Essays on Information Regimes and Uncertainty in Emerging Economies



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To Nouri,
whose light shines brighter every day,
my partner Piotr,
who will never seize to amaze me,
and my twin sister Isabella,
whose shared Nigerian heritage has uplifted, empowered and emboldened me since I can remember.

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Abstract

This thesis contains three papers studying the behavioural response of firms, households and investors in emerging economies to changes in expectations and uncertainty. The first paper investigates how economic agents change their consumption and investment behaviour to news about unanticipated changes to power sector plans by exploiting a novel dataset for the identification of news shocks in a vector–autoregression. While there is an immediate expectations–driven GDP response to a surprise capacity addition in OECD economies, this positive output effect is absent in developing and emerging countries. Firms and households in developing countries respond to failed power projects by immediately reducing investments and are also subjected to lower economic growth as a result relative to the pre–disappointment period. The second paper examines regulatory uncertainty as a barrier affecting infrastructure service investments and quality in Brazil. Exploiting a quasi–natural experiment for identification, it finds that electric utilities partially shielded from uncertainty significantly increase their irreversible investments in electricity distribution infrastructure. This response is qualitatively consistent with optimal behaviour as prescribed by real options theory. The third paper uses an autoregressive distributed lag model and finds that sovereign and corporate bond yields in emerging economies exhibit a long–run relationship that is not explained by economic conditions, firm fundamentals, bond properties, or external factors affecting emerging countries. The paper identifies perceived transfer risks by investors as the key channel. Investors' perceived transfer risks lead corporate and sovereign bond yields to co–move and generally converge. The introduction and conclusion of the thesis further explain common themes.

Introduction

The management of expectations in the economy is crucial to the ability of regulators, governments and policy–makers to achieve the desired outcomes for firms and households. Reducing uncertainty and increasing the clarity of pricing tools has been particularly encouraged in emerging and developing countries, notably by the international development institutions: emerging economies have been encouraged and even ranked to improve the transparency of forward–looking plans, particularly in sectors where investments are large and irreversible such as infrastructure (Foster et al., 2017). Still, insufficient capital expenditures by firms in these critical sectors are found to constrain productivity and limit businesses' ability to grow (Escribano, Guasch and Peña, 2008). This thesis sheds light on the extent to which the expectations of emerging country investors matter for policy outcomes, and offers explanations for how economic output, investment levels and even firm borrowing costs can be adversely impacted by uncertainty and risk.

The first chapter "The Impact of News Shocks in Infrastructure on Short Term Expectations and Long Term Growth" proposes a narrative approach to the identification of news shocks in VAR settings, and applies it to the infrastructure sector. Physical infrastructure is heralded as a driver of both long–term growth prospects (due to its role as a productivity–enhancing investment opportunity) and higher short–term business cycle vulnerabilities (given its ability to either dampen or amplify external shocks). Yet little remains known about the impact of infrastructure projects on the economy through changes in agents' expectations. This paper exploits surprises and disappointments in the form of unexpected and failed power generation projects. These occur across OECD and developing countries and can be interpreted as affecting the future productive capacity of an

economy without affecting current fundamentals other than through the channel of expectations. In utilizing a novel dataset capturing infrastructure news events, this chapter contributes an alternative methodology to the identification of news shocks in the presence of foresight. I apply a narrative approach to the identification of infrastructure news shocks to future productivity, and estimate their impact on short–term expectations-driven macroeconomic activity and long-term economic growth.

Two key results emerge. The first main result of the paper is that while there are immediate demand–side responses to an increase in the stock of power infrastructure assets across country groupings, weak long-run supply–side effects fail to deliver sustained economic growth in non–OECD countries. These findings support the view that the provision of physical electricity infrastructure assets can be considered a necessary but non–sufficient requirement for long–term economic growth. The question then arises of the role of infrastructure *services* in contributing to long-term growth dividends. These improvements require investments in maintenance as well as grid–system optimization.

The second key result reveals that disappointments in infrastructure provision are costly and harbour larger and longer–lasting investment and GDP–hampering effects in the developing world than implied by the existing literature. It is often posited that firms and households ascribe limited credibility to government plans when there exists i) a history of widespread reversals in power sector reform and policies, and ii) a lack of regulatory independence (Kessides, 2013). However, the results show that private agents in developing countries appear to not only respond to such deviations from energy plans by immediately reducing investments, but are also subjected to lower economic growth as a result in the long–run.

One possible explanation for this effect is anchored in real options theory, which seeks to explain how firms make irreversible investment decisions in uncertain

environments (Dixit and Pindyck, 1994*a*). Its central prediction posits that an increase in uncertainty, by raising the value of awaiting further information on the rate of return on an investment, increases the profitability threshold required to make these investments worthwhile. Since sunk cost investments are commonplace in the infrastructure sector, deviations from energy plans could represent an increase in uncertainty urging firms to defer investments as they update their beliefs about energy plans. These deferred investments can in part explain the suppressed long–run growth trajectory in developing countries depicted in the analysis.

The absence of a long–term suppression of GDP in OECD countries implies that these economies recover more quickly from bad news. Possible reasons are higher levels of trust in the energy regulatory agencies, which tend to be less independent of the Ministry of Energy in developing countries. The empirical results thus imply that the provision of energy plans can lead to adverse investment effects if such plans are unreliable and project failures occur. Hence, while the provision of energy plans is heralded as a good practice objective of energy sector reforms, this paper calls for careful examination of these information regimes in the presence of high probabilities of project failure.

The second chapter "The Effects of Uncertainty on Lumpy Firm Investments: Evidence from Electricity Provision in Brazil" anchors itself on real options theory and further investigate the role of uncertainty in affecting infrastructure services and subsequently infrastructure quality. My difference–in–difference estimation relies on a pseudo–natural experiment in Brazil to identify plausibly exogenous variation in the policy environments facing firms. To begin with, I re–construct a measure of electricity firm profitability widely used in power utility finance, which reflects the extent to which retail electricity prices in Brazil cover electricity distributors' expenses on power purchases – the cost–recovery ratio. A drought led to exogenously–induced changes in utility profitability. Using Brazilian power sector data, I exploit a pseudo–experiment set–up that was in part triggered by the 2013

drought. I interpret the introduction of a tariff flag mechanism as an improvement in the predictability of future retails electricity prices by power providers. The paper first shows that improvements in the profitability of electricity firms are associated with contemporaneous increases in marginal and lumpy investment activity, as well as with reductions in electricity outages in the subsequent period. Next, it finds that firms exposed to this policy responded by significantly increasing their irreversible investments in the distribution grid: sunk cost investments in the form of substation replacements and constructions increase for these power providers relative to unexposed firms and to the pre–policy era. I perform a number of tests and provide qualitative evidence to strengthen my case for the validity of the results in a diff–in–diff setting. These include parametric and non–parametric methods for matching treatment and control groups on observables, structural break tests as well as standard error adjustments for serial correlation of the residuals.

The insights are consistent with existing research on the investment–deterring effects of uncertainty and real options theory. The results also suggest that firm beliefs over the predictability of power prices have real impacts on the policy variables that regulators seek to control in developing and emerging economies. The impact of uncertainty has to my knowledge not previously been documented as a potential cause for deficient infrastructure quality across the developing world. If the phenomena highlighted in this paper hold true across other power markets, then this would have implications for the optimal policy design in these regions. Like chapter 1, this chapter suggests information regimes could be used by regulators as an additional policy tool for managing investments by firms. The results provide a possible explanation for a well–known puzzle in infrastructure, namely the co–existence of high prices and low service quality in public goods provision. The findings suggest the power price level is not the only metric affecting firm investments, but that the degree of price variation relative to costs facing utilities matters, too.

The third chapter "Long–Term Relationships between Sovereign and Corporate Bond Yields: Evidence from Emerging Markets" is motivated by the observation that – until the pandemic – emerging countries' breathing space for running a primary fiscal deficit has been shrinking over the past 10 years: periods in which the median economic growth rate across emerging economies exceeded the rate payable on sovereign hard currency bonds have become shorter–lived since 2000. It examines how rising sovereign risks could affect firms through the channel of investor beliefs. This question has empirically been complicated by the fact that bond yields have been found to display a mix of stationary and unit–root type qualities (Almeida et al., 2017). I build on the literature by employing a strategy that explicitly tests and allows for this time–series characteristic and by allowing for unobservable cross–dependencies between interconnected economies. The paper discovers that sovereign and corporate bond yields exhibit a long–run relationship that is not explained by economic conditions, firm fundamentals, bond properties and external factors affecting emerging countries as a whole. The paper is able to identify this relationship as driven by perceived transfer risks, i.e. the likelihood in investors' eyes that sovereigns will transfer some of their repayment burden to private foreign creditors. Transfer risks lead corporate and sovereign bond yields to co–move – and generally converge within 12 weeks of a deviation from the long–run relationship.

The effect on firms' borrowing costs is strongest when sovereign bond yields are rising, and during periods when firms can borrow more cheaply than their "associated" governments. The results imply that firms which currently enjoy lower borrowing costs than their sovereign country of risk may be more likely to experience a belief–driven deterioration in borrowing conditions in the future. With yields rising in developed economies this could spill over to emerging economies and contribute to a heightened sense of transfer risks. The findings hold policy implications for emerging country governments that wish to manage the impact of their foreign debt obligations on the private sector. For example, the findings show that avoiding explicit transfer events does not prevent private borrowing costs from rising even in

the absence of a deterioration of private sector fundamentals.

In what follows each of the three chapters is presented as an individual paper, before concluding remarks discuss thoughts on avenues for further research.

1

The Impact of News Shocks in Infrastructure on Short–Term Expectations and Long–Term Growth

Abstract

How do economies respond to news shocks in the infrastructure sector, and adjust once the anticipated shock is realized in the long-run? Such shocks occur across OECD and developing countries and can be interpreted as affecting the future productive capacity of an economy without changing current fundamentals. They take the form of surprises, when unplanned projects are announced, and disappointments, when planned projects fail to materialize. Exploiting a novel dataset for the narrative identification of news shocks in a vector–autoregression (VAR), this paper examines the expectations–driven impact of infrastructure news shocks, while isolating developing countries' responses from OECD country effects. While there is an immediate expectations–driven GDP response to a surprise capacity addition in OECD economies, this positive output effect is absent in developing and emerging countries. Firms and households in developing countries appear to not only respond to failed power projects by immediately reducing investments, but are also subjected to lower economic growth as a result relative to the pre–disappointment period. Alternative tests of non–fundamentalness are implemented. The findings call for careful examination of policy proposals for developing countries to i) close the infrastructure gap by means of physical infrastructure additions and ii) increase the availability of energy plans. The results also highlight the role of infrastructure service quality and information regimes as an economic policy variable.

1.1 Introduction

Shocks to beliefs about future fundamentals have been posited to drive business cycle fluctuations since the earliest contributions of Pigou (1927) and others. In recent work this fact has been highlighted in the form of structural VARs and in empirical DSGE studies (notably Beaudry and Portier (2006) and Schmitt-Grohé and Uribe (2012) among others). Most if not all of the attention of this literature has focused on developed economies, with few studies on how news shocks propagate in developing countries.

The lack of empirical evidence on expectations–driven business cycles in developing and emerging countries stands in contrast to the attention these economies have received for their increasing susceptibility to short–term macroeconomic risks as well as vanishing macroeconomic growth outlooks: nearly 80 percent of the World Bank's macroeconomic growth forecasts for non–OECD countries were downgraded by on average 0.2 percentage points over the past 10 years, including major downward revisions of long–term growth forecasts (Arteta et al., 2019); physical infrastructure is heralded as one key simultaneous driver of both dwindling long-term growth prospects (due to its role as a productivity–enhancing investment opportunity) and higher short–term business cycle vulnerabilities (given its ability to either dampen or amplify external shocks). Yet very little remains known about the impact of infrastructure projects on the economy through changes in agents' expectations.[1] In utilizing a novel dataset capturing infrastructure news events, this paper aims to shed light on this understudied question, while contributing an alternative methodology to the identification of news shocks in the presence of foresight.

This paper answers the following research question: how do news shocks in power generation affect economic output in developing countries, and how

[1]The most related paper to a study on how infrastructure news shocks propagate in the developing world is by Arezki, Ramey and Sheng (2017), and examines the response of macro-aggregates to giant oil field discoveries in a global dataset.

does this economic response to changes in expectations differ from the OECD country responses?

I apply a narrative approach to the identification of infrastructure news shocks to future productivity, and estimate their impact on short–term expectations-driven macroeconomic activity and long-term economic growth. First, I construct a variable representing news events in the infrastructure sector by extracting electricity generation projects that were not previously announced in governments' national energy plans as surprises, and projects that had been announced but were never realized as disappointments. The raw news measures are quantified relative to the size of each country's existing cumulative electricity capacity. Due to time–to–build lags embedded in infrastructure provision these measures can be interpreted as shocks to the future productive capacity of the economy that agents learn about ahead of the actual change in the fundamental. From this raw news shock measure the paper recovers the component that is not predicted by its own past lags, historical macro-aggregates and their forecasts. It is conjectured that this unforecastable component of unplanned power projects is sufficiently purged of noise, and can be interpreted as a classical news shock.

This paper introduces the narratively identified news shock measure into an otherwise standard VAR model employing a Cholesky identification scheme. Under this environment, infrastructure surprises and disappointments are ordered first to reflect their posited exogeneity. A problem endemic in news VARs arises when agents' information sets are larger than that of the econometrician, leading to potentially spurious interpretations of the impulse responses obtained. This paper implements a test for informational sufficiency, finding that the principal components of the macro-variables in the VAR model are orthogonal to past lags of the principal components of a larger macro-dataset. This lends support the key assumption in the VAR analysis, namely that it is sufficiently rich to estimate the structural news shock.

Equipped with these insights, the paper reveals markedly different responses to infrastructure news in OECD and developing economies, first in the expectations–driven short run and subsequently at long run horizons. While good news in infrastructure in the form of a surprise power project leads to an immediate expectations–driven output expansion in OECD economies, this positive output effect is absent in developing and emerging countries. Further, in contrast to higher–income countries disappointments in the form of unrealized power plants cause permanent economic contractions that persist in the long run in developing economies. Unlike lower income firms and households, OECD economies do not treat failed power projects as "bad news", suggesting that the reasons for why projects are abandoned matter crucially for the economy. I explore potential explanations for these results in real options theory and the role of infrastructure services as a necessary condition for new generation plants to imply an improvement in future productivity.

Albeit related in its sectoral focus to studies employing microeconomic techniques to assess the impacts of infrastructure on trade and income levels in a country case study (Donaldson, 2018), and to the macroeconomic literature on global infrastructure-growth dynamics (including Roller and Waverman (2001); Aschauer (1989); Calderón, Moral-Benito and Servén (2014)) conceptually the analysis sits closest to the literature on the impact of news shocks on the macroeconomy. Within this realm, studies exploit high–frequency data and the randomness of major oil field discoveries to identify how commodity–linked expectations affect the economy (Känzig, 2021; Arezki, Ramey and Sheng, 2017). If we look beyond the energy sector, the same literature analyzes the role of technology and fiscal shocks in driving the economy (Beaudry and Portier, 2006; Leeper, Walker and Yang, 2013).

This paper differs from previous research along multiple dimensions. First, it proposes a new source of news shocks that lies in discrepancies between plans and implementation in infrastructure. This study is not the first to investigate the role

of news shocks on the macroeconomy. Yet it is – to my knowledge – the first to identify and exploit news events in the infrastructure sector.

Finally, while the majority of studies on infrastructure and the economy report a significant positive effect of infrastructure on output, productivity and long–term growth, I find that this finding might not apply once a) causal effects between growth and infrastructure are disentangled, b) developing country responses are isolated from OECD country effects, or c) projects were not previously announced in the energy plan.

The results of this paper hold important policy implications. Developing country governments are being asked to provide publicly available forward–looking plans which commit to generation capacity additions (World Bank, 2015). This paper highlights multiple unexpected and negative consequences of such energy plans if developing country governments deviate from them: the results of this chapter show that surprise power plants can reduce output temporarily, and disappointments can lead to permanent output declines.

In what follows, I first outline the features of power generation which lend themselves to the study of news shocks before detailing the data collection and news shock construction. Next I illustrate key patterns in the power plant database, and purge the news shock variable of potential confounding factors. I then proceed to the estimation of the short–run and long–run impacts of surprise and disappointment news shocks on key economic variables notably consumption rates, saving rates, investment rates, the current account and GDP. Next I interpret the results before testing for informational sufficiency of the VAR model and concluding.

1.2 Identifying infrastructure news shocks

Electricity infrastructure data possess characteristics that make them well-suited for studying the short and long run impact of anticipated sectoral shocks on the

economy for a number of reasons.

First, power infrastructure additions are widely interpreted as increases in the productive capacity of the economy. In emerging and developing countries electricity supply shortages have been shown to not only lead to output losses, but also negatively impact productivity. Drawing from partial equilibrium models of firm responses to deficient infrastructure supply (Allcott, Collard-Wexler and O'Connell, 2016), this negative TFP effect is caused by power outages via a shut–down and input-variation channel. This is supported by the recent revelation that infrastructure accounts for 30–60 percent of the effect of investment climate on firm productivity, ahead of corruption and other factors (Escribano, Guasch and Peña, 2008). In a general equilibrium framework, Hulten and Srinivasan (2006) show that growth of roads and electric generation capacity accounted for half of productivity growth in manufacturing between 1972 and 1992 in countries including India.

Second, the information flow on infrastructure news is governed by well–observed institutional and physical structures, which result in lags that can be exploited for the sake of isolating expectations-driven responses. Following their announcement to the public, power infrastructure projects are realized after a substantial time–to–build delay of on average 4-6 years, depending on the generation technology. A surprise power project therefore signals future productivity changes without affecting the fundamentals for a number of years, allowing short-term impulse responses to be interpreted as anticipatory in nature.

Thirdly, the nature of the infrastructure news dataset, and in particular the occurrence of failed projects, allows for the interpretation of disappointment shocks in addition to surprise shocks. This presents a rare opportunity to assess the symmetry of expectations-driven responses to a shock variable.

1.3 Data collection and news shock variable construction

Two key datasets allow for the identification of news events in the infrastructure sector. The first database I constructed by individually collecting the energy plan documents of 140 economies for each year spanning from 1995 through 2015. Energy plans containing proposed plant additions are published on the websites of the ministries of energy, the electricity regulatory agencies and/or other energy sector players at varying forward–looking horizons ranging from 3 year plans to 10 year plans and more. They contain information on the generation plants the government intends to either self-fund, or realize by means of private sector investments. I first obtained these energy plan documents through desk research, and then manually copied the quantitative data on the timing and capacity in MW of the proposed generation plant additions from each pdf into Excel. The second, proprietary dataset contains all realized electricity generation plants, where realized refers to the reaching of a financial close of the transaction underpinning the plant delivery. This dataset was obtained from the World Bank's Energy and Extractives division. It records the years of all documented generation plants' financial close as well as their capacity, making it an annual dataset.

These two datasets are compared to categorize a novel surprise and disappointment shock variable using data on unannounced new built (greenfield) or add–on (brownfield) power plants. The occurrence of electricity generation plants and capacity additions to existing plants is compared to the annual list of plants that had been envisioned by the government via its energy plan. A year in which a power plants or plant addition was realized but not listed in the relevant energy plan is categorized as an instance of a surprise for that plant. A year in which a power plant or addition was supposed to reach financial close but did not is categorized as an instance of a disappointment for that plant. Years in which the plan matches the occurrence of a plant mean that the plant was neither a surprise nor a disappointment – these instances do not feature in the final news shock

database that forms the analysis.

From these instances I obtain classifications of surprise and disappointment shocks as follows. For the surprise (disappointment) shock, the generation capacities of all surprise (disappointment) plants are added up for a given year, and then expressed as a ratio in the total capacity of the associated country in that year. As an additional step, the disappointment shock data require truncation: there are some plants that fail to be realized and have capacities which exceed 150 percent of existing system capacity. There are 39 projects (out of 113 failed generation projects) whose capacity exceed the existing generation capacity of the respective country. 35 of these are projects in OECD countries, and were based on long run capacity addition plans of over 10 years. To ensure the robustness of the VAR estimations, I truncate the disappointment shock measure (share in the total capacity) at 100 and 200 percent. The choice of truncation alters the savings response in OECD countries, and changes the magnitude of the output response (see Appendix 1). It also changes the long–term output response in emerging and developing countries, as well as the investment response in these economies.

I purposefully exclude plants that were eventually realized after a delay from the news shock dataset. This is because most energy plans span an interval of multiple years, making it impossible in the vast majority of cases to discern whether a project was truly delayed, and by how long. Figures 1.1 and 1.2 below depict examples of a surprise capacity addition and a disappointment.

In 2013 the Brazilian regulatory agency disclosed that a 3750 MW power plant construction had been agreed between foreign electric utilities (figure 1.1 panel b). This power plant was not listed in the 10–year "Plano Decanal de Expansao de Energia", which was a plan published by the Ministry of Energy spanning 2006 through 2015 (panel a). The capacity of this surprise addition is divided by the total capacity of the country at the end of the prior calendar year to yield the raw

surprise shock measure.

In 2011 it was reported that the 660 MW Bhavanupadur thermal power plant in Andhra Pradesh, India was on hold due to local strikes against its construction site (figure 1.2 panel b). The plant was listed in India's 5–year energy plan spanning 2010 through 2015 and there are no reports suggesting its completion (panel a).

1.4 Characterizing surprises and disappointments

The database captures a total of 1711 generation capacity additions spread across 44 countries, of which 21 are OECD economies. Across the power plant database, failed power plants are more than twice as common as surprises, in that more economies experience them (figure 1.3). A total of 162 planned power plants fail even before they are constructed, compared to 62 surprise generation projects. Yet, where projects fail they do so less frequently on a year–to–year basis. Unexpected power plants, on the other hand, are concentrated in a smaller number of countries in which they appear more often.

To shed light on the importance of these power generation projects for their host countries' electricity grids, figure 1.4 focuses on the sizes in megawatts of the power sector events captured in the database.

Failed power plants are markedly smaller than surprise capacity additions. While the vast majority of failed power plants are less than 200 MW large, more than half of the surprise power projects exceed the same threshold. The distribution also reveals that the largest power plants are those that failed.The existence of failed mega–projects, and the lack of surprise mega–projects is consistent with a higher positive visibility associated with mega power plants. If mega power plants are likely to be announced to improve a government's image, and are not primarily planned to serve future electricity demand, then this could lead to them being abandoned at times. This government-image hypothesis would also explain why surprise power

Figure 1.1: Illustration of power plant surprises

Aproveitamento	Rio	UF	Subsistema	Potência (MW)	Data	Classificação
Uruçuí	Parnaíba	PI/MA	NE	164	mar-12	Indicativa
Riacho Seco	São Francisco	BA/PE	NE	240	abr-12	Indicativa
Traíra II	Suaçuí Grande	MG	SE/CO/RO	60	mai-12	Indicativa
Água Limpa	Das Mortes	MT	SE/CO/RO	320	mai-12	Indicativa
Baixo Iguaçu	Iguaçu	PR	S	340	set-12	Indicativa
Porto Galeano	Sucuriú	MS	SE/CO/RO	139	nov-12	Indicativa
Pai Querê	Pelotas	RS/SC	S	291,9	nov-12	Indicativa
São Roque	Canoas	SC	S	214	nov-12	Indicativa
Belo Monte Complementar	Xingu	PA	N/Man	181,3	dez-12	Indicativa
Tocantins	Tocantins	TO	SE/CO/RO	480	dez-12	Indicativa
São Domingos	Verde	MS	SE/CO/RO	48	dez-12	Com Concessão
Serra Quebrada	Tocantins	TO/MA	N/Man	1.328	dez-12	Indicativa
Itapiranga	Uruguai	SC/RS	S	580	mar-13	Indicativa
Estreito Parn.	Parnaíba	PI/MA	NE	86	mar-13	Indicativa
Pedra Branca	São Francisco	BA/PE	NE	320	abr-13	Indicativa
Novo Acordo	Sono / Médio Tocantins	TO	SE/CO/RO	160	mai-13	Indicativa
Cachoeirão	Juruena	MT	SE/CO/RO	64	mai-13	Indicativa
Torixoréu	Araguaia	GO/MT	SE/CO/RO	408	mai-13	Indicativa
Tucano	Verde	GO	SE/CO/RO	157	mai-13	Indicativa
Tupiratins	Tocantins	TO	N/Man	619,8	nov-13	Indicativa
Cebolão	Tibagi	PR	S	152	dez-13	Indicativa
Volta Grande	Chopim	PR	S	54,7	dez-13	Indicativa
Belo Monte (1ª etapa)	Xingu	PA	BM	5.500	dez-13	Indicativa
Toricoejo	Das Mortes	MT	SE/CO/RO	76	jan-14	Indicativa
Castelhano	Parnaíba	PI/MA	NE	96	mar-14	Indicativa
Juruena	Juruena	MT	SE/CO/RO	46	mai-14	Indicativa
Jataizinho	Tibagi	PR	S	155	ago-15	Indicativa
Total				**31.144,5**		

(a) Brazil's 2006–2015 energy plan

GDF Suez, Mitsui partner to develop 3750MW hydro power plant in Brazil

By NS Energy Staff Writer 14 May 2013

NEWS

French multinational electric utility GDF Suez and Mitsui have agreed to partner for the construction and operation of 3750MW Jirau hydro power plant in Brazil.

As per the agreed terms, Mitsui will take a 20% stake in the project being constructed on the Madeira River in the State of Rondônia, for BRL5.7bn (€ 2.2bn).

The hydro power facility consisting of 50 turbines is expected to become completely operational in 2015.

About 73% of the total power generated at the hydro power plant will be sold to a group of distribution companies, under 30-year power purchase agreements. The remaining amount of power will be sold to the existing shareholders.

Commenting on the transaction, GDF SUEZ chairman and chief executive officer Gérard Mestrallet said that the Jirau hydro power plant is the company's major project in Brazil and strengthens the company's commitment in meeting the increasing energy demand in the country.

"It is part of GDF SUEZ strategy to partner with strong industrial companies to facilitate the completion of major developments. The commissioning of Jirau will demonstrate our ability to deliver important energy infrastructure in key markets," added Mestrallet.

A loan agreement on a project finance basis has been executed with Brazilian Development Bank and a group of commercial banks, while both the operational license and environmental license have already been granted.

The transaction will be completed during the second half of 2013, following the approvals from ANEEL – Electricity Energy Regulatory Agency and CADE – Brazilian anti-trust entity and lenders.

(b) Brazil's Surprise Addition News Clipping

Figure 1.2: Illustration of power plant disappointment

Sl. No.	PLANT NAME	FUEL TYPE	STATE	AGENCY	CAPACITY
24	India Bulls - Nasik TPP Ph-I, U4	Coal	MAHARASHTRA	India Bulls Realtech Limited	270
25	India Bulls - Nasik TPP Ph-I, U5	Coal	MAHARASHTRA	India Bulls Realtech Limited	270
26	Lanco Mahanadi, Vidarbha TPP U1	Coal	MAHARASHTRA	Lanco Mahanadi Power Pvt Ltd	660
27	Lanco Mahanadi, Vidarbha TPP U2	Coal	MAHARASHTRA	Lanco Mahanadi Power Pvt Ltd	660
28	D B Power TPP, Sidhi U-1	Coal	MADHYA PRADESH	DB Power (Madhya Pradesh) Ltd	660
29	Maheshwar HEP U1	Hydro	MADHYA PRADESH	SMHPCL	40
30	Maheshwar HEP U2	Hydro	MADHYA PRADESH	SMHPCL	40
31	Maheshwar HEP U3	Hydro	MADHYA PRADESH	SMHPCL	40
32	Maheshwar HEP U4	Hydro	MADHYA PRADESH	SMHPCL	40
33	Maheshwar HEP U5	Hydro	MADHYA PRADESH	SMHPCL	40
34	Maheshwar HEP U6	Hydro	MADHYA PRADESH	SMHPCL	40
35	Maheshwar HEP U7	Hydro	MADHYA PRADESH	SMHPCL	40
36	Maheshwar HEP U8	Hydro	MADHYA PRADESH	SMHPCL	40
37	Maheshwar HEP U9	Hydro	MADHYA PRADESH	SMHPCL	40
38	Maheshwar HEP U10	Hydro	MADHYA PRADESH	SMHPCL	40
39	Thamminapatnam TPP U3	Coal	ANDHRA PRADESH	Meenakshi Energy Pvt. Ltd.,	350
40	Thamminapatnam TPP U4	Coal	ANDHRA PRADESH	Meenakshi Energy Pvt. Ltd.,	350
41	Bhavanapaddu TPP U1	Coal	ANDHRA PRADESH	East Coast Energy	660
42	Bhavanapaddu TPP U2	Coal	ANDHRA PRADESH	East Coast Energy	660

(a) India's 2010 energy plan

MoEF again stops Bhavanapadu power plant's construction work

The central government has once again issued stop-work order against construction of 2,640 MW Bhavanapadu Thermal Power Plant of East Coast Energy in Srikakulam district of Andhra Pradesh.

PTI | November 7, 2011 | Updated 18:57 IST

The central government has once again issued stop-work order against construction of 2,640 MW Bhavanapadu Thermal Power Plant of East Coast Energy in Srikakulam district of Andhra Pradesh.

The Ministry of Environment and Forests (MoEF) in a order in October has told the state government that the project work would have to stop because it had received several serious complaints.

Only a month-and-a-half ago, the ministry had allowed East Coast Energy to resume the project work after a gap of six months.
Earlier in March, MoEF had ordered suspension of work, following a clash between protesters and police in which two protesters were killed and several others, including policemen, were injured.

Locals are opposing the project fearing pollution.

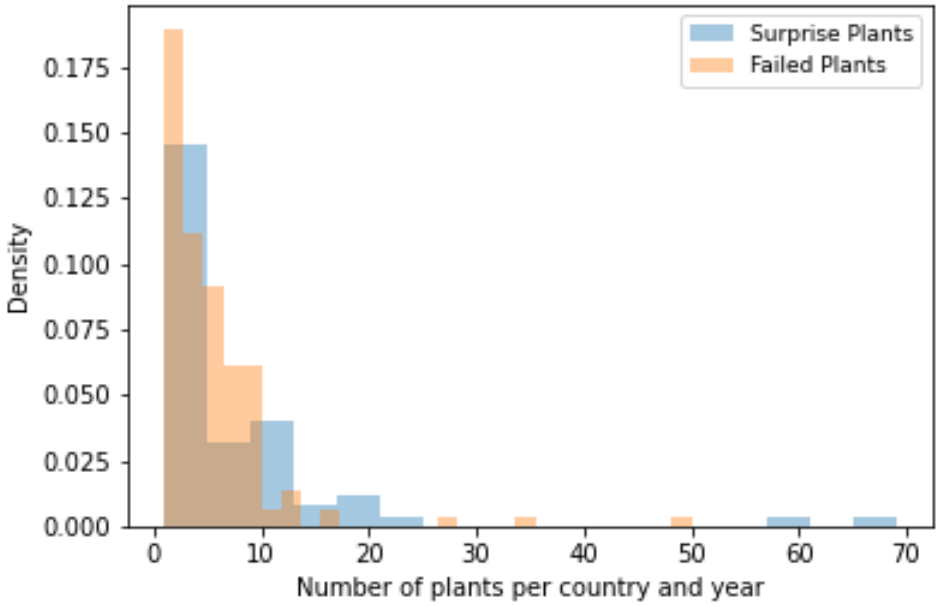
The fresh order says that "after issuance of withdrawal order dated September 22, fresh complaints of serious nature have been received. These complaints are under examination."

"Ongoing construction activity at the project site shall be stopped until further orders", the MoEF order said.

(b) India's Cancellation News Clipping

plants are rarely mega power plants: pre-announcing projects sufficiently ahead of their construction would lengthen the period during which the population looks forward to the addition.

Figure 1.3: Distribution of surprises and failures (1990-2015)

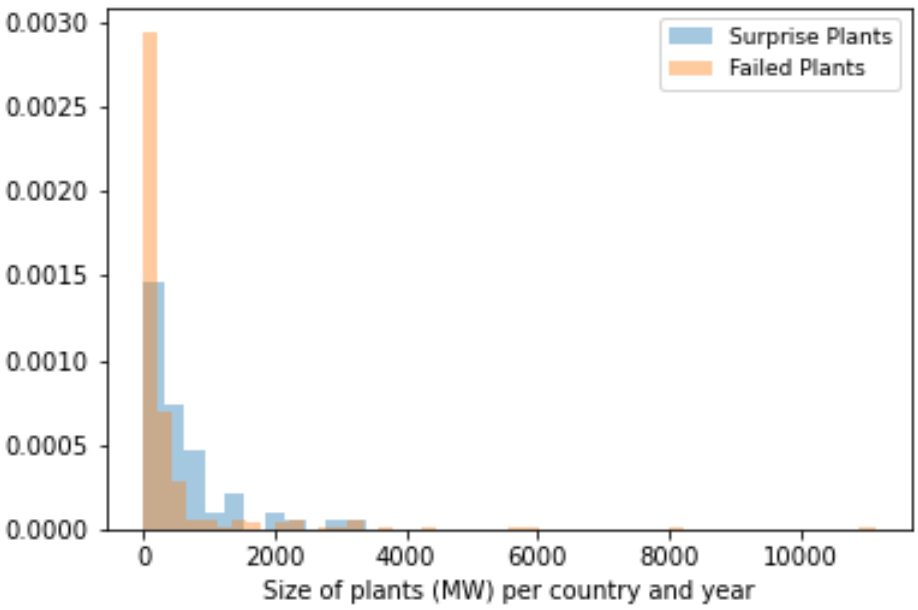


Notes: Figure plots the distribution of the number of surprise and failed power generation projects by year and country in the dataset. The unit of observation is a country–year pair. Each bar depicts the density, so that the area under each cluster of bars adds up to 1.

The relative predominance of failed plants is driven in particular by the time period after 2010 (figure 1.5). While the relative frequency of both unexpected and failed projects vis–a–vis all plants in the database declined starkly from 2010 onwards, this is disproportionately the case for surprise capacity additions.

The decline in the share of power plants that were either unannounced or failed coincides with a period during which emerging economies were implementing regulatory reforms at a higher rate than during the slowdown of the late 2000s (Foster et al, 2017). Accordingly, the number of power sectors regulated by independent entities increased from 60 percent in 2010 to over 70 percent in 2015, driven in large by reforms in Sub-Saharan Africa and the East Asia Pacific region. Independent regulatory entities are considered important mechanisms for shielding the electricity service providers and broader sector policy from "short–term opportunistic political

Figure 1.4: Distribution of the sizes of surprises and failures (1990-2015)

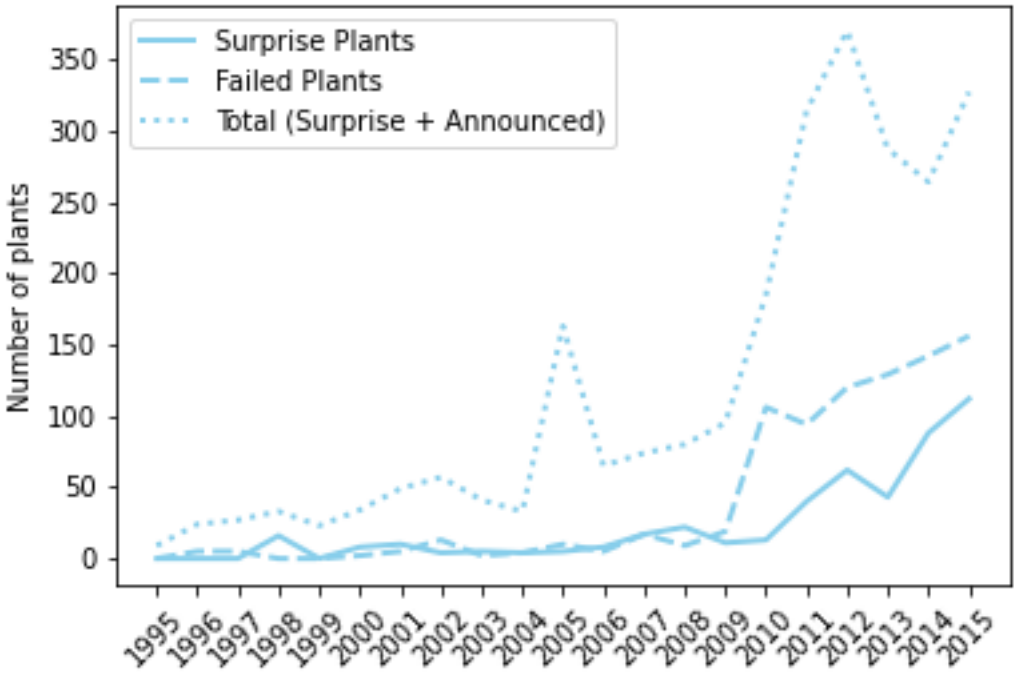


Notes: Figure plots the distribution of the size of surprise and failed power generation projects by year and country in the dataset.

interference" (Foster et al, 2017). An increase in autonomous regulation with less interference from the Ministry of Energy and therefore the government is consistent with the trend of relatively less frequent failed plants. If generation capacity additions are more likely to be decided on by a dedicated body driven by long–term strategic goals, then this improvement could lead to fewer politically driven power plant announcements that may ultimately fail.

