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Essays on Applied Macroeconometrics and on Energy in Firm Productivity

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ABSTRACT

This thesis covers two distinct topics in the field of applied econometrics. The relationship between government debt and GDP growth is explored in chapters 1 and 2. The subsequent chapters 3 and 4 are dedicated to a newly evolving field of research analysing the effects of energy price changes on firm-level performance.

Despite covering a wider array of topics, the thesis is bound together by its emphasis on data-driven work, using advanced and novel approaches to describing complex economic relationships. The novelties introduced in this work range from visual simplifications of inter-temporal dynamics (chapter 1), new econometric techniques (chapter 2), the identification of largely unaddressed heterogeneities of economic mechanisms in relation to market signals (chapter 3) as well as the elaboration on innate differences in these market fundamentals subject to firm-level characteristics (chapter 4). What unites all four stand-alone chapters is a focus on econometrically-driven empirical analyses addressing the issues of model specification, causality and endogeneity, as well as cross-sectional spillovers along multiple lines.

In what remains, this section offers a brief description of each of the four chapters. As some of the chapters are co-authored, the following paragraphs also describe the author's contributions to the separate works.

Chapter 1 offers a straightforward and descriptive contribution to the debate on fiscal discipline made popular by the seminal work of Reinhart and Rogoff (2010)[1] after policymakers have sought foundation and justification of a policy known as *austerity*, following the sovereign debt crisis of 2010. The piece

[1]Reinhart, C.M., Reinhart, V.R., Rogoff, K.S., 2012. Public Debt Overhangs: Advanced-Economy Episodes since 800. Journal of Economic Perspectives 26, 69–86. doi:10.1257/jep.26.3.69.

revisits the debate on whether or not higher debt levels impede growth rates. It contributes to the field by offering a synopsis of the recent literature and a time series perspective using a corrected data set, including a more recent and higher frequency source.

The chapter is co-authored with Paul Middleditch. It was published as *Amann, J., & Middleditch, P. (2020). Revisiting Reinhart and Rogoff after the crisis: a time series perspective. Cambridge Journal of Economics, 44(2), 343-370.* My contribution to this paper covers the literature survey, the visualisations and the drafting of the article. Paul's support and insights were of great help leading up to and during the publication process.

Chapter 2 is a continuation of chapter 1. It critically assesses the empirical literature on the *debt-threshold literature* and introduces a Granger-causality testing framework to test for regime-dependent Granger-causality under cross-sectional dependence. The chapter provides simulation evidence illustrating the validity of the proposed approach under different scenarios and provides an empirical application of the *debt-threshold hypothesis*. Finally, it offers a vignette for the statistical package written in the *R* programming language to make the proposed approach more accessible to practitioners. All work on this chapter is my own.

Chapter 3 examines the effect of a change in fossil fuel subsidies on the manufacturing industry of an oil-rich Middle Eastern economy. Using a novel firm-level micro data set on Omani manufacturing enterprises, the piece shows that fossil fuel price increases lead to improved productivity, efficiency, and notable business upgrading. The findings in this paper indicate that subsidy reforms may not only be used to achieve environmental goals but may also drive upgrading and modernisation processes of firms and can ultimately improve economic performance.

This chapter is co-authored with Nicola Cantore, Massimiliano Calí, Valentin Todorov, and Charles Fang Chin, and was published as *Amann, J., Cantore, N., Calí, M., Todorov, V., & Cheng, C. F. C. (2021). Switching it up: The effect of energy price reforms in Oman. World Development, 142, 105252.* The respective contributions to the piece are as follows: N. Cantore and M. Calí provided critical inputs on various versions of the draft pertaining to the structure of the argument and robustness of the results. C.F.C Cheng worked with me on the construction of the database and provided valuable inputs about the Omani pol-

icy context and government initiatives. V. Todorov provided valuable insights regarding the survey of industrial firms and deflation of Omani production data. All remaining work including the data compilation, empirical work, drafting of the paper and revisions during the peer-review process were done by me.

Chapter 4 is a continuation of the theme of chapter 3. It is collaborative and ongoing work with the World Bank. The objective of this project is to (i) gather more cross-country evidence on the effect of policy change in the energy market; (ii) further explore heterogeneities in firm-level responses to energy price surges; (iii) investigate potential non-linear effects energy price variations have on firm performance. Parts of this chapter were prepared to contribute to the *Energy Sector Management Assistance Program - Energy Subsidy Reform Facility*'s flagship study.

This chapter is work in progress and a collaboration with Arti Grover and Dirk Heine from the World Bank, both of which provided valuable insights and comments during the various drafting stages of the document. All data work, the empirical analysis and the drafting of the chapter is my own work.

DECLARATION

The University of Nottingham
PhD by published work Candidate Declaration

Candidate Name: Juergen Amann

School: School of Economics, The University of Nottingham

Thesis Title: Essays on Applied Macroeconometrics and on Energy in Firm Productivity

Declaration to be completed by the candidate:

I declare that no portion of this work referred to in this thesis has been submitted in support of an application for another degree or qualification of this or any other university or other institute of learning.

Signed:  Date: December 17, 2022

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CHAPTER

ONE

REVISITING REINHART & ROGOFF AFTER THE CRISIS: A TIME SERIES PERSPECTIVE.

Abstract This paper offers a straightforward and descriptive contribution to the recent and busy debate on fiscal discipline made popular by the seminal Reinhart and Rogoff (2010) paper, after policymakers have sought foundation and justification for a policy known as austerity measures, following on from the sovereign debt crisis of 2010.

We revisit the debate on whether or not higher debt levels impede growth rates and offer a time series perspective of a corrected data set and a more recent higher-frequency source. We find that with further hindsight, and from a time series perspective, there is little to no support for the view that higher levels of debt cause reductions in economic activity. In contrast to Reinhart and Rogoff (2010), we suggest that economic slumps tend to cause debt build-ups rather than vice versa.

Key words: Austerity, Macroeconomic policy, Fiscal policy
JEL classifications: E60, E62, E65

1.1 Introduction

In the aftermath of the global financial crisis, we revisit the widespread debate on fiscal discipline that has become crystallised around a controversial study by Reinhart and Rogoff (2010), the findings of which suggest that countries with higher debt/GDP ratios (of above 90 per cent) are associated with impeded growth rates. Our contribution to this debate is a time series analysis that makes use of a more recent and higher-frequency data source, obtained from the OECD, alongside a corrected version of the original data from Herndon et al. (2014), responsible for previous implications that austerity measures can be implemented to reactivate an economy experiencing a deep recession.

In contrast to most of the literature on the debt growth nexus, our approach uses a deliberately simplistic time series perspective. We provide an exploratory and descriptive presentation of the facts in a similar way to Reinhart et al. (2012) and some of the earlier literature surrounding the findings of Reinhart and Rogoff (2010). We do so alongside a comprehensive debt-threshold analysis which we suggest is perfectly adequate for providing further evidence to this busy debate, offering an alternative view on a topic that has been the subject of wide controversy inside and beyond the field of economics. We consider our contribution a worthy addition to the literature, given that more sophisticated empirical studies have so far found inconclusive support for Reinhart and Rogoff (2010) 's 'debt-threshold' hypothesis and are often confronted with severe empirical challenges. We find little to no evidence to support the view that higher public debt levels dampen economic growth. Instead, the converse case, where depressions in output lead to higher debt, is more likely in line with other papers that have stressed the reverse-causal nature of economic growth and public debt (Irons and Bivens, 2010; Lof and Malinen, 2014; Puente-Ajovín and Sanso-Navarro, 2015).

The financial crisis of 2007 brought an extraordinary slump in economic activity and significant increases in gross government debt for many Western developed economies. Increased borrowing and the need to recapitalise financial institutions left European governments vulnerable in terms of borrowing ability, consequently facing sovereign debt crises. Even though the causality linkage between public debt and economic growth is rather complex and not yet fully understood, it is believed to be best described through a bidirectional relationship. In conventional views, public debt build-ups through increases in

public spending are assumed to have a positive short-run expansionary effect on demand, but also a crowding out of capital and thus lower economic growth in the medium to long run. On the other hand, low economic growth is also likely to induce higher public debt.

Given the rise in gross government debt figures across advanced nations, it is of little surprise that scholars have tried to find an answer to the question of whether or not economic growth can be stifled by excessive public debt. Up until the unfolding of the financial crisis, there had been little research in this area. An influential study by Reinhart and Rogoff (2010) found a link between public debt and economic growth with evidence of a debt-threshold (of 90 per cent) at which economic growth is reduced by half. This study is not alone in support of the debt-threshold hypothesis (Baum et al., 2013; Caner et al., 2010; Casni et al., 2014; Cecchetti et al., 2011; Woo and Kumar, 2015).

In an environment of surging public debt and crumbling growth rates, international organisations and policymakers have found their own interpretation of studies such as this to legitimise rigorous public spending cuts, commonly referred to as austerity measures; a term used to describe a form of fiscal discipline, and discussed in detail by Konzelmann (2014).[1] Whereas the effectiveness and legitimacy of austerity measures are widely discussed in public, economic and political arenas, the findings in Reinhart and Rogoff (2010) have also provoked rigorous discussion in the field of applied economics, including contradictions by Chang and Chiang (2012) and Panizza and Presbitero (2014), and serious technical challenges to the validity of the methodology, such as Herndon et al. (2014), Kourtellos et al. (2013) and Minea and Parent (2012).

In this study, we revisit the busy debate on the causal relationship between economic growth and sovereign debt by offering a twofold contribution: In Section 2, we compartmentalise the literature using the most recent empirical contributions surrounding the field of fiscal discipline, discussing empirically motivated work in the context of correlation, causality, endogeneity and cross-

[1] See, for example, Olli Rehn, EU Commissioner for Economic Affairs, International Labor Organisation on 9th April 2013: *'Yet, public debt in Europe is expected to stabilise only by 2014 and to do so at above 90 per cent of GDP. Serious empirical research has shown that public debt is a permanent drag on growth at such high levels. If it is not reduced, it will become an ever-heavier burden on our economies, eating resources that could otherwise be channelled into productive investment needed to support job creation'* (http://europa.eu/rapid/press-release_SPEECH-13-294_en.htm?locale=en); Paul Ryan in 'The Path to Prosperity. A Blueprint for American Renewal'; or, as noted in Minea and Parent (2012), by the OECD, the EU Commission and the French Report on Public Finance in 2010.

country heterogeneity. We argue that the field has yet to present a coherent framework with consistent evidence for the existence of a 'debt-threshold' or a strong case for a significant and negative causal link between public debt and economic growth. In Section 3, we emphasise the need for a more careful evaluation in the context of heterogeneity patterns and threshold effects via a simple visual analysis of two comprehensive data sets. We employ the corrected Reinhart and Rogoff (2010)'s data provided by Herndon et al. (2014) and a more recent and higher-frequency OECD data source to provide a non-parametric and descriptive evaluation of both the potential causal link between the variables as well as the 'debt-threshold' hypothesis. A key advantage of extending our analysis to encompass the higher-frequency data source is the additional ability to consider higher-frequency information, including intra-annual fluctuations. However, one might question the usefulness of this additional measure, given the notorious persistence in debt and growth dynamics generally. In the context of the Reinhart and Rogoff (2010) study, the usefulness arises from the period in question. The OECD sample benefits from an increased scope and captures a period of increased volatility witnessed since 2009. We, therefore, suggest that the perspective provided by the OECD quarterly data is an enhanced one. The use of lower-frequency data can lead to confusion over the direction of causality, a vital issue in the context of this debate over the relationship between debt and growth. The causal relationship can appear immediate in lower-frequency data, which may explain the conflicting findings in the debt growth nexus literature, underlining the value of exploratory analyses such as this study. By comparing different frequencies with varying data coverage, we illustrate robustness in our findings that holds despite some data discrepancies across both sets.[2] Given that debt levels have risen consistently from the beginning of the financial crisis. For many countries surpassing the 90 per cent mark of the sovereign debt-to-GDP ratio, this study benefits from a longer time series perspective on the correlation between debt levels and economic growth, one which encompasses these crucial debt build-ups in the latter part of the OECD sample. We find consistency across both data sets and provide particular focus and com-

[2]It has to be noted that the debt regime classifications follow the ad hoc rule outlined in the paper based on two secondary data sets and may at times be inconsistent or provide data for non-overlapping periods only; see, for example, Italy or Greece in Figure 1.7 vs. Figure 1.9. It is noteworthy to point out that the results of the analysis hold despite these inconsistencies. Whereas there is considerable variation regarding some of the values of the variables analysed in this study across both data sets, Table 1.1 reveals that they are internally consistent over their domains.

parison of the period leading up to the financial crisis with the period of global recovery, where economic activity has returned to more normal levels.

With the benefit of the extended scope provided by the two different data sets, we illustrate that there is little to no evidence to support the view that higher public debt levels dampen economic growth. Instead, our findings support those who suggest a reverse causality, where slumps in economic activity are primarily responsible for increases in public debt. This becomes obvious as debt build-ups typically proceed with economic downturns, a regular pattern, irrespective of the actual debt level. At the same time, even though actual growth outcomes are shown to be high in volatility, median growth rates for countries above the 90 per cent threshold are indistinguishable from their low-debt counterparts. As debt levels have risen consistently from the beginning of the financial crisis, and for many countries surpassing the 90 per cent mark of the sovereign debt-to-GDP ratio, this study benefits from a longer and time series perspective on the correlation between debt levels and economic growth. With the benefit of extended scope and hindsight, our study finds no clear-cut evidence in favour of the debt-threshold hypothesis. We conclude that our analysis throws serious doubt over previous findings that austerity measures might assist in the reactivation of an economy from a deep recession.

1.2 Literature

Within the empirical literature, numerous approaches have been employed in the attempt to shed light on the debt–growth relationship.[3] While we acknowledge the broader scope of the literature, the purpose of this study is to concentrate on the empirical post-crisis strand, which has the 'debt-threshold' hypothesis at its core. We argue that this particular strand of the literature has yet to present a coherent framework with consistent evidence for the existence of a 'debt tipping point' or a strong case of a significant and negative causal link between public debt and economic growth. We show this in the preceding paragraphs using the delineating factors of exploratory evidence, correlation and causality, endogeneity, and cross-country heterogeneity.

[3]Multiple studies have been concerned with the sustainability of debt policies which, among other techniques, base their assumption of debt sustainability on time series properties of the debt variable; see, for example, Bohn (2008); Claeys (2009) for a comprehensive survey of this field.

1.2.1 Exploratory evidence

In their influential study, Reinhart and Rogoff (2010) suggest a debt threshold of 90 per cent at which growth is reduced by half for a sample of OECD countries. More precisely, their '[...] main result is that. In contrast, the link between growth and debt seems relatively weak at 'normal' debt levels; median growth rates for countries with public debt over roughly 90 per cent of GDP are about one per cent lower than otherwise' (Reinhart and Rogoff, 2010, p. 573). Even if it is clear that association or correlation between two variables by no means implies a causal effect of one variable on the other, the authors are probably over-ambitious in arguing that, '[...] when gross external debt reaches 60 per cent of GDP, annual growth declines by about two per cent; for levels of external debt above 90 per cent of GDP, growth rates are roughly cut in half' (Reinhart and Rogoff, 2010, p. 573).

The Reinhart and Rogoff (2010) paper has also sparked controversy through the empirical replication of their study by Herndon et al. (2014), who reported that coding errors, selective data exclusion and unconventional weighting of summary statistics lead to an inaccurate representation of the relationship between public debt and GDP growth for the data sampled. After correcting these deficiencies, the previously reported extraordinary debt threshold effect becomes significantly smaller, leading Herndon et al. (2014, p. 278) to conclude that 'policy-makers cannot defend austerity measures on the grounds that public debt levels greater than 90 per cent of GDP will consistently produce sharp declines in economic growth'. Nonetheless, it would be unjust and relatively convenient to hold Reinhart and Rogoff (2010) accountable for the direction into which the public, political or economic debate has been leaning, let alone for the economic consequence of austerity itself. Reinhart and Rogoff (2010) might have been the first to discuss the adverse effect of high sovereign debt levels on economic growth outcomes after 2008, but their findings have not remained without support. Various studies have provided a more formal test of the 90 per cent 'debt-threshold' hypothesis. They have, to some extent, confirmed its existence: In a literature survey-style paper, Reinhart et al. (2012) extend the analysis of Reinhart and Rogoff (2010) to analyse 'debt overhangs' in advanced economies starting from the year 1800. The authors define 'debt overhangs' as periods where debt-to-GDP ratios exceed 90 per cent for at least five years and compare average GDP growth rates for these regimes. The authors do not ad-

dress the legitimacy of their 90 per cent ad hoc 'debt-threshold' view or their choice of the five-year selection window, even though they argue that their results are '[...] reasonably robust to choices other than 90 per cent as the critical threshold' (Reinhart et al., 2012, p. 70). What makes the threshold argument problematic in the eyes of the authors of this study is that through their ad hoc selection rule, Reinhart et al. (2012) identify 26 debt overhang periods for only 13 out of their sample of 22 advanced countries and note that '[...] many debt overhangs result from costly wars. There are distinct clusters around World War II and, to a lesser extent, World War I' (Reinhart et al., 2012, p. 77). This leaves the analysis for more recent public debt overhangs since 1970 for advanced economies to no more than six countries: Belgium (1982–2005), Canada (1992–99), Greece (1993–2012), Ireland (1983–93), Italy (1988–2012) and Japan (1995–2012) (Reinhart et al., 2012, Table 2).

Among other studies that have motivated their research using descriptive and exploratory data analysis, only a few have found quantitatively similar effects. For example, Égert (2015B) performs a descriptive analysis of the original Reinhart and Rogoff (2010) data, its corrected counterpart (Herndon et al., 2014) as well as a third data set by Égert (2015A) where the author had matched government debt data presented in Reinhart and Rogoff (2010) with growth rates from Barro and Ursula's (2010) data set. In line with the results in Égert (2015A), Égert (2015B) shows that real GDP growth slows down considerably when the debt-to-GDP ratio surpasses 30 per cent but also finds no further slowdown for higher debt levels. This observation holds when using lagged central government debt levels and 10-year average growth rates. Furthermore, this study claims that exact instances where public debt of over 90 per cent slows economic growth are not present in the data: the stylised fact of a debt overhang regime appears to be a statistical fallacy (Égert, 2015B).

Similar to Égert (2015A), Minea and Parent (2012) was one of the first papers to investigate the claims made by Reinhart and Rogoff (2010) by compiling an alternative data set which features the same countries as in the original study and resorts to GDP data from Bolt and van Zanden (2014) to calculate the GDP growth rate as well as from Ali Abbas et al. (2011) for public debt measures. Minea and Villieu (2009) undertake the same empirical analysis based on discrete bins associating pre-defined debt threshold levels of <30 per cent, 30–60 per cent and >90 per cent with their corresponding growth rates and find a much less pronounced dip for the 'debt overhang' regime. Similar exercises were un-

dertaken by Irons and Bivens (2010), Bell et al. (2015) or Amann and Middleditch (2017) who all use scatter-plot techniques of (variations of) Reinhart and Rogoff (2010) original data and find little to no evidence of a comparably devastating drop in GDP growth rates at or around the 'debt overhang' threshold. The same observation is made by Puente-Ajovín and Sanso-Navarro (2015), who plot the 10-year average government debt-to-GDP ratio against real GDP growth for 16 OECD countries between 1980 and 2009, or by Afonso and Jalles (2013), who plot GDP growth rates against sovereign debt (Afonso and Jalles, 2013, see Figure 1b for the sub-set of OECD countries). On the other hand, (Woo and Kumar, 2015, Figure 3) look at 38 advanced and emerging economies over the period 1970–2007 and present evidence suggesting that average five years ahead GDP growth rates are considerably lower for countries with an initial debt-to-GDP ratio of above 90 per cent, compared to countries with a ratio of below 30 per cent across various country groups.

Finally, Pescatori et al. (2014) follow a somewhat different set-up to explore the long-run relationship between debt and growth when plotting the average growth rates of the sets of countries exceeding a varying threshold parameter over a time window of 1, 5, 10 and 15 years. The authors conduct their analysis for a sample of 34 countries spanning the period 1875–2010 and find no evidence in favour of the 'debt-threshold' hypothesis or a debt overhang regime with innately different growth dynamics. Instead, they report that the association of debt and growth tends to weaken for higher debt levels, particularly when controlling for peer country growth rates. The authors also present evidence suggesting that the debt trajectory may be even more important than the actual level of debt to predict future economic growth outcomes.

1.2.2 From correlation to causality

From an empirical point of view, a less sophisticated approach to learning about the relationship between economic growth and sovereign debt would be to investigate the sign and statistical significance of the debt-to-GDP ratio coefficient of the simple linear model:

$$GR_t = \alpha + \beta D_t + u_t \tag{1.2.1}$$

where GR_t is the percentage growth rate of output between periods t and $t+k$ (also referred to as the k-period ahead growth rate) and D_t is the debt-to-GDP

ratio (country subscript i suppressed). An immediate issue arising is the obvious endogeneity problem resulting in an estimation bias of:

$$\hat{\beta} \longrightarrow \frac{\beta\sigma_v^2 + \theta\sigma_u^2}{\sigma_v^2 + \theta^2\sigma_u^2} \tag{1.2.2}$$

which we derive in Appendix 1.B. This concern has been addressed in the literature, among others, by Bell et al. (2015); Dube (2013); Panizza and Presbitero (2014). Apart from this algebraic relationship, the endogenous connection between both variables becomes even more illustrative if we consider the fact that some policy interventions may explicitly further endogenise both variables. For example, it is widely believed that debt financing can be a useful way to help an economy recover more quickly from a recession. It is hence more likely to occur during episodes of meagre growth. Conversely, austerity advocates have argued that particularly high debt levels must be brought down to allow for a swift economic recovery.

