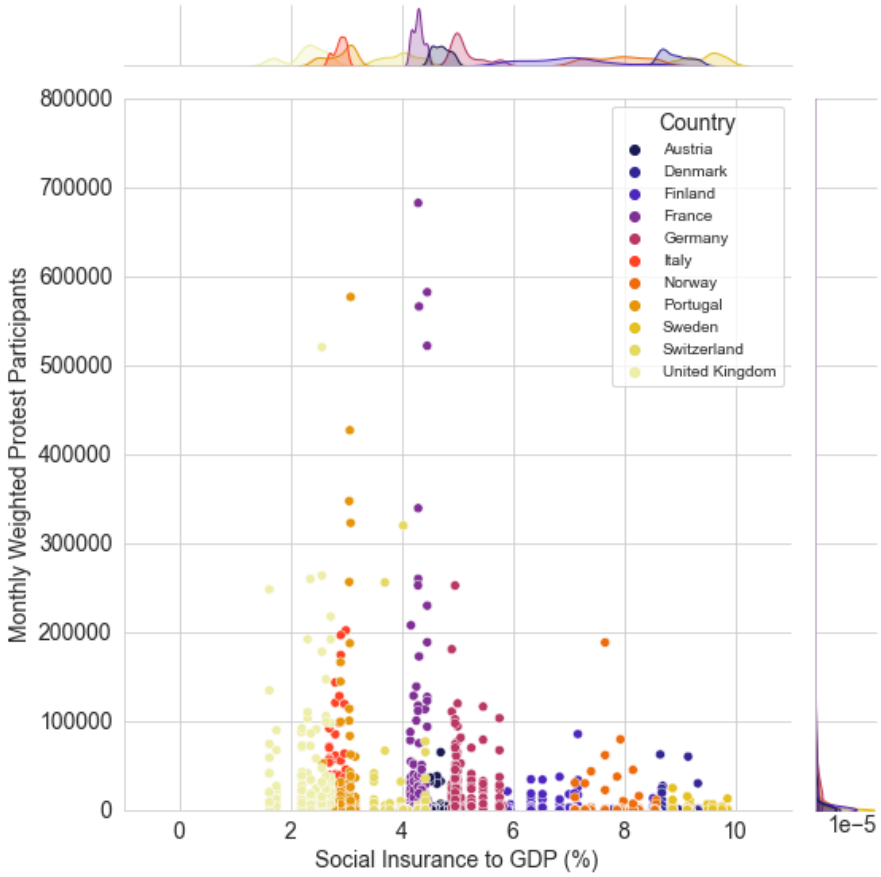


FIGURE 2.5: Monthly protesters by country and SI level. There are far fewer protesters in the higher range of SI generosity than the lower range.

general measures are commonly reported by almost all countries, but their aggregated nature means that they gloss over more detailed views of the economy. Indicators more directly important to households, such as the price of housing or the level of the stock market, are not always reported frequently by a broad variety of countries across a long period of time. As a result, insisting on high-frequency economic data that is directly relevant to households restricts both the sample of countries and the number of points in time for which each country reports the indicator.

Inherent in choosing economic indicators is the possibility of introducing sample bias. It is possible that countries that report more economic indicators more

frequently have higher state capacity than those that do not. Fortunately, most countries in Europe for which protest data is available are OECD countries; they therefore have robust reporting structures. The analysis will use indicators that more precisely measure the theoretical concepts in pursuit of improved construct validity, giving preference to indicators that matter directly to households and are reported more frequently. For example, both interest rates and money supply (M0, for instance) could be indicators of liquidity. Households are exposed directly to interest rates when transacting with banks, but are further removed from the overall money supply.

I define Y_{ct} to include unemployment rate ($Unemp$), an annual indicator, to measure the fraction of the employed or job-seeking population without labor market income. The consumer price index (CPI), reported monthly, measures the price of a bag of goods relative to a reference time period and can be interpreted as the purchasing power of nominal income. Wages ($Wages$), reported quarterly, measures the average nominal monthly earnings. These indicators taken together measure real income, theorized as labor market income relative to prices.[11]

I define W_{ct} to include aggregate national savings ($Savings$), calculated as the difference between money supplies M2 and M1, which encompasses savings deposits, money market, mutual funds, and other time deposits like CDs. Spikes in $Savings$ occur during times when individuals guard safe assets, perhaps out of fear of drops in the prices of other other assets. The housing index ($Housing.Index$), reported monthly, measures a significant source of an individual's wealth: the market prices of new and existing residential housing.[12] I include a country's stock market

[11] I omit taxes because I use country fixed effects and panel empirical specifications where all compatriots are subject to the same tax regime.

[12] Home ownership rates vary across Europe, but are usually high. Among the 21 countries included in this analysis, the only two countries with low values are Germany and Switzerland, which both hit lows in late 2015 of 51.9% and 43.4%, respectively. Ownership rates between 65 and 85% are more common.

($Stock.Market$), which is an important measure of the asset values of individuals' invested wealth. The interest rate ($Int.Rate$), reported daily but aggregated to monthly, measures liquidity: the ease or difficulty of borrowing money. Borrowing money allows individuals to smooth consumption using borrowed funds. Higher rates increase the cost of this consumption smoothing, pricing some out of the market.

Analytically, these indicators correspond to the theoretical definitions of wealth and income established above. I have backfilled those indicators reported less frequently than monthly into the months since the last report: for example, an indicator reported quarterly would see April's value backfilled into February and March.

Social Insurance Data

Some kinds of social insurance benefits target only those who are not employed, and other kinds target everyone. The former is the relevant kind of benefit for this paper ("social insurance" henceforth refers to government-funded social insurance against income loss). To measure the breadth of a country's social insurance against income shocks, I duplicate the *Spending on Insurance Against Income Loss* measure from Moene and Wallerstein (2001), which captures at the country-year level the share of GDP or total government expenditures a country targets at the unemployed. I call this measure SI for social insurance. It measures government-funded social insurance that functions as income to smooth consumption for those who have undergone income shocks and is shown in Figure 2.6. As an alternative to liquidating wealth to smooth consumption, it is an important control variable. SI sums government and mandated private expenditures on disability cash benefits, occupational injury and disease, sickness benefits, services for the disabled and elderly, survivors benefits, active labor market programs, and unemployment insurance. To ensure the figure does not cover benefits that include the employed or planned reductions in income, the measure excludes old age cash benefits, family benefits, housing benefits, pensions,

and benefits for other contingencies. Although Moene and Wallerstein (2001) include a manipulated version of healthcare spending in their measure of social insurance, I exclude it because the countries in my sample have healthcare funding schemes with varying degrees of public and private funding, making it difficult to distinguish how much government healthcare funding targets the unemployed (OECD, 2020). *SI* is likely to be robust to the ideological composition of country governments: Moene and Wallerstein (2003) find evidence that patterns of spending on unemployment insurance do not vary systematically across government partisanship in Western Europe.[13]

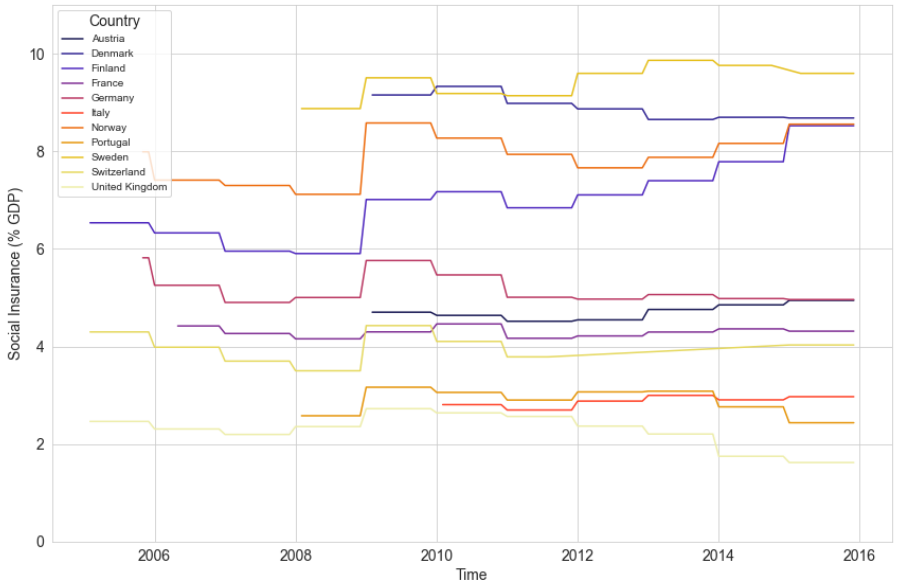


FIGURE 2.6: *SI* by Country Over Time. Within-country *SI* level changes over time, but between-country *SI* ordering stays fairly constant.

Alternative Explanations

The literature on institutional and social determinants of protest shows that citizen coordination and the size of the protest is important to influence an individual's

[13] Rueda (2005: p. 70) finds evidence that government partisanship does not significantly affect the provision of active labor market policies, an important part of social insurance in these countries. This also implies that the beneficiaries of such policies do not change with elected governments.

decision to participate. I therefore control for characteristics of the state that influence citizen coordination. I use the Polity V and Polity State Failure data sets to control for government capacity to oppress, ability to turnover politically, government responsiveness to past demands, repression of information, and establishment of private sector. In some empirical specifications these variables are co-linear.

2.4.2 Survey Data

In addition to the relationship between macroeconomic conditions and aggregate protests, I conduct two survey analyses to more precisely adjudicate the individual-level relationship between economic circumstances and likelihood to protest.

Europe Survey Data

I first consider this phenomenon in the same 11 European countries as above, sourcing survey data from the European Social Survey (ESS) (NSD, 2020). The survey asked respondents questions about their views on social, economic, and political matters. I construct several variables to isolate the theoretical concepts of interest. The first is the dependent variable *Protest*: whether the respondent has taken part in a lawful public demonstration in the past 12 months. I code all respondents who are currently unemployed but had a paid job one year ago or more recently as experiencing a recent income shock ($RecentEmpShock$). I code all respondents whose primary source of household income is unemployment benefits as relying on publicly funded social insurance ($SIIncome$).

The ideal measurement of private insurance would be some tally of liquid assets upon which the respondent can easily draw in the case of an income shock. Such assets could be cash, stocks, bonds, or other relatively liquid financial or non-financial assets. But surveys tend to ask about respondent possession of more illiquid assets such as houses and businesses. So, to measure respondent wealth, and in line with

Ansell (2014), I consider respondents who own a home to have a form of material wealth that can serve as "self-supplied insurance" (Ansell, 2014: p. 383). I define the binary variable $HomeOwner$ to be one for respondents who own their home outright or have some equity in their home, but make monthly mortgage payments. Unfortunately, the home ownership question was only asked during the 2004 wave of the ESS, restricting the time scope of my analysis.

But what if the respondent's decision to (not) protest is not a function of their economic insurance, but rather is influenced by their social circles? Kuran (1991) and others have shown that an individual's network affect her willingness to protest: if others are protesting, an individual is more likely to do so themselves. Although the literature finds substantial evidence for this phenomenon, I expect economic insurance to provide a reason notwithstanding the network effects of information. So, to isolate the effect of economic insurance, I account for the transmission of information through an individual's network by including a measurement of whether respondents have a close friend with whom they can discuss intimate matters ($CloseDiscuss$) and a measurement of how many days in the week prior to the survey the respondent was socially active ($SocialDays$).

I include the standard demographic control variables gender, age, education, and political partisanship. I also control for union membership because literature has found evidence connecting union membership and protest (See, *inter alia*, Ebbinghaus and Visser, 2000; Hamann, Johnston and Kelly, 2013; Engels, 2015).

The literature on consumption smoothing also suggests that rich and poor individuals cope with income loss differently. Zimmerman and Carter (2003) find that when faced with income loss, poor individuals are more likely to sacrifice consumption to conserve assets while rich individuals generally liquidate assets to smooth consumption. This suggests that when experiencing an income shock, poor individuals likely benefit more from government social insurance than rich individuals. In

the absence of a sufficient social insurance scheme, poor individuals might be particularly risk-averse and preemptively choose a reliable but low level of income to avoid any potential shock (Morduch, 1995; Chetty and Looney, 2006). I include household income $HHIncome$ in my analysis.

However, the ESS has several drawbacks. First, $HomeOwner$ is only available for one wave of the survey. Second, the ESS lacks a question explicitly measuring the network effects of political discussion. Although I construct variables measuring if the respondent has "anyone to discuss intimate and personal matters with" and if so how many people, these measures do not explicitly measure political networking effects.

US Survey Data

The American National Election Survey (ANES), run during even-numbered years since 1968, remedies both of these problems. I source individual-level survey data from the 2020 wave, which included questions in both areas the ESS omits. The ANES' major advantage is explicitly asking respondents how many days in the week prior to the survey the respondent discussed politics with their family or friends. I include a measurement of this ($TalkPolitics$) to account for the transmission of political information through an individual's network.

The ANES data does have one big downside, however. It does not include information about whether the recipient received any social insurance from the government. I therefore focus on examining the effect of private insurance on protest in this analysis. However, to accommodate the state-level variation in US social insurance programs, I use state-level fixed effects.

Although individuals make their own political choices about voting and protesting, individuals do not operate in isolation within households; households usually share finances and discuss politics. Any income loss in a household is important,

not just the respondent. Furthermore, the respondent isn't always employed or the dominant wage earner of the household. To best reflect this, I measure income shocks by considering a variable measuring whether any family member or close personal friend of the respondent has lost a job within the 12 months preceding the survey. I code all respondents who answer in the affirmative as having a recent income shock ($AnyLostJobs$).

Like in the ESS, I include the demographic control variables age, education, income, marital status, children, gender, race/ethnicity, and political party identification. I also control for various aspects of an individual's connection with social networks that may make her more likely to protest. I also include measures of household union membership, whether the respondent feels close to a political party ($CloseToParty$), and how close they feel to that party ($HowClose$).

2.5 Results

2.5.1 Aggregate Results

I first look for evidence supporting my hypothesis at the aggregate level. I assess the effect of each aggregate economic variable y_{jct} and w_{kct} on p_{ct} to gain a more specific understanding of the relationships between macroeconomic trends and protest. Because these variables are correlated, I use a error correction model to separate their effects.

Autocorrelated data like many economic indicators is often fit with autoregressive (AR) and ARIMA models, which assume that the data is stationary: its mean and variance do not vary over time. Figure 2.7 shows selected economic indicators in the United Kingdom that are clearly non-stationary.[14] The presence of these long-

[14] The order of integration $I(d)$ of a time-series is the minimum number of first differences required to obtain a covariance-stationary series. Several time-series are said to be cointegrated if their linear combination is stationary. The Johansen test uses likelihood ratio tests to determine the number of cointegrating relationships between non-stationary time series and allows for more than

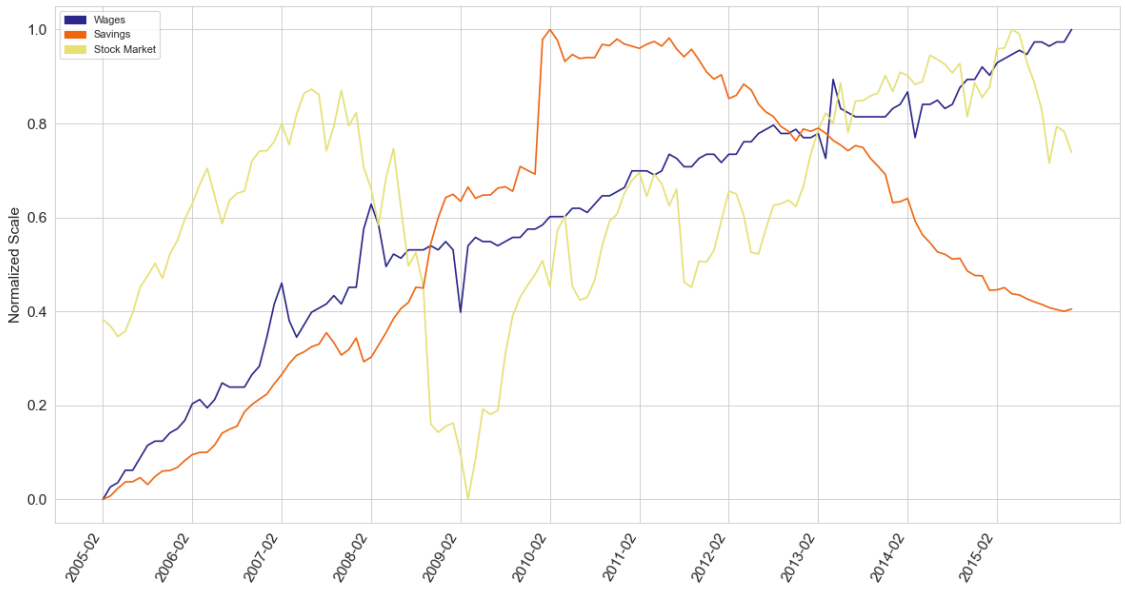


FIGURE 2.7: Non-stationary economic indicators in the United Kingdom: mean and standard deviation change over time.

run stochastic relationships between predictors requires the use of a Vector Error Correction Model (VECM). Normal VECM models would suffice if the data included just one country, but since the sample includes multiple countries for a single time period, a panel approach is needed.

To account for cointegrated variables' responsiveness to any deviation from the long-run equilibrium, the error correction model below makes short-run dynamics of variables a function of deviation from equilibrium. The VECM model incorporates the possibility that the variables income, wealth, and protest are related, with the error correction parameterization:

$$\Delta p_{ct} = \phi_c \left(p_{ct-1} - \theta_{0c} - \theta_{1c} Y_{ct} - \theta_{2c} W_{ct} \right) + \delta_{11c} \Delta Y_{ct} + \delta_{21c} \Delta W_{ct} + \epsilon_{ct} \tag{2.1}$$

two cointegration relationships between variables, which is the maximum that the Cointegrated Augmented Dickey Fuller test (Engle-Granger procedure) is capable of (Engle and Granger, 1987; Johansen, 1988; Michieka and Gearhart, 2015). Countries of this paper's panel contain between two and five cointegrating relationships between p_{ct}, W_{ct}, and Y_{ct}.

for countries $c = 1, 2, \ldots, C$ and time periods $t = 1, 2, \ldots, T$. The error-correction speed of adjustment is denoted by ϕ_c; θ_{1c} and θ_{2c} are the long-run coefficients of the variables of interest. For $I(1)$ cointegrated variables like the economic indicators, the error term ϵ_{ct} is $I(0)$ for all c.[15]

One advantage of a VECM is the ability to separate long-run and short-run stochastic effects. Short-run deviations in cointegrated variables from the long-run equilibrium interact with changes in the dependent variable to return to the long-run equilibrium. If protest is driven by departures from the long-run equilibrium, then it responds to this feedback. If not, it responds only to short-term shocks to the stochastic environment given by the ΔY_{ct} and ΔW_{ct} terms. The speed of reversion ϕ_c is the coefficient of the error correction term; a statistically significant negative value would indicate that the long-run relationship directly drives protest (Blackburne III and Frank, 2007; Michieka and Gearhart, 2015).

I ran a pooled mean group (PMG) estimation Panel VECM model for each combination of y_j and w_k.[16] Because of the panel structure of the data, the varying scales and currency units of the independent variables between countries, and the simultaneous necessity of preserving inter-country variation for the control variables, I normalized the economic variables on a scale of zero to one within country and normalized the control variables on the same scale across countries. The dependent variable p remains non-normalized.

In all specifications, the error correction term ECT (ϕ_c in the Equation 2.1) is statistically significant and highly negative, implying that the long-run relationship

[15] The above derivation follows Blackburne III and Frank (2007).

[16] As in Blackburne III and Frank (2007). A PMG estimator "allows the intercepts, short-run coefficients, and error variances to differ freely across groups, but constrains the long-run coefficients to be the same" (Pesaran, Shin and Smith, 1999). These assumptions fit this data well because the protest literature suggests that the same relationships between economics and protest hold true over decades. Furthermore, the social insurance literature finds evidence that the same relationships between consumption smoothing and political participation also hold true over decades in these countries. The same assumption in the short term is less defensible.

Table 2.1: Selected Panel VECM Results

	(1)		(2)		(3)	
VARIABLES	Long-term	Short-term	Long-term	Short-term	Long-term	Short-term
Constant		27,859***		12,679		17,515**
		(6,106)		(7,875)		(7,597)
ECT		-0.945***		-0.941***		-0.955***
		(0.0465)		(0.0461)		(0.0562)
SI	-793.8		2,030*		1,540**	
	(1,408)		(1,127)		(632.0)	
ΔCPI		-63,312				
		(124,780)				
$\Delta Housing.Index$		37,562				
		(55,493)				
$\Delta CPI : Housing.Index$		-125,794**				
		(63,781)				
CPI	-3,838					
	(2,747)					
$Housing.Index$	4,690					
	(4,673)					
$CPI : Housing.Index$	-3,249					
	(5,751)					
$Wages$			1,476		-4,442**	
			(1,770)		(2,032)	
$Int.Rate$			9,224***			
			(2,641)			
$Wages : Int.Rate$			-23,781***			
			(5,679)			
$\Delta Wages$				-11,451		-129,222
				(49,578)		(78,799)
$\Delta Int.Rate$				2,854		
				(81,658)		
$\Delta Wages : Int.Rate$				-30,741		
				(62,857)		
$Savings$					-2,930	
					(3,330)	
$Wages : Savings$					2,536	
					(4,102)	
$\Delta Savings$						-206,162*
						(116,127)
$\Delta Wages : Savings$						178,958
						(119,595)
Observations	1,123	1,123	1,013	1,013	1,013	1,013

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
The lagged dependent variable is treated as endogenous.

between the variables directly drives protest. Table 2.1 shows the results of the three statistically significant interactions between income and wealth variables. Four combinations of income and wealth indicators have statistically significant relationships with protest. Of these, one merits special attention: the interaction between $Unemp$ and $Int.Rate$. Because central banks consider employment levels when making interest rate decisions, the interaction term is endogenous and its interpretation is ambiguous.[17] As a result, I have omitted it from Table 2.1.

Specification one shows that the short-term interaction between changes in CPI and $Housing.Index$ is statistically significantly correlated with protest. Although

[17] The European Central Bank (ECB) sets interest rates primarily based on price level but also based on employment and economic growth. Non-Euro central banks either use monetary policy to maintain a stable exchange rate with the Euro or incorporate employment into monetary policy decisions (Danmarks Nationalbank, 2009; Norges Bank, 2020; Sveriges Riksbank, 2020).

neither has an effect statistically distinguishable from zero on its own, simultaneous short-term increases in CPI and decreases in $Housing.Index$ correspond to increases in protest. This corresponds to theoretical predictions because such a scenario embodies simultaneous decreases in real income and asset valuation, which make smoothing consumption more difficult.

Specification two shows that the interaction between $Wages$ and $Int.Rate$ is statistically significant. Protests increase when $Int.Rate$ increases with wages set at zero (their lowest within-country value), but decrease from that with incremental increases in wages. Increasing wages mitigate the positive effect that rising interest rates have on protest. This aligns with theoretical expectations: although rising interest rates restrict liquidity in the market and make liquidating assets more difficult, wage increases decrease the need to do so in the first place.

Lastly, specification three shows that, as theoretically expected, $Wages$ has a statistically significant negative long-term relationship with protest when $Savings$ is zero (at their lowest value within country): rising wages mitigate protests. The same effect also exists in the short term, albeit at lower levels of statistical significance. Moreover, the interaction between $Wages$ and $Savings$ in the short-term exhibits weak statistical significance. A concurrent positive change in $Savings$ mitigates the increase in protest resulting from a wage cut. Because of the magnitudes of the respective coefficients, if the increase to $Savings$ is very large, the overall effect on protests can even be negative.

This merits further discussion. $Savings$ increases when individuals are insecure about the valuation of other assets. For example, the sharp increase in $Savings$ in March 2020 at the beginning of the coronavirus pandemic has been attributed to slowing investment, dropping rates encouraging home purchases, decreased demand for capital expenditures, and fiscal easing. The general explanation for an increase in $Savings$ is that people sell investments, cease borrowing, and shift to cash because

of unsure revenue streams. Having cash on hand is a sign that people expect to have to smooth consumption using accumulated assets. Albeit with less statistical confidence, this short-run interaction term provides evidence for the consumption smoothing hypothesis.

Generally, these results show the expected statistically significant relationships between income shocks, wealth shocks, and protest. Furthermore, these results support my hypothesis because they suggest that the effect of consumption smoothing via increased housing prices, decreased interest rates, or increased savings appears to mitigate the effect of increasing prices and dropping wages on changes in protest.

2.5.2 Survey Results: EU

Now that I have shown a relationship between aggregated protests and select macroeconomic indicators, I move on to the level of analysis at which the decision to protest is actually made: the individual. I first start in Europe, using ESS data. With a simple logit model, I assess the effect of an income shock ($RecentEmpShock$) on an individual's protest participation as moderated by a binary variables measuring the respondent's wealth ($HomeOwner$) and another indicating whether the individual receives most of her income from unemployment benefits ($SIIncome$). The expectation that $RecentEmpShock$ is moderated by both wealth and $SIIncome$ implies a three-way interaction term, which also requires including in the model specification each two-way interaction and each term on its own. The specification for the full empirical model is

$$p_{ict} = \alpha + \beta_1 RecentEmpShock_i + \beta_2 HomeOwner_i + \beta_3 SIIncome_i +$$
$$\beta_4 RecentEmpShock_i * HomeOwner_i + \beta_5 RecentEmpShock_i * SIIncome_i +$$
$$\beta_6 HomeOwner_i * SIIncome_i +$$
$$\beta_7 RecentEmpShock_i * HomeOwner_i * SIIncome_i + Z_i + \phi_c + \gamma_t + \epsilon_{ict}. \quad (2.2)$$

Because I hypothesize that access to insurance decreases the likelihood of protest, negative values of β_3 would support my hypothesis (Franzese and Kam, 2009: p.50-51). I also expect positive values for β_1.