The database is sufficiently rich to map out the landscape of planned and unplanned power plants on a country–by–country basis. One notable example is India, the emerging market's second largest electricity generator and one of the fastest growing consumers of power between 2008 and 2018 (figure 1.6) (BP, 2019). While surprise projects are far less frequent than pre–announced generation additions, they cover a similar range of capacities. Nevertheless, India's largest power plants are pre–announced.

Figure 1.5: Power generation plants over time (1990-2015)



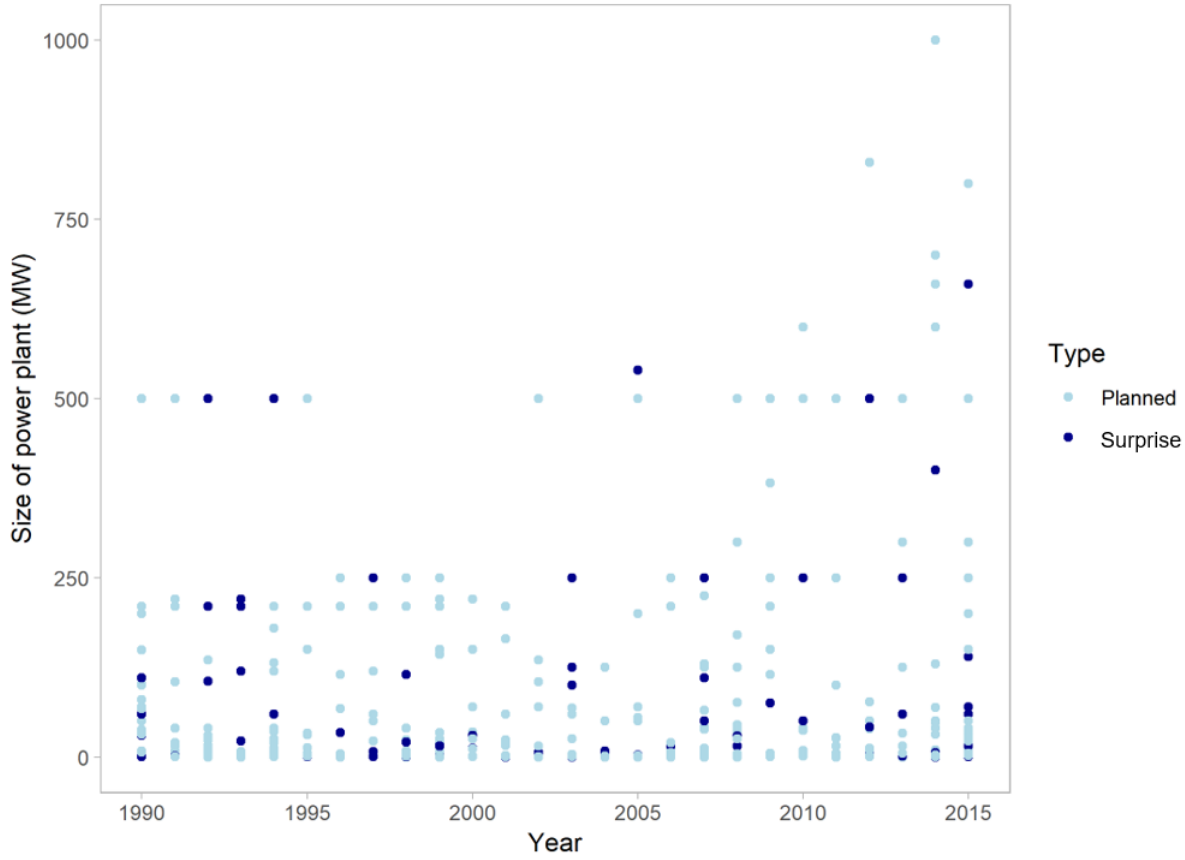
Notes: Figure plots the historical evolution (flow) of the number of surprise, failed and total power generation projects in the database by year.

1.5 Identifying news shocks via direct measurement of news events

The approach employed in this paper is akin in spirit to previous direct approaches which identify news shocks via plausibly exogenous news events (see Mertens (2012) and Luca (2012) among other seminal works employing this method). This method relies on deciphering information contained in public policy events such as tax legislation or forecast revisions in order to extract direct news shock observables. In my case I measure news shocks directly by extracting data on deviations of power projects from forward–looking energy plans.

A central requirement needs to be met for this method to yield insights on business cycle responses to news shocks. This is that the generation capacity additions are unrelated to other changes in the economy that could also drive the

Figure 1.6: (Un–) Planned power plants in India (1990–2015)



Notes: Figure plots the annual number and sizes (in MW) of planned and surprise power generation projects in India, by year of commissioning.

output-responses that I study in this chapter. (Romer and Romer, 2010)'s approach avoids using the raw measure of legislative tax changes to capture the fiscal shock to the economy, as for example a tax increase could be motivated by foreshadowing a change in output, rather than merely causing it. The result is a measure of fiscal policy that is filtered by its motivation to be as exogenous as possible.

I filter the raw measure of generation additions by measuring action as a deviation from the plan. By focusing on unexpected changes in electricity generation capacity, I minimize the potential endogeneity of the independent variable. Capacity additions in energy plans directly reflect policy-makers' forecast of future power demand, and through this future economic output. When projects deviate from plans

in the database, however, a scan of the corresponding news publications reveals that the more common reasons for surprises are breakthroughs in public-private partnerships that make projects more likely to reach financial close. Similarly, for the disappointments in the database, commonly cited reasons identified are force majeure clauses due to government contract breaches and environmental concerns over displacements and environmental license revocations, particularly in the case of hydro-power projects. These reasons are not directly linked to future economic prospects, supporting the notion that deviations from plans can be considered exogenous.

Beyond exploiting exogenous deviations, in order to estimate the impact of news shocks on the economy I take additional steps to alleviate the possibility of confounding variables that may be driving the news shock measure as well as macro-aggregates. In particular it is conceivable that governments commission unannounced power projects as a result of forecast errors, such as when electricity demand forecasts are revised upwards. Without accounting for such factors, my impulse responses will not capture the intended effect of changes in agents' expectations. Instead they will reflect the impact of increased future electricity demand on current project occurrences and current productivity.

Hence I construct the final news shock measure to be used in the estimation from the residuals of the following estimation:

$$shock_{it} = c + A(L)shock_{it} + \sum_{h=0}^{H} \beta_h^1 E_t[X_{i,t+h}^1] + \sum_{j=0}^{J} \beta_j^2 E_t[X_{i,t-j}^2] + \alpha_i + \epsilon_{it}, \quad (1.1)$$

where $shock_{it}$ contains the measure for the surprise (or disappointment) shock, $X_{i,t+h}^1$ contains forecasts of GDP growth, CPI growth and the current account change 1 year ahead while $X_{i,t-j}^2$ contains historical values of GDP, the savings share in output, the investment share in output and the current account share in output. As a reminder, any power plants or plant additions that were realized yet not listed in

Table 1.1: Surprise and Disappointment Shock Descriptives

	Surprise Shock				**Disappointment Shock**			
	Mean	**Sd**	**Min**	**Max**	**Mean**	**Sd**	**Min**	**Max**
Emerging	0.03	0.12	0.00	0.99	0.03	0.12	0.00	0.70
OECD	0.02	0.06	0.00	0.34	0.09	0.22	0.00	0.93

Note: Table reports descriptive statistics for surprise and disappointment shocks. A surprise shock is the share of the capacity (MW) of an unannounced power project in the existing power capacity in OECD countries. A disappointment shock is the share of the capacity (MW) of an announced power project that was never completed in cumulative existing power capacity in OECD countries.

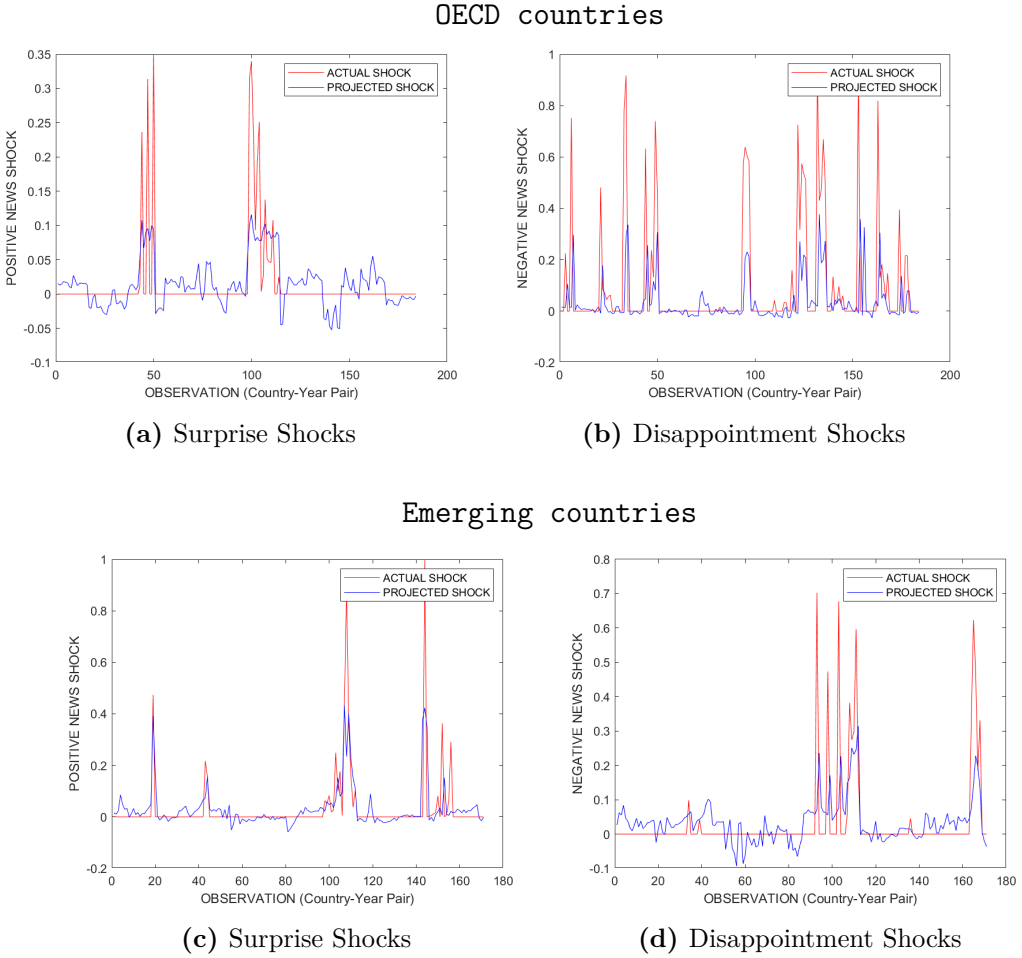
the relevant energy plan are categorized as surprise shocks. Disappointment shocks are identified as electricity generation projects that were listed in an energy plan, but were never constructed. The shock measure is obtained as the ratio of the capacity addition in the existing capacity of the given country. Details on the construction of the macro–variables are located in the appendix. Based on information criteria the following lag selection is made: $h = 5$, $j = 2$ and $A(L) = A_1 shock_{it-1}$. Country fixed effects α_i are included.

Descriptive statistics for the raw news shock measures are presented in Table 1.1. In developing and emerging economies surprise shocks are on average larger than corresponding shocks in industrialized countries, taking up a share in capacity that is three times larger on average. Disappointment shocks on the other hand appear to be smaller in magnitude in emerging economies, and on average constitute about a third of the existing electricity system compared to their OECD country counterparts.

There are marked differences between OECD and developing countries in the size and forecastability of disappointment shocks (Figures 1.7 and 1.8). In the latter country grouping the raw shocks are to a large extent related to past performance of the economy and forecasts of macrovariables a year as well as five years ahead. In OECD countries however, infrastructure surprises and disappointments are

substantially harder to predict using historical and forecasted measures of the business cycle, implying more complex processes.

Figure 1.7: Computed News Shocks in OECD and Emerging Countries



(a) Surprise Shocks **(b)** Disappointment Shocks

(c) Surprise Shocks **(d)** Disappointment Shocks

Notes: Figure depicts the fitted and actual surprise and disappointment raw shock measure in OECD countries (top row) and emerging economies (bottom row). A surprise shock is the share of the capacity (MW) of an unannounced power project in cumulative existing power capacity. A disappointment shock is the share of the capacity (MW) of an announced power project that was never completed in cumulative existing power capacity.

1.6 Expectations–driven responses to infrastructure news shocks

The resulting news shock measure is employed in a six variable VAR(1) specified in levels. The model is estimated on two pooled samples. The OECD sample contains data on 357 OECD country–year pairs, while the developing country

sample includes 224 country–year pairs. The Cholesky scheme is employed for the sake of identifying the system: the shocked news shock variable is ordered ahead of second news shock, the savings share, the investment share, the current account share and real GDP. The intuition is that while the shocked news shock is treated as exogenous by this scheme, the savings share responds to the news shock, investment responds to the news shock and savings, the current account responds to all variables but real GDP, and real output responds to all variables in the model. The surprise news shock is ordered ahead of the disappointment shock. This choice reflects the possibility that unannounced or expedited projects could lead to failures down the line due to insufficient planning. It is possible that disappointments lead to surprise additions by the government as a measure to compensate firms and households for project cancellations. I treat this scenario as less likely since compensation plants are unlikely to be organized at such short notice. Overall, the Pearson correlation coefficient (calculated as the covariance between the shocks, divided by the product of their standard deviations) between surprise and disappointment shocks in developing and OECD economies is 0.11 and 0.02 respectively, suggesting that reordering the shocks vis–a–vis one another should have relatively little impact on the following results. The VAR representation and system coefficients are summarized in section 2 of the appendix.

Short–term responses to surprise shocks are markedly different among industrialized and developing countries (table 1.2). In the short run, in developed countries a surprise infrastructure news shock triggers a private consumption rate increase and conversely savings rate reduction (figure 1.9). The investment share decreases, which corresponds to an improvement in the current account share. Real GDP rises immediately following unanticipated news on future infrastructure projects, and stays elevated throughout the 20 years following the announcement.

In emerging and developing countries, surprises to future infrastructure additions also prompts households to increase their consumption rates in anticipation of future

Table 1.2: Peak Impulse Responses (in percent) to News Shocks

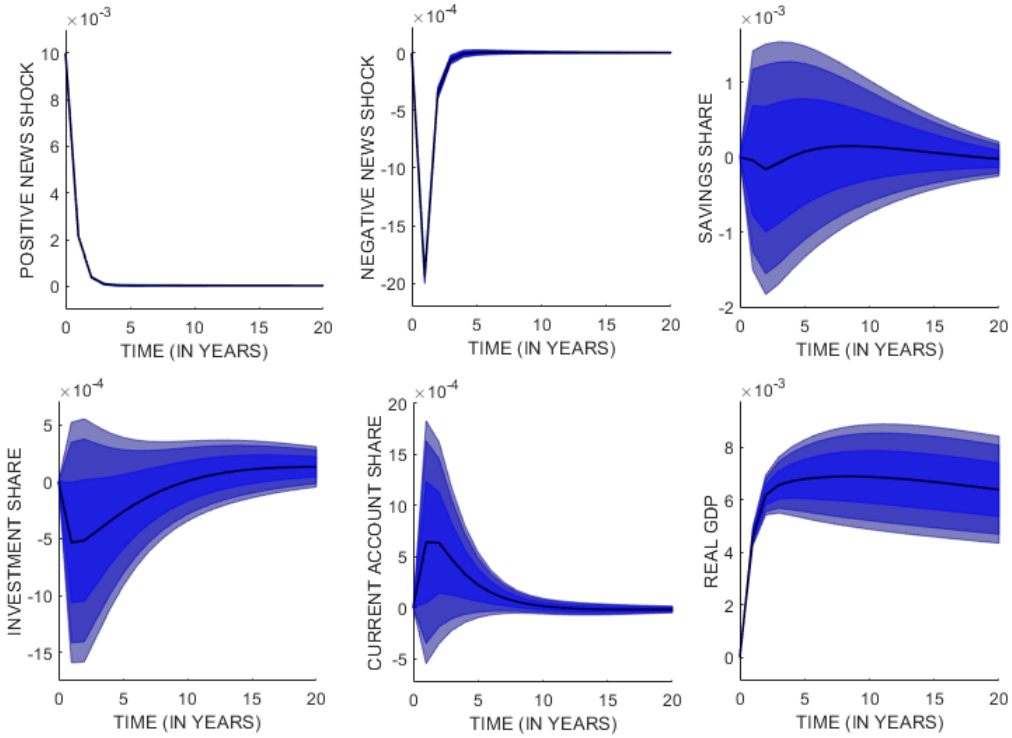
Shock	Country Group	S	I	CA	GDP
Surprise	OECD	-0.015	-0.051	0.063	0.071^{+}
	Developing	-0.012^{+}	0.015	0.033	-0.012^{+}
Disappointment	OECD	-0.045^{+}	-0.059^{+}	$+0.030^{+}$	0.075
	Developing	0.123	-0.015^{+}	-0.003	-0.225^{+}

Note: Table reports impulse responses in percent of the macro–aggregates (S,I,CA,GDP) to 1 percent surprise and disappointment news shocks, at their peak impulse response. + denotes responses that persist at a 20–year horizon.

increases in the economy's productive capacity (figure 1.10). The savings rate falls in response to a surprise infrastructure project. Investments rise sharply, and the current account deteriorates. Unlike OECD countries, where output increases in anticipation of the capacity addition, real GDP contracts. This negative developing country response is at odds with theories of intertemporal consumption adjustments, whereby output increases to smooth the transition from the anticipation period to the actual capacity addition. Surprise projects appear to be "bad new" in the eyes of economic agents. This output–reducing reaction could be explained by the nature of surprise projects in non–OECD countries. Instances of corruption in power generation have been identified as common in a number of key developing countries (Foster et al., 2017). Hence, if planned power projects are published in energy plans that are overseen by an independent regulatory agency, then surprise power projects lack the equivalent transparency and strategic oversight by an autonomous authority. Higher perceived levels of corruption in emerging economies could then imply that unannounced projects are viewed as non–productive and associated with high opportunity costs.

Expectations–driven responses to surprises and disappointments are strikingly asymmetric in OECD and developing economies (figures 1.11 and 1.12). In the short–term, in developed economies disappointment news shocks elicit anticipatory consumption increases, investment declines and an expectations–driven real output rise. In non–OECD economies on the other hand unrealized electricity generation

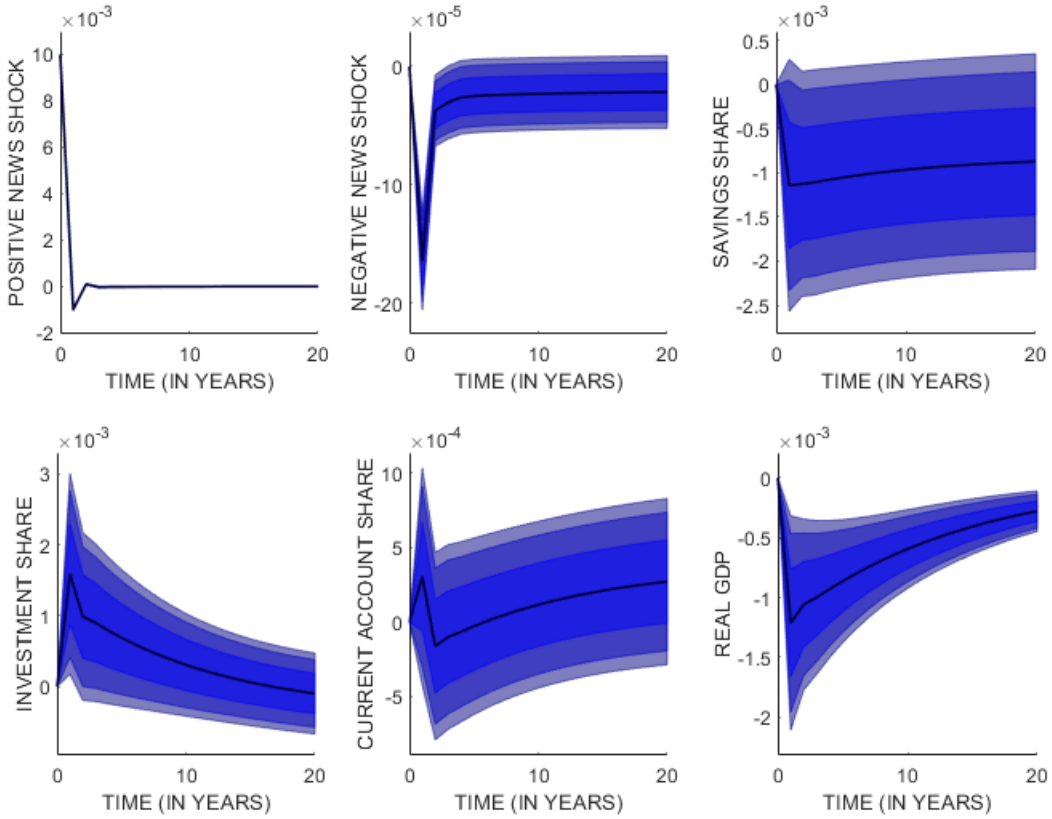
Figure 1.8: Impulse Responses in OECD Countries to a Surprise Shock



Notes: Figure depicts impulse responses of the macrovariables (S,I,CA,GDP) to a 1 percent surprise shock. The shocks are identified via Cholesky restrictions on the contemporaneous relationships between variables. The VAR(2) is estimated in levels (except for GDP) with 1 lag. The unit of the vertical axis is in decimal deviations from the pre–shock levels. The blue intervals correspond to the 30, 50 and 60 percent bootstrapped confidence bands. The sample consists of a pooled dataset including 21 OECD countries spanning 357 country–year pairs.

projects lead to an output decline before the project is realized. The economic expansion in response to failed projects in developed economies stands in contrasts to the contraction in non–OECD economies. This difference between both country groupings is in line with higher levels of transparency that could also explain the positive output effect of a surprise projects revealed above: when electricity projects are cancelled in OECD countries, firms and households could perceive this as "good news", if they expect that the capacity addition was abandoned for the right reasons. In contrast, where perceived levels of corruption are higher, failed projects – which happen to also be the largest power plants in the dataset – could reveal to economic agents that the capacity addition was not a priori driven by economic considerations.

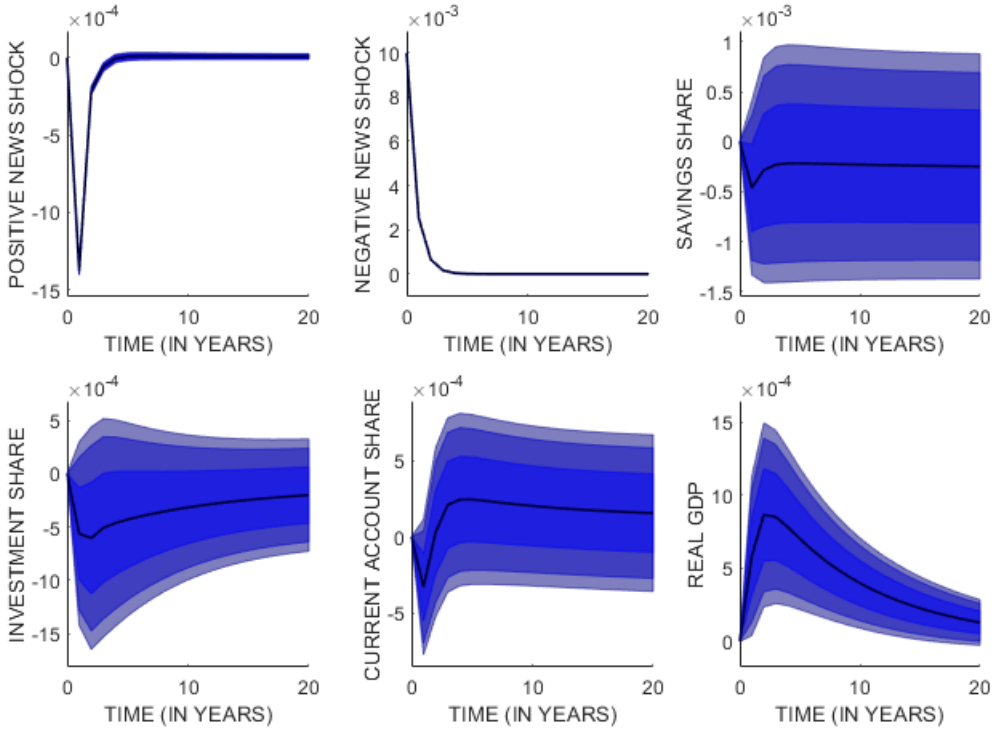
Figure 1.9: Impulse Responses in Emerging Countries to a Surprise Shock



Notes: Figure depicts impulse responses of the macrovariables (S,I,CA,GDP) to a 1 percent surprise shock. The shocks are identified via Cholesky restrictions on the contemporaneous relationships between variables. The VAR(2) is estimated in levels (except for GDP) with 1 lag. The unit of the vertical axis is in decimal deviations from the pre–shock levels. The blue intervals correspond to the 30, 50 and 60 percent bootstrapped confidence bands. The sample consists of a pooled dataset including 16 emerging countries spanning 224 country–year pairs.

Overall, good news in infrastructure leads to an expectations–driven economic expansion in OECD countries, but appears to be perceived as "bad news" in developing economies. Disappointments affect the lower and higher income world asymmetrically. While disappointments lead to short–term output increases in developed economies, they are associated with an expectations–driven contraction in non–OECD countries.

Figure 1.10: Impulse Responses in OECD Countries to a Disappointment News Shock

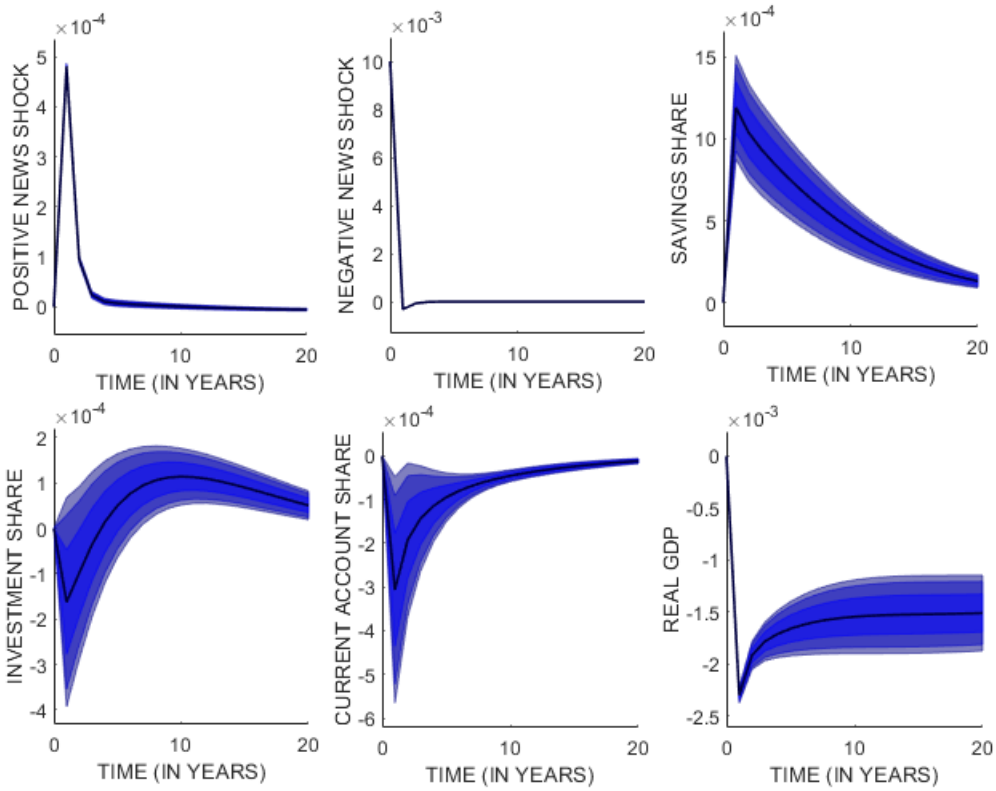


Notes: Figure depicts impulse responses of the macrovariables (S,I,CA,GDP) to a 1 percent disappointment shock. The shocks are identified via Cholesky restrictions on the contemporaneous relationships between variables. The VAR(1) is estimated in levels (except for GDP) with 1 lag. The unit of the vertical axis is in decimal deviations from the pre–shock levels. The blue intervals correspond to the 30, 50 and 60 percent bootstrapped confidence bands. The sample consists of a pooled dataset including 21 OECD countries spanning 357 country–year pairs.

1.7 Long–term growth effects of infrastructure news shocks

We have thus far been concerned with an economy's short–term expectations–driven response to news about impending additions to the stock of productive infrastructure assets. The plausibly exogenous news shock measure employed in the preceding analysis also lends itself to studying the long run growth dividends of infrastructure projects, thus shedding light on the relationship between infrastructure and economic growth. The long run horizons of the impulse responses for OECD countries reveal that the output expansion due to a surprise infrastructure shock persists long after the project is materialized (Table 1.3). 20 years following news about an

Figure 1.11: Impulse Responses in Emerging Countries to a Disappointment News Shock



Notes: Figure depicts impulse responses of the macrovariables (S,I,CA,GDP) to a 1 percent disappointment shock. The shocks are identified via Cholesky restrictions on the contemporaneous relationships between variables. The VAR(2) is estimated in levels (except for GDP) with 1 lag. The unit of the vertical axis is in decimal deviations from the pre–shock levels. The blue intervals correspond to the 30, 50 and 60 percent bootstrapped confidence bands. The sample consists of a pooled dataset including 16 emerging countries spanning 224 country–year pairs.

infrastructure project that ends up materializing output remains 0.065 percent above its pre–shock level. This evidence is in line with the notion that realized infrastructure projects contribute to the long–term productive capacity of the economy in developed countries.

The size and dynamics of emerging and developing country responses in the long run are markedly different. The output contraction experienced by these economies as a result of surprise projects power project begins to revert to pre–shock levels before the project is constructed, but does not reach its pre–shock levels even 20

years following the project. The lack of an output increase following a surprise capacity addition in emerging economies is consistent with the hypothesis that unannounced power plants are less likely to be productive assets to the economy due to the lack of transparency associated with them. The long–term output contraction provides evidence that agents' distrust of unannounced projects is warranted. Taking into account bootstrapped confidence error bands, this long run output gap in the developing world is statistically different from 0. Hence, in non–OECD economies, the addition of an unannounced power generation project tends to fall short of contributing positively to economic output in the long run.

Turning towards disappointment news shocks, the results show that such disappointments elicit long run output increases in OECD economies relative to the pre–shock level: the real GDP increase is statistically significant up to year 15 following the cancellation of the project. Why do cancelled projects contribute to economic growth in OECD countries even long after news of the non–realization has spread throughout the economy? This is particularly counter–intuitive given that power plant cancellations tend to involve job losses associated with plant construction and the wider supply chain. One possible explanation is that power plant failures give rise to more economic projects, with larger output effects than the originally envisioned generation capacity addition.

Strikingly, developing and emerging economies experience a lasting economic contraction due to cancelled projects, which remains significantly different until 20 years following the disappointment. GDP growth remains 0.15 percentage points below its pre–shock level. This long–term growth–hampering impact could arise from the investment channel. In particular, additional investments in non–OECD power sectors could be deterred by previous failures (thus contributing to the output contraction), if a cancelled project signals rising uncertainty over the return on such power sector investments. This dynamic is akin to a real options–type effect, whereby increasing rising uncertainty increases the value of awaiting additional information

Table 1.3: long run Infrastructure – Growth Dynamics

Real GDP Responses to Surprise Shocks				
Shock Identification	**Country Group**	$t+5$	$t+10$	$t+20$
Cholesky	OECD	0.711	0.712	0.655
	Developing	-0.091	-0.069	-0.043
Real GDP Responses to Disappointment Shocks				
Shock Identification	**Country Group**	$t+5$	$t+10$	$t+20$
Cholesky	OECD	0.081	0.039	0.025
	Developing	-0.176	-0.159	-0.158

Note: Table reports the impulse response (in percent) of real GDP to a 1 percent surprise shock and disappointment shock 5, 10 and 20 years following the shock. The shocks are identified via Cholesky restrictions on the contemporaneous relationships between variables. The VAR(2) is estimated in levels (except for GDP) with 1 lag. The sample consists of a pooled dataset including 16 emerging countries spanning 224 country pairs and 21 OECD countries spanning 357 country–year pairs.

about the profitability of building power plants. The real–options channels is plausible given the significant sunk costs that investing energy companies have to endure when generation projects fail – £ 2.1 bn in the case of a failed project written off by energy company Hitachi in 2019. Overall, the lasting damage to economic growth revealed by the analysis implies that unrealized electricity infrastructure may have a larger real growth–inhibiting effect in lower–income countries than suggested by an expectations–driven framework.

1.8 Interpretation of results

Three key results emerge. First, while there is an immediate expectations–driven GDP response to an unexpected increase in the stock of power infrastructure assets in OECD economies, this positive output effect is absent in developing and emerging countries. The growth–hampering impact of surprise generation plant additions in these countries is consistent with the notion that unplanned plants are not perceived as productive assets in low–credibility environments.

Second, in the long run non–OECD economies are no better off from an economic growth perspective than they were before the surprise power plant was announced or constructed. What are the factors that could prevent unexpected additions to the physical stock of infrastructure assets from yielding sustained economic growth in developing and emerging economies? The electric utility literature suggests that the provision of physical electricity infrastructure assets can be considered a necessary but non–sufficient requirement for long–term economic growth, and stresses the importance of infrastructure *services*.

These electricity services face challenges that have been found to be specific to developing countries. First, under–investment in the maintenance of *existing* generation, transmission and distribution infrastructure has been identified as the main cause for two types of electricity losses in developing countries, namely outage losses and technical transmission and distribution losses (McRae, 2015). Further, insufficient metering technology has been identified as the main driver of electricity theft and under–collection of bills, exacerbating a lack of cost–recovery and under–investment (Burgess et al., 2019). If these service quality deficiencies are more pre–dominant in unannounced plants – for example because these surprise plants are rushed throughout without sufficient planning, then this could explain why unexpected infrastructure additions fail to sustain increased economic output in the long run in developing countries.

The second key result reveals that disappointments in infrastructure provision are costly and harbour larger and longer–lasting investment and GDP–hampering effects in the developing world than implied by the existing literature. It is often posited that firms and households ascribe limited credibility to government plans when there exists i) a history of widespread reversals in power sector reform and policies, and ii) a lack of regulatory independence (Kessides, 2013). However, the results show that private agents in developing countries appear to not only respond to such deviations from energy plans by immediately reducing investments,

but are also subjected to lower economic growth as a result relative to the pre–disappointment period in the long run.

One possible explanation for this effect was briefly touched upon and is anchored in real options theory, which seeks to explain how firms make irreversible investment decisions in uncertain environments (Dixit and Pindyck, 1994*a*). Its central prediction posits that an increase in uncertainty, by raising the value of awaiting further information on the rate of return on an investment, increases the profitability threshold required to make these investments worthwhile. Since sunk cost investments are commonplace in the infrastructure sector, deviations from energy plans could represent an increase in uncertainty urging firms to defer investments as they update their beliefs about energy plans. These deferred investments can in part explain the suppressed long run growth trajectory in developing countries depicted in the analysis. Strikingly, in the short run OECD firms and households appear to respond to supposed disappointments similarly to "good news". A plausible explanation is that when power projects are abandoned in these countries, that this occurs for the right reasons. Possible reasons are higher levels of trust in the energy regulatory agencies, which tend to be more independent of the Ministry of Energy than in developing countries.

The negative output effect of unexpected infrastructure projects in non–OECD countries is also consistent with existing evidence on the political economy of public investments. First, developing country governments have been shown to manipulate the level of public investments in infrastructure depending on 1) how important the affected group is to their re–election success, and 2) how far they are from an election year (Gupta, Lui and Mulas-Granados, 2015). 2) is particularly relevant:the main findings is that non–OECD countries experience substantially higher degrees of manipulation than their OECD counterparts, and that the level of public investment co-moves consistently with election cycles. The study also finds that such manipulations do not hold for the investment projects that take the longest, which suggests a motivation by incumbent politicians to reap the benefits

of the manipulation during their term in office. Given that pre–announced energy plans are largely influenced by expected future economic conditions, then household and firms could interpret deviations from the plan as a sign of such manipulative governance and politicians' tendency to place personal interests over public interests. Such an interpretation would be qualitatively consistent with the negative impact on output observed in the data, as economic agents brace for economic mismanagement (by relocating, protesting or sabotaging the incumbent that they have now learnt to distrust). The negative output effect of unanticipated power plants could also be explained by subnational competition between cities. Morris and Hough (1987) show that cities and states – when competing for national funds – are incentivized to understate the cost and overstate the benefits of their proposed infrastructure projects, leading to higher failure rates. If unanticipated power plants are more likely to originate in cities rather than on a national level in developing countries, and if the public is aware of the risk of "overdressing" proposed projects, then this could explain the lack of a positive GDP effect of surprises found in my estimation. The lack of data on this variable prevents me from exploring this hypothesis further.