A straightforward way common within the literature to address this issue is by treating both GR and D as endogenous, employing a vector autoregressive (VAR) model of the form:

$$GR_t = \alpha + \sum_{\ell=1}^{p_D} \beta_\ell D_{t-\ell} + \sum_{\ell=1}^{p_{GR}} \gamma_\ell GR_{t-\ell} + u_t \tag{1.2.3}$$

$$D_t = \gamma + \sum_{\ell=1}^{p_{GR}} \theta_\ell GR_{t-\ell} + \sum_{\ell=1}^{p_D} \phi_\ell D_{t-\ell} + v_t, \tag{1.2.4}$$

where (p_D, p_{GR}) denotes the selected lag length of both variables. One can then think of an endogeneity test of past debt-to-GDP ratios on GDP growth as a simple test of non-Granger causality formulated as:

$$H_0 : \beta_\ell = 0 \quad \text{for} \quad \ell = 1, \ldots, p_D.$$

In Equation 1.2.3, a positive side-effect is that this set-up accounts for the effect of debt trajectory, which is rarely addressed outside the VAR framework (Dube, 2013; Pescatori et al., 2014). In this context, an interesting point has been raised by Irons and Bivens (2010), who discuss that the traditional crowding-out argument is more concerned with the actual flow (deficit) rather than the stock (of debt). Therefore, debt build-ups are rather a symptom than the causal root of

stifled economic growth. This rationale provides a more theoretical argument for a 'reverse causality' link from low growth to high debt, where high deficit spending may be one but not the only driving force of crumbling GDP growth rates.

As it turns out, these 'reversely causal' links are readily found in the literature. In a sample of 16 OECD countries spanning 30 years, Puente-Ajovín and Sanso-Navarro (2015) apply a panel bootstrap Granger causality test developed by Kónya (2006), which accounts for cross-country heterogeneity as well as cross-sectional dependence. The authors fail to report evidence that government debt Granger causes real GDP growth but find evidence that economic growth negatively affects gross debt.

The same implication of 'reverse causality' is found by Lof and Malinen (2014) who employ a VAR analysis to investigate the debt–growth nexus utilising impulse-response functions for a panel of 20 and 10 developed and developing countries between 1905–2008 and 1954–2008, respectively. As before, the authors find no significant impact on the GDP growth rate when introducing a shock to debt, but a significant negative effect of a GDP shock on debt. By applying the Hsiao (1981) procedure, Gómez-Puig and Sosvilla-Rivero (2015) test for bidirectional Granger causality analysis for central and peripheral countries of the European Economic and Monetary Union (EMU) between 1980 and 2013 with little evidence of a causal relationship between both variables. However, when allowing for endogenously detected breakpoints, the authors report evidence of an adverse effect of debt on economic growth for some EMU countries and a significant impact for some other members.

Another study which analyses a Granger causal relationship is Irons and Bivens (2010), who study the debt–growth nexus for the case of the USA and fail to reject non-Granger causality going from debt to growth but finds significant evidence of a reverse causality link from growth to debt. Bell et al. (2015) analyse the causality link of growth and debt employing random coefficients and multi-level distributed lag models to capture time- and country-dependent heterogeneity. They present empirical evidence of a reverse causality link going from (low) economic growth to (high) public debt, with results that indicate strong patterns of heterogeneity and claim that the aggregated presentation of the debt–growth nexus through 'stylised facts' as in Reinhart and Rogoff (2010) is misleading, due to cross-country variation. Amann and Middleditch (2017), in a UK-specific study, make use of Granger causality and co-integration tests that

account for structural breaks and find no empirical support for the hypothesis that fiscal discipline can directly restore economic activity after a recession or that there is any statistically significant relationship between increasing public debt and growth. By contrast, Afonso and Hauptmeier (2009) analyse the Granger causality relationship between economic growth and the current primary surplus over GDP and the debt-to-GDP ratio. For 20 OECD countries between 1988 and 2001, they report a significant, bivariate Granger causality relation between the debt-to-GDP ratio and GDP growth. They add further evidence to the possibility of country-specific heterogeneity (Afonso and Hauptmeier, 2009, Appendix 1). One crucial point regarding the above-discussed literature is that even though Granger causality-related testing procedures can adequately model endogeneity, the issues arising from omitting relevant variables from the model remain. This is shown by Lütkepohl (1982) who argues that causal structures of sub- processes of multivariate stochastic processes do not allow the drawing of a conclusion about the causal structure of the higher dimensional process. Consequently, any Granger causality findings in a subprocess may be due to an omitted variable (and vice versa) and affect empirical results. Relationships between aggregated variables are highly complex with possible multi-directional statistical connections, feedback loops and two-way multi-lagged effects, all contributing to the realised chain of economic events. Moreover, the VAR approach, as described in Equations 1.2.3 and 1.2.4, is limited to the linear domain by construction.

As shown in Baek and Brock (1992), such parametric and linear Granger causality tests are found to have low power against certain non-linear alternatives. Even though advances have been made to (endogenously) account for non-linearities, and threshold effects (Galvão, 2006) or time-varying coefficients (Christopoulos and León-Ledesma, 2008), to the knowledge of the authors, none of these techniques have been applied to the debt–growth nexus yet.

1.2.3 Endogeneity in a cross-country setting

An alternative way to address reverse causality is employing a method involving instrumental variables. However, researchers have found identifying appropriate or adequately exogenous instruments difficult. Panizza and Presbitero (2014) instrument the debt-to-GDP ratio with the valuation effect of debt held in foreign currency and exchange rate movements. They find little to no ev-

idence of a debt threshold, arguing against the threshold view as a justification for fiscal consolidation. However, their instrument of choice has values close to zero with zero variance for France, Germany, Japan, the Netherlands and the USA. These five countries do not hold sovereign debt in foreign currency.

Checherita-Westphal and Rother (2012) investigate the average impact of government debt on per capita GDP growth for 12 Euro area countries spanning the period 1970–2008. Their exogenous instrument for country i is the averaged debt-to-GDP ratio of all remaining $(12 - i)$ countries; a proposition which may cast doubts in terms of validity: Only if 'there is no strong relationship between debt levels in other Euro areas countries and the per-capita GDP growth rate in one specific country' (Checherita-Westphal and Rother, 2012, p. 1398), their chosen instrument can be considered adequate. In practice, the proposition that there is no connection between the debt-to-GDP ratios and GDP growth rates for a set of countries within a monetary union might be questionable. As rightly pointed out by Panizza and Presbitero (2014), this is like assuming that under similar absolute debt measures for all countries, the GDP of country i is not affected by all remaining countries. Furthermore, the existence of 'debt spill-overs' is far from just a theoretical possibility; for example, in a scenario like the European banking crisis where member countries had to join forces to rescue systemic, transnational banking conglomerates, it is entirely imaginable that such joint efforts may have carried over to affect the economic performances of all involved countries. The collective bailout of Fortis by Belgium and the Netherlands in September 2008 is an illustrative example.

Checherita-Westphal and Rother (2012) also employ endogenous instruments to combat endogeneity challenges through a Generalized Method of Moments (GMM) estimation framework. Similarly, Furceri and Zdzienicka (2012) adopt a two-step GMM-system estimator when analysing the effect of debt, currency and banking on growth for a sample of 154 countries from 1970–2008. They conclude that debt crises can cause long and persistent losses in output growth and may be more harmful to economic growth than the latter two. This approach has proven to be quite popular; see, for example, Afonso and Jalles (2013); Padoan et al. (2012); Woo and Kumar (2015) who all use GMM alongside more conventional tools such as simple Ordinary Least Squares (OLS) or panel fixed and random effects (FE and RE).

Unfortunately, including lagged, endogenous instruments may not adequately address the (potential) endogeneity problem in this case, as this method was ini-

tially designed for microeconomic purposes. It is not well suited for data sets with a relatively small number of cross-sectional observations (Bond, 2002), a key feature of macroeconomic data sets. More specifically, difference and system GMM may be severely affected by the problem of weak instruments (Bun and Windmeijer, 2010), which may not be solved by simply introducing more lags to the system but instead increases the probability of introducing spuriousness (Bazzi and Clemens, 2013). These significant limitations raise doubts regarding its legitimacy in this context: Panizza and Presbitero (2014) point to evidence suggesting potential problems with some of the GMM estimation results reported in the literature.

To summarise, empirical efforts to unfold the debt-to-growth relationship through a cross-country framework are challenged by their capability to control adequately for model simultaneity. Regarding the IV approach, valid exogenous instruments are difficult to find and inadequate in most cases. In contrast, an endogenous instrument set-up may be ill-suited for this empirical exercise. An interesting point is raised by Dube (2013, p. 7), who observes that most papers following a cross-country approach add lagged GDP levels, but hardly ever lagged GDP growth rates as controls to their estimation. He stresses the importance of accounting for the evolution of growth dynamics to adequately account for reverse causality linkages, as increases in debt are found to affect past but not future growth figures.

1.2.4 Heterogeneity

Important implications arise around the issue of cross-country heterogeneity. In this context, we do not intend to direct the discussion toward the effects of debt on growth concerning the development stages of country groups. We recognise that the broad empirical literature indicates that debt composition and the general impact of debt overhangs affect developing countries more than their developed counterparts. However, a more pronounced survey of this field would go beyond the scope of this paper. Instead, we expand on the observation by Eberhardt and Presbitero (2015) who apply the same visual analysis as Reinhart and Rogoff (2010) using discrete bins as well as a fractional polynomial regression of GDP growth against the debt-to-GDP ratio, thereby producing visual evidence in general support of the 90 per cent threshold hypothesis. At the same time, their empirical models, which allow for country-specific coefficients,

fail to produce supporting econometric evidence of this result.

This is a crucial interjection: Given a model that assumes parameter homogeneity and finds an aggregated significant effect of some sort, it does not necessarily follow that the observed aggregated pattern holds true for most units on an individual level as well. In other words, recorded non-linearities may simply be spuriously occurring results of aggregating cross-sectional heterogeneities rather than systemic nature (Haque et al., 1999). Eberhardt and Presbitero (2015) also point out that underlying and unobserved factors might significantly affect the estimation outcomes and crucially depend on the data set used for the analysis. Particularly for a set of homogeneous and strongly interconnected entities, cross-sectional dependence may severely bias estimation results. Using a dynamic Common Correlated Effects (CCE) Mean Group estimator, which allows for varying country-level coefficients, they find no empirical evidence of a common cross-country debt threshold. The importance of accounting for cross-country heterogeneity is also stressed by Chudik et al. (2017), who provide evidence of cross-sectional heterogeneity of the 'debt-threshold' hypothesis in a dynamic, heterogeneous panel set-up. The authors offer threshold estimates of between 60 and 80 per cent for the full sample and 80 per cent for the advanced economies, and between 30 and 60 per cent for developing countries, respectively, when not accounting for unobserved common factors. Their cross-sectionally augmented models fail to reject the hypothesis of no simple debt threshold. Consequently, they draw no conclusive picture of a significant debt threshold for either advanced or developing economies.

Further evidence of cross-country heterogeneity in debt effects is also made in the VAR framework by Gómez-Puig and Sosvilla-Rivero (2015). The authors find no evidence of Granger causality between debt and economic growth when analysing a sample up to 2009. However, when allowing for endogenously detected breakpoints, the authors report evidence of a significant, negative Granger causality for five of the 11 countries for the period after the breaks with potential threshold levels ranging from 53 to 103 per cent but also identify another set of other countries which retain their positive Granger causality relationship of debt to growth even after the crisis. The authors thus show how cross-country heterogeneity is of fundamental importance even when looking at a relatively homogeneous group of (EMU) countries. The observation of heterogeneity has also been raised on the inter-temporal front: Exemplarily, Minea and Parent (2012) acknowledge the existence of countries with high public debt

ratios (above 115 per cent) and high economic growth; particularly in the post-World War II period and last decade of the twentieth century. As discussed before, Baum et al. (2013) find evidence of a considerable increase in the significant debt threshold (from 66 to 72 per cent and 96 per cent for the non-dynamic and dynamic panel, respectively) if the crisis years 2008–10 are added to the analysis. Including the period 1980–89 results in insignificant threshold estimates.

1.2.5 Summary

In this section, we have surveyed a wide and contradictory field of academic research concerned with the importance of public debt for economic activity. We have focused on the most consistent and well-developed body of the 'debt literature' in advanced economies regarding modelling choices. Our reading of the literature is that the debt–growth relationship is more complex and heterogeneous when considered across countries and periods than initially assumed. Moreover, it also raises the question of whether gross debt is indeed the correct measure for this investigation in the first place because of its inherent inability to consider the cross-sectional heterogeneity of national debt compositions. In other words, if there is a common factor in the composed measure of sovereign debt that stifles economic growth, the current practice of analysing (aggregated) sovereign gross debt makes it impossible to disentangle such a force in the same way that aggregation might lead to a potentially misleading formulation of the 'debt-threshold' hypothesis.

To date, one interesting characteristic of the debate is that as the argument develops over time, the consensus appears to be reversing, with later studies tending to disagree with the existence of the debt threshold. We can see this through the increase in studies finding evidence against the debt-threshold nexus; or failing to reject the reverse causality hypothesis since the intervention by Herndon et al. (2014), a study that appears to have attracted the attention of more technical treatments capable of reversing the earlier findings in favour of the debt threshold when allowing for issues such as cross-country heterogeneity and parameter instability. Until more accentuated data are available, researchers must make an educated guess as to how homogeneous or heterogeneous country-level data may allow for a precise aggregated analysis of the debt–growth nexus. We intend to address this in the second part of the paper,

where we look at two separate data sets and discuss both common and heterogeneous patterns in the data before exploring ways to identify debt tipping points.

1.3 Exploratory data analysis: a time series perspective

In this section, we revisit the central claims made in Reinhart and Rogoff (2010) by employing a novel exploratory approach to visually disentangle the linkage between economic growth and public debt. We do this by using the corrected Reinhart and Rogoff (2010)'s data set following Herndon et al. (2014), which we contrast with up-to-date and higher-frequency OECD data.

We offer a deliberately uncomplicated contradiction to the findings of Reinhart and Rogoff (2010) and the earlier supporting literature, by employing basic visual and exploratory techniques, in a similar way to Reinhart et al. (2012), allowing us to highlight channels and directions of causality that might remain difficult to disentangle in more sophisticated empirical techniques. In doing so, we use an approach that we hope will facilitate broader access to this critical debate beyond the field of economics. Our goal is to develop insights regarding the time-dependent causality pattern of debt and growth, their structural nature, as well as any potential threshold effects of debt by comparing the patterns of the same set of countries and periods at different frequencies. We have chosen to compare the two different data sets to mitigate possible bias in previous studies caused by the loss of high-frequency information and also to see how the marked increase in volatility for the debt-to-GDP ratio since 2009 affects our results for the k-period-ahead average growth rate for various debt thresholds.

1.3.1 Inter-temporal linkage of debt and growth

To evaluate the relationship between public debt and time, we introduce three regimes for debt and growth, which we define based on the findings in the literature: As for sovereign debt, we chose the first regime postulated by Reinhart and Rogoff (2010) with a gross government debt-to-GDP ratio of above 90 per cent and following the empirical literature by defining a middle regime for ratios ranging between 60 and 90 per cent of GDP and a third debt regime

of below 60 per cent. Furthermore, we define three growth regimes associated with GDP growth rates above 2 per cent, between 2 and 0 per cent and below 0 per cent.

In our efforts to analyse the inter-temporal relation between economic growth and public debt, we plot the median annual, one-period-ahead, GDP growth rates for each country of the Reinhart and Rogoff (2010) data set over time. To reduce the noise of the series, we replace the actual series with their respective third-order moving-average representation and associate each regime with the corresponding high-, medium- and low-debt regimes of above 90 per cent, between 60 and 90 per cent and below 60 per cent using the colours red, blue and green, respectively.[4]

The findings in Reinhart and Rogoff (2010) imply that countries with higher public debt levels are associated with much lower growth rates. Thus, with the data presented in this way, we would expect to see a 'sandwich-like' pattern in Figure 1.1a where the red line would be present at lower GDP growth rates, followed by blue and eventually green, both of which associated with more favourable debt regimes. Instead, we observe that gross government debt levels in excess of 90 per cent are more apparent at the sample's beginning and the very end (see Figure 1.1b). Between 1960 and 1980, most countries are associated with a low-debt regime. Additionally, we see that the general pattern of GDP growth rates is downward-sloping and seems less volatile across the sample of countries since the 1980s.

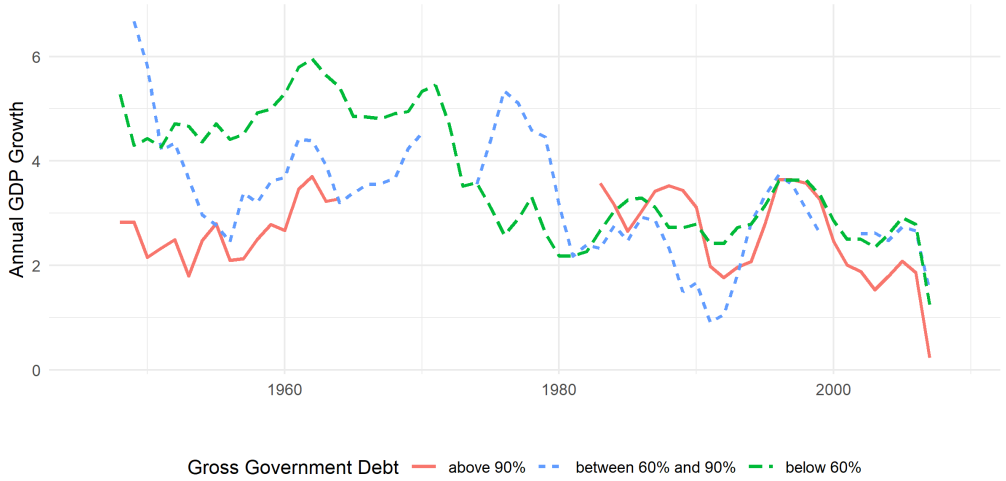
For the period starting in 1980, there seems to be very little difference in the growth performances of countries across all three regimes. Furthermore, we can see the devastating effect of the financial crisis on GDP growth rates at the very end of the sample, represented by a universal and steep drop across all observed countries. Figure 1.1b allows for some additional insights regarding the volatility of economic growth across the three regimes, illustrated by adding the actual median growth rates for each year and three debt regimes, respectively. This helps explain the strong variability pattern across all regimes, with no distinct regime-specific patterns.

Equipped with these insights, we next apply the same visual tools to the quarterly OECD data. We do this as we are interested to see if the previously observed patterns also hold for the more recent past, as well as on a higher-

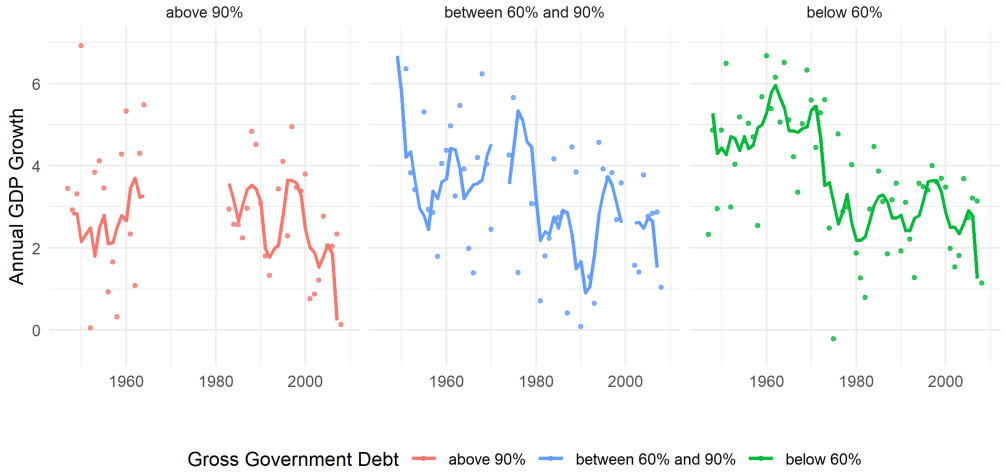
[4]We present a disaggregated, individual-country-type version of this graph in Figure 1.7 for further reference.

Figure 1.1: GDP growth, Reinhart and Rogoff (2010) data, 1946–2009

(a) Smoothed median GDP growth rates over time



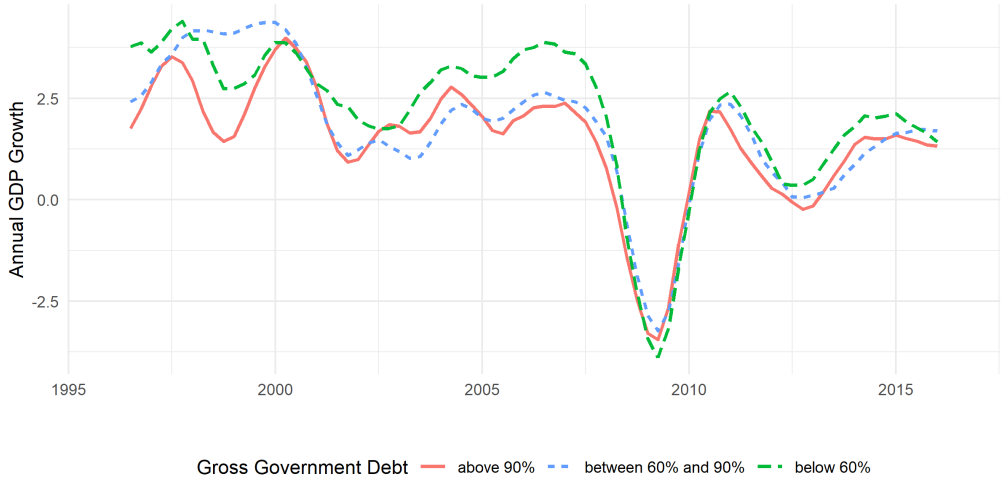
(b) Smoothed median GDP growth rates and scatters per debt regime over time



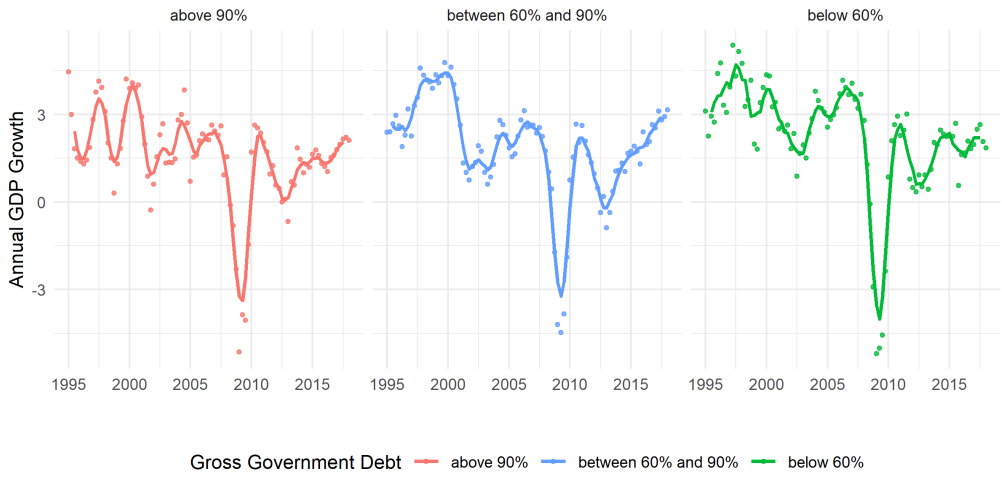
Source: Authors' calculations based on Reinhart and Rogoff (2010), see Appendix 1.A.