Table 2.2: The Effect of Insurance on Probability of Protesting, EU

	Dependent variable:
	Pr(Protest)
RecentEmpShock	0.283
	(0.209)
HomeOwner	−0.054
	(0.076)
SIIncome	−0.188
	(0.319)
Age	−0.021***
	(0.002)
Female	−0.178***
	(0.063)
HSGrad	0.586***
	(0.109)
HHIncome	0.0004
	(0.015)
Kids	−0.217***
	(0.066)
Urban	0.124*
	(0.071)
MemberParty	1.618***
	(0.119)
RightPolitics	−0.204***
	(0.015)
Union	0.993***
	(0.080)
SocialDays	0.255***
	(0.026)
CloseDiscuss	0.067
	(0.118)
RecentEmpShock:HomeOwner	−1.066***
	(0.352)
RecentEmpShock:SIIncome	0.165
	(0.446)
HomeOwner:SIIncome	1.291***
	(0.471)
RecentEmpShock:HomeOwner:SIIncome	−0.171
	(0.871)
Observations	13,783
Log Likelihood	−3,797.974
Akaike Inf. Crit.	7,635.947
Bayesian Inf. Crit.	7,786.571
Note:	*p<0.1; **p<0.05; ***p<0.01

The ESS survey results, shown in Table 2.2, provide limited support for my hypothesis. Although income shocks on their own do not affect baseline likelihood

to protest for non-home owners and non-recipients of social insurance, individuals appear much less likely to protest in the face of an income shock if there is a relatively large, negative, statistically significant relationship between home ownership and protest. This supports my hypothesis. However, the results find no relationship between public insurance and protest.

Moreover, there are statistically significant relationships between demographic characteristics and protest. Protests are more likely among younger people, males, high school graduates, those with no children, city dwellers, people with left-leaning politics, union members, and those who spend more time socializing with close friends.

To illustrate this, consider a 30-year-old female high school graduate in the United Kingdom in 2004 with middle income who is not a member of a union. She is not a member of a political party and has independent politics; she has friends with whom she talks closely three days a week. Table 2.3 shows that if someone in her household loses their job, her probability of protesting increases. This probability is mitigated by owning her home but not by receiving social insurance from the government.

Table 2.3: The Effect of Insurance on Probability of Protest, EU.

	Baseline	Shock	Private	Public	Both
$RecentEmpShock$	✗	✓	✓	✓	✓
$HomeOwner$	✗	✗	✓	✗	✓
$SIIncome$	✗	✗	✗	✓	✓
P($Protest$)	4.41%	5.77%	1.96%	5.66%	5.66%

Notable here is the lack of a statistically significant relationship between public insurance ($SIIncome$) and protest. Not only is there no baseline effect of public insurance on overall likelihood to protest, but recipients of public insurance are also no more or less likely to protest if they have private insurance than if they do not.

2.5.3 Survey Results: US

Although the ESS results provide limited support for my hypothesis, it is possible that the deficiencies in the ESS survey data could lead to results that do not illustrate the whole picture. Therefore I also conduct a similar analysis using the 2020 ANES. I expect to find stronger results because the ANES has decreased cross-country variation and a stronger measure of the effect of networks on political involvement.

Because US employment benefits are administered at the state level, they vary tremendously in generosity from state to state. To account for this, I use a multi-level model with state effects ϕ_s to ensure that cross-state variation in social insurance generosity is not driving my results.

Using a simple logit model, I assess the effect of an income shock ($AnyJobLoss$) on an individual's protest participation as moderated by a binary variable indicating whether the individual owns her home ($HomeOwner$). The expectation that $AnyJobLoss$ is moderated by both $HomeOwner$ implies an interaction term. I test whether the results of these interaction terms hold under the inclusion of a vector Z of controls including Age, $Educ$, $Children$, $Female$, $Married$, $RaceEthnicity$, $HHIncome$, $PartyID$, $CloseToParty * HowClose$, $TalkPolitics$, and $Union$. The specification for the full empirical model is

$$p_i = \alpha + \beta_1 AnyJobLoss_i + \beta_2 HomeOwner_i + \beta_3 AnyJobLoss_i * HomeOwner_i + Z_i + \phi_s + \epsilon_i. \quad (2.3)$$

Because I hypothesize that access to insurance decreases the likelihood of protest, negative values of β_3 would support my hypothesis (Franzese and Kam, 2009: p.50-51). I also expect positive values for β_1.

The results, shown in Table 2.4, indicate that the baseline likelihood of protest

Table 2.4: The Effect of Insurance on Probability of Protesting, US

	Dependent variable:
	P(Protests)
Age	-0.031^{***}
	(0.005)
Educ	0.132^{***}
	(0.048)
Children	-0.160^{*}
	(0.088)
Female	-0.044
	(0.179)
Married	-0.451^{**}
	(0.184)
RaceEthnicity	0.079
	(0.070)
HHIncome	0.018
	(0.016)
TalkPolitics	0.197^{***}
	(0.038)
PartyID	-0.052
	(0.094)
CloseToParty	-0.053
	(0.037)
HowClose	0.163^{***}
	(0.061)
Union	0.213
	(0.223)
HomeOwner	-0.037
	(0.299)
AnyLostJobs	0.756^{**}
	(0.310)
CloseToParty:HowClose	-0.009
	(0.014)
HomeOwner:AnyLostJobs	-0.702^{*}
	(0.379)
Note:	*p<0.1; **p<0.05; ***p<0.01

is higher if your household has known a job loss (positive value of $\beta_1 = 0.756$).

As expected, the results also indicate a negative estimate for β_3: the value of the coefficient β_3 is -0.702. Individuals whose household has had a recent job loss are far less likely to protest when they own their home. This means that the increase in probability of protest associated with a job loss is almost entirely offset by owning a home: respondents who have known a job loss and own a home are only barely more likely to protest than individuals who have not known a job loss at all.

To illustrate this, consider a 30-year-old female college in the US in 2020 who is not a member of a union and whose income is between $65-70k. She is not a member of a political party nor does she closely align with one, is a political independent, and talks about politics with her friends and family one day a week. Table 2.5 shows that if someone in her household loses their job and she does not own her home, her baseline probability of protesting is 23.4%. If she has private insurance via home

ownership, her probability of protesting drops to 12.7%.[18]

Aside from the hypothesized connection between economic insurance and protest, these results indicate statistically significant support for relationships between protest and other variables. First, results generally support the biographical availability theory. Protest is more likely among younger respondents (the probability of protest decreases by 0.36% for each year they're older), respondents with no children (1.6% lower for each kid), and unmarried respondents (4.3% less if you're married). Results also indicate that more educated respondents are more likely to protests (0.3% for each level of education), as are respondents who feel close to a political party (4% more likely for those who are very close to a party compared to those who are not close). Lastly, the results support the theory that network flows of information affect the decision to protest. The effect varies, but respondents are more likely to protest the more frequently they talk politics (about 3% more per additional day a week).

Table 2.5: The Effect of Insurance on Probability of Protest, US.

	Baseline	No Insurance	Private
$AnyLostJobs$	✗	✓	✓
$HomeOwner$	✗	✗	✓
P($Protest$)	12.5%	23.4%	12.7%

2.6 Robustness

2.6.1 Aggregate Robustness

I test an alternate dependent variable for the aggregate analysis to test the robustness of my aggregate macroeconomic results. Rather than merely counting up the events one by one, the PolDem data set includes a measure of the weighted occurrence of the protest that adjusts for sample selection, newswire, and country population biases. I use this variable summed at the country-month level as a measure of the

[18] Tables made using XTable in R (Dahl et al., 2009).

weighted number of protests in that country during that month. I use the same Panel VECM analysis structure and expect similar results. Indeed, several combinations of income and wealth indicators have statistically significant interaction terms: $Wages$ and $Int.Rate$, $Wages$ and $Stock.Market$, and $Unemp$ and $Stock.Market$, shown in Table A.1. As in the analysis of protest participant counts, all error correction terms are strongly negative and statistically significant, implying that the long-term relationship between the variables drives protest count.

The first specification shows that in the long-term, protests are more associated with higher interest rates, but increases in wages mitigate this trend. This aligns with the findings from above using the weighted participant count as the dependent variable, providing further evidence that concerns about market liquidity are assuaged by increasing nominal income.

The second and third specifications show that movements in the $Stock.Market$ influence the number of protests. The second specification shows that in the long run, drops in $Wages$ correspond with more protests when the stock market is at its lowest value; drops in $Stock.Market$ also correspond to more protests when wages are at their lowest value. However, increases in either the stock market or wages mitigate the higher protests associated with decreases in the other. An increasing stock market increases asset values, decreasing worries about consumption smoothing; increasing wages increase nominal income and relieve the need to smooth consumption in the first place.

$Stock.Market$ also interacts with $Unemp$. The third specification shows that in both the long run and the short run, when the stock market is set to zero (its lowest value for that country), increasing unemployment corresponds to increasing protest counts. This effect is far stronger in the short run. In both the short run and the long run, increases in the stock market mitigate the higher protest counts that arise under rising unemployment while drops in the stock market contribute to even

higher protest counts. This is evidence that increasing asset values mitigate concerns about decreased nominal income among a society with increasing unemployment and falling asset values exacerbate them.

Although the individual variables involved are slightly different, this analysis' findings confirm previous findings: negative shocks to income and wealth are associated with higher protest counts and these associations are higher when both income and wealth are shocked simultaneously. The analysis supports the general conclusion that protest reactions to income shocks are highly contingent on the availability of insurance via private wealth.

2.6.2 Survey Robustness

The results are robust to inclusion of variables accounting for belief that voting is a duty and belief that society should make sure everyone has equal opportunity.

The ANES also includes data on other kinds of political activity: I construct binary variables for whether the respondent has boycotted a product for political reasons (*Boycott*) or signed a petition (*Petition*) in the last 12 months. My theory implies that the relationship between income, insurance, and protest exists because protesting provides an immediate avenue for demanding relief unconstrained by exogenous election timelines. Boycotting and petitioning, on the other hand, are acts of political participation that operate over a longer time frame than protests and thus are not suitable for immediate policy relief of poor economic circumstances that individuals without private insurance are unable to surmount. Therefore *Petition* and *Boycott* should be unrelated to a recently unemployed individual's access to public or private insurance.

The results indeed reflect these expectations. Table A.2 shows that although all three forms of political participation become more likely once an individual loses her job, the availability of private insurance is unrelated to recently unemployed person's

likelihood to petition or boycott products.

I also test for biased coefficient estimates resulting from omitting variables from the empirical specification. To mitigate unobservable omitted variable bias, I follow work by Gonzalez and Miguel (2015) and Oster (2019) designed to determine whether "unobservable characteristics would reduce the estimated coefficient of interest to zero" (Justino and Martorano, 2019: p. 2141). A consistent estimator of the effect of the main independent variables on the dependent variable can be expressed as

$$\bar{\beta} = \beta^* - (\beta - \beta^*) \times (R_{max} - R^*)/(R^* - R), \tag{2.4}$$

where β^* is the coefficient resulting from the regression after inclusion of all observable covariates and β is the coefficient from the regression without covariates computed using OLS with two-way fixed effects.[19] R^* is the R^2 from the regression with all covariates, R is the R^2 from the regression without controls, and R_{max} is the value of R^2 when controlling for all observable and unobservable factors (unknown), which is given by $(R^* - R)$.

If the test estimates values of $\bar{\beta}$ under different assumptions about R_{max} to be close to the model's estimated coefficient, the test provides evidence against large omitted variable bias. Table A.3 in the Appendix shows the percentage difference between $\bar{\beta}$ and the actual β for the survey specification. The results indicate that omitted variable bias decrease when the maximum claimed maximum explanatory power for the model approaches the R^2 of the model, 0.22.

2.7 Conclusion

In conclusion, I find individual-level evidence that access to wealth as measured by home ownership mitigates the effect to which income shocks induce protest. Aggre-

[19] Gonzalez and Miguel (2015), as cited in Justino and Martorano (2019). Assumes that observable and unobservable variables will have the same explanatory power.

gate protests in the face of macroeconomic income shocks decrease when measures of aggregate savings and wealth increase. At the individual level, I find that an individual's likelihood to protest after an income shock decreases markedly if she has private insurance via owning a home. I do not find evidence supporting the same relationship for public insurance.

This analysis has its drawbacks, however. It is weakened by its imperfect measures of private wealth and receipt of public insurance and would be improved by knowing the amount of income replaced by private wealth or social insurance. However, the weight of the evidence supports the hypothesis that income shocks are more likely to lead to protest when individuals have less insurance.

The connection I find between economic circumstances and protest might not surprise some readers. However, although there exists an abundant literature studying the relationship between income shocks and protest, existing studies conceive of an individual's economic circumstances as one-dimensional. In doing so, they ignores or obscures the reality of many people: economic well-being is dependent on accumulated assets or support from the government in addition to income. Until now, this remained an untested assumption.

I address this fundamental unanswered question in this paper. These results help explain why protests occur in response to changes in some economic indicators and not to others: although wealth and income can affect protests independently, their effects change based on the availability of the other.

One potential implication of these findings is that policies increasing public or private insurance can possibly improve societal resilience to income shocks. Possible future work includes separating out the type of protest resulting from different economic circumstances. Furthermore, a deeper understanding of these concepts could be gained from an interrogation of the relationship between these concepts across regions.

3

The Effect of Ownership Concentration on Government Bond Volatility and Yields

3.1 Introduction

Countries fund themselves using money from a variety of sources: taxation, loans, state-owned assets, and debt markets. Since the Brady debt restructurings in the early- to mid-1990s, countries have increasingly relied on issuing bonds. I seek to understand a dynamic of this market that can be opaque and unnoticed: the number of entities that own the government's debt. This paper will answer the question, "how does a concentrated ownership structure of government debt securities affect a government fiscally?"

The connection between governments and investors is increasingly important because of today's international financial markets. The government's ability to manage the possibly conflicting interests of bondholders and constituents is relatively understudied and stands apart from the literature on financial markets, collective action, bureaucracy, and regulation. Government capture by bond markets could lead to dependency, and the distribution of property among bondholders is something we

do not yet understand well. That creditors had enough bargaining power over the Argentine government to (temporarily) impound an Argentinian naval frigate shows that this topic matters to governments, investors, and financial intermediaries.

Forecasting financial market movements is important because it helps explain how investors capitalize political phenomena through specific changes in value of securities. Although equity markets can provide insight into tariff policy and others that affects publicly listed companies, government bond markets reflect the market's assessment of a government's ability and willingness to repay its debt. While aggregated cross-country analyses have found that countries with more bond debt have improved fiscal balances, I focus instead on the effects of changes in the ownership of specific securities. Because of this granular focus, my approach is useful for forecasting financial market movements.

The concentration of a security's ownership structure changes based on how many investors own what share of the security. For example, one bond whose outstanding debt is held exclusively by two investors is more concentrated that that of a bond that is held broadly by a large investor base. Notwithstanding the concentration of a security when issued, relative movements in the concentration could produce effects.

I propose that a concentrated ownership structure (fewer investors own more assets) of a government bond corresponds to higher secondary market yields. These higher yields then push up the primary market yields of debt issued to replace maturing securities, increasing future debt service payments. This paper contributes to the literature granular, security-level clarity of the relationship between ownership concentration and yield. More generally, it contributes a clear theoretical understanding of one way the ownership structure of a government bond can affect debt service costs. These findings have implications for redistribution and the tension between democratic accountability and economic credibility.

It is possible that attributes of the government borrower contribute to determin-

ing ownership concentration in the first place by affecting the investors who enter the market and the size of the positions they accrue. For example, governments with a large amount of outstanding debt who have robust repayment histories could be attractive to many types of investors, while governments who only come to market periodically or have a history of default or restructuring could only be within the risk tolerance of a small subset of specialized investors.

I address these concerns theoretically and empirically. Theoretically, I argue that while risk factors may affect an investor's decision to enter the market, the size of position (conditional on market entry) is likely determined by other factors. If an investor's position size is unrelated to their risk appetite, then the ownership concentration of the security must also be unrelated to risk, because concentration is dependent on the position size of all investors. I also implement empirical design and methodological controls to eliminate the effect of other issuer-related concerns that could affect both yield and ownership concentration.

The next section discusses existing literature that helps form the analytical basis for my theoretical framework, which is detailed in the third section. The fourth section discusses research design, the fifth discusses results, and the final section concludes.

3.2 Background

Why the bond market? Modern countries rely heavily on bond markets for funding from creditors that are institutional investors, individuals, central banks, sovereign wealth funds, international financial institutions, and others. Naturally, these investors have different budget constraints and investment objectives. Creditors exert bargaining power over governments, sometimes with substantial policy effects: the extreme version of this phenomenon is creditor bargaining power over a government after a default. The hold-out investors referred to above restricted Argentina's ability

to pay other creditors who had agreed to a debt restructuring before repaying them the original, un-restructured debt. One of the creditors' conditions to resolving the Argentinian standoff was input in future Argentinian domestic market fund-raising (Stevenson, 2016).

3.2.1 Aggregated PE Knowledge

To better understand how such ownership dynamics affect governments, the political economy literature offers several lessons. This literature has a strong tradition of examining the relationship between capital and governments (Przeworski and Wallerstein, 1988), and more recently the connection between financial markets and governments. But most of the recent literature focuses on only one of the two possible causal directions: the effect of various political phenomena on financial assets (Ferrara and Sattler, 2018). Political phenomena affect, among other things, the price level (Roberts, 1990; Campello, 2015) and volatility (Bechtel, 2009) of financial markets, as well as the currency composition (Ballard-Rosa, Mosley and Wellhausen, 2021) and maturity structure (McDade, Mosley and Rosendorff, 2021) of debt issuances. However, as Ferrara and Sattler (2018: p. 21) note, the connection between politics and financial markets is bi-directional: financial markets also affect the government. This should be particularly true for government bond markets.

Nevertheless, the political economy literature offers insights about why countries make choices about certain characteristics of the debt they issue. Countries can strategically choose to issue debt denominated in local currency or foreign currency in order to minimize currency risk or achieve domestic political goals (Eichengreen and Hausmann, 1999; Ballard-Rosa, Mosley and Wellhausen, 2021). Along similar lines, they can choose to issue short-term or long-term debt, choose to issue a large amount of debt at once or issue smaller amounts more frequently, and choose to default or not to default (Roos, 2019). But the effects of who owns debt securities

remain murky.

Two main works provide specific insight into the effects of concentrated ownership of government debt. The first argues that countries' unwillingness to default on sovereign debt derives in part from the increasing concentration of the global financial system (Roos, 2019). Because states can only really finance themselves via state-owned enterprises, taxation, or borrowing (O'Connor, 1979), such concentration imposes market discipline on debtor states by eliminating alternative financing options for countries in distress (Roos, 2019: p. 71). While Roos' analysis is insightful, it does not draw data on the ownership of particular securities, leaving room for interrogation of the mechanisms.

The second work digs deeper into the policy effects of bond market dependency. Kaplan (2013) offers a collective action explanation for how bond market indebtedness constrains fiscal policy. When faced with a fiscal situation that does not prioritize debt repayment, the small cost of market exit incentivizes bondholders to do so. Such market exit then "yield[s] a higher-risk premium quickly that translates into rising funding costs for sovereign borrowers" (Kaplan, 2013: p. 10). Countries with high bond market exposure, in this line of reasoning, are more susceptible to creditor influence, and tend to have more orthodox fiscal policy as a result.

Moreover, in a follow-up paper, Kaplan and Thomsson (2017) show that countries whose external debt is heavier on bonds exhibit greater fiscal balance. The authors conclude that because the "bond market" prefers governments to retain orthodox fiscal policy to better pay off debt, countries with more bond debt conform their fiscal policy to the position bondholders most prefer. But this work suffers from several flaws that muddy its conclusions. First, it depends on aggregated data that does not permit examination of the proposed mechanism, price pressure. Second, its conclusions depend on the assumption that bond market actors homogeneously prefer a certain kind of fiscal policy. If this assumption does not hold true, then

different actors in the market would do different things in reaction to government fiscal policy, not necessarily resulting in more expensive financing. Moreover, the authors do not probe the ownership structure of the bond debt itself; they merely consider its size in relation to the issuing country's total external debt.

3.2.2 Heterogeneous Preferences

For the bond market to function, investors must buy and sell bonds. Aside from risk-adjusted expected rate of return, there are two reasons to do so, each of which informs the investor's perception of default risk. The first, policy preference, is an ideal point on the policy spectrum of the government's ability to repay. The second, risk preference, is tolerance over deviation from that ideal point. For movement in the market to occur, there must exist some heterogeneity among bond market investors across policy or risk preferences such that different investors buy and sell debt under the same conditions.

Nevertheless, traditional capital market models such as CAPM and Black-Scholes assume homogeneous investor preferences; some authors argue that homogeneous preferences in these models does not accurately reflect the dynamics of equity markets and instead results in predictable and repeatable market cycles (Levy and Levy, 1996; Chan and Kogan, 2002; Abbot, 2017).

If investor preferences were homogeneous, markets should exhibit certain tendencies. Mosley (2000: p. 746) theorizes that when preferences are homogeneous, the policy consequences of investor behavior in the issuing country will be greater. Mosley finds that institutional investors use the same indicators to inform their decisions, namely inflation and fiscal balance. Therefore, if preferences were homogeneous and investors use the same information to inform decisions, markets should clearly react to microeconomic policy announcements. But Mosley, Paniagua and Wibbels (2020) show that prices of sovereign debt in bond markets do not systemat-

ically react to significant changes in microeconomic policy, implying that there is no entity called "the market" that reacts systematically, as a whole, to microeconomic policy changes. In fact, Brooks, Cunha and Mosley (2019) show that higher investor uncertainty about government willingness and ability to repay does not lead to the market agreeing upon a higher risk premium for that government's debt. Instead, different actors make different decisions, leading to higher volatility of bond spreads.

The economics and finance literature finds clear support for heterogeneous preferences. Even if institutional investors generally inform their actions with the same indicators (Mosley, 2000), investor preferences vary across three general categories. The first is belief about repayment, which can manifest in preferences over policy of the issuing government (Hardie, 2006; Brock and Durlauf, 2010; Mosley, Paniagua and Wibbels, 2020) or beliefs about the underlying economic growth rate (Cvitanić et al., 2012; Chabakauri, 2015). The second is risk preferences (Levy and Levy, 1996; Fischer, Arnold and Gibbs, 1996; Campbell and Viceira, 2001; Isaenko, 2008; Condie, 2008; Weinbaum, 2009; Sarasvathy et al., 2010; Christensen, Larsen and Munk, 2012; Cvitanić et al., 2012; Chabakauri, 2015; Hauser and Kedar-Levy, 2018), which results in some customers exiting markets before others (Hirschman, 1970: pp 33-43). The third is investment goals derived from investor position, like time horizon (Modigliani and Sutch, 1966; Wachter, 2003; Sangvinatsos and Wachter, 2005; Chan and Kogan, 2001; Isaenko, 2008; Cvitanić et al., 2012; Wellhausen, 2015) or liquidity (Hauser and Kedar-Levy, 2018; Chen et al., 2020). All three kinds of heterogeneity contribute to making markets work.

3.2.3 Effects of Ownership Structures

Heterogeneity means different bonds have different creditors who enter and exit the market at different times for different reasons. Therefore each asset has a particular ownership structure, which then has an effect on its price. For example, investor

movements into and out of managed investment funds can distort prices away from the fundamental values of the assets in which the fund invests (Vayanos and Woolley, 2013). Much of the scholarship on the pricing effect of the ownership structure of bonds analyzes what is called the preferred habitat hypothesis: that investors who prefer assets of a certain time horizon will propel movements in the prices of those assets (Modigliani and Sutch, 1966). Recent empirical work has found support for the preferred habitat hypothesis (Wachter, 2003; Greenwood and Vayanos, 2010), especially in relation to pension and insurance company demand for assets at the long end of the yield curve (Greenwood and Vissing-Jorgensen, 2018). The preferred habitat hypothesis is one example of how the heterogeneity that causes investors to enter and exit certain securities causes prices to move and results in concentration or dispersion.

3.2.4 Takeaways

Because the two main bodies of literature examining the effects of ownership structures remain largely unconnected, this phenomenon deserves another look. Political economy literature often relies on assumptions about market preference distribution and untested mechanisms driving conclusions. The finance literature analyzes these mechanisms, but stops short of security-level analysis of the pricing effects of ownership structures of government bond markets. I will attempt to bridge this gap by filling in some of the gaps in the political economy literature using tools from the finance toolbox.

3.3 Argument

Despite all the useful context, the literature leaves unanswered the relationship between ownership concentration and yield. The political economy literature in particular comes closest, but it does not offer empirical evidence at a granular enough level

of analysis to validate its mechanisms and depends on assumptions thoroughly refuted by the finance literature. Moreover, although political economists have offered general lessons about trends in government-finance relations, there is no clear answer to how the concentration of a security's ownership affects governments. I argue that government bonds with higher ownership concentration have higher price volatility. This higher volatility results in a volatility risk premium, which translates to higher future yields. I also address the potential that the "riskiness" of a security affects both its ownership concentration and its yield by arguing that the factors that drive an investor to enter a market are different from the factors that affect the size of the position they accrue. In doing so, I contribute security-level clarity of the relationship between ownership concentration and yield. More generally, I contribute a clear theoretical understanding of one way the ownership structure of a government bond can affect yields.