1.9 Informational sufficiency of VAR model

An established problem affecting news VARs occurs when attempting to interpret impulse responses to news shocks as the economy's reaction to structural shocks in the presence of foresight. If agents receive signals about future infrastructure additions other than current and past values of the infrastructure news variable measured by the econometrician, or if structural shocks affect the economy with a delay, then the information set employed in the VAR will be misaligned with that of economic agents. This issue is endemic to the news shock literature (Leeper, Walker and Yang, 2013).

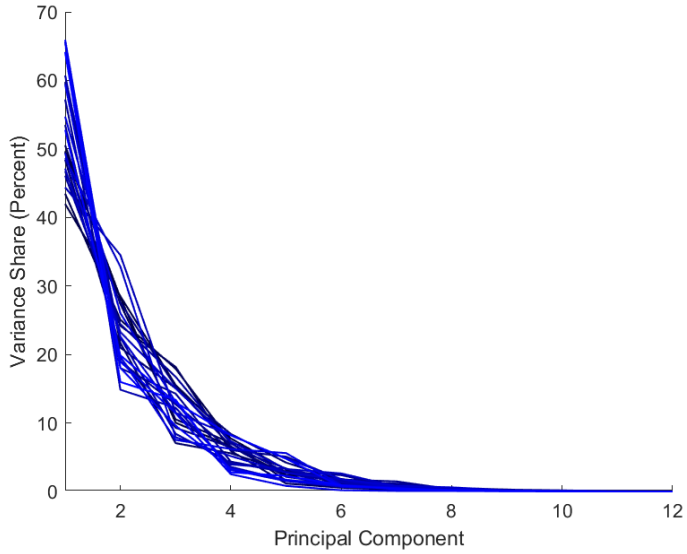
Foresight is one of the reasons for non–fundamentalness, i.e. when the VAR residuals become useless for recovering the structural shocks from the MA–representation of interest (Hansen and Sargent, 1991; Lippi and Reichlin, 1993, 1994; Chari, Kehoe

and McGrattan, 2008). In this environment different choices of measurement of the news shock will at best produce varying inferences. At worst, under the presence of foresight, estimating the impact of news shocks without further adjustments yields biased impulse responses and spurious interpretations.

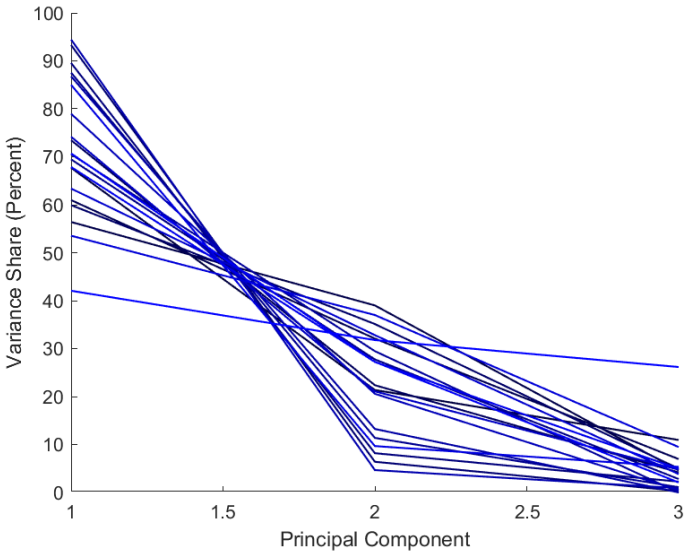
Studies have attempted to tackle the problem of non–fundamentalness in news by means of narrative methods (Romer and Romer, 2010), creative additional restrictions (Barsky and Sims, 2011) and explicit modelling of fiscal foresight (Schmitt-Grohé and Uribe, 2012). One diagnostic solution initially proposed by Forni and Gambetti (2014) involves testing for whether the variables included in the VAR model contain enough information to estimate the structural shocks. This test for informational sufficiency proceeds by first estimating the identified shocks as done previously. One then obtains the principal components of a large–scale VAR dataset including all available macro–aggregates. The large–scale dataset covering all countries in the sample spans 12 variables from the unemployment over government revenue to the output gap. The test then requires testing for whether lags of the first F_1 principal components of these variables are orthogonal to the first F_2 components of the variables used in the VAR(2) model. If this is the case, the VAR system is considered to contain sufficient information for the identification of the 6 structural shocks for the news measures, savings share, investment share, current account share and real GDP.

Figure 1.13 depicts the share of the sample variance explained by each principal component, for the large–scale dataset (a) and the VAR dataset (b). Each line tracks a different country. The first 3 components of the large–scale dataset embed 90 percent of the sample variance on average. Based on this evidence F_1 is set to 3. Most of the variance of the VAR sample is explained by 2 components on average, hence $F = 2$ is selected. Results are reported in table 1.4 and reveal it is unlikely that the VAR lacks sufficient information to be used for global identification, so for identification of all 6 structural shocks.

Figure 1.12: Variance Shares Explained by Principal Components



(a) 12–variable macro–dataset



(b) 5–variable VAR dataset

Notes: Figure depicts the share (in percent) of the total variance of the 12–variable large macro–dataset and 6–variable VAR dataset explained by each of their 12 (3) principal components. Each line denotes a separate country. The first 3 principal components of the large are used to test if the VAR dataset is sufficiently rich.

1.10 Conclusion

This paper has implemented a narrative approach to the identification of news shocks and applied it to a sector that is estimated to require investments in the order

Table 1.4: Test for Informational Sufficiency

Specification	Mean Wald Stat	Z-Stat	p-val
1 on 1	0.892	-0.241	0.809
1 on 2	2.605	0.639	0.529
2 on 1	3.079	0.948	0.343
2 on 2	1.979	0.209	0.834
3 on 1	2.522	0.574	0.566
3 on 2	1.979	0.369	0.211

Note: Table reports the results of a Granger–causality test for informational suffiency results for each of the 6 specifications. 3 principal components of the large–scale macrodataset are tested against 2 principal components of the VAR dataset. Test is conducted on a panel data level following the group mean Wald Statistic procedure by Dumitrescu and Hurlin (2012). Individual Wald Statistics are obtained in country–level regressions and next averaged across units. One lag is included. The null hypothesis is the absence of Granger causality in any of the countries studied, which is tested against the hypothesis of Granger causality for at least one of the economies.

of USD 28 trillion over the next 20 years (Heathcote and Mulheirn, 2020). While there is an immediate expectations–driven GDP response to an unexpected increase in the stock of power infrastructure assets in OECD economies, this positive output effect is absent in developing and emerging countries. In the long run non–OECD economies are no better off from an economic growth perspective than they were before a surprise power plant was announced or constructed. Finally, firms and households in developing countries appear to not only respond to failed power projects by immediately reducing investments, but are also subjected to lower economic growth as a result relative to the pre–disappointment period.

The findings call for attention to i) the importance of infrastructure services in affecting the productivity of capacity additions, ii) the impact of information regimes on the value added from infrastructure stock additions in developing countries and iii) the potentially negative investment–impacts of power sector disappointments.

The empirical results reveal that infrastructure capacity additions do not always contribute to long–term economic growth and can even detract from growth in the short–term, if they are unexpected. These findings support the view that the provision of physical electricity infrastructure assets can be considered a necessary

but non–sufficient requirement for more and cheaper electricity as a factor input into production, and ultimately economic growth. Sufficient conditions for sustained contributions to output remain improvements in infrastructure *services*, which will need to come from investments in maintenance as well as grid–system optimization. Further, deviations from energy plans through failed projects could represent an increase in uncertainty causing developing country firms to defer investments as they update their beliefs about energy plans. In the data, private agents in developing countries appear to not only respond to such disappointments by immediately reducing investments, but are also subjected to lower economic growth as a result in the long run.

Infrastructure surprises appear to serve as useful signals to firms and households, who extract information about the productivity of future power generation assets from the announcement. How infrastructure additions are communicated to the public thus becomes an economic policy variable available to policy–makers to manage in addition to conventional electricity sector strategies. This role of information regimes has not been the focus of the existing literature.

Where projects frequently fail, the provision of energy plans in emerging economies carries with it significant risks that can lead to adverse investment and output impacts. Hence, while the provision of energy plans is heralded as a good practice objective of energy sector reforms, this paper calls for careful examination of the potential economic impacts of committing to infrastructure capacity additions in low–credibility environments.

2

The Effects of Uncertainty on Lumpy Firm Investments: Evidence from Electricity Provision in Brazil

Abstract

How do firms make decisions on irreversible investments in response to uncertainty events? Such investments are commonplace in the power sector, where electricity prices are largely designed to regulate natural monopolies and encourage sufficient improvements in electricity reliability. In reality, high electricity prices and deficient infrastructure quality in the form of frequent power outages coexist in many developing countries. This paper provides one possible explanation for this phenomenon which is rooted in real options theory: a policy–induced improvement in uncertainty over future electricity prices lowers firms' option value of awaiting additional information, and prompts them to bring forward irreversible sunk–cost investments in electricity quality. Using Brazilian power sector data, I exploit a pseudo–experiment set–up triggered by the 2013 drought in order to tackle multiple empirical challenges faced by the existing literature. I interpret the introduction of a tariff flag mechanism as an improvement in the predictability of future retails electricity prices by power providers and consequently a reduction in uncertainty surrounding firm profitability. Employing a difference–in–difference specification, I find that firms exposed to the policy responded by significantly increasing their irreversible investments in the distribution grid: sunk cost investments in the form of substation replacements and constructions increase for these power providers relative to unexposed firms and to the pre–policy era. Importantly, firms do not ramp up marginal maintenance expenditures. This lumpy investment response is qualitatively consistent with optimal behaviour as prescribed by real options theory. I perform a number of tests and provide qualitative evidence to strengthen my case for the validity of the results, and the plausible exogeneity of treatment assignment in a diff–in–diff setting. These include parametric and non–parametric methods for matching treatment and control groups on observables, structural break tests as well as standard error adjustments for serial correlation of the residuals. The impact of uncertainty has to my knowledge not previously been empirically documented as a potential cause for deficient infrastructure quality in power distribution.

2.1 Introduction

Real options theory seeks to explain how firms make irreversible investment decisions in uncertain environments (Bernanke, 1983; McDonald and Siegel, 1986; Dixit and Pindyck, 1994*b*). Its central prediction states that an increase in uncertainty, in raising the value of awaiting further information on the rate of return on an investment, increases the profitability threshold required to make investments worthwhile. The electricity distribution sector lends itself to the study of firm behaviour in this context due to the single–agent nature of firms acting as natural monopolists with non–overlapping service areas. Lumpy investments for such firms consist of large infrastructure replacements in the distribution grid, which directly affect the quality of supply. Hence, price–cap regulation, a common incentive pricing scheme in developed power markets, is specifically designed to ensure that in fixing electricity tariffs for a period of time, power distributors are driven to increase their efficiency. One such realm for efficiency reductions lies in reducing power outages: outages impose the opportunity cost of unsold electricity as well as replacement costs from physical equipment damage on power distribution firms (Ladeira, 2016).

Nevertheless, many developing countries are marked by low infrastructure quality despite the fact that utilities can recover costs through prices. Explanations for this phenomenon put forward by the literature include explicit subsidies for low–income end–users (McRae, 2015) and quality monitoring failures (Burgess et al., 2017). In this paper I explore an alternative explanation that makes use of Brazilian power sector data. In line with real options theory, I show that uncertainty plays a key role in affecting firm behaviour and determining lumpy investments in distribution substations. These substation investments in turn hold important consequences for electricity reliability.[1]

[1]Substations connect electricity firms' distribution infrastructure to the transmission grid.

This paper characterizes the role of a policy–induced decline in uncertainty in determining investment behaviour. My identification strategy relies on a pseudo–natural experiment. To begin with, I re–construct a measure of electricity firm profitability widely used in power utility finance, which reflects the extent to which retail electricity prices in Brazil cover electricity distributors' expenses on power purchases – the cost–recovery ratio. A drought led to exogenously–induced changes in utility profitability. These changes were due to generation cost variations that were not fed through to consumers until an extraordinary tariff review. The profitability changes were accompanied by a policy intervention, namely a tariff flag mechanism introduced by the government in the same year which signalled future electricity prices depending on hydro–generation conditions. Firms that did not receive permission to adjust prices monthly based on the price signalling mechanism operate within the isolated power market. These firms serve as the control group in the pseudo–natural experiment set–up. I interpret the tariff flag as a policy induced decline in the variability of profitability of the firms that were allowed to act on it. Within this context I estimate the marginal propensity to undertake a lumpy investment in the distribution grid in response to a relative decline in uncertainty in a diff–in–diff strategy. Real options theory predicts that being exposed to the tariff flag policy lowers firms' option value of deferring lumpy investments: given the improvement in policy certainty, additional information on the new distribution of electricity prices should be less useful for allowing these firms to achieve a higher net present value of their irreversible investments.

This study relates to an extensive literature on the economics of lumpy firm investments under uncertainty. Building on the earliest treatments of the topic, Cukierman (1980) studies a single agent's decision problem and establishes that even risk–neutral firms may find it optimal to delay investment opportunities in order to await additional information on the project's future returns. These theoretical results are corroborated by Bernanke (1983), McDonald and Siegel (1986) and Dixit and Pindyck (1994*b*). I apply this framework to the case of electricity provision,

and introduce uncertainty as an increase in the noisiness of the parameter that links generation costs to power prices.

Empirically, relatively few studies have examined firms' investment behaviour within the context of the infrastructure industry, where regulation is routinely aimed at encouraging sufficient investments in transmission and distribution grids. Recent research has focused on the moral hazard and time inconsistency problems affecting electric utility investments (Lim and Yurukoglu, 2018) and investment–deterring incentives arising from end–user electricity subsidies (McRae, 2015). Within this strand the focus has been exclusively on marginal, "small", reversible investments by firms. A limited number of studies incorporate uncertainty over returns and find that increased uncertainty over these returns can explain deviations in power (generation) investments from theoretical predictions (Madlener, Kumbaroğlu and Ediger, 2005) as well as a lack of adjustment of oil refinery capacity (Dunne, 2010).

In focusing on a unique source of uncertainty within the context of Brazilian electricity provision, I offer multiple contributions to the existing literature. This paper is the first to my knowledge to address the paradox of sufficient cost–recovery and low power reliability in developing countries in a causal setting. It also is the first estimation–driven explanation for why increases in generation capacity alone may not bring about economic growth if distribution grid maintenance is poor. In focusing on the role of government–induced (reductions in) uncertainty in utilities' business environment, this study also holds important policy implications: based on the findings of this paper, regulators wishing to incentivize utilities to make critical lumpy investments should – beyond specifying the *level* of cost–recovery – examine and mitigate sources of *uncertainty* of cost–recovery.

The paper offers multiple empirical and methodological advances from the existing literature. The aforementioned studies treat movements in the uncertainty–inducing variable as if they only affect firms through the risk channel. This

assumption likely does not hold in the many cases where macroeconomic sources of uncertainty are employed. Where macroeconomic measures of uncertainty are used, uncertainty can affect firm investments through channels other than the option value: as an example, the volatility of oil prices can be associated with changes in the level of these variables. More expensive commodity prices in turn affects the end–users of the investment good and could reduce lumpy investments by firms without changing the option value. If these two impacts are not disentangled, then the effect of uncertainty through the option value can be overestimated. The inability to separate the impact of the uncertainty proxy from the wider economic effects of the proxy variable is considered a central challenge in the uncertainty literature (Bloom, 2014). This paper's pseudo–natural experiment allows me to address this by identifying the impact of a policy–induced reduction in uncertainty separately from indirect effects that could confound the results. Rather than utilizing electricity price movements themselves as a proxy variable, I exploit the government's introduction of an electricity price signalling mechanism as an improvement in the pass–through of power price risks to consumers.

My findings are relevant for the literature on uncertainty shocks in industrial organization. Specifically Hurn and Wright (1994), Moel and Tufano (2002), and Dunne (2010) estimate the effect of resource price volatility on oil drilling investments, mine operations and refinery investments, respectively. After carefully minimizing the risk of contaminated estimates, this paper yields results that are qualitatively in line with existing findings that an increase in uncertainty leads firms to reduce lumpy investments. Once more in line with the comparative predictions of the classical approach to investment decisions and the real options approach, marginal investments due not increase by more than lumpy investments if we require the effect to be statistically significant. The results correspond to the notion that firm beliefs over the volatility of power prices have real impacts on the policy variables that regulators target in developing economies.

In what follows, I first discuss the suitability of the electricity sector to the study of lumpy firm investments under uncertainty in a real options theory framework. Next, I cover the relevant institutional details of Brazil's power industry that enable the pseudo–natural experiment. I then describe the experiment setting, before estimating the impact of the uncertainty event on lumpy as well as marginal firm investments in infrastructure. The paper then conducts statistical tests to corroborate the results and address key identification concerns.

2.2 Irreversible investments in electricity provision

Real options theory as developed by Marschak (1949), Arrow (1968), Bernanke (1983) and Dixit and Pindyck (1994*b*) among others tells us that firms that make sunk cost investments are affected by uncertainty differently than in the case of reversible investments. Let us define baseline uncertainty as the existence of states of nature with positive and negative investment returns, to which positive probabilities are attached. An increase in uncertainty corresponds to a shift in the probability density of power prices net of variable costs away from the center of the distribution. When power distribution investments are reversible, traditional notions of optimal behaviour such as the Tobin's q approach suggest firms will invest if the expected net present value of the project is non–negative. When investments are irreversible, or if it is very costly to do so, this naive investment timing rule ignores the value of holding on to the option to invest in the future: if the distribution of the expected rate of return can be better revealed to investors in subsequent periods, the deferral of lumpy investments entails the opportunity to await additional information on that observable parameter. This allows firms to make the most optimal investment decisions from an ex–post perspective.

Hence, even when holding expected returns constant, a larger dispersion of future returns should lead an optimizing firm to view the option of waiting as more valuable. In other words, increased uncertainty will reduce the current level of

sunk–cost investment by making it more profitable to await more information before choosing an investment project.

The power sector lends itself well to the study of firm investments within the real options framework. First, sunk cost investments by electricity firms are commonplace and necessary for the provision of electricity. Investments by electricity distribution firms in the electricity grid are capital–intensive sunk costs: such investments consist of augmentations of distribution infrastructure primarily in the form of substation replacements and new–builds. Once these repairs are made (i.e. the materials and labor are expended), they cannot easily be recovered without additional labor and material expenses. As of 2016, a substation expansion in Brazil required an investment of between $2.3 mn and $4.8 mn (Interconexión Eléctrica S.A. E.S.P ISA, 2016) depending on the specific site. Variability of these costs stems primarily from location differences. Further, components used are usually processed and specific to the electricity industry, with little value attached to them outside of the power sector.

Next, real options theory assumes that the investment decision is not merely of the "now or never" type. Electricity firms have the option of deferring quality investments for a period of time. In a price–cap system the baseline electricity price is set each year, as is the case in Brazil (Enel Américas, 2017). Within this period, electricity distribution firms which have concession licenses lasting many years are granted autonomy over the timing of investments. Investments can be delayed if firms find this optimal. The "real" asset of a substation investment is therefore similar in nature to an American–type financial call option. A call option gives the contract holder the right but not the obligation to buy an underlying asset at the pre–specified price at a later date. Like a call option, it is reasonable to consider the cost of a lumpy distribution grid investment as "pre–specified", since the required manual labour and material costs should be relatively constant over time. This pre–specified or fixed nature of the project is important, because it ensures that a

project that is delayed due to uncertainty is still considered *the same project.*

An additional theoretical element is the expiration date of the option: under the real options framework investments cannot be delayed without consequence forever, as there is an expiry date to being able to undertake them. In the case of electricity provision an implicit end–date takes the form of minimum quality controls imposed by regulators. These controls require firms to undertake minimum lumpy investments at some point irrespective of the option value of waiting.

It is also important for the real options set–up that firms cannot otherwise hedge against the uncertainty of the investment. Hedging would lessen the option value of awaiting information and any substantial impacts of uncertainty. In Brazil (and other partially liberalized electricity markets), risk–hedging is not possible. While forward contracts exist in Brazil, the 60 electric distribution firms exist in the "captive" market and have no access to the forward market. For this reason increased generation costs incurred by hydro–power producers, most notably the Itaipu dam in 2013, have been passed on to electricity firms through mandated purchase price increases (Itaú BBA, 2015). This changed in 2015, as section 3 details.

Finally, the real options set–up does not prescribe the stochastic process that determines the project pay–off to be non–stationary. Dixit and Pindyck (1994*b*) show that the stronger the underlying asset price's tendency to revert to a long–term mean – and if the investment cost lies below the future expected mean pay–off of the project, then the larger the investment value. This is because – all else held constant – a stronger rate of mean reversion In theory the assumption of mean–reversion does not appear to change the impact of uncertainty on the level of lumpy investments.

Framework for investments by electricity firms This framework allows us to think about lumpy quality investments made by electricity firms. Consider a risk–averse price–taking electricity firm that is deciding whether to undertake a

lumpy quality investment in the distribution grid. It faces a finitely–lived call option to execute quality investments take the form of substation replacements and constructions – finite, because of the quality controls and limited concession contract. Retail electricity prices p are announced by the regulator in the annual tariff reviews at the beginning of each period, after which firms make their investment decisions. The irreversible investment will incur a lumpy cost c. There is also a "cost of goods sold" – the marginal costs of running a distribution company – for the additional power provided, denoted as v.

The decision to invest in electricity quality leads to reductions in power outages and losses, allowing additional amounts q_t of electricity purchased by the electricity firm to be sold to the end–user at prices p_t. In a net–present value framework, the value from making the lumpy quality investment is described as follows for a given firm:

$$\textit{Pure Net Present Value} = \sum_{t=1}^{T} \frac{(p_t - v_t)q_t}{(1+r)^t} - c, \tag{2.1}$$

where r is the risk–adjusted discount rate. The risk–adjusted discount rate incorporates both the time–value of money and a risk–premium that reflects additional compensation an investor (in this case the electricity firm) requires for taking on the possibility that the future cash flow from the lumpy investment will not materialize. In the traditional NPV framework the utility would undertake the investment if the net present value of the project is positive. Uncertainty appears in this framework as a change in the levels of $p_t - v_t$, as well as an adjustment in the (risk–adjusted) discount rate r, as firms are assumed to be risk–averse. When uncertainty associated with the project in the form of variability of net prices $p - v$ increases, the risk–adjusted discount rate rises to reflect the fact that the electricity firm requires a higher return to compensate for the additional risk. The sum of future cash flows on the other hand could rise or fall depending on the distribution of uncertainty throughout $t = 1$ through T. However, where real options exist, otherwise unsuitable investment projects can become viable options after a period of waiting. The waiting period changes the payoffs of the project. However I still consider it to be the "same project" as prior to the waiting period

since electricity firms' projects are tied to a fixed physical structure and location. The net present value of the investment option is a sum of the pure value of the project and the option value of waiting:

$$\begin{aligned} &\textit{Net Present Value of Investment Option} = \\ &\textit{Pure Net Present Value} + \textit{Option Value}. \end{aligned} \tag{2.2}$$

Now uncertainty over net prices affects the net option value of an irreversible project through 1) the discount rate (due to risk aversion), and the level changes in future cash flows, but also through 2) the option value of deferring the investment opportunity. If the project is reversible – i.e. in the classical NPV framework, waiting and seeing holds relatively less (or no) value, and the real–options channel does not exist. For irreversible, lumpy investments, if future price volatility rises then the potential upside from waiting increases while the potential downside is still largely capped at zero (firms have the option, not the obligation to invest). As a result the investment is more likely to be deferred. *The testable hypothesis is that lumpy investments should therefore – all else held constant – be more sensitive to uncertainty changes than marginal investments.* This paper uses the quantitatively different predictions for marginal and lumpy investments to show whether real options are at work. I do so by comparing the estimates in both cases to one another.

Generation costs as a source of option value Firms' optimal decision rule (the principles that determine when they should invest based on the electricity price) will depend on time and a more concrete stochastic process for net prices as state variables, and on two control variables: timing and scope of the lumpy investment decisions.

Rational electricity firms form expectations over future net electricity prices as set by the regulator when deciding on lumpy quality investments. In particular,

based on the institutional set–up in Brazil, prices can be described as based on the regulator's official price setting policy rule:

$$p_{i,this\,period} = f(p_{i,last\,period}, \Delta MC_{i,last\,period}) \tag{2.3}$$

The gap between periods refers to the time between two tariff reviews. In the case of Brazil this is a year (until 2015). Prices at the next tariff review depend on the current price level and the last 12 months' sectoral costs. These sectoral costs consist almost entirely of generation costs. Inflation as well as social cost adjustments are abstracted from in this equation for simplicity, and thus prices can be taken to be real prices in this framework. In the case of electricity provision in Brazil generation costs are dominated by hydro–power assets, which take up 75 percent of the generation mix as of 2020 (IEA, 2020).

Under this price–cap system electricity firms in Brazil were exposed to generation cost volatility between any two reviews. This system has been documented to have caused cash–flow problems for utilities throughout but not limited to 2013: firms were in certain months unable to pay generators for contracted power, which eventually led to the 2015 policy change that is the focus of this paper (Practical Law, 2015).

I view this volatility in costs while prices are fixed as the manifestation of uncertainty throughout the paper. As generation costs can fluctuate throughout the 12 months between tariff reviews, the value of waiting from the perspective of firms lies in them waiting until they are told the next period's electricity price. Once this information is received, firms can determine which projects they can execute given their financial circumstances.

The next section discusses the institutional set–up in Brazil.

Table 2.1: Ownership structure of 10 largest distribution firms

Cemig	publicly traded with mixed shareholders incl. Minas Gerais State
ENEL São Paulo	publicly traded with mixed shareholders incl. Italian government
CPFL Paulista	mixed privately owned
Copel Distribução	Local state owned by Parana
Celesc	publicly traded with mixed shareholders incl. state of Santa Catarina
Light SESA	publicly traded with private shareholders
Coelba	owned by Iberdrola, which has mixed shareholders
RGE	owned by CPFL Energia, which is a private company
Elektro	owned by Iberdrola, which has mixed shareholders
Bandeirante	owned by EDP which is publicly traded and mixed owned

Notes: Table describes the ownership structure of the 10 largest power distributors in Brazil, as measured by their market share in electricity sold.

2.3 Institutional background in Brazil

In Brazil electricity is sold by 60 electricity distribution firms in their respective concession areas (da Silva, 2007). The key owners within the 10 largest of the firms studied are summarized in table 2.1 below. Most firms are publicly traded, with shareholders that are a mix of private and public. Among the top 10, just a single firm is owned solely by the local state government. Shareholders can have a direct influence on business decisions through voting rights. They tend to cast their vote in company board of directors elections and at shareholder meetings. Their voting rights do not usually extend to individual investment decisions, and are instead used to influence overall corporate strategy as well as for accounting or remuneration decisions (Mallin and Melis, 2012).

These firms purchase their power from two distinct market arrangements. Since 2004 electricity firms have been required to fully cover expected electricity demand via the purchase of contracts. This can be fulfilled in two ways. The free market, which comprises 30 percent of electricity sales as of 2018, allows electricity firms to purchase their power through over–the–counter bilateral contracts or in organised

exchanges at the spot price. This spot price is usually set at a spread to the reference price (the PLD), which is calculated by the electricity market operator – CCEE – based on computer models. Most electricity sales to date have however occurred in the regulated contracting environment, where electricity firms purchase their power at least one year in advance from generators in a government–run auction with regulated maximum prices. These auctions are dominated by hydro–power contracts, and ensure that electricity firms face similar average electricity purchasing costs irrespective of their location. It is the regulated contracting environment that provides an appropriate institutional setting for studying firms' investment decisions under uncertainty. This is for several reasons.

First, electricity firms hold local monopolies over electricity distribution and sales in their respective concession areas, and are unlikely to take into account other firms' choices when making irreversible investment decisions. As a result, the framework can be treated as a single–agent problem, with minimal spillovers between firms. While there is the possibility of customers migrating to better quality concession areas, a key factor suggests this must not complicate the theoretical framework: costs associated with geographic relocation and transaction costs due to switching between electricity providers should mitigate such competition (consumers would have to relocate to another municipality in order to switch providers).

Second, despite their local monopoly status, electricity providers with concessions are price and generation cost–takers in Brazil. Retail electricity prices are adjusted by the regulator based on changes in various cost components on an annual basis (Enel Américas, 2017). Hence the electricity supply firms' main choice variable is the level of investment in the distribution grid and operations.

Third, the official retail electricity price setting mechanism is published by the regulator, allowing me to establish how the natural experiment is likely to have affected the future predictability of prices from the perspective of firms (Table 2.2)

Table 2.2: Weightings in Electricity Price Setting Formula

Cost component	Weight in formula
Power purchase charges	42%
Power transmission charges	9%
Sectorial charges	19%
Power distribution costs	30%

Notes: Table reports published weightings of various cost components in the electricity price–setting formula.

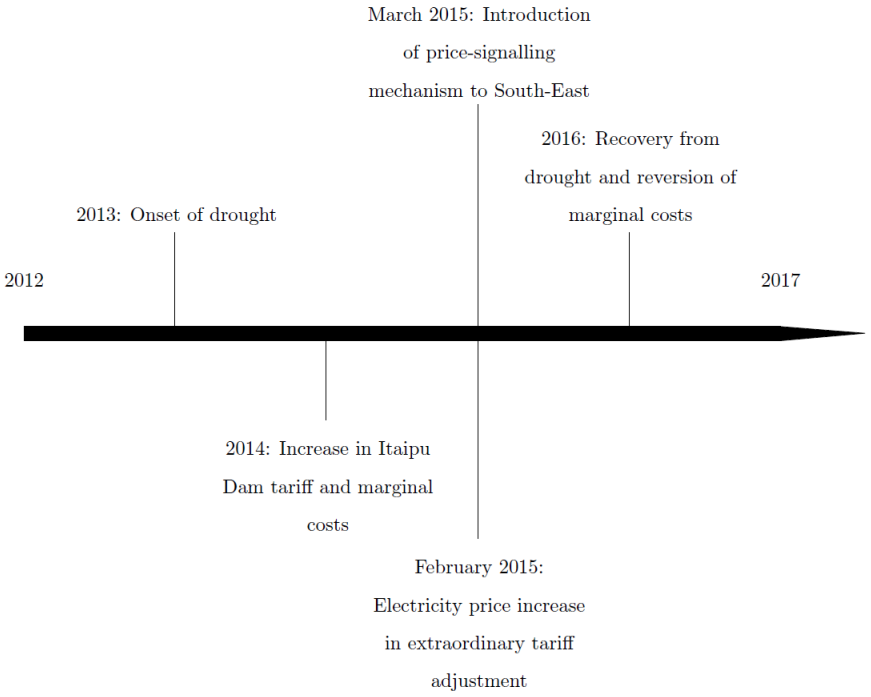
(Agéncia National de Energia Elétrica, 2011). In particular, prices are officially set based on a manageable and unmanageable component from the perspective of the electricity firm. Adjustments for generation costs occur every year. Price adjustments at time t incorporate observable marginal cost information in the 12 months leading up to the revision. The manageable component consists of operating and maintenance costs and fixed depreciation of distribution assets. The unmanageable component comprises power purchase costs, transmission charges and sectorial charges dominated by subsidies for low–income customers. Finally, the cost components are scaled by an inflation index. This formula should result in a mapping between marginal costs in the power sector and electricity prices that is close to 1:1, all else held constant (Enel Américas, 2017). There has only been a single case in which electricity firms have successfully requested electricity price changes due to an exogenous shock, namely in the 2015 extraordinary tariff review.

The presented institutional setting allows for the unfolding of a unique natural experiment which I exploit for the purpose of identification.

2.4 The natural experiment

> "Let's give some good news when it comes up. We do not take pleasure in giving bad news." – Director General of Brazil's National Energy Agency ahead of the 2015 electricity price increase.

Figure 2.1: Chronology of Events



Notes: Figure depicts the key dates in the natural experiment set–up.

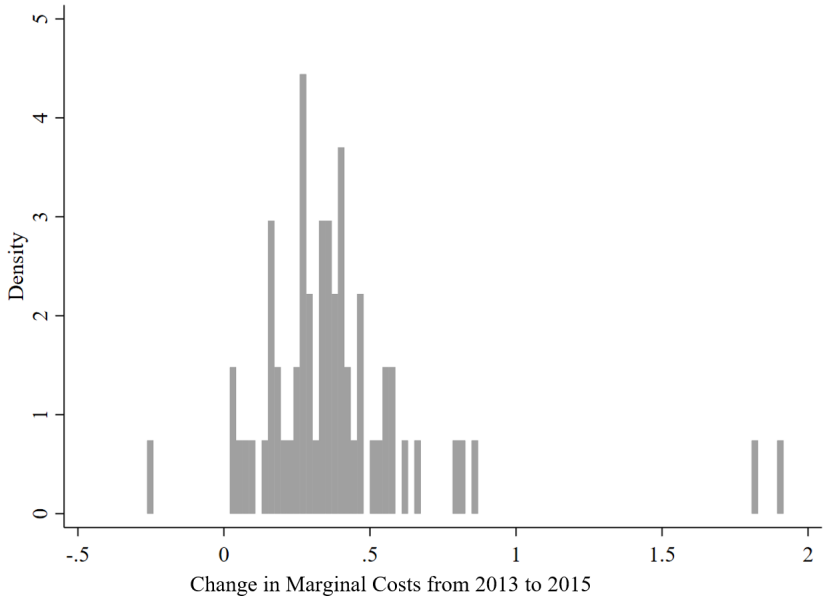
2.4.1 The drought

Figure 2.1 summarizes the chronology of events that constitute the natural experiment set–up.

Starting in 2013, lower than expected hydro–power availability due to a major drought led to electricity generation shortages at the Itaipu Dam, and hydro–power plants nationwide. The average annual inflow 2014 to the reservoir was 10,024 cubic meters per second, thus 14.6 percent lower than the historical average for the period 1983 through 2013 of 11.741 cubic meters per second (Itaipu Binacional, 2014).

Given contractual electricity sale obligations set in the forward power auction market, the shortfall in electricity generation had to be purchased by hydro–power generators, most notably Itaipu, on the spot market. Additionally, thermal generation in Northern Brazil was ramped up to fill the supply gap. Thermal power plants operated at elevated costs in 2014 and 2015 despite reductions in

Figure 2.2: Drought–Induced Increase in Marginal Costs

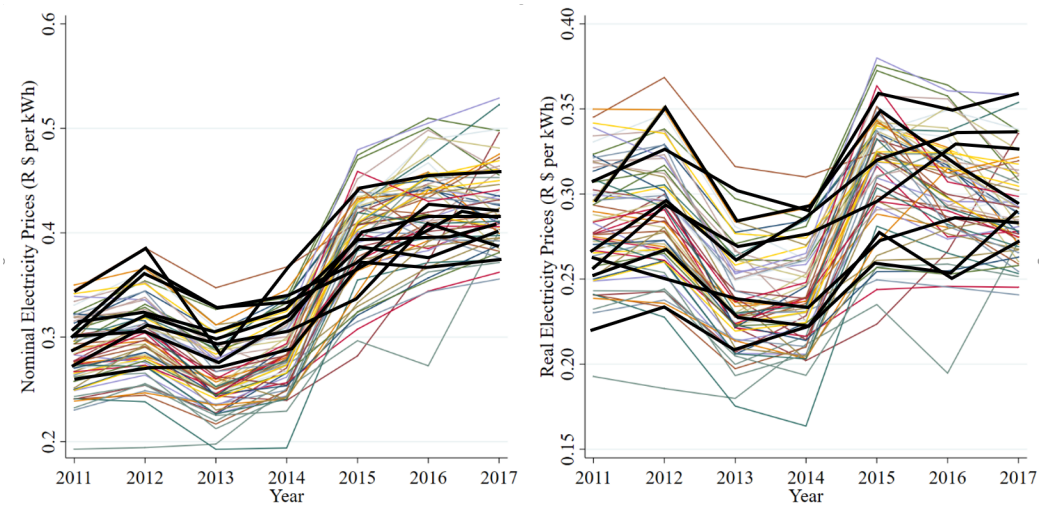


Notes: Figure depicts the change in the per unit cost of purchasing electricity (which are measured in R$ per kWh) by electricity firm, from 2013 to 2015.

the global oil price, due to technical characteristics of the plants commissioned in 2007 through 2009, many of which had not been designed for continuous dispatch (Luz de Brito et al., 2015). Unlike hydro–power, which is purchased through forward energy contracts in long–term markets, thermal power supply is offered via availability contracts (*contratos por disponibilidade*) in short–term markets, where annual variable costs are paid by electricity firms at the time that thermal power is actually dispatched, i.e. during the drought. While previously increasing thermal generation costs had been covered by a government fund, the *Reserva Global de Reversão*, prior to 2013, the fund came to a halt at the end of the year (Gazeta do Povo, 2014). Hence the increased marginal costs in generation facing generators like Itaipu were passed on to electricity distribution firms. The 2014 shift in the geography of generation led to heterogeneous increases in power purchase costs for each electricity firm in 2014 (Figure 2.2).