Figure 1.2: GDP growth, quarterly OECD data, 1996Q01 to 2018Q2

(a) Smoothed median GDP growth rates over time



(b) Smoothed median GDP growth rates and scatters per debt regime over time



Source: Authors' calculations based on quarterly OECD data, see Appendix 1.A.

frequency level and, in particular, how the financial crisis and the post-crisis era have left their marks on the data.[5] Interestingly, in Figure 1.2a, we again fail to recognise the previously hypothesised sandwich-like layering of growth rates but find that most countries recorded moderate growth rates and appear to move together. What is striking is how similar the evolution of the median growth rates across all three debt regimes plays out. This common trend also continued during the crisis: Independent of the level of debt, all countries were similarly struck by this seismic economic event with very little volatility in the variation of growth rates across debt regimes (Figure 1.2b). There also seems to be little visual evidence to suggest that the absorption of the financial crisis reflected by the decline in the growth rates seems to be associated with the debt regime of the corresponding countries in any way: As a matter of fact, we observe the highest drops in GDP growth rates during the financial crisis for two nations associated with the low-debt regime, Finland and Ireland, a slump which Greece only surpassed in 2011.

We see this as clear empirical evidence against the idea that countries with higher debt levels have been struck more severely by the crisis or have experienced a more sluggish recovery after the crisis. Quite to the contrary, it seems suggestive that the crisis has had a 'unifying' effect on the growth dynamics across regimes. While the above analysis provides some critical insight into the relationship between economic growth and public debt over the last decades and through the financial crisis, in particular, it does not provide sufficient evidence to make a statement concerning the inter-temporal direction of causality on a more general level.

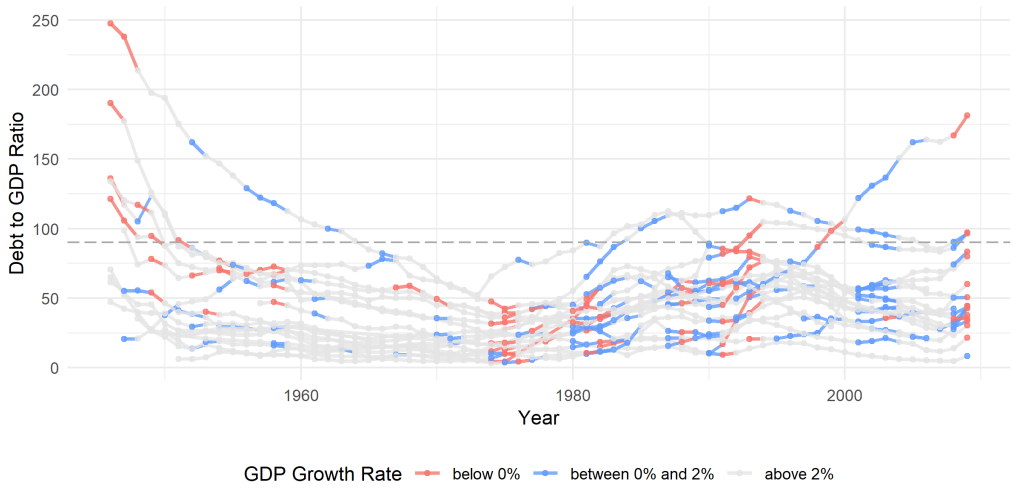
We try to address this issue in Figure 1.3 where we again use Reinhart and Rogoff (2010) annual data set and plot the levels of the debt-to-GDP ratio against time.

As we are particularly interested in learning about the timing of the changes in the debt structures associated with the lower-growth regimes, we fade out the high-growth regime of above 2 per cent by only depicting it in a light grey.[6] Our rationale is the following: If high public debt is causing low economic growth, we would assume to see more medium- and low-growth regimes associated

[5] We present a disaggregated, individual-country-type version of this graph in Figure 1.9 for further reference.

[6] We keep the previously defined colours for the medium and low regimes. For the country-level figures in the appendix, we resort to the previously defined colour pattern for both the Reinhart and Rogoff (2010) data in Figure 1.8 as well as the quarterly OECD data in Figure 1.10.

Figure 1.3: Debt-to-GDP ratio, Reinhart and Rogoff (2010) data, 1946–2009



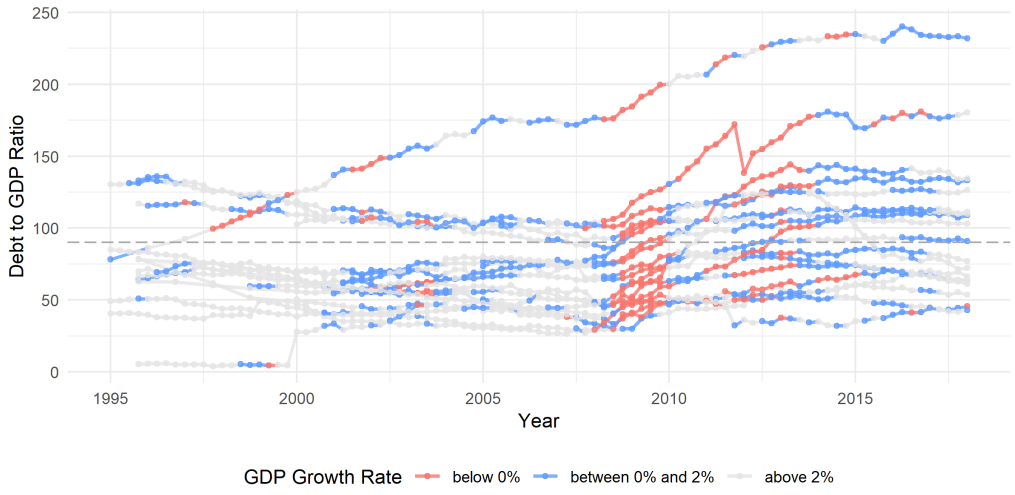
Source: Authors' calculations based on Reinhart and Rogoff (2010), see Appendix 1.A.

with countries with higher debt levels. Furthermore, if the negative link from high debt to low growth holds universally true, we would expect their growth regime to change from grey to blue for lower levels of debt and consequently to red at (or around) the debt overhang regime of 90

However, the empirical evidence presented in Figure 1.3 draws a different picture: Rather than being typically present for extremely high levels of the sovereign debt ratio, periods of low economic growth seem to occur at similar periods across countries (and have become more frequent in the 1980s to mid-1990s). In other words, visual evidence is more suggestive of economic growth being affected by exogenous factors other than the actual level of the debt-to-GDP ratio at the time of the crisis. Further inspection of the graphs shows that build-ups typically follow periods of low economic growth in public debt. This can be seen when noting that almost all more accentuated line segments associated with lower-growth regimes are upward-sloping. This pattern is particularly pronounced in the 1980s, suggesting a reverse causality link between economic growth on public debt instead.

We believe that Figure 1.3 provides a strong counterargument against the 'debt-threshold' hypothesis represented by the vertical, dashed line at 90 per cent of the debt-to-GDP ratio. Given the evolution of debt levels over time and their corresponding growth regimes, this figure leaves little to no room for the credible argument that, at least for this sample of countries, there could be a

Figure 1.4: Debt-to-GDP ratio, quarterly OECD data, 1996Q01 to 2018Q2



Source: Authors' calculations based on quarterly OECD data, see Appendix 1.A.

tipping point at 90 per cent which would demolish economic growth. Finally, we again turn to the quarterly OECD data in Figure 1.4. We observe that the debt levels before the financial crisis were stable, and most countries reported growth rates of two or more per cent, similar to previous observations. Regarding the timing of movements, the financial crisis led to a significant drop in economic activity resulting in a negative growth rate for almost all countries in the sample, followed by a considerable increase in sovereign debt levels.

These results, together with the historical evidence on how economies reacted to the unfolding of the financial crisis, throw doubt on any hypothesis suggesting the debt build-ups were caused by anything else but the financial crisis. Furthermore, even though debt levels rose for almost all countries in the sample, GDP growth rates turned positive around the beginning of 2010, which can be verified in Figure 1.10. For example, Italy, with a very high debt ratio before the financial crisis, also had growth rates returned to normal in 2010. After the second setback in late 2011, GDP growth returned to levels between 0 per cent and 2 per cent in 2013—at a debt ratio of 130Similar trends of non-negative growth regimes far above the 90 per cent debt threshold can be observed in a wide range of countries, including France, Spain, the UK and the USA.

We summarise the following points. Employing a straightforward inspection of two separate data sets, we present evidence that low-growth periods happen around the same time for our sample of countries. This suggests that

common external economic shocks might be partially responsible for this pattern. Furthermore, the plots indicate that the actual levels of the debt-to-GDP ratio are improbable to be associated with a particular growth regime. A critical insight of this analysis is that if, as argued by Reinhart and Rogoff (2010), high public debt is detrimental to economic growth, high debt levels should generally be associated with more and longer spells of low growth rates. Instead, we see that growth slumps typically appear to happen before an increase in the debt level; but it does not seem to depend on the initial level of the debt share. Consequently, we believe that the notion of a significant and homogeneous debt threshold at 90 per cent lacks any factual or empirical foundation.

1.3.2 Non-linearities and threshold effects

We acknowledge the argument that even if previous debt levels might not have been detrimental in causing low economic growth, as suggested in our analysis, this does not mean that sovereign debt-induced economic slowdowns are an impossibility. In other words, even though our analysis has shed some light on the question of potential causality linkages and highlights the role of global economic factors and dynamics, we cannot argue that there might not be a debt threshold level at which economic growth is impeded. Even if this threshold may not be at 90 per cent for the annual data set, we might find a threshold effect at other levels. Conversely, as far as the more recent quarterly data are concerned, numerous countries have reported ever-increasing debt levels due to the financial crisis.

From the empirical tools, it is unclear how these countries experienced growth rates significantly lower than their counterparts, below the 90 per cent cut-off. With this issue in mind, we analyse the evolution of average, short- to long-run growth rates for various debt levels. In doing so, we explicitly consider the growth performances of countries that either surpass a particular debt threshold level τ from below and remain above the threshold for the entire consecutive period; and countries that report debt levels above τ in both periods. We do this because we are interested in the distribution of contemporary and mid-run growth performances conditioned on varying debt threshold levels. In more mathematical terms, we define the k-period-ahead average growth rate for each debt threshold level τ as:

$$\bar{g}_{k,\tau} = \frac{1}{T_\tau N_\tau} \left(\sum_{l=1}^{T_\tau} \sum_{i=1}^{N_\tau} g_{kit} \times \mathbf{1}(D_{it}, \tau) \right),$$

where the k-period-ahead average growth rate for an arbitrary country i and time period t is calculated as:

$$g_{kit} - \left(\left(\frac{GDP_{i,t+k}}{GDP_{i,t}} \right)^{m/k} - 1 \right),$$

for annual ($m = 1$) and quarterly ($m = 4$) data, respectively, and:

$$\mathbf{1}(D_{it}, \tau) = \begin{cases} 1 & \text{if } D_{it} \geq \tau; \\ 0 & \text{otherwise.} \end{cases}$$

Furthermore, we define:

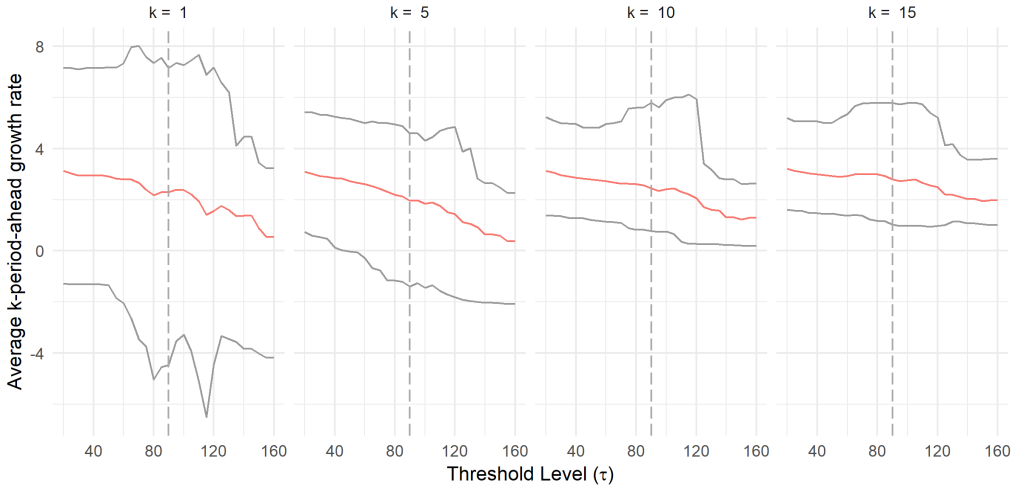
$$T_\tau = \sum_{t=1}^{T_\tau} \mathbf{1}(D_{it}, \tau),$$

and equivalently for N_τ.[7] We assess the relationship between GDP growth and potential debt thresholds by calculating for $\tau = \{20, 25, ..., 155, 160\}$ and $k = \{1, 5, 10, 15\}$ for the annual as well as $k = \{4, 20, 40\}$ for the quarterly data set. This allows us to compare the one-, five- and 10-year-ahead GDP growth rates from both data sets of our interest. We present the results in figures 1.5 and 1.6 where we plot the average growth rates g, k, τ and the corresponding 5^{th} and 95^{th} percentile against the possible levels of τ, using the annual as well as quarterly data sets respectively.

Generally speaking, our results align with comparable approaches undertaken in the literature (e.g. Pescatori et al., 2014. More precisely, in Figure 1.5, we observe that for the annual data, the one-period-ahead growth rate presented in the outer left panel can be associated with a relatively strong negative trend for GDP growth rates. Similar to other exploratory pieces exploiting the corrected Reinhart and Rogoff (2010) database, we observe that one-period-ahead GDP growth rates at a threshold level of $\tau = 90$ (illustrated by a dashed

[7] It would be interesting to compare the debt trajectories of countries surpassing the debt threshold from below with the average growth rate and its deviation for the group of countries that surpass the same debt threshold from above or to introduce an additional requirement that any country would have to stay above (below) the various threshold values for a pre-specified period to eliminate short-term disturbances. As it stands, this is left for further research.

Figure 1.5: Average k-period-ahead GDP growth rates vs. debt threshold levels τ, Reinhart and Rogoff (2010) data, 1946–2009

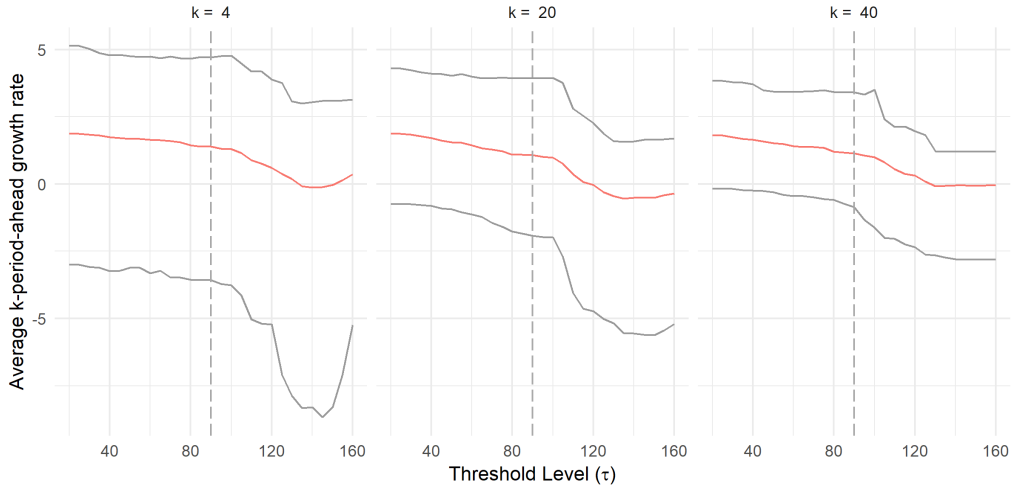


Source: Authors' calculations based on Reinhart and Rogoff (2010), see Appendix 1.A.

vertical line) are associated with a mean GDP growth rate of somewhat above 2 per cent. We also observe a very high degree of heterogeneity: The grey, vertical lines associated with each plot show the variation of the 5^{th} to 95^{th} percentile of the data. Observing the evolution of average growth rates over increasing values of τ in the outer left panel, we also note that there seems to be little to no evidence of a debt threshold compatible with the 90 per cent hypothesis. However, we observe a notable yet steady drop in average one-period-ahead growth rates for a debt level of 20 per cent (somewhat above 3 per cent) versus a debt level of 160 per cent (0.5 per cent).

As noted earlier, one should be cautious when deducing a causal relationship between high debt adversely affecting GDP growth because of (possible) endogeneity feedback mechanisms. Consequently, and similar to the empirical literature discussed in the first part of this piece, we try to mitigate this problem by extending the time horizon of the analysis. This approach also lets us smooth out business-cycle effects and outliers to analyse the five-, 10- and 15-year-ahead GDP growth rate in panels 2 to 4 in Figure 1.5. Comparing these findings with the simple one-period-ahead growth rates, three observations seem particularly noteworthy: First, we see noticeably less variation in the data when increasing the period considered. This result seems to go hand-in-hand with a less pronounced reduction of average GDP growth rates with rising lev-

Figure 1.6: Average k-period-ahead GDP Growth rates vs. debt threshold levels τ, quarterly OECD data, 1996Q01 to 2018Q2



Source: Authors' calculations based on quarterly OECD data, see Appendix 1.A.

els of τ; that is, the observed reduction in average growth rates associated with increased sovereign debt ratios decreases over the mid-term. Interestingly, this rotation of the average growth rate line seems to result from fewer observations with high, negative growth rates. This result is intuitive and in line with our previous observations: Crises can typically be characterised by a sharp drop in average growth rates and seem to lead to a debt build-up afterwards. However, when considering more extended periods, these dramatic events are typically smoothed out; hence, the hypothesised causal relationship from debt to growth would be expected to wash out too. This is precisely what we observe in Figure 1.5 when comparing, in particular, the 5^{th} percentile across all four panels. Most notably, for the case of $k = 15$, we see that 95^{th} of all recorded cases show an average growth rate of approximately 1.5 per cent for a debt level of $\tau = 20$ compared to approximately 1 per cent for $\tau = 160$.

Finally, we apply the same approach to our quarterly data set from the OECD. For this case, and because of data availability issues, we limit our analysis to only the one-, five- and 10-year-ahead growth rates. From Figure 1.6, we can see some consistency with our results using the corrected Reinhart and Rogoff (2010) data set: Extending the time horizon smooths out adverse effects and reduces variation, also that mid-run growth rates seem to be consistently above the 1 per cent mark for 95 per cent of the considered sample and debt

levels up to $\tau = 160$. In contrast to our earlier results, we can see more clearly the increased uncertainty around k-period-ahead growth rates caused by the quarterly fluctuations and the increase in volatility in growth rates from 2009 onward. Nevertheless, growth figures are nowhere near the negative aggregates reported in Reinhart and Rogoff (2010) for the 90 per cent threshold, of which there appears to be very little evidence.

1.4 Conclusion

Since the financial crisis, the continuing debate over the imposition of measures to deal with sovereign indebtedness has become centred on the seminal contribution by Reinhart and Rogoff (2010). With the wide adoption of fiscal discipline policies, also known as austerity, the importance of this perspective for economic policy should not be overstated. Since the publication of various inaccuracies in methodology pointed out by Herndon et al. (2014) among others, the argument has continued unabated. Our straightforward contribution to this topic critically assesses the results in Reinhart and Rogoff (2010) and the empirical strands of literature that have evolved from the 'debt-threshold' view.

The discussion of the empirical literature reveals mixed to little evidence of any negative causal effect of high sovereign debt upon economic growth, and that cross-country heterogeneity seems to play a crucial role in the analysis of the relationship.

In the second part of our study, we have offered a time series perspective by contrasting the widely used Reinhart and Rogoff (2010) data with a second, more recent, and higher-frequency OECD data source. Using the second data source allows a robustness exercise in our analysis and offers a more sophisticated view of the direction of this contentious relationship. Furthermore, we enjoy a sample that encompasses a full decade after the financial crisis and one that controls for changes in the dynamics between public debt and economic growth since.

When analysing the inter-temporal linkage between growth and debt, our time series analysis provides crucial evidence on the much-debated possibility that growth can be endogenous on the level of external public debt. Whereas some previous studies have assumed that this causality is one way, our study of the data lends itself to the 'reverse' causality hypothesis. We build this argument, firstly, on the observations that the times series plots for GDP growth over

the last six decades reveal a downward trend over the sample. This could be the explanation for any negative correlation between debt and growth. Secondly, after the financial crisis, it is evident that the number of higher debt regimes has exploded, offering support for the reverse causality of debt caused by economic slumps. Thirdly, it seems that periods of low growth tend to be clustered around common periods for many countries and not bound to country samples displaying exceptionally high levels of debt, thereby offering further support for reverse causality.

In addressing the question of the existence of a debt threshold, we find no evidence that countries will experience significant reductions in GDP growth after surpassing a certain percentage of the debt-to-GDP ratio. We draw this conclusion from observing the evolution of average growth rates (and their variation) throughout very low to very high aggregate debt levels. We demonstrate that the average negative relationship between economic growth and the debt-to-GDP ratio depends on the time dimension of consideration. Accounting for business-cycle effects and short-term volatility explains much of the negative variation in growth figures. Our observations add to the evidence for the case of 'reverse causality'. They suggest that a much more rigorous analysis of the effects of higher public debt is required, especially where the policy impact of a study is so high.