3.3.1 Ownership and Yield

One important characteristic of a security's ownership structure is how concentrated its ownership is among its investors. This ownership concentration can change independent of the security's price. The two arise from different properties of the buy/sell transaction: changes in price come from aggregated buyer willingness (i.e. demand) to buy the same quantity on offer at a different price than the seller is offering, and changes in concentration come from a different *number* of investors willing to buy the same amount of the security for the same price as the seller is selling. Concentration must increase, decrease, or stay the same with every transaction depending on whether the buyers number less than, more than, or the same as the number of sellers.[1]

[1] Consider an example. Seller X brings 10 shares of stock ABC to market when the market price is \$10/share. If 10 buyers are willing to buy one share each for \$10/share, price would remain constant but concentration would decrease. If one buyer is willing to buy five shares at \$10/share and another

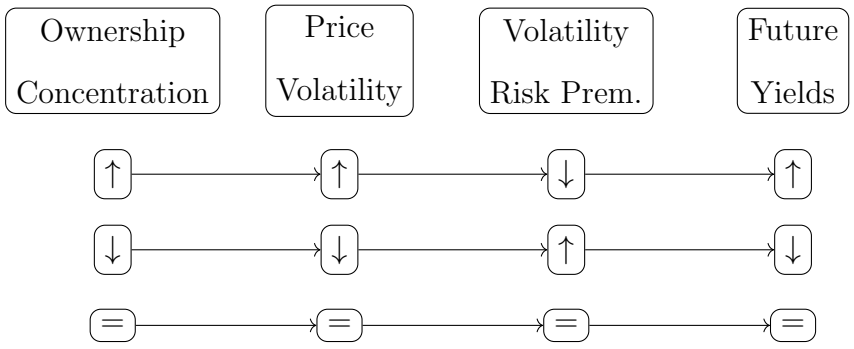
Like other measures of the ownership structure of securities, ownership concentration has been shown to affect prices. Greenwood and Thesmar (2011) show that ownership concentration makes asset price more susceptible to swings in non-fundamental flows such as movements into and out of a managed fund: securities with higher ownership concentration have higher price volatility. The intuition behind these findings is that, holding all else equal, when an asset is held primarily by several large players, any unexpected movement into or out of the security is "unlikely to be 'cancelled' by the trades of the other owners, resulting in price impact" (Greenwood and Thesmar, 2011: p. 472). But because liquidity shocks are inherently difficult to predict, the authors focus on predicting volatility.

The financial economics literature clearly establishes that higher volatility in bond prices is associated with a negative risk premium, implying that more volatile bonds have lower prices and higher yields (Almeida and Vicente, 2009; Chung, Wang and Wu, 2019). I extend these findings by arguing that investors demand a premium to compensate them for the volatility risk, including in situations where volatility risk is derived from a concentrated ownership structure.

Why does concentration lead to volatility? Greenwood and Thesmar (2011) show that in equity markets, more concentrated ownership corresponds to higher price volatility. The underlying reasoning is that in situations when each holder owns more of the asset, decisions to divest can result in larger quantities on the market at the same time, resulting in larger price swings. I expect similar dynamics to exist in bond markets. Although bond market prices incorporate information about the debtor's willingness and ability to repay the debt, many of the factors affecting debtor

buyer is willing to buy five shares at \$9/share, the price and concentration both decrease. If one buyer is willing to buy all 10 shares at \$9/share, price decreases and concentration remains steady. If one buyer buys all 10 shares from Seller X as well as 10 shares from Seller Y, but is only willing to pay \$9/share, concentration increases and price decreases. If the same buyer buys these 20 shares from Sellers X and Y but is willing to pay \$12 for each share, concentration and price both increase.

government willingness and ability to repay remain stable over time.[2] Prices change much more frequently than fundamentals do. These minute-to-minute movements in bond prices are not derived from changes in fundamentals; instead, they respond to external factors such as stock prices, investor liquidity, and changes in investor expectations about fundamentals. I expect that in a concentrated environment, these movements into and out of government bonds due to non-fundamental factors will result in higher volatility because sales by large holders will be unmatched on the demand side.



Drawing upon this reasoning, I expect securities with high ownership concentration to have higher price volatility that leads to higher secondary market yields. But why does this matter to governments? In the case of stocks, investor exit that causes a share price to drop doesn't have an immediate effect on the finances of the issuing company. But in the case of bond markets, it is possible to think of each investor having an exit *threat*: market exit via sale can negatively affect the issuing government. The political economy literature shows one high-level example of this, which relies on a more micro-level mechanism.

The high-level explanation is that bondholder exit threats are a failure of collective action that can result in "indirect influence over debtor governments" (Kaplan and Thomsson, 2017: p. 607). Analytically, exit from a bond market is more com-

[2] Such as domestic institutions, government composition, credit history, etc.

plicated than exit from other markets because even in a collective action failure the investor still retains most of his/her investment. Because each creditor has such a small share of the borrower's debt exposure, the creditor is incentivized to exit the market instead of holding their assets or providing new funds. Here, the collective good for which the creditors fail to bargain is a policy change that would increase government willingness and/or ability to repay the debt. In the bond market, decentralized creditors "benefit from their coordination problem," thereby "indirectly increas[ing] their influence over debtor governments": "if countries do not demonstrate commitment to policies that ensure debt repayment, bondholders can cut their financial ties without incurring a severe profitability shock" (Kaplan and Thomsson, 2017: p. 607).

But when governments issue bonds in the primary market, the coupon and yield to be paid upon maturity are agreed upon at the start; the secondary market is merely an appraisal of the issuing government's likely willingness and ability to carry out its promise to repay. So why would secondary market exit a-la-Kaplan and Thomsson affect the issuing government? The answer is that Kaplan and Thomsson's high-level explanation rests on a micro-level explanation: secondary market price changes affect the issuance terms of new debt, which in turn affect debt service payments.

When governments issue debt, they sell a tranche of debt securities to an underwriter at a previously agreed-upon price. The underwriter then resells the securities to actors over a secondary market, who buy the debt at market prices. In this way, the secondary market price can differ from the original price at which the government sells the debt security to the underwriter, and therefore from the amount due to the holder upon maturity. As market actors buy and sell a government debt security on the secondary market, its price and yield (face value of the debt minus the price) fluctuate accordingly. Higher demand corresponds to lower yields, and lower demand to higher yields. As debt securities reach their maturity date, the government issues

new debt to take the place of the maturing debt. The secondary market yield of the maturing debt then informs the yield at which the government issues the new debt (Duffie, 2010; Lou, Yan and Zhang, 2013; Eisl et al., 2019; Cole, Neuhann and Ordonez, 2020; Sigaux, 2020).

Earlier, I said I expect that securities with more concentrated ownership structures will have higher price volatility and therefore higher secondary market yields. Because secondary market prices inform the terms of new bond issuances, any pricing effects of ownership structure should affect the yields of subsequent issuances. This is important because governments with higher ownership concentration across their debt securities could, in the long run, be required to pay higher yields on debt to attract investors, leading to higher debt service levels.

3.3.2 Investor Selection

However, it is possible that the "riskiness" of a security could influence both its ownership concentration and its yield. If true, there is a potential that a security's riskiness could confound the relationship between concentration and yield. The relationship between yield and riskiness is fairly intuitive: investors will demand a higher premium to invest in the debt of a country if the risk of default is higher. But the relationship between riskiness and concentration merits consideration in more detail. It is possible that only certain kinds of investors are willing to buy into a security of a certain riskiness, and that riskiness also affects the investor's position. By affecting the selection of investors and the size of their positions, riskiness could affect concentration.

To resolve this endogeneity, I propose an investor-level complement to this theory, which has so far focused on the security level. An investor taking a position in a security is a two-step process: first, an investor decides to enter the market or not, and, contingent upon entering the market, the investor decides what size position to

take. I define the "riskiness" of a debt security to be the market's valuation of the likelihood the security's issuer will be unwilling or unable to repay.

If a security's riskiness affects ownership concentration, it should do so by affecting both investor decisions: market entrance (the selection stage) and position size (the outcome stage). But the riskiness of the security itself is not the operative concern, nor the riskiness of the issuer, since those are constant across all investors at any given point in time. Rather, the characteristics of the investor push different investors to make different decisions using the same information.

I expect that there is enough variation in investor tactics that investors will vary significantly in their interpretation of the same information. Consider, for example, credit default swap (CDS) spreads as a measure of issuer "riskiness."[3] CDS spreads are commonly available information; all investors can use them as a benchmark of the market's expectation that an issuer might default. If spreads make a move and the market reacts, by definition some investors took that movement as a signal to sell and others took it as a signal to buy. There are two questions: whether these groups of investors who co-move all have the same risk tolerance and whether risk tolerance affected the position decisions of those who ended up entering the market.

I expect that the two processes are different – investors use different criteria to decide when to get into a market than to decide about the size of their position. While traits such as risk acceptance and the characteristics of the fund (price-to-earnings ratio, etc.) likely affect whether or not to invest in the security at all, other factors likely affect the size of the stake once they have decided to enter. For example, leverage and non-fundamental flows could affect the amount of capital available to invest (Greenwood and Thesmar, 2011); movements in other markets likely affect

[3] In such a security, the purchaser buys the CDS as insurance against the issuer defaulting. If the issuer defaults, the CDS provider (lender) reimburses the CDS purchaser. CDSs are usually paid for incrementally in a manner similar to an insurance premium, and their "spread" is the annual premium in relation to the notional amount insured, expressed in basis points.

how hedged investors wish to be; whether the investor uses fundamentals-based or quantitative investing strategies likely affects their investment tactics (Satchell and Scowcroft, 2000); whether an investor is a pension fund or an insurance company or a hedge fund can affect their preferred environment (Modigliani and Sutch, 1966); behavioral and demographic attributes can also affect portfolio construction (Frijns, Koellen and Lehnert, 2008).

3.3.3 Hypothesis

This theoretical set-up leads to two hypotheses.

1. Ceteris paribus, a more concentrated ownership structure for a government bonds will lead to higher secondary market yields.

2. Although a security's riskiness may affect investor decisions about market entrance, I do not expect it to have a significant relationship with position size.

3.4 Research Design

To isolate the effect of ownership concentration on security price volatility and yield level, I undertake a three-part empirical approach. First, I examine the way investors select into the market to assess my expectation that the processes driving selection and position size are indeed different. Second, I examine the time-series relationship between ownership concentration and return volatility of a single representative security. Third, I extend these findings with a time-series cross-sectional approach for the population of bonds for which complete data is available.

3.4.1 Empirical Setting

I set my empirical study in California municipal bonds from 2013 to the present. This empirical setting keeps constant many variables that affect market perception

of government ability and willingness to pay: the issuing entity itself remains the same, maintains largely continuous fiscal policy, and issues all its in US Dollars.

Moreover, Californian municipal bonds have several qualities that make them suitable for this study. They are numerous, cover a long period of time, have varying maturities, are widely invested in, and have been a continuous financial tool over the last decade. They are common investments for institutional investors, pension funds, and mutual funds. But even though municipal bonds are well-traded by institutional investors, they remain outside the mainstream of financial assets. Their slightly niche nature means that however well-capitalized their investors are, municipal bonds are not as good as cash; in some cases, they go days without a trade. Therefore, many municipal bonds are subject to influence by individual market actors.

3.4.2 Data Description

Assessment of this hypothesis has stringent data requirements: to my knowledge, this paper is the first time that a comprehensive data set of bond ownership has been used in political economy literature. First, holdings data on government bonds is quite difficult to come by. Even when procured, it is limited by the reporting requirements of the relevant jurisdictions. I source ownership data from the FactSet Standard Ownership Data Feed V5. This data describes each holder of a bond: who they are, how much of the security they hold, and more (FactSet, 2022). FactSet sources this data from regulatory filings as well as text-based data from investor websites and portfolio descriptions. I derive the ownership concentration of the security from this data.

Secondly, because my theory relies on time-series pricing of securities, I must obtain a historical security-level pricing data. Even in high-fidelity commercial data repositories like Bloomberg, such data is spotty at best. I source historical municipal bonds pricing data from the Municipal Securities Transaction Database from

the Municipal Securities Rulemaking Board (MSRB) (Municipal Securities Research Bureau, 2022). I source descriptive data on each security (e.g. coupon rate, maturity date, amount outstanding) from Bloomberg (Bloomberg, 2022).

I also incorporate data to account for other possible explanations. For example, the amount of debt that a security has outstanding could affect volatility by changing the market size, offering more or less liquidity. I include security-level characteristics such as a security's yield at issue, its coupon structure, its maturity length, and its monthly close price. It is possible that securities that vary across these attributes could exhibit different volatility patterns, so I include them as explanatory variables in my time-series cross-sectional models. Furthermore, the months remaining until a security's maturity is likely related to the amount and kind of transactions in the secondary market, and therefore to security volatility. I also include it as an explanatory variable.

3.4.3 Empirically Accounting for Endogeneity

It is possible that a country's underlying "riskiness" affects both the ownership concentration of its debt securities and the yield of those securities, raising concerns about endogeneity. The independent variable, ownership concentration, is a function of the decisions of individual investors to enter the market or not, which is likely related to whether or not investor attributes (e.g. risk acceptance) align with the underlying riskiness of the security. The dependent variable, yield of the bond, reflects many things, chief among them the premium required to compensate investors for the possibility that the debtor is unwilling or unable to repay – that is to say, riskiness.

I take several steps to address the endogeneity concern. First and foremost, I select a research design that reduces the effect of riskiness as a confounding variable. I restrict the empirical setting to one issuer, which means the issuer's budgets, con-

stituents, and services remain more stable than if I considered multiple issuers. This reduces variation in aspects of the government that could contribute to riskiness – if these attributes do not change over time, they cannot affect the regression results. Moreover, I examine the entire population of that issuer's securities during the time period in question in order to avoid sample selection bias.

Ideally, I would attempt to eliminate the effect of riskiness on my empirical models by including a security-level measure of California's riskiness. But the most commonly accepted measure, credit default swap (CDS) spreads, are not security-specific; they are a general indication of the creditworthiness of the issuer at a given point in time. Figure 3.1 shows that although CDS spreads are priced differently for debt with different maturity lengths, these spreads follow the same general pattern over time. Unlike price, volatility, and ownership concentration, CDS spreads do not vary by security over time and therefore cannot explain the security-level relationship between ownership concentration and volatility; I exclude them as a measure of riskiness of the issuer.

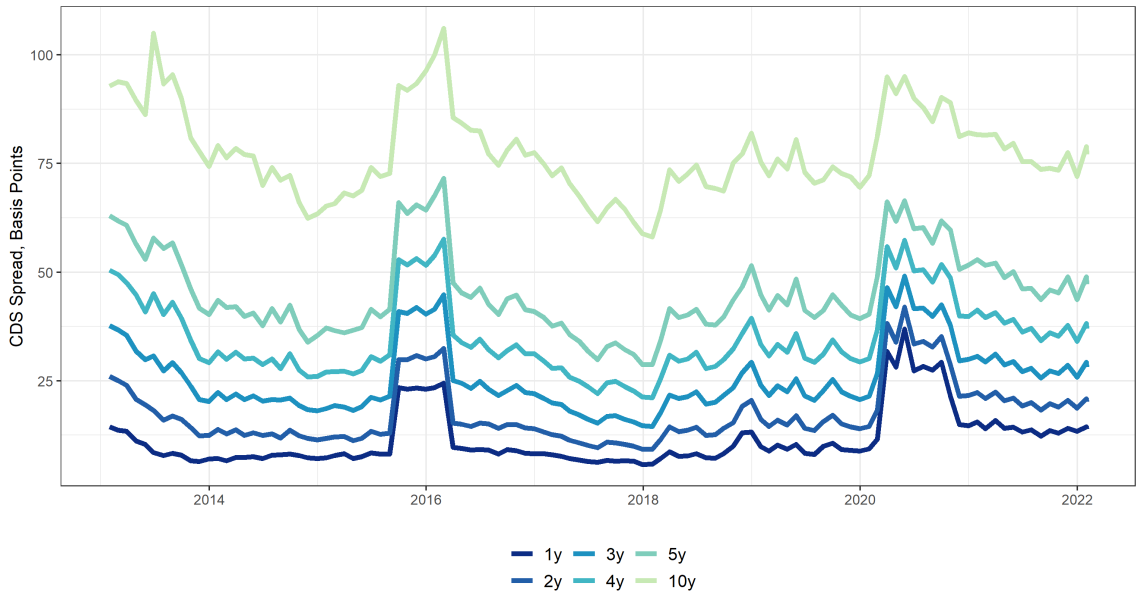


FIGURE 3.1: California CDS Spread Prices.

Moreover, because CDS spreads are the same for the whole market at any given time, they cannot actually cause any investor decisions. Rather, decisions are caused by the way that different investors process the same information. Another way I address endogeneity is to analyze the way that investor attributes, including risk acceptance, drive investor decisions about market entry and position size. Such an analysis helps contextualize the relationship between investor risk acceptance and concentration and therefore contributes to my main theoretical concern: the relationship between concentration and yield. An investor's choice about whether to enter the market for a particular security, the "selection effect," is a separate decision from how big a position to take once the investor enters the market. These two decisions could be driven by the same factors or two separate sets of factors.

I compare these two sets of drivers. Any discrepancy between the drivers of selection and position size would help provide evidence that although investors with certain attributes (e.g. risk accepting) may select into the market, those investors do not automatically develop large positions. Such a result would suggest that the processes driving selections and positions are different. A lack of relationship between investor risk acceptance and position size suggests a limit on the relationship between risk and concentration. More specifically, if an investor's risk acceptance is related to selection but not to position, then risk likely does not affect concentration and therefore is likely not a confounding variable.

To carry out such an analysis, I merge the security-level data set used in the first part of the analysis with a data set describing the holders of debt (FactSet, 2022). Specifically, the data includes attributes of specific funds such as the price-to-earnings (PE) ratio, price-to-books (PB) ratio, dividend-yield ratio, market beta, and other descriptors of the fund's portfolio. This data set covers some 150,000 funds, only several dozen of which have holdings large enough to report during any given period. One disadvantage of this data set is that it is a static snapshot of the most recent val-

ues for a given fund. Ideally, I would have all these attributes in time-series, but this may not be a large disadvantage because funds oftentimes pick portfolio attributes in advance of launching the fund and only rebalance periodically at the margins. As a result, I do not expect that the static nature of this data set will contaminate my results. Nonetheless, I do exclude some metrics that are time-dependent, such as those that illuminate the price movements of the fund's portfolio. This does not hamper testing my theory, however, because I am theoretically interested in more static attributes like general risk profile, as reflected in indicators PE ratio and beta.

3.4.4 Variable Definitions

The data set starts with descriptive data on all Californian municipal bonds issued after 2002 from Bloomberg, containing information such as issuance dates, maturity dates, coupon rates, ratings, yield at issue, and more (for 4,709 securities). MSRB has pricing data for 3,999 of these securities, but FactSet only has ownership data for 1,713 of them.[4] There are 1,661 securities that have both pricing and holdings data, only 399 of which had non-null holdings data. Of these 399, only 136 have data on the amount of debt outstanding at any given time, which is necessary to calculate percentage ownership. Only 114 have $h_{im} \in [0, 1]$.

This description of the data set immediately an issue: the overall lack of holdings data. Holdings data is much harder to come across than security details or pricing information. Moreover, holdings data is only available from 2013 to the present. Although holdings data is available individually for some bonds before 2013, this early holdings data is inconsistent across time, type of bond, and often within one bond. Some of this inconsistency can be explained by issuers calling a bond before its maturity date, resulting in some bonds having reported holdings data for only a subset of their original maturity.

[4] Ownership data is only available after 2013, and some securities had matured by then.

I use the Hirschman-Hirfindahl Index (HHI) to measure my main explanatory variable, ownership concentration.[5] For security i in month t with total amount outstanding o_{it}, I calculate the ownership concentration h_{it} across all owners $j = 1, 2, \ldots n$, where holder j holds a amount of the security, to be

$$h_{it} = \sum_{j=1}^{n} \left(\frac{a_{jit}}{o_{it}} \right)^2 . \tag{3.1}$$

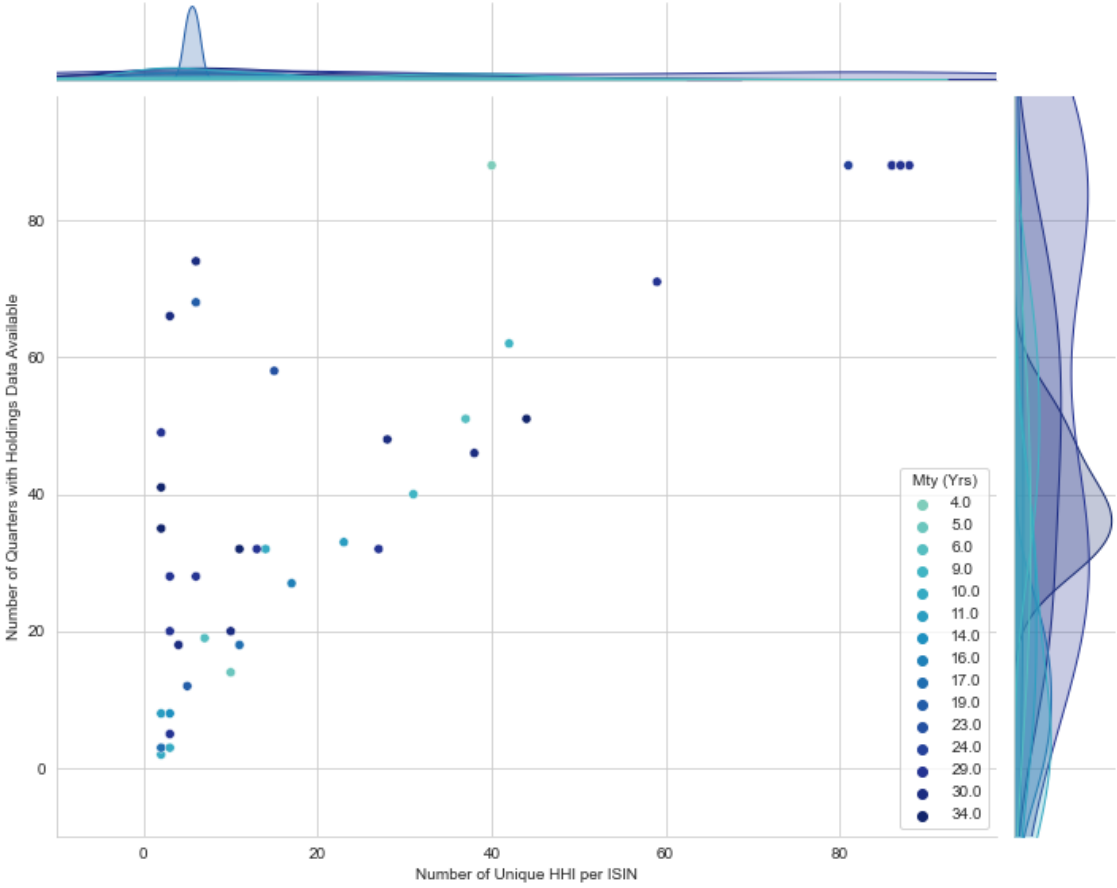


FIGURE 3.2: Data availability for securities with holdings data.

Figure 3.2 shows that the ownership concentration of many bonds does not change much over time. The x-axis shows the number of unique values of HHI that a security

[5] In line with (Cetorelli et al., 2007; Peltonen, Scheicher and Vuillemey, 2014; Boermans, 2015), among others.

has had; higher values indicate more changes in ownership concentration. The y-axis shows the number of months for which a security has available holdings data; higher values indicate more data. The vertical cluster going up the left-hand side shows that it is fairly common for securities to exhibit stable ownership over time.

Several factors likely contribute to this phenomenon. Bond investors can hold a position over a long period of time because they seek conservative long-term returns or because they are passive investors (Sangvinatsos and Wachter, 2005; Sushko and Turner, 2018). Moreover, only some holders are required to report, so those that do report can often be institutions who hold stable positions. Lastly, bond investors often do not pay careful attention to the contract terms of their bonds and their positions may remain constant as a result (Kahan and Klausner, 1997; Gulati and Scott, 2012; Gulati and Kahan, 2018; Kahan and Gulati, 2021). Even though we may know some of its causes, this lack of variation in h_{it} poses an analytical challenge.

However, there are some securities that have both a long time-series of available data and a variation in h_{it}. A security's price volatility is typically measured with respect to a trailing time frame: the annualized standard deviation of the logged daily price differences of the past n trading days. For price p, I calculate the n-day price volatility v_{it}^n of security i to be

$$v_{it}^n = \sigma\left(\left[\forall d \in [t-n, t] \mid \ln\left(\frac{p_{id}}{p_{id-1}}\right)\right]\right) * \sqrt{(}252). \tag{3.2}$$

Figure 3.4 shows h_{it} plotted over time, at a monthly level, alongside the daily volatility v_{it}^{10}, v_{it}^{90} for what I will call my showcase bond: CUSIP 13063A5G5. This security is a 30-year bond issued in 2009, maturing in 2039, rated AA-, and is the largest bond California currently has outstanding at 3 billion USD. Figure 3.3 shows h_{it} plotted alongside its price. Because price, yield, and volatility are reported at the daily level; for the time-series cross-sectional analysis I take the monthly average to

match the level of analysis of the holdings data.

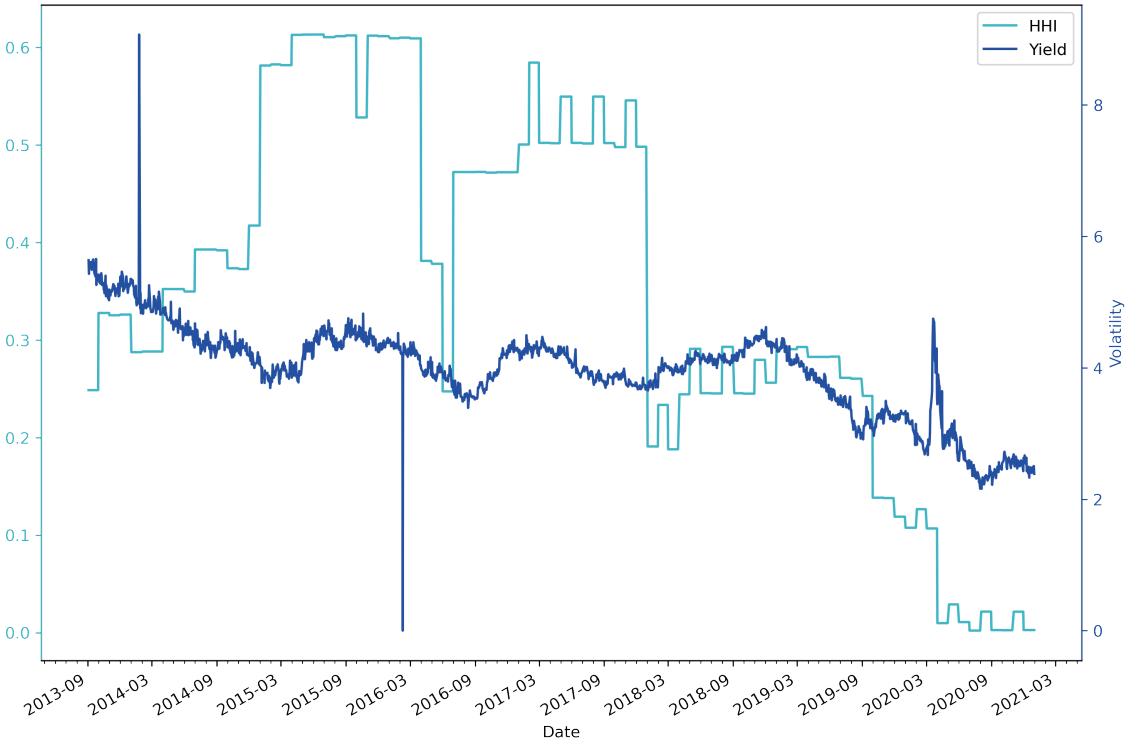


FIGURE 3.3: Price and ownership concentration over time.