In order to offset the impact of the increase in thermal power purchasing costs on electricity firms, the regulator in 2015 allowed for heterogeneous retail electricity

Figure 2.3: Heterogeneous Electricity Price Hike in 2015



Notes: Figure depicts real and nominal average retail electricity price (R$ per kWh), 2011 – 2017, by electricity firm. Real prices are obtained by deflating using Brazil's CPI, with 2011 serving as the base year. Firms that received the 2015 tariff flag are shaded in black.

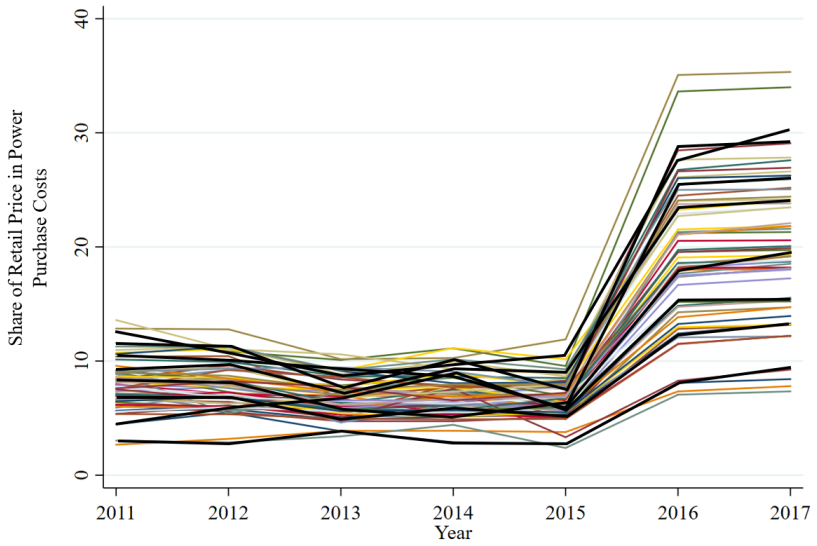
price increases in an extraordinary price adjustment, as shown in Figure 2.3 (Agéncia National de Energia Elétrica, 2015). This non–scheduled price adjustment was company specific, and varied depending on the share of power the firm had purchased from the affected Itaipu dam. The extraordinary price adjustment achieved its policy objective, stabilizing the ratio of prices to marginal costs (Figure 2.4).

In 2016 the shift in generation geography was significantly reversed, with hydro–power replacing thermal power purchases by electricity firms.

2.4.2 Policy event

While existing theories of firm investments have interpreted profitability shocks as uncertainty events, this approach brings with it the difficulty of disentangling the impact of profitability from the impact of the suspected uncertainty change. If profits affect a firm's propensity to make investments, then any observed investment response may not be entirely attributable to real options–type workings. This paper's natural experiment set–up allows me to separate the impact of profitability from the impact of government discretion in electricity price–setting. While all

Figure 2.4: Increase in Ratio of Retail Prices to Marginal Costs of Electricity Purchases



Notes: Depicted ratios are calculated as the share of average electricity prices in power purchases, were both the denominator and numerator are measured in R$ per kWh, 2011 – 2017, by electricity firm. Firms that received the 2015 tariff flag are shaded in black.

electricity firms were exposed to (varying degrees of) the 2015 drought, only a subset was also exposed to a regulatory change that improved the predictability of retail electricity prices. In particular, electricity firms in January 2015 received access to an electricity price signal in the form of "electricity tariff flags" (*bandeiras tarifarias*). These flags officially announced generation costs and consequently future retail electricity price increases one month ahead (Table 2.3) (Agéncia National de Energia Elétrica, 2018). However only a subsection of firms were allowed to act upon the tariff flag and adjust their electricity prices on a month to month basis. For these firms, the government to committed to keeping the retail prices of these utilities linked to generation costs on a month-to-month basis. Meanwhile, the firms to which these flags were not applied were exposed to generation cost fluctuations, but were not allowed by the regulator to adjust their electricity prices until each tariff review, which continued to take place annually.

Prior to the flag system introduction, unexpected generation cost changes that occurred during the fiscal year were only factored into retail prices in the next tariff

Table 2.3: Electricity Price Signalling Mechanism Introduced in 2015

Flag color	Pending tariff increase	Marginal costs (p. MWh)
Green	None	<R$200
Yellow	Up to R$1.5 p. kWh	>R$200 & <R$350
Red	Up to R$4.0 p. kWh	>R$350 & <R$400
Dark Red	Up to R$6.0 p. kWh	>R$400 & <R$500

Note: Table reports system introduced by the electricity regulator in March 2015 for announcing future electricity price movements. Flag colors indicate pending electricity price increases based on estimated marginal costs in power generation as published by the regulator. Source: ANEEL's tariff flag guidelines and Normative Resolution No. 649 of February 2015.

adjustment, which would occur after a one–year delay (Agéncia National de Energia Elétrica, 2011). Unless electricity firms were able to predict those tariff adjustments ahead of time, lumpy investments in distribution infrastructure would have been undertaken in an environment where retail prices can diverge from marginal costs in the short run. The tariff flag mechanism was a government commitment to actively forecast generation conditions and costs and announce an adjustment to retail prices one–month ahead (prices were to be reviewed monthly by the National Electricity System Operator). Linking back to our stylized process for retail electricity prices, the tariff flag mechanism eliminated the generation cost risk facing distributors between reviews. They are now passed through to consumers. This intervention is interpreted by this paper as a government–induced reduction in uncertainty and the option value of awaiting further information. According to the predictions of real options theory, firms now had a more precise understanding of their future profitability, allowing them to undertake lumpy, irreversible investments with a lower green–light threshold. The remaining electricity firms that received no such price–signalling mechanism constitute the control group.

The key features of the policy change are summarized in table 2.4.

In order to determine whether the 2015 electricity price revision indeed constitutes an mitigation of uncertainty in the eyes of the treated firms, it needs to be established that treated electricity firms regarded the price–signalling mechanism

Table 2.4: Pricing Arrangements

Policy Component	**Treated Firms**	**Non–Treated Firms**	**Pre–Policy**
Frequency of Price Announcement	Monthly	Monthly	Annual
Nature of Information Disclosed	Next–Period Tariff	Next–Period Tariff	Next–Period Tariff
Cost Risks Between Reviews	Rests on Consumers	Rests on Distributors	Rests on Distributors
Ability to Change Price Between Reviews	Exists	Does Not Exist	Does Not Exist
Price Is Adjusted	Monthly	Annually	Annually

as credible and significant. The ratings of international credit rating agencies, which assess financial and economic risks facing seven of Brazil's electricity firms, are a qualitative source of evidence. Fitch, a credit rating agency, factored the tariff flag mechanism into its liquidity risk and rating reports in March 2015 (Businesswire, 2015) and September 2016 (Fitch, 2016): "New Initiatives Lower Brazilian Power Distributors' Risk", March 2015 and "Fitch Affirms Eletropaulo's Ratings at 'BB'; Outlook Stable", September 2016. Moody's mentioned the tariff flag system in its credit outlook reports in December 2014 (Moody's, 2014), March 2015 (Moody's, 2015*b*) and November 2015 (Moody's, 2015*a*): "Moody's downgrades ELETROPAULO to Ba2 from Ba1; outlook negative", December 2014, "Moody's places Escelsa's Baa3/Aa1.br issuer ratings under review for downgrade", March 2015 and "Moody's confirms the ratings of Light S.A. and Light Serviços de Eletricidade S.A. to Ba2, outlook is negative", November 2015. The importance attached to the introduction of the tariff flag for electricity firms' risks by the agencies provides evidence that the treated firms experienced a change in uncertainty.

I form the following hypothesis: The electricity firms were offered the price signalling mechanism should exhibit higher sunk–cost investments in their distri-

bution grids compared to the control firms, relative to the pre–flag period, and conditional on the changes in utility profitability. Further, reversible operating expenditures should not be statistically significantly higher for the treatment group. This is because option value changes are less relevant for investments that are non–lumpy and (or) reversible.

2.5 Estimation and identification strategy

A diff–in–diff specification is estimated to evaluate whether the electricity firms that were offered the price signalling mechanism invested more in quality compared to the remaining electricity firms – relative to the pre–shock years. I interpret the tariff flag as a policy–induced improvement in uncertainty while controlling for any level changes in the retail price that accompanied the flag introduction. In doing so I address what is considered a central challenge in the uncertainty literature (Bloom, 2014). The control structure is thus threefold and comprises:

- cross–sectional variation by region (with the Roraima and Amazonas states as containing the control municipalities, and the remaining regions as the treated variables)
- temporal variation by year (with treatment starting in 2015 and lasting through 2017)

This yields the below reduced form equation to be estimated.

$$\begin{aligned} InvLump_{ist} = \alpha + \beta_1 \cdot (Post_t \times Flag_i) + \beta_2 \cdot Post_t + \beta_3 \cdot Flag_i + X'_{ist}\beta_4 \\ + \gamma_i + \delta_t + t_s + \epsilon_{ist}, \end{aligned} \tag{2.4}$$

where $Flag_i$ is an indicator variable assuming the value of 1 for the electricity providers that received the price–signalling mechanism, and 0 for the remaining electricity firms. $InvLump_{ist}$ denotes the number of irreversible investments in municipality i, state s and year t. $Post_t$ is a dummy variable that equals 1 on and after the year 2016. X_{ist} is a set of control variables that includes rainfall, lightning intensity, electricity demand and importantly a measure of a utility's ability to

cover its operating costs through retail prices. The data are described in detail in the following section. It is important to control for the changes in profitability, as this allows for isolating the impact of uncertainty from the impact of a general improvement in firms' abilities to recover costs.

The diff–in–diff strategy controls for any confounders on the country–level, including economic activity that may affect both the government's decision to improve transparency and lumpy investments by firms. The coefficient β_1 is the expected differential change in lumpy investments by firms that received the flag, relative to the pre–flag era and the control firms.

When estimating the impact on lumpy investment activity I choose a Poisson estimation method in addition to reporting OLS results. I do so to account for the nature of the data. First, given the physical boundaries of each utilities' concession area, it is plausible that lumpy investment activity declines over time as fewer opportunities for such investment remain after each spend. As opposed to a logit or probit model, which models the unconditional probability of an event, a Pseudo Poisson Maximum Likelihood (PPML) estimation method with the option of an exposure variable allows for modelling of the probability of a lumpy investment in period t conditional on the firm not having undertaken the lumpy investment up to $t-1$. The PPML estimates are also consistent under heteroscedasticity of the error terms(Santos Silva and Tenreyro, 2006). Finally, the PPML estimator circumvents the problem caused by taking the log of observations of zero. Importantly, PPML approaches can produce consistent estimates in the context of panel–data when the inclusion of fixed effects is desired. This alleviates a common challenge when estimating log–linear equations. In particular, when the number of time–units is small, inclusion of a large number of fixed effects in a maximum likelihood estimation entails the incidental parameter problem, which would contaminate the estimated coefficients (Neyman and Scott, 1948). I include fixed effects by municipality γ_i and year δ_t in the PPML estimations. While I am not able to control for all

unobserved state–level time–varying factors in the PPML set–up, the subsequent matching–on–observables approach compensates for this issue.

Behind the backdrop of this natural experiment set–up, there are several identification challenges when estimating the impact of regulatory uncertainty on firm investments and power reliability. Electricity firms and the government communicate frequently for the purpose of reporting performance data. While electricity prices are officially set based on objective power sector metrics, unobserved institutional arrangements between electricity firms and the regulator could allow firms with higher investment needs to be less exposed to uncertainty by the government, thus contaminating the relationship between uncertainty and firm investments. Hence, the direction of causality between the uncertainty event and investments needs to be carefully established. It is also possible that the treatment and control firms would have diverged in the absence of the tariff flag introduction. This could occur if both groups were on diverging paths to begin with, for example because of their location, geographical traits and other characteristics. I address these concerns through a number of tests – including a parallel trends test – which follow the main empirical analysis.

2.6 Data construction

The data employed in the analysis span 2011 through 2017 and have been collected on the raster, municipality and firm by year level.

Technical reliability data are included in order to control for potential investment needs that could have affected treated firms around the time of the flag introduction. The data were obtained from employees at the Brazilian Electricity Regulatory Agency (ANEEL) as well as collected from the regulator's publicly accessible website.

In particular, outage frequencies (number of outages per year) and durations (hours of outages per year) were compiled by ANEEL on the electrical cluster level (several electrical clusters make up a municipality). In order to aggregate these figures to the electricity firm level, the micro–level numbers are averaged by ANEEL across electrical clusters. The reason for averaging rather than summing the data stems from the fact that they represent an annual measure of the interruption facing an average customer. Since electrical clusters are non–overlapping, a given customer is not affected multiple times by outages in other regions, unless it directly affects the portion of the grid the customer is located on. Technical distribution losses are measured as the percentage share of electricity entering the distribution network that reaches the final customer (Antmann, 2009).

Investment and profitability data were extracted from the financial statements of the 60 electricity firms for each year between 2011 and 2017, available from ANEEL. In general utilities experience a number of costs including *costs of goods sold* and other (non–)operating expenses. Costs of goods sold include electricity purchases from generators, while operating expenses consist of employee compensation as well as other administrative costs. This paper focuses primarily on costs of goods sold as a the key input into the main explanatory variable. The main explanatory variable is a widely used measure of electricity firm profitability, the cost–recovery ratio:

$$CRR_{ist} = \frac{Average\ electricity\ price\ (R\$\ per\ kWh)}{Cost\ of\ power\ purchases\ (R\$\ per\ kWh)}, \tag{2.5}$$

for municipality i in state s and year t. It captures the extent to which electricity prices as set by the power sector regulator allow the electricity firm to cover its costs of purchasing electricity (Huenteler et al., 2017). Importantly, the denominator consists exclusively of the cost of purchasing power from the power generators. If we were to include capital expenditures on grid upgrades – i.e. lumpy investment expenditures – then lumpy investments would be included on both sides of the estimating equation.

Investment expenditure data are obtained from the annual reports of the electricity firms. Irreversible investments are measured as the number and value of main–grid connected substations that are replaced and newly constructed. These are so–called capital expenditures, as they are considered to result in an asset that then allows the firm to generate future revenues. They represent large investments in the distribution grid, and I refer to them as lumpy or sunk–cost investments interchangeably throughout the text. The justification for selecting main grid–connected substations as the key outcome measure of firm investments is two–fold. Substations affect electricity reliability as they are the most common source of distribution system outages within the equipment failure classifications (Brown, 2008). Second, substation replacements are highly lumpy and thus make real options theory applicable: As of 2016, a Brazilian substation expansion required an investment of between $2.3 mn and $4.8 mn (Interconexión Eléctrica S.A. E.S.P ISA, 2016) which was not recoverable or transferable to another company. The variable which tracks the substation replacements by distribution firms is denoted as $InvLump$ throughout the paper.

The cost–recovery ratio, which is the key independent variable, and lumpy investment expenditure, the dependent variable, should only affect each other in one direction. First, power purchase costs are unlikely to be influenced by distribution firms. They are, as occurred during the 2013 drought, determined by the decisions and factors affecting the availability of hydro power. This is in part because of the vertically disintegrated nature of Brazilian power provision. There is no evidence that they are influenced by distributors through the decisions of their directors or staff. Costs associated with maintaining the grid on the other hand relate to the physical infrastructure owned by each distribution firm, and this paper argues that a higher cost–recovery ratio should be associated with a larger value of operating and capital expenditures.

Environmental and economic data stem from publicly available sources, and are selected to control for the most relevant weather and demand–side variables that could jointly affect our left and right hand side variables simultaneously. Rainfall can primarily damage distribution infrastructure by flooding substation equipment, leading to increased lumpy investments. Tumultuous weather could concurrently lead regulators to improve pricing transparency as a means of mitigating risks facing distributors. If both of these effects are present in the data, then the effect of the introduction of the tariff flag cannot be easily disentangled from weather effects. I therefore collect raster–level rainfall data from the Climate Hazard Group at the University of California Santa Barbara, which are then aggregated to the municipal polygon level. Similarly, lightning data are computed to control for damages to power distribution. Lightning raises the voltage across the affected infrastructure asset, causing a short circuit. It can also exceed the energy handling capability of the apparatus, thus causing damage to physical infrastructure capital. Strikes can affect surrounding structures that can physically damage substation equipment (IEC, 2010). In addition to immediate outages, additional outages can occur if equipment is not repaired by distribution utilities. Lightning intensity data are not available for Brazil between 2011 and 2017 on a municipality level. Hence I follow Romps et al. (2014) and predict lightning intensity as

$$\textit{Lightning Flash Density} \propto P \times CAPE,$$

where P is the precipitation rate (mm per area per year) and CAPE is an area's convective available potential energy (Joule per kilogram per area). CAPE captures the degree of atmospheric instability, and is an indication of impending storms and extreme weather. In order to compute the lightning data for Brazil's municipalities, the above mentioned rainfall data are combined with atmospheric instability data obtained from the National Oceanic and Atmospheric Administration's Global Forecast System. The System provides raster–level CAPE estimates at a 0.5 degree resolution in separate files by time of day, which I first process into un–weighted annual averages before aggregating to the municipal polygon level. Geospatial

Table 2.5: Descriptive Statistics

	Mean	**Std.Dev.**	**Min**	**Max**	**Obs**
Operating Costs (R$ per kWh)	0.4	(0.12)	0.2	1.6	372
-"- net power purchases (R$ mn)	261.3	(73.08)	0.9	322.3	372
Retail Electricity Price (R$ per kWh)	0.3	(0.72)	0.2	0.5	372
Electricity Purchase Recovery Ratio	3.6	(2.17)	0.7	11.1	372
Lumpy Investment Expend. (R$ mn)	52.7	(230.71)	6.6	170.1	372
Nr of Lumpy Investments (per year)	3.7	(2.63)	0.0	21.5	372
Outage Duration (hrs per year)	47.2	(32.35)	0.9	253.7	372
Outage Frequency (outages per year)	28.0	(21.09)	1.0	150.0	372
Electricity Demand (TWh)	1,685.0	(1,413.66)	1.2	4,645.8	372
GDP per capita (R$ 1k)	45.8	(472.37)	0.2	35,054.4	27,214
Rainfall (mm)	1,371.0	(570.55)	144.5	3,988.3	37,991
CAPE (Joule per kg per area)	400.8	(201.78)	52.9	1,212.4	37,991
Predicted Lightning (Rainfall × CAPE)	608.3	(483.85)	14.4	4348.2	36,895

Notes: Table reports summary statistics. Operating cost, Electricity Sales, Average Price, Profitabiltiy and Outage measures are at the electricity firm by year level. Rainfall and Lightning variables as well as GDP per capita are measured at the municipality by year level. Electricity purchase recovery ratio is measured as the ratio of the average retail electricity price (R$ per kWh sold) to electricity purchase cost (R$ per kWh sold).

polygon shapefiles for Brazil's 5,570 municipalities are downloaded from the Brazilian Institute of Geography and Statistics. Municipality–level GDP per capita data are also collected from the Statistical Institute, but are only available until 2016. The data are employed for robustness tests, but do not feature in the main estimations. Given the co–movement between electricity demand and economic growth, electricity sales (in MWh) by electricity firm shall serve as a proxy for the lacking GDP p.c. data on a munipality level. The electricity demand data are available through ANEEL. It is important to note that there is a risk that electricity sales are influenced by one of the other control variables, namely electricity reliability: higher electricity sales could be caused by an improvement in outages and losses.

Electricity sector data consist of average electricity prices by electricity firm and electricity demand by municipality and are available at ANEEL and from the financial statements respectively. For each firm, electricity prices are averaged across the residential, commercial and industrial customer groupings and weighted

by the each group's respective share in total electricity consumption.

Table 2.5 presents summary statistics of the main variables employed in the empirical analysis. There are 37,991 municipality–by–year observations and 372 firm–by–year observations. The average municipality is served by an electricity firm that charges R$0.33 (£0.06 in 2018) per kWh of electricity consumed. On average almost four substation replacements are made within a given municipality each year, while firms each spend R$52.8 mn (£9.8 mn in 2018) on distribution infrastructure operations. Infrastructure quality varies widely, with outage frequencies ranging from less than 1 to 150 per year. Durations are even more dispersed, averaging at 47 hours of blackouts per year in a given municipality. Figure 2.5 reveals a visual divergence of the annual average number and value of lumpy investments between the treatment and control group from 2015 onwards. Specifically, both the treated and control firms experience an increase in the number of sunk–cost investments in power distribution following 2015. The change following 2015 is larger for treatment firms, but persists for longer in the case of their counterparts. Part b of the figure shows that both types of firms also take on higher value projects, as measured by the value in $ of the investments undertaken. This time the effect dissipates more quickly for the control firms.

2.7 Results

The PPML specification reveals a significant impact of uncertainty on irreversible firm investments. The number of irreversible investments in the distribution grid in the form of substation constructions/replacements increases by $(e^{0.124}-1)*100 = 13$ percent among treated firms relative to the control utilities and to the pre–2015 era. (Table 2.6, column (3)). With an average annual number of 3.73 lumpy investments, and applying the coefficient, this amounts to an increase to 4.2 lumpy investments per year, worth an additional $1.7 to $2.1 million in investments [2].

[2]The dollar–value of the additional investments is obtained by multiplying the increase in the number of lumpy investments by the estimated cost of a large substation replacement from the earlier analysis

Table 2.6: PPML Estimation of Impact of Uncertainty on Irreversible Investments

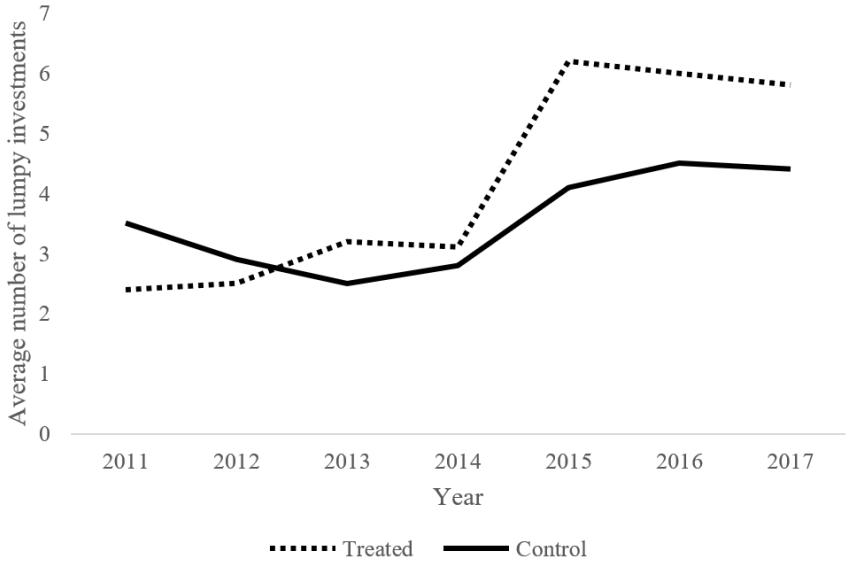
	Dep. var: Lumpy investment incidents		
	(1)	**(2)**	**(3)**
Flag × Post	0.341**	0.265**	0.124*
	(0.104)	(0.101)	(0.05)
Post	0.404*	0.235**	0.094*
	(0.200)	(0.098)	(0.040)
Year FE	X	X	X
Municipality FE		X	X
Controls			X
R Squared	0.67	0.67	0.69
Observations	36,116	37,451	36,988

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Table reports impact of policy–induced uncertainty reduction on the number of lumpy investments, estimated in a PPML model. Standard errors in parentheses are clustered by electricity firm. Fixed effects by municipality and by year are included.

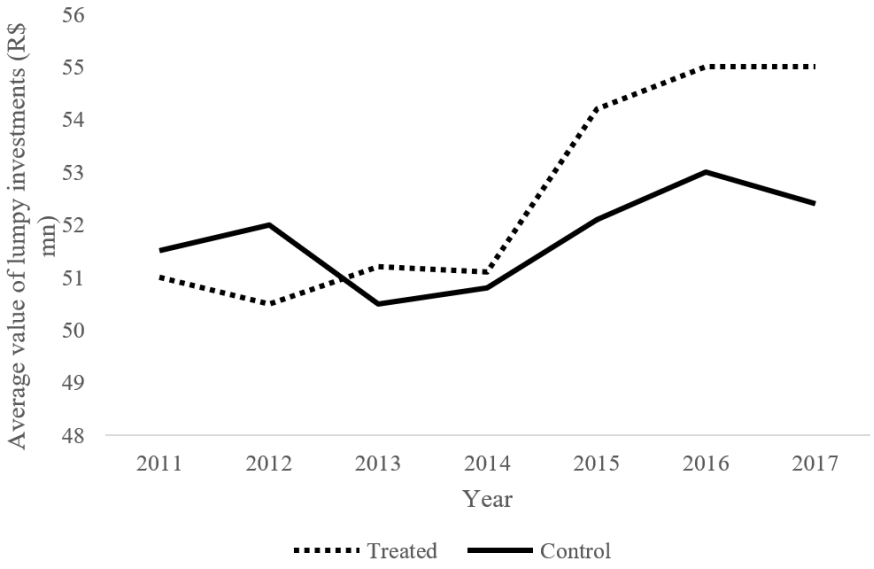
The policy–induced uncertainty alleviation via the tariff flag introduction accounts for a statistically significant percent relative increase in lumpy investment activity. Further, the *Post* coefficient reveals that lumpy investment activity increased for the firms that did not receive the tariff flag following the policy.

For comparison, results from the OLS regression are reported in table 2.7. The OLS results are of the same direction as the PPML estimates, but suggest a relatively large increase in the number of lumpy investments. When controlling for the full set of municipality and year fixed effects as well as additional controls, the results suggest that the tariff flag raised lumpy investment activity by 3.32 projects (column (4)). With an average annual project count of 3.73 in the data, this would amount to nearly a doubling of lumpy investment activity in Brazil. The difference between these two coefficients is statistically significant, based on a Wald test. The difference between the OLS and PPML estimation results confirm that since I am also interested in the magnitude of the effect I am inclined to place more weight on the PPML results as the technique addresses the incidental parameters problem.

Figure 2.5: Average annual number and value of lumpy investments



(a) Average annual number of lumpy investments



(b) Average annual value (in million R $) of lumpy investments

Notes: Figures depicts annual value and number of lumpy investment investments in Brazil between the treatment and control firms.

The analysis thus focuses exclusively on the results of the PPML estimation.

Implicit in real options theory is the prediction that while lumpy firm investments should be affected by changes in the option value of awaiting further information,

Table 2.7: OLS Estimation of Impact of Uncertainty on Irreversible Investments

	Dep. var: Log Lumpy investment incidents			
	(1)	**(2)**	**(3)**	**(4)**
Flag × Post	-0.202	2.492	1.952*	3.326***
	(0.249)	(1.737)	(1.167)	(0.362)
Post	-0.311	1.155*	2.433	1.678*
	(0.254)	(0.511)	(1.321)	(0.762)
Year FE	X	X	X	X
Municipality FE		X	X	X
State-Specific Trends			X	X
Controls				X
R Squared	0.77	0.78	0.78	0.77
Observations	35,112	35,112	35,112	30,041

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Table reports impact of policy–induced uncertainty reduction on the number of lumpy investments, estimated by OLS. Standard errors in parentheses are clustered by electricity firm. Fixed effects by municipality, year and state–specific trends are included.

the same should not hold true to the same extent for *reversible* ongoing operating expenses. The dataset allows me to test this hypothesis by estimating the following model by OLS, as the key dependent variable is now continuous:

$$\begin{aligned} InvOPEX_{ist} = \alpha + \beta_1 \cdot (Post_t \times Flag_i) + \beta_2 Post_t + \beta_3 Flag_i \\ + X'_{ist}\beta_4 + \gamma_i + \delta_t + t_s + \epsilon_{ist}, \end{aligned} \tag{2.6}$$

where all variables are as defined in equation 3. The exception is $InvOPEX_{ist}$, which denotes reversible, smaller operating expenses rather than lumpy firm investments - measured in million R$. I have cited USD figures occasionally throughout the text, but where R% is mentioned, this refers to the Brazilian currency. This variable is calculated as the value of all reversible operating expenditures reported by a utility on their income statements – rather than capital expenditures reported on the cash flow statement. Examples of the line items that are aggregated include refundable management fees paid to consultants and stand–alone equipment such a meters

procured by suppliers which can be returned.

If the coefficient describing the impact of the tariff flag on marginal, reversible investments is insignificant, then this would hold at two key consequences. First, this result would support the this paper's reliance on the tariff flag policy as a plausibly exogenous policy event: If the tariff flag was caused by unobserved factors that also influenced firms' propensity to undertake investments, then this confounder would presumably also lead to increases in reversible investments. Second, if uncertainty only affects lumpy investments and to a lesser extent marginal investments, then this asymmetric impact would support the comparative predictions outlined in section 1: if firms are risk averse, then an increase in uncertainty affects marginal decisions through the discount rate, but affects lumpy decisions through both a higher discount rate and a higher option value.

Combining insights from tables 6 and 8, it appears that as opposed to irreversible investments, reversible operating expenditures as expected did not respond statistically significantly at the 90 percent confidence level to the tariff flag policy. The average response indicates that the tariff flag resulted in a statistically insignificant increase by 0.451 units, or R$ 451,000, of ongoing maintenance expenditures, relative to the control group and pre–tariff flag years. With average annual operating costs net power purchases of R$ 261 million as seen in table 2.5, this would amount to an increase by 0.17 percent. Traditional approaches to optimal investment decisions suggest that there should be some negative impact of rising uncertainty on reversible investments. Our data show that this is not the case empirically. One possible reason for this is that movements in marginal and lumpy investments are negatively correlated and that lumpy investments are prioritized over marginal investments when uncertainty is perceived to be lower.

Table 2.8: Estimation of Impact of Uncertainty on Reversible Expenditures

	Dep. var: Rev. Operating expenditure		
	(1)	**(2)**	**(3)**
Flag × Post	0.700*	0.577	0.451
	(0.342)	(0.346)	(0.379)
Post	0.565	0.471*	0.399*
	(0.342)	(0.225)	(0.178)
Year FE	X	X	X
Municipality FE		X	X
State–level time trends	X	X	X
Controls			X
R Squared	0.54	0.58	0.59
Observations	35,025	35,011	34,908

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.Table reports impact of policy–induced uncertainty reduction on reversible operating expenses (in R $), estimated by OLS. Standard errors in parentheses are clustered by electricity firm. Fixed effects by municipality, year and state–specific trends are included.

While these results tell us how the number of lumpy investments responds to changes in uncertainty, we are also interested in the value of lumpy investments. I repeat the baseline estimation of table 6 and the following equation:

$$\begin{aligned} InvVal_{ist} = \alpha + \beta_1 \cdot (Post_t \times Flag_i) + \beta_2 \cdot Post_t + \beta_3 \cdot Flag_i + X'_{ist}\beta_4 \\ + \gamma_i + \delta_t + t_s + \epsilon_{ist}, \end{aligned} \tag{2.7}$$

where all variables are as previously defined, except for $InvVal_{ist}$, which denotes the value in R$ of irreversible investments in municipality i, state s and year t.

Table 2.9 shows that the R$ value of lumpy investments increased statistically significantly by R$855,000 for the treatment firms relative to the control firms and the pre–treatment years. The investment–inducing effect of the tariff flag policy on the number of investments therefore qualitatively matches the reaction of the value of investments, further lending evidence to the workings of real options theory, and to the significance of the government policy for utility behaviour.

Table 2.9: PPML – Impact of Uncertainty on Irreversible Investment Value

	Dep. var: Lumpy investment value		
	(1)	**(2)**	**(3)**
Flag × Post	0.801*	0.823**	0.855**
	(0.323)	(0.245)	(0.221)
Post	0.564	0.542*	0.559**
	(0.342)	(0.225)	(0.178)
Year FE	X	X	X
Municipality FE		X	X
State–level time trends	X	X	X
Controls			X
R Squared	0.61	0.68	0.67
Observations	36,116	37,451	36,988

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Table reports impact of policy–induced uncertainty reduction on the value of lumpy investments in R$, estimated in a PPML model. Standard errors in parentheses are clustered by electricity firm. Fixed effects by municipality and by year are included.

2.8 Alternative explanations

A potential challenge to interpreting the empirical results is that the increase in lumpy investment expenditure may also be due to effects other than a reduction in uncertainty. The 2015 policy change is akin to moving away from price–cap regulation towards rate–of–return regulation. Under the latter scheme, firms are allowed an electricity price that ensures a sufficient rate of return on capital investments made by the utility, and generation cost variations are passed on to end–users. In comparison to price–cap regulation the constrained capital return level can lead firms to increase their capital to labour ratio – the Averch–Johnson effect. A rising capital–labour ratio does not imply an increase in the quantity of capital investments (which includes lumpy investments) if the quantity of labour declines. Still, a rise in lumpy investment expenditure could be consistent with an Averch–Johnson–type mechanism. Due to a lack of data I am unable to directly rule out the impact of rate–of–return regulation on the interpretation of the results. However, there are important alternative explanations that I am able to explicitly test for.

It is conceivable that treated electricity firms entered into the drought period with stronger balance sheets, and/or easier access to external financing. Easier access to finance and healthier balance sheets could in turn allow certain firms to recover from the 2014–2015 generation cost crisis more quickly. When firms require access to financing, their balance sheet health can influence a firm's cost of and possibility for accessing additional funding. Access to funding can in turn lead to relatively higher (lumpy) investment responses starting in the recovery year 2015 for these firms. If these dynamics are involved and co–move with the treated–non–treated boundary, then the estimated increase in lumpy investment activity among the treated firms may not be solely attributed to an improvement in uncertainty.

In order to examine this possibility, I obtain additional data on the balance sheet strength of the treated and non–treated firms in 2013 – the year preceding the crisis. The data are only available for a subset of firm–year observations (40 percent of the observations summarized in the results table 2.6), and for this reason the I exclude the variable from the main estimations of the paper to avoid a reduction in the sample size, which would reduce the precision of the estimates.

The net debt to EBITDA (earnings before interest, taxes depreciation and amortization) ratio measures the extent to which debt obligations less cash and cash equivalents outweigh a firm's earnings before any losses on capital equipments are taken into account. All else equal, a higher ratio implies that a firm has fewer earnings available to service higher debts owed. The average net debt to EBITDA ratio for the subset of treated firms lies at 3.9, compared to 6.3 for the smaller sample of non–treated firms. This differential implies that non–treated firms had on average less sturdy balance sheets heading into the crisis. It is therefore possible that this difference in the ability to service debt through income earned is driving at least part of our results.

However, while financing differences might explain part of the main results of this paper, the earlier insights on the marginal investment response are inconsistent with the hypothesis of a strong balance sheet effect. If weaker balance sheets make it harder for firms to recover from generation cost pressures, then this effect should have an impact on ongoing maintenance expenditures in addition to lumpy replacements and constructions. The fact that only lumpy investments increase for treated firms relative to non–treated firms and the pre–policy period suggests that balance sheet dynamics might have a limited impact on the interpretation of the results.

There are two additional explanations for the results that are difficult to account for quantitatively. First, it is plausible that the increase in lumpy investment activity observed among treated firms relates to a backlog, i.e. an accumulation of projects during the crisis years. If treated firms experience a larger backlog than control firms ahead of 2015, then this could explain the investment patterns observed in the data subsequently. It is unclear what would lead the firms that received permission to act upon the tariff flag to hold back on investments to a higher degree than control firms. The above dissection of firms' balance sheets suggests the contrary: control firms faced a higher degree of difficulty in repaying their debt obligations. It appears more likely that these more distressed firms would have a higher incentive to withhold lumpy investments until a recovery in generation conditions. The results could also be driven by firms accelerating investment projects after 2015, if they perceived a risk of government expropriation of their retained earnings. In order for this effect to be driving our results, non–treated firms would have to have experienced a (perceived) increase in expropriation risks relative to treated firms and to the pre–2015 period. Expropriation risks remain a potential alternative explanation of the results.

2.9 Threats to identification

In the employed diff–in–diff strategy, the key identifying assumption is that conditional on all controls and in the absence of the uncertainty shock lumpy investment

behaviour would not have diverged between the treated and remaining firms starting in 2016. There are several possible threats to this assumption.

The most important question underlying the admissibility of my diff–in–diff set–up is: *why, if the treatment and control firms are similar, did some received the flag and others not?* This is important because the set of treated utilities may have been selected ex–ante by the Brazilian government to receive the flag based on initial conditions which could affect lumpy investments irrespective of the uncertainty event. It is crucial to guard against the concern that there exist qualities shared by treated electricity firms, rather than the 2015 policy experience, that are driving subsequent lumpy investment activity.