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Appendix Chapter 1

1.A Data

For annual data, we use the corrected Reinhart and Rogoff (2010) data set by Herndon et al. (2014) [8] and follow the authors' sampling choices to obtain one-period-ahead GDP growth rates for a sample of 20 OECD countries for the period 1946–2009. We composed our quarterly data from the following two sources of OECD.StatExtract:

- We use the OECD Quarterly National Accounts data set (http://stats.oecd.org/Index.aspx?DataSetCode=QNA, last visit September 2018) to obtain 'US dollars, volume estimates, fixed PPPs, OECD reference year, annual levels, seasonally adjusted' (VPVOBARSA) in order to calculate GDP growth rates.
- For the gross government debt ratio, we use 'general government total gross debt, percentage of GDP at current prices and national currencies' (PCTGDPA) taken from OECD's Quarterly Public Sector Debt (http://stats.oecd.org/Index.aspx?DataSetCode=QASA_TABLE7PSD, last visit September 2018).

We restrict our analysis to the same group of countries which results in a sample of 19 countries, given that there is no quarterly OECD debt data available for New Zealand. Please see Table 1.1 for a summary statistic of both data sets.

1.B Endogeneity

For the discrete time Solow model consider the production function $Y_t = K_t(A_tN_t)^{1-\alpha}$ with $K_{t+1} = I_t + (1-\delta)K_t$, $S_t = sY_t$, $I_t = S_t$ and $X_{t+1} = (1+g_X)X_t$ for $X = \{Y, K, N, A\}$. In intensive form $x_t = Z_t/(A_tN_t)$ for $Z = \{Y, K, S, I\}$ it follows that the dynamics of the system between periods t and $(t+1)$ as:

[8] Publicly accessible at https://www.peri.umass.edu/publication/item/526does-high-public-debt-consistently-stifle-economic-growth-a-critique-of-reinhartand-rogo-ff, last visit February 2017).

Table 1.1: Summary statistics

	Period		GDP growth rate		Debt-to-GDP ratio	
	RR	OECD	RR	OECD	RR	OECD
Min.	1946	1995	-10.94	-10.28	3.28	4.02
1st quartile			1.93	0.87	21.74	53.51
Median			3.31	2.09	39.84	71.95
Mean			3.43	1.92	45.39	82.23
3rd quartile			5.10	3.31	60.24	106.03
Max	2009	2018	27.33	29.12	247.48	240.15

Note: Corrected Reinhart and Rogoff (2010) data (referred to as RR) obtained by following Herndon et al. (2014) with $\tau = 90, k = 1$ left, quarterly OECD data (referred to as OECD) with $\tau = 90, k = 4$ right. Country (obs.) Reinhart and Rogoff (2010) data: Australia (64), Austria (59), Belgium (63), Canada (64), Denmark (56), Finland (64), France (54), Germany (59), Greece (21), Ireland (63), Italy (59), Japan (54), Netherlands (53), New Zealand (61), Norway (64), Portugal (58), Spain (42), Sweden (64), UK (63), USA (64). Country (obs.) quarterly OECD data: Australia (93), Austria (73), Belgium (90), Canada (93), Denmark (73), Finland (73), France (70), Germany (78), Greece (73), Ireland (77), Italy (90), Japan (85), Netherlands (78), Norway (90), Portugal (78), Spain (90), Sweden (90), UK (93), USA (93).

$$
\begin{aligned}
y_t &= k_t^{\alpha} \\
(1+g_y) &= \left(\frac{k_{t+1}}{k_t}\right)^{\alpha} \\
&= \left(\frac{K_{t+1}}{A_{t+1}N_{t+1}} \times \frac{A_t N_t}{K_t}\right)^{\alpha} \\
&= \left(\frac{K_{t+1}}{K_t} \times \frac{A_t}{A_{t+1}} \times \frac{N_t}{N_{t+1}}\right)^{\alpha} \\
&= \left(\frac{(I_t + (1-\delta)K_t)}{K_t} \times \frac{1}{(1+g_A)} \times \frac{1}{(1+g_N)}\right)^{\alpha} \\
&= \left(\frac{(sY_t + (1-\delta)K_t)}{K_t} \times \frac{1}{(1+g_A)} \times \frac{1}{(1+g_N)}\right)^{\alpha}.
\end{aligned}
$$

Substituting for K_t an the fact that $log(1+g_X) \approx g_X$ we arrive at a growth regression like representation:

$$
g_y = \beta_1 log(\tilde{y}) + \boldsymbol{X'\beta} + \epsilon_t, \tag{1.B.1}
$$

where $\beta = (\alpha - 1)$, $\tilde{y}$ denotes the (transformed) level of GDP, ϵ_t a stochastic term capturing all omitted influences and $\boldsymbol{X}$ a series of controls (including g_A, g_N from the derivation above). The representation in Equation 1.B.1 is similar to

those estimated by, among others, Barro and Sala-i Martin (1992), Sala-i Martin (1997) where **X** contains country-specific characteristics such as, among others male/female schooling rates, fertility rate, the rate of investment, government-consumption ratio, institutional variables and potentially also public debt ratios.

Conversely, we again follow the standard textbook discussion for debt dynamics. We build on the argument developed in Domar (1944) who analyses the economic effect of sovereign debt. Assuming a balanced budget, the debt-to-GDP ratio in period t, D_t , is can be written as

$$\frac{Debt_t}{GDP_t} := D_t = (1 + r_t) \times \frac{Debt_{t-1}}{GDP_{t-1}} \times \frac{GDP_{t-1}}{GDP_t}.$$

Using the fact that

$$\frac{GDP_t}{GDP_{t-1}} = (1 + GR_t),$$

the above relationship can be expressed as a log-linear approximation:

$$log(D_t) = \delta \boldsymbol{X} + \theta GR_t + v_t,$$

where we collect the lagged debt ratio and the interest rate in $\boldsymbol{X}$. Using Equation 1.2.3, the OLS coefficient for D_t is given by :

$$\hat{\beta} \longrightarrow \frac{Cov(GR_t, D_t)}{Var(D_t)},$$

with $E[u_t] = E[v_t] = E[u_t, v_t] = 0$, $E[u_t^2] = \sigma_u^2$, $E[v_t^2] = \sigma_v^2$ for all t, then $\hat{\beta}$ is unbiased only if $\theta = 0$, as:

$$\begin{aligned} Cov(GR_t, D_t) &= E([GR_t - E[GR_t]] \times [D_t - E[D_t]]) \\ &= E[[\beta v_t + u_t] \times [\theta u_t + v_t]] \\ &= E[\beta v_t^2 + \theta u_t^2 + \gamma v_t \theta u_t + u_t v_t] \\ &= \beta \sigma_v^2 + \theta \sigma_u^2 \end{aligned},$$

and

$$\begin{aligned} Var(D_t) &= E\left([D_t - E[D_t]]^2\right) \\ &= E\left([\theta u_t + v_t]^2\right) \\ &= \theta^2\sigma_u^2 + \sigma_v^2. \end{aligned}$$

1.C Additional figures

Figure 1.7: Individual country plots of GDP growth, Reinhart and Rogoff (2010) data, 1946–2009

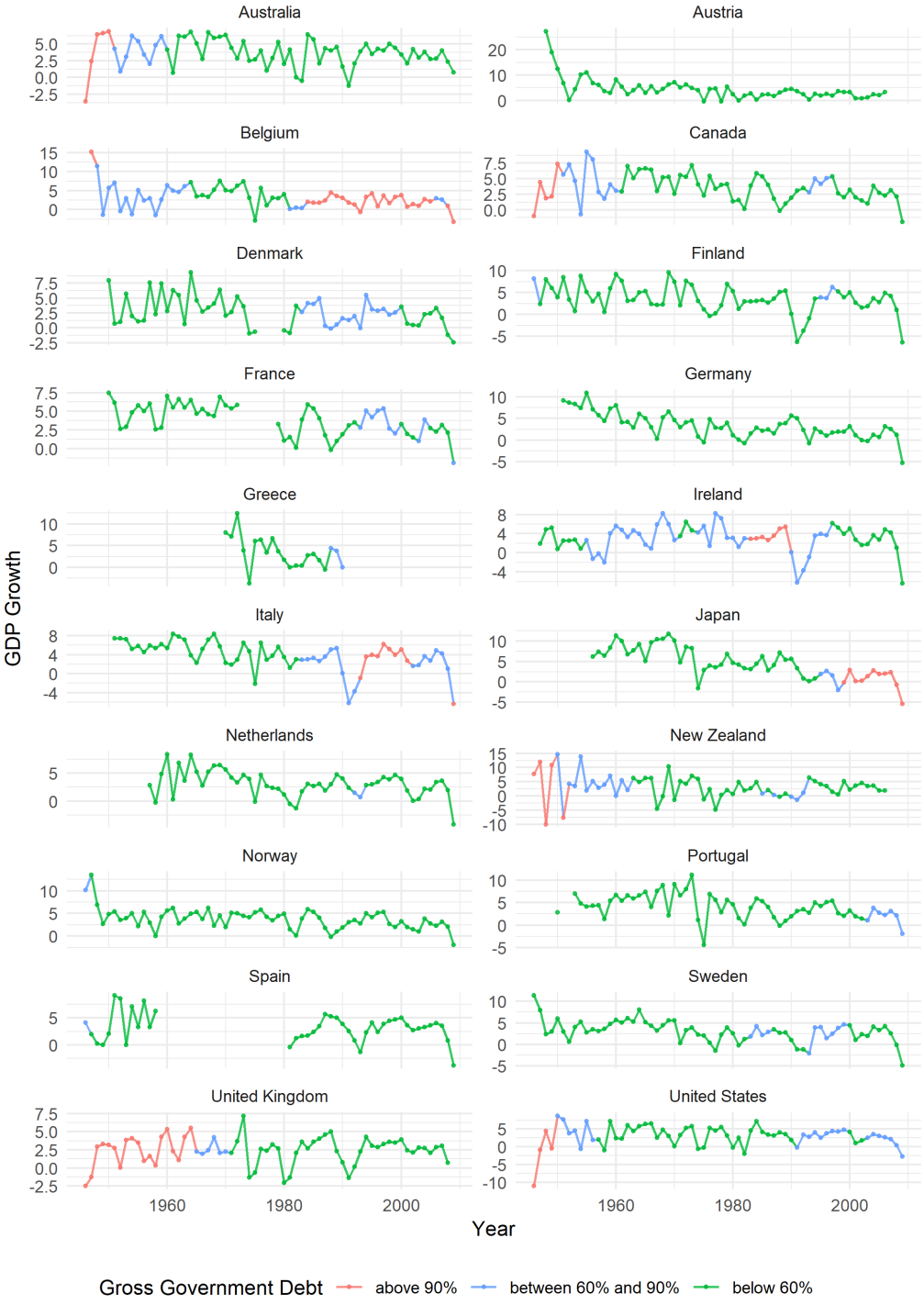


Figure 1.8: Individual country plots of debt-to-GDP ratio, Reinhart and Rogoff (2010)data, 1946–2009

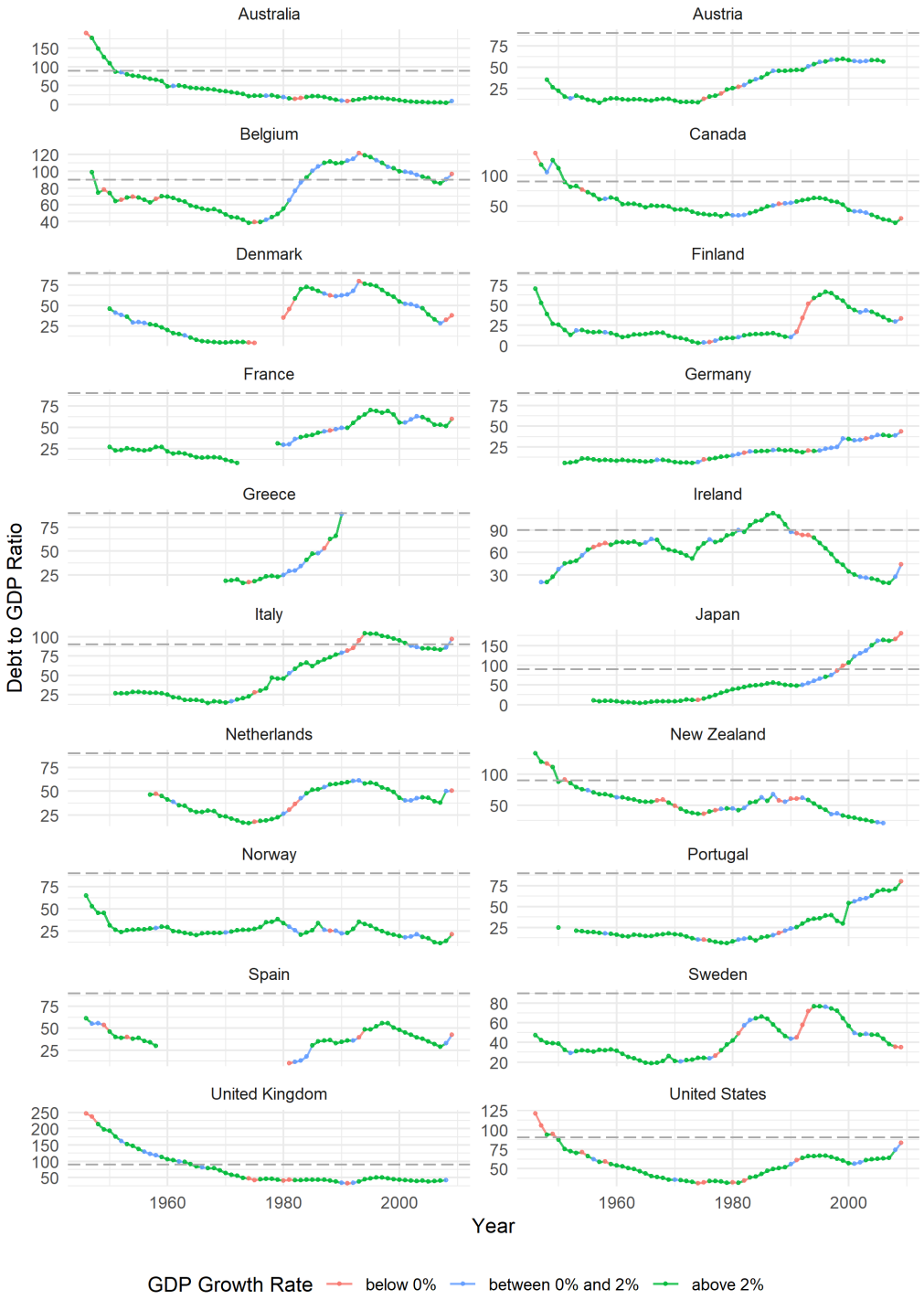


Figure 1.9: Individual country plots of GDP growth, quarterly OECD data, 1996Q01 to 2018Q2

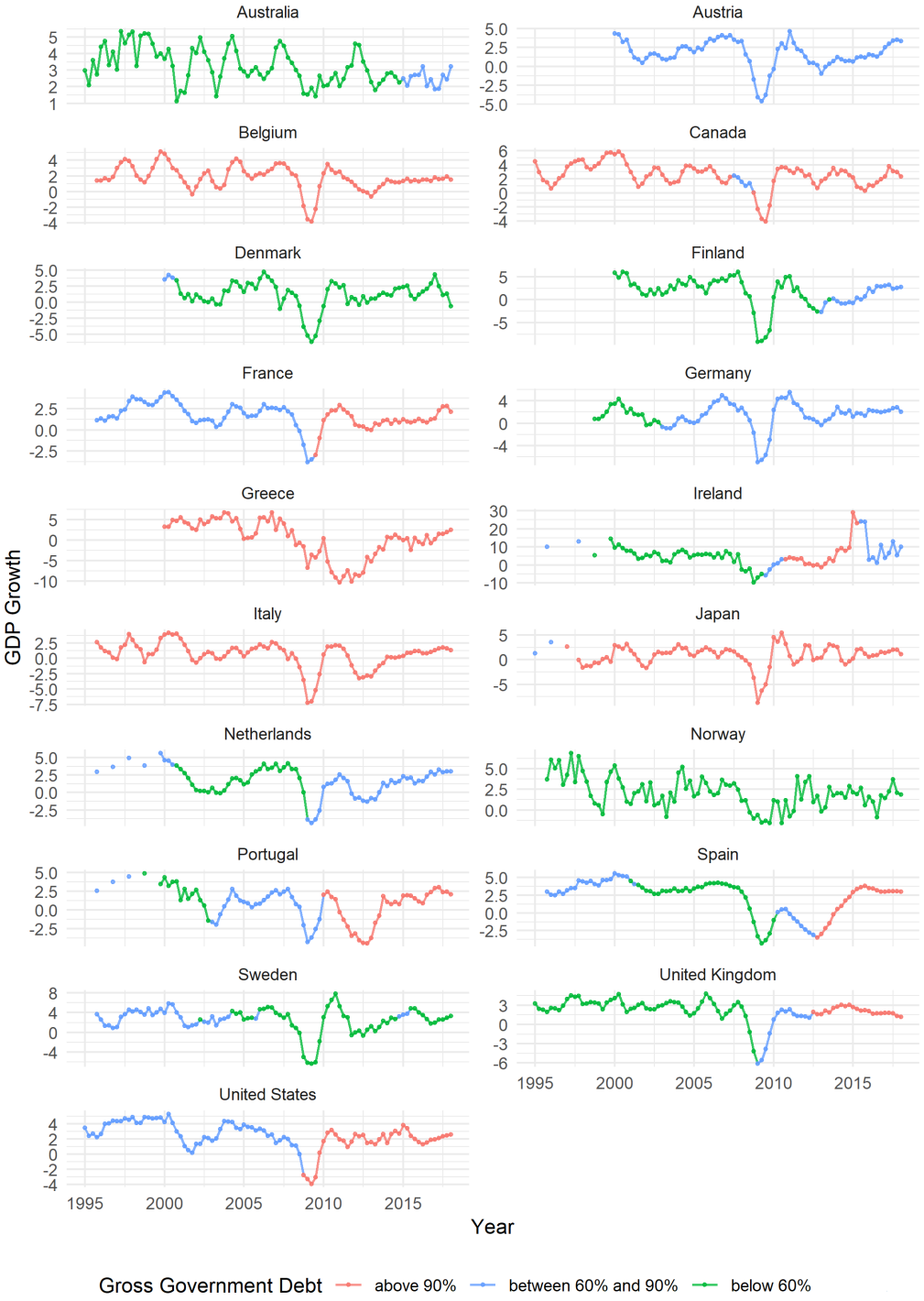
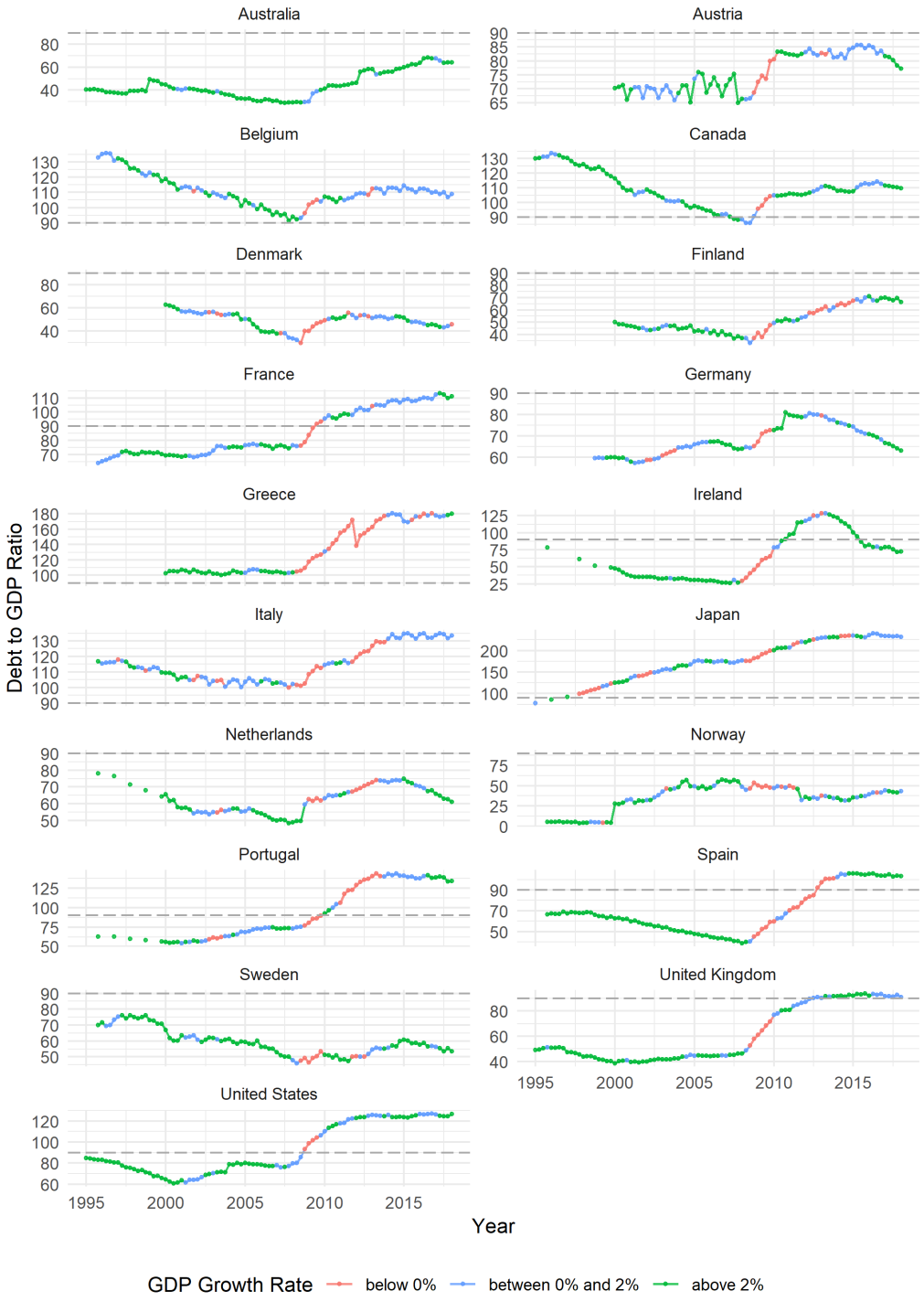


Figure 1.10: Individual country plots of debt-to-GDP ratio, quarterly OECD data, 1996Q01 to 2018Q2



CHAPTER

TWO

TESTING THE DEBT THRESHOLD HYPOTHESIS: A STRUCTURAL BREAK THRESHOLD GRANGER CAUSALITY TEST UNDER CROSS-SECTIONAL DEPENDENCE

Abstract This paper proposes a new method to test for regime-dependent Granger causality in a cross-sectionally dependent panel data setting using vector autoregressive (VAR) models and seemingly unrelated regressions (SUR). Estimation of regime-dependent model parameters is based on bootstrapped Wald test statistics. Extensive Monte Carlo simulations demonstrate that the proposed tool has the desirable small sample properties and outperforms conventional approaches in most evaluated scenarios. We also apply our testing framework to revisit the relationship between GDP growth and sovereign debt. Our results fail to find support for the debt threshold hypothesis (Reinhart and Rogoff, 2010) when imposing an exogenously introduced threshold of 90 per cent of the debt-to-GDP ratio. However, our analysis provides evidence for debt threshold effects when allowing for endogenous regime detection, which is spread over a wide range (40 per cent to 100 per cent of the debt-to-GDP ratio).