For the investor selection analysis, I merge the security-level data set with a data set describing the holders of debt (FactSet, 2022). Specifically, the data includes attributes of specific funds such as the PE ratio, PB ratio, Dividend-Yield ratio, market beta, and various descriptors of the price momentum of the fund's portfolio. This data set covers some 150,000 funds, only several dozen of which have holdings in a given security large enough to report during any given period. One disadvantage of this data set is that it is a static snapshot of the most recent values for a given fund. Ideally, I would have all these attributes in time-series, but this may not be a large disadvantage because funds oftentimes pick portfolio attributes in advance of launching the fund and only rebalance periodically at the margins. As a result, I do not expect that the static nature of this data set will contaminate my results.

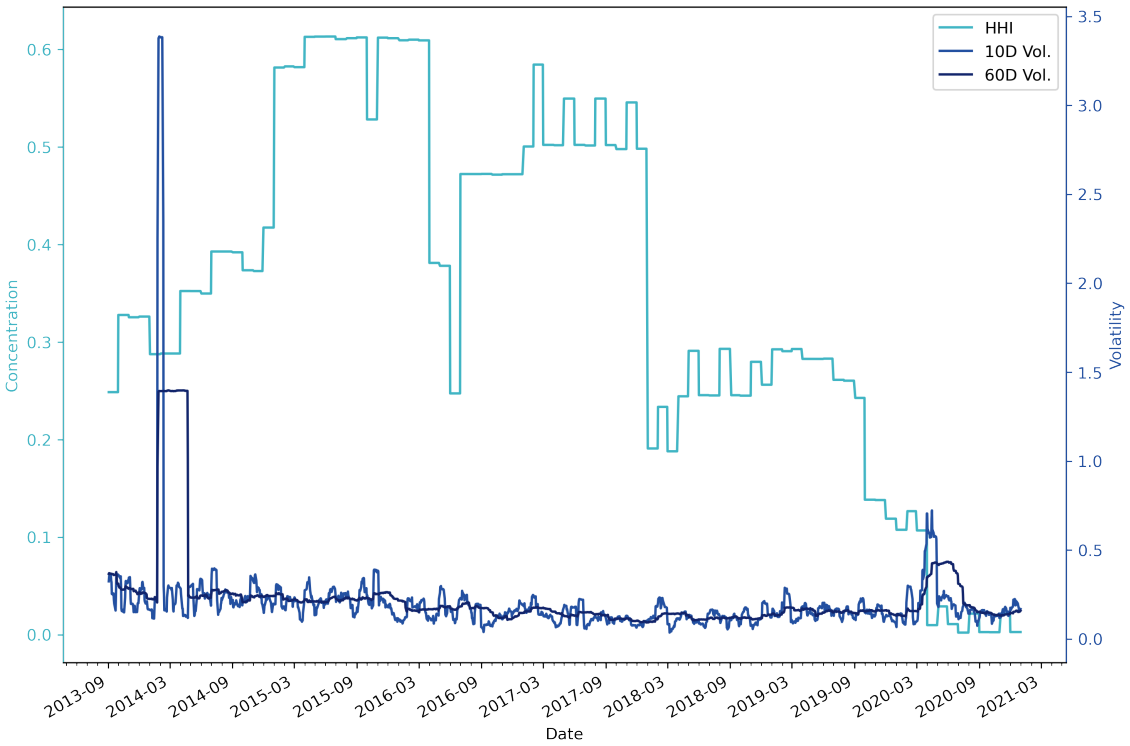


FIGURE 3.4: The volatility and ownership concentration over time.

Nonetheless, I do exclude some metrics that are time-dependent, such as those that illuminate the price movements of the fund's portfolio. This does not hamper testing my theory, however, because I am theoretically interested in more static attributes like general risk profile, as reflected in indicators PE ratio and beta.

I include all investors in my selection data set. This includes all "funds" (e.g. mutual funds, exchange-traded funds, pension funds. etc.) who have taken a position in a security large enough to require public reporting. Naturally, this results in the vast majority of funds without a position in a given Californian bond during a given month. I focus on two major attributes of these funds: beta and PE ratio. The beta of a fund is a measure of its volatility in relationship to the overall market.[6] A PE

[6] Values of beta greater than one mean the fund is more volatile than the market, a beta of one means that it is equally as volatile as the market, and values between zero and one mean the fund is less volatile than the market. Negative betas mean the fund exhibits an inverse relationship with

ratio is a measure of a stock, but the aggregate PE ratio of a fund is the weighted average of the PE ratios of the stocks in its portfolio. Although not related to the bonds it holds, a fund's PE ratio is a loose measure of its risk. With a range set in advance of the fund's launch, the PE ratio has two attributes that makes it appealing for inclusion in this analysis. First, it is relatively static over time. Second, even though it does not pertain directly to bonds, it is a general measure of how a fund positions itself with regard to growth and value, and therefore risk.

3.5 Empirical Models and Results

I conduct three empirical analyses. The first assesses whether riskiness affects both investor selection into the market and investor's position size contingent upon entering the market. The second uses an approach new to Political Science literature to consider the effect of concentration on price volatility of the largest bond that California currently has outstanding. The third approach extends these analyses to a more conventional time-series cross-sectional analysis.

3.5.1 Investor Selection Results

To recap from earlier, an investor purchasing a position in a security is a two-step process consisting of a decision to enter the market or not and, contingent upon entering the market, a separate decision about the size of position to accrue. I expect that selection into the market is driven by risk attributes and position size is driven by different concerns. once they are in the market. Results supporting my hypothesis would show that the investor attributes driving market entrance are indeed different from those driving position size. Such results would provide basis to conclude that a security's "riskiness" is unrelated to the size of position that investors take, and therefore that riskiness cannot affect ownership concentration.

the market (consider gold and the S&P 500).

Such findings would alleviate concerns about endogeneity and permit moving on to a security-level analysis of the relationship between ownership concentration and volatility.

To adjudicate my hypothesis, I employ a Heckman selection model to understand the two stages of my theorized investor-level decision-making process. In the first stage (the selection stage), investors decide whether or not to take a position in the security in question. In the second stage, those investors who have decided to take a position decide what size position to take.

There are some implementation issues, however. First, my data set is a panel data set of individual securities over time. The panel nature of this is complicating; it is difficult to estimate selection models for panel data. Because the data set of investors is so large, I settle on focusing on the dynamics of a representative security. I will call the security in question my showcase bond, introduced above. Second, the time-series nature of the data poses a concern: it is possible that an investor's lagged position predicts their current position. I include lagged position as a predictor in both stages of the analysis. I estimate separate models for each month and consider the effects of the variables over time.

I estimate a two-stage selection model, where the first stage predicts whether investor i enters the market at time t, given by $z_{it} \in \{0, 1\}$, and the second stage predicts the investor's position in the security conditional on market presence, given by y^*_{it}:

$$z_{it} = \gamma X'_i + u_i, \tag{3.3}$$

$$y^*_{it} = \beta X'_i + \epsilon_i, \tag{3.4}$$

where X_i is a vector of investor attributes (including position in the security at $t-1$), and y^*_{it} only observed if $z_{it} = 1$. I include as independent variables several

attributes of portfolios: PE ratio, PB ratio, and Dividend-Yield ratio. This last measure has no relationship with the particular time value of a particular security and therefore is not post-treatment; it is merely a description of how much a fund's portfolio pays out in dividends relative to its return.

However, I expect the PE ratio to absorb most of the cross-investor variation in selection. Because it is important to consider the distribution of PE ratios across the market, I include in X_i a term for the squared PE ratio. My hypothesis will find support from a statistically significant relationships in γ but not β. More specifically, I expect to find that γ contains statistically significant relationships between PE ratio and market selection, with a positive coefficient for the first-order term and a negative relationship for the squared term. These choices reflect my expectation that PE ratios are normally distributed across the market.

Table 3.1 shows the results for both selection and outcome stages of the model for the latest time period in the data set, December 2020. The results indicate that investors with higher PE ratios are more likely to have a position, but the effect of PE ratio on position size is statistically indistinguishable from zero. The results of this regression indicate a statistically significant quadratic relationship between PE ratio and market selection, suggesting that a fund's placement within the market is closely related to how appropriate they view the security in question. For these investors, there is a sweet spot around 30: the average PE ratio for the Standard and Poor's 500 index in March 2022 is 24.56, implying that funds that focus on California municipals have a higher PE ratio on average than the market (Nasdaq, 2022). This suggests that Californian municipals are more likely to be in the portfolios of funds that have a growth mindset.

But Table 3.1 shows just a snapshot in time. Figure 3.5 shows how the effect of PE ratio on selection changes over time. Although not every time period shows a statistically significant relationship, those that do usually have a strong positive

Table 3.1: Regression Results, Heckman Selection, December 2020

	Dependent variable:	
	Investment Binary	Position Size
	probit	*OLS*
	Selection	Outcome
	(1)	(2)
Beta	−0.00001	−129,769.5000
	(0.0014)	(280,298.6000)
PE Ratio	0.0429***	526,093.0000
	(0.0157)	(631,892.2000)
PE Ratio Sq.	−0.0005*	−6,571.7570
	(0.0003)	(7,539.9630)
PB Ratio	0.0054	−14,407.4900
	(0.0041)	(162,773.8000)
PB Ratio Sq.	−0.1834	−2,542,321.0000
	(0.1194)	(2,749,789.0000)
Dividend Yield	0.000000***	5.1343
	(0.000000)	(5.8762)
Lagged Holding		11,224,022.0000
		(15,878,167.0000)
Inv. Mills Ratio	−4.1126***	−48,562,448.0000
	(0.2292)	(68,718,866.0000)
Observations	116,928	21
R^2		0.9695
Adjusted R^2		0.9530
Log Likelihood	−181.8180	
Akaike Inf. Crit.	377.6360	
Residual Std. Error		216,018.7000
F Statistic		58.9808***
Note:		*p<0.1; **p<0.05; ***p<0.01

effect on selection. The squared value of the PE ratio also has a persistent, negative, statistically significant relationship with selection over time. Taken together, these results provide strong evidence that the attributes of an investor are closely tied to whether or not that investor enters the market for a security. [7]

Figure 3.6 shows that the non-relationship between PE ratio and portfolio size

[7] Figures 3.5 and 3.6 have been filtered to only show the time periods with statistically significant relationships. For full results, see Figures B.1 and B.2 in the Appendix.

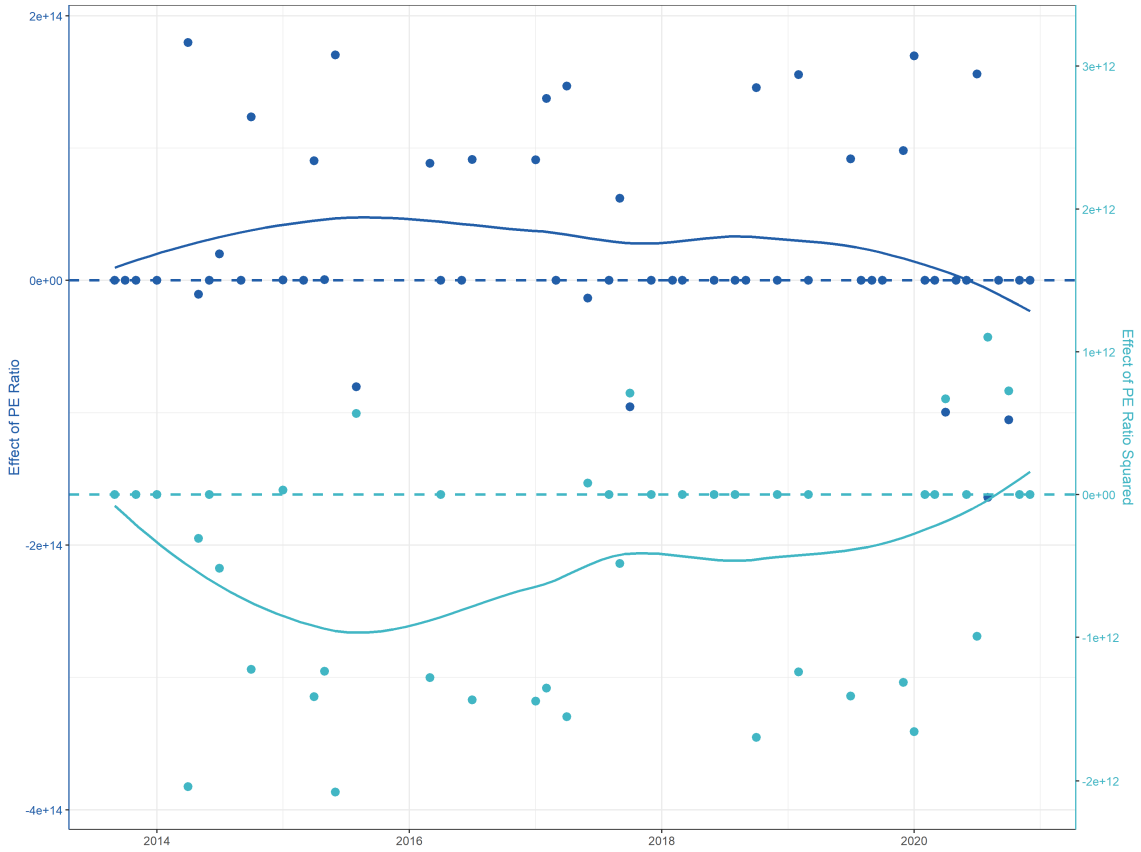


FIGURE 3.5: The Effect of PE Ratio and PE Ratio Squared on selection into the market over time.

is persistent over time. Although the number of observations is far lower for the outcome model because of the limited number of investors who are required to report holdings, an inconclusive relationship is persistent over time. This suggests that other attributes affect portfolio allocation decisions, which is consistent with my expectations.

An investor's PE ratio has a statistically significant relationship with the selection stage but not with the outcome stage. This is consistent with my theoretical expectations: when a fund is created, investors decide what the fund's profile will be with regard to asset class, risk tolerance, and returns. Such decisions are made well before particular securities are considered for inclusion in the portfolio. This

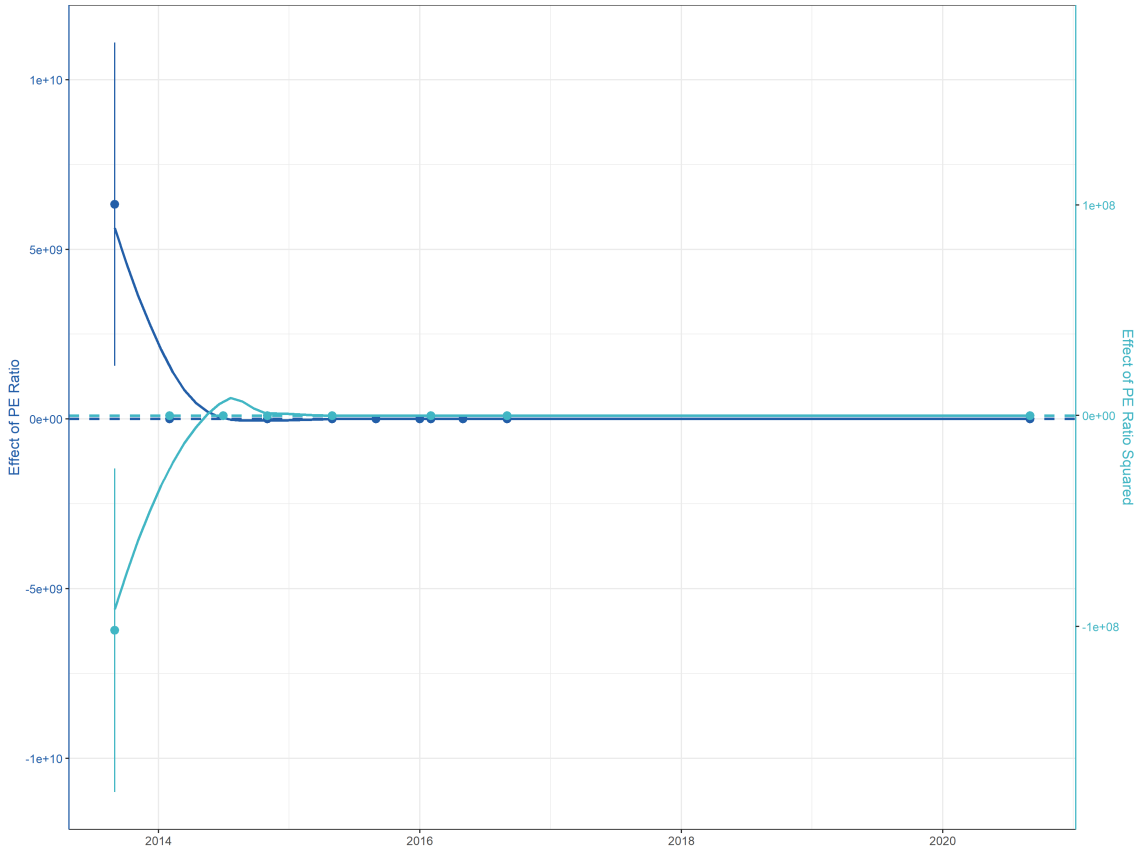


FIGURE 3.6: The Effect of PE Ratio and PE Ratio Squared on investor position size over time.

explains why funds with a certain PE ratio are more likely to invest in Californian municipals. Furthermore, the results of the outcome stage of the selection model indicate that these same fund attributes are unrelated to position size. This supports my expectation that position size is determined by other factors aside from the risk matching between the investor and the security in question.

Moreover, these results provide support to my theoretical expectation that the "riskiness" of a debt security is not related to the size of position an investor takes. Without this relationship, it is not possible for riskiness to confound the results of the prior stage of analysis. These results undergird the empirical support for my expectation about the relationship between concentration and yield.

3.5.2 GARCH-MIDAS

Analysis

Now that there is some baseline support that ownership concentration is not affected by a risk match between a security and investors, I move on to examining the relationship between concentration and yield volatility.

Ownership concentration is reported at the monthly level but security prices fluctuate daily. To account for these different sampling frequencies of h_{it} and v_{it}^{n}, I use a technique common in the finance literature: a GARCH-MIDAS approach. GARCH-MIDAS[8] models combine two kinds of empirical models to suit this analysis situation (Engle, Ghysels and Sohn, 2013). By assuming an ARMA (AutoRegressive Moving Average) structure for the error variance, GARCH models account for two tendencies of financial pricing data: autoregression and oscillation between periods of high and low volatility (Bollerslev, 1986).[9] MIDAS models allow for inclusion of variables sampled over different time periods, such as holdings data sampled monthly and pricing data sampled daily (Ghysels, Santa-Clara and Valkanov, 2004). GARCH-MIDAS models combine the two approaches by modeling short-term fluctuations of a GARCH component "around a time-varying long-term component that is a function of (macroeconomic or financial) variables" (Conrad and Kleen, 2020: p. 19). GARCH-MIDAS models have been increasingly popular in the finance literature to use macroeconomic variables to explain volatility of stocks (Girardin and Joyeux, 2013; Asgharian, Hou and Javed, 2013; Wang et al., 2020), cryptocurrencies (Conrad, Custovic and Ghysels, 2018), exchange rates (You and Liu, 2020), and commodities (Pan et al., 2017), and tend to outperform other specifications (Conrad and Kleen,

[8] Generalized AutoRegressive Conditional Heteroskedasticity-MIxed DAta Sampling.

[9] Where volatility is a function of both the prior volatility and the value of the error in the prior period. This is opposed to the ARCH model, which assumes the error follows an AR model.

2020: p. 20).[10]

Because of the intense computational requirements of the GARCH-MIDAS model, it is not possible to include a panel of covariates, so I consider only the relationship between ownership concentration and return, leaving inclusion of covariates for the time-series cross-sectional analysis in the next section. For log return on day i in month t, the GARCH-MIDAS approach follows the process given by:

$$r_{it} - E_{i-1,t}(r_{it}) = \sqrt{\tau_t g_{it}}\epsilon_{it}, \forall i = 1, 2, \ldots, N_t, \tag{3.5}$$

$$\epsilon_{it}|\psi_{i-1,t} \sim N(0,1), \tag{3.6}$$

where N_t is the number of trading days in month t, $E_{i-1,t}()$ is the conditional expectation given information up to time $(i-1)$, and $\psi_{i-1,t}$ represents the information set up until day $i-1$ of period t. The volatility has two separate components: the short-term component g_{it} and the long-term component τ_t, initially assumed to be fixed for month t. Because I expect ownership concentration to influence short-term volatility, I choose a mean-reverting unit-variance GJR-GARCH(1,1) process for the short-term component g_{it}:

$$g_{it} = (1 - \alpha - \gamma/2 - \beta) + (\alpha + \gamma \mathbb{1}_{\{\epsilon_{i-1,t}<0\}})\frac{\epsilon^2_{i-1,t}}{\tau_t} + \beta g_{i-1,t}, \tag{3.7}$$

where $\alpha, \beta > 0$ and $\alpha + \beta < 1$. The long-term component τ_t follows the smoothed realized volatility of the MIDAS regression:

$$\tau_t = m + \theta \sum_{k=1}^{K} \phi_k(\omega_1, \omega_2) RV_{t-k}, \tag{3.8}$$

where RV_t denotes the fixed time realized volatility RV at time t:

[10] See also Conrad and Kleen (2020); Virk et al. (2021).

$$RV_t = \sum_{i=1}^{N_t} r_{it}^2. \tag{3.9}$$

$\phi_k(\omega_1, \omega_2)$ is the function that defines the weighting scheme of MIDAS filters parameterized via the Beta weighting scheme (Conrad and Kleen, 2020):

$$\phi_k(\omega_1, \omega_2) = \frac{(k/K)^{\omega_1 - 1}(1 - k/K)^{\omega_2 - 1}}{\sum_{j=1}^{K}(j/K)^{\omega_1 - 1}(1 - j/K)^{\omega_2 - 1}}, \tag{3.10}$$

where the weights sum to one:

$$\sum_{\ell=1}^{K} \phi_\ell(\omega_1, \omega_2) = 1.$$

I follow the convention established in the literature by Engle (1982) and others in regressing

$$X_t = \sum_{j=1}^{12} \alpha_{jt} D_{jt} + \sum_{j=1}^{12} \beta_j X_{t-j} + \epsilon_t, \tag{3.11}$$

where D_{jt} is a monthly dummy variable. The squared residuals ϵ_t^2 are taken as the proxy of volatility of macroeconomic variable X_t.

The GARCH-MIDAS approach can account for the mixed sampling frequencies of the data points, but cannot account for the panel structure of the data. Moreover, the varying availability of historical holdings data does not facilitate a GARCH-MIDAS approach for every security. I accordingly run a GARCH-MIDAS model for the spotlight bond mentioned above as a proof of concept. This approach obviously lacks the desirable quality of generalizability across the entire universe of Californian

municipal bonds, but does showcase the time-series relationship between ownership concentration and volatility.

My hypotheses would find support from statistically significant estimations of $\alpha,\ \beta,\ \gamma,$ and θ.

Results

As a result, I present models for the two bonds with the longest time series of complete data. I analyze the same bond as in the selection stage, the showcase bond, which has 88 months of price and ownership data available.

Table 3.2 shows the GARCH-MIDAS output for the showcase bond.[11] Using the Bollerslev-Wooldridge reported in the "OPG SE" column as the reference standard errors, the parameters $\alpha, \beta,$ and γ are statistically significant, meaning that the model fits the data. The α and β terms sum to close to 1, confirming the existence of a strong volatility persistence effect.

Table 3.2: GARCH-MIDAS Results, Spotlight Bond

Term	Estimate	Rob.SE	P-value	OPG SE	OPG P-value
μ	-0.032	0.028	0.260	0.034	0.350
α	0.201	0.139	0.149	0.048	0.000
β	0.521	0.213	0.014	0.070	0.000
γ	0.170	0.099	0.086	0.086	0.047
m	0.332	0.214	0.120	0.197	0.093
θ	-859.594	519.090	0.098	489.145	0.079
$w2$	319.369	44.618	0.000	301894.818	0.999

My primary theoretical expectation is that securities with higher concentration will lead to higher short-term volatility because flow-induced trades happen over the short term. The statistically significant positive γ parameter means that a higher h has a greater short-term effect on volatility than a decrease does; in other words, h has an asymmetrical effect on this bond's returns.

[11] Using the mfGARCH R package (Kleen, 2020), with $K = 38$..

The results in table 3.2 also permit conclusions about the base long-term volatility m and the effect of h on long-term volatility, given by θ. The estimated value of m, the intercept of the long-term component of the volatility, is negative and statistically significant, suggesting that base long-term volatility is positive. The negative coefficient estimate for θ implies that higher values of h lead to less long-term volatility of returns. Although this seemingly contradicts my hypothesis, the statistical significance on these estimates is lower, giving less confidence in these results. At minimum, it merits further analysis.

The Variance Ratio (Engle, Ghysels and Sohn, 2013) is often used to quantify the relative importance of the long-term and short-term volatility on returns, and is defined by:

$$VR = \frac{var(log(\tau_t))}{var(log(\tau_t g_t))} \tag{3.12}$$

For this specification, we find $VR = 10.71$, implying that 10.71% of expected variation in returns can be attributed to variation in h. These results provide preliminary support for my hypothesis: changes in h affect the volatility of returns of the spotlight bond. However, the diverging signs for the coefficient estimates of the short-term and long-term volatility of returns suggests that volatility over different time frames merits further consideration in the time-series cross-sectional analysis.