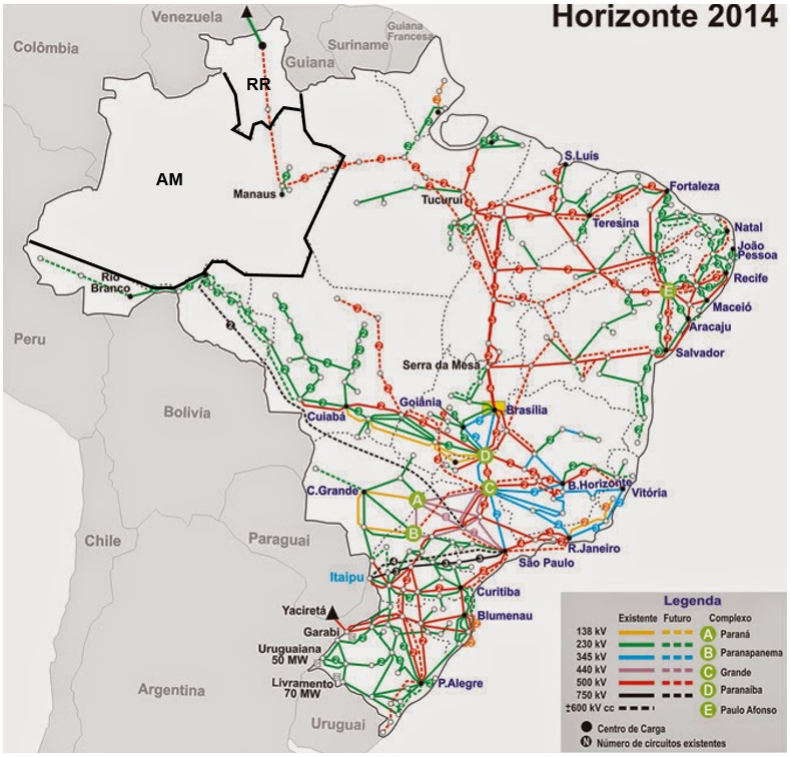
As an example, if the treated region was most severely affected by the drought in ways not captured by the control variables, it is plausible that damages to the distribution grid induced by weather and variability in the power supply had a greater effect on treated electricity firms than those outside of major drought region. These affected firms could have responded to such weather–induced damages by ramping up lumpy repair investments in the distribution grid. In this case the presented empirical findings would have been due to firms' responses to exogenous environmental shocks rather than policy–induced uncertainty changes. A related point is that even if this self–selection problem is absent in my data, it is possible that there were random events that occurred in 2015 and only affected the treated firms. My aim is to make the case that these threats are unlikely to be significantly driving the results of this paper.

From a contextual perspective there is evidence that (self–)selection by firms into the tariff flag system – in a manner that correlates with the outcome variable – cannot be evidently argued for. The evidence focuses on the criteria behind the partial coverage of the tariff flag. The states that did not receive the tariff flag are Roraima and Amazonas, which contain 441 municipalities. The officially

communicated reason for their exclusion from the price transparency initiative was that "the non–recipient regions were not connected to the national transmission grid at the time" (Agéncia National de Energia Elétrica, 2018). This selection criterion is consistent with ANEEL's communicated tariff flag objective of managing electricity consumption by signalling prices ahead of time. The decision to exclude non–transmission grid connected firms from the flag system, coupled with the focus on power consumer behaviour, suggests that treated firms were selected based on relatively fixed pre–existing characteristics, rather than variable future investment needs. The decision–maker – the electricity regulatory agency – thus appears to have been targeting criteria other than the outcome variable when implementing the flag policy. This set–up makes it more likely for the demonstrated effect on lumpy investments to have been a by–product of the policy intervention, rather than its criterion. It is plausible that despite the official thinking process additional unobserved considerations that affect future lumpy investments could have affected the coverage of the flag mechanism.

Another potential confounding factor is corruption. Politically favoured states could receive the tariff flag mechanism as a benefit in return for electoral support. Specifically, federal audit reports have revealed that nearly 80% of monitored municipalities experienced at least one incident of corruption (Ferraz and Finan, 2011). If favoured municipalities or states were given the tariff flag mechanism (treatment), while also being supported in other ways that increased firms' investment outcomes after 2015 (outcome) then this would link the treatment and outcome in ways unrelated to he uncertainty channel. Figure 2.6 confirms that the two states that did not receive the tariff flag mechanism – Roraima (RR) and Amazonas (AM) – are both located in the North–western corner of Brazil, and were as of 2015 – the year of the flag – not interconnected with the national transmission grid. The spatial characteristics of the treated states confirm that treatment assignment was determined by the pre–determined geography of these two states relative to the others, and that time–varying corruption effects are unlikely to confound my

Figure 2.6: Spatial Location of Control and Treatment States



Notes: Map depicts Brazilian states, of which Roraima (RR) and Amazonas (AM) are shown as not interconnected to the transmission network as of 2014 (dashed transmission lines depict planned connections, while solid transmission lines depict existing connections).

estimates.

To address any potential additional latent confounding factors, I implement a number of statistical tests in addition to the rhetorical case I have made for "natural experiment" feature of the 2015 events that underpin my diff–in–diff strategy. The first is a parallel trends test.

2.9.1 Structural break test

First, I borrow from time–series econometrics and test for a structural break in the regression coefficient β_1 around the true date of the uncertainty event in 2015. The structural break test allows me to confirm whether there were any divergent tendencies between the treated and the control groups *before* the introduction of the flag traffic light system that could have influenced selection into the tariff flag as

well as lumpy investment expenditure. If there are no structural breaks affecting the difference in lumpy investments between the treated and control groups prior to and after 2015, then this shows that there are unlikely to have been other concurrent trends leading up to the flags that would strengthen the uncertainty–investments relationship. I employ the Quandt Likelihood Ratio Test based on F–tests for multiple unknown structural breaks. Akin to Greenstone (2014) I choose not to assume a known break point in order to achieve a stronger test of whether the 2015 event caused a shift in coefficient of interest. In order to undertake a structural break type analysis, the following equation is estimated in 7 different versions

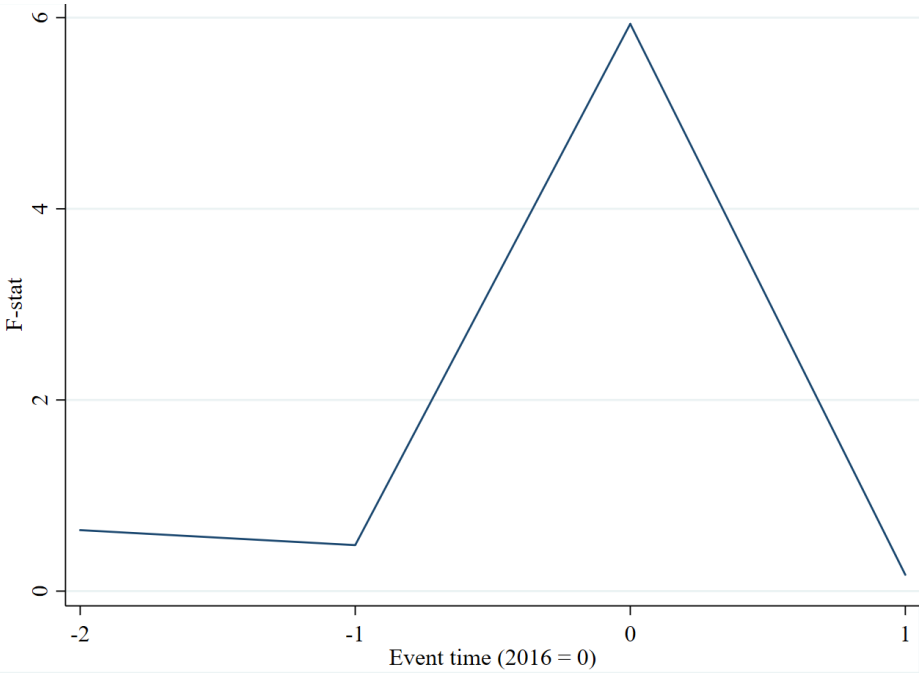
$$InvLump_{ist} = \alpha + \beta_1 Flag_i + \gamma(D_t \times Flag_i) + X'_{ist}\beta_2 + \delta D_t + \epsilon_{ist}, \tag{2.8}$$

where D_t is replaced in each equation by a Dummy that takes on the value of 1 in year 2011 through 2017 in turn. For each of the versions of equation 6 the test then computes the F–statistic over the range of break years, and selects the maximum of said statistics, the Quandt Likelihood Ratio Statistic. The null hypothesis states that $\gamma = 0$. If the resulting probability is greater than the chosen threshold, then I cannot reject the null hypothesis of stable coefficients. A structural break therefore requires rejection of the null hypothesis.

Figure 2.7 depicts the resulting F–statistics for each of the possible breakpoints between 2013 and 2016. The test selects the year 2015 – the year of the tariff flag introduction – as harbouring a significant break with an F–statistic of about 5.935 and a probability value of 0.024, thus supporting the results of the diff–in–diff estimation. Lumpy investment activity by treated electricity firms seems to have diverged significantly from control firms no earlier or later than 2015, the year in which the tariff flag was introduced.

There is a lingering risk to the causal identification strategy I have employed in this diff–in–diff setting. The results of the structural break test I have conducted could hold true under the following scenario. If there was a simultaneous event

Figure 2.7: QLR F–Statistics for Structural Break Test



Notes: Figure depicts the F–statistic for the possible breakpoints 2014, 2015, 2016 and 2017, computed as part of the Quandt Likelihood Ratio test. Years 2011 through 2013 serve as data outside of the test window.

other than the tariff flag that affected only the treated electricity distribution firms after 2015, then even in the absence of differential pre–flag trends the relationship would be contaminated by unobservables. Such factors would not necessarily show up as pre–existing divergent trends or structural breaks in non–treatment years.

2.9.2 Matching on observables

To alleviate this concern I employ a matching–on observables approach. I first match the treated municipalities to the most similar untreated municipalities and re–estimate the main PPML equation based on this reduced, matched control group. The idea is that if I have cleanly identified the key relationship in equation 3, then the coefficient estimates from the PPML estimation should continue to hold when the model is re–estimated based on the re–matched control group. This is because municipalities' exposure to geographical events, economic changes and political trends that affect lumpy firm investments – is being held closer to constant between the treatment and new control group post–matching. In other words, if we believe

that the treated municipalities are somehow self–selected into treatment due to their characteristics, and if these special characteristics are at least in part continuous (non–categorical), then we want to find a control group for them that is as similar as possible to them in those traits and see if our results weaken. If the relationship vanishes or diminishes after this adjustment, then this would indicate that there are factors that differentiate the treatment firms from the control firms.

There exist a multitude of options for obtaining matches. I decide to match on propensity scores using the nearest–neighbour technique to ensure sufficiently high ex–post balance of the treated and matched control firms. A propensity score is the conditional probability of receiving a treatment – in this case the tariff flag – given a set of observable variables. It ranges from 0 to 1, where 1 suggests complete certainty that the observation received treatment. The propensity score allows us to ask how probable it is for a municipality to have received the tariff flag based on its characteristics compared to the treated municipalities, even if in reality it did not receive the flag. Within this family of methods my choice of a particular technique is motivated by the minimization of assumptions over the functional form underlying the propensity score estimations. While the log–linear specification of the logistic regression is the most commonly employed functional form for estimating the probability of being treated conditional on the observable covariates, the assumption of linearity in the logistic has been highlighted as problematic. Hence, in addition to a parametric binomial model I opt for a non–parametric method for estimating the propensity scores using a random forest statistical learning algorithm. The algorithm works by repeatedly splitting the data into subsets so as to minimize the residual sum of squares (i.e. the error) when predicting treatment assignment using the selected regressor(s). Each split is characterized by a newly selected random sub–set of regressors used to predict treatment assignment. This is to force the resulting regression trees obtained by randomly drawing subsets of the dataset to vary from one another in the ordering of regressors by split, creating a random forest.

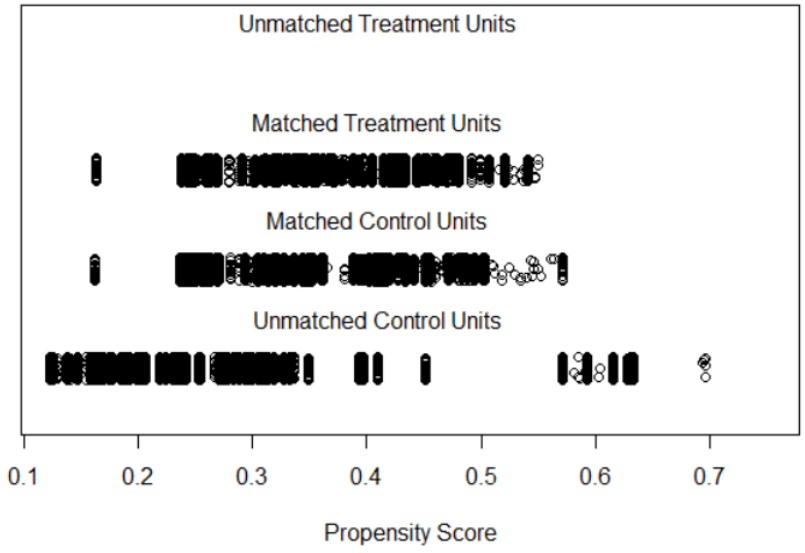
The resulting propensity scores are then used to match treated municipalities to untreated municipalities using the nearest neighbour technique without replacement. In choosing the option of non–replacement, I follow Smith and Todd (2005), who suggest that the distribution of propensity scores by matched treatment and control group should be plotted. If the distribution is markedly similar, replacement is deemed unnecessary. I include the control variables, as well as data on utility debt sustainability (measured as the debt service coverage ratio), municipality–level GDP growth data and state–level political governance quality indicators in the matching procedure, albeit restricted to pre–2015 data. Following standard practices in the literature I restrict the data in this way in order to ensure that I am matching based on pre–treatment characteristics. Data on distribution losses by electricity firm for the years between 2011 and 2017 capture the need for urgent repairs. Industry practitioners have indicated that distribution losses are mainly comprised of the following factors in descending order of importance:

- Variable technical losses created by the heating impact of electricity
- Fixed technical losses that are a characteristic of the network infrastructure
- Non–technical losses related to electricity theft and un–metered supplies

The resulting propensity scores from matching in the logistic specification are depicted in Figure 2.8 for the matched and unmatched municipalities. The distribution of propensity scores of control units that were matched to each treatment unit differ markedly from the distribution of propensity scores for control municipalities that were not matched. The plot also confirms that the option of matching without replacement of control units has produced close matches. The matched control municipalities now serve as the alternative control group.

To assess whether the results of the PPML model estimations are robust to restricting the sample of control municipalities to the nearest–neighbour–observations, equation 3 is re–estimated. The results provide no evidence of selectivity of electricity

Figure 2.8: Distribution of Propensity Scores Estimated by Logit



Notes: Figure depicts the distribution of propensity scores obtained from nearest neighbour logistic matching, for each observation. Each observation is a municipality. The propensity score allows us to ask how probable it is for a municipality to have received the tariff flag based on its characteristics compared to the treated municipality, even if in reality it did not receive the flag. A score of 1 suggests complete certainty that the municipality would have received the tariff flag.

firms into the price–signalling mechanism (Table 2.10). Compared to the unmatched PPML model estimates of table 2.6, the coefficients are no smaller in magnitude when controlling for the full set of fixed effects and regressors (column (3)). From a significance perspective, the impact of uncertainty on electricity firms' irreversible investment decisions is reduced but remains significant at the 10 percent level. Next, I turn towards the results from the random forest algorithm for computing propensity scores as an alternative approach.

An advantage of the method is that it allows for assessing the importance of individual regressors in predicting treatment assignment. For a given tree, the variable that is used to partition the sample in the initial splits has the highest predictive power when estimating the propensity score (i.e. minimizes the mean–square error of prediction). Thus the observables can be ranked according to their use for splitting higher up in the trees. The resulting ordering is depicted in Figure 2.9 for the most important covariates, with importance measured as the reduction

Table 2.10: Impact of Uncertainty on Irreversible Investments (Logit Matches) – PPML

	Dep. var: Lumpy investment activity		
	(1)	**(2)**	**(3)**
Flag × Post	0.614**	0.311*	0.155*
	(0.298)	(0.151)	(0.065)
Post	0.743*	0.311*	0.155
	(0.358)	(0.149)	(0.085)
Year FE	X	X	X
Municipality FE		X	X
Controls			X
R Squared	0.68	0.70	0.70
Observations	23,032	23,032	23,032

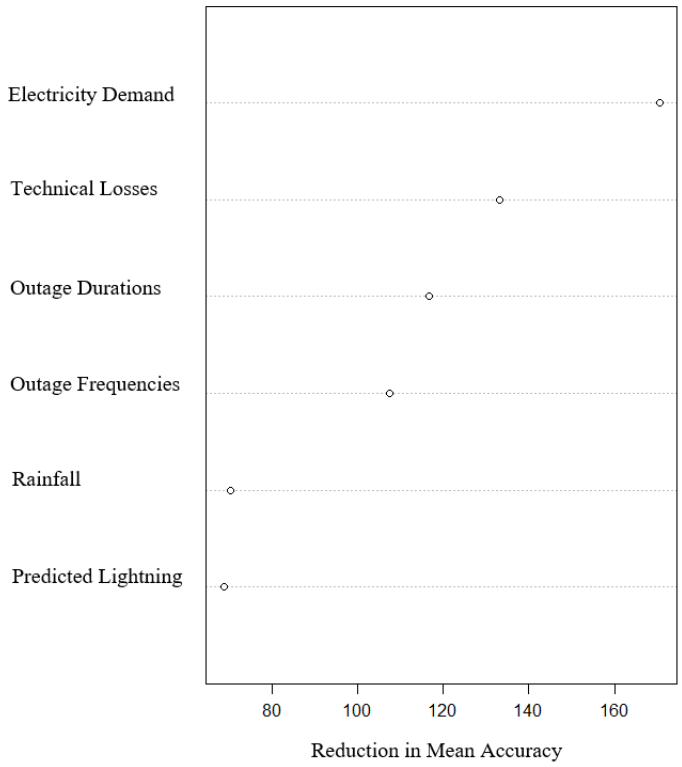
Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.Table reports impact of policy–induced uncertainty reduction on the number of irreversible investments, estimated in a PPML model. Standard errors in parentheses are clustered by electricity firm. The sample of control municipalities has been restricted to the nearest–neighbour–matches based on logistically estimated propensity scores. Fixed effects by municipality and year are included.

in the prediction accuracy that would occur if it were excluded from each regression tree, averaged over all trees. While the absolute values are not to be interpreted, the ranking of variables is noteworthy. The result confirms the suspicion that technical losses in the distribution grid are a potential predictor of whether an electricity firm received the price signalling mechanism in 2015.

Re–estimating equation 3 based on the re–matched control group following the random forest algorithm yields remarkably similar results to the re–matched logistic estimation as well as the baseline results from table 2.6. In particular, the number of lumpy distribution grid investments increases by $(e^{0.122} - 1) * 100 = 12.9$ percent for treated firms relative to the control firms and to the pre–treatment period, conditional on receiving the price signalling mechanism (Table 2.11, column (3)).

In addition to restricting the control group to matched municipalities I also obtain the nearest neighbour matching estimator of the average treatment effect on the treated – the average of differences in outcomes between each matched

Figure 2.9: Importance of Selected Covariates for Generating Random Forest Splits



Notes: Figure depicts the average absolute reduction in the predictive power (increase in the mean–square error) resulting from the exclusion of each variable from the random forest algorithm. The ranking, rather than the absolute value of the mean accuracy reduction, should be noted.

treatment–control pair. Table 2.12 reports these, revealing that the uncertainty improvement increased the number of lumpy investments by $0.535/3.73 = 14$ percent on average for electricity firms exposed to the tariff flag relative to their peers and the pre–flag years. Overall, this exercise has revealed that matching on key observables including technical distribution losses does not weaken the conclusions of the preceding empirical analysis.

Crucially, the matching techniques employed are not robust to treatment assignment that is predicted by *unobservables*, i.e. a violation of the main identifying assumption of orthogonality of treatment assignment to confounders. To assess how sensitive the results are to various degrees of such selection on unobservables, I conduct sensitivity analysis using the bounding approach by Rosenbaum (2002).[3]

[3]Rosenbaum's approach is implemented using code generated by Sascha Becker and Marco

Table 2.11: Impact of Uncertainty on Irreversible Investments (Random Forest Matches) – PPML

	Dep. var: Lumpy investment activity		
	(1)	**(2)**	**(3)**
Flag × Post	0.456**	0.314*	0.122*
	(0.150)	(0.148)	(0.057)
Post	0.355	0.436*	0.298
	(0.250)	(0.211)	(0.190)
Year FE	X	X	X
Municipality FE		X	X
Controls			X
R Squared	0.68	0.71	0.70
Observations	23,134	23,134	23,134

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Standard errors in parentheses are clustered by electricity firm. Table reports impact of policy–induced uncertainty reduction on the number of lumpy investments, estimated in a Poisson model. The sample of control municipalities has been restricted to the nearest–neighbour–matches based on propensity scores estimated via the random forest algorithm.

Table 2.12: Nearest Neighbour Matching Estimates – Comparison of Means

	Dep. var: Log Lumpy investment activity				
	Treated	**Controls**	**Difference**	**S.E.**	**T-Stat**
ATT	1.905	1.370	0.535	0.023	39.53

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Table reports the average treatment effect on the treated as the result of a comparison of mean outcome values between each treatment municipality and its nearest neighbour match. Propensity scores are estimated via logit.

This procedure allows me to determine how strong the influence of an unobserved variable on the selection process would have to be in order to contaminate the results of the matching process. Table 2.13 reports the significance of the parametric matching results as a function of the odds of being assigned to treatment due to unobservables.

A value of 1 implies no bias from unobservables influencing selection into the tariff flag, while a value of 2 implies a bias that would double the odds of being selected for

Caliendo.

Table 2.13: Sensitivity of Estimates to Relaxation of Unconfoundedness Assumption

Gamma	1.0	1.2	1.4	1.6	1.8	2.0
P-Value	0.024	0.054	0.073	0.177	0.340	0.508

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Table reports the p–values associated with varying levels of odds of differential assignment due to selective treatment assignment, in the case of the logistic estimation of propensity scores. Higher "Gammas" imply higher odds of bias introduced by selection based on unobservables. Calculations are based on Rosenbaum's 2002 bounding approach. The underlying statistical function was written by Sascha O. Becker and Marco Caliendo.

the tariff flag mechanism for the treated relative to the control municipalities. The results show that the post–matching estimates would be insensitive to unobservables that would create a difference between the odds of selection into treatment of the treatment and control groups of 1.4. Beyond a difference between odds ratios of 1.4, the confidence interval for the estimated treatment effect could include 0. While this exercise cannot control for the potential effect of unobserved initial conditions that may have led to increases in lumpy investments by treated electricity firms, the results provide some insights into how sensitive the main findings would be to violations of the identifying assumption.

2.9.3 Standard error adjustments

As a final robustness check, I make adjustments to the computed standard errors. I address a well–known criticism of standard difference–in–difference strategies as carried out by Bertrand, Duflo and Mullainathan (2004). Accordingly, even when standard errors are clustered conservatively, i.e. by the most aggregated level of treatment (by electricity firm in this study), the resulting errors may be biased downwards, leading to falsely rejected null hypotheses. The main reason for this is serial autocorrelation of the outcome and treatment variable $Post_t$, with high positive autocorrelations resulting in underestimated standard errors.

The results presented in this study are less prone to the dangers outlined for the following reason: while the average difference–in–difference estimations employs

Table 2.14: Autocorrelation–Robust Impact of Uncertainty on Irreversible Investments – PPML

	Dep. var: Lumpy investment activity		
	(1)	**(2)**	**(3)**
Flag × Post	0.224	0.367*	0.313*
	(0.417)	(0.155)	(0.141)
Flag	0.322	0.167*	0.255
	(0.257)	(0.081)	(0.155)
Year FE	X	X	X
Municipality FE		X	X
Controls			X
Observations	35,112	35,112	35,112

Notes: * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$. Table reports impact of policy–induced uncertainty reduction on lumpy investments, estimated by PPML. The data have been collapsed into a single pre–shock time observation (the average of 2011–2014) and a single post–shock time observation (the average of 2015–2017). Standard errors in parentheses are autocorrelation–robust and clustered by electricity firm.

more than 10 years of data, this study focuses on the period from 2011 through 2017, so 7 years of data. As a result the time–dimension, and thus serial correlation, carries less weight in the estimation than in a difference–in–difference setting with large T. Nevertheless, I ignore the time–series dimension of the data–set, collapsing the data into a single pre– and post–2015 year. Equation 3 is then re–estimated via PPML, with results reported in table 2.14. After accounting for serial correlation of the treatment indicator variable and lumpy investment activity, the coefficient of interest remains statistically significant at the 10 percent level (column (4)). The magnitude of the effect is reduced by 2.1 percentage points to 0.313 percent. The estimates suggest that even in a conservative specification from the perspective of standard errors, the main message holds: the flag policy led exposed electricity firms to increase their lumpy investments in electricity distribution relative to the untreated firms and the pre–2015 period.

2.10 Conclusion

In this paper I have sought to provide one possible explanation for the co–existence of high electricity prices and frequent power outages in many developing countries that is rooted in real options theory: policy–induced improvements in uncertainty over future profitability can lead electricity firms to ramp up irreversible investments in the distribution grid. These distribution investments have been shown in the literature to affect infrastructure quality as measured by outage frequencies and durations.

Using Brazilian power sector data, I exploit a pseudo–experiment set–up that was in part triggered by the 2013 drought. I interpret the introduction of a tariff flag mechanism as an improvement in the predictability of future retails electricity prices by power providers. I find that firms exposed to this policy responded by significantly increasing their irreversible investments in the distribution grid: sunk cost investments in the form of substation replacements and constructions increase for these power providers relative to unexposed firms and to the pre–policy era.

I perform a number of tests and provide qualitative evidence to strengthen my case for the validity of the results in a diff–in–diff setting. These include parametric and non–parametric methods for matching treatment and control groups on observables, structural break tests as well as standard error adjustments for serial correlation of the residuals.

The insights are consistent with existing research on the investment–deterring effects of uncertainty and real options theory. The results also suggest that firm beliefs over the predictability of power prices have real impacts on the policy variables that regulators seek to control in developing and emerging economies. The impact of uncertainty has to my knowledge not previously been documented as a potential cause for deficient infrastructure quality across the developing world. If the phenomena highlighted in this paper hold true across other power markets, then this

would have implications for the optimal policy design in these regions, as high tariff levels alone may still lead to deficient investments in distribution infrastructure.

3

Long–Term Relationships between Sovereign and Corporate Bond Yields: Evidence from Emerging Markets

Abstract

Access to foreign borrowing is becoming an increasingly important outside funding source for emerging market firms: the amount of outstanding corporate external debt surpassed its sovereign counterpart in 2012 and continues to increase in relative importance. I first establish that emerging countries' breathing space for running a primary fiscal deficit has been shrinking over the past 10 years: periods in which the median economic growth rate across emerging economies exceeded the rate payable on sovereign hard currency bonds have become shorter–lived since 2000. I investigate how this trend could affect firms through the channel of investor beliefs. Using weekly bond–level data for 46 emerging markets over 2016–2019, I find that the majority of emerging market bond yields display unit–root qualities. Employing a model that accounts for this mix of time–series properties, I discover that sovereign and corporate bond yields exhibit a long–run relationship that is not explained by economic conditions, firm fundamentals, bond properties and external factors affecting emerging countries as a whole. I focus on perceived *transfer risks* as a channel, i.e. the likelihood in investors' eyes that sovereigns will transfer some of their repayment burden to private foreign creditors. Transfer risks lead corporate and sovereign bond yields to co–move and generally converge. The effect on firms' borrowing costs is strongest when sovereign bond yields are rising, during periods when firms can borrow more cheaply than their "associated" governments and when the real interest rate exceeds the growth rate.

3.1 Introduction

Access to borrowing is becoming an increasingly important external funding source for emerging market firms: the amount of outstanding corporate external debt surpassed the value of bank loans in 2011, started exceeding its sovereign counterpart in 2012, and quadrupled from 2000 to 2014 (Management, 2017; Caballero, Fernández and Park, 2019).[1]

At the same time, sovereign external debt financing is taking up an increasing share relative to the size of emerging market economies.[2] With sovereign emerging market debt dominated by external obligations, and its OECD counterpart by domestic obligations, I begin by focusing on recent trends in overall debt levels (so domestic plus external public debt) (Stiglitz and Rashid, 2020). While median debt to Gross Domestic Product (GDP) ratios have dropped by almost 15 percent in OECD economies since 2013, the ratio increased by 28 percentage points in emerging economies since hitting a trough in 2008 (figure 3.1). This emerging country dynamic is reflected in coupon payments taking up the highest share in GDP since 2005 (Heathcote and Mulheirn, 2020).[3]

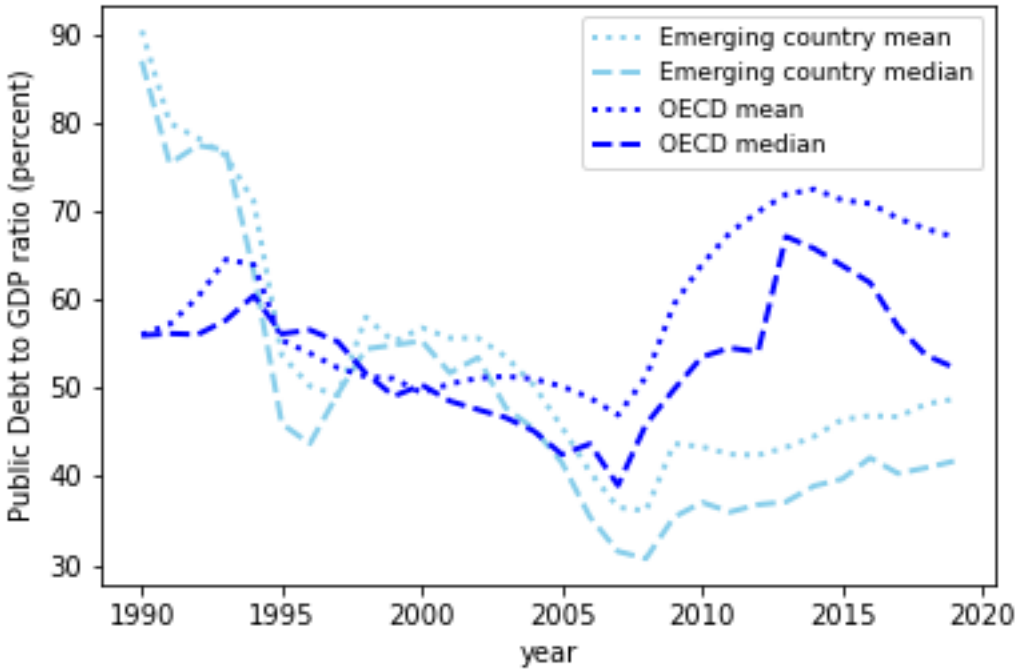
Although median sovereign debt to GDP ratios in emerging markets are well within historical ranges, I find that this metric can mask the true risks to the borrowing costs of firms in these economies. Specifically, periods in which the median economic growth rate across emerging economies exceeded the rate payable on sovereign hard currency bonds have become shorter–lived since 2000. Further, I

[1]Our working definition of emerging economies includes the countries that form part of the MSCI Emerging Markets Index as of February 2020. These include: Argentina, Brazil, Chile, China, Colombia, Czech Republic, Egypt, Greece, Hungary, India, Indonesia, Korea, Kuwait, Malaysia, Mexico, Pakistan, Peru, Philippines, Poland, Qatar, Russia, Saudi Arabia, South Africa, Taiwan, Thailand, Turkey and United Arab Emirates.

[2]Sovereign debt is in this paper treated as the total gross amount of bond liabilities outstanding, with the issuer being the central government and sub–national government entities in a given country. This notion of government debt excludes publicly owned enterprises, which are captured in the database as corporate issuers with government–dependence. I treat hard currency and external debt as identical, even though hard currency debt may be held by domestic creditors.

[3]The analysis is based on data that pre-dates the start of the Covid–19 pandemic, which altered the level and composition of emerging market debt.

Figure 3.1: Evolution of Public Debt to GDP, across emerging country grouping



Notes: Figure depicts the mean and median across emerging markets and OECD countries of the share of public debt in GDP. Public debt includes both domestic and foreign currency denominated debt. IMF Historic Public Debt Database.

find that the wedge between yields and growth rates has been narrowing during each green–shaded episode since 2000. If these historical trends are reliable indicators for future projections, this potential shrinking of the maximum sustainable primary fiscal deficit suggests a more challenging debt environment for emerging economies than implied by low bond yields alone.

This paper investigates the following questions: how could a future deterioration in governments' debt sustainability adversely affect firms' borrowing costs through the transfer risk channel? What kinds of firms are more likely to be impacted by changes in sovereign bond yields? And finally, under what broader economic conditions are firms more vulnerable to be impacted by transfer risks? To answer these questions, I test the following 6 hypotheses:

- H1: Bond yields may not be stationary.

- H2: There exists a long–run relationship between sovereign and corporate bond yields that is not explained by economic conditions, firm fundamentals, bond properties and external factors affecting emerging countries as a whole.

- H3: This relationship is driven by transfer risks.

- H4: If sovereign bond yields exceed corporate bond yields, we would expect increasing *perceived* transfer risks to eventually lead to convergence. This implies a larger speed of conversion.

- H5: If sovereign bond yields are rising, we would expect increasing *perceived* transfer risks and thus a larger speed of adjustment to the long run.

- H6: Moving into a $r > g$ phase is associated with a stronger link between sovereign and corporate bond yields.

Most of the empirical work on the link between sovereign and corporate borrowing risks can be divided into explorations of the transfer–risk and credit–ratings channels. Focusing on the transfer–risk channel, based on a dataset of 65 corporations in 10 emerging market economies, Cavallo and Valenzuela (2010) study the pass–through from sovereigns to corporates, and find that it is less than 1 to 1. Augustin et al. (2018) quantify spillover effects from sovereign to corporate credit risks for Euro–zone firms. The authors are among the few to pay attention to the confounding impact of government–dependence on the sovereign–corporate pass–through.[4]

On the other side, Durbin and Ng (2005) explore the sovereign rating channel, and find that sovereign bond yields in firms' "home" countries often – but not always – impose a lower bound on their corporate yields. Almeida et al. (2017) similarly focus on the impact of sovereign ratings on syndicated loan yield spreads.

[4]Government–dependence of corporate entities is defined in the database as instances involving explicit guarantees from the government and where firms are legally majority government–owned.

There also exists a body of literature which does not specifically focus on either transmission channel, but nevertheless documents a sovereign–corporate relationship. Here, Agca and Celasun (2012) study borrowing costs in emerging economies, finding that syndicated loan yield spreads are higher in countries with higher public external debt. Bevilaqua, Hale and Tallman (2020) find a non–linear relationship between sovereign and corporate local currency bond yields in emerging economies that weakens in high–risk periods.

Against this backdrop, this paper offers 5 contributions to understanding the role that sovereign yields – and investor beliefs – play in determining corporate borrowing costs in emerging economies. First, Dittmar and Yuan (2008) is the only empirical paper that has discussed the *long–run* relation between sovereign and corporate bonds prior to this analysis. While they focus on credit spreads, focusing on bond yields instead allows me to circumvent dropping all firms with an insufficient number of issuances – this condition is a requirement for estimating credit spreads from bond yields. In avoiding such data truncation I prevent biases in the estimated coefficients that could be caused if for example firms that issue multiple bonds are less likely to be affected by transfer risks.

Next, I investigate and deal with the time–series properties of bond yields in our emerging market dataset. This is a crucial precondition for applying the appropriate model to potentially non–stationary series. Existing studies either take first–differences of the yield data, thus omitting level information (Almeida et al., 2017), or they examine bond spreads rather than bond yields (Cavallo and Valenzuela, 2010; Augustin et al., 2018)– which can no longer be interpreted as reflecting total borrowing costs – or they estimate a model of bond yields in a level specification without accounting for the potential spurious nature of the resulting estimates (Bevilaqua, Hale and Tallman, 2020). I avoid the disadvantages of these approaches by employing an autoregressive distributed lag (ARDL) model. The key advantage is that the ARDL setup can be rewritten into error–correction

form, and based on this specification allows for ex–post testing of the existence of a cointegration relationship. Most importantly, the Bounds testing procedure does not require knowledge of whether the individual bond yields are $I(0)$ or $I(1)$ (Pesaran, Shin and Smith, 2001).

Thirdly, standard panel data approaches to non–stationary data assume that there are no cross–linkages between interconnected economies. Such cross–section dependence can occur when economies are affected by the same or similar external shocks but in different ways. This is relevant given the impact of changes in developed market rates – notably the United States – on emerging economies. Interlinkages between countries can also occur when there are direct economic and political ties in the form of trade, common currency zones and geographical links. Such interlinkages between countries can lead to cross–dependent error terms in the estimation. To test for the robustness of our results to these country–linkages, I employ a cross–sectionally augmented distributed lag model. This means that I can control for cross–country and cross–bond common shocks that cause them to correlate. I am able to do so due to the high dimensionality of the dataset, which spans over 300,000 observations.

Fourth, I pay attention to isolating the transfer risk channel which may drive the sovereign–corporate relationship from other confounding drivers. This makes this paper – to my knowledge – the first to both i) separate mechanical ties between emerging market sovereigns and firms from the channel of interest and ii) account for feedback effects from firms to government creditworthiness. I do the former by including a measure of government–dependence in our estimations. I account for the fundamental health of firms, and the fact that corporate trends could affect sovereign bond yields in cases where large corporations influence the creditworthiness of their home countries. This reverse causality would prevent us from being able to interpret our estimates as capturing the impact of sovereigns on corporates. I address this by including firm–level profitability and debt sustainability indicators as control

variables.