Key words: Bootstrapping, cross-sectional dependence, Granger causality, growth, non-linear VAR, public debt
JEL classifications: C31, C32,C33, C59, H60

2.1 Introduction

With the emergence of panel data featuring more extensive time coverage and broader cross-sectional dimension, the necessity of estimation techniques to accommodate challenges such as dynamics of the form of lagged dependent variables, cross-sectional dependence or parameter heterogeneity has arisen.

This paper proposes a new method to endogenously test for threshold effects and structural instabilities (breaks) in a vector autoregressive (VAR) model. Our method identifies (changes in) the Granger causal relationship in the presence of cross-sectional error correlation employing a seemingly unrelated regressions (SUR). In an extensive Monte Carlo simulation, we demonstrate that the proposed framework performs well compared to standard estimators and various forms of cross-sectional dependence. The proposed procedure is shown to outperform conventional tools in most evaluated scenarios.

We then apply this testing framework to the debt-growth nexus. We find strong evidence of a negative Granger causal relationship of (low) growth to (high) debt (and vice versa) but fail to identify a 'reverse causal' connection from debt to growth. We also fail to find empirical evidence supporting the *debt threshold hypothesis* (Reinhart and Rogoff, 2010) when imposing an exogenously introduced threshold of 90 per cent of the debt-to-GDP ratio. At the same time, the analysis does provide evidence for debt threshold effects when allowing for endogenous regime detection, which, however, are spread over a wide range (between 40 per cent and 100 per cent of the debt-to-GDP ratio). Therefore, the empirical analysis of this piece suggests that the relationship between sovereign debt and GDP growth is more heterogeneous than often assumed in the literature and that heterogeneous, country-level non-linearities do not seem to be the exception but rather the norm. Based on the proposed method and set of countries analysed in this piece, we conclude that generalising assumptions on a homogeneous debt tipping point should be treated with caution.

The remainder of this paper is structured as follows. In section 2.2, we provide a brief survey of the field, focusing on the empirical literature on the debt-growth nexus. In section 2.3, we introduce the testing procedure for regime-dependent Granger causality under cross-sectional dependence before turning the empirical application in the context of the debt-growth nexus in section 2.4. We offer a conclusion in section 2.5.

2.2 Literature

Recent advancements in data availability exacerbate the traditional limitations of empirical macroeconomic research. For example, the problem of cross-section dependence has largely been addressed in the panel data literature without considering lagged dependent variables; see Bai (2009); Moon and Weidner (2010); Pesaran (2004). Most recent advances do allow for weakly exogenous regressors and lagged dependent variables (Chudik and Pesaran, 2015; Song, 2013) and have been extended to the case of the autoregressive distributed lag (ARDL) model (Chudik et al., 2016).

Chudik et al. (2017) proposed an ARDL specification to endogenously identify threshold effects in the presence of unobserved common factors. As one of the few papers addressing methodological questions in the context of the debt-growth nexus, the authors provide a test for threshold effects in a dynamic, heterogeneous panel with cross-sectionally dependent errors to study the relationship between public debt and economic growth. Apart from their methodological contribution, the authors raise doubts about a universal debt threshold effect propagated in earlier works in this field but point out the adverse effects of public debt surges in economic growth.

The method proposed in this paper differs from Chudik et al. (2017) insofar as that it offers a testing apparatus for (time-dependent) instabilities as well as a test to identify (changes in) the Granger causal relationships. Our testing procedure also provides a methodological improvement over earlier contributions that have employed VAR models to test the Granger causal relationship between debt and growth: As discussed more extensively in Amann and Middleditch (2020), the literature either addresses the issue of structural instabilities, cross-sectional correlation or parameter heterogeneity separately, but never jointly.

For example, addressing the issue of cross-sectional dependence, Puente-Ajovín and Sanso-Navarro (2015) apply a panel bootstrap Granger causality test developed by Kónya (2006). The authors fail to report evidence that government debt Granger causes real GDP growth but find evidence that economic growth negatively affects gross debt. However, their procedure does not allow for parameter heterogeneity of parameter instabilities.

Gómez-Puig and Sosvilla-Rivero (2015) test for bidirectional Granger causality analysis for central and peripheral countries of the European Economic and Monetary Union between 1980 and 2013. Allowing for endogenously detected

breakpoints in a single-equation setup, the authors report evidence of an adverse effect of debt on economic growth for some EMU countries and a significant impact for some other members. Yet, the issue of cross-sectional spillovers remains unaddressed in their framework. In a cross-country study for 20 OECD countries between 1988 and 2001, Afonso and Hauptmeier (2009) report a significant, bivariate Granger causality relation between the debt-to-GDP ratio and GDP growth and provide further evidence of the possibility of country-specific heterogeneity (Afonso and Hauptmeier, 2009, Appendix 1).

Parameter heterogeneity and cross-sectional dependence may plague the identification of the underlying data generating process (DGP) in a VAR setting. Lutkepohl (1982a) illustrates that causal structures of sub-processes of multivariate stochastic processes do not allow one to draw conclusions on the causal structure of the higher dimensional process. More precisely, Baek and Brock (1992) illustrates that such parametric and linear Granger causality tests have low power against certain non-linear alternatives. While the literature addresses the issue of (endogenously) accounting for non-linearities and threshold effects (Galvão, 2006) as well as time-varying coefficients (Christopoulos and Leon-Ledesma, 2008), to the knowledge of the authors, none of these techniques have been (i) expanded to address the issue of cross-sectional dependence or (ii) applied to the debt–growth nexus yet.

2.3 Regime-dependent Granger causality in a partially heterogeneous panel and cross-sectional dependence

In this section, we introduce our proposed testing framework. We do so by first providing a formal definition of Granger causality in a VAR setup before addressing the issue of cross-sectional correlation, and regime-dependent VARs following Galvão (2006)'s structural break threshold VARs (SBTVAR). We then combine the SURE and SBTVAR to introduce our bootstrap test for potentially non-linear Granger causality under cross-sectional dependence. We follow this up with a Monte Carlo study to illustrate the adequacy of the proposed methods. Finally, this section briefly introduces the `PSBTGC` package (Amann, 2022), which

makes the proposed procedure available in the R[1] programming language.

2.3.1 Definition of Granger causality in a VAR setup

Let $\mathbf{z}_t = (z_1, ..., z_T)$ denote a time series where each z_t is a M-dimensional real value vector for $t = 1, ...T$ and $i = 1, ..., N$ cross-sectional units. Without loss of generality we set $M = 2$ and define $z_{i,t} = [y_{i,t}, x_{i,t}]'$. In other words, $z_{i,t}$ contains all observations of both variable vectors of our system for cross-sectional unit i up to period t, respectively. Furthermore, let $u^j_{i,t+1},\ q = \{x, y\}$ denote the one-step ahead forecast error for $y_{i,t+1}$ conditional on $\mathbf{z}_t$ and $y_{i,t}$, respectively, such that:

$$
\begin{aligned}
u^y_{i,t+1} &= y_{i,t+1} - E[y_{i,t+1}|\mathbf{z}_t], \\
\tilde{u}^y_{i,t+1} &= y_{i,t+1} - E[y_{i,t+1}|y_{i,t}],
\end{aligned}
$$

are the one-step ahead error forecasts of $y_{i,t}$ conditional on $\mathbf{z}_t$ and $y_{i,t}$, respectively. In this set-up, the definition of 'Granger causality' (Granger, 1969) states that $x_{i,t}$ is not Granger causal for $y_{i,t}$ if past or current information of $x_{i,t}$ does not improve the mean square forecast error of $y_{i,t}$:

$$
E[(u^y_{i,t+1})^2] = E[(\tilde{u}^y_{i,t+1})^2] \ \ \forall\, t \ \geq 0.
$$

Translating the above definition of causality to the context of a simple autoregressive VAR(p) system of the form:

$$
\begin{bmatrix} y_{i,t} \\ x_{i,t} \end{bmatrix} = \begin{bmatrix} \alpha_i^{(y)} \\ \alpha_i^{(x)} \end{bmatrix} + \sum_{\ell=1}^{p} \begin{bmatrix} \gamma_{i\ell}^{(y)} & \delta_{i\ell}^{(y)} \\ \gamma_{i1\ell}^{(x)} & \delta_{i\ell}^{(x)} \end{bmatrix} \begin{bmatrix} y_{i,t-\ell} \\ x_{i,t-\ell} \end{bmatrix} + \begin{bmatrix} e_{i,t}^{(y)} \\ e_{i,t}^{(x)} \end{bmatrix}, \tag{2.3.1}
$$

the prediction of $y_{i,t+1}$ conditional on $\mathbf{z}_t$ is:

$$
E[y_{i,t+1}|\mathbf{z}_t] = y_{i,t+1} - e_{i,t}^{(y)},
$$

with the necessary and sufficient condition for no Granger causality of $x_{i,t}$ on $y_{i,t}$ in Equation 2.3.1 being that $\delta_\ell^{(y)} = 0$ for all $\ell = 1, ..., p$.

[1] See https://cran.r-project.org/.

2.3.2 Granger cuasality with cross-sectional correlation

As noted before, Equation 2.3.1 follows a simple VAR(p) representation and can be estimated with ordinary least squares (OLS) for each cross-sectional unit $i = 1, j, \ldots, N$ separately. This approach assumes that there is no correlation between the data generating processes behind $y_{i,t}$ and $y_{j,t}$, as long as $i \neq j$.

Relaxing this assumption, a system of seemingly unrelated regressions (SUR) consists of a system of individual relationships that are linked through the observation that disturbances are correlated, i.e. $Cor(e_{i,t}^{(q)}, e_{j,t}^{(q)}) \neq 0$ for $i \neq j,\ q = \{x, y\}$. Such models have been widely used in the field of economics in general (Fiebig, 2001), and in macro-level VAR settings in particular (Kónya, 2006; Puente-Ajovín and Sanso-Navarro, 2015, among others).

More formally, consider Equation 2.3.1 and define $z_{i,t-\ell} = [1, y_{i,t-\ell}, x_{i,t-\ell}]'$ as the vector of exploratory variables and the vector of coefficients $\beta^{(q)} = [\alpha_i, \ldots, \gamma_{i\ell}^{(q)}, \ldots, \delta_{ip}^{(q)}]'$ for cross-sectional unit i, time lag $\ell = 1, \ldots, p$ and $q = \{x, y\}$. By stacking all $i = 1, \ldots, N$ cross-sectional units per equation we rewrite Equation 2.3.1 as:

$$\boldsymbol{y} = \boldsymbol{z}'\beta^{(y)} + \boldsymbol{e}^{(y)} \tag{2.3.2a}$$

$$\boldsymbol{x} = \boldsymbol{z}'\beta^{(x)} + \boldsymbol{e}^{(x)} \tag{2.3.2b}$$

where:

$$\boldsymbol{y} = \begin{bmatrix} \boldsymbol{y}_1 \\ \boldsymbol{y}_2 \\ \vdots \\ \boldsymbol{y}_N \end{bmatrix}_{[NT\times 1]} \qquad \boldsymbol{x} = \begin{bmatrix} \boldsymbol{x}_1 \\ \boldsymbol{x}_2 \\ \vdots \\ \boldsymbol{x}_N \end{bmatrix}_{[NT\times 1]}$$

$$\boldsymbol{z}_t = \begin{bmatrix} z_1 & \mathbf{0} & \ldots & \mathbf{0} \\ \mathbf{0} & z_2 & \ldots & \mathbf{0} \\ \vdots & \vdots & \ddots & \vdots \\ \mathbf{0} & \mathbf{0} & \ldots & z_N \end{bmatrix}_{[N^2T\times MNp)]} \qquad \boldsymbol{e}^{(q)} = \begin{bmatrix} \boldsymbol{e}_1^{(q)} \\ \boldsymbol{e}_2^{(q)} \\ \vdots \\ \boldsymbol{e}_N^{(q)} \end{bmatrix}_{[NT\times 1]}$$

In this SUR set-up, for each cross-section i, $z_i = [z_{i,1}, ..., z_{i,T}]'$ has full rank. The error terms are iid over time with zero mean and homoskedastic variance $\Sigma^{(q)} = E[e_t^{(q)} e_t^{(q)'} | \boldsymbol{z}_t]$, and the cross-error correlation between the $(i, j)^{th}$ ele-

ment of $\Sigma^{(q)}$ is denoted $\sigma_{i,j}^{(q)} = E[e_{i,t}^{(q)} e_{j,t}^{(q)'} | \boldsymbol{z}]$. We further assume that $\Sigma^{(q)}$ is positive definite, and the covariance matrix of the entire error disturbance vector $\boldsymbol{e}^{(q)} = [e_{1,1}^{(q)}, ..., e_{N,T}^{(q)}]$ is given by $E[vec(\boldsymbol{e}^{(q)}) \times vec(\boldsymbol{e}^{(q)})'] = \Sigma^{(q)} \otimes I_t$.

Under the assumption of cross-equation error correlation, OLS is inefficient, and estimators capable of exploiting the information on the correlation of the error terms can lead to a notable increase in efficiency in this case. One standard approach is to do this by employing feasible generalised least squares (FGLS) (Timm, 2002). The FGLS approach follows the two-step procedure for each of the two equations in 2.3.2:

- Obtain the residuals of the OLS regression $\hat{\boldsymbol{e}}^{(q)} = \boldsymbol{y} - (\boldsymbol{z}^\top \boldsymbol{z})^{-1} \boldsymbol{z}^\top \boldsymbol{y}$ and calculate the estimated residual co-variance matrix $\hat{\Sigma}^{(q)}$.
- Substitute the estimated residual co-variance matrix of the residuals for $\Sigma^{(q)}$ of the standard GLS representation to obtain the SUR estimator (SURE):

$$\hat{\boldsymbol{\beta}}_{FGLS}^{(q)} = \left(\sum_{t=1}^{T} \boldsymbol{z}_t \hat{\Sigma}^{(q)^{-1}} \boldsymbol{z}_t' \right)^{-1} \sum_{t=1}^{T} \boldsymbol{z}_t \hat{\Sigma}^{-1} \boldsymbol{q}_t.$$

for $q = \{x, y\}$. The efficiency gains of the SURE over OLS depend on the form of the cross-equation error correlation.[2] Furthermore, it requires data where $T > N$ to ensure the invertibility of the estimated covariance matrix $\hat{\Sigma}^{(q)}$. Lastly, the SUR approach is sensitive to including contemporaneous explanatory variables subject to unobserved common correlated effects. In this case, the approach would lead to inconsistent results (Coakley et al., 2006).

2.3.3 Granger causality in regime-dependent VARs

So far, we have assumed that the underlying DGP is linear, i.e., not subject to structural breaks or threshold effects. However, such structural stabilities are widely recognised in empirical and theoretical work (Baek and Brock, 1992; Lutkepohl, 1982b). In this section, we first introduce the Structural Break Threshold VAR (Galvão, 2006) before adopting the method to accommodate cross-sectional dependence.

[2]Some special cases should be noted here: First, in the case of no error correlation, FGLS is equivalent to equation-wise OLS (Amemiya, 1985, p. 198). The efficiency gains of FGLS over OLS also disappear in some special cases, as described in Kruskal's theorem (Kruskal, 1968). For a more detailed discussion see Moon and Perron (2006), Davidson et al. (1993) or Greene (2003).

The Structural Break Threshold VAR

Consider Equation 2.3.2 and define $z_t = [z_t, x_t]'$ and $z_{t-\ell} = [1, y_{t-1}, \ldots, z_{t-\ell}]'$ where ℓ denotes the autoregressive order for all $t = 1, ..., T$. We split the vector of coefficient estimates all cross-sectional unit $i = 1, \ldots, N$, $\beta_i^{(y)}$, into a subset of its intercepts $\alpha_i^{(y)}$ autoregressive components $\gamma_i^{(y)}$ as well as coefficients $\delta_i^{(y)}$ of the set lagged coefficients of $x_{i,t}$ given by $\beta_i^{(y)} = [\alpha_i^{(y)}, \gamma_i^{(y)}, \ \delta_i^{(y)}]'$.

We further drop superscript (y) for the vector of parameter coefficients for clarity and use superscripts to identify the set of regime-dependent parameter vector representations of the coefficient vectors. Following Galvão (2006), we can then write a structural break threshold VAR (SBTVAR) as:

$$\begin{aligned} z_{i,t} = &\left\{ \left[\left(z_{i,t-1}\beta_i^{(1)} \right) I_{i,t-d_1}(r_1) \ + \ \left(z_{i,t-1}\beta_i^{(2)} \right)(1 - I_{i,t-d_1}(r_1)) \right] I_t(\tau_i) \right\} \ + \\ &\left\{ \left[\left(z_{i,t-1}\beta_i^{(3)} \right) I_{i,t-d_1}(r_2) \ + \ \left(z_{i,t-1}\beta_i^{(4)} \right) (1 - I_{i,t-d_1}(r_2)) \right] (1 - I_t(\tau_i)) \right\} + e_{i,t}, \end{aligned} \tag{2.3.3}$$

where $I_{i,t-d_i}(r_i)$ is an indicator function that depends on a transition variable g_i, a threshold r_i and a delay d_i:

$$I_{i,t-d_i} = 1\left(g_{i,t-d_i} \leq r_i\right),$$

as well as $I_{it}(\tau_i)$ an indicator function that depends on a break point τ_i:

$$I_t(\tau_i) = 1(t \leq \tau_i).$$

As before, β denoted the vector of parameters and e the vector of errors. Galvão (2006)'s SBTVAR allows for one threshold VAR (TVAR) in each sub-sample identified by a breakpoint. Hence, the parameters of the regime-switch indicator function are also affected by the break.[3] Conversely, if there is (i) no threshold but a breakpoint; (ii) a threshold and no breakpoint; (iii) neither a break nor a threshold; the respective structural (SVAR) or threshold (TVAR) or linear VAR representations can be written as:

[3]We also define a restricted model where $r_1 = r_2$ given $d_1 = d_2$ where the parameters of the dynamics are allowed to change in each sub-sample but not the parameters of the regime-switching function. This model is referred to as the SBTVARc in (Galvão, 2006).

$$\text{(i): } z_{i,t}^{SBVAR} = (z_{i,t-1}\beta_i^{(1)})I_t(\tau_i) \; + \; (z_{i,t-1}\beta_i^{(2)})(1 - I_t(\tau_i)) + e_{i,t}^{SBVAR};$$
$$\text{(ii): } z_{i,t}^{TVAR} = (z_{i,t-1}\beta_i^{(1)})I_{i,t-d_1}(r_1) \; + \; (z_{i,t-1}\beta_i^{(2)})(1 - I_{i,t-d_1}(r_1) + e_{i,t}^{TVAR};$$
$$\text{(iii): } z_{i,t}^{VAR} \; = z_{i,t-1}\beta_i^{(1)} + e_{i,t}^{VAR};$$

whereas the full SBTVAR representation including a break and threshold follows Equation 2.3.3.

Identification of structural instabilities and coefficient estimation

An obvious problem in Equation 2.3.3 is the fact that the set of nuisance parameters, which identify structural instabilities in our system of equations, is not identified under the null hypotheses; see Davies (1987), Andrews and Ploberger (1994) or Hansen (1996). This implies that, before testing for potentially non-linear Granger causality patterns across equations, we must first identify the correct model specification for each of the i cross-sectional units in our sample. We achieve this by applying the identification procedure proposed in Galvão (2006). This approach follows a "general-to-specific" testing sequence which identifies a preferred non-linear specification for each of the respective SBVAR and TVAR representations of the underlying DGP before selecting the final, most adequate model representation. To this end, the selection mechanism takes the following steps:

1. For a given delay d_i, autoregressive order ℓ and transition variable r_i, and for each $i = 1, ..., N$, we estimate the coefficient matrices $\hat{\beta}_i^{(k)}$, $k = \{1, 2, 3, 4\}$ for varying values of r_1, r_2, τ_i over a grid of possible values of the thresholds and for breakpoints. The grid is chosen such that at least 100π per cent of all observations are available to estimate the autoregressive coefficients in each regime.[4]

2. We obtain the residuals $\hat{u}_t$ of all the possible combinations of $\hat{\beta}_i^{(k)}$ for each group of models. Assuming we estimated K models for each group of models (SBVAR, TVAR, SBTVAR), we are left with $K \times 3$ model representation at this initial stage.

[4]Following Clements and Galvão (2004); Galvão (2006) we set $\pi = 0.1$.

3. Next, we eliminate $(K-1)$ models from each group of models (SBVAR, TVAR, SBTVAR) such that we are left with only one model representation for each group. The model of choice for each group is the one that minimises the sum of squared residuals over the respective intervals and covariance matrix. Following Galvão (2006) we allow for time-varying covariance matrices which, for the most general case of the SBTVAR, have $k = 4$ regimes and are computed for the T_k observations of $\hat{u}_t$ for regime k as:

$$\hat{r}_1, \hat{r}_2, \hat{\tau}_i \; = \; \min_{\substack{r_l \leq r1 \leq r_u \\ r_l \leq r2 \leq r_u \\ \tau_l \leq \tau_i \leq \tau_u}} \left(\sum_{j=1}^{k} \frac{T_j}{2} log(det(\hat{\Sigma}_{ij}(r_1, r_2, \tau_i))) \right).$$

4. In order to discriminate between the VAR, TVAR, SBVAR or SBTVAR, we follow Galvão (2006) and employ a Wald and LM statistic (Altissimo and Corradi, 2002) which we describe in Appendix 2.B. At this stage, we are left with one VAR, SBVAR, TVAR or SBTVAR representation for each cross-sectional unit i.