3.5.3 Time-Series Analysis

I next attempt to widen the scope of the analysis by considering the entirety of Californian bonds with available data. Because these bonds are all accessible by the same investors, it is possible that their prices, volatilities, and ownership patterns co-vary. To account for such a possibility, I test for cross-sectional dependence.[12] I

[12] Using the PLM package from R (Croissant and Millo, 2008). Because the data set has a much larger n than T, I use both scaled LM and Pesaran's CD tests.

find the securities are indeed cross-sectionally dependent, requiring an appropriate empirical approach.

Further estimation difficulties could arise from the varying reporting cadences of the data points: holdings data is reported monthly and price data daily. I take monthly averages of price data by security, and use time-series cross-sectional models to examine the effect of ownership concentration on a monthly average of n-day running volatility of log returns (denoted v_{it}^n). This approach has two advantages over the GARCH-MIDAS approach: it permits the examination of multiple securities at once and also allows for inclusion of covariates.

I test two specifications. First, I use a within-security approach to measure the effect of ownership concentration on the level of volatility. Second, given the statistically significant relationship between h and volatility in the GARCH-MIDAS results, I use a first-difference approach. The empirical model for the within approach is given by:

$$v_{it}^n = \alpha + \beta h_{it} + \gamma Z_{it} + \mu_i + \epsilon_{it}, \tag{3.13}$$

My hypothesis would find support from a positive value of β, which would imply that higher levels of h are associated with higher volatilities v_{it}^n.

For the approach where I first-difference the time-variant variables, removing the time-invariant components of the regression, the empirical model is given by:

$$\Delta v_{it}^n = \beta \Delta h_{it} + \gamma \Delta Z_{it} + \Delta \mu_i + \Delta \epsilon_{it}, \tag{3.14}$$

where $\Delta v_{it}^n = v_{it}^n - v_{i,t-1}^n$ and Z_{it} is the vector of aforementioned time-varying control variables. Instead of measuring the actual level of volatility estimated in the first time-series approach, the first-difference approach shown in Equation 3.14 measures the monthly *change* in volatility.

As a last step, I examine the effect of volatility on price and yield of a bond. I again use a first-difference approach to the empirical model:

$$\Delta y_{it} = \beta \Delta v_{it}^n + \gamma \Delta Z_{it} + \Delta \mu_i + \Delta \epsilon_{it}, \tag{3.15}$$

where $\Delta v_{it}^n = v_{it}^n - v_{i,t-1}^n$, etc. and Z_{it} is the vector of aforementioned time-varying control variables. Instead of measuring the actual level of the yield, the first-difference approach estimates monthly *change* in yield. I expect positive and statistically significant values of β, which would suggest that there is a positive volatility risk premium.

3.5.4 Time-Series Results

Predicting Volatility

The results based on Equation 3.13 do not show any statistically significant relationship between ownership concentration and volatility. Perhaps this is because, according to my theory, the driver of price variation is *changes* in the ownership concentration, which propel movements in price when holders buy and sell. I therefore focus on the results of the first difference specification, shown in Equation 3.14.

Table 3.3 shows the results of the first-difference model. Here, the results show the relationships between Δv_{it}^n and h_{it} are positive and statistically significant for $n = 10, 30$. Substantively, this means that an increase in the ownership concentration as measured by HHI corresponds to, on average across all bonds, an increase in ten-day and 30-day volatility. Interpreting the size of these coefficients is tricky, though; because HHI is measured on a scale from zero to one, a movement from zero to one is not very meaningful in the real world.[13] Moreover, volatilities also usually fall between zero and one. So, a one-one-hundredth of a unit change in HHI would

[13] Zero would mean an infinitesimally small amount of the asset is held by an infinite number of holders, and one would mean that one person holds everything.

correspond to a 0.12-unit increase in ten-day volatility, which is quite large. Changes in HHI have an effect almost twice as large on the ten-day volatility as the 30-day. Notwithstanding the variation in effect size, these two positive effects support my hypothesis.

Table 3.3: The Effect of Ownership Concentration on Bond Price Volatility, FD Models, Robust SE

	DV: Number of Days Rolling Volatility Δv_{it}^n					
	3d	5d	10d	30d	60d	90d
	(1)	(2)	(3)	(4)	(5)	(6)
HHI	−5.3209	2.3315	11.9268***	6.8989*	5.0483	4.5194
	(5.4463)	(3.3506)	(3.6124)	(3.7384)	(3.4326)	(3.1180)
Pct. OS Known	0.6641*	0.0309	−0.8095***	−0.5639**	−0.4013*	−0.3643*
	(0.3706)	(0.2335)	(0.2412)	(0.2485)	(0.2259)	(0.2047)
Months to Maturity	−73.5956***	19.4323**	135.7924***	94.5575***	81.0514***	75.3684***
	(14.1408)	(8.8040)	(30.6310)	(22.4655)	(20.8633)	(19.8434)
Months to Maturity Sq.	−12.3064***	8.5741***	34.4706***	25.0800***	22.9463***	22.6083***
	(3.0597)	(1.8382)	(6.0283)	(4.4444)	(4.0405)	(3.9237)
Constant	0.0066***	−0.0007	−0.0099***	−0.0071***	−0.0060***	−0.0053***
	(0.0014)	(0.0009)	(0.0028)	(0.0021)	(0.0019)	(0.0018)

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

Interestingly, the percentage we know of a bond's outstanding ownership is negatively correlated with volatilities measured in windows ten days or greater. This suggests that the more a bond is owned by funds we observe (i.e. funds that have stringent reporting requirements such as mutual funds, pension funds, insurance companies, etc.), the lower its volatility. Intuitively, this makes sense: securities owned by institutions likely are being held for long-term purposes and therefore see less churn in ownership, and correspondingly low volatility. On the other hand, securities that have large positions by hedge funds and others not required to report are likely more actively managed and pursued for short-term, activist, or other investment strategies that produce volatile prices.

Besides characteristics of the ownership structure, other variables hold statistically significant relationships with v_{it}^n. For all values of n, volatility follows a quadratic path over the maturity term of the bond. But this relationship looks different for different values of n. For $n > 3$, the initial effect of months to maturity on v_{it}^n is positive: volatility is higher when bonds are further away from maturity. But as the number of months until maturity decreases, volatility drops. This makes intuitive sense because as a bond gets closer to maturity, it is closer to redemption; therefore its price should converge to its face value. But for $n = 3$, the opposite is true. The initial effect of months to maturity on v_{it}^3 is negative: volatility is lower when bonds are further from maturity. As maturity approaches, volatility slowly approaches zero. This likely captures some variation in the way that lower values of n forget past data sooner.

Predicting Yield

Table 3.4 shows that increases in volatility, no matter what time window is considered, correspond strongly and statistically significantly with increases in yield. This supports my hypothesis: higher volatility corresponds to yield premia. A one-tenth

of a unit increase in the annualized standard deviation of log day-over-day returns corresponds to at least a 2.8 basis point premium on yield. This effect is largest for 30-day volatility, where the overall effect is a 10 basis point increase.

Table 3.4: The Effect of Volatility on Yield, Robust SE

	DV: Monthly Average Close Yield					
	(1)	(2)	(3)	(4)	(5)	(6)
3-day Vol.	0.28^{***}					
	(0.02)					
5-day Vol.		0.54^{***}				
		(0.03)				
10-day Vol.			1.00^{***}			
			(0.03)			
30-day Vol.				0.68^{***}		
				(0.02)		
60-day Vol.					0.60^{***}	
					(0.02)	
90-day Vol.						0.58^{***}
						(0.03)
Months to Maturity	-88.35^{*}	-91.40^{*}	-94.16^{*}	-86.71^{*}	-84.30^{*}	-82.71^{*}
	(47.11)	(48.67)	(50.08)	(47.00)	(46.63)	(46.48)
Months to Maturity Sq.	-40.69^{***}	-41.34^{***}	-41.51^{***}	-40.36^{***}	-40.10^{***}	-40.27^{***}
	(9.12)	(9.37)	(9.63)	(9.13)	(9.07)	(9.05)
Constant	-0.01^{***}	-0.01^{**}	-0.01^{**}	-0.01^{***}	-0.01^{***}	-0.01^{***}
	(0.005)	(0.005)	(0.01)	(0.005)	(0.005)	(0.005)

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

3.6 Conclusion

In conclusion, I find evidence that government bonds with higher ownership concentration are likely to have more volatile prices and higher yields. This connection helps shine a light on the pricing effect of the ownership structure of debt. Although some authors have studied the relationship between highly aggregated debt data and fiscal policy, and others have studied the determinants of bond volatility, none have yet linked the two fields of study.

I address this fundamental unanswered question in this paper. Testing my hypotheses on Californian municipal bonds issued since 2002, I find that a bond's volatility is likely to be higher if its ownership is more concentrated in the hands of fewer investors. This effect likely occurs because such a security is more susceptible to large pricing effects from large inflows and outflows. I also find evidence that higher security-level volatility corresponds to a yield premium to persuade creditors to invest.

It is possible, however, that a debt security's underlying "riskiness" drives both its yield and the set of investors willing to purchase it. I find evidence supporting my expectation that this is not the case; a risk match between an investor and a security may drive the investor to take a position in a security, but the size of the investor's position is driven by other things. Therefore, riskiness cannot drive a security's ownership concentration and cannot confound the relationship between ownership concentration and yield. I have taken further steps in empirical design and empirical methodology to reduce concerns about endogeneity, which nonetheless remain important to keep in mind when interpreting these results.

Each of the three empirical methods I use has its drawbacks. The investor selection model relies on time-invariant attributes of investors. Even though the investor attributes I consider do not change very much over time, I would still prefer to have

a (regretfully unavailable) time-series data set. My second empirical analysis, the GARCH-MIDAS approach, does not permit the inclusion of covariates, which can be troublesome when considering the multitude of factors that likely affect security volatility and return. Furthermore, its inability to accommodate a panel approach propelled me to pursue a more conventional time-series cross-sectional analysis. The panel approach itself has its own drawbacks: it regresses a monthly metric on smoothed monthly measures of daily price volatility for many securities, several of which do not have much variation in ownership concentration over time. The biggest drawback of the empirical approach, however, is data availability. Ideally, I would have access to complete holdings data unencumbered by the variation in reporting requirements. But the only way I know of to find such data is through clearinghouses, which charge far more for data access than I could dream to pay. However, taken as a whole, the weight of the evidence supports the hypothesis that securities with higher ownership concentration have more volatile prices and higher yields.

This paper's theory and findings bear on the relationship between the concentration of debt holding and power, which has implications for the democratic peace literature and markets peace literature. For example, is there something called a debt peace? Does China have power over the United States because it holds a large amount of US government bonds? Or does the US instead have control over a portion of the Chinese balance sheet? Does the structure of debt ownership confer power or weakness?

This research has one major implication for governments. Other research has shown that government bond yields on primary markets are influenced by secondary market price dynamics such as the one this paper depicts. This paper suggests that governments whose debt is owned by a more concentrated group of investors could end up paying more in debt service costs in the long run.

Possible future work could include replication of this study with more high-fidelity holdings data. Also, an examination of the different aspects of the ownership structure of government debt would be productive; different kinds of debt holders have different goals, which likely means that they have divergent policy tolerances. A deeper understanding of these concepts could arise from similar investigations.

4

China's Foreign Investment: Hedging Against Policy Uncertainty

4.1 Introduction

防患未然、未雨绸缪

Prepare in advance for rainy days.

In November 2020, the State Grid Corporation of China, a State-Owned Enterprise (SOE) and the second largest corporation in the world by revenue, announced a purchase of 97% of Chile's largest electric utility provider, Compañía General de Electricidad (CGE). Why would they do so? The management literature would suggest that such acquisitions could be done to shore up supply chains, in search of a new market, or to acquire an innovative technology or a particularly profitable company. But none of these explanations apply here: electricity cannot be exported from Chile to China, and CGE runs on legacy technology and has its profits tightly regulated by Chilean electric law.

CGE poses little other use to the Chinese state or economy. But this acquisition is just one of many that Chinese SOEs have made on public and private markets across

the world over the last 20 years that seem to have no other explanation other than that it is part of the Belt and Road Initiative (BRI) and therefore must somehow be in the interests of the Chinese state. But, if all the aforementioned explanations fail, why is this sort of economic action in China's interest?

Most of the research on China's position in the international economy focuses on its involvement in development finance and suggests that China's investments abroad are strategic. The typical story is that investment happens over long time periods where returns are relatively high compared to risks. But China has a broad portfolio of foreign influence activities beyond development finance. Moreover, as the above example shows, even conventional explanations for Chinese overseas investment fall apart under close scrutiny. So, why does China invest in some places at some times and not others?

I offer an explanation counter to the existing narrative: China favors economic investments abroad when political uncertainty is highest as a hedge against changing political fortunes. For example, when the State Grid acquired CGE, Chile was in the midst of re-writing of its constitution after a year of civic upheaval. If it is possible that the target country's government will change policy in a way that adversely affects China, or that a policy change will occur as a result of a change in government, China will take steps to secure its influence over that policy.

China has shown itself willing to put long-term power dynamics ahead of short-term profit (Kaplan, 2018). I suggest that this long-term focus could make non-economic influence activities lose some salience during a period of policy uncertainty, which can be accompanied by transitions in government personnel or resources. For example, diplomats may rotate out of office, requiring new relationships to be formed; new regulation may be implemented that reduces the value of soft power activities; target country resources for certain policy programs can be re-allocated. On the other hand, economic actions, even in unprofitable sectors, are investments in times

of uncertainty that retain their value throughout a government transition. Ownership of a key asset in the target country can serve as a hedge against the possibility that future policy in the target country will not align with China's preferences. As a result, I expect that when future policy is uncertain in recipient countries, China increases its economic influence activity and decreases other categories of influence operations.

To adjudicate my hypothesis, I rely on a novel high-frequency, cross-national, machine-coded event data set called Machine Learning for Peace (MLP), which identifies Resurgent Authoritarian Influence (RAI) events and Civic Space (CS) events. The MLP data set was derived from over 60 million news articles scraped from Chinese, Russian, international, and domestic news websites over 122 months. A state-of-the-art Transformers-based Natural Language Processing (NLP) model then categorized the news stories into 22 RAI categories based on a corpus of 3,400 double-blind human-coded articles and 19 CS categories based on a training corpus of 2,800 stories. MLP represents a step forward in the measurement of influence events, which to date has relied on non-systematic analyses of one-off events and detailed analysis of development financial flows. In addition to economic influence events such as these, RAI includes systematic cross-national data on diplomacy, hard power, soft power, and domestic interference.

I measure policy uncertainty in two ways. First, the competitiveness of elections in target countries produces outcome uncertainty, which in turns results in uncertainty about post-election policy. Uncertainty about post-election outcomes also relates to the time until the election. But elections are not always free and fair, and do not take place in every country. To account for this, I introduce a second measure of policy uncertainty: civil space turmoil derived from the CS data set. Civil space events such as protests, violence, and government coercion occur regardless of a country's governmental institutions. Civil space turmoil breeds instability in

the relationship between the government and the governed, which results in policy uncertainty.

In addition to introducing MLP, this paper has two major contributions. The first is a clear theoretical model of how China assembles its portfolio of influence operations. The second is a contribution to the literature examining China's participation in international economic markets. Because of the availability of data sets like AidData (Custer et al., 2021; Malik et al., 2021), a relatively large literature has analyzed China's participation in development finance (e.g. loans, grants, and infrastructure projects). But development finance is a relatively small piece of the global financial pie. I expand the literature's conception of China's involvement in global economic markets by arguing that state-affiliated actors use public economic markets for strategic political purposes.

The next section offers an overview of the existing literature on relevant topics and discusses questions still to be answered. The third section more clearly articulates a theoretical framework. The fourth section explains the design of the empirical research and the fifth discusses results. The final section concludes.

4.2 Background

4.2.1 Who Are We Talking About?

"China" is not a single actor. The Chinese government is not a unilateral actor, nor is the Chinese economy. The Chinese economy is composed of the vast spectrum of private and public enterprises expected of a nation of billions of people. Many of these enterprises are disconnected from the government. However, its biggest enterprises remain subject to a certain amount of government influence, more than in any other major economy. These large enterprises fall into two categories: State-Owned Enterprises (SOEs) and Privately-Owned Enterprises (POEs). SOEs are just that: owned and controlled by the state. The state is not a monolith; it is composed

of competing and varying players with divergent interests. However, on the whole, SOEs are well-positioned to carry out government policy.

However, in China the difference between a governmental and private entity is blurry (Huang and Tang, 2017; Kirkegaard, 2019). This blurred line obscures how politically motivated a company's activities can be, but it is possible to generally rank companies on their relationship with the state. At one end of the spectrum, the Chinese central government is deeply involved in the investment, management, and supervision of large banks (Chen, 2012: p. 226-7). The Chinese government owns banks tasked with financing Chinese policy overseas, such as the Export-Import Bank of China (ExIm), China Development Bank (CDB), and Agricultural Development Bank of China (ADBC). Likewise, through state-owned holding companies and government agencies, the Chinese government owns at least a majority if not the entirety of all large commercial banks.

Slightly more independent are companies like the Chinese state-owned shipping giant COSCO, Although owned by the state, COSCO is a standalone shipping company that is supposed to make its own money without being propped up by the state. COSCO's recent acquisition a majority stake in the Piraeus (Greece) Port Authority appears to be explicitly connected to Chinese policy goals: "The port has served as a transport hub linking the Maritime Silk Road with European countries" (CGTN, 2019; Xinhua, 2021).

More debatably government-influenced are Foreign Direct Investment (FDI) transactions between Chinese businesses and governmental and non-governmental entities in foreign countries. FDI can take the form of mergers, acquisitions, investments, or other transactions, and sometimes benefits from cheap loans issued by Chinese government banks or contracting processes that favor Chinese businesses. Such FDI has recently been in the news across South America, Europe, Southeast Asia, and the South Pacific.

4.2.2 Firm Motivations

Indeed, a large body of management literature argues that Chinese businesses, both SOEs and POEs, expand internationally for the same reasons multinational corporations (MNCs) do: in pursuit of resources, technology, markets, diversification, and/or strategic assets (Deng, 2004). Like MNCs, SOE motivations for overseas acquisitions pursue either shareholder value or stakeholder utility (Florio, Ferraris and Vandone, 2018).

A subsection of this literature focuses on reasons why Chinese SOEs in particular invest in overseas companies (Baroncelli and Landoni, 2019). Chinese SOE acquisitions of companies in North America, Western Europe, and Oceania from 2009-2017 have established research and development centers in foreign markets, acquiring technology, and high returns. Such a backdrop would suggest that Chinese companies should expand to improve technology, shore up their supply chain, provide a material gain to their bottom line, or provide some other vague strategic benefit. By this logic, Chinese investment overseas should be unrelated to political events.

So far, the literature's conclusions about Chinese economic investment are sector-agnostic, meaning that the nuances of motivations for specific deals can become lost. Moreover, most empirical assessments stop short of the time period where the BRI was in full swing, and largely neglect Chinese SOE investments in emerging market companies. But even recent studies that study the details of Chinese investment in the South American energy sector simply assert that such investment is in the interest of the Chinese state (Ellis, 2021; González Jáuregui, 2021). However, they fail to explain why. Is it because Chinese SOEs gain technology from investing in foreign companies? Is it because the power produced by these investments can fuel other Chinese investments in the target country? Is it because the investment makes enough money to produce a financial return?

Another subset of political economy literature makes a slightly different version of the conventional argument. Its general claim is that regulatory ambiguity, corruption, and inconsistent rule of law are disincentives for foreign investment because investment relies on guarantees of property rights (Mauro, 1995; Wei, 2000). But the theoretical scope of this argument is not sufficient to answer my research question for several reasons. First, I consider state actors or private actors whose behavior is influenced by a state. There is a strong linkage between Chinese government policy and Chinese outbound investment; central policy direction likely outweighs firm-level concerns about the target country governance. Second, China has in recent years taken steps to prevent expropriation of Chinese property in target countries, mitigating the main theoretical mechanism proposed in this literature. Third, I hypothesize a short-term relationship between fluctuations in investment transactions and short-term policy uncertainty, not a persistent long-term relationship between aggregate levels of investment and aspects of a target country's political-economic system.

As a whole, this literature provides useful context to the motivations of economic actors in a fragmented landscape. But it often does not acknowledge the channels of control that the Chinese government has established over companies. The first is law and regulation, such as the 2017 National Security Law; the second is institutional controls over SOEs, like the 2003 establishment of the State-Owned Assets Supervision and Administration Commission; third, state monitoring of and pressure on private companies such as establishment of company CCP cells or pressure to de-list private companies from foreign stock exchanges (Kastner and Pearson, 2021).

4.2.3 Explanations From Foreign Aid Literature

Furthermore, the management literature on Chinese overseas expansion does not make the connection between Chinese enterprises and influence operations. The literature examining China's foreign aid activities gets closer to identifying the sources

and effects of economic influence. Despite the complexity of the relationships with the Chinese state and their motivations for investing abroad, scholarship has argued that Chinese entities use development finance for political purposes in three ways: chequebook diplomacy, debt trap diplomacy, and patient capital.

Chequebook diplomacy is a term mainly used to refer to PRC and Taiwanese competing offers of aid to countries in exchange for diplomatic recognition. This mainly occurred in small countries, especially Pacific Island countries, during the 1990s and 2000s (Atkinson, 2010; Nowak, 2015; Hille, 2019; Salem, 2020). These transactions are often discussed in the context of China's proclivity to issue aid tied to conditions, backed by commodity purchases, or in exchange for trade. But, importantly, the government of the Chinese state itself is not the investor in these transactions – instead, SOEs often are.

Proponents of "debt trap diplomacy" argue that Chinese entities are so willing to loan money to countries that they ignore creditworthiness in doing so. This then results in a build-up of Chinese credit so heavy that it contributes to a debt burden the country is unable to repay, making the country beholden to Chinese interests. Bräutigam explains that this concept contains elements of truth, but these truths have not led to China extracting "unfair or strategic advantages of some kind in Africa, including 'asset seizures'" (Bräutigam, 2020: p. 6). Later scholarship has shown, however, that non-disclosure agreements are commonly included in loans from Chinese entities, especially where disclosure of the debt is not compelled by law (Gelpern et al., 2021: Ch. 3.1). Such non-disclosure can limit the transparency required to assess ability to pay before other entities issue debt to the same debtor. In so doing, Chinese overseas investment accrues political leverage for the Chinese state.

These two patterns point towards a third: the long time horizon and patience associated with China's overseas investment strategy, as opposed to the impatience

usually associated with financial markets (Jacobs, 2011; Thatcher and Vlandas, 2016; Deeg and Hardie, 2016; Lin and Wang, 2017; Kaplan, 2018). It may however be the case that this patience is only a characteristic of non-tradable debt (loans and grants) issued by Chinese entities. In other influence transactions, China may be as impatient as everyone else.

The vehement disagreements in this literature obscure its takeaways, which makes it difficult to assess the veracity of its conclusions. Furthermore, this literature, by design, only considers economic influence and does not relate foreign aid distributions to other Chinese influence activities (Diamond and Schell, 2019). And it does not recognize that the main reasons a recipient country's foreign policy is likely to be similar to China's are its regime characteristics, trade linkages, and shared patterns of political globalization (Flores-Macías and Kreps, 2013; Strüver, 2016; Kastner, 2016).

4.2.4 Analysis of the BRI

Zooming out, a large body of literature has examined the Belt and Road Initiative (BRI) and its effects on Chinese outbound investment since it was announced in 2013. The BRI has been assessed within the context of maritime transport (Lee et al., 2018), environmental challenges (Ascensão et al., 2018), outward foreign direct investment (FDI) (Sutherland et al., 2020), recipient country political economy (Loughlin and Grimsditch, 2021), national security (Ellis, 2013; Shah, 2021; Farah and Richardson, 2021), and much more. Interrogations of specific episodes of economic influence provide detailed descriptions of Chinese investment. The main takeaway is often that BRI investments are in China's national interest, but the literature stops short of asking why. For example, why is a large Chinese presence in the Latin American energy sector in China's national interest (Ellis, 2013, 2021)?

Kastner and Pearson (2021)'s recent study of China's foreign economic influence helps set the stage for the relationship between the BRI and economic influence. Among other contributions, the authors conclude that, although the Chinese government also has other goals like supporting Chinese companies and strengthening the national economy, China intends to use economic means for political influence. The authors posit several causal mechanisms, including using economic ties as a source of coercion and inducements in bargaining power, creating vested interests, transforming public and elite opinion, and structural power (Kastner and Pearson, 2021).

But this causal chain can be hard to discern because official statements on BRI often omit discussion of its strategic policy risks and benefits (Wuthnow, 2017). The Chinese government often characterizes BRI transactions as win-win initiatives, based on non-interference and mutual benefit (Kastner and Pearson, 2021: p. 23). Indeed, the BRI itself has changed over time, perhaps because broad Chinese policy initiatives are not in fact tightly centrally planned but are coalitions of diverse actors given a leash of varying length (Huang and Tang, 2017; Kirkegaard, 2019; Kroeber, 2020).

But Kastner and Pearson's causal mechanisms can help illuminate the Chinese State Grid's recent purchase of Chilean electricity distributor CGE. Why is Chinese investment in a low-profit, tightly-regulated company that relies on legacy technology and produces a non-exportable good a "win" for the Chinese state?

4.2.5 Questions remain

Taken as a whole, this literature has one main weakness in trying to explain why China chooses economic influence operations under different circumstances: it fails to explain why some of these influence actions are in China's national interest. In the following section, I attempt to do so.

4.3 The Theoretical Model

Conventional accounts expect that Chinese overseas investment is driven by returns over a long time period (Kaplan, 2018) driven by target profit, technology access, or new markets (Deng, 2004; Florio, Ferraris and Vandone, 2018; Baroncelli and Landoni, 2019). This narrative has several problems. First, large-scale studies do not carefully define investment, instead implying that Chinese policy banks have a unique funding relationship with target countries, and do not explain the conditions under which investment should occur in the first place. Although the rest of the literature is more specific on both of these points, it is far from comprehensive, forgetting the connection between Chinese firms and the Chinese state. Two key questions remain unanswered. First, how does China strategically engage in influence abroad outside development finance? Second, why are economic influence operations in its national interest?