Finally, I exploit the time–dimension of our data to gain insights into whether the corporate response to sovereign bond yields is more or less extreme during high–yield periods, when corporate firms can borrow more cheaply than their associated governments and when yields exceed economic growth rates. These non–linearities will provide us with insights into what types of firms are most likely to experience a deterioration in their borrowing conditions due to shifts in investor beliefs. While non–linearities have been observed in the local currency bond market (Bevilaqua, Hale and Tallman, 2020), I believe that they entail important policy implications for the optimal financing decisions facing emerging economy firms, as well as for how governments can manage the impact of their foreign debt holdings on the wider economy.

My findings are as follows. Using weekly bond–level data for 46 emerging markets over 2016–2019, I investigate the time–series properties of bond yields against the backdrop of a declining wedge between emerging market growth rates and interest rates on external sovereign debt. I acknowledge that the bond yield data display a mix of unit–rooted and stationary qualities, and account for this feature in our econometric approach. I find that sovereign and corporate bond yields exhibit a cointegrating relationship not explained by economic conditions, firm characteristics, bond properties and common factors affecting emerging countries as a whole. The long–run relationship is not weakened when government–dependent firms are excluded from the estimation sample. This result is in line with the existing literature. The findings that sovereign–corporate pass–through is stronger for government–dependent firms and for non–exporting firms are novel and allows me to attribute at least part of the linkage to transfer risks. Transfer risks are stronger when sovereign bond yields exceed corporate bond yields, when sovereign bond yields are rising and when r exceeds g. These non–linear impacts have not yet been documented in the literature. They hold policy implications as they indicate

when firms' borrowing costs could rise more than would be expected in the absence of these non–linearities. To avoid inconsistent estimates, I account for unobserved common factors in the estimation, and as a result the magnitude of the short–run and long–run linkage between sovereigns and corporates reduces, but the effect remains statistically significant.

In what follows, I first compare bond yields to economic growth rates and discover a new pattern in the data that motivates the research question. I then characterize the bond yields data, test for its stationarity properties (H1), and develop the econometric approach that takes into account its time–series characteristics. Next I proceed to the results of estimation: each of the 5 remaining hypotheses are tested, before conducting a robustness check to account for unobserved common factors and then concluding.

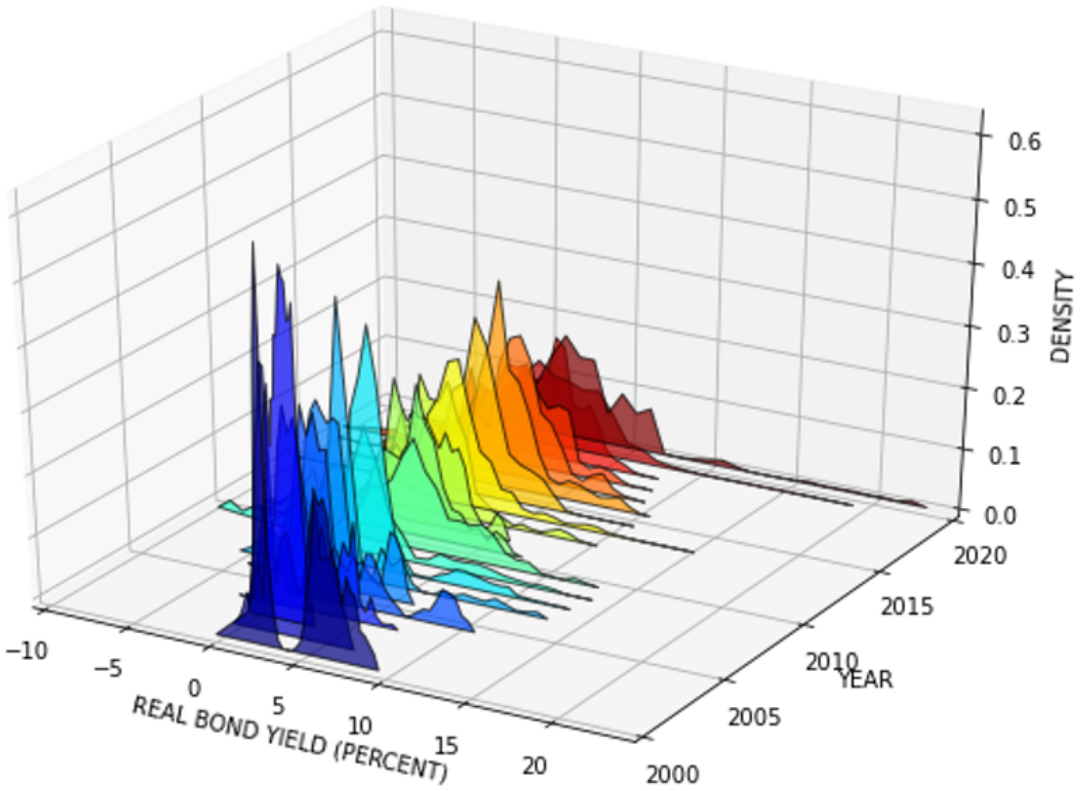
3.2 Motivation

In order to motivate the research question, I assess the sustainability of the depicted trends in debt to GDP ratios (henceforth referred to as "debt ratios" and "debt levels") in emerging markets using a government budget constraint framework.

Existing studies suggest that emerging economy external borrowing costs have been declining and thus debt sustainability is improving. Indeed my bond yield dataset confirms this claim (figure 3.2). The distribution is bimodal in the early 2000s, with bond yields concentrated around the 2 percent and 7 percent. This is driven by a split between countries that experienced lower borrowing costs, including Malaysia, and those facing higher bond yields, notably India and South Africa. This bifurcation softens towards 2020, primarily due to a decline in the yields of the higher risk borrowers.

However, to understand debt sustainability it is important to assess the r–g relationship, rather than just the r. To see why, let us focus on total debt (external and domestic) for the sake of simplification (Hall and Sargent, 2011).

Figure 3.2: Distribution of Sovereign Bond Yields over Time



Notes: Figure depicts the distribution of sovereign bond yields across all sovereign bonds in the dataset, by year. Both variables are computed in real terms, by subtracting the inflation rate from the nominal data. Inflation rates are computed as the percentage change in the Consumer Price Index year–on–year. CPI and growth data stem from the IMF. Yields are computed as the midpoint between the ask and bid yield to maturity.

Under a typical debt accumulation framework (Escolano, Shabunina and Woo, 2017), debt ratio dynamics for an economy with domestic and foreign debt are described by the following relationship:

$$d_t = \left[1 + (1 - \phi_{t-1}) r^d_{t-1,t} + \phi_{t-1}(r^f_{t-1,1} + \Delta s_{t-1,t}) - g_{t-1,t}\right] d_{t-1} + def_t, \qquad (3.1)$$

where g denotes the real GDP growth rate, r^f the real interest rate on foreign debt – or real yield –, r^d on domestic debt, ϕ the share of debt that is issued to foreign lenders, Δs the percentage change in the real exchange rate between periods and d the real value in USD of all bond obligations outstanding as a share of GDP. def is

the primary budget deficit as a share in real GDP, or a surplus if it is negative. Debt is measured at the beginning of the period. Δs is measured in domestic currency per USD, so a rise in it means that the domestic currency has depreciated. The share of foreign public debt in total public debt is assumed to be constant across time.

A decline in real yields, a local currency appreciation, an increase in real GDP growth and an improvement in the primary deficit (or rise in the primary surplus) all contribute to a reduced debt level.[5] This relationship is the basis for an established measure of a necessary condition for sustainability – the notion of intertemporal solvency – for which a useful starting point is checking whether the average rate of real GDP growth exceeds the average bond yield paid by a sovereign issuer. While intertemporal solvency requires projecting the debt dynamics into the future, I use recent historical developments as a simple starting point that does not involve forecasts. The equation shows that overall debt ratios can remain low even as foreign debt obligations and servicing rises: this either requires domestic debt levels or the primary deficit to decline, or for GDP growth to exceed the weighted average real interest rate.

Following Escolano, Shabunina and Woo (2017), this equation can be expressed in terms of changes in the debt ratio:

$$\Delta d_{t-1,t} = \left[(1-\phi_{t-1})r^d_{t-1,1} + \phi_{t-1}r^f_{t-1,1} - g_{t-1,t}\right] d_{t-1} + def_t, \tag{3.2}$$

In order for the debt ratio to not explode in the long run, a sufficient condition is that the primary deficit not exceed the debt ratio times the difference between real economic growth, the average interest rate and the real exchange rate. This implies the following steady state relationship:

$$def \leq \left[g - (1-\phi)r^d - \phi r^f\right] d. \tag{3.3}$$

[5]The described impact of any one right–hand side variable on debt levels assumes that all else is held constant, and that there are no feedback effects that interfere with these mitigating influences.

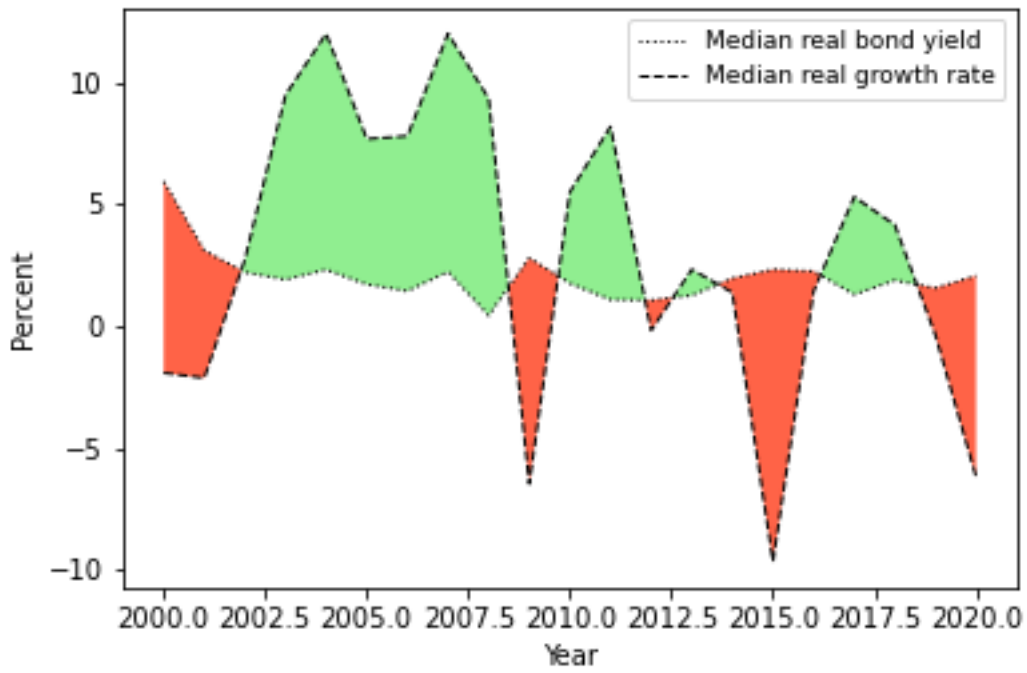
Using this budget constraint framework, I assess the evolution of foreign debt sustainability in emerging market governments over the past 20 years. If the average rate of real GDP growth g has tended to exceed the average bond yield r paid by governments on their hard (or foreign) currency debt, this could allow a government to run a limited primary deficit – i.e. to spend more than it earns – every year without becoming insolvent within the hard–currency context in the long run. In other words, the government could continue to roll over existing foreign currency debt without having to increase tax rates to finance repayments. If the average yield exceeds the growth rate, little room is left for governments to break even: in this case a primary surplus is required for debt–to–gdp ratios to not explode over time, and additionally real exchange rates need to be favourable in the case of external debt. A simplified test for this form of sustainability thus involves a comparison of real hard currency sovereign bond yields and real economic growth rates over time, akin to Blanchard (2019). [6]

Figure 3.3 compares the real yield data with real GDP growth figures. The green and red shaded areas show that the evolution of the yield–growth gap goes through swings. The growth–yield evolution follows cycles marked by relatively large shifts from positive to negative wedges. Periods in which the median economic growth rate across emerging economies exceeds the rate payable on external bonds – shaded in green – have occurred every few years and have become shorter–lived since 2000. Further, the wedge between yields and growth rates has been narrowing during each green–shaded episode since 2000. If these historical trends are reliable indicators for future projections, this potential shrinking of the maximum sustainable primary fiscal deficit suggests a more challenging debt environment for emerging economies than implied by low bond yields alone.

These results show that while emerging market debt ratios are within historical bounds in line with the literature, a previously undocumented fact is that countries' breathing space for running trade deficits has been declining due to a less favourable

[6] All bond data depicted in this paper are henceforth hard currency obligations unless otherwise stated

Figure 3.3: Sovereign bond yields and GDP growth rates



Notes: Figure contrasts real sovereign bond yields and real GDP growth rates (based on USD equivalent GDP), calculated as the median across emerging economies. Growth rates are calculated based on converting each country's currency to the USD equivalent. The figure hence takes into account the roke of exchange rates.

relationship between the economic rate of growth and external sovereign borrowing costs. This paper is concerned with how this pattern could affect firms' borrowing costs in these economies.

3.3 Descriptive analysis

The bond yields in our database span the years 2016 through 2019. Two contextual features of emerging market macroeconomic conditions are noteworthy in this context.

Our dataset spans a period following the end of a commodity–led demand–fuelled expansion, subsequent supply–side disruptions and eventually a downturn in the commodity price cycle from 2014 through 2016. By the end of 2015, commodity

prices, as measured by the IHS Material Price Index, had fallen by 50% between the peak of the cycle in 2011 and its definite end in 2014 (Markit, 2015). One of the ways in which the economies in our sample were affected by this downturn was through a drop in the average emerging country primary fiscal balance (as a share in GDP) from the break–even point to negative 3 percent of GDP by 2016 (Dept, 2019). This drop is consistent with the notion that resource–exporting governments experienced declines in government revenues as commodity prices dropped. It is also consistent with emerging economies' attempts to cushion the impact of the price decline through fiscal expansions. These increasingly large primary fiscal deficits in emerging economies match the rising public debt–to–GDP levels I outlined in figure 3.1. Coupled with the recent shrinking of the growth–interest rate wedge depicted in figure 3.3 the period spanned by our dataset offers an interesting environment for our analysis from a public finance perspective.

The end of the commodity up–cycle was accompanied by one of the largest slowdowns in net capital inflows into emerging economies since 1980, measured as a share of GDP, relative to the peak of capital inflows minus outflows in 2010 (Dept, 2019). This slowing of net capital inflows was driven primarily by a reduction in inflows by 4.9 percentage points between 2010 and 2015. A closer look reveals that the key component of the inflow reduction – or "sudden stop" – was a sharp decrease in foreign direct investments, in portfolio debt flows, as well as "other" flows into emerging markets' banking sectors by foreign entities. The empirical evidence suggests that such stops in emerging economies are more likely to be associated with increases in global derivative price volatility, as well as economic contagion factors (Forbes and Warnock, 2012). This implies that reductions in capital inflows reflect global risks, and that the chances of a "sudden stop" are positively associated with a recent stop experienced by a major economic–financial partner. These facts make it important to pay attention to interlinkages between the economies studied in this paper. If these common risk and country drivers create such interconnections between countries, and if the factors affect countries

Table 3.1: Descriptive Statistics

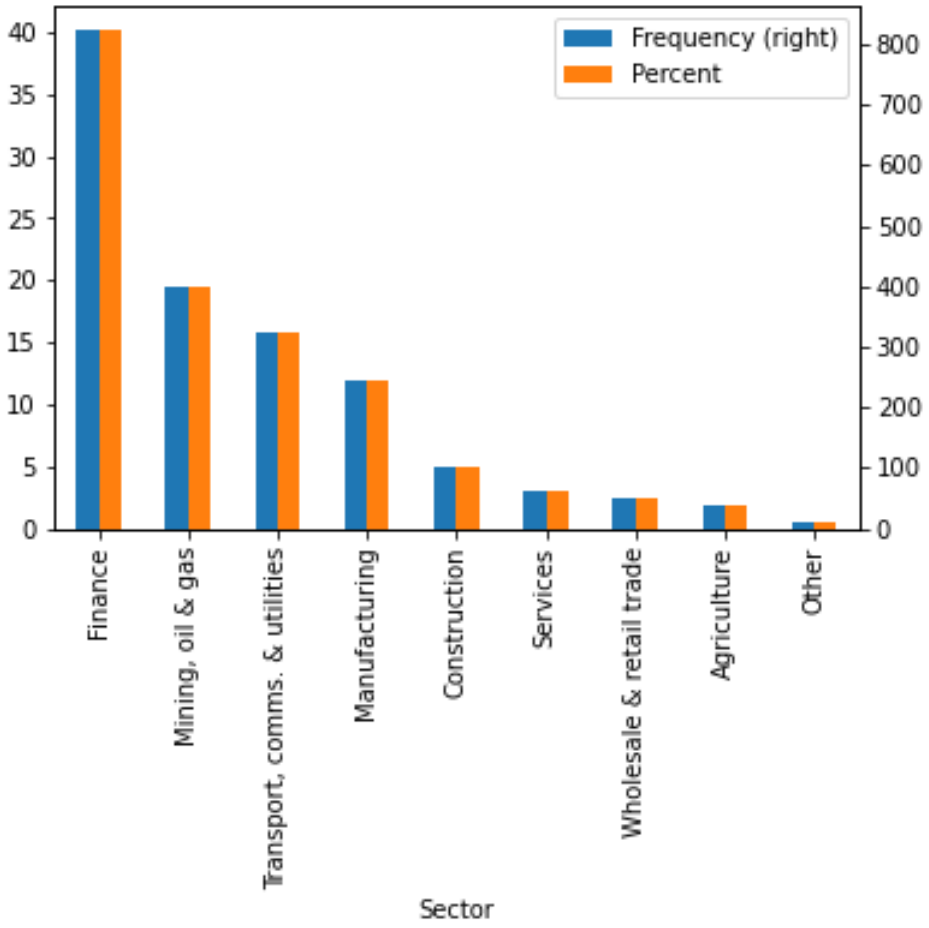
	Mean	**Std.Dev.**	**Min**	**Max**	**Obs**
Sovereign yield (percent)	5.38	2.03	1.05	23.18	386,306
Corporate yield (percent)	6.44	4.89	0.00	54.4	386,306
Bond yield (percent) weighted mean	5.7	3.83	0.002	54	386,306
Bid–ask spread (percent)	0.04	0.41	0	162	386,306
Maturity (years)	8	8	1	100	386,306
Ratings	BB		CC	AAA	355,741
Interest coverage (EBIT to int. expenses)	3.8x	4.9x	-3.34x	25x	211,700
Profit margin (net profit as share in rev.)	3%	20%	-70%	50%	211,700
Leverage (debt to assets)	40%	25%	6%	100%	211,700
Share of bond type in	**Number of bonds**		**Issue amount**		
High yield bonds	54 percent		53 percent		
Investment grade bonds	46 percent		47 percent		
Sovereign bonds	21 percent		39 percent		
Corporate bonds	79 percent		61 percent		
Government–rel. corporate bonds	27 percent		48 percent		

Note: Table reports summary statistics for the bond–level dataset. Ratings are averaged across Moody's, Fitch and S&P. Profit margins are calculated as net profit as a percentage of revenue. Government–rel. corporate bonds refer to government–dependent bonds.

differently over time, then our estimates of the relationship between sovereign and corporate bond yields could be inconsistent unless I account for these dynamics in our econometric approach. I address this feature by accounting for common factors and cross–correlations across economies in a cross–sectionally augmented distributed lag model approach (Chudik and Pesaran, 2015).

Our database captures *secondary* real bond market data on bond debt issued by emerging market sovereign and corporate entities, denominated in USD. Secondary markets capture bond sales and purchases by investors after they have been issued by the sovereign or corporate entity on the primary bond market. Most of these trades occur over the counter, i.e. in private contracts.

Figure 3.4: Distribution of non–sovereign bonds by sector



Notes: Figure depicts distribution of non–sovereign bonds by sector.

How then do bond price and yield data make it to our proprietary dataset? They do so by being captured in the order books of market makers, which are recorded as the "screen prices" on over–the–counter platforms. These order books capture the prices at which dealers of bonds would like to sell (ask) as well as are willing to purchase (bid) bonds. These bid and ask quotes are transformed by the platform providers (such as Tradeweb, Bloomberg, Marketview and others) into synthetic "averages" of all quotes in the market on a given day. The midpoint between bid and ask quotes is recorded as the hypothetical market price of the bond. Data on the coupon are available from the initial issuance of the bond on the primary market, and are combined with the secondary market data in the final dataset. Given the weekly frequency of our dataset, and the trading frequencies

of bonds, it is highly unlikely for a given bond to not have been traded over the course of one time observation. However, in the event that no active quotes occur in the secondary market in a given week, the bid and ask data are estimated by the data provider using proprietary statistical interpolation models. Changes in the bond yield of a given corporate or sovereign bond reflect price movements at each trade, brought about by macroeconomic news or correlations with other (stock) markets. The same holds for sovereign bonds. In contrast, changes in average yields aggregated by country – I refer to them as indices throughout this paper – reflect not only changes in the underlying bond prices that occur when bonds are traded, but also information on newly issued bonds.

The weekly emerging market bonds in our dataset are summarized in table 3.1. I pay special attention to selecting a subset of the bond dataset in order to i) ensure maximum comparability across bonds, and at the same time ii) isolate the transfer risk channel – which captures the risk of explicit government action that prevents borrowers from servicing external creditors – from other risks related to currencies and the nature of the cash flow. I do so by focusing on hard currency bond issuances only, excluding bonds with embedded options, as well as excluding zero–coupon bonds and inflation–indexed bonds. Perpetual bonds are dropped, as are defaulted bonds. I winsorize the bonds yields to exclude the bottom and top 1 percent of yields across all countries. Weeks in which bonds yields are missing are linearly interpolated. Importantly, our data contain bid–ask spreads, which allow us to control for differences in bond liquidity that could affect bond yields on both sides of the estimation[7].

Among the 46 countries in the dataset, the majority of bonds (79 percent) are issued by corporate entities. These bonds are on average of a smaller issue amount, so that sovereign bonds constitute nearly 40 percent of the total value in USD of

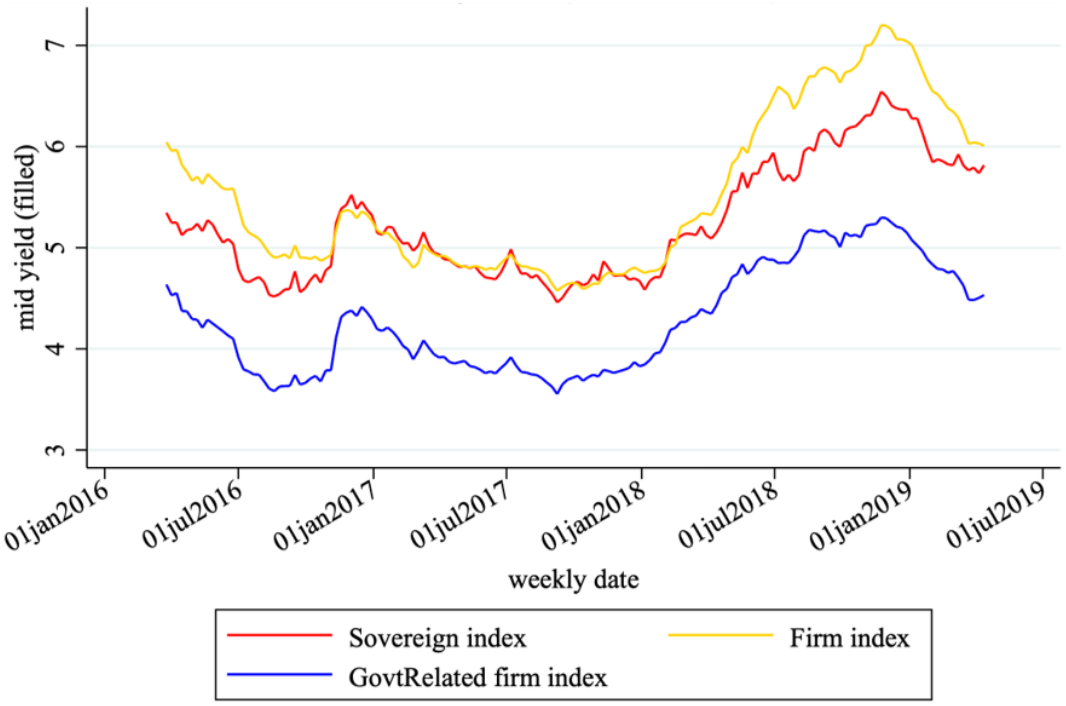
[7]Bid–ask spreads are not entirely defined by liquidity, as they can also reflect the risks that the trader perceives the asset to be associated with, including severe price fluctuations. However it is widely understood that bid–ask spreads can serve as a proxy measure for liquidity.

bonds. Out of the corporate bonds, roughly a third are categorized as government–dependent (by number), while almost half fall into the government–related bucket if I take into account the value of bonds. Government–relatedness as defined in this paper implies government involvement in the form of state–ownership and/or explicit guarantees.[8] As expected due to the risk premium attached to corporate issuers relative to their "home government", sovereign bond yields are on average 1.1 percentage points lower than corporate bond yields. They are also less volatile and exhibit a narrower range. Figure 3.4 depicts the sectoral distribution of the corporate bonds in the dataset. A large share of our emerging market bonds is issued by financial firms (40 percent of the total), followed by corporates that operate in the extractive industries (20 percent) and the infrastructure sector (17 percent). When aggregated to a single index of yield means across countries, the dataset reveals a persistent upward trend in corporate, sovereign and government–related yields between 2018 and 2019 and relatively stable yields between 2017 and 2018 (figure 3.5). It is the later time period in particular which provides some visual indication of potential non–stationary behaviour, due to the change in the level of the average bond yield. I investigate these time–series characteristics more formally in the next section.

Government–related bond yield indices track their sovereign counterpart relatively closely, which can be explained by the fact that the majority of government–related bonds are corporations that are almost entirely owned by the government, and therefore are seen as quasi-government entities. The figure also reveals that there exist periods during which corporate bond yields lie below their respective risk–free counterpart - examples for this include the period between December 2016 and March 2017. I shall pay special attention to this data feature in our main analysis.

[8]Explicit government guarantees are defined in this paper as a legal commitment on behalf of the government to take on the principal and interest payments of the firm in the case of default.

Figure 3.5: Bond yield indices over time (percent)

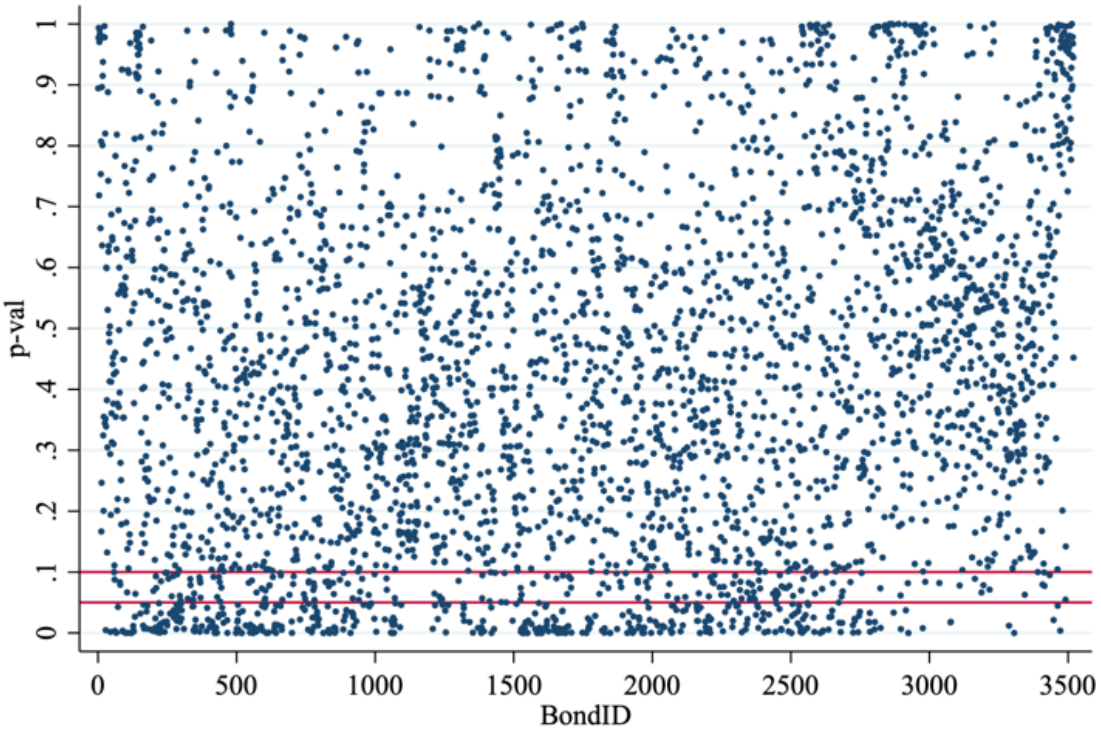


Notes: Figure depicts cross-country bond indices by week for sovereigns, corporates and government–related firms. Data spans the 1st of January 2016 through the 1st of July 2019. Indices are obtained by averaging across all bonds and countries within each of the three groupings reported. Gaps in the bond–level data are interpolated.

3.4 H1: Time–series characteristics of bond yields

Empirically, bond yield data have given rise to mixed conclusions on their non–stationary properties. While earlier work has found that bond yields are non–stationary Campbell and Shiller (1987); Wu and Chen (2001) among others present panel–data evidence which suggests the contrary. Non–stationarity suggests that bond yields follow a persistent pattern over time which is not (entirely) cyclical in nature. The concept also implies that temporary shocks shift the trend of the yield series over time, without it returning to its pre–shock trajectory. How plausible is it from a theoretical perspective that bond yields contain an underlying component whose yield distribution changes over time? Is it plausible that these permanent shifts due to temporary shocks in the distribution of bond yields exist, and that they are not caused by occasional permanent shocks? There is a marked lack of a theoretical framework that would provide answers for these questions. Instead, this

Figure 3.6: Unit–root test results across bonds



Notes: Figure depicts results of the unit root tests based on the Phillips–Perron procedure with 6 lags, without a trend, for each of the 3699 bonds in the dataset. The results are fairly robust to changes in the lag structure and the inclusion of a time trend. The red lines denote the thresholds for rejecting the null hypothesis of a unit root at a 10 percent and 5 percent level respectively. P–values that fall below a given threshold imply that the null hypothesis is rejected at that threshold level, and that the bond does not exhibit unit root properties. Gaps in the bond–level data have been interpolated.

paper proposes two potential trends that could explain why (some) nominal bond yields exhibit non–stationary behaviour.

A potential influence on the time–series properties of the data are secular stagnation trends, which consist of declining global GDP growth and falling policy rates. It is unclear whether movements spanning decades are responsible for the behaviour of the bond yields in our database, which spans 3 years. Short datasets make it difficult to distinguish unit–rooted processes from stationary series, because both of these characteristics are long–run data features. Despite the lack of obvious economic drivers of unit–root type behaviour, if the data exhibit unit–root type properties, it is crucial to account for these econometrically for the sake of inference.

Given the theoretical and empirical uncertainty surrounding the random–walk properties of bond yields in emerging markets, I proceed by first investigating the prominence of non–stationarity among the bonds in our sample. [9]

As reported in figure 3.6, the null–hypothesis of a unit root, and thus non–stationarity cannot be rejected at a 90 percent confidence level for a large share of bonds - 77.8 percent across all bond types. These results imply that for the majority of bond yields in our emerging market dataset, it is not unlikely that temporary shocks to bond yields result in permanent shifts in a yield's trajectory. In other words, the emerging market bond yields captured in our sample of 46 emerging economies might not be mean–reverting.

Table 3.2 provides a disaggregated view of the country and issuer characteristics that are responsible for unit–root type behaviour among bond–yields based on time–series and panel unit root tests. It reports disaggregated results for the Phillips–Perron test. Sovereign bond yields are most likely to exhibit unit–root properties in our data (95 percent of them), a share which is higher than the 85 percent of corporate bond yields which exhibit such characteristics. Bonds issued by manufacturing firms in upper middle income countries are the least likely to exhibit non–stationary traits, while high income based bonds in the agricultural sector most commonly exhibit unit roots. Bonds that are classified as high–yield, i.e. bonds from issuers with an average credit rating below "BBB-" from rating agency S&P, are more likely to be stationary than their higher–rated counterparts.

Table 3.3 reports the panel–level test results for non–stationarity – both the aggregate result and the result by bond characteristic. I employ the Im–Pesaran–Shin (IPS) and Harris–Tsavalis (HT) panel data tests. For the IPS–test, the null hypothesis is that all bonds contain a unit root, while the alternative is that a fraction of the panel is stationary. A (N,Y) entry implies that the HT test does

[9] Stationarity is considered in the weak sense wherever mentioned throughout this paper.

Table 3.2: Drivers of unit–root type behaviour among bond yields – time–series

Share of stationary bonds by country/issuer characteristics – PP-test					
Firm sector		**Income group**		**Bond type**	
Agriculture	0.07	High	0.09	Sovereign	0.05
Construction	0.18	Upper middle	0.12	Corporate	0.15
Finance	0.09	Lower middle	0.10	Govt.–related	0.09
Manufacturing	0.19				
Mining, Oil and Gas	0.17				
Other sector	0.38				
Services	0.19				
Infrastructure	0.12				
Wholesale/ Retail Trade	0.17				
Rating					
High–Yield	0.16				
Investment–Grade	0.07				
Total number of bonds: 3,699					

Note: Table reports stationarity results from the Phillips–Peron time–series test. The table reports the share of bonds for which the null hypothesis of a unit root is rejected at the 5 percent level.

not reject the null hypothesis, while the IPS test does. They largely confirm the time–series results: a mix of stationary and non–stationary data is indicated by IPS for all characteristics expect for Mining, oil and gas, middle income countries and government–related bonds. The HT test fails to reject the null hypothesis of a unit root for all bond types.

These insights have multiple key implications for establishing stylized facts about the relationship between corporate and sovereign bond yields – or any multi–variate analysis involving emerging market bond yields akin to those in our dataset. First, studies that estimate relationships containing emerging market bond yields on either side of the regression risk mis–specifying the model.[10] This is particularly the case when yields are included in a level specification, i.e. without the appropriate data

[10] Bevilaqua, Hale and Tallman (2020); Almeida et al. (2017) employ specifications in levels

Table 3.3: Drivers of unit–root type behaviour among bond yields – panel

Aggregate results – HT and IPS tests					
	Statistic	p–value			
HT–test	$\rho = 0.87$	0.33			
IPS–test	$Z_{t-\tilde{b}ar} = 7.32$	0.51			
Null–hypothesis rejection by country/issuer characteristics – (HT result,IPS result)					
Firm sector		**Income group**		**Bond type**	
Agriculture	(N,Y)	High income	(N,Y)	Sovereign	(N,Y)
Construction	(N,N)	Upper middle	(N,N)	Corporate	(N,Y)
Finance	(N,Y)	Lower middle	(N,N)	Govt.–related	(N,N)
Manufacturing	(N,Y)				
Mining, Oil and Gas	(N,N)				
Other sector	(N,Y)				
Services	(N,Y)				
Infrastructure	(N,Y)				
Wholesale/ Retail Trade	(N,Y)				
Rating					
High–Yield	(N,Y)				
Investment–Grade	(N,Y)				
Total number of bonds: 3,699					

Note: Table reports stationarity results from the Im–Pesaran–Shin test and Harris–Tsavalis panel data tests. For the HT test, the null hypothesis is that the panel contains a unit root, and the alternative that the panel does not contain a unit root. For IPS, the alternative hypothesis is that at least a fraction of the panel is stationary.

transformations to alleviate difference–stationarity. As a result coefficients obtained in this manner can wrongly point towards significant relationships between the variables, when in reality the variables are both being influenced by a common non–stationary component. Second, difference–stationary bond yield data suggest that shocks to policy rates might have persistent effects on the economy that do not revert over time.

This paper focuses on a third implication: if sovereign and corporate bond yields are both difference–stationary, and their exists a linear combination of their yields which is stationary, then corporate bond yields are inexorably tied to their sovereign counterpart in the long run, i.e. cointegrated.

Country–level risks constitute a key component in the assessment of corporate credit risks involving bond obligations by banking supervisory agencies (the BIS) and credit rating institutions. Country risks include two components that are relevant for understanding cointegrating relationships between sovereign and corporate bond yields. First, transfer risks denote deliberate action taken by government which affects corporates' ability to repay foreign creditors, and secondly collective debtor risks refer to non–deliberate economy-wide events that lead to private contracts not being fulfilled. I focus on transfer risks, as they are particularly relevant in light of the rising importance of external debt for emerging market firms. I are able to do so by capturing macroeconomic and stock market conditions in our dataset, as well as firm–level performance metrics that control for direct impacts of country events on firm bond yields. A prominent example of such transfer risk events includes Argentina's 2001 country crisis, during which a moratorium was imposed on payments to private foreign creditors. Other identified realizations of transfer risks include Argentina in 2003, Pakistan in 1999, Ecuador in the same year and Russia a year prior.