5. Steps (1) to (4) above follow the procedure described in Galvão (2006) for the single-equation case. In the SUR context proposed here, the grid search procedure is performed on the SUR equations. Given the identified (potentially non-linear) N vector autoregressive models, we set up a system of equations following the representation in Equations 2.3.2, which we estimate with the VAR system employing the SURE as motivated before. This last step leaves us with a coefficient vector $\beta^{(q)}, q = \{x, y\}$

The test for Granger causality on the estimated $\beta^{(q)}$-vectors then follows a bootstrapped Wald test statistic which we outline in the next section.

2.3.4 A bootstrap test for potentially non-linear Granger causality under cross-sectional dependence

The test for Granger causality is an extension of the procedures based by Dumitrescu and Hurlin (2012); Kónya (2006) which we extend to the non-linear TVAR, SBVAR and TSBVAR cases. Across all variations, the test builds on a set of country-level Wald tests for each $i = 1, \ldots, N$ where the corresponding crit-

ical values are obtained employing a bootstrap procedure where cross-sectional blocks of residuals are drawn as demonstrated below. [5]

Wald test statistic

Then, the necessary and sufficient condition for no Granger causality of $x_{i,t}$ on $y_{i,t}$ for i is that $\hat{\delta}_i^{(y)} = 0$ for all $\ell = 1, ..., p$. The corresponding test statistic is given by:

$$(R_i \times \hat{\delta}^{(y)} - 0)^\top [R_i \hat{\mathbf{\Sigma}}_i^{(y)} R_i^\top]^{-1} (R_i \times \hat{\delta}^{(y)} - 0),$$

The corresponding critical values of these tests are obtained using the bootstrapping procedure outlined below.

Bootstrap procedure to obtain critical values

1. We estimate the SUR system in Equation 2.3.2a under the null hypothesis. For example, if we are to test for Granger causality of $x_{i,t}$ on $y_{i,t}$ for all countries and models, then $H_0 : \delta_i^{(y)} = 0$ for $1, ..., N$, as:

$$y_{i,t} = \alpha_i^{(y)} + y_{i,t}^p \gamma_i^{(y)} + e_{i,t}^{(y)},$$

where $y_{i,t}^{p_i}$ denotes the lagged autoregressive regressors up to order p for country i as before. The residuals of the above regression can be obtained as a $[NT \times 1]$ vector:

$$e^{(y)H_0} = y_{i,t} - \hat{\alpha}_i^{(y)} - y_{i,t}^p \hat{\gamma}_i^{(y)},$$

from which we can develop an $[N \times T]$ residual matrix of the form:

$$[\boldsymbol{e}^{(y)H_0}] = \begin{bmatrix} e_{1,1}^{(y)H_0} & e_{1,2}^{(y)H_0} & \dots & e_{1,T}^{(y)H_0} \\ e_{2,1}^{(y)H_0} & e_{2,2}^{(y)H_0} & \dots & e_{2,T}^{(y)H_0} \\ \vdots & \vdots & \ddots & \vdots \\ e_{N,1}^{(y)H_0} & e_{N,2}^{(y)H_0} & \dots & e_{N,T}^{(y)H_0} \end{bmatrix}.$$

[5] The bootstrap testing procedure below is described for Equation 2.3.2a for the sake of brevity where we return to superscripts (y) for the coefficient vector for clarity but also holds for Equation 2.3.2b. The coefficients for a 'reverse causal' test of $y_{i,t}$ on $x_{i,t}$ can be partitioned in a similar way. Results are not shown here for the sake of brevity. The procedure is described below for the linear VAR and the TVAR and SBVAR cases only.

2. In order to preserve the contemporaneous cross-correlation structure of the error term, a full column of the $[\boldsymbol{e}^{(y)H_0}]$ denoted $\boldsymbol{e}_t^{*(y)H_0}$ is randomly selected.

3. For each i, we then generate a bootstrap sample of y_i by again assuming no causal link for $t = 1, ...T^*,\ T^* \geq T$:[6]

   - For the case of a linear VAR, the $y_{i,t}^*$s are computed as follows:

   $$y_{i,t}^* = \hat{\alpha}_i^{(y)} + y_{i,t}^* \hat{\gamma}_i^{(y)} + e_{i,t}^{*(y)H_0}.$$

   - In case of either a TVAR or SBVAR model, we generate bootstrapped $y_{i,t}^*$s for both regimes as:

   $$y_{i,t}^{*(l)} = \hat{\alpha}_i^{(y)(l)} + y_{i,t}^{*(l)} \hat{\gamma}_i^{(y)(l)} + e_{i,t}^{*(y)H_0},$$

   and

   $$y_{i,t}^{*(h)} = \hat{\alpha}_i^{(y)(h)} + y_{i,t}^{*(h)} \hat{\gamma}_i^{(y)(h)} + e_{i,t}^{*(y)H_0},$$

   for the "lower" (l) and "upper" (h) regimes respectively. Depending on the previously identified regime changes identified by $\hat{\tau}_i, \hat{r}_i$, both these series are use to reconstruct $y_{i,t}^*$ as:

   $$y_{i,t}^* = \begin{cases} y_{it}^{*(l)} & \text{if } I(\hat{\tau}_i \leq t) \text{ or } I(x_{i,t-1} \leq \hat{r}_i); \\ y_{it}^{*(h)} & \text{else.} \end{cases}$$

4. Substitute the final sequence of $y_{i,t}^*$ for $t = 1, ...T^*$ of length T for $y_{i,t}$ and estimate:

   $$y_{i,t}^* = \alpha_i^{(y)} + y_{i,t}^{p_i} \gamma_i^{(y)} + x_{i,t}^{q_i} \delta_i^{(y)} + e_{i,t}^{*(y)H_0},$$

   and perform a Wald test (of no-Granger causality) on the $\delta_i^{(y)}$s for each i.

5. Repeat steps 2 to 4 R times to develop the empirical distribution of the Wald test statistic. Critical values are the appropriate percentiles of the sampling distributions.

In case the critical values suggest the rejection of the previously hypothesised null, the previously obtained SUR regression output for the (T/SB)VAR models

[6] For this study $T^* = 3 \times T$ in order to account for the initialisation of the algorithm.

can be consulted to learn about the sign and hence direction of the significant Granger causal relationship between the variables.

2.3.5 Monte Carlo simulations

In this section, we provide Monte Carlo evidence of the proposed procedure, which we compare against a set of commonly used estimators for different forms of cross-equation error correlation and varying degrees of structural instabilities. The Monte Carlo simulation evaluates various GDPs across a selection of estimators readily available to practitioners.

Estimators

The estimators which are analysed by means of their bias and root means square errors (RMSE) of the coefficient estimates are as follows: (i) equation-wise OLS; (ii) the previously outlined procedure which we refer to as the SURE; (iii) the cross-sectionally augmented ARDL (CS-ARDL) approach (Chudik et al., 2016) as well as (iv) the commonly adapted Panel Vector Autoregression (PVAR) model and (v) a simple, linear homogeneous VAR.[7] This is done under the assumption of the linear as well as partially non-linear model which we denote DGP_1, DGP_3 (under the assumption of simple cross-equation correlation of the error terms) and DGP_2, DGP_4 (assuming common correlated effects in the error terms), respectively.

The reasons behind the choice of included estimators are as follows. For one, it is of interest to learn about the behaviour of the three estimators that allow for the highest degree of cross-sectional heterogeneity - estimators (i), (ii) and (iii) - given different forms of cross-sectional dependence. Secondly, the simulation intends to elaborate on the importance of the adequate unit-specific formulation of the estimation equation as wrongfully imposed parameter homogeneity can lead to severely biased results. This is expected to become obvious for the particular case of the partially heterogeneous data samples (following DGP_3 and DGP_4).

[7] Estimator (iv) has been adopted from the R package `Panelvar` (Sigmund and Ferstl, 2017). All remaining approaches are generated using the package `PSBTGC` (Amann, 2022). A short illustration on how they relate to packages available online is provided in Appendix 2.E. The CS-ARDL approach in (iii) as described in (Chudik et al., 2016) follows a cross-sectionally augmented version of an ARDL model estimated as $y_{it} = \alpha + \sum_{\ell=1}^{py} \phi_{i\ell} y_{i,t-\ell} + \sum_{\ell=1}^{px} \varphi_{i\ell} x_{i,t-\ell} + \sum_{\ell=0}^{p\bar{z}} \psi'_{i\ell} \bar{z}_{i,t-\ell} + \epsilon_{i,t}$ where $\bar{z} = [\bar{y}_t, \bar{x}_t]'$ and the integer of $p\hat{z} = [T^{1/3}]$; see Chudik et al. (2016) for more information.

Data generating processes

For the purpose of the simulations in this paper, the dependent variable and regressors are generated using a simple (piece-wise) VAR(1) where our main parameter of interest is the one that identifies a potentially (regime-dependent) Granger causal relationship of $x_{i,t-1}$ on $y_{i,t}$.[8]

- Linear VAR:

$$y_{it} = \alpha_i^{(0)} + \gamma_i^{(0)} y_{it-1} + \delta_i^{(0)} x_{it-1} + e_{it}^{(y)} \tag{2.3.4}$$

- Threshold VAR (TVAR):

$$\begin{aligned} y_{it} =& [\alpha_i^{(1)} + \gamma_i^{(1)} y_{it-1} + \delta_i^{(1)} x_{it-1}] I_{i,t-1(x_i)} \\ & [\alpha_i^{(2)} + \gamma_i^{(2)} y_{it-1} + \delta_i^{(2)} x_{it-1}](1 - I_{i,t-1(x_i)}) + e_{it}^{(y)} \end{aligned} \tag{2.3.5}$$

- Structural Break VAR (SBVAR):

$$\begin{aligned} y_{it} =& [\alpha_i^{(3)} + \gamma_i^{(3)} y_{it-1} + \delta_i^{(3)} x_{it-1}] I_t(\tau_i) \\ & [\alpha_i^{(4)} + \gamma_i^{(4)} y_{it-1} + \delta_i^{(4)} x_{it-1}](1 - I_t(\tau_i)) + e_{it}^{(y)} \end{aligned} \tag{2.3.6}$$

Given this specification, we want to compare how precisely we can estimate parameters $\delta_{i1}^{(b)}, b = \{0, 1, 2, 3, 4\}$ which identify a possibly (regime-dependent) Granger causal relationship from x_{it-1} on y_{it} across all different specifications.

Linear DGPs. For the simple linear VAR case (Equation 2.3.4) we generate data based on the following structure where we drop the superscripts for the sake of clarity:

$$y_{it} = \alpha_i + \gamma_i y_{it-1} + \delta_i x_{it-1} + u_{it}, \tag{2.3.7}$$

with

[8] The discussion below is limited to this case. The analysis of the "reverse causal" channel would be identical.

$$x_{it} = \rho_{xi} x_{it-1} + \boldsymbol{\phi}_i' \boldsymbol{f}_t + \xi_i' \chi_t + e_{xit} \tag{2.3.8a}$$

$$u_{it} = \rho_{ui} u_{it-1} + \boldsymbol{\gamma}_i' \boldsymbol{f}_t + e_{uit} \tag{2.3.8b}$$

$$f_{mt} = \rho_{fm} f_{mit-1} + e_{fmt} \tag{2.3.8c}$$

$$\chi_t = \rho_{\chi} \chi_{it-1} + e_{\chi t} \tag{2.3.8d}$$

which follows the general set-up in Coakley et al. (2006) with $m = 2$ factors such that $\boldsymbol{f}_t = [f_{1t}, f_{1t}]'$, $\boldsymbol{\phi}_i = [\phi_{1i}, \phi_{2i}]'$ and $\boldsymbol{\gamma}_i = [\gamma_{1i}, \gamma_{2i}]'$.[9] We generate heterogeneous intercepts as $\alpha_i \sim iidU[0.5, 0.5]$ and impose homogeneous slopes $\delta_i = \delta = \{0, 0.2, 0.4, 0.8\}$. We also set $e_{jt} \sim iidN(0, \sigma_j^2), \sigma_j = 1 \; \forall \; j$ and $j = \{xi, ui, \chi i, f_1, f_1\}$.[10] Further parameter values vary depending on the different model specifications chosen. More specifically:

- DGP_1: $\rho_{xi} = 0.8$, $\gamma_m, \phi_{mi} \sim iidU(0.5, 1.5)$, $m = 1, 2$ all other $\xi_i = \rho_{ui} = \rho_f = \rho_{\chi} = 0$ which follows the "stationary settings" case in Coakley et al. (2006);
- DGP_2: $\rho_{xi} = 0.8$, $\gamma_i, \phi_i, \xi_i \sim iidU(0.5, 1.5)$ and $\rho_{ui} = \rho_{fm} = \rho_{\chi} = 0.3$, $m = 1, 2$ which introduces autocorrelation in the common factor error structure.

Non-linear DGPs. In order to account for non-linearities in the panel setting, for each of the initial two DGPs described above, we generate two regimes where $\delta_{i1}^{(b)} = 0$ for $b = \{1, 3\}$ while we introduce regime-dependent causality for $b = \{2, 4\}$ for $N \times 2/3$ of the data following Equation 2.3.5 or Equation 2.3.6 while the remaining $N/3$ follows the linear set-up of Equation 2.3.4. This way, we generate a heterogeneous panel with a partially non-linear causality structure of x on y. More precisely:

- For the break models, we randomly selected a $N/3$ sample of the data and draw a random breakpoint $\tau_i \sim iidU[\pi_L, \pi_H]$ where π_L, π_H identify the cut-offs of the 20^{th} and 80^{th} percentile of each time series.

[9] The set-up differs in so far as we impose a VAR(1) data structure with no contemporaneous explanatory variable in Equation 2.3.7 which would yield to inconsistency of the standard SUR estimator (Coakley et al., 2006).

[10] Since our focus is not on elaborating on the difference between fixed effects and mean group estimators as in Coakley et al. (2006) we do not impose cross-unit variance variation through σ_{xi}.

- For the threshold models, we discard the first π_L observation and identify the country-level threshold such that $I_{i,t-d_1=1(x_{i,t-1}\geq QU_4)}$ for each time series of the randomly selected $N/3$ panel members, i.e., when the lagged value of x_{it} first surpasses the 4th quantile of the data series for the first time.

Then, the final data series are obtained as:

$$y_{i,t} = \begin{cases} y_{it}^{(l)} & \text{if } I(\tau_i \leq t) \text{ or } I(x_{i,t-1} \leq QU_4); \\ y_{it}^{(h)} & \text{else;} \end{cases}$$

which we denote DGP_3 and DGP_4 if the error structure follows the description in DGP_1 and DGP_2 respectively. As for the linear case we simulate $\delta_{i1}^{(b)} = \{0.8, 0.4, 0.2, 0.0\}$ for $b = \{0, 2, 4\}$ while $\delta_{i1}^{(b)} = 0$ for $b = \{1, 3\}$. Therefore, we impose Granger causality by construction on a particular sub-set of parameter values. All DGPs are constructed for $R = 1000$ repetitions and $N = \{6, 21, 21, 30\}$ cross-sectional unites. Furthermore, we simulate data for $T = \{20, 30, 50, 80, 150\}$ which are typical dimensions of a macro panel data model. The series are generated over the time interval $(t = -99, -98, ..., 0, 1, ..., T)$ with the starting values $(t - 100 = 0)$ for all autoregressive processes where the first $(t = -99, ..., 0)$ observations are discarded to minimise the effect of the initialisation of the DGP routines. We offer a summary of the proposed DGPs in Table 2.1.

Next, we evaluate how close the competing estimators get to recovering the true population parameter in this partially heterogeneous panel setting. The reason we focus on this rather simple set-up and the different forms of DGPs is many-fold. First of all (particularly in DGP_1 and DGP_2), we want to illustrate when and how the performance increase of SURE over OLS becomes more apparent when we increase the transmission of cross-sectional correlation in the system by introducing autocorrelation of the correlated error structures. As far as the non-linear domain is concerned, DGP_3 and DGP_4 follow the error structures of DGP_1 and DGP_2 as illustrated above. These two DGPs illustrate the potential problems arising from wrongfully imposing homogeneity , i.e. the "linear VAR" estimator (which imposes the identical model structure across all is thereby omitting the partial non-linear nature of the DGPs), or (ii) by pooling, i.e. the PVAR case.

Table 2.1: Monte Carlo Simulation: Summary of DGPs

	Estimators	Error structure	Emp. model
Linear DGPs			
DGP_1	OLS, SUR, CS-ARDL, PVAR	stationary error correlation	linear VAR
DGP_2	OLS, SUR, CS-ARDL, PVAR	autoregressive error correlation	linear VAR
Partially non-linear DGPs			
DGP_3	OLS, SUR, linear VAR, PVAR	see DGP_1	partially non-linear (SB/T)VAR
DGP_4	OLS, SUR, linear VAR, PVAR	see DGP_2	partially non-linear (SB/T)VAR

Note: Results for 'OLS', 'SUR' and 'VAR' based on R package `PSBTGC` (Amann, 2022); see Appendix 2.E. Results for column 'PVAR' based on R package `Panelvar` (Sigmund and Ferstl, 2017).

Monte Carlo findings

Linear model with cross-equation error correlation (DGP_1). The results for the linear cases assuming cross-equation error correlation are presented in Table 2.8 for the bias and the root mean squared error in Table 2.9, respectively. Overall, there is little difference between the performance of OLS, SUR and the ARDL estimator. As expected, increasing T improves all estimators' precision. The ARDL estimator reports noteworthy gains with increasing T, as noted in Chudik et al. (2016). Furthermore, for low N, PVAR performs poorly but notably improves for higher N.

Linear model with common correlated effects error structure (DGP_2). The results for the linear cases assuming cross-equation error correlation are presented in Table 2.10 for the bias and the root mean squared error in Table 2.11, respectively. By and large, PVAR behaves similarly to before. We also see a notable difference in the performance between OLS and the SUR and ARDL estimators. Whereas the former is notably affected by the correlated error structure, the ARDL and SUR estimators remain largely unaffected by the altered DGP.

Non-linear model with cross-equation error correlation and common correlated effects error structure (DGP_3, DGP_4). The results for the non-linear cases assuming no cross-equation error correlation are presented in Table 2.12 for the bias and the root mean squared error in Table 2.13 for respectively. The results for the linear cases assuming common correlated effects are presented in Table 2.14 for the bias and the root mean squared error in Table 2.15 respectively. As far as the non-linear DGPs are concerned, the results align with the observations made for the linear simulations. For the stationary and autocorrelated data process, the small sample bias of the SURE remains unaffected mainly by the choice of DGP. The performance of the alternative estimators is also as expected as they are either plagued by wrongfully assuming cross-sectional homogeneity (PVAR and, to a lesser extent, VAR) or the lack of addressing cross-sectional dependence (OLS and VAR).

2.3.6 The PSBTGC package

The previously described method is made available via the `R` package `PSBTGC` (Amann, 2022) and can be downloaded from the package web page. All simulations and empirical applications presented in this paper are integrated into the current version of the package. For more information and documentation on the routine, please see Appendix 2.E or consult the project web page: https://amannj.github.io/resources/PSBTGC/.

2.4 Application: The debt-growth nexus

We now apply the Granger causality testing framework to the debt-growth nexus. For the analysis, we will focus on quarterly OECD data spanning the period 1995Q1 to 2017Q4; see Appendix 2.A for a description of the data. This analysis focuses on quarterly OECD data rather than the much more broadly used annual Reinhart and Rogoff (2010) data on sovereign debt and economic growth set because of the following reasons. Firstly, the quarterly data benefits from a longer time series and encompasses the financial crisis and its recovery afterwards. Consequently, the OECD sample benefits from increased scope up until 2018 and captures a period of increased volatility witnessed since 2009. Moreover, in the post-financial crisis period, numerous sampled countries experienced an increase in the respective debt-to-GDP ratios of up to (and above)

the 90 per cent mark. Furthermore, the use of lower frequency data can lead to confusion over the direction of causality, a key issue in the context of this debate over the relationship between debt and growth. The causal relationship can appear immediately in lower frequency data, which may explain the conflicting findings in the debt growth nexus literature, underlining the value of exploratory analyses such as this study.

For the empirical analysis, we proceed as follows. We first establish the existence of cross-sectional dependence of our data sample to ensure the adequacy of our proposed procedure in subsection 2.4.1. We then set out to test for bi-directional Granger causality in subsection 2.4.2 assuming linearity first. This serves two purposes: On the one hand, it allows us to reproduce some selected results of the literature which has employed similar tools, thereby adding to the credibility of our approach. On the other hand, by allowing for heterogeneity in the specification of the estimation equations across i, we illustrate the importance of correctly identifying the lag orders of the respective equations.

A particularly useful feature of the proposed method in this paper is that it allows us to easily test for endogenous and exogenous instabilities. This is demonstrated by testing for Granger causality of the relationship between debt and growth in the context of the Reinhart and Rogoff (2010) debt threshold hypothesis in subsection 2.4.3. We can conduct such a test in a very convenient way by exogenously defining the debt threshold cut-off of 90 per cent for the set of countries surpassing this landmark. At the same time, we can retain the group of countries that remain above/below said cut-off over the whole sample period, thereby using the cross-sectional information of the entire sample.

After the linear model and the debt threshold analysis, we finally test for endogenously determined instabilities by fully employing the previously outlined approach in subsection 2.4.4. As it turns out, the endogenously determined model specification is non-linear for most countries. Moreover, while there seems to be a considerable variation in the characteristics of identified debt threshold regimes across countries, the financial crisis is consistently selected as a structural breakpoint across multiple economies.

2.4.1 Pre-testing

We start our empirical analysis by first testing for cross-sectional dependence to confirm that the above-outlined SUR approach is to be favoured over

Table 2.2: Cross-sectional dependence tests, quarterly OECD data

Test	Test Statistic	Sig.
LM	2525.49	***
CD	43.86	***

Note: *p<0.1; **p<0.05; ***p<0.01. Results obtained for balanced sub sample 2000Q1-2018Q2 based on R package `PSBTGC` Amann (2022); see Appendix 2.E.
Source: Authors' calculation based on quarterly OECD data; see Appendix 2.A.

equation-wise OLS. We do so by employing the Breusch and Pagan (1980) Lagrange multiplier (LM) as well as Pesaran (2004)'s CD test. As Table 2.2 illustrates, the null of no cross-sectional dependence is rejected at the 1 per cent level for the LM and the CD test, confirming the validity of the SUR approach.

Next, we determine the optimal lag length for the application in two distinct ways. First, we employ the method suggested by Kónya (2006) to select the lags based on an AIC and assume that these lags are the same across all cross-sectional units. The results in Table 2.3 indicate that the optimal model based on the AIC and SIC requires four lags for the debt-to-GDP ratio and the GDP growth rates, respectively.