In this paper, I fill these gaps by proposing that, contrary to the suggestions of existing literature, Chinese investments in target countries are more likely when the future policy of the target country is uncertain. Investments function as a kind of hedge for several reasons: first, the investor is entitled to rights in the target country as a property owner; second, the investor can use the property itself for strategic purposes; and third, the investment transaction provides an opportunity to transfer wealth to an entity in the target country, itself a possible vector for influence. I develop the literature in two ways. First, I contribute a clear theoretical model of how investments serve as a hedge against future policy uncertainty. Second, I explain one way that China strategically participates in global economic markets apart from development finance.

In this transaction, there are two actors: the sender (China) and the target country. Although it might seem possible that this argument could be applicable to

other senders, the tight connection between China's government and its economic actors make it a special case. In countries with more tenuous connections between the government and the economy, such as the United States, it is less likely that specific firms would invest abroad in accordance with the national interests of their home government.

The target countries in this model have developing economies and developing political systems. The policy of any country fluctuates over time, but here I focus on the uncertainty that accompanies the fluctuation in target government policy surrounding different kinds of political events. Instead of focusing on the policy itself, I am interested in how uncertainty in future policy affects China's influence activities.

Accordingly, policy uncertainty derives from situations in which future policy is not foreseeable. I consider two major drivers of policy uncertainty. The first is elections: in countries with elections timelines known in advance, observers are unsure of what policy will occur after the election. This policy uncertainty derives from uncertainty over the outcome of the election. However, elections can only result in uncertainty when they are free, fair, and competitive. When these conditions are not satisfied, civil instability is another driver of policy uncertainty. For example, in countries with elections where opposition parties are excluded, the government in power will likely stay in power through an election cycle. In such a case, the policy uncertainty that I expect will affect investment actions derives from events the government controls, which are part and parcel of the government's policy and therefore could serve as bellwethers for future policy.

This model rests on one assumption: a "unified actor" model of decision-making on the part of China. While there are a range of players involved in China's foreign investment, control in recent years has been shepherded by the state. Whether the state encouraged outward investment or put guard rails on it, Chinese central

government policy has, in broad strokes, driven the movements of the actors. I assume that the preferences of major Chinese investors all align around stability and predictable policy.

China undertakes influence actions every day that range from military exercises to diplomatic engagement and have magnitude ranging from a small transfer of consumer electronics to the purchase of a major company. China is continually making decisions about which kind of influence to engage in, if any. Given the periodic nature of policy uncertainty in target countries, some of these decisions take place when future target country government policy is known and others take place in more uncertain environments. I consider economic influence events. To be more precise, I disaggregate economic influence into three subcategories: investment, trade agreements, and economic aid. Each accomplishes different goals and is executable over a different time frame.

Investment, narrowly defined, is the use of sender country capital to purchase an asset in the target country. Investment can be used in pursuit of returns or for the use of the asset, but the distinguishing feature of investment is that the investor literally owns the asset in the target country. The time frame over which an investment transaction is feasible varies depending on its type: a purchase of equity over public markets may take place over one or several days, a purchase of real estate could take weeks, and a purchase of a large company could take months to negotiate.

Trade agreements are an agreement between the sender and target country to have a freer trade relationship. Trade agreements are likely to increase commerce between the two countries and as a result could be construed as a broadly-defined "investment" in a longer-term economic relationship. Trade agreements are negotiated by the governments themselves, not by other actors in the countries, and are long and complex documents that take months or years to write and approve.

Economic aid is a grant or concessional loan issued by the sender to an entity in

the target country. Often, the issuer is a sender country government agency[1] and the recipient is a target country government entity. Time frames over which aid can be issued vary broadly, with China at the quick end of the spectrum (Zeitz, 2019). Aid also varies in how political its motives are (Dreher et al., 2019).

Each of these sub-types of economic influence are suitable for different actors with different aims over different time periods. But, as a whole, the category of economic influence has several advantages over non-economic influence. The main advantage is that economic influence makes some entity richer. Investment in particular results in a sender country entity owning an asset in the target country and a target country entity being compensated for that asset.

To get a sense of what investment entails, consider a few examples. In April 2014, the Colombian judiciary blocked the potential sale of the government's majority stake in power generator Isagen to investors including China's Huadian Corporation. Although this attempt at influence did not pan out – the sale eventually went to a Canadian asset management company – it would have resulted in Chinese ownership of part of the Colombian power grid. On the other side of the world, Reuters reported another investment act in August 2018: the Chinese-financed Kyauk Pyu deepwater port in Myanmar was being amended to assuage concerns about unsustainable debt practices. China's likely long-term goals for this project are to develop its southwestern region and avoid the Straits of Malacca (Asia Maritime Transparency Institute, 2018).[2]

To better understand how investment might result in influence, I next consider the two sides of that transaction in detail. A sender country entity who now owns an asset in the target country can use that asset in several ways. The first way is overt: the owner of an asset in the target country is likely entitled to certain rights that

[1] Or multilateral body.

[2] These are both investment events pulled from the MLP data set.

(usually) do not change based on who is in office. The second way is more coercive. The State Grid of China might exert a very extreme kind of policy influence if it shut off electricity distribution to CGE's customers (around 70% of Chile's population) to help achieve one of China's political goals. There would be significant costs for doing so, but the capability theoretically exists. The third way to use that asset is to sell it: all the time the sender country entity owns the target country asset, the asset retains value.

The specter of expropriation or nationalization can hang over foreign investments, possibly diminishing the value of sender investment in the target country. Of course, the likelihood of expropriation varies tremendously from country to country, but it is no idle threat: Mexico created its national oil giant Petróleos Mexicanos (PEMEX) in the 1930s by expropriating and nationalizing most of the foreign oil presence in Mexico. However, in recent years, China has taken considerable steps to mitigate this threat. Recent scholarship has shown that one of the ways Chinese lenders use loans for political purposes is to structure the terms of loans so that they receive other things of value to compensate for unrealized financial returns. This alternative compensation can take many forms, but one relevant way is to trigger default or acceleration under nationalization or dissolution of a "PRC entity" in the debtor country (Gelpern et al., 2021).[3] In other words, Chinese lenders use loans as a tool to reduce or eliminate the likelihood that Chinese investments are expropriated, thereby ensuring that they retain market value.

Choosing investment for influence is costly in both money and time. Buying assets is expensive and requires a careful matching of prospective buyers with sellers. Granted, other kinds of influence can be expensive, too: arms transfers, surveillance, and intelligence collection are all very expensive. But such actions are often funded directly by government budgets. Diplomacy, intelligence, and military functions are

[3] China has created novel and effective ways of enforcing these contracts; they are not toothless.

core to what a government does and their development is key to the functioning of the state. A large part of foreign investment, on the other hand, relies on the resources of corporations or individuals. Even though the distinction between the government and enterprise in China is blurrier than most places, the government budget serves as a back stop at best for such transactions.

But in return for the expense of investment, the sender country gets a lasting presence in the target country that persists through episodes of policy turmoil that can scuttle other diplomatic efforts, intelligence collection, or military cooperation. Alongside that presence, the investor gets rights, the ability to use the asset it owns for strategic purposes, and the ability to sell the asset if it wants to divest. This is a much more compelling long-term story than other kinds of influence operations. A change to government policy can reduce the benefits of diplomatic influence over particular politicians, quickly turn around social and cultural programs or media influence operations, or round up intelligence collection.

I argue that China is well aware of the trade-offs between certain types of influence. It is more likely to use economic influence when when policy uncertainty is high because economic investments generally hold their strategic value through policy change. China is so aware of this, in fact, that it has taken considerable steps to prevent expropriation of its investments abroad. Although non-economic influence may be cheaper and easier than investments in the short term, their payoff is more susceptible to changes in government policy.

But Chinese investors cannot react instantaneously to policy uncertainty in the target country. Even if they could, it is not apparent that they should think one protest event should indicate sustained civil space pressure on the government, or is the first sign of a long uncertain period. As much as investors would like to be able to forecast policy uncertainty in the target country, I expect that they base their decisions on patterns that only emerge after some period of time. And even after

coming to a decision, investors require still further time to act on those decisions. Consequently, I expect that policy uncertainty will have persistent ripple effects on investment behavior for weeks or months.

Moreover, the timing of this ripple effect should be different for each kind of influence act. For example, trade agreements are negotiated between governments over long periods of time; I expect that any change in trade agreements would occur behind the scenes or would take several months or years to occur. Investments, on the other hand, can be changed more quickly.

While the above logic may help explain why China chooses certain kinds of influence actions, it neglects one key actor: the target country itself. The target country government can signal openness to certain kinds of influence and preclude others. For example, while Chile allows foreign entities to own public utilities, the United States has the Committee on Foreign Investment in the United States (CFIUS) that has the power to review foreign investments in US companies or operations for national security purposes, and has blocked such transactions from occurring (McLaughlin, 2016). The target government implements such a process after weighing both the costs and benefits of foreign influence. For example, Argentina has chosen to solicit Chinese investment in its energy sector (González Jáuregui, 2021). But, after all, target government solicitation of foreign investment is itself a policy that is subject to uncertainty, and therefore one against which China could hedge.

One possible counter-argument to this theory is that Chinese investors merely enter the market at the same time any seasoned investor would: during turmoil, when valuations are volatile and there is a chance an investment's value will increase. This is merely another way of phrasing my argument. If an investment's value increases after turmoil, it could do so in two ways: in financial value or strategic value. Either way, the investor benefits. Although this strategy may seem intuitive to those familiar with financial markets, it is the opposite of the common story about

China's international investment strategy, where investment is allocated abroad with a long time horizon to places with a high likelihood of return in a manner that avoids default risk (Kaplan, 2018: p. 12). Moreover, I argue that investment events are different in kind from other economic events such as trade agreements and aid, which are not likely to exhibit this pattern.

This argument leads me to expect that Chinese economic influence activity, especially investments, will be higher during periods of policy uncertainty in the target country. Specifically, when policy is uncertain because of civil space turmoil, I expect Chinese investments to increase. I also expect investments to be higher when policy is uncertain because of closer or more competitive elections in the target country.

4.4 Research Design

4.4.1 Data Description

To assess this hypothesis, I rely on several data sets. The first and most novel is the MLP data set, built by the DevLab@Duke team at Duke University, which collects events from news stories across the world. MLP is composed of the Resurgent Authoritarian Influence (RAI) and Civil Space (CS) event data sets, which are high-frequency, machine-generated, and cross-national. The RAI data set encompasses Russia and China's efforts to influence the politics of target countries across the world, and the CS data set focuses on movements in the relationship between government and civil society.

RAI and CS are based on over 60 million news articles scraped from Chinese, Russian, international, and domestic online news sources over 122 months. Not every article that appears in a newspaper contains a relevant event, though: the project team trained a supervised Natural Language Processing (NLP) algorithm to identify certain event types. For the RAI data set, the team constructed a training corpus of "double-blind, human-coded newspaper articles hand built for our purposes" that

consists of 3,400 articles across the 22 event types, which can categorized into five broad themes: soft power, hard power, economic power, diplomacy, and domestic interference (Springman, Wibbels and Vu, 2022). The team did the same for the CS data set, using a similar process to build a training set of 2,800 articles across 19 event types that can be broadly categorized into restrictions on civil freedoms (RCF), protests, and government coercion and force (CAF).

The NLP algorithm in question, state of the art in computer science but so far under-utilized in social science research, depends on a technology called Transformers that have better context comprehension and prediction capabilities than other methods. One of the most ubiquitous Transformer models is the Bidirectional Encoder Representations from Transformers (BERT) model; the DevLab team uses a refinement of BERT called RoBERTa. By training the model on the training sets, the DevLab team achieves 80% out-of-sample classification accuracy across RAI event types and 90% accuracy for the CS event types.

In addition to the RAI data set, I also incorporate data from other sources. I use data on the timing of elections from the World Bank's Database of Political Institutions (DPI) and data on vote margins of elections from VDem to measure policy uncertainty around elections (Cruz, Scartascini and Keefer, 2020; Coppedge et al., 2022; Pemstein et al., 2022). I source bilateral trade data from the IMF's Direction on Trade Statistics database (The International Monetary Fund, 2022) and data on total trade as a percentage of GDP from the World Bank (The World Bank, 2022*a*). Moreover, to allow for the conventional explanation that investment is more likely when rule of law is better, I check the robustness of my results against this explanation by including the World Bank's World Governance Indicators for Rule of Law, Government Effectiveness, and Regulatory Quality in my empirical models (The World Bank, 2022*b*).

4.4.2 Empirical Setting

The event data sets cover 33 target countries selected in consultation with "existing research, partners in civil society and the media, and representatives from the United States Agency for International Development" (USAID) (Springman, Wibbels and Vu, 2022).[4] The countries in the selection share certain attributes. For example, even though the countries represent a variety of political systems, the list generally does not contain large, well-functioning democracies or countries whose government institutions are strong enough to withstand pressure from abroad.[5] They are also all countries with USAID missions, and there is a large focus on Africa and Eastern Europe because of regional buy-in to the project. Nonetheless, there is considerable variation: the countries are physically located across the world and have a broad range of natural resource endowments, economic sizes and structures, and geographic features.

The event data sets consist of event counts at the country-day-event type level of granularity. The CS data set describes the state of civil society in the target country; I restrict my analysis to the portion of the RAI data set that describes actions China takes in the target country. To better observe patterns over time, I aggregate to the month level.

Because the online news ecosystem has improved over the period of time these data sets cover, one might expect that event counts would rise over time in proportion to increased information availability. To account for this, I normalize all the event counts as a percentage of the total articles published about the target country in

[4] Albania, Belarus, Benin, Cambodia, Colombia, Congo, Ecuador, El Salvador, Ethiopia, Georgia, Ghana, Guatemala, Honduras, Jamaica, Kenya, Kosovo, Mali, Mauritania, Morocco, Niger, Nigeria, Paraguay, Philippines, Rwanda, Serbia, Tanzania, Tunisia, Turkey, Uganda, Ukraine, Zambia, and Zimbabwe.

[5] Nigeria is perhaps the largest democracy on the list, but has in recent years has had "repressed" political participation (Marshall and Gurr, 2018).

a given month. This transforms the event count indicator into a measure of the salience of the event type in the local news cycle for the time period in question. This temporal correlation is another reason to use time-series analytical techniques.

4.4.3 Variable Definitions

The RAI data set has 22 event types. Six of them could be construed as economic events: aid operations, corruption, transnational organized crime, investment actions, trade or financial sanctions, and trade agreements or exchanges. I exclude organized crime and corruption because even though they rely on economic incentives, both affect policy through the actions of particular policymakers, a different mechanism than I am testing. I further exclude sanctions because they are coercive; the mechanism through which they affect policy again differs from my theory.

This leaves three kinds of economic events: aid, investment, and trade agreements. All are ways of investing in the long-term health of the economy of a target country. But aid and trade agreements are government-to-government transactions that do not result in foreign ownership of assets in the target country. My main dependent variable derives from the narrow definition of investment events, but I also test a broader definition of economic investment that encompasses trade agreements and economic aid in addition to investment. More specifically, the dependent variable will be the count of investment events that China undertakes in a target country every month divided by the sum total of all articles about a country in a given month. This normalization smooths over fluctuations in overall article output and allows for a consistent interpretation even if one news source goes offline for a period of time.

Figure 4.1 gives an overview of how raw Chinese influence event counts are distributed across the RAI event categories in the Philippines, Cambodia, and Nigeria. The right-hand panels show the distribution of investment events, in green, com-

FIGURE 4.1: Raw event counts over time. The left-hand panels show all event categories; right-hand panels show investment and non-investment influence.

pared to non-investment events. I showcase these three countries for three reasons. First, and most broadly, the scale of the number of events is different across the three

countries. Cambodia, for example, is the target of relatively few influence acts when compared to Nigeria and especially the Philippines. Second, investment is a relatively common influence tactic – there are many investment events in a given month, especially when considered in proportion to the sum of all non-investment events. Moreover, the distribution of investment compared to non-investment varies across countries, implying that Chinese influence strategies vary across country and time. Third, investment counts in all three countries increase relative to non-investment events from 2016, when outbound BRI-related investment reached its peak, through the 2020 outbreak of COVID-19. This time period corresponds to high levels of Chinese outbound investment. Chinese restrictions on outbound investment, enacted in 2017 and 2018 after Beijing restricted "irrational" outflows, are reflected in dips around 2018 in Cambodia and Nigeria (although the Philippines seems not to have experienced a dip in investment around this time).

Aside from investment, the crucial measure is how to conceptualize policy uncertainty. One possibility is using elections: in countries with regular election cycles, usually presidential systems, there will be outcome uncertainty around which government will take office after the elections, which results in policy uncertainty. Countries with parliamentary systems also hold elections, but because parliamentary elections are endogenously timed, there is less ex ante public awareness of when an election will be held and therefore less opportunity to change course of action based on the timing, circumstances, and probabilistic outcome distribution of the election. In countries with no free and fair elections, such as Cambodia, it is likely that the regime in power will stay in power, and drivers of policy uncertainty will have to be found elsewhere.

I use election results as a measurement of policy uncertainty for one stage of my analysis. However, although its use is common in the political science literature, such a measure is not ideal for this analysis for several reasons. First, the 33 target

countries do not all have regular, exogenously-timed elections; using such a measure drastically reduces sample size. Second, the high-frequency nature of the event data set means that, even for the countries with regular elections, there need not have been an election in a country for several years. Using such a measure for the independent variable would throw away the advantages of such a granular measure of the dependent variable.

So, I also consider an alternative measure of policy uncertainty. In all countries, civil space unrest is a major disruptor to policy status quo. I test the relationship between economic investment and three different types of civic strife. The CS data set provides event counts for protests (P), restrictions on civic freedoms (RCF), and coercion and force (CAF). All are ways of measuring civil strife in a target country, but from different points of view. While protests measure population discontent with government policy, RCF and CAF focus on the supply side: tactics governments use to quell discontent. RCF focus on nonviolent and legal restrictions on legally-granted freedoms; by using CAF, the government takes a harder line. These counts serve as my dependent variables. Figure 4.2 shows the distribution of the normalized counts of civil space events.

Countries with different economic markets should see different investment patterns, as should countries with different economic relationships with China. For example, countries with more open economies should see more investment overall, suggesting that Chinese investment could be less effective as influence operations because of a crowding out effect. To account for this, I include the target country's Trade to GDP ratio as a measure of overall economic openness. To account for a country's economic dependence on China, I include measures of export and import dependence. China could be more likely to invest in countries who send more of their exports to China because China has a material stake in the continuity of that trade process and as a result places a higher premium on policy influence. On the

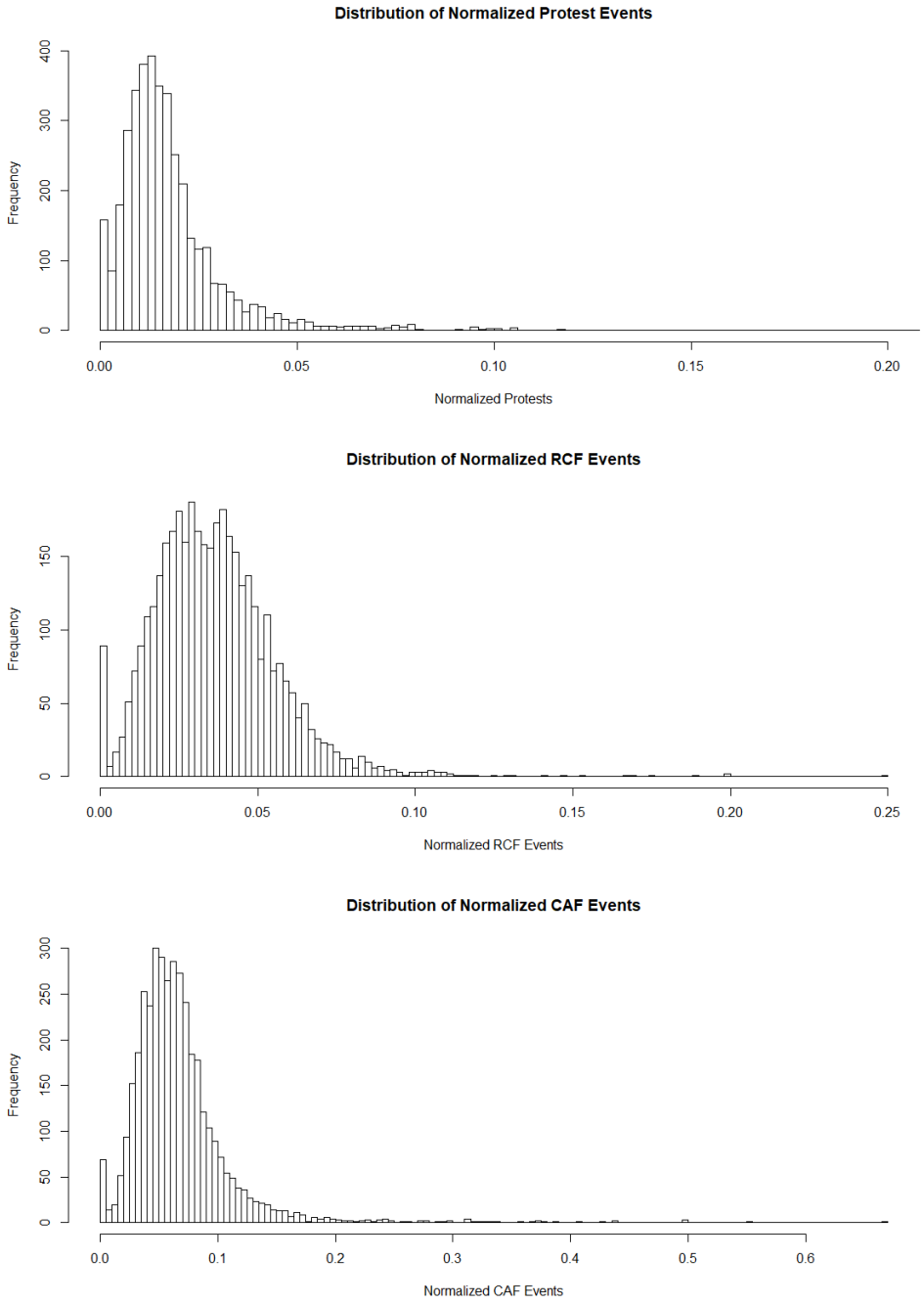


FIGURE 4.2: Histograms of the occurrence of normalized civil space events.

other hand, China could be less likely to invest countries that get more of their imports from China because import dependence is already a measurement of economic leverage. It is possible that China could view additional investment as duplicative.

Moreover, investment in a target country is also likely related to the overall level of Chinese influence operations at a given time, of all kinds. To account for this, I include a measurement of non-investment influence events as an explanatory variable. Because it's likely that economic influence depends on prior investment, I consider a time-series analysis. For example, economic influence at time t could be higher if investment at $t-1$ was also high, which would indicate a trend of continued investment. On the other hand, if investment was high at $t-1$, there could be fewer available assets to purchase at time t. A Gourieroux, Holly and Monfort (1982) style LaGrange multiplier test of individual and time effects confirms that there are indeed significant country and time effects.

4.5 Results

4.5.1 Elections as Measures of Uncertainty

Because this specification predicts investment based on the salience of the next election, I include a scalar measure of the months until the next election (m). For robustness, I also construct binary measures of whether the next election is less than 12 months or six months away.[6] Moreover, I expect that the outcome uncertainty of a given election will also affect investment, so I include a scalar measure of the vote margin between the winner and the runner-up (u) as an explanatory variable. I use the following linear model to predict investment based on the uncertainty surrounding a given election:

$$\gamma_{i,t} = \alpha + \beta_0 \gamma_{i,t-1} + \beta_1 \nu_{i,t} + \beta_2 m_{it} + \beta_3 u_{it} + \phi_i + \tau_t + \epsilon_{it}. \tag{4.1}$$

[6] Binary DV results in Appendix.

My hypotheses would find support from a negative value of β_2, indicating increasing investment as the election gets closer, and a negative value of β_3, which would indicate more investment before more competitive elections.

Table 4.1: The Effect of Elections on Investment

	DV: Investment Events			
	Narrow		Broad	
	(1)	(2)	(3)	(4)
Lagged DV, Narrow	0.166***	0.073**		
	(0.032)	(0.036)		
Non-Investment, Narrow	0.394***	0.367***		
	(0.031)	(0.031)		
Lagged DV, Broad			0.159***	0.065*
			(0.032)	(0.036)
Non-Investment, Broad			0.410***	0.381***
			(0.032)	(0.032)
Months To Next Election	−0.00004**	−0.00003*	−0.00004**	−0.00004**
	(0.00002)	(0.00002)	(0.00002)	(0.00002)
Executive Vote Margin	−0.0003***	−0.0002***	−0.0003***	−0.0002***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Protest Events		0.002		−0.003
		(0.017)		(0.018)
RCF Events		−0.039***		−0.039***
		(0.013)		(0.013)
CAF Events		0.034***		0.036***
		(0.007)		(0.007)
Exp. Dep.	−0.026***	−0.022***	−0.027***	−0.023***
	(0.006)	(0.006)	(0.006)	(0.006)
Imp. Dep.	0.001	0.001	0.001	0.001
	(0.003)	(0.003)	(0.003)	(0.003)
Trade/GDP	0.0001**	0.0001**	0.0001*	0.0001**
	(0.00004)	(0.00004)	(0.00004)	(0.00004)
Observations	1,177	1,177	1,177	1,177
R^2	0.349	0.374	0.343	0.368
Adjusted R^2	0.279	0.304	0.272	0.298
F Statistic	81.425***	63.183***	79.208***	61.554***
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 4.1 shows the results of this specification. The coefficient for election competitiveness (Executive Vote Margin) is negative and statistically significant in all

specifications, suggesting that investment events are more numerous before more competitive elections. Investment events also have a statistically significant and negative relationship with the amount of time until the next election, suggesting that investment increases when elections are close. These terms provide initial support for the hypothesis. These results are robust to including binary definitions of the time until elections variable.[7]

The size of the coefficients helps illuminate the precise relationship between election-derived uncertainty and investment events. The portion of the media ecosystem devoted to investment events is likely to increase by three or four one-thousandths of one percent when an election becomes one month closer. The size of the coefficient for Executive Vote Margin is one order of magnitude larger: investment coverage increases by two or three one-hundredths of a percent for every percent closer an election is. Although these may seem like small numbers, it is worth keeping in mind that investment events made up less than 1% of the media ecosystem for 92.8% of the data set's 3,893 country-month combinations. So even a small change because of an election can make a big difference: the investment landscape could change appreciably between the time period immediately following an election and the time period immediately preceding the next one.