Where investors believe in transfer risks, a deterioration in the creditworthiness of a government can lead to a deterioration in the creditworthiness of companies under its "sphere of influence" (holding other debtor risks constant). The definition of this "sphere" is important, as it should reflect ties between the associated government and firms which allow for transfer risks to play out. In our Bloomberg dataset, the corporate issuer's "country of risk" is a proprietary value set by the data provider based on the following criteria: a company's legal country of domicile, the primary stock exchange on which it trades, the location from which the majority of its

revenue comes, and its reporting currency" (Bloomberg, 2020). This definition provides a reasonable basis for sovereign yield changes to spill over to the associated corporates – after stripping out the economic conditions in the home country and mechanical ties to the government via ownership.

These transfer risks could form the basis for a cointegrating relationship between corporate and sovereign bond yields: corporate rates can be modelled as the sum of their associated country rate reflecting transfer and collective debtor risks, plus a risk premium owing to additional default and liquidty risks associated with the firm entity. For there to be a long–run self–correcting relationship between sovereign and corporate bond yields, the involved series need to be non–stationary.

3.5 Econometric approach

The focus of this paper is on the impact of movements in sovereign bond yields on corporate borrowing costs in emerging economies. Standard estimation models require either non–stationary or stationarity across all units in the dataset in order to yield reliable estimates. Given our evidence for a mix of random–walk and mean–reverting behaviour among bond yields, I employ an autoregressive distributed lag (ARDL) model. The key advantage is that the ARDL setup can be rewritten into error–correction form, and based on this specification allows for ex–post testing of the existence of a cointegration relationship. Most importantly, the Bounds testing procedure does not require knowledge of whether the individual bond yields are $I(0)$ or $I(1)$ (Pesaran, Shin and Smith, 2001). This is enabled because the Bounds procedure for obtaining critical values computes a range of these values from all variables being $I(0)$ to all variables being $I(1)$.

Even after accounting for the time–series properties of emerging market bond yield data, there are a number of obstacles when attempting to identify the sovereign–corporate relationship – and its specific direction – in reduced–form estimations.

Our aim is to identify causal estimates rather than correlations in the data.

First, there may exist a reverse association between both variables, i.e. when corporate creditworthiness spills over onto governments' perceived solvency risks. Examples include when governments take on part of the debt burden of struggling private financial entities. As a prominent example of such spillovers, Acharya, Drechsler and Schnabel (2014) present evidence on the financial – sovereign transfer for the Eurozone. In order for the estimates to be interpreted as causal, I need to minimize the risk of these reverse spillovers.

Second, there are potential interfering variables that I might confuse for the cointegration relationship of interest: an economy–wide downturn or political crisis may worsen firms' fundamental outlook while simultaneously increasing the chances of a government repayment crisis. I need to control for such economic and political conditions in order to separate this effect from our relationship of interest.

Finally, as I shall address later in this paper, it is crucial to account for external factors, which can cause interlinkages between bond yields across countries. Examples of such factors are foreign shocks including as monetary policy actions (i.e. policy rate changes by central banks) that affect each country but propagate differently by economy. Economic and geographical links between countries including trade unions and shared electricity grids could also cause cross–correlations between countries. These cross–correlations – if left unaccounted for – cause inconsistent OLS estimates, as I demonstrate later in the paper.

I address each concern in turn. On the one hand I collect additional data. To account for country–level confounding variables, I control for the key financial and macroeconomic conditions that could affect firms and governments simultaneously. The ratio of **Public debt to GDP** is relevant as highly indebted governments could implement tax increases and other policies that can link their bond yields to

that of the private sector.

The **current account as a share in GPD** is relevant as large current account deficits can be caused by countries financing domestic consumption through foreign borrowing. If such current account deficits become unsustainable, they can increase borrowing costs for governments and firms alike: large current account deficits can put pressure on foreign reserves, raising government bond yields, while also increasing corporate bond yields if firms borrow excessively from abroad.

Real GDP per capita reflects general economic conditions that could affect both perceived government bond risks (by increasing debt to GDP ratios, for example) and corporate risks if downturns imply lower revenue and retained earnings.

If a country's **exchange rate** depreciates then both the sovereign and corporates are likely to find it more difficult or costly to service dollar-denominated debt. A similar mechanism holds true when the domestic currency appreciates. I want to separate transfer risks from the exchange rate channel. The real effective exchange rate relative to the USD is therefore selected as a control variable to separate transfer risks from a general co–movement of government and corporate bonds due to a currency appreciations or depreciations.

Stock market returns can capture forward–looking variables that could generate co–movement between sovereign and corporate bond yields even in the absence of transfer risks. One example are US inflation expectations. Rising US inflation expectations can lead to rising short–term bond yields for both corporates and sovereigns. The stock market has been shown to reflect expectations over inflation risks (Boons et al., 2020) and is thus included as a proxy variable.

Finally, I include a standard deviation measure of **stock market return volatility** as a simple approximation of (many things but also) changes in investors'

appetite for risk. Changes in risk aversion can lead investors to avoid certain asset classes as a whole, which could confound our transfer–risk driven link between sovereign and corporate bond yields.

The described confounding mechanisms could also be present on a within–country (i.e. bond) level. To absorb any time–varying bond characteristics that may impact both corporate and sovereign bond yields, I include a number of bond–level variables. If investors tend to prefer bonds with certain characteristics, such as a certain maturity or a certain liquidity, then bond yields between sovereigns and corporates could be linked because of these preferences rather than due to intrinsic transfer risks. As an example, institutional investors are known to adhere to strict mandates, which often see them commit to investing in assets with very specific characteristics. To strip out this effect I include bond maturities, the bid–ask spread as a widely accepted measure of liquidity, and the primary bond issue amounts as control variables.

On reverse causality, I expect that firms could affect governments through their profitability and solvency status. The evidence suggests that corporate bond yields could primarily affect government creditworthiness through the performance of the firm. As a mechanism, a distressed corporate or bank entity – or collection of corporate and bank entities – induces a government bailout. Prominent examples include the bailout of the South African electric utility Eskom, which has taken up 5 percent of the average annual government revenues since 2019 alone (Daily Maverick, 2002) and the capital injection into India's Unit Trust mutual fund in 2002. The cost of such government bailout in turn negatively impacts the creditworthiness of the government, thus increasing its bond yields. This is the extreme case in which a bailout occurs. However given the role of investor beliefs in affecting bond yields, I can imagine that outside of the extreme scenario as distress increases, the perceived risk of a costly bailout increases government bond yields. In summary, the most likely channel through which corporate bond yields affect government bond yields

is through the health of the firm.

In controlling for key corporate solvency and profitability metrics, I intercept the key channel through which reverse causality is enabled. I include data on interest coverage ratios which capture the extent to which firms are able to cover their interest payments through earnings before interest and taxes are deducted. A larger ratio implies that interest is relatively easy for the firm to service, and tends to be good news. Profit margins capture how well revenues translate into profits while accounting for the size of the firm, with an increase spelling good news from the perspective of investors. Debt leverage ratios capture how much debt a firm is taking on to generate its revenue, where a higher ratio tends to create suspicion among investors. These interpretations are simplified, as they assume that a metric cannot be artificially inflated or boosted at the expense of other long–term indicators.

A time–trend t is included in the estimation for two reasons. First it captures any unobserved variables which follow a linear trend that might influence both sovereign and corporate bond yields simultaneously. Second, as the bond yields enter into the model in non–differenced form in the error correction term, it is important to strip out any deterministic trends that might lead to spurious results for the long–run relationship. Such a deterministic trend is an alternative candidate source of non–stationarity of bond yields, but as opposed to difference–stationarity such a variable tends to return to its mean after a disturbance. Both types of non–stationarity are difficult to distinguish, particularly in short time samples. Our approach to include a time–trend errs on the side of caution.

By following Pesaran's 1999 pooled mean group estimator approach (Pesaran, Shin and Smith, 1999), I allow for the intercepts, error variances and short–run relationship between sovereign and corporate bond yields to vary by country, but impose a homogeneous long run coefficients on the model. This choice reflects the notion that while there may be country–characteristics that result in a

different corporate–sovereign relationship in the short–run, in the long run emerging economies may be more similar than they are not. This choice is also made in order to maintain sufficient degrees of freedom in the estimation: Limiting the number of parameters in the long–term places a lower bound on the precision of the resulting estimates. Pesaran and Smith (1995) show that the main consequence of restricting long–run coefficients to be homogeneous when they are not is a downward–biased speed of conversion coefficient. In other words, the speed of conversion coefficient could be seen as a lower bound on the true coefficient. With this effect in mind, I view the efficiency gains from restricting the long–run coefficients as acceptable.

In error–correction form the estimation is thus based on:

$$\begin{aligned}\Delta Corp_{it} = c_0 + c_1 t - \alpha(Corp_{i,t-1} - \theta Sov_{c(i),t-1}) + \sum_{j=1}^{p-1} \gamma_{c(i)j} \Delta Corp_{i,t-j} \\ + \sum_{j=0}^{q-1} \rho_{c(i)j} \Delta Sov_{c(i),t-j} + \sum_{j=1}^{r-1} \boldsymbol{\lambda}'_{c(i)j} \Delta \boldsymbol{X}_{i,t-j} + \varepsilon_{it},\end{aligned} \tag{3.4}$$

where $Corp_{it}$ refers to the corporate bond yield for bond i at time t of the weekly dataset. Each bond index i is uniquely associated with a country of origin $c(i)$ and the issuer firm f. $Sov_{c(i)t}$ refers to a country–week level index of sovereign bond yields. It is obtained by averaging the bond yields across bonds, by country and by week. $\boldsymbol{X}_{i,t-j}$ contains a set of control variables on a bond i level. Bond characteristics include the bid–ask spread as a measure of liquidity, a bonds fixed maturity and the initial issue amount. Macroeconomic characteristics include public debt to GDP, the current account as a share in GDP, inflation, real GDP per capita, public debt to foreign reserves, the real effective exchange rate, stock market returns and return volatility. The stock market return is the growth rate of the annual average stock market index in each country and is constructed by taking the average of the daily stock market indexes available on Bloomberg. Firm controls include interest coverage, profit margins and leverage.

In additional specifications I also include a country–year fixed effect to capture any time–varying economy traits that could affect both sovereign and corporate creditworthiness and are not capture by the control variables. In order to preserve degrees of freedom I do not include additional fixed effects by country, bond or firm. This is because I believe that it is unlikely for there to exist fixed characteristics that are not captured by a country's GDP, a firm's fundamentals and the bond traits that I include in our estimation. In other words, given our model set–up, the efficiency costs of including additional fixed effects outweighs the potential benefits from an identification perspective.

The lag structure, i.e. values for p, q and r, is determined using the Akaike Information Criterion (AIC). This is because – relative to the Bayesian Information Criterion – the AIC tends to prefer models with more lags, thus reducing the potential for serial correlation of the residuals. The long–run coefficient is a pooled estimate, with the same coefficient imposed on all countries. The short–run coefficients on the other hand and allowed to vary by country, and are averaged out in order to obtain a single pooled mean group estimate. This choice reflects two considerations. First, in order to prevent reducing the precision of the estimation estimates, it is advantageous to limit either the short–run or long–run coefficients to not vary by country. Second, while there may be frictions between countries that lead their sovereign bond yields to impact their firms differently, in the long run these frictions should become alleviated. I do not estimate the coefficients in a joint two–equation vector error–correction model. This is because such a model would require us to impose an explicit assumption as to which variable (sovereign or corporate bond yields) is exogenous relative to the other. In the current specification I do not impose exogeneity of sovereign bond yields. For our purposes it suffices for us to rule out that corporate bonds drive sovereign bonds and that there are un–captured variables that drive both bond types simultaneously. I do so using the approaches outlined above.

Our prior expectation on the magnitude of the long–run relationship between sovereign and corporate bonds yields – captured in θ in equation 4 – is agnostic. I would expect a coefficient relatively close to 1. The speed of adjustment parameter α tells us how long it takes for sovereign and corporate bond yields to converge after straying from one another. Importantly, I expect this parameter to depend on the direction in which sovereign yields have moved, and whether they are higher or lower than their corporate counterpart. This is because the concept of transfer risks is asymmetric in the existing literature: corporate firms are expected to be negatively impacted by sovereign actions in the event of sovereign repayment crises, yet there is no precedent in the literature for expecting positive transfer risks. This would be a situation in which governments in improving creditworthiness situations undertake action to specifically and directly help firms repay their foreign lenders. I form the following hypotheses:

- If sovereign bond yields exceed corporate bond yields, we would expect increasing *perceived* transfer risks to eventually lead to convergence. This implies a larger speed of adjustment coefficient.

- If sovereign bond yields are rising, we would expect increasing *perceived* transfer risks and thus a larger speed of adjustment coefficient.

- Transfer risks should be stronger for corporate bonds that are classified as government–dependent.

The overall speed of convergence will depend on the prevalence and weight of these three effects in the data. This result on the speed of convergence is particularly relevant because recorded historical transfer events are relatively rare, occurring over the years. I believe that given our econometric set–up I am able to isolate transfer risks as the key driving force behind the cointegration relationship in our data. The speed of adjustment coefficient also provides insights on how quickly investors exploit arbitrage opportunities.

Table 3.4: Relationship between Sovereign and Corporate Bond Yields

	Dep. var: Corporate Bond Yield				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
LR Sovereign Bond Yield θ	1.72***	0.99***	0.99***	0.90***	0.89***
	(0.38)	(0.23)	(0.20)	(0.12)	(0.24)
LR Cointegrating Relationship	Y	Y	Y	Y	Y
SR Sovereign Yield (lag 1) ρ_1	0.50***	0.80***	0.79***	0.73***	0.71***
	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
Country–time FE		X	X	X	X
Sector FE			X	X	X
Bond–controls				X	X
Macroeconomic–controls					X
Observations	311,033	327,521	327,521	322,101	311,033
Adjusted R–squared	0.82	0.89	0.87	0.90	0.88
Speed of adjustment α	8%	6%	7%	6%	6%
Durbin Watson Statistic	1.7	1.6	1.7	1.7	1.7
DW Critical Upper Bound	1.47	1.49	1.49	1.42	1.40
Hausman test (p-value)	(0.32)	(0.41)	(0.31)	(0.30)	(0.44)

Note: Table reports short–run and long–run responses in percent of corporate bond yields to 1 percent increase in sovereign bond yields, controlling for various confounders. The test for a long–run relationship is based on the Bounds testing procedure. Coefficients are obtained by estimating equation 4 on a bond level. Bond characteristics include the bid–ask spread, maturity and issue amount. Macroeconomic characteristics include public debt to GDP, the current account as a share in GPD, inflation, real GDP per capita, public debt to foreign reserves, the real effective exchange rate, stock market returns and return volatility.

3.6 Results

3.6.1 H2: Long–run and short–run links between sovereign and corporate bond yields

Baseline results

I begin by looking at the overall impact of sovereign bond yields on corporate borrowing costs, both in the short–run and long run. Emerging market corporate bond yields respond statistically significantly to movements in their sovereign counterparts (Table 3.4). A one percentage point increase in sovereign bond yields leads to an over 1 percentage point rise in corporate yields in the long run (column 1).

This long–run coefficient declines significantly, but remains close to 1 when controlling for bond characteristics, sector fixed effects and unobservables that vary by country and over time (column 4), as well as after inclusion of a set of macroeconomic control variables (column 5). The bounds test confirms a cointegrating relationship between corporate and sovereign yields across all specifications reported. In the short run, the pass–through from sovereign bond yields to corporates in markedly weaker: a 1 percentage point increase in the sovereign bond yield is associated with a 0.71 percentage point rise in corporate yields in the most complete specification. The weaker short–run dynamics between sovereign and corporate borrowing costs, and a speed of adjustment of on average 8 percent per week, suggest that the adjustment of fixed income prices towards long–run equilibrium is not instantaneous. It takes approximately $100/8 = 12.5$ weeks for deviations to correct towards the long–run relationship. This result is compatible with the existence of financial market frictions which create a wedge between the short–run dynamics of sovereign and corporate borrowing costs, and their stronger long–run economic relationship. The fact that the adjustment towards the long–run equilibrium is not instant (this would imply a speed of 100 percent) matches the fact that transaction costs, technical issues and the existence of "noise traders" that create bubbles can prevent arbitrageur investors from exploiting all mean–reverting relationships to the full extent. In order to verify if the long–run homogeneity assumption of the pooled mean group estimator is valid (compared to the mean group estimator which allows all parameters to vary by country) I employ a Hausman test to determine the effect of heterogeneity on the coefficients. The null hypothesis that the pooled mean group estimator is preferred is not rejected in the data across specifications 1 through 5 in table 3.4, lending support to my choice of estimator.

Confounding firm characteristics and government ties

The inclusion of firm–level characteristics in the analysis serves two purposes. First, this allows us to test whether our results are driven by firm–level characteristics. Second, as our interest is in the impact of sovereign borrowing conditions on

corporates I need to purge our estimation of one source of potential reverse causality. Such a reverse relationship is plausible given that certain large enterprises are of national interest to the government and could transfer some of their corporate debt burden to the state through bailouts. Table 3.5 reports the long–run impact of sovereign on corporate bond yields when accounting for firm–specific profitability and creditworthiness characteristics. The results are relatively robust to this specification, with the key difference being a better model fit and slightly larger speed of adjustment of bond yields towards the long–run equilibrium of 7 percent per week.

The long–run sovereign–corporate relationship is also not explained by direct government involvement in these firms. Such involvement includes credit guarantees and partial state–ownership, and could have two adverse impacts on our analysis. First, given our focus on the effect of sovereigns on corporates, such direct links could be responsible for reverse causality: struggling firms can affect the creditworthiness of their government investors. Petrobras in Brazil, China Petrochemical in China, Petronal Nasional Berhad in Malaysia and Pemex in Mexico are just some examples of large state–owned enterprises in emerging markets which have impacted sovereign credit ratings in these economies (Reuters, 2020).

Second, even if the direction of causality were from sovereign guarantors and shareholders to their corporate partners, this "ownership channel" is not the focus of the analysis. The aim of this paper is not to understand how firms are affected when their government shareholders' creditworthiness declines and leads them to 1) sell their stakes or 2) ask for a change in corporate strategy (if they hold voting rights). Instead, as investigated further in the next section, our focus lies on transfer risks – which are linked to investor beliefs – as a possible transmission mechanism. Table 3.6 purges the estimation of mechanical sovereign–corporate linkages by estimating a sub–sample for firms that are categorized as government–dependent. Based on the bounds test, the long–run relationship in emerging markets remains

Table 3.5: Robustness of results to firm–profitability and credit–worthiness

	Dep. var: Corporate Bond Yield	
	(1 – Baseline)	**(2 – Firm Controls)**
LR: Sovereign Bond Yield θ	0.89***	0.90**
	(0.24)	(0.23)
LR Cointegrating Relationship	Y	Y
SR Sovereign Yield (lag 1) ρ_1	0.71***	0.69***
	(0.02)	(0.05)
SR Interest Coverage		-0.33*
		(0.14)
SR Profit Margin		-0.45**
		(0.17)
SR Leverage		0.52**
		(0.17)
Country–Time FE	X	X
Sector FE	X	X
Bond–controls	X	X
Macroeconomic–controls	X	X
Firm–controls		X
Observations	311,033	210,500
Adjusted R–squared	0.88	0.89
Speed of adjustment α	6%	7%
Durbin Watson Statistic	1.8	1.75
DW Critical Upper Bound	1.52	1.36

Note: Table reports short–run and long–run responses in percent of corporate bond yields to 1 percent increase in sovereign bond yields, controlling for firm confounders. The test for a long–run relationship is based on the Bounds testing procedure. Coefficients are obtained by estimating equation 4 on a bond level. Firm controls include interest coverage, profit margins and leverage.

even after this adjustment: a 1 percentage point increase in government bond yields is associated with a 0.81 percentage point rise in corporate bond yields (column 2). While the coefficient describing the long-run relationship between sovereigns and corporates differs statistically significantly between both sub-samples, the speed of adjustment parameter does not. These results come from a Wald test for the equality of two coefficients, where the null hypothesis is that the coefficients are equal. For the purpose of the Wald test, I simplify the formula for obtaining the variance of the difference between the baseline and dummy coefficients by assuming that the covariance between the long–run coefficients is zero. This simplifying

Table 3.6: Robustness of results to government–dependence

	Dep. var: Corporate Bond Yield		
	(Baseline)	**Government dependence**	**No gov. dependence**
LR Sovereign Bond Yield θ	0.89***	0.90***	0.77***
	(0.24)	(0.20)	(0.17)
LR Cointegrating Relationship	Y	Y	Y
SR Sovereign Yield (lag 1) ρ_1	0.71***	0.83***	0.56***
	(0.02)	(0.07)	(0.05)
Country–Time FE	X	X	X
Sector FE	X	X	X
Bond–controls	X	X	X
Macroeconomic–controls	X	X	X
Firm–controls			
Observations	311,033	112,590	200,089
Adjusted R–squared	0.88	0.80	0.88
Speed of adjustment α	6%	7%	7%
Durbin Watson Statistic	1.8	1.7	1.7
DW Critical Upper Bound	1.52	1.31	1.36

Note: Table reports short–run and long–run responses in percent of corporate bond yields to 1 percent increase in sovereign bond yields, for the baseline and a sample of firms that are categorized as government–dependent in the dataset. The test for a long–run relationship is based on the Bounds testing procedure.

assumption circumvents the fact that the covariance between the coefficients cannot be computed since they stem from separate regressions. Whether the assumption is justified remains an open question – however in sufficiently large samples covariances between coefficients in the same estimation tend to converge towards zero, providing some support to our approach.

3.6.2 H3: Transmission channels

I focus on a key channel for the impact of sovereigns on corporates, namely transfer risks. Transfer risks are realized when private agents find themselves unable to fulfil their obligations towards *foreign* lenders due to explicit government actions. Examples of transfer events include governments' decisions to transfer part of their debt burden onto the private sector by means of the removal of guarantees and

restrictions on the transfer of repayment funds abroad. Importantly transfers should involve restrictions specifically geared towards halting payments to foreign lenders. Transfer risks constitute a key country risk component that is identified and tracked by the Bank for International Settlements (Claessens and Embrechts, 2003).

Transfer risks are distinct from broader country–wide risks, and I can isolate them empirically. Country–wide risks do not involve directed, deliberate government action to improve its debt servicing position. Instead, they involve events like wars and other economy–wide developments that affect private contracts. Such events can affect firms' profitability and creditworthiness directly, but can also reduce government revenues and the primary balance, leading to a deterioration of the sovereign debt position. Both the sovereign channel and the direct profitability channel could increase corporate yields (the former through transfer risks). How do I then separate out the transfer risk channel – which is a transmission channel from wider country risks to firms – from wider country risks? As I shall detail in section 3, I do so by including data on firm–level performance indicators in our estimation. Controlling for profitability and debt service capacities strips the estimation of the direct impact of country–wide events on firms, leaving us with transfer risk channels as the remaining factor.

Historically, transfer risks have been realized during sovereign crises and have played a role in influencing corporate credit ratings. In 1998 Russia imposed a 90–day restriction on private borrowers' ability to repay foreign lenders in USD, affecting borrowings totalling over USD 1 billion over that period. From September 15th 2020, the Argentinian government permitted domestic firms to repay only up to 40 percent of their foreign currency denominated principal coming due by accessing the foreign exchange market. 60 percent of the repayments would instead have to be refinanced by issuing additional hard currency debt (Nicholson y Cano, 2020) or by exchanging the debt obligations for equity stakes. And in 2005, the credit rating agency S&P upgraded the ratings of 25 firms following a review of transfer risks,

among other variables. This paper does not directly observe individual transfer risk events, but instead empirically tests for perceived transfer risks as a residual channel through which sovereign yields affect corporate yields.

For completeness, there are other channels through which sovereigns can affect firm borrowing costs which I do not focus on in this paper. In particular, rising sovereign risk measures have consistently been found to negatively affect firms' borrowing costs and their access to financing in these economies (Durbin and Ng, 2005; Peter and Grandes, 2005; Bedendo and Colla, 2015). On access to credit, this involves the extreme case of when sovereign debt payable becomes so high relative to governments' repayment capacities that rolling over their debt is no longer possible. What can occur as a result is sovereign default as well as debt restructuring, which has in the past restricted private firms' access to external finance beyond demand–side effects (i.e. firms were unable to obtain financing even when they wanted to) (Arteta and Hale, 2008). The main channel that has been identified in this literature is an output reduction that accompanies the sovereign crisis. This paper controls for economic output in order to separate this channel from the transfer risk channel. The second documented mechanism through which sovereign debt default affects firms' ability to borrow at a given rate is the sovereign debt ceiling. This describes the formerly common practice of rating ceilings, which have been imposed by credit rating agencies, as well as institutions such as the London Clearing House on the ratings of corporate entities relative to their sovereign "home" entity. In other words, when sovereigns are downgraded because they have defaulted on a coupon or principal repayment, firms whose ratings would be better than their host government are downgraded, too. Arteta and Hale (2008) provide evidence that – after controlling for deteriorating economic fundamentals – these firms obtain less external financing. I include a credit ratings as a variable in the estimations to isolate transfer risks from this confounding channel.

In order to isolate this mechanism, I strip away trivial, mechanical factors that may cause co–movement between sovereign and corporate bond yields in emerging markets.

Ties between private firms and government are common in emerging markets, where quasi-fiscal private entities are widespread: firms with some degree of state–ownership were estimated to 20 percent of the MSCI Emerging Markets Index in 2021 (Morningstar, 2021). The government–dependence variable in our dataset enables us to explicitly test for the existence of transfer risks. In particular, where government–dependence exists I expect perceived transfer risks – and thus the pass–through from governments to firms – to be stronger than in their absence. This is because governments can more easily remove guarantees granted to these firms, and influence operational decisions as shareholders in the case of equity ownership. Hence if transfer risks exist in the minds of investors, they should result in a stronger corporate–sovereign relationship in equation 4 for government–related firms. I estimate the following regression:

$$\begin{aligned}\Delta Corp_{it} = c_0 + c_1 t - \alpha(Corp_{i,t-1} - \theta Sov_{c(i)t} - \phi Sov_{c(i)t} \times D_i) + \sum_{j=1}^{p-1} \gamma_{c(i)j} \Delta Corp_{i,t-j} \\ + \sum_{j=1}^{q-1} \delta_{c(i)j} \Delta Sov_{c(i),t-j} \times D_i + \sum_{j=1}^{q-1} \mu_{c(i)j} \Delta Sov_{c(i),t-j} + \eta D_i \\ + \sum_{j=1}^{r-1} \boldsymbol{\lambda}'_{c(i)j} \Delta \boldsymbol{X}_{i,t-j} + \varepsilon_{it},\end{aligned} \tag{3.5}$$

where all variables are as in equation 4, but $Sov_{c(i)t} \times D_i$ is an interaction variable between a dummy variable which assumes the value of 1 for government–related firms, and 0 otherwise, and the sovereign bond yield. This interaction variable allows us to examine whether there exists significant heterogeneity in the impact of sovereign bond yields on corporate bond yields.

Table 3.7: Comparative statistics for government–related bonds

	Mean	**Std.Dev.**	**Diff. (in Std.Dev.)**
Bond yield (percent) weighted mean	4.39	1.64	1.00
Bid–ask spread (percent)	0.04	0.10	0.09
Maturity (years)	8.6	9.2	0.11
Issue amount (1,000 USD)	965,170	714,346	0.10
Share of bond type in total	Number of bonds		
High yield bonds	28 percent		0.83
Investment grade bonds	72 percent		0.84

Note: Table reports summary statistics for government–related bond yields. The last column lists the difference in means between government–related and non–government related bond characteristic, defined as a share of a standard deviation of government–dependent bonds.

Despite our efforts to control for key bond–, firm– and country–level confounders, it is possible that government–related bonds are qualitatively different from other bonds. If this is the case, then it is plausibly more likely for additional characteristics to be strengthening any empirical co–movement between sovereign and corporate bond yields. To provide additional reassurances, I examine the characteristics of government bond yields against their non–government counterparts. These are summarized in table 3.7. The differences are not necessarily large in magnitude: bond yields differ by one standard deviation of government–related bond yields. Issue amounts differ by 10 percent, similarly to an 11 percent difference in the maturity. The larger differences appear in the ratings category: the share of government–dependent bonds rated as investment grade exceeds its non–government related counterpart by 40 percentage points. From a sector perspective, government–related bonds are dominated by firms in the finance and manufacturing industries, while their non–government–related counterparts are primarily dominated by companies in mining, oil and gas as well as finance.

Overall, there is some evidence that government–related bonds differ statistically significantly from their counterparts. Whether this poses a problem from an inference perspective depends on whether there are other such omitted properties that I

Table 3.8: Heterogeneous impacts by government–dependence

	Dep. var: Corporate Bond Yield
LR: Sovereign Bond Yield $\times D_i$ ϕ	0.20^{**}
	(0.07)
LR: Sovereign Bond Yield θ	0.70^{*}
	(0.39)
SR Sovereign Yield (lag 1) $\times D_i$ δ_1	0.23^{***}
	(0.08)
SR Sovereign Yield (lag 1) μ_1	0.60^{*}
	(0.29)
Country–Time FE	X
Sector FE	X
Bond–controls	X
Macroeconomic–controls	X
Firm–controls	X
Observations	31,0051
Adjusted R–squared	0.80
Speed of adjustment α	6%
Durbin Watson Statistic	1.8
DW Critical Upper Bound	1.41

Note: Table reports short–run and–long run responses in percent of corporate bond yields to 1 percent increase in sovereign bond yields, accounting for heterogeneous effects by government–dependence. The test for a long–run relationship is based on the Bounds testing procedure.

have not accounted for in our model specification. Returning to our examination of heterogeneous effects. Table 3.8 confirms that being government–dependent increases the impact of sovereign bond yields on corporate bond yields by 0.20 percentage points relative to non–government–related firms.

This result does not contradict the earlier finding that the long–run relationship holds for both government–dependent and independent sub–samples in table 3.6. Rather, it is complementary: the long run empirical relationship I identify between sovereign and corporate bond yields is in part driven by mechanical ties between firms and their government–owners or shareholders, a fact that is consistent with the notion that these firms are exposed to larger transfer risks.

I implement an additional test of the transfer risk hypothesis. If transfer risks are a key determinant of how investors price fixed income assets, the sovereign–corporate yield pass–through should be weaker for firms that are exporters than for non–exporting firms. Capital controls, and in particular policy controls on the inflow of private foreign capital, have been employed by emerging market governments to i) stabilize fluctuations in the domestic interest rate and ii) prevent above optimal exchange rate appreciations Alfaro, Chari and Kanczuk (2017). Such policies can serve to stabilize yields on government bonds, but can transmit to companies by increasing the yield on foreign bonds to compensate foreign investors for the cost of the capital control. Exporting corporations have direct access to foreign currency proceeds, which could make a restriction on retaining foreign currency less damaging to them if they can substitute their hard currency revenues for otherwise borrowed hard currency. Exporting firms are defined as firms that obtain any of their revenue from outside its headquarters country. In the case of the estimation, which takes on the same form as equation 5, firm–level controls ensure that I am capturing exporter status specifically, rather than other firm–level characteristics such as profitability or leverage that may correlate with a firm's decision to export. I find that exporter–status reduces the long–run sovereign–corporate spillover by 27 percentage points, albeit not significantly so (Table 3.9). The short–run relationship is also weaker for exporting firms than for non–exporters – this time by 32 percentage points. Again, this effect has the expected sign but is not statistically significant. The results show that exporting firms' bond yields are not cointegrated with their sovereign counterparts. The finding implies that investors do not perceive transfer risks in the case of exporting companies. This is plausible if exporting firms have access to USD proceeds in the case when non-exporting firms would have to convert local currency proceeds to USD on the exchange market. If a government imposed exchange restrictions, then this transfer event would affect exporting firms to a lesser degree.

Table 3.9: Heterogeneous impacts by exporter status

	Dep. var: Corporate Bond Yield
LR: Sovereign Bond Yield $\times D_i$	-0.27
	(0.17)
LR: Sovereign Bond Yield	0.50*
	(0.25)
SR Sovereign Yield (lag 1) $\times D_i$	-0.32
	(0.28)
SR Sovereign Yield (lag 1)	0.55*
	(0.25)
Country–Time FE	X
Sector FE	X
Bond–controls	X
Macroeconomic–controls	X
Firm–controls	X
Observations	310,051
Adjusted R–squared	0.80
Speed of adjustment α	6%
Durbin Watson Statistic	1.8
DW Critical Upper Bound	1.41

Note: Table reports short–run and long–run responses in percent of corporate bond yields to 1 percent increase in sovereign bond yields, accounting for heterogeneous effects by exporter status. An exporting firm is associated with $D = 1$. The test for a long–run relationship is based on the Bounds testing procedure.

3.6.3 H4 – H6: Non–linear impacts

The presented results hold true for the dataset as a whole, but do not provide insights into whether the sovereign–corporate pass–through is symmetric. As a reminder, my key hypotheses regarding non–linear effects are:

- H4: If sovereign bond yields exceed corporate bond yields, we would expect increasing *perceived* transfer risks to eventually lead to convergence. This implies a larger speed of adjustment.

- H5: If sovereign bond yields are rising, we would expect increasing transfer risks and thus a larger speed of adjustment to the long run.

- H6: If r exceeds g, we would expect larger transfer risks and therefore a larger

speed of adjustment to the long run.

Our database allows us to test these hypotheses. This exercise is useful from a forward–looking perspective: it can uncover what types of firms are most likely to experience rising borrowing costs due to a future shift in investor beliefs that is *unrelated* to company fundamentals. The findings always reveal the economic conditions under which firms' borrowing costs are more vulnerable to the perceptions of investors.

To investigate H4 and H5, I include two interaction variables in our original error correction specification from equation 4 in turn, $\times Direction_{c(i)t}$ which is set to 1 when the sovereign bond yield has increased relative to the previous period and $\times Size_{it}$ indicates whether the sovereign bond yield exceeds the corporate bond yield.

Explicitly, this involves estimating the following regression:

$$\begin{aligned}\Delta Corp_{it} = {} & \beta^1_{c(i)} + \tau_t + \beta^2 D_{c(i)t} + \alpha(Corp_{i,t-1} - \theta_1 Sov_{c(i),t-1}) \\ & + \beta^3 D_{c(i)t} \times (Corp_{i,t-1} - \theta_2 Sov_{c(i),t-1}) + \sum_{j=1}^{p-1} \gamma_{c(i)j} \Delta Corp_{i,t-j} \\ & + \sum_{j=0}^{q-1} \rho_{c(i)j} \Delta Sov_{c(i),t-j} + \sum_{j=1}^{r-1} \boldsymbol{\lambda}'_{c(i)j} \Delta \boldsymbol{X}_{i,t-j} + \varepsilon_{it},\end{aligned} \tag{3.6}$$

where D_{it} denotes the two interaction variables in turn (they are not included in the estimation together but rather separately).

Sovereign bond yields are larger than their corporate counterparts 24 percent of the time. I find evidence for both hypotheses, as reported in table 3.10. In particular, the magnitude of the short–run relationship is largely unchanged, although the short–run impact is no longer significant when I include heterogeneous effects depending on sovereign yields exceeding corporate yields. The long–run impacts are not significantly larger when the sovereign bond yield has previously increased, or when it exceeds the corporate bond yield (based on a Wald test). As expected, I

Table 3.10: Asymmetric impacts of sovereign bond yields on corporates

	Dep. var: Corporate Bond Yield	
Interaction variable	$\times Direction_{c(i)t}$	$\times Size_{it}$
LR: Sovereign Bond Yield θ_1	0.91**	0.93**
	(0.35)	(0.46)
LR: Sovereign Bond Yield θ_2	0.95**	0.92
	(0.38)	(0.49)
SR Sovereign Yield (lag 1) ρ_1	0.57***	0.65
	(0.28)	(0.38)
Country–Time FE	X	X
Sector FE	X	X
Bond–controls	X	X
Macroeconomic–controls	X	X
Firm–controls	X	X
Government–dependence Dummy	X	X
Observations	310,051	310,051
Adjusted R–squared	0.78	0.76
Speed of adjustment β^3	13%	13%
	(0.02)	(0.02)
Speed of adjustment α	8%	9%
	(0.02)	(0.01)
Durbin Watson Statistic	1.65	1.66
DW Critical Upper Bound	1.51	1.51

Note: Table reports short–run responses and long run impacts of sovereign bond yields on corporate bond yields, while accounting for asymmetric effects on the speed of adjustment coefficient.

find a larger speed of conversion coefficient of 13 percent associated with periods of rising sovereign bond yields, and when firms can borrow more cheaply than their associated government, relative to time periods when this is not the case.