To allow for more cross-country heterogeneity, we next establish the optimal lag length of the debt-to-GDP level variable for each country, which is presented together with the breakpoint test results in Table 2.17. As can be readily seen, there is a notable difference in the identified lag lengths across both applications where the individually determined lag orders are smaller than for the joint test, indicating a potential gain in efficiency of the country-specific equation specification.

Table 2.3: Optimal lag length selection, quarterly OECD data

Criterion	GDP Growth rate	Debt-to-GDP Ratio
AIC	4	4
SIC	4	4

Note: Results obtained for balanced sub sample 2000Q1-2018Q2 based on R package `PSBTGC` Amann (2022); see Appendix 2.E.
Source: Authors' calculation based on quarterly OECD data; see Appendix 2.A.

2.4.2 The linear VAR specification

In this section, we analyse different linear model specifications. More precisely, we evaluate if the additional flexibility of the SUR approach leads to different results than when fixing the model specification cross-sectionally. To demonstrate this, we apply the previously outlined SUR approach to the simple VAR case by comparing the suggested results when running a simple VAR(1), VAR(2) and VAR(4) as well as the fixed model selection proposed by the SIC in Table 2.3 and country-level specification described in Table 2.17 for the VAR(i) specification, respectively. We present the Wald test statistics results, including the identified sign of the Granger causal relationship for the various estimated models in Table 2.18.[11] Following the previously introduced notation, we estimate:

$$y_{it} = \alpha_{i0}^{(0)} + \sum_{i=\ell}^{p_i} \gamma_{i\ell}^{(0)} y_{it-\ell} + \sum_{i=\ell}^{q_i} \delta_{i\ell}^{(0)} x_{it-\ell} + e_{it}.$$

The results in Table 2.18 indicate a notable difference in reported, significant Granger causal relationships across the different models. Exemplarily, Table 2.18 indicates that for the case of Australia, VAR(1), VAR(2) and VAR(i) all report a significant Granger causal relationship from the debt-to-GDP ratio to GDP growth. However, for the VAR(4) specification, the Wald test fails to reject the null non-Granger causality of debt on growth. Moreover, the case of Australia also serves as a good example to highlight the consistency of the VAR(i) in comparison with the other proposed models. The preferred VAR(1) model specification, as seen in Table 2.17, generates very similar test statistics as in the VAR(i). Similar observations can be made for the case of France, Greece and Canada.

Table 2.18 allows us to develop further insights regarding the direction of the Granger causal relationships across models. Exemplarily, for the case of Australia, a negative Granger causal relationship of past debt-to-GDP ratios negatively affecting GDP growth is recorded for the VAR(1) VAR(i) case. In contrast, Germany only reports a significant Granger causal relationship from growth to debt for the VAR(1) and VAR(4) models. In short, this section's empirical results support a more pronounced 'reverse-causal' (and negative) relationship

[11] The coefficient estimates of the various models have not been added to this version of the paper for brevity.

Table 2.4: Results Granger causality for linear VAR specification

Sign	$Debt \Rightarrow Growth$	$Growth \Rightarrow Debt$
+		
-	Australia**, Portugal**	Canada***, Denmark**, Finland**, France**, Greece**, Italy**, Spain***, Sweden*, UK*, US*

Note: Bootstrapped Wald test statistics are based on country-level specification and simulated based on 500 repetitions. Sign of causal relationship reported in parenthesis. Significant results at the 1, 5 and 10 per cent level are denoted ***, ** and * respectively. For Wald test statistics see Table 2.18. Results based on R package `PSBTGC` Amann (2022); see Appendix 2.E.
Source: Authors' calculation based on quarterly OECD data; see Appendix 2.A.

between growth and debt. In other words, low growth episodes are found to significantly increase sovereign debt build-ups, whereas the latter is not found to affect growth performances in a significant way. As elaborated before, this is very much in line with other papers in the literature, which have resorted to a similar linear VAR framework to test for Granger causality. Most notably, Lof and Malinen (2014); Puente-Ajovín and Sanso-Navarro (2015) support the hypothesis that debt build-ups are instead a symptom rather than the causal root of stifled economic growth in the sense that high deficit spending may be one but not the only driving force of crumbling GDP growth rates.

2.4.3 Testing the Reinhart and Rogoff (2010) hypothesis

The observation regarding the 'reverse-causal' transmission channel may align with the literature findings. Still, the imposed framework is constrained by its imposed assumption of a linear relationship between both variables. It also has to be noted that these findings do not imply that there is generally no effect of high debt-to-GDP ratios, particularly in a non-linear setting. Equipped with these insights, we set out to investigate the potentially non-linear relationship between economic growth and public debt employing a hypothesised, exogenously fixed 90 per cent threshold of the debt-to-GDP ratio as suggested by Reinhart and Rogoff (2010)'s debt threshold hypothesis.

In this set-up, the advantage of our testing procedure becomes apparent through its flexibility. As we want to test the hypothesis of an exogenous threshold across all countries, following a homogeneous-equation set-up, only countries experiencing this regime would be observed, leading to a significant loss in information. With country-wise equations, we can test for threshold-

dependent causality patterns while also addressing cross-sectional spillovers for the entire sample. This insight is particularly useful as out of the 19 countries in our sample, only Canada, France, Ireland, Portugal, Spain, the UK, and the US experience a threshold transition. The others either remain below (Australia, Austria, Denmark, Finland, Germany, Hungary, Netherlands, Norway, Sweden) or above (Belgium, Greece, Italy, Japan) the hypothesised cut-off at all times.

We explore how this restriction may affect the estimation by again comparing the Wald statistics and sign of the cumulative coefficient estimates for the TVAR(1), TVAR(2), TVAR(4) and (T)VAR(i). Where the former three models are only estimated for the sub-sample of countries that experience a rise in their respective debt thresholds at some point, the final model specification includes both linear as well as threshold-specific estimation equations:

$$\begin{aligned} y_{it} = &\Big[\alpha_{i0}^{(1)} + \sum_{i=\ell}^{p_i} \gamma_{i\ell}^{(1)} y_{it-\ell} + \sum_{i=\ell}^{q_i} \delta_{i\ell}^{(1)} x_{it-\ell}\Big] I_{i,t-d_i(r_i)} + \\ &\Big[\alpha_{i0}^{(2)} + \sum_{i=\ell}^{p_i} \gamma_{i\ell}^{(2)} y_{it-\ell} + \sum_{i=\ell}^{q_i} \delta_{i\ell}^{(2)} x_{it-\ell}\Big] (1 - I_{i,t-d_i(r_i)}) + e_{it}, \end{aligned}$$

where we exogenously impose a one-period delay $d_i = 1$ on the threshold variable which is the debt-to-GDP ratio at the 90 percent level, i.e. $I_{i,t-1} = 1(x_{t-1} \leq 90)$ for all i. Table 2.16 provides a summary of the respective model specification for all countries.[12]

Results for the threshold regression exercises are provided in Table 2.19: As before, a negative and significant relationship between growth and debt can be reported in most cases. This result holds across specifications. Under the debt threshold hypothesis, debt levels will adversely cause growth beyond the sovereign-debt-to-GDP ratio of 90 per cent. Only for Portugal and Spain can we report such a threshold effect for the (T)Var(i) specification, which remains insignificant. In short, our analysis does not find a significant and causal relationship from high debt to low growth for any of the countries surpassing the 90 per cent debt threshold. What is more, it illustrates the advantages of the flexible testing procedure which allows us to test for the threshold hypothesis

[12]Please note that for the case of Canada, only very few observations for the low debt regime are available. Consequently, the results for this country should be treated with caution and were not estimated for the TVAR(4) specification.

Table 2.5: Results Granger causality for exogenous regime change at 90 percent of the debt-to-GDP ratio

Sign	*Debt* ⇒ *Growth*	*Growth* ⇒ *Debt*
+		**Canada***** , **Spain*****
-	Australia**	**Canada*****, Denmark**, Finland**, **France****, **France****, Germany*, Greece**, Italy***, **Portugal*****, **Portugal*****, Sweden** , **Spain*****

Note: Bootstrapped Wald test statistics are based on country-level specification and simulated based on 500 repetitions. Sign of causal relationship reported in parenthesis. Significant results at the 1, 5 and 10 percent level are denoted ***, ** and * respectively. The lower left-hand side consistent with the 'threshold literature' view is highlighted in red. Regimes below and above the 90 per cent sovereign debt-to-GDP ratio. Threshold models highlighted in **bold**. Results based on R package `PSBTGC` Amann (2022); see Appendix 2.E.
Source: Authors' calculation based on quarterly OECD data; see Appendix 2.A.

while simultaneously taking into account cross-country correlation effects for countries in the sample that do not experience the same kind of regime change. On the other hand, only the (T)VAR(i) specification allows us to recover France's significant and negative Granger causal relationship from growth to debt. Additionally, borderline significant results for the UK and US in the linear VAR(i) set-up are no more present in the (T)VAR(i) specification.

Table 2.5 provides a quick visual overview of the identified Granger causal relationships of the data sample. If the Reinhart and Rogoff (2010) debt threshold hypothesis held in our sample, we would expect to find a substantial number of significant countries highlighted in red (which corresponds to a debt-to-GDP ratio above the proclaimed 90 per cent) in the lower left panel of the table. This would identify all countries that report a negative Granger causal relationship from countries as they surpass the 90 per cent debt threshold. However, this hypothesised pattern does not seem to be present in the data.

2.4.4 Granger causality under endogenous instabilities

Finally, we turn to the fully fleshed model, which allows us to evaluate (changes in) Granger causal patterns based on endogenously identified instabilities. We first summarise the results of the endogenously determined models in Table 2.20 following the grid search procedure with time-varying covariance matrices (HML). Table 2.20 summarises the identified instabilities for the tested structural break and threshold VAR models for all countries in our sample. For most countries, break dates close to the financial crisis have been identified

consistently across the different models. Regarding potential thresholds, one can note a very high degree of variation across countries (ranging from 31.88 for Norway to 232.39 for Japan). In contrast, similar threshold values are again identified consistently across the different models. For example, in the case of Austria, the single structural break (threshold) model identifies 2011Q2 (68.26 per cent of the debt-to-GDP ratio) while the SBTVAR (SBTVARc) model identifies 2012Q4 and 68.35 per cent as well as 2007Q3 and 73.09 per cent.

Out of these competing model specifications, Table 2.21 presents the BWald statistic for all possible tests of the two-step model selection procedure as described before.[13] As can readily be seen, the majority of the preferred models are suggested to follow a non-linear representation and either incorporate a structural break - for the case of Australia (2012 Q2), Austria (2011 Q2), Belgium (2008 Q4), Italy (2013 Q2) as well as Norway - or a threshold - France (69.36), Greece (103.22), Netherlands (72.74), Portugal (57.46), Spain (103.66), UK (41.57), US(69.53). Whereas the identified break models consistently report a change in the relationship of the system in the aftermath of the financial crisis, the identified threshold models are characterised by a considerable variation. Based on this insight, the empirical model thus takes the form:

$$\begin{aligned} y_{it} =&\{[\alpha_{i0}^{(1)} + \sum_{i=\ell}^{p_i} \gamma_{i\ell}^{(1)} y_{it-\ell} + \sum_{i=\ell}^{q_i} \delta_{i\ell}^{(1)} x_{it-\ell}] I_{i,t-d_i(r_i)} + \\ & [\alpha_{i0}^{(2)} + \sum_{i=\ell}^{p_i} \gamma_{i\ell}^{(2)} y_{it-\ell} + \sum_{i=\ell}^{q_i} \delta_{i\ell}^{(2)} x_{it-\ell}](1 - I_{i,t-d_i(r_i)})]\} I_t(\tau_i) + \\ & \{[\alpha_{i0}^{(3)} + \sum_{i=\ell}^{p_i} \gamma_{i\ell}^{(3)} y_{it-\ell} + \sum_{i=\ell}^{q_i} \delta_{i\ell}^{(3)} x_{it-\ell}] I_{i,t-d_i(r_i)} + \\ & [\alpha_{i0}^{(4)} + \sum_{i=\ell}^{p_i} \gamma_{i\ell}^{(4)} y_{it-\ell} + \sum_{i=\ell}^{q_i} \delta_{i\ell}^{(4)} x_{it-\ell}](1 - I_{i,t-d_i(r_i)})]\}(1 - I_t(\tau_i)) + e_{it} \end{aligned} \tag{2.4.1}$$

where the threshold and structural break parameters which has been identified through the grid search procedure are identified as $I_{i,t-1} = 1(x_{t-1} \leq r_i)$ and $I_t(\tau_i)$ based on the previously discussed results. As before, Equation 2.4.1 only describes the estimated model to test Granger causality from past debt-to-GDP ratios on growth but holds for the 'reverse-causal' case as well; as before we account for unobserved common factors in Equation 2.4.1 employing the SURE. The results are summarised in Table 2.6 with more detailed results available in Table 2.22.

For countries that are potentially subject to a negative, individual debt thresh-

[13] In all cases, neither the SBTVAR nor SBTVARc representations were selected by BWald and are therefore not reported here.

Table 2.6: Results Granger causality based on endogenously identified instabilities

Sign	*Debt* ⇒ *Growth*	*Growth* ⇒ *Debt*
+	**Greece**** **UK***	*Italy****, **Spain*****, **UK*****
-	**Greece**** , **Portugal****, **Portugal****, UK**	*Austria****, *Austria****, *Belgium****, *Belgium****, Canada***, Denmark***, Finland**, Germany***, *Italy****, Sweden***, **Portugal*****, **Portugal*****, **Spain*****, **UK***, **US*****, US***

Note: Bootstrapped Wald test statistics are based on country-level specification and simulated based on 500 repetitions. Sign of causal relationship reported in parenthesis. Significant results at the 1, 5 and 10 percent level are denoted ***, ** and * respectively. The lower left-hand side consistent with the 'threshold literature' view is highlighted in red. Threshold (structural break) models highlighted in **bold** (*italics*) with the endogenously determined lower and upper regimes highlighted respectively. Significant thresholds identified for: Finland at debt-to-GDP ratio of 42 per cent, Greece (103 per cent), Netherlands (73 per cent), Portugal (57 per cent), Spain (104 per cent), UK (42 per cent), US (70 per cent). Results based on R package `PSBTGC` Amann (2022); see Appendix 2.E.
Source: Authors' calculation based on quarterly OECD data; see Appendix 2.A.

old effect can be identified if they are highlighted in red (corresponding to the previously identified debt threshold) and placed in the lower left corner of Table 2.6 (significant effect of increasing debt levels having a negative Granger causal effect on the GDP growth rate). As can readily be seen, there is only limited support for the hypothesis that the relationship between debt and growth changes depending on a specific country-level threshold. Except for Spain and the UK, which both experience a change in the sign of the respective regimes, all other regime-dependent cases only observe a change in the magnitude of past debt (growth) on growth (debt).[14]

We acknowledge that the lack of controls imposes a significant limitation on the current analysis. Most notably, this paper does not incorporate other relevant macroeconomic variables such as inflation, interest rates, and alternative debt, which will be left for further research.

2.5 Conclusion

The continuing debate over the imposition of measures to deal with sovereign indebtedness since the financial crisis has centred around the seminal contribu-

[14]For debt to growth these include Greece (0.12, -0.85), Portugal (-0.17, -0.31), UK (-0.13, 0.38). For growth to debt these are Austria (-0.37, -0.64), Belgium (-0.52, -0.82), Italy (0.92, -0.36), Portugal (-0.50, -0.91), Spain (0.06, -0.67), UK (-0.52, 0.03), US (-0.51, - 0.42).

tion by Reinhart and Rogoff (2010). With the wide adoption of fiscal discipline policies, also known as austerity, in developed nations, the importance of this perspective for economic policy should not be overstated. The approach presented in this piece tries to address the question of (regime-dependent) changes in Granger causal relationships without imposing the assumption of a unifying debt threshold. This piece finds strong evidence of a negative Granger causal relationship of (low) growth to (high) debt (and vice versa). Still, it fails to identify a 'reverse causal' connection from debt to growth. It also fails to find empirical evidence in support of the debt threshold hypothesis when imposing an exogenously introduced threshold of 90 per cent of the debt-to-GDP ratio in line with Reinhart and Rogoff (2010). At the same time, the analysis does provide evidence for debt threshold effects when allowing for endogenous regime detection, which, however, are spread over a wide range (between 40 per cent and up to and above 100 per cent of the debt-to-GDP ratio).

The contribution of this piece is twofold. On the methodological side, it proposes a simple framework to test for (changes in) endogenously determined, regime-dependent causal patterns in a non-linear bi-variate vector auto-regressive (VAR) system in the presence of cross-sectional error correlation employing a seemingly unrelated regressions (SUR). In an extensive Monte Carlo simulation study, we demonstrate that this methodology performs well compared to standard estimators when evaluated in a heterogeneous and cross-sectionally dependent panel.

The empirical analysis of this piece stresses that the relationship between sovereign debt and GDP growth is more heterogeneous than frequently assumed in the literature and that heterogeneous, country-level non-linearities do not seem to be the exception but rather the norm. Consequently, based on the proposed method and set of countries analysed in this piece, generalising assumptions on a homogeneous debt tipping point should be treated with great caution.

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Appendix Chapter 2

2.A Data

The OECD data was taken from **OECD.StatExtract** as of February 2018 and covers the periods 1995Q01 to 2017Q04. For the sake of completeness, we here restrict our analysis to the same group of countries which results in a sample of 19 countries, given that there is no quarterly OECD debt data available for New Zealand. The variables, for which we provide summary statistics in Table 2.7 included for this exercise are as follows:

- We use the **OECD Quarterly National Accounts** data set at http://stats.oecd.org/Index.aspx?DataSetCode=QNA and obtain *'US dollars, volume estimates, fixed PPPs, OECD reference year, annual levels, seasonally adjusted' (VPVOBARSA)* to calculate GDP growth rates. Since this is quarterly data, setting $l = 1$ to calculate growth rates is equivalent to OECD's *'growth rate compared to the previous quarter, seasonally adjusted' (GPSA)*. Conversely, $k = 4$ lets us recover *'growth rate compared to the same quarter of the previous year, seasonally adjusted' (GYSA)*, which is the variable of our interest.

- For the gross government debt ratio, we use *'general government total gross debt, percentage of GDP at current prices and national currencies' (PCTGDPA)* taken from OECD's **Quarterly Public sector Debt** database available at http://stats.oecd.org/Index.aspx?DataSetCode=QASA_TABLE7PSD.

2.B Model selection decision rule

The decision rule to discriminate against the competing VAR, TVAR and SBVAR representations of the empirical models employed in this paper follow Altissimo and Corradi (2002). The authors illustrate how to compute asymptotic bounds based on the law of the iterated logarithm such that a decision rule for rejecting the null hypothesis can be derived and is effective in correctly choosing between a linear and a threshold model. As in Galvão (2006), the model selection is employed to derive bounds of supLM and supWald statistics

Table 2.7: Summary statistic of quarterly OECD data with $\tau = 90, k = 4$ (OECD code: GYSA).

Period	GDP Growth Rate	Debt to GDP ratio
Min. :1995	Min. :-10.2813	Min. : 4.015
1st Qu.:2002	1st Qu.: 0.8864	1st Qu.: 53.513
Median :2008	Median : 2.0869	Median : 71.954
Mean :2007	Mean : 1.9244	Mean : 82.233
3rd Qu.:2013	3rd Qu.: 3.3113	3rd Qu.:106.028
Max. :2018	Max. : 29.1193	Max. :240.149

Note:
Countries (nobs.) included in this data are: Australia (93), Austria (73), Belgium (90), Canada(93), Denmark (73), Finland (73), France (70), Germany (78), Greece (73), Ireland (77), Italy (90), Japan (85), Netherlands (78), Norway (90), Portugal (78), Spain (90), Sweden (90), United Kingdom (93), United States (93).

to discriminate between the VAR, TVAR, SBVAR, SBTVARc as well as SBTVAR models for each country i.

More specifically, the decision rule to distinguish between models uses the asymptotic bounds $(1/2ln(lnT))$ and the maximum value of a Wand and LM statistic over the grid of previously obtained model variations of the nuisance parameter. Both tests are computed based on the sum of squared residuals (SSR) under the null and alternative as

$$W(\theta_2) = n\left(\frac{SSR(\hat{\theta}_1) - SSR(\theta_2)}{SSR(\theta_2)}\right), \quad LM(\theta_2) = n\left(\frac{SSR(\hat{\theta}_1) - SSR(\theta_2)}{SSR(\hat{\theta}_1)}\right)$$

where vector θ_1 has parameters such as the threshold and breaks of the respective model under the null while θ_2 contains those of the alternative. The rule ensuring that type I and II errors are asymptotically zero is such that the model under the alternative must be chosen if

$$\left[\frac{1}{2ln(ln(T))}\left[sup_{\theta_2^L \leq \theta_2 \leq \theta_2^H} W(\theta_2)\right]^{1/2}\right]$$

and a similar rule can be written for $sup_{\theta_2^L \leq \theta_2 \leq \theta_2^H} LM(\theta_2)$. The respective statistics are denoted BWald and BLM, respectively. To discriminate between the models at hand, the following procedure is carried out:

- Step 1: For each country, i obtain the SSR of the linear VAR. Also estimate a TVAR and SBVAR using the HML estimator described in subsection 2.3.3. For both non-linear alternatives, compute BWald (BLM). If, in both cases, the alternative hypothesis is not rejected, the procedure concludes, and the linear VAR specification is chosen. If at least one of the tests suggests rejecting the linear model, the decision rule moves on to the next step.

- Step 2: If BWald (BLM) with TVAR [SBVAR] under the alternative is larger than with SBVAR [TVAR], this step verifies if the inclusion of a break [threshold] improves the model using two alternative models, the SBTVAR and SBTVARc. First, BWald (BLM) is calculated with the TVAR [SBVAR] as the restricted and SBTVARc and SBTVAR as the unrestricted models. If both BWald (BLM) statistics are smaller than one, TVAR [SBVAR] is chosen. If the statistic with SBTVARc under the alternative is larger (smaller) than that of SBTVAR, the former (latter) is preferred.

2.C Results Monte Carlo simulations

Table 2.8: Monte Carlo evidence homogeneous panel VAR simulation with cross-equation error structure (DGP_1): Bias (x100).