Table 4.1 also shows that investment is correlated with past investment and other non-investment influence events: investment is likely to continue where it already exists, and it usually takes place alongside other influence events. Moreover, investment is more likely in countries that are less export-dependent on China and countries with more open economies.

But there is a caveat: the sample size of this analysis is relatively small because elections are intermittent and do not occur in every country. To get a better sense of the hypothesized relationship, I move on to study civil space turmoil as a measure

[7] See Tables C.1 and C.2 in the Appendix.

of policy uncertainty.

4.5.2 Civil Space Turmoil Measuring Uncertainty

I next focus on predicting narrowly-defined investment using civic space turmoil as a measure of policy uncertainty. But changes in investment events likely need some time to occur in reaction to policy uncertainty. Therefore, I expect civil space conditions at time t to correspond to influence events at time $t - n$, where n varies based on the kind of influence. Using a time-series estimator, I regress the dependent variable, investment γ, on civil strife $cs \in$ [P, RCF, CAF] with within-country effects ϕ_i and time effects τ_t. Because influence activities are a portfolio, I also include as explanatory variables a lagged dependent variable, non-economic influence events in the country i in month t as $\nu_{i,t}$, and a vector of controls ζ that includes a country's trade to GDP ratio as a proxy for its economic openness and its export and import dependence on China.

$$\gamma_{i,t} = \alpha + \beta_0\gamma_{i,t-1} + \beta_1\nu_{i,t} + \beta_2 cs_{i,t} + \beta_3 cs_{i,t-n} + \beta_3\zeta_{i,t} + \phi_i + \tau_t + \epsilon_{it} \tag{4.2}$$

My hypothesis would find support from positive values of β_2 and/or β_3, which would signify that economic investment increases during or after policy uncertainty.

Table 4.2 shows the regression results predicting investment. Present investment is strongly and positively correlated with concurrent non-investment influence events. In other words, investment events occur at the same time as other influence events. But the precise relationship between investment and policy uncertainty varies by the way policy uncertainty is measured. The regression coefficients show that CAF has a positive and statistically significant relationships with investment, even in specifications three and four, which include lagged values for civic space turmoil. RCF and protest appear to have no notable relationship with investment. Substantively, this

Table 4.2: Baseline Results: The Effect of Civil Space Turmoil on Investment.

	DV: Investment Events			
	No Lags	1 Lag	2 Lags	3 Lags
	(1)	(2)	(3)	(4)
Lagged DV	-0.08^{***}	-0.07^{***}	-0.07^{***}	-0.08^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Non-Investment Events	0.44^{***}	0.45^{***}	0.45^{***}	0.45^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Protest Events	-0.05^{*}	-0.05^{*}	-0.04	-0.03
	(0.03)	(0.03)	(0.03)	(0.03)
RCF Events	-0.002	-0.01	-0.02	0.004
	(0.03)	(0.03)	(0.03)	(0.03)
CAF Events	0.15^{***}	0.17^{***}	0.18^{***}	0.18^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Protest, $t-1$		-0.02	-0.02	0.02
		(0.03)	(0.03)	(0.03)
RCF, $t-1$		0.03	0.02	0.01
		(0.03)	(0.03)	(0.03)
CAF, $t-1$		-0.04^{***}	-0.02	-0.01
		(0.01)	(0.01)	(0.01)
Protest, $t-2$			-0.05^{*}	-0.02
			(0.03)	(0.03)
RCF, $t-2$			0.06^{*}	0.07^{**}
			(0.03)	(0.03)
CAF, $t-2$			-0.04^{***}	-0.04^{***}
			(0.01)	(0.01)
Protest, $t-3$				-0.18^{***}
				(0.03)
RCF, $t-3$				-0.04
				(0.03)
CAF, $t-3$				0.02
				(0.01)
Exp. Dep.	0.03^{***}	0.03^{***}	0.03^{***}	0.03^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Imp. Dep.	-0.02^{***}	-0.02^{***}	-0.02^{**}	-0.02^{**}
	(0.01)	(0.01)	(0.01)	(0.01)
Trade to GDP	0.0000	0.0000	0.0000	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Observations	3,293	3,293	3,262	3,231
R^2	0.42	0.43	0.43	0.44
Adjusted R^2	0.40	0.40	0.40	0.41
F Statistic	290.22^{***}	212.44^{***}	167.28^{***}	141.25^{***}

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

suggests that Chinese economic influence actions are more likely in the wake of government use of force but are unrelated to government restrictions on civic freedoms or to public demonstrations.

Specification two shows that the first month's lagged value of CAF events has a positive and statistically significant relationship with investment events. Substantively, this suggests that more investment occurs while or immediately after target governments clamp down on civil spaces via coercion and force. More specifically, specification two shows that a one-percentage-point increase in the target country's media coverage of CAF events corresponds to a 0.17% increase in coverage of investment transactions. The magnitude and sign for same-month effects remain consistent even when incorporating further lags. This is strong evidence supporting my hypothesis.

The first and second month lags of CAF also have statistically significant relationships with investment, but the sign is negative. Notwithstanding turmoil at $t = t$, turmoil from two months beforehand still corresponds to a decrease in investment. Even if there is no turmoil in month t, Chinese entities are less likely to invest if there was turmoil one or three months prior. Perhaps this is a return to normal investment patterns after an increase during turmoil.

4.5.3 Robustness and Placebo Tests

Robustness

To further isolate the relationship between investment and civil space turmoil, I run the same baseline civil space turmoil analysis using a broader definition of investment events. Results are virtually identical, providing further support that the nature of investment itself is driving the results.[8]

To allow for the conventional explanation that investment is more likely when

[8] See Table C.3 in the Appendix.

rule of law is better, I check the robustness of my results against this explanation by including the World Bank's World Governance Indicators for Rule of Law, Government Effectiveness, and Regulatory Quality in my empirical models. I find that investment has statistically significant relationships with rule of law and regulatory quality. But the interpretation is challenging. Table 4.3 shows the results. The negative sign of the rule of law coefficient implies that investment is higher when rule of law is worse, which is the opposite of the traditional literature's predictions. But the coefficient for the regulatory quality is positive, suggesting that investment is higher when regulation is better.

While the inclusion of these measures increases the standard errors of the time until election coefficient, it does not reduce the effect of the competitiveness of the election on investment. Moreover, if the story in the traditional literature were to hold true, the signs of the rule of law and regulatory quality coefficients should align. But they do not. These results suggest that Chinese investment is higher when the rule of law is worse, but when regulatory quality is better. The coefficients conflict; it is hard to argue that these results provide support for the existing narrative. Moreover, the inconclusiveness intensifies when these variables have no explanatory power over the relationship between investment and civil space turmoil.[9]

Placebo Analyses

In order for there to be a truly meaningful relationship between policy uncertainty and investment, investment should have a different relationship with civil space turmoil than other kinds of influence. Accordingly, I conduct several placebo analyses. First, I conduct the same baseline analysis using civil space turmoil as a measure of policy uncertainty, but I predict non-investment influence events. I expect the coefficients to have the opposite signs from the baseline analysis above: the coefficients

[9] Because there is no relationship, I have omitted this table from the write-up.

Table 4.3: The Effect of Elections and Governance on Investment

	DV: Investment Events			
	Narrow		Broad	
	(1)	(2)	(3)	(4)
Lagged DV, Narrow	0.142***	0.063*		
	(0.032)	(0.036)		
Non-Investment, Narrow	0.384***	0.359***		
	(0.031)	(0.031)		
Lagged DV, Broad			0.137***	0.057
			(0.032)	(0.036)
Non-Investment, Broad			0.399***	0.373***
			(0.032)	(0.032)
Months To Next Election	−0.00001	−0.00001	−0.00001	−0.00001
	(0.00002)	(0.00002)	(0.00002)	(0.00002)
Executive Vote Margin	−0.0003***	−0.0002**	−0.0003***	−0.0002**
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Protest Events		−0.004		−0.008
		(0.017)		(0.017)
RCF Events		−0.036***		−0.035***
		(0.013)		(0.013)
CAF Events		0.031***		0.032***
		(0.007)		(0.007)
Exp. Dep.	−0.021***	−0.019***	−0.022***	−0.020***
	(0.006)	(0.006)	(0.006)	(0.006)
Imp. Dep.	0.00004	0.0004	0.0001	0.0005
	(0.003)	(0.003)	(0.003)	(0.003)
Trade/GDP	0.0001*	0.0001**	0.0001*	0.0001**
	(0.00004)	(0.00004)	(0.00004)	(0.00004)
Gov. Effectiveness	0.00002	0.00002	0.00003	0.00003
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Rule of Law	−0.0004***	−0.0003***	−0.0004***	−0.0003***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Reg. Quality	0.0003***	0.0003***	0.0003***	0.0003***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Observations	1,177	1,177	1,177	1,177
R^2	0.364	0.383	0.356	0.376
Adjusted R^2	0.293	0.313	0.285	0.305
F Statistic	60.552***	50.464***	58.667***	49.000***

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

should have negative signs, implying that non-investment influence events decrease with turmoil.

Table 4.4 shows the results using civil space turmoil to predict non-investment influence events. These results share some similarities with those predicting investment. For example, non-investment influence events and investment events are strongly positively correlated. However, the relationship between civic space turmoil and non-investment events is completely different.

First, there is a statistically significant positive relationship between protest and non-investment influence. Heightened public demonstrations appear to correspond with higher non-investment influence. This makes intuitive sense, as public protests are opportunities to sow and foment unrest among the target country's population and present an opportunity to influence target country policy accordingly. This relationship is strongest at the same time as the unrest; it appears to swing back to a negative relationship one month after the protests and then increase again two and three months after. This could signify a reversion to the mean over the course of four months. This is a marked change from the relationship between protest and investment, which is statistically indistinguishable from zero.

Second, restrictions on civic freedoms are negatively correlated with concurrent non-investment influence. This relationship again deviates from the relationship between RCF and investment, and could represent hesitancy on the part of the Chinese government to interfere in the domestic policy of the target country. This could perhaps be evidence of China's longstanding "non-interference" policy, where it vows to stay out of the domestic politics of other countries. Although the MLP data set and this paper provide much evidence to the contrary, Chinese entities may see target governments exercising legally-based restrictions as categorically different from civil space actions based in coercion. The RCF coefficient also displays changes in sign throughout the lag periods, again possibly showing reversion to the mean.

Table 4.4: Predicting Non-Investment

	DV: Non-Investment Events			
	No Lags	1 Lag	2 Lags	3 Lags
	(1)	(2)	(3)	(4)
Lagged DV	-0.11^{***}	-0.10^{***}	-0.12^{***}	-0.12^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Investment Events	0.93^{***}	0.88^{***}	0.88^{***}	0.88^{***}
	(0.02)	(0.02)	(0.02)	(0.02)
Protest Events	0.10^{***}	0.14^{***}	0.13^{***}	0.10^{**}
	(0.04)	(0.04)	(0.04)	(0.04)
RCF Events	-0.16^{***}	-0.23^{***}	-0.23^{***}	-0.23^{***}
	(0.04)	(0.04)	(0.04)	(0.04)
CAF Events	-0.15^{***}	-0.24^{***}	-0.26^{***}	-0.27^{***}
	(0.02)	(0.02)	(0.02)	(0.02)
Protest, $t-1$		-0.03	-0.03	-0.10^{**}
		(0.04)	(0.04)	(0.04)
RCF, $t-1$		0.23^{***}	0.28^{***}	0.28^{***}
		(0.04)	(0.04)	(0.04)
CAF, $t-1$		0.25^{***}	0.20^{***}	0.19^{***}
		(0.02)	(0.02)	(0.02)
Protest, $t-2$			-0.004	-0.01
			(0.04)	(0.04)
RCF, $t-2$			-0.11^{***}	-0.13^{***}
			(0.04)	(0.04)
CAF, $t-2$			0.11^{***}	0.07^{***}
			(0.02)	(0.02)
Protest, $t-3$				0.27^{***}
				(0.04)
RCF, $t-3$				0.05
				(0.04)
CAF, $t-3$				0.12^{***}
				(0.02)
Exp. Dep.	0.0002	0.01	0.01	0.01
	(0.02)	(0.02)	(0.02)	(0.02)
Imp. Dep.	-0.02^{**}	-0.02^{*}	-0.02^{**}	-0.03^{**}
	(0.01)	(0.01)	(0.01)	(0.01)
Trade to GDP	0.0001	0.0000	0.0000	-0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Observations	3,293	3,293	3,262	3,231
R^2	0.42	0.45	0.46	0.48
Adjusted R^2	0.39	0.43	0.44	0.45
F Statistic	280.11^{***}	235.99^{***}	191.53^{***}	165.43^{***}

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

Third, despite being positively correlated with investment, CAF events are negatively correlated with non-investment influence. Substantively, this means that non-investment influence decreases when CAF events occur, much like RCF, and are possibly subject to the same explanations.

Before moving on, it is worth spending a moment to note other trends among the event analysis results. Investment predicts non-investment, meaning that influence operations go up and down as a unit. But these two analyses make clear how that distribution changes in response to different kinds of civil space turmoil. Moreover, investment and non-investment influence are more common in countries less import dependent on China. This could perhaps be because import dependence is already a version of economic leverage, and thus additional investment could be duplicative.

I also perform placebo analyses predicting the relationship between policy uncertainty and aid events and trade agreement events. Because aid and trade are undertaken on long-term, ad-hoc bases and are negotiated by the governments themselves, I expect null results.

I first focus on the association between policy uncertainty arising from elections and aid events or trade agreements. I find no statistically significant relationship between the competitiveness of an upcoming election and aid events or trade agreement events. Although there is a statistically significant relationship between the number of months to an election and aid events, Table C.4 shows that the sign is the reverse of the expectation for investment: aid events decrease close to elections. Lastly, Table C.4 shows the very small but statistically significant relationship between the time until an upcoming election and trade events. But because there is no relationship between trade events and the competitiveness of an election, it is tough to make the case that policy uncertainty derived from uncertainty about the outcome of elections affects trade events.

The results of the placebo analyses show that aid and trade events have differ-

ent relationships with policy uncertainty than investment does.[10] For example, aid events are more likely during times of increased protest and less likely when the target government restricts civil freedoms. Because aid is aimed at governments instead of private society and results in little to no ownership or revenue streams for the sender, it likely does not function as insurance against policy uncertainty. Trade has inconsistent results. Perhaps the one significant relationship is a negative association between CAF and trade, but the coefficient is very small and negative in sign. Even though the substantive suggestion of these results is in line with the theoretical expectation for investments, the weakness of the results corresponds to my expectation that trade is a less suitable than investment to hedge against policy uncertainty.

4.6 Conclusion

In conclusion, I find evidence that China is more likely to choose economic investment as a hedge against policy uncertainty in the target country. I use two measures of policy uncertainty: elections and civil space turmoil. Policy after elections is inherently uncertain because the outcome is uncertain, and movements in civil spaces, especially those undertaken by governments, can indicate instability in the target country and suggest that the target government policy could be uncertain moving forward. I suggest that economic influence is favored under such circumstances because its value is more durable than non-economic influence under policy uncertainty: owning an asset in the target country can serve as a hedge against the possibility that future policy will be unfavorable to China's interests.

This paper uses a new, high-frequency, cross-national, machine-coded event data set called Machine Learning for Peace (MLP), which identifies Resurgent Authoritarian Influence (RAI) events and Civic Space (CS) events by applying the latest in

[10] See Tables C.5 and C.6 in the Appendix.

NLP technology to over 60 million news articles scraped from the internet over 122 months. The measurement of influence events has to date relied on non-systematic analyses of one-off events and detailed analysis of development financial flows, but MLP represents a step forward.

Taken as a whole, this paper's empirics find a notable and robust relationship between investment and policy uncertainty that supports the notion that China uses investments as a hedge against policy uncertainty in target countries. These results are not spurious; the results of the robustness and placebo tests suggest that the way investment relates to uncertainty is different from how it relates to other economic events.

This paper has two theoretical contributions. The first is a clear theoretical model of how China assembles its portfolio of influence operations. The second is a contributes to the literature examining China's participation in international economic markets. Because of the availability of data sets like AidData (Custer et al., 2021; Malik et al., 2021), a relatively large literature has analyzed China's participation in development finance (e.g. loans, grants, and infrastructure projects). But development finance is a relatively small piece of the global financial pie. I expand the literature's conception of China's involvement in global economic markets by arguing that state-affiliated actors use other economic markets for strategic political purposes.

Future work could include an examination of the relationships between different event types across different countries. Does China implement its portfolio of influence activities differently in different target countries? If so, how and why? Because MLP pulls from a broad variety of news sources, it also permits analysis of how news is reported differently by different media outlets. Future research could examine, for example, if there is a systematic way that news reporting varies between sender and target countries.

5

Conclusion

My dissertation has shown how three different kinds of economic markets can influence government policy. I have shown that economic markets of various kinds affect constituent participation in protests, the cost of government borrowing, and the way that foreign states influence government policy. The breadth of these effects shows clearly why and how economic markets' effect on government policy is important. Moreover, knowing these relationships can help forecast what will happen in the future, which is tremendously important because taxpayers, governments, and investors all have skin in the game of effective use of government resources.

My first essay showed that individuals with access to economic insurance are less likely to protest in the face of an income shock than those without. Intuitively, this makes sense, but political economy literature so far has not distinguished between income and wealth, which are two very different things. These results are easy to comprehend because they focus on factors that affect an individual's political participation, a level of analysis very common in the political science literature. Although I would have preferred more direct measures of individual wealth, and to assess my hypothesis in multiple countries, data availability prevented me from doing

so. I nonetheless find evidence supporting my theoretical expectations.

My second essay shifted its focus from household economics to financial markets for government debt securities. I found that government bonds with more concentrated ownership structures have higher price volatility. As a result, these securities have a volatility risk premium; in other words, investors are willing to pay less for bonds whose prices vary more. Although it is possible that a security's "riskiness" affects its ownership concentration in addition to its yield, I argue that such concerns are unfounded because investor decisions about what size of position to take are driven by factors other than risk. I find evidence supporting this, which lends further credibility to my results. Although data on holdings of government bonds is very difficult to find, I have assembled a collage of evidence that supports my theoretical expectations. The findings of my second essay matter because they show how countries whose debt ownership is more concentrated could face higher debt service payments over time. Moreover, the second essay has implications for the democratic peace literature and markets peace literature. Is there something called a debt peace? Does the structure of debt ownership confers power or weakness?

My third essay showed how China uses foreign outbound investment as a strategic policy tool. Although the literature has analyzed China's participation in development finance, other economic flows dwarf foreign aid and loans. I argue that Chinese outbound investment increases during periods of policy uncertainty there, functioning as a hedge against the possibility that future policy will not be favorable. This runs counter to the traditional narrative of foreign investment, which suggests that investment is more likely when policy is stable and property rights are guaranteed. However, there are some weaknesses in my empirical assessment. First, because I rely on event data scraped from news stories, I am actually counting the salience of announcements about investment in the target country media environment; I do not know the start dates, end dates, or magnitude of the actual investments. Second, I

would prefer to improve my identification by comparing Chinese investments against those of a country who does not have such a tight linkage between its government and economy, but such data is not available at present. Nonetheless, this essay contributes a novel theoretical framework and interesting, convincing empirical evidence supporting it.

Each essay alone contributes a substantive expansion in the state of knowledge on its topic. But taken together, my dissertation paints a picture of the breadth of ways that economic markets influence government policy. Governments have to contend with the economic interests of constituents who can demonstrate publicly, investors who can affect the price of their debt, and other states that can use investment as a way to secure influence over future policy.

Appendix A

Household Economic Insurance and Protest Mobilization

A.1 Supplementary Aggregate Results

Table A.1: Robustness to Weighted Protest Occurrence Dependent Variable.

	(1)		(2)		(3)	
VARIABLES	Long-term	Short-term	Long-term	Short-term	Long-term	Short-term
Constant		1.331**		2.661***		2.722***
		(0.551)		(0.454)		(0.429)
ECT		-0.817***		-0.835***		-0.834***
		(0.0535)		(0.0632)		(0.0538)
SI	0.162		0.00104		-0.122	
	(0.143)		(0.105)		(0.155)	
$\Delta Wages$		-2.608		0.529		
		(4.130)		(3.287)		
$\Delta Int.Rate$		-7.491**				
		(3.723)				
$\Delta Wages : Int.Rate$		3.805				
		(5.877)				
$Wages$	0.199		-0.949***			
	(0.245)		(0.352)			
$Int.Rate$	0.898**					
	(0.389)					
$Wages : Int.Rate$	-1.728**					
	(0.774)					
$Stock.Market$			-0.767**		0.376	
			(0.311)		(0.467)	
$Wages : Stock.Market$			1.332**			
			(0.534)			
$\Delta Stock.Market$				2.299		8.241***
				(2.921)		(2.955)
$\Delta Wages : Stock.Market$				-1.129		
				(5.537)		
$Unemp$					1.065*	
					(0.557)	
$Unemp : Stock.Market$					-2.000**	
					(0.929)	
$\Delta Unemp$						9.635***
						(3.538)
$\Delta Unemp : Stock.Market$						-14.29***
						(5.215)

*** p<0.01, ** p<0.05, * p<0.1

A.2 Supplementary Survey Results

Table A.2: Robustness to Alternate Survey Dependent Variable Specifications.

	Dependent variable:		
	Protest	Petition	Boycott
	(1)	(2)	(3)
Age	-0.030^{***}	-0.019^{***}	-0.005^{*}
	(0.005)	(0.003)	(0.003)
Educ	0.085^{**}	0.108^{***}	0.120^{***}
	(0.036)	(0.024)	(0.022)
Children	-0.131^{*}	-0.030	-0.113^{***}
	(0.069)	(0.045)	(0.041)
Female	0.109	0.437^{***}	0.037
	(0.139)	(0.093)	(0.084)
Married	-0.291^{*}	-0.146	0.160^{*}
	(0.164)	(0.107)	(0.096)
RaceEthnicity	0.051	0.080^{**}	0.034
	(0.050)	(0.036)	(0.034)
HHIncome	0.021	0.011	-0.001
	(0.013)	(0.009)	(0.008)
TalkPolitics	0.190^{***}	0.191^{***}	0.170^{***}
	(0.032)	(0.021)	(0.019)
PartyID	-0.005	0.038	0.032
	(0.072)	(0.050)	(0.045)
CloseToParty	-0.084^{**}	-0.151^{***}	-0.083^{***}
	(0.042)	(0.024)	(0.021)
HowClose	0.194^{**}	0.048	0.037
	(0.079)	(0.039)	(0.037)
Union	0.337^{**}	-0.001	-0.063
	(0.168)	(0.122)	(0.111)
HomeOwner	-0.208	0.050	-0.118
	(0.241)	(0.165)	(0.138)
AnyLostJobs	0.822^{***}	0.572^{***}	0.564^{***}
	(0.244)	(0.182)	(0.166)
CloseToParty:HowClose	-0.006	0.020^{**}	0.010
	(0.018)	(0.009)	(0.008)
HomeOwner:AnyLostJobs	-0.703^{**}	-0.282	-0.144
	(0.296)	(0.210)	(0.192)
Observations	3,188	3,188	3,188
Log Likelihood	-803.692	$-1,510.503$	$-1,765.149$
Akaike Inf. Crit.	1,643.383	3,057.006	3,566.298
Bayesian Inf. Crit.	1,752.592	3,166.215	3,675.507
Note:			*p<0.1; **p<0.05; ***p<0.01

Table A.3: Potential Omitted Variable Bias for Varying Values of R^2_{max} (Measured in %).

Variable	0.2	0.3	0.4
HomeOwner	-2.30	-99.80	-197.30
AnyLostJobs	0.20	6.70	13.20
HomeOwner:AnyLostJobs	1.10	47.00	93.00

Appendix B

The Effect of Ownership Concentration on Government Bond Volatility and Yields

B.1 Selection Effect Graphs

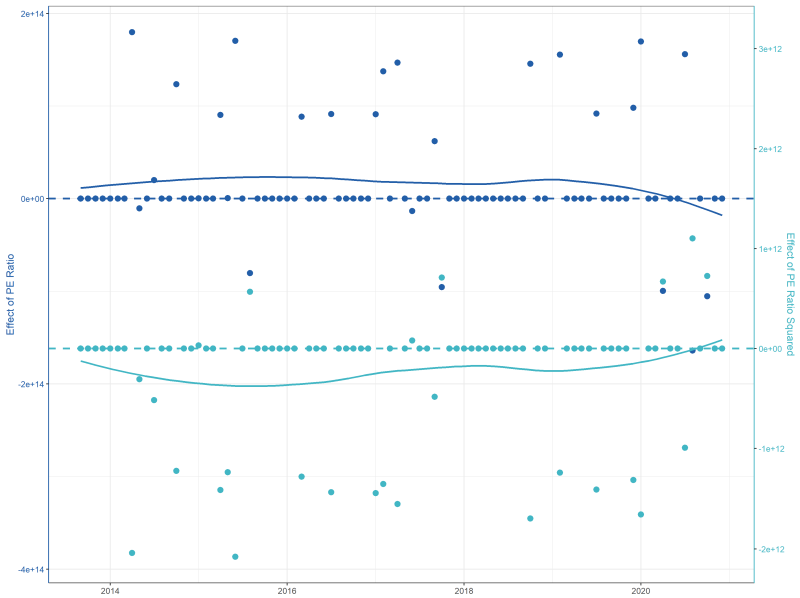


FIGURE B.1: The Effect of PE Ratio and PE Ratio Squared on investor selection, over time.