It is not clear from these results whether the speed of adjustment coefficient is *significantly* larger when sovereign bond yields exceed corporate bond yields, and when sovereign bond yields are rising, relative to when these conditions do not hold. Again I run a Wald test. Specifically, the null hypothesis is that the coefficients are equal and the test statistic is given by

$$\frac{\alpha - \beta^3}{sd(\alpha - \beta^3)} \tag{3.7}$$

where α refers to the speed of adjustment coefficients in both estimations of equation 3.6 (table 3.10), and β^3 equally differs for columns 1 and 2 of table 3.10. The resulting p–values are 0.0232 and 0.078 for the $\times Direction_{c(i)t}$ and $\times Size_{it}$ specifications respectively. This implies that corporate bond yields experience a statistically stronger pull towards their sovereign counterpart when perceived sovereign risks are on the rise (thus making an explicit transfer event more likely), and to a lesser extent when they lie below their associated risk–free rate (i.e. their respective sovereign bond yield).

The analysis began with a look at the wedge between real sovereign hard currency bond yields and real economic growth rates, showing that periods in which the latter exceeds the former in emerging economies appear to be becoming shorter–lived and less pronounced. By testing H6 I revisit the implications of this pattern for the corporate–sovereign relationship. I estimate whether inflection points in the r–g relationship co–move with perceived transfer risks: if – after controlling for exchange rates – moving into a $r > g$ phase is associated with a stronger link between sovereign and corporate bond yields, then this would be consistent with the notion that declining economic growth rates make firms particularly vulnerable to sovereign borrowing conditions when hard currency yields are rising.

I re–estimate equation 3.6 with the distinction that D_{it} now denotes an indicator variable taking on the value of 1 if r exceeds g, and 0 otherwise.

The results show that the sovereign–corporate relationship tightens during periods when r exceeds g (table 3.11), as measured by a Wald test based on the speed of adjustment coefficient β^3 relative to α (p–value of 0.0455). Corporate and sovereign bond yields tend to move more quickly towards their long–run relationship relative to the baseline estimation of equation 4. The short–run link as measured by ρ_1 also strengthens in $r > g$ periods relative to baseline.

Table 3.11: Asymmetric impacts of sovereign bond yields on corporates

	Dep. var: Corporate Bond Yield
LR: Sovereign Bond Yield θ_1	0.90^{*}
	(0.43)
LR: Sovereign Bond Yield θ_2	0.91^{**}
	(0.39)
SR Sovereign Yield (lag 1) ρ_1	0.62^{**}
	(0.29)
Country–Time FE	X
Sector FE	X
Bond–controls	X
Macroeconomic–controls	X
Firm–controls	X
Government–dependence Dummy	X
Observations	310,051
Adjusted R–squared	0.76
Speed of adjustment α	8%
	(0.03)
Speed of adjustment β^3	13%
	(0.01)
Durbin Watson Statistic	1.69
DW Critical Upper Bound	1.51

Note: Table reports short–run responses and long–run impacts of sovereign bond yields on corporate bond yields, while accounting for whether r exceeds g.

3.7 Robustness check: unobserved common factors

Dynamic panel estimations that ignore cross–sectional co–movement and dependence between countries can generate biased cointegration test results (Cerra and Saxena, 2010). Further, if such latent factors are left unaccounted for, the reported association between corporate and sovereign bond yields cannot necessarily be attributed to the transfer risk and segmented markets channels. This is because the estimation of the long–run coefficients via OLS is no longer consistent due to violation of the standard assumption that

$$plim_{n\to\infty}\frac{1}{n}X'\varepsilon = 0. \tag{3.8}$$

To see why this is the case, the data in the presence of unobserved factors can be described as follows:

$$Corp_{it} = Sov_{c(i)t}\beta_c + f_t\lambda_c + \varepsilon_{it}, \tag{3.9}$$

if we abstract from any of the control variables, time trends, long–run links and intercepts for simplicity. Not only is the error term in the corporate bond yield affected by an unobserved factor f_t, but the key regressor – the sovereign bond yield variable – will also be influenced by f_t in a different way for each country. The heterogeneous impact of the latent factor on each country is captured by the fact that the factor loading ψ_c (and γ_c above) is country–dependent:

$$Sov_{c(i)t} = f_t\psi_c + \nu_{it}, \tag{3.10}$$

The issue with estimating a model with unobserved factors lies in the correlation between the error term of the main equation 6 (i.e. $f_t\lambda_c + \varepsilon_{it}$) and the regressor $Sov_{c(i)t}$. Defining X and Y as, respectively, vectors with entries of the form $Sov_{c(i)t}$ and $Corp_{it}$, and defining the vectors f, ν, ε and u accordingly, recall that the OLS estimator is

$$\hat{\beta}_c = (X'X)^{-1}X'Y. \tag{3.11}$$

Let us define

$$u_{it} = f_t\lambda_c + \varepsilon_{it}. \tag{3.12}$$

Hence we can see that:

$$\hat{\beta}_c = (X'X)^{-1}X'(X\beta_c + u) \tag{3.13}$$

$$= (X'X)^{-1}X'X\beta_c + (X'X)^{-1}X'u \tag{3.14}$$

$$= \beta_c + \left(\frac{1}{n}X'X\right)^{-1}\frac{1}{n}X'u \tag{3.15}$$

For the distribution of $\hat{\beta}_c$ to concentrate around the true β_c asymptotically as the sample size increases, we require the term $\frac{1}{n}X'u$ to go to zero.

$$\frac{1}{n}X'u = \frac{1}{n}X'(f\lambda_c + \varepsilon) \tag{3.16}$$

$$= \frac{1}{n}(f\psi_c + \nu)'(f\lambda_c + \varepsilon) \overset{plim}{\rightarrow} E[(f\psi_c + \nu)'f\lambda_c] \tag{3.17}$$

$$= E[\psi_c'(f' + \nu')f\lambda_c] \tag{3.18}$$

$$= E[\psi_c']E[f'f]E[\lambda_c] \tag{3.19}$$

It is unclear if this last requirement is fulfilled in the data. Therefore consistency of the OLS estimator is not guaranteed.

As a prominent example of cross–country factors that might link countries to one another, monetary policy actions in influential developed economies routinely propagate to the emerging market world, affecting capital inflows and fixed income pricing in those regions.[11] This channel is particularly relevant in the case of USD–denominated borrowings. Similar external shocks could affect multiple countries at the same time, albeit in different ways depending on their idiosyncrasies. Even though US policy variables are observable, their impact on individuals countries and interlinkages between countries cannot be easily controlled for. First, linear time trends would no alleviate the situation. as such a time trend does not capture feedback effects across countries. Further, time trends apply equally to all countries in the dataset, and again would not capture this country–specific interfering factor. Time – country fixed effects would capture the heterogeneous impact of external shocks on individual countries, but again would not account for the feedback effects between countries.

There are multiple approaches for dealing with cross–correlated error terms. The key choice is between employing principal components analysis akin to Coakley, Fuertes and Smith (2002) and Song (2013), and a direct proxy approach akin to

[11] Brüning and Ivashina (2017) show that Fed rate hikes in the US lead to declines in capital flows to emerging economies, and a reduction in USD–denominated loans in those countries.

Chudik et al. (2016). The principal components approach treats the factors as coefficients that can be directly estimated. It works by first estimating the model (a suitably transformed version of equation 6) without accounting for the common shocks or interlinkages, via OLS. Based on the estimated model residuals, the first k potential factors that explain the largest share of the variance of the residuals are obtained and included in the final estimation. A key disadvantage of this approach is that it requires knowledge of k, i.e. of how many unobserved latent factors I am seeking to account for. Further, as Pesaran (2006) shows, the resulting estimator will not necessarily be consistent because the first–stage of the procedure – the estimation of the model without accounting for cross–links – generates an inconsistent estimator to begin with. It is for these two reasons that I select the Common Correlated Effects approach. I chose the CS–ARDL approach over the CS–DL approach for a number of reasons. First, the CS–DL approach computes the long run coefficients directly without estimating the short–run relationship between the sovereign and corporate bond yields. I am however interested in the short–run dynamics, as they reveal to use how far bond yields can stray from their cointegrating relationship. This knowledge provides insights into how large the arbitrage opportunities and corrections can be in the fixed income market. An additional advantage of the cross–sectionally augmented autoregressive distributed lag (CS–ARDL) approach over the direct estimation of the long run relationship is that the latter approach is inconsistent if there is reverse causality in the data from lagged corporate bond yields to contemporaneous sovereign bond yields.

The CS–ARDL approach amends the analysis to account for common factors that may affect a) multiple bonds of the same country and b) several emerging economies simultaneously. Under certain assumptions the cross–section averages of the sovereign bond yield and the corporate bond yield approximate the unobserved factors. The five assumptions are: 1) the covariance–stationarity of the unobserved common factors and 2) of the residuals, 3) identically and independently distributed factor loadings, 4) the absence of correlation between the factor loadings and

the random deviations of each country's long run coefficient from other countries' coefficients in a model and 5) heterogeneous coefficients β_c that are randomly distributed around a common mean. The appendix derives why – if these conditions hold – the differential effects of the unobserved factors on the estimation are eliminated asymptotically as N goes to infinity. The estimation is based on the following ARDL model in error correction form:

$$\begin{aligned}\Delta Corp_{it} = \beta_i + \tau_{it} - \alpha_i(Corp_{i,t-1} - \theta_i Sov_{c(i),t-1}) + \sum_{j=1}^{p-1}\gamma_{ij}\Delta Corp_{i,t-j} \\ + \sum_{j=0}^{q-1}\rho_{c(i)j}\Delta Sov_{c(i),t-j} + \sum_{j=0}^{z}\omega_{ij}\overline{Corp}_{t-j} + \sum_{j=0}^{z}\nu_{c(i)j}\overline{Sov}_{t-j} \\ + \sum_{j=1}^{r-1}\boldsymbol{\lambda}'_{ij}\Delta \boldsymbol{X}_{i,t-j} + \varepsilon_{it},\end{aligned} \tag{3.20}$$

where the averages of the sovereign bond yield and corporate bond yield across countries by week are denoted by

$$\overline{Sov}_{t-j}, \overline{Corp}_{t-j}. \tag{3.21}$$

I selected various lag orders (i.e. 1, 4 and the maximum recommended lag order of 5, or $T^{1/3}$) for the lagged terms in equation 18. The results do not differ significantly across lag specifications, and thus I present the results associated with the maximum lag of 5 as the inclusion of additional lags reduces the risks of serial correlation of the error terms. Serial correlation poses a problem for the CS–ARDL estimates from a bias perspective, i.e. the difference between the estimated coefficients and the true parameters. As in the preceding analysis I explicitly test for autocorrelation as part of the main estimation.

The mean group coefficients are estimated as the un–weighted averages of all cross-sectional specific coefficients. The inclusion of cross–sectional averages reduces the magnitude of both the long–term and short–term responses of corporate bond yields, which is consistent with the existence of unobserved factors that interfere with both sides of the estimation (table 3.12). A key drawback of the CS–ARDL

Table 3.12: Impact of Common Factors on Sovereign–Corporate Bond Yield Relationship

	Dep. var: Corporate Bond Yield	
	Baseline	**CS–DL model**
LR Sovereign Bond Yield θ	0.89^{***}	0.76^{**}
	(0.24)	(0.30)
LR Cointegrating Relationship	Y	Y
SR Sovereign Yield (lag 1) ρ_1	0.71^{***}	0.61^{**}
	(0.02)	(0.25)
Country–Time FE	X	X
Sector FE	X	X
Bond–controls	X	X
Macroeconomic–controls	X	X
Firm–controls	X	X
Observations	311,033	311,033
Adjusted R–squared	0.88	0.83
Speed of adjustment α	6%	10%
Durbin Watson Statistic	1.8	1.6
DW Critical Upper Bound	1.42	1.36

Note: Table reports short–run and long–run responses in percent of corporate bond yields to 1 percent increase in sovereign bond yields, for the baseline estimation and the CS–ARDL–model. The estimations now include cross–section averages for bonds and countries, which approximate unobserved common factors that affect multiple bonds within the same country. Coefficients are estimated via the CS–ARDL model on a bond–level. The test for a long–run relationship is based on the Bounds testing procedure. Bond characteristics include the bid–ask spread, maturity and issue amount. Macroeconomic characteristics include public debt to GDP, the current account as a share in GPD, inflation, real GDP per capita, public debt to foreign reserves, the real effective exchange rate, stock market returns and return volatility. Firm controls include interest coverage, profit margins and leverage.

approach is that the long–run estimates require sufficiently large samples along the time dimension for consistency (Chudik et al., 2016). The literature cites 30 to 50 units of time observation as moderately large, a range which our dataset exceeds by over 100 observations. Our concerns in this regard are therefore limited.

3.8 Conclusion

Using weekly bond–level data for 46 emerging markets over 2016–2019, I have investigated the time–series properties of bond yields against the backdrop of a declining wedge between emerging market growth rates and interest rates on external sovereign debt. I acknowledge that the bond yield data display a mix of unit–rooted

and stationary qualities, and account for this feature in our econometric approach. I find that sovereign and corporate bond yields exhibit a cointegrating relationship not explained by economic conditions, firm characteristics, bond properties and common factors affecting emerging countries as a whole. I test for a key theoretical transmission channel and find that investors appear to believe in transfer risks. Our results suggest that even when there are no explicit government actions to transfer the burden of debt repayment onto private foreign creditors, investor beliefs are a key force in driving bond yields together. This holds important policy implications for emerging country governments that wish to manage the impact of their foreign debt obligations on the private sector. In our data, avoiding explicit transfer events does not prevent private borrowing costs from rising even in the absence of a deterioration on private sector fundamentals.

Importantly, the studied transfer risks appear to strengthen as i) sovereign bond yields rise, ii) when sovereign bond yields exceed corporate bond yields and iii) when borrowing costs exceed economic growth rates. At a time where recent debt dynamics in emerging markets – if extrapolated – point towards a potential tightening of the GDP growth–nominal interest rate wedge (recall figure 3.3), these asymmetries in the sovereign–corporate pass–through could become particularly relevant. If I ascribe predictive qualities to the estimated relationships, then firms that currently enjoy lower borrowing costs than their sovereign country of risk, those operating in environments with rising sovereign risk and those dependent on economies where r exceeds g may be more likely to experience a belief–driven deterioration in borrowing conditions in the future.

The results relate to the behavioural finance literature if I focus on the role of rational investor behaviour in diminishing arbitrage opportunities (De Long et al., 1990). With trading strategies that involve pairs trading – i.e. when two assets are expected to re–converge, leading to one being shorted and the other being purchased – a cointegrating relationship between sovereign and corporate bonds could be

consistent with arbitrageur investors who eventually force corporate bond yields towards their sovereign equivalent. There are a number of potential reasons that are compatible with the short–term deviations from the cointegrating relationship that I reveal. First, the existence of sentiment–driven investors – referred to as "noise investor" in the behavioural finance literature – could contribute to the short–term wedge between sovereign and corporate borrowing costs. Second, transaction costs and technical constraints have made pairs trading strategies much harder to implement profitably than one would expect. These and similar other factors could very well explain why the short–run coefficient between sovereign and firm borrowing costs is smaller than the long–term pass–through suggested by our results.

Finally, this paper's insights are related more widely to the literature on firm biases and behaviour. The notion that firms may be limited in their ability or willingness to track key macroeconomic conditions has been evidenced empirically and underpinned theoretically (as shown by Coibion, Gorodnichenko and Kumar (2018), Alvarez, F. and Paciello (2011) and Gorodnichenko (2008) among others). The behavioural biases that firms have been documented to exhibit – if applied to this paper's context – could prevent them from tracking deviations of bond yields from their long–run relationship. Inattention to future circumstances by anchoring too much on present circumstances could lead firms to take on too little (too much) debt relative to equity, or to issue debt at suboptimal times, leading to deteriorating fundamentals or lost opportunity when yields revert to the long–run cointegrating relationship. Stationary linear combinations of sovereign and corporate bond yields imply that borrowing costs for firms in emerging markets may depend on their perceived home countries' creditworthiness – even if they are at times operationally independent of these sovereign counterparts. The implications of such a reverting relationship for emerging market firms depends on i) whether they remain aware of their inexorable ties to sovereign borrowing costs during times of yield divergence, and ii) how they behave during periods in which corporate and sovereign yields

stray from one another.

Let us focus on periods during which sovereign and corporate bond yields depart from each other in the short term, as was the case across emerging economies between January 2016 and January 2017 in our dataset, as well as between March 2018 and July 2019. The relevant question is whether during these times, firms adjust their debt and equity fundraising behaviour and timing. In other words, this paper's result on cointegrated sovereign and corporate bond yields provokes among others the question of whether firms sufficiently plan for their borrowing costs to eventually revert back towards their home governments' yield. If instead a short–term wedge between sovereign and corporate bond yields leads firms to underestimate the gravity force of their associated sovereign entity, then this behaviour could lend further evidence to models of rational inattention (see Sims (2003), Reis (2006), Maćkowiak and Wiederholt (2009) and Afrouzi (2016) among others). This final question offers an additional avenue for further research.

Conclusion

This thesis has aimed to enhance our understanding of the role of expectations and uncertainty in emerging country policy outcomes, by examining the regulatory and market conditions that affect lumpy investments by firms, their borrowing costs and the success of large power projects. There are a number of avenues for future research to be highlighted.

Chapter 1 shows that encouraging emerging and developing economies to adopt and publish forward–looking commitments to power generation projects can have adverse effects on economic growth. The findings reveal that infrastructure capacity additions do not always contribute to long–term economic growth and can even detract from growth in the short–term, if they are unexpected. These findings support the view that the provision of physical electricity infrastructure assets can be considered a necessary but non–sufficient requirement for more and cheaper electricity as a factor input into production, and ultimately economic growth. Sufficient conditions for sustained contributions to output remain improvements in infrastructure *services*, which will need to come from investments in maintenance as well as grid–system optimization. Further, deviations from energy plans through failed projects could represent an increase in uncertainty causing developing country firms to defer investments as they update their beliefs about energy plans. Additional data on the mode and language phrasing by which news are released by governments would allow for an investigation of the specific channels through which good and bad news filter through to households and firms. This would provide additional insights on whether how governments communicate matters as much as what they communicate. While the role of phrasing and wording is well–explored in the central bank literature (Hansen and McMahon, 2016), it is understudied in the context of sector policy in developing and emerging countries.

Chapter 2 reveals a conclusion that calls for further investigation. Given that an improvement in policy uncertainty significantly increases emerging country firms' irreversible investments in the distribution grid, the next question would be the extent to which this result can be found in other country contexts. Additionally, the supply–chain shortage induced increases in retail electricity prices in 2021 and 2022 are likely to lead some regulators to rethink their price–setting mechanisms for utilities in order to allow for more timely cost–recovery for utilities. A cross–country panel dataset would allow me to exploit exogenously induced policy shifts across the non–OECD country grouping as an additional layer of identification while being able to investigate if differences in government–credibility mitigate the negative effects of uncertainty found in Brazil.

The results of chapter 3 shed light on the particular risks from rising sovereign yields faced by firms that are constrained in their ability or incentives to track deviations of bond yields from their long–run relationship. Inattention to future circumstances by anchoring too much on present circumstances could lead firms to take on a sub–optimal amount of debt relative to equity, or to issue debt at suboptimal times, leading to deteriorating fundamentals or lost opportunity when yields revert to the long–run cointegrating relationship. This idea of rational inattention has been explored in other settings by Coibion, Gorodnichenko and Kumar (2018); Alvarez, F. and Paciello (2011); Gorodnichenko (2008) among others. Testing for the existence and economic consequences of this specific behavioural bias among emerging firms could be the focus of future work and shed light on how firm management could be incentivized to manage their perceived transfer risks.

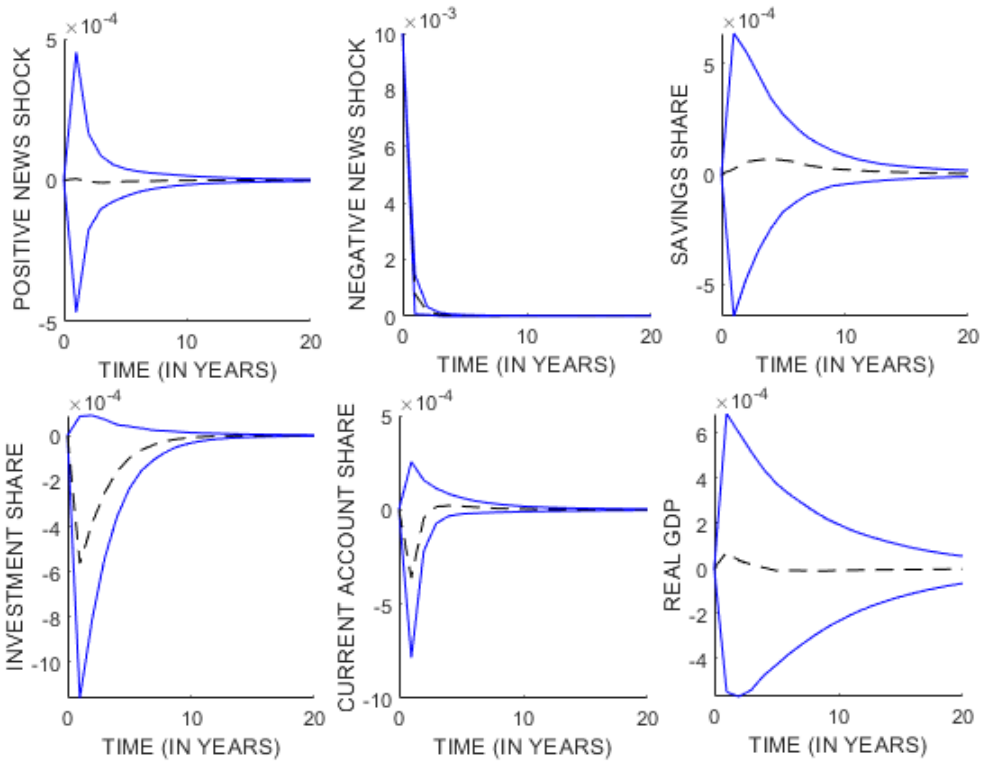
Appendices

A

Chapter 1 Appendix

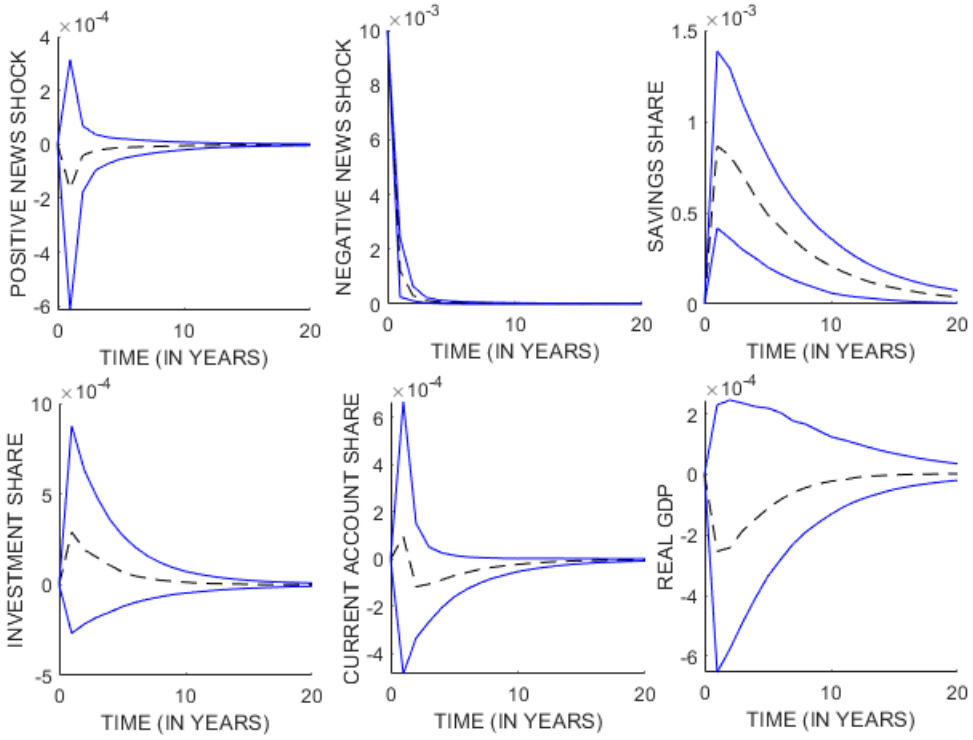
A.1 Alternative Truncations of Disappointment Shocks

Figure A.1: Impulse Responses in OECD Countries to a Disappointment Shock



Notes: Figure depicts impulse responses of the macrovariables (S,I,CA,GDP) to a 1 percent disappointment shock. The shocks are identified via Cholesky restrictions on the contemporaneous relationships between variables. Disappointment shocks are truncated at 200 % of existing capacity. The VAR(1) is estimated in levels (except for GDP) with 1 lag. The unit of the vertical axis is in decimal deviations from the situation without the shock. The blue interval correspond to the 60 percent bootstrapped confidence bands. The sample consists of a pooled dataset including 21 OECD countries spanning 163 country–year pairs.

Figure A.2: Impulse Responses in Emerging Countries to a Disappointment Shock



Notes: Figure depicts impulse responses of the macrovariables (S,I,CA,GDP) to a 1 percent disappointment shock. The shocks are identified via Cholesky restrictions on the contemporaneous relationships between variables. Disappointment shocks are truncated at 200 % of existing capacity. The VAR(1) is estimated in levels (except for GDP) with 1 lag. The unit of the vertical axis is in decimal deviations from the situation without the shock. The blue interval correspond to the 60 percent bootstrapped confidence bands. The sample consists of a pooled dataset including 21 OECD countries spanning 163 country–year pairs.

A.2 VAR representation and system coefficients

The vector autoregression model with a single lag in reduced form is represented by the following equation:

$$\begin{bmatrix} Surprise_{it} \\ Disappointment_{it} \\ SShare_{it} \\ IShare_{it} \\ CAShare_{it} \\ GDP_{it} \end{bmatrix} = A_0 + A_1 \begin{bmatrix} Surprise_{it-1} \\ Disappointment_{it-1} \\ SShare_{it-1} \\ IShare_{it-1} \\ CAShare_{it-1} \\ GDP_{it-1} \end{bmatrix} + \begin{bmatrix} \epsilon_{it}^{Surprise} \\ \epsilon_{it}^{Disappointment} \\ \epsilon_{it}^{SShare} \\ \epsilon_{it}^{IShare} \\ \epsilon_{it}^{CAShare} \\ \epsilon_{it}^{GDP} \end{bmatrix}, \quad \text{(A.1)}$$

where $E(\epsilon\prime\epsilon) = \Omega$ denotes the variance covariance matrix of the error terms. If we were to estimate a model based on this system of equations, we would have

difficulty when interpreting the impulse response functions. Because the error terms ϵ could be correlated with one another, we cannot isolate a shock to a variable from a shock to the other five variables in the system. In order to ameliorate this, we find a matrix C which we apply to the original VAR variables such that the off–diagonal elements of Ω become zero. In other words, we find a matrix C which removes the cross–correlations between residuals. This matrix will also ensure that we can uniquely define an impulse response to a shock in ϵ out of the possible impulse responses. To see how I use the Cholesky decomposition to achieve these goals, consider the below, where the equation is post–multiplied by the inverse of a Cholesky–decomposition of Ω.

$$\begin{bmatrix} Surprise_{it} \\ Disappointment_{it} \\ SShare_{it} \\ IShare_{it} \\ CAShare_{it} \\ GDP_{it} \end{bmatrix} C^{-1} = A_0 C^{-1} + A_1 \begin{bmatrix} Surprise_{it-1} \\ Disappointment_{it-1} \\ SShare_{it-1} \\ IShare_{it-1} \\ CAShare_{it-1} \\ GDP_{it-1} \end{bmatrix} C^{-1} + \begin{bmatrix} Surprise_{it-1} \\ Disappointment_{it-1} \\ \epsilon_{it}^{SShare} \\ \epsilon_{it}^{IShare} \\ \epsilon_{it}^{CAShare} \\ \epsilon_{it}^{GDP} \end{bmatrix} C^{-1}, \tag{A.2}$$

where C is

$$\begin{bmatrix} C_0^{1,1} & 0 & 0 & 0 & 0 & 0 \\ C_0^{2,1} & C_0^{2,2} & 0 & 0 & 0 & 0 \\ C_0^{3,1} & C_0^{3,2} & C_0^{3,3} & 0 & 0 & 0 \\ C_0^{4,1} & C_0^{4,2} & C_0^{4,3} & C_0^{4,4} & 0 & 0 \\ C_0^{5,1} & C_0^{5,2} & C_0^{5,3} & C_0^{5,4} & C_0^{5,5} & 0 \\ C_0^{6,1} & C_0^{6,2} & C_0^{6,3} & C_0^{6,4} & C_0^{6,5} & C_0^{6,6} \end{bmatrix} \tag{A.3}$$

The zero–elements of the upper triangular portion of C impose assumptions on which shocks can affect which variables. The Cholesky ordering is discussed in the main part of the paper. The Cholesky matrix transforms the original variables of my model so that the impulse responses can be interpreted as the response of each variable to a shock to any other variable in the system.

If we rename the transformed variables in a vector to $\tilde{Y}$, and the transformed error term vector to u we can through repeated substitution of $\tilde{Y}_{t-1}$ in the VAR equation system obtain an expression for the impulse responses in terms of the model coefficients:

$$\tilde{Y}_{t+h} = \sum_{i=0}^{h} \tilde{A}_0 + \sum_{i=0}^{h} A_1^{h-i} F u_{t+i} + A_1^{n+1} \tilde{Y}_{t-1}. \tag{A.4}$$

F is a diagonal identity matrix that allows us to feed the vector of impulse responses into all 6 VAR variables.

The A_1 matrix is printed below for all estimations conducted in the main paper.

Surprise shock in OECD countries

$$A_1 = \begin{pmatrix} 0.215 & 0.048 & 0.0149 & -0.00418 & 0.00644 & 0.00394 \\ -0.194 & -0.0317 & -7.5e^{-6} & -0.0267 & -0.027 & 0.00147 \\ -0.00421 & 0.119 & 0.923 & -0.0838 & 0.119 & -3.54e^{-4} \\ -0.0534 & -0.0162 & 0.0531 & 0.828 & -0.00893 & 0.00387 \\ 0.0641 & -0.0306 & -0.0221 & -0.00644 & 0.684 & -0.00102 \\ 0.46 & -0.23 & 0.0551 & -0.217 & 0.0983 & 0.993 \end{pmatrix} \tag{A.5}$$

Disappointment shock in OECD countries

$$A_1 = \begin{pmatrix} -0.0996 & -0.138 & 0.00383 & -0.0175 & 0.0228 & -0.00336 \\ -0.0164 & 0.253 & 0.0204 & 0.00554 & 0.00927 & 3.03^{e}{-4} \\ -0.114 & -0.0462 & 0.937 & -0.068 & -0.201 & 0.00707 \\ 0.159 & -0.0561 & -0.0738 & 0.747 & -0.416 & 0.00604 \\ 0.0305 & -0.0332 & -0.198 & -0.286 & 0.292 & -0.00127 \\ -0.121 & 0.0583 & 0.00828 & -0.0543 & -0.037 & 0.877 \end{pmatrix} \tag{A.6}$$

Surprise shock in emerging countries

$$A_1 = \begin{pmatrix} 0.215 & 0.048 & 0.0149 & -0.00418 & 0.00644 & 0.00394 \\ -0.194 & -0.0317 & -7.5^{e}{-6} & -0.0267 & -0.027 & 0.00147 \\ -0.00421 & 0.119 & 0.923 & -0.0838 & 0.119 & -3.54^{e}{-4} \\ -0.0534 & -0.0162 & 0.0531 & 0.828 & -0.00893 & 0.00387 \\ 0.0641 & -0.0306 & -0.0221 & -0.00644 & 0.684 & -0.00102 \\ 0.46 & -0.23 & 0.0551 & -0.217 & 0.0983 & 0.993 \end{pmatrix} \tag{A.7}$$

Disappointment shock in emerging countries

$$A_1 = \begin{pmatrix} -0.0996 & -0.138 & 0.00383 & -0.0175 & 0.0228 & -0.00336 \\ -0.0164 & 0.253 & 0.0204 & 0.00554 & 0.00927 & 3.03e^{-4} \\ -0.114 & -0.0462 & 0.937 & -0.068 & -0.201 & 0.00707 \\ 0.159 & -0.0561 & -0.0738 & 0.747 & -0.416 & 0.00604 \\ 0.0305 & -0.0332 & -0.198 & -0.286 & 0.292 & -0.00127 \\ -0.121 & 0.0583 & 0.00828 & -0.0543 & -0.037 & 0.877 \end{pmatrix} \quad \text{(A.8)}$$

A.3 Data Sources

Energy plans were obtained via desk research. They span 3 – 10 years of planning and were primarily found on websites hosted by the Ministry of Energy, Electricity Regulatory Agency and electric utilities.

Power plant data quantifying the timing, size (in MW), location and energy source of power plants stem from a proprietary industry database obtained through the World Bank.

Historical macro–data stem from the World Bank Development Indicators. They include Real Gross Domestic Product, Total Investment, Gross National Savings and Current Account. All variables but GDP are expressed as a share in GDP. Investment or gross capital formation is measured by the total value of the gross fixed capital formation and changes in inventories and acquisitions less disposals of valuables for a unit or sector. Gross national saving is gross disposable income less final consumption expenditure after taking account of an adjustment for pension funds. Current account is all transactions other than those in financial and capital items. The major classifications are goods and services, income and current transfers.

The **large scale macro–dataset** employed in the test for informational sufficiency is compiled from the IMF WEO tables.

Macroeconomic forecast data come from the IMF WEO forecast tables.

B

Chapter 3 Appendix

B.1 Approximation of unobserved common factors through cross–section averages

This section describes how – as part of Pesaran's (2006) CS–ARDL approach – the unobserved time–varying common factors can be approximated by the cross–section averages of the sovereign and corporate bond yields, under certain conditions.

Let us recall the following equation:

$$Corp_{ict} = \beta_c Sov_{ct} + \alpha_c d_t + \epsilon_{ict}, \tag{B.1}$$

if we abstract from any of the control variables, time trends, long–run links and intercepts for simplicity. d_t refers to observable common factors. Let us also describe how the sovereign bond yield is affected by the unobserved common factors, as well as observable time trends:

$$Sov_{ct} = \Gamma_c f_t + A_c d_t + \nu_{ict}. \tag{B.2}$$

Also note that

$$\epsilon_{ict} = \gamma_c f_t + e_{ict}. \tag{B.3}$$

The heterogeneous impact of the latent factor f_t on each country is captured by the fact that the factor loadings γ_c and Γ_c below are country–dependent. We

can combine these equations describing the impact of the common factors into a system of equations:

$$z_{ict} = \begin{bmatrix} Corp_{ict} \\ Sov_{ict} \end{bmatrix} = B_c' d_t + C_c' f_t + u_{ict}, \tag{B.4}$$

where

$$u_{ict} = \begin{bmatrix} e_{ict} + \beta_c \nu_{ict} \\ \nu_{ict} \end{bmatrix}, \tag{B.5}$$

$$B_c = \begin{bmatrix} \alpha_c & A_c \end{bmatrix} \begin{bmatrix} 1 & 0 \\ \beta_c & I \end{bmatrix} \tag{B.6}$$

and

$$C_c = \begin{bmatrix} \gamma_c & \Gamma_c \end{bmatrix} \begin{bmatrix} 1 & 0 \\ \beta_c & I \end{bmatrix}, \tag{B.7}$$

Next, consider calculating the cross–country averages of equation 25 using weights w (this involves first averaging without applying weights across bond by country, akin to the construction of a bond index).

$$\bar{z}_{wt} = \bar{B}_w' d_t + \bar{C}_w' f_t + \bar{u}_{wt}. \tag{B.8}$$

Assuming that $\bar{C}_w'$ is invertible, we can solve for the unobservable common factors:

$$f_t = (\bar{C}_w \bar{C}_w')^{-1} \bar{C}_w (\bar{z}_{wt} - \bar{B}_w' d_t - \bar{u}_{wt}). \tag{B.9}$$

Under the assumption that the individual–specific errors are distributed independently for all i, c, t and lags, and that they follow a stationary process with absolute summable autocovariances, the expected value of $\bar{u}_{wt}$ is 0.

Further, Pesaran (2006) shows that $N\bar{u}_{wt}$ is stochastically bounded, implying that there exists an arbitrarily small number such that the probability that $N\bar{u}_{wt}$ exceeds the ratio of two numbers M and N is smaller than the arbitrarily small limit. As a consequence of the probability limit of $N\bar{u}_{wt}$ is 0 as the number of countries increases. Further, the weighted average of the realizations of the factor

loadings (which are random variables) in C_c is shown by Pesaran (2006) to converge in probability to their expected value C_w. Given this and since $E(\bar{u}_{wt}) = 0$:

$$f_t - (C_w C_w')^{-1} C_w (\bar{z}_{wt} - \bar{B}_w' d_t) \xrightarrow{p} 0. \tag{B.10}$$

Hence $\bar{z_{wt}}$, i.e. the cross–section averages of the sovereign and corporate bond yields, lend themselves to being used as a proxy for the unobservable common factors which could otherwise render the estimator inconsistent.

B.2 Data Sources

- Public Debt Data for figure 1: IMF Historic Public Debt Database
- Stock market data: Global Financial Development Database
- Proprietary bond market and firm–level data: Bloomberg, IHS Markit and Dealogic
- Macroeconomic data: IMF World Economic Outlook Database

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