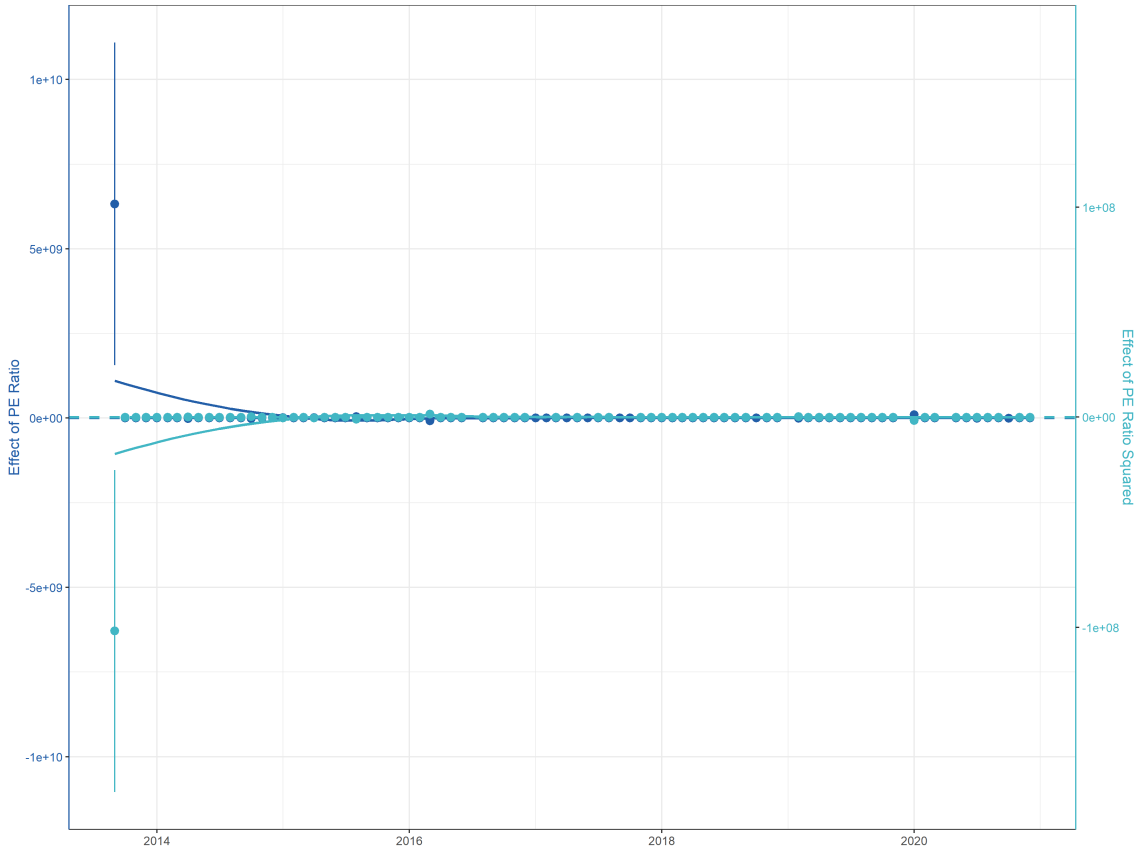


FIGURE B.2: The Effect of PE Ratio and PE Ratio Squared on investor position size, over time.

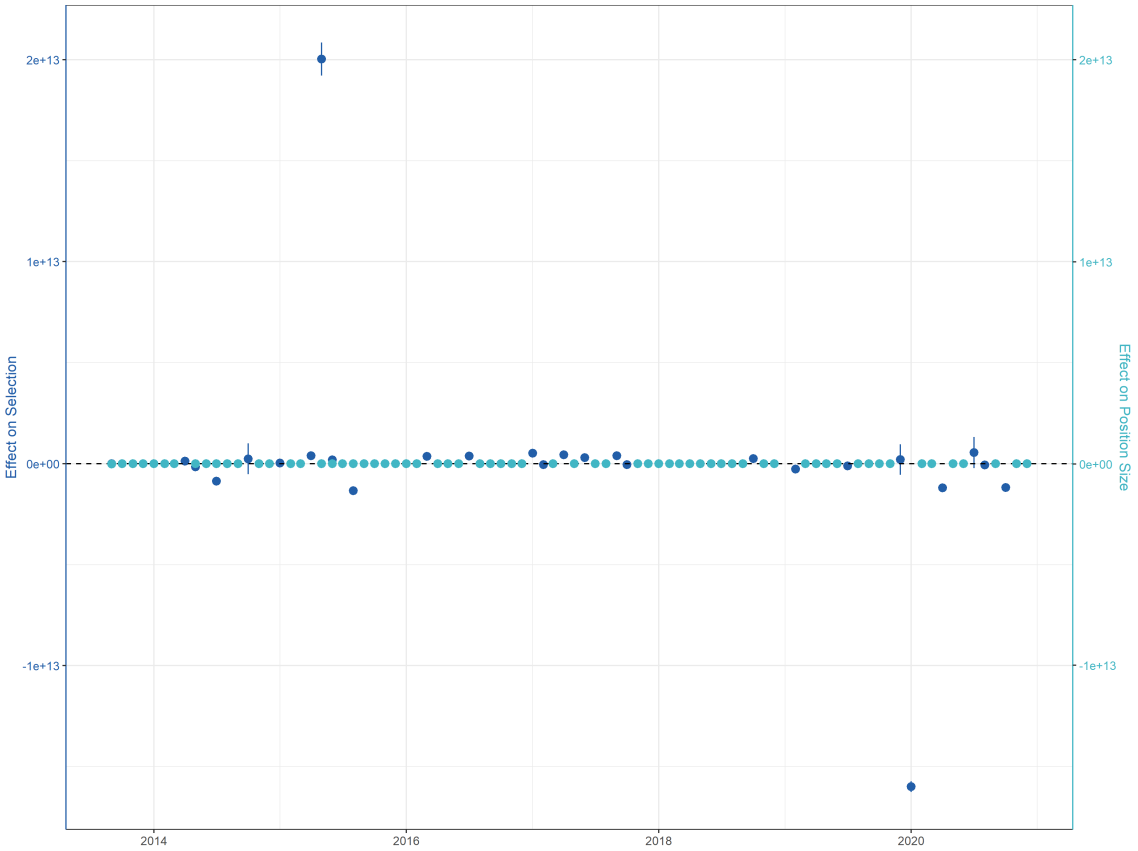


FIGURE B.3: The Effect of Dividend Yield Ratio on investor selection and position size, over time.

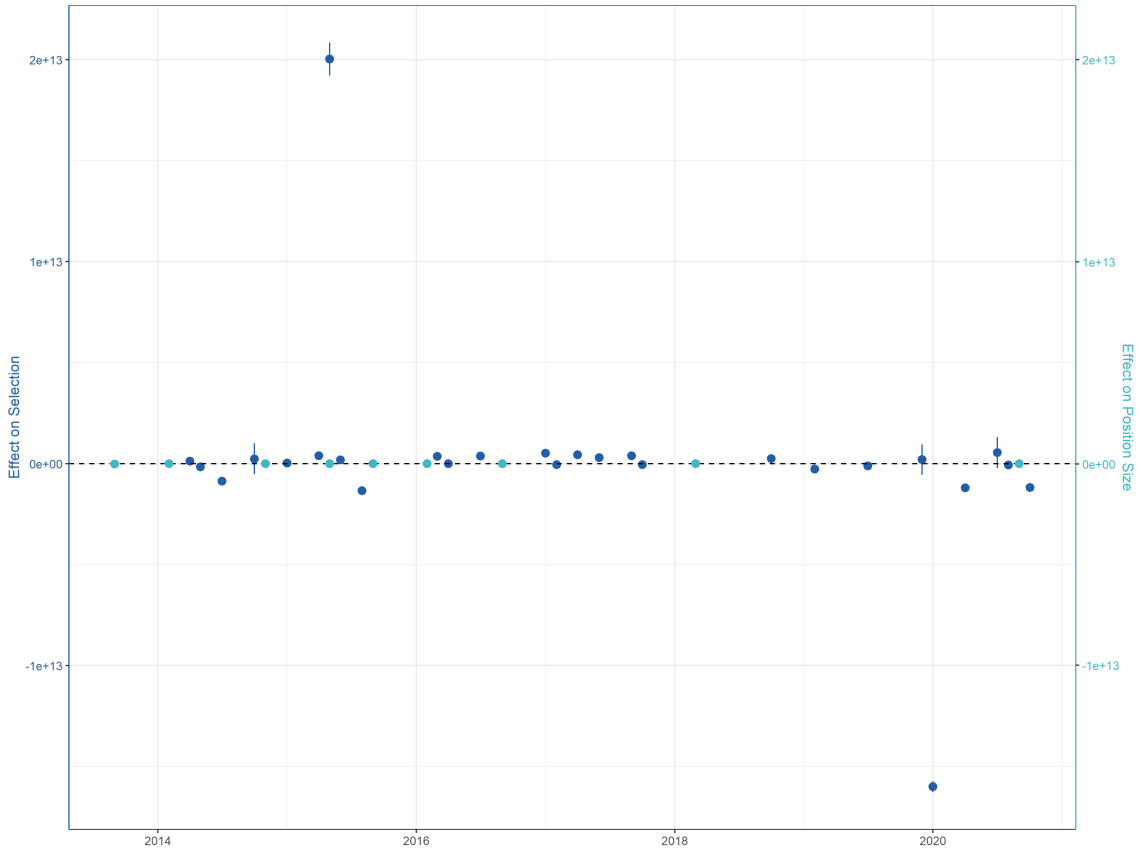


FIGURE B.4: The Effect of Dividend Yield Ratio on investor selection and position size, over time. Statistically significant results only.

B.2 Additional Time Series Results

Table B.1: The Effect of Ownership Concentration on Bond Price Volatility, Within Models

	DV: Number of Days Rolling Volatility Δv_{it}^{n}					
	3d	5d	10d	30d	60d	90d
	(1)	(2)	(3)	(4)	(5)	(6)
HHI	0.0000	−0.0000	−0.0000	0.0000	−0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Pct. OS Known	−0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Amt. Outstanding	0.0000***	0.0000***	0.0000***	0.0000***	0.0000***	0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Mty. Size	0.0000***	0.0000***	0.0000***	0.0000***	0.0000***	0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Months to Maturity Sq.	−0.0000	−0.0000	0.0000	−0.0000	−0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Observations	2,493	2,493	2,493	2,493	2,493	2,493
R^2	0.1676	0.1578	0.0633	0.2342	0.2084	0.0632
Adjusted R^2	0.1001	0.0895	−0.0127	0.1721	0.1442	−0.0128
F Statistic	85.4500***	149.0904***	95.4925***	89.3366***	126.4709***	35.3654***
Note:						*p<0.1; **p<0.05; ***p<0.01

Table B.2: The Effect of Ownership Concentration on Bond Price Volatility, Within Models, Robust SE

	DV: Number of Days Rolling Volatility Δv_{it}^n					
	3d	5d	10d	30d	60d	90d
	(1)	(2)	(3)	(4)	(5)	(6)
HHI	0.0000	−0.0000	−0.0000	0.0000	−0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Pct. OS Known	−0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Amt. Outstanding	0.0000***	0.0000***	0.0000***	0.0000***	0.0000***	0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Mty. Size	0.0000***	0.0000***	0.0000**	0.0000***	0.0000***	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Months to Maturity Sq.	−0.0000	−0.0000	0.0000	−0.0000	−0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)

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

Table B.3: The Effect of Ownership Concentration on Bond Price Volatility, FD Models

	DV: Number of Days Rolling Volatility Δv_{it}^n					
	3d	5d	10d	30d	60d	90d
	(1)	(2)	(3)	(4)	(5)	(6)
HHI	−5.3209	2.3315	11.9268***	6.8989***	5.0483***	4.5194***
	(4.4114)	(3.4294)	(2.7853)	(2.5991)	(1.9113)	(1.6265)
Pct. OS Known	0.6641**	0.0309	−0.8095***	−0.5639***	−0.4013***	−0.3643***
	(0.3299)	(0.2565)	(0.2083)	(0.1944)	(0.1430)	(0.1216)
Months to Maturity	−73.5956***	19.4323	135.7924***	94.5575***	81.0514***	75.3684***
	(19.1687)	(14.9015)	(12.1026)	(11.2936)	(8.3050)	(7.0673)
Months to Maturity Sq.	−12.3064**	8.5741**	34.4706***	25.0800***	22.9463***	22.6083***
	(5.4864)	(4.2650)	(3.4640)	(3.2324)	(2.3770)	(2.0228)
Constant	0.0066**	−0.0007	−0.0099***	−0.0071***	−0.0060***	−0.0053***
	(0.0028)	(0.0022)	(0.0018)	(0.0017)	(0.0012)	(0.0010)
Observations	2,397	2,397	2,397	2,397	2,397	2,397
R^2	0.0084	0.0033	0.0560	0.0330	0.0461	0.0577
Adjusted R^2	0.0068	0.0016	0.0544	0.0314	0.0445	0.0561
F Statistic	5.0904***	1.9598*	35.4704***	20.4050***	28.8699***	36.6211***

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

Table B.4: The Effect of Volatility on Yield

	DV: Monthly Average Close Yield					
	(1)	(2)	(3)	(4)	(5)	(6)
3-day Vol.	0.28***					
	(0.02)					
5-day Vol.		0.54***				
		(0.02)				
10-day Vol.			1.00***			
			(0.03)			
30-day Vol.				0.68***		
				(0.03)		
60-day Vol.					0.60***	
					(0.04)	
90-day Vol.						0.58***
						(0.05)
Months to Maturity	−88.35***	−91.40***	−94.16***	−86.71***	−84.30***	−82.71***
	(22.81)	(22.49)	(21.86)	(22.56)	(22.86)	(22.93)
Months to Maturity Sq.	−40.69***	−41.34***	−41.51***	−40.36***	−40.10***	−40.27***
	(6.98)	(6.88)	(6.69)	(6.90)	(7.00)	(7.02)
Constant	−0.01***	−0.01***	−0.01***	−0.01***	−0.01***	−0.01***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	10,856	10,856	10,856	10,856	10,856	10,856
R^2	0.03	0.05	0.11	0.05	0.02	0.02
Adjusted R^2	0.03	0.05	0.11	0.05	0.02	0.02
F Statistic	102.07***	208.45***	434.92***	183.78***	84.66***	63.68***

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

Appendix C

China's Foreign Investment: Hedging Against Policy Uncertainty

C.1 Alternate Election Definitions

Table C.1: The Effect of Elections on Investment

	DV: Investment Events			
	Narrow		Broad	
	(1)	(2)	(3)	(4)
Lagged DV, Narrow	0.159***	0.068*		
	(0.032)	(0.036)		
Non-Investment, Narrow	0.388***	0.362***		
	(0.031)	(0.031)		
Lagged DV, Broad			0.153***	0.241***
			(0.032)	(0.035)
Non-Investment, Broad			0.404***	
			(0.032)	
Months To Next Election	−0.0001***	−0.0001***	−0.0001***	−0.0001***
	(0.00002)	(0.00002)	(0.00002)	(0.00002)
Election Within One Year	−0.002***	−0.002***	−0.002***	−0.002***
	(0.001)	(0.001)	(0.001)	(0.001)
Executive Vote Margin	−0.0003***	−0.0002***	−0.0003***	−0.0003***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Protest Events		0.001		0.004
		(0.017)		(0.019)
RCF Events		−0.037***		−0.049***
		(0.013)		(0.014)
CAF Events		0.033***		0.043***
		(0.007)		(0.008)
Exp. Dep.	−0.029***	−0.024***	−0.029***	−0.023***
	(0.006)	(0.006)	(0.006)	(0.007)
Imp. Dep.	0.001	0.001	0.001	0.0002
	(0.003)	(0.003)	(0.003)	(0.004)
Trade/GDP	0.0001**	0.0001**	0.0001*	0.0001**
	(0.00004)	(0.00004)	(0.00004)	(0.00004)
Observations	1,177	1,177	1,177	1,177
R^2	0.356	0.379	0.349	0.290
Adjusted R^2	0.287	0.310	0.279	0.211
F Statistic	73.419***	58.750***	71.164***	43.162***
Note:				*p<0.1; **p<0.05; ***p<0.01

Table C.2: The Effect of Elections on Investment

	DV: Investment Events			
	Narrow		Broad	
	(1)	(2)	(3)	(4)
Lagged DV, Narrow	0.161^{***}	0.069^{*}		
	(0.032)	(0.036)		
Non-Investment, Narrow	0.393^{***}	0.367^{***}		
	(0.031)	(0.031)		
Lagged DV, Broad			0.155^{***}	0.245^{***}
			(0.032)	(0.035)
Non-Investment, Broad			0.408^{***}	
			(0.032)	
Months To Next Election	-0.0001^{***}	-0.00004^{**}	-0.0001^{***}	-0.0001^{***}
	(0.00002)	(0.00002)	(0.00002)	(0.00002)
Election Within Six Months	-0.001^{**}	-0.001	-0.001^{*}	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Executive Vote Margin	-0.0003^{***}	-0.0002^{***}	-0.0003^{***}	-0.0003^{***}
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Protest Events		0.001		0.004
		(0.017)		(0.019)
RCF Events		-0.036^{***}		-0.049^{***}
		(0.013)		(0.014)
CAF Events		0.034^{***}		0.044^{***}
		(0.007)		(0.008)
Exp. Dep.	-0.028^{***}	-0.023^{***}	-0.028^{***}	-0.021^{***}
	(0.006)	(0.006)	(0.006)	(0.007)
Imp. Dep.	0.001	0.001	0.001	0.0002
	(0.003)	(0.003)	(0.003)	(0.004)
Trade/GDP	0.0001^{**}	0.0001^{**}	0.0001^{*}	0.0001^{**}
	(0.00004)	(0.00004)	(0.00004)	(0.00004)
Observations	1,177	1,177	1,177	1,177
R^2	0.352	0.375	0.345	0.284
Adjusted R^2	0.281	0.305	0.274	0.205
F Statistic	71.950^{***}	57.714^{***}	69.824^{***}	41.955^{***}

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

C.2 Robustness and Placebo Analyses

Table C.3: The effect of civil space turmoil on broadly-defined economic investment influence events.

	DV: Investment Events			
	No Lags	1 Lag	2 Lags	3 Lags
	(1)	(2)	(3)	(4)
Lagged DV	-0.09^{***}	-0.08^{***}	-0.08^{***}	-0.08^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Non-Investment Events	0.44^{***}	0.45^{***}	0.45^{***}	0.45^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Protest Events	0.001	0.002	0.01	0.01
	(0.03)	(0.03)	(0.03)	(0.03)
RCF Events	-0.03	-0.03	-0.04	-0.01
	(0.03)	(0.03)	(0.03)	(0.03)
CAF Events	0.14^{***}	0.16^{***}	0.17^{***}	0.16^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Protest, $t-1$		-0.04	-0.04	-0.01
		(0.03)	(0.03)	(0.03)
RCF, $t-1$		-0.0001	-0.001	-0.01
		(0.03)	(0.03)	(0.03)
CAF, $t-1$		-0.05^{***}	-0.02^{*}	-0.02
		(0.01)	(0.01)	(0.01)
Protest, $t-2$			-0.03	0.004
			(0.03)	(0.03)
RCF, $t-2$			0.04	0.05
			(0.03)	(0.03)
CAF, $t-2$			-0.05^{***}	-0.06^{***}
			(0.01)	(0.01)
Protest, $t-3$				-0.19^{***}
				(0.03)
RCF, $t-3$				-0.04
				(0.03)
CAF, $t-3$				0.05^{***}
				(0.01)
Exp. Dep.	0.03^{***}	0.03^{***}	0.03^{**}	0.03^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Imp. Dep.	-0.03^{***}	-0.03^{***}	-0.03^{***}	-0.03^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Trade to GDP	0.0000	0.0000	0.0001	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Observations	3,293	3,293	3,262	3,231
R^2	0.41	0.42	0.42	0.43
Adjusted R^2	0.39	0.39	0.39	0.40
F Statistic	276.82^{***}	203.11^{***}	159.83^{***}	137.37^{***}

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

Table C.4: Robustness: The Effect of Elections on Aid and Trade Events

	DV: Investment Events			
	Aid		Trade	
	(1)	(2)	(3)	(4)
Lagged Aid	−0.010	−0.007		
	(0.030)	(0.030)		
Non-Aid	0.009***	0.010***		
	(0.002)	(0.002)		
Lagged Trade			0.026	0.016
			(0.031)	(0.031)
Non-Trade			−0.002	−0.006**
			(0.002)	(0.003)
Months To Next Election	0.00000**	0.00000**	−0.00001***	−0.00001***
	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Executive Vote Margin	0.00001	0.00001	−0.00001	−0.00001
	(0.00001)	(0.00001)	(0.00001)	(0.00001)
Protest Events		−0.003		−0.001
		(0.002)		(0.003)
RCF Events		0.0005		−0.001
		(0.002)		(0.002)
CAF Events		−0.0003		0.003***
		(0.001)		(0.001)
Exp. Dep.	0.0004	0.0003	−0.002**	−0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
Imp. Dep.	0.0001	0.0001	−0.0002	−0.0002
	(0.0004)	(0.0004)	(0.001)	(0.001)
Trade/GDP	−0.00000	−0.00000	0.00000	0.00000
	(0.00000)	(0.00000)	(0.00001)	(0.00001)
Observations	1,177	1,177	1,177	1,177
R^2	0.028	0.029	0.022	0.030
Adjusted R^2	−0.076	−0.078	−0.083	−0.078
F Statistic	4.347***	3.213***	3.430***	3.241***
Note:				*p<0.1; **p<0.05; ***p<0.01

Table C.5: The Effect of Civic Space Turmoil on Aid Events.

	DV: Aid Events			
	No Lags	1 Lag	2 Lags	3 Lags
	(1)	(2)	(3)	(4)
Lagged DV	−0.01	0.02	0.03*	0.05***
	(0.02)	(0.02)	(0.02)	(0.02)
Non-Aid Events	−0.001**	−0.001*	−0.001*	−0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Protest Events	0.05***	0.05***	0.04***	0.05***
	(0.002)	(0.002)	(0.002)	(0.002)
RCF Events	−0.01***	−0.01**	−0.01**	−0.01**
	(0.002)	(0.002)	(0.002)	(0.002)
CAF Events	−0.001	−0.001	−0.001	−0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Protest, $t-1$		−0.01***	−0.01***	−0.01***
		(0.002)	(0.002)	(0.002)
RCF, $t-1$		−0.002	−0.0003	−0.0001
		(0.002)	(0.002)	(0.002)
CAF, $t-1$		−0.0004	−0.0003	0.0003
		(0.001)	(0.001)	(0.001)
Protest, $t-2$			0.02***	0.02***
			(0.002)	(0.002)
RCF, $t-2$			−0.003	−0.003
			(0.002)	(0.002)
CAF, $t-2$			−0.001	−0.001
			(0.001)	(0.001)
Protest, $t-3$				−0.01***
				(0.002)
RCF, $t-3$				−0.001
				(0.002)
CAF, $t-3$				−0.002
				(0.001)
Exp. Dep.	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Imp. Dep.	−0.0003	−0.0003	−0.0003	−0.0002
	(0.001)	(0.001)	(0.001)	(0.001)
Trade to GDP	−0.0000	−0.0000	−0.0000	−0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Observations	3,293	3,293	3,262	3,231
R^2	0.15	0.15	0.18	0.19
Adjusted R^2	0.11	0.11	0.14	0.15
F Statistic	66.93***	50.47***	49.79***	43.47***

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

Table C.6: The Effect of Civic Space Turmoil on Trade Events.

	DV: Trade Events			
	No Lags	1 Lag	2 Lags	3 Lags
	(1)	(2)	(3)	(4)
Lagged DV	−0.01	−0.01	−0.01	−0.01
	(0.02)	(0.02)	(0.02)	(0.02)
Non-Trade Events	−0.003	−0.002	−0.002	−0.005**
	(0.002)	(0.002)	(0.002)	(0.002)
Protest Events	−0.01	−0.01	−0.01	−0.01*
	(0.01)	(0.01)	(0.01)	(0.01)
RCF Events	−0.01**	−0.01	−0.01	−0.004
	(0.01)	(0.01)	(0.01)	(0.01)
CAF Events	−0.01**	−0.005	−0.004	−0.01***
	(0.003)	(0.003)	(0.003)	(0.003)
Protest, $t-1$		−0.01	−0.01	−0.01
		(0.01)	(0.01)	(0.01)
RCF, $t-1$		−0.01*	−0.01	−0.01
		(0.01)	(0.01)	(0.01)
CAF, $t-1$		−0.004	−0.003	−0.004
		(0.003)	(0.003)	(0.003)
Protest, $t-2$			−0.004	0.001
			(0.01)	(0.01)
RCF, $t-2$			−0.01*	−0.02**
			(0.01)	(0.01)
CAF, $t-2$			−0.01*	−0.02***
			(0.003)	(0.003)
Protest, $t-3$				0.002
				(0.01)
RCF, $t-3$				−0.001
				(0.01)
CAF, $t-3$				0.03***
				(0.003)
Exp. Dep.	−0.001	−0.001	−0.001	−0.001
	(0.003)	(0.003)	(0.003)	(0.003)
Imp. Dep.	−0.004**	−0.004**	−0.004**	−0.004**
	(0.002)	(0.002)	(0.002)	(0.002)
Trade to GDP	0.0000	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Observations	3,293	3,293	3,262	3,231
R^2	0.01	0.01	0.01	0.04
Adjusted R^2	−0.04	−0.04	−0.04	−0.01
F Statistic	2.15**	1.99**	1.98**	6.79***

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

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Biography

Timothy R. McDade is a political economist specializing in economic markets, political unrest, and quantitative methods. Prior to arriving at Duke, he worked for Microsoft Azure in Seattle and Beijing. From 2014 to 2018, he was in charge of constructing analyses for Microsoft Azure China. His reports informed company financials as well as internal decisions on data center capacity, marketing campaigns, and supply chain improvements. He spent one week a month on the ground in China from 2014 to 2016 and relocated to Beijing full-time from 2016 to 2018.

Timothy's undergraduate degrees are in Mathematics (with Honors) and Chinese from the College of William & Mary. His Mathematics Honors Thesis modeled oyster repopulation efforts in the Chesapeake Bay. After studying Chinese language at Tsinghua University, his Chinese senior thesis analyzed the relationship between Chinese state-owned enterprises and the central government.

Timothy was a Monroe Scholar at the College of William & Mary and has received numerous grants funding his research at Duke University.