T	N=6				N = 12				N = 21				N = 30			
	OLS	SUR	ARDL	PVAR	OLS	SUR	ARDL	PVAR	OLS	SUR	ARDL	PVAR	OLS	SUR	ARDL	PVAR
Bias (x100) for $\delta_{i1}^{(0)} = 0.0$																
20	-0.42	0.09	0.14	70.02	1.62	0.55	1.53	-1.10								
30	0.47	-0.31	-3.34	-285.43	-0.23	-0.22	1.29	0.60	-1.03	-0.53	-5.24	-1.32				
50	0.62	0.16	-1.16	-529.58	1.93	0.30	0.22	2.05	0.38	0.03	0.70	-1.42	-0.06	0.22	0.09	-0.70
80	0.41	0.26	-1.11	202.34	0.04	0.03	0.10	-0.13	1.09	0.25	2.16	0.32	-1.05	-0.22	-0.43	-1.20
150	1.00	-0.07	0.57	235.28	0.78	0.05	-0.41	0.17	-0.24	0.00	-0.55	-0.72	0.23	0.00	0.40	-0.25
Bias (x100) for $\delta_{i1}^{(0)} = 0.2$																
20	0.97	0.44	5.22	-369.84	2.23	0.89	2.54	-4.60								
30	-0.55	0.61	1.72	-263.31	-1.64	-0.20	6.71	-5.73	1.31	0.76	-1.83	-3.70				
50	1.90	0.50	2.25	-164.04	1.95	0.45	2.16	-3.66	-0.12	-0.03	0.74	-4.27	2.00	0.86	1.78	-2.15
80	-0.37	-0.09	-0.38	-254.06	1.96	0.23	1.59	-2.88	-0.43	-0.06	3.33	-4.28	1.13	0.12	2.02	-3.81
150	-0.20	0.04	1.07	106.69	1.21	0.06	0.53	-3.50	0.93	0.09	1.67	-3.92	0.54	0.16	1.06	-4.03
Bias (x100) for $\delta_{i1}^{(0)} = 0.4$																
20	2.24	0.29	10.22	-666.23	0.92	-0.23	1.73	-10.83								
30	2.73	1.05	5.05	538.41	0.52	0.02	3.70	-8.99	2.47	1.45	8.22	-6.65				
50	4.86	0.89	2.23	-253.63	2.72	0.60	4.38	-6.06	3.98	0.92	3.81	-7.77	2.11	0.72	4.16	-8.04
80	1.85	0.38	2.96	-271.41	1.90	0.21	2.27	-6.68	-0.12	-0.04	0.51	-8.32	2.52	0.28	2.67	-7.17
150	1.45	0.33	1.60	-335.99	0.85	0.05	0.87	-6.80	0.52	0.02	0.80	-7.62	-0.19	0.02	0.06	-7.87
Bias (x100) for $\delta_{i1}^{(0)} = 0.8$																
20	0.91	0.29	13.41	220.85	0.87	0.14	10.60	-14.85								
30	-0.22	0.46	22.18	-70.24	5.30	1.60	5.38	-14.60	0.96	0.45	6.28	-20.70				
50	1.40	0.39	2.27	-26.13	2.60	0.52	4.50	-14.90	3.15	0.71	3.74	-12.28	2.35	0.75	3.17	-15.64
80	1.88	0.50	3.46	121.88	1.27	0.14	1.81	-16.39	2.09	0.31	3.01	-15.51	0.19	0.07	1.56	-16.76
150	1.31	0.19	1.89	227.01	0.94	0.05	2.02	-15.89	0.80	0.05	0.77	-16.11	1.03	-0.01	0.77	-15.35

Note: Results for estimated parameter based on R = 1000 repetitions. Results for columns 'OLS', 'SUR' and 'ARDL' based on `R` package `PSBTGC` (Amann, 2022); see Appendix 2.E. Results for column 'PVAR' based on `R` package `Panelvar` (Sigmund and Ferstl, 2017).

Table 2.9: Monte Carlo evidence homogeneous panel VAR simulation with cross-equation error structure (DGP_1): RMSE (x100).

T	N=6				N = 12				N = 21				N = 30			
	OLS	SUR	ARDL	PVAR	OLS	SUR	ARDL	PVAR	OLS	SUR	ARDL	PVAR	OLS	SUR	ARDL	PVAR
RMSE (x100) for $\delta_{i1}^{(0)} = 0.0$																
20	13.68	3.86	25.33	814.11	13.81	6.25	25.55	13.68								
30	11.04	2.72	15.96	1154.65	11.86	3.50	16.57	10.36	9.41	5.15	24.45	9.97				
50	7.80	1.29	7.91	1249.52	7.58	1.29	8.75	7.90	6.38	1.75	12.34	7.45	8.38	2.78	8.49	7.39
80	6.70	1.15	7.31	1040.94	5.36	0.55	5.39	6.67	6.00	0.88	7.92	6.23	6.00	1.05	7.51	5.60
150	3.95	0.45	4.61	1168.05	4.80	0.28	4.46	4.84	4.09	0.34	4.85	3.85	4.81	0.42	4.76	4.51
RMSE (x100) for $\delta_{i1}^{(0)} = 0.2$																
20	12.18	3.70	18.80	974.97	9.82	4.57	20.05	13.10								
30	12.54	3.07	16.14	1391.40	11.51	3.13	13.60	10.79	8.05	4.31	23.62	9.16				
50	8.84	1.47	7.75	1182.14	8.29	1.37	7.84	8.33	9.16	2.28	12.92	9.83	7.31	2.78	11.48	8.49
80	6.18	0.88	7.01	1558.48	6.13	0.61	5.75	6.57	6.18	0.85	9.41	8.00	6.55	1.34	7.32	7.37
150	4.44	0.41	4.10	1334.45	4.58	0.24	3.79	5.63	4.45	0.32	4.79	6.18	3.79	0.59	4.83	5.70
RMSE (x100) for $\delta_{i1}^{(0)} = 0.4$																
20	10.50	3.71	25.73	1244.11	10.38	4.64	25.97	17.41								
30	12.28	3.16	15.79	1614.87	13.19	3.77	16.13	14.19	9.05	4.80	23.41	14.21				
50	9.23	1.81	7.76	1519.40	10.70	1.78	10.54	11.61	10.55	2.72	10.61	12.85	8.64	3.25	10.30	11.93
80	5.74	0.88	7.15	1535.95	6.98	0.76	7.59	10.29	7.02	0.98	6.97	10.74	6.48	1.39	8.37	9.66
150	4.99	0.58	4.65	1284.21	4.55	0.27	4.30	8.04	4.11	0.27	4.18	8.44	4.27	0.49	5.21	8.95
RMSE (x100) for $\delta_{i1}^{(0)} = 0.8$																
20	13.70	3.61	27.43	1022.40	10.28	4.41	22.91	26.29								
30	13.24	2.90	32.96	986.43	12.45	3.43	12.24	24.00	7.94	4.05	21.98	25.68				
50	10.61	1.78	9.43	1334.58	9.24	1.67	10.07	19.88	9.86	2.43	14.64	18.31	8.52	3.26	10.91	20.57
80	7.89	1.14	7.15	1763.54	6.73	0.70	6.56	18.75	7.97	1.17	8.25	17.76	6.52	1.26	8.30	18.53
150	4.79	0.58	5.09	2276.04	5.46	0.29	5.27	17.21	5.07	0.36	6.20	16.70	5.36	0.60	5.63	15.55

Note: Results for estimated parameter based on R = 1000 repetitions. Results for columns 'OLS', 'SUR' and 'ARDL' based on `R` package `PSBTGC` (Amann, 2022); see Appendix 2.E. Results for column 'PVAR' based on `R` package `Panelvar` (Sigmund and Ferstl, 2017).

Table 2.10: Monte Carlo evidence homogeneous panel VAR simulation with common correlated effects error structure (DGP_2): Bias (x100).

T	N=6				N = 12				N = 21				N = 30			
	OLS	SUR	ARDL	PVAR	OLS	SUR	ARDL	PVAR	OLS	SUR	ARDL	PVAR	OLS	SUR	ARDL	PVAR
Bias (x100) for $\delta_{i1}^{(0)} = 0.0$																
20	-0.17	-0.07	1.05	-433.92	0.34	-0.10	0.07	-12.27								
30	0.54	-0.23	-1.94	94.49	0.66	0.24	-0.40	-12.82	-1.08	-1.03	-0.94	0.90				
50	-0.31	-0.35	-0.55	246.17	-0.85	-0.23	0.14	-20.54	-0.50	-0.39	0.00	-5.85	0.31	0.31	0.59	1.96
80	0.18	0.20	0.61	-400.12	0.45	0.35	0.19	-41.50	-0.35	-0.34	-0.36	-7.70	0.05	-0.05	0.16	-2.26
150	-0.18	-0.25	-0.02	82.47	-0.17	-0.23	0.00	-73.43	0.10	0.20	0.18	-19.44	-0.15	-0.22	-0.22	-4.38
Bias (x100) for $\delta_{i1}^{(0)} = 0.2$																
20	-0.12	-0.35	-0.65	-310.44	1.93	-0.94	0.63	-4.13								
30	1.35	-0.32	1.46	380.54	1.42	-0.52	0.50	-9.95	1.52	-0.16	0.10	-3.06				
50	0.46	0.92	0.58	-93.59	-1.00	-0.36	-0.12	-34.91	-0.39	-0.32	0.29	-3.31	-0.62	-0.46	0.39	-5.33
80	-0.70	1.83	-0.49	89.88	-1.24	1.27	-0.40	-37.39	-1.01	-0.30	0.60	-8.82	-0.68	-0.38	0.08	-5.53
150	-1.00	0.27	0.28	288.99	-1.80	1.88	-0.25	-55.00	-0.95	1.50	0.09	-22.71	-1.47	-0.34	0.07	-11.51
Bias (x100) for $\delta_{i1}^{(0)} = 0.4$																
20	-0.87	-1.99	-0.46	-156.90	-0.79	-1.33	4.71	-16.55								
30	-0.90	-0.91	1.31	-316.37	-1.28	-1.00	1.48	-26.84	-0.01	-0.96	2.15	-8.40				
50	-2.20	-0.76	0.52	-383.25	-1.36	-0.80	0.33	-22.18	-1.47	-1.09	0.96	-13.32	-2.06	-1.18	1.08	-8.18
80	-2.95	0.17	1.07	209.59	-3.66	-0.19	0.78	-35.32	-2.91	-0.04	0.21	-14.40	-2.74	-1.08	0.45	-13.05
150	-4.13	-0.14	-0.09	437.81	-4.00	0.01	0.01	-59.09	-3.78	0.32	0.13	-30.10	-3.89	-0.96	0.12	-17.49
Bias (x100) for $\delta_{i1}^{(0)} = 0.8$																
20	-2.21	-3.71	4.70	-215.92	-0.48	-2.19	8.26	-20.27								
30	-1.79	-2.13	6.06	334.00	-2.21	-1.51	7.05	-21.21	-1.30	-2.15	6.09	-17.84				
50	-5.33	-1.68	3.93	-142.98	-3.55	-0.38	3.84	-22.22	-4.66	-1.72	3.97	-27.33	-3.65	-2.03	3.86	-22.87
80	-5.75	0.26	2.30	-589.63	-6.43	0.49	2.32	-25.65	-6.66	-1.06	2.31	-30.71	-6.02	-1.76	2.42	-26.32
150	-8.03	-0.03	0.53	-868.69	-7.35	0.71	0.84	-76.20	-7.86	0.16	0.42	-43.83	-7.67	-1.39	0.52	-27.86

Note: Results for estimated parameter based on R = 1000 repetitions. Results for columns 'OLS', 'SUR' and 'ARDL' based on `R` package `PSBTGC` (Amann, 2022); see Appendix 2.E. Results for column 'PVAR' based on `R` package `Panelvar` (Sigmund and Ferstl, 2017).

Table 2.11: Monte Carlo evidence homogeneous panel VAR simulation with common correlated effects error structure (DGP_2): RMSE (x100).

T	N=6				N = 12				N = 21				N = 30			
	OLS	SUR	ARDL	PVAR	OLS	SUR	ARDL	PVAR	OLS	SUR	ARDL	PVAR	OLS	SUR	ARDL	PVAR
RMSE (x100) for $\delta_{i1}^{(0)} = 0.0$																
20	7.15	6.20	11.49	1115.21	8.06	6.58	7.92	34.93								
30	6.22	5.82	8.21	1167.14	6.59	5.33	6.69	37.66	6.96	5.54	4.54	14.99				
50	5.51	4.83	6.36	1085.00	4.51	3.62	3.57	48.29	4.11	2.78	2.94	14.72	4.96	3.64	2.42	10.07
80	4.56	3.76	4.43	1120.17	4.02	2.84	2.53	64.30	3.96	2.43	2.13	16.54	3.79	2.51	2.11	12.28
150	3.40	2.74	2.94	1132.99	2.96	2.10	2.28	110.27	2.71	1.70	1.55	28.89	2.58	1.48	1.29	13.62
RMSE (x100) for $\delta_{i1}^{(0)} = 0.2$																
20	6.23	2.41	10.82	804.75	6.37	2.28	8.69	39.92								
30	5.95	3.40	8.27	1184.93	5.27	3.02	5.69	52.48	4.48	1.75	4.97	12.94				
50	4.50	3.70	5.66	1198.13	4.52	3.60	4.53	70.77	4.89	2.42	2.65	16.62	4.91	1.37	2.15	12.26
80	4.34	4.48	5.02	911.87	4.50	4.57	3.32	73.15	3.78	3.33	2.38	17.12	3.91	2.10	1.97	12.68
150	2.93	4.59	2.87	1440.49	3.28	4.96	2.20	97.65	3.07	4.06	1.50	28.49	2.77	3.50	1.27	15.09
RMSE (x100) for $\delta_{i1}^{(0)} = 0.4$																
20	6.43	3.13	13.18	788.99	5.77	2.08	8.24	41.35								
30	5.77	3.79	8.85	949.09	6.52	2.79	6.23	48.39	5.24	1.76	4.32	16.82				
50	5.44	3.72	5.27	1195.89	4.89	3.64	4.04	54.15	5.09	2.60	2.85	22.18	4.60	1.61	2.68	13.11
80	5.35	4.31	4.43	1406.47	5.16	3.99	2.74	78.69	4.87	3.10	2.09	22.71	4.27	2.26	1.99	15.98
150	4.83	4.65	3.19	927.35	4.35	5.08	2.10	90.00	4.49	4.17	1.71	33.16	4.23	3.15	1.21	20.59
RMSE (x100) for $\delta_{i1}^{(0)} = 0.8$																
20	7.01	4.03	12.53	906.06	5.54	2.44	11.56	39.38								
30	5.32	3.54	8.90	996.42	5.57	2.90	8.34	49.03	5.09	2.38	7.23	21.75				
50	6.92	4.23	5.40	1281.39	5.85	3.34	4.99	68.34	6.48	2.75	4.34	30.12	5.16	2.10	4.00	23.60
80	6.33	4.05	3.61	1755.97	6.82	4.29	3.61	74.78	7.03	3.43	2.82	33.84	6.30	2.50	2.65	27.00
150	8.18	4.92	2.53	1924.47	7.45	4.76	1.73	98.24	7.94	4.01	1.35	47.39	7.71	3.14	1.11	27.95

Note: Results for estimated parameter based on R = 1000 repetitions. Results for columns 'OLS', 'SUR' and 'ARDL' based on `R` package `PSBTGC` (Amann, 2022); see Appendix 2.E. Results for column 'PVAR' based on `R` package `Panelvar` (Sigmund and Ferstl, 2017).

Table 2.12: Monte Carlo evidence heterogeneous panel VAR simulation with cross-equation error structure (DGP_3): Bias (x100).

T	N=6				N = 12				N = 21				N = 30			
	OLS	SUR	VAR	PVAR	OLS	SUR	VAR	PVAR	OLS	SUR	VAR	PVAR	OLS	SUR	VAR	PVAR
Bias (x100) for $\delta_{i1}^{(b)} = 0.0$																
20	0.74	1.31	1.37	-38.94	-0.73	-0.72	-0.60	0.53								
30	-1.45	-0.12	-0.70	-53.33	0.30	-0.51	-0.24	1.79	-1.52	-1.17	0.19	1.39	0.04	-0.01	0.61	1.15
50	-0.88	-1.20	-1.34	-29.18	-0.15	-0.48	0.59	-0.59	2.22	0.90	1.61	1.41	-0.77	-0.34	-0.19	-0.28
80	0.33	-0.20	-0.57	-44.18	-0.81	-0.13	-0.97	-0.01	-0.33	-0.21	0.58	-0.66	-0.17	-0.08	-0.59	-0.06
150	1.27	0.97	0.83	7.60	0.66	-0.05	0.43	-1.22	-0.02	-0.02	0.03	-0.36	-0.36	0.02	-0.06	-0.70
Bias (x100) for $\delta_{i1}^{(b)} = 0.2$																
20	-0.99	-0.59	-5.85	22.96	-1.05	-1.08	-6.25	-11.68								
30	-0.50	-0.07	-5.08	-13.52	0.23	-0.31	-5.82	-13.06	-1.83	-0.77	-6.72	-11.83	-2.22	-0.90	-6.94	-12.14
50	-0.37	-0.16	-4.95	-22.87	-0.33	-0.48	-3.97	-9.09	0.11	-0.26	-6.58	-14.79	-1.49	-0.79	-6.58	-14.42
80	-1.99	-0.19	-4.90	-164.05	-0.10	-0.14	-3.89	-4.13	-0.78	-0.26	-5.63	-11.05	-0.17	-0.31	-5.27	-10.52
150	0.53	0.24	-2.20	-76.81	-1.88	0.07	-4.36	-5.77	0.34	-0.12	-4.11	-5.01	1.09	0.46	-3.14	-6.10
Bias (x100) for $\delta_{i1}^{(b)} = 0.4$																
20	-1.51	-1.24	-14.34	-50.21	-2.02	-1.09	-13.90	-23.07								
30	-0.76	-0.76	-11.80	-92.06	-0.10	-0.82	-12.34	-27.53	-2.04	-1.27	-13.82	-23.04	-2.04	-0.98	-13.71	-22.48
50	-0.63	0.03	-11.41	-33.56	-0.88	-0.54	-12.06	-19.91	-1.40	-0.74	-13.14	-29.20	-1.28	-0.93	-12.75	-29.86
80	0.96	0.45	-8.94	-54.62	0.23	-0.36	-9.22	-16.75	-1.30	-0.39	-11.65	-20.34	0.46	0.02	-10.79	-20.64
150	1.01	0.32	-6.96	11.22	-0.25	-0.25	-7.61	-9.92	-0.03	-0.30	-9.84	-15.41	-0.05	-0.21	-8.53	-12.31
Bias (x100) for $\delta_{i1}^{(b)} = 0.8$																
20	-2.05	-1.83	-28.63	-54.54	-3.55	-1.77	-29.34	-45.33								
30	-1.36	-0.69	-26.47	-30.74	-1.80	-1.30	-25.87	-59.56	-3.39	-1.69	-29.79	-45.78	-4.33	-1.71	-29.89	-46.26
50	-0.11	-0.04	-22.32	-110.02	-0.09	-0.39	-24.01	-44.57	-1.31	-1.26	-26.14	-60.99	-1.70	-1.07	-25.92	-61.56
80	0.20	-0.56	-19.46	-52.97	0.51	-0.15	-21.11	-32.63	-0.52	-0.72	-22.78	-43.93	0.11	-0.46	-22.35	-43.63
150	1.27	-0.30	-18.22	-100.26	0.38	0.30	-12.93	-22.02	0.73	-0.18	-19.83	-32.71	-0.18	-0.29	-20.46	-29.44

Note: Results for estimated parameter based on R = 1000 repetitions. Results for columns 'OLS', 'SUR' and 'VAR' based on `R` package `PSBTGC` (Amann, 2022); see Appendix 2.E. Results for column 'PVAR' based on `R` package `Panelvar` (Sigmund and Ferstl, 2017).

Table 2.13: Monte Carlo evidence heterogeneous panel VAR simulation with cross-equation error structure (DGP_3): RMSE (x100).

T	N=6				N = 12				N = 21				N = 30			
	OLS	SUR	VAR	PVAR	OLS	SUR	VAR	PVAR	OLS	SUR	VAR	PVAR	OLS	SUR	VAR	PVAR
RMSE (x100) for $\delta_{i1}^{(b)} = 0.0$																
20	12.04	7.70	8.49	313.58	10.04	6.46	7.28	7.70								
30	9.55	6.40	7.87	279.58	10.17	5.07	7.78	6.91	7.68	5.71	7.28	6.97	8.13	6.12	6.49	5.71
50	7.35	4.97	6.07	167.78	7.52	3.46	5.33	4.69	8.09	3.89	5.57	4.24	7.36	3.75	5.59	4.12
80	5.64	3.52	3.67	241.87	5.68	2.17	4.40	3.58	5.42	1.61	4.31	3.66	6.31	2.01	5.08	3.00
150	4.86	3.17	3.48	128.70	4.39	1.33	3.14	2.92	3.39	0.76	2.78	2.27	3.65	0.86	2.69	2.49
RMSE (x100) for $\delta_{i1}^{(b)} = 0.2$																
20	3.74	2.06	6.19	153.68	4.88	1.46	6.68	11.70								
30	4.44	2.25	5.99	171.68	5.44	1.92	6.68	13.14	4.47	1.11	6.75	11.83	4.38	1.21	6.99	12.14
50	6.27	2.52	7.14	207.80	6.68	2.55	6.30	10.19	5.80	1.86	7.03	14.82	5.28	1.83	7.00	14.42
80	5.94	2.30	7.07	292.25	6.40	2.42	7.63	9.29	5.32	2.16	6.14	11.27	5.45	2.47	6.62	10.65
150	7.25	2.65	8.87	250.73	6.31	2.58	9.59	10.42	4.38	2.48	6.89	9.50	3.49	2.33	6.35	10.35
RMSE (x100) for $\delta_{i1}^{(b)} = 0.4$																
20	3.56	2.12	14.34	134.03	4.11	1.51	13.90	23.07								
30	4.97	2.58	12.03	161.28	6.01	2.03	12.35	27.53	4.66	1.48	13.82	23.04	5.16	1.36	13.71	22.48
50	4.82	2.34	11.43	206.61	6.14	2.19	12.38	19.96	6.01	1.95	13.15	29.20	5.73	1.98	12.75	29.86
80	5.73	2.37	10.74	294.22	5.75	2.54	10.05	18.04	5.57	2.30	11.79	20.63	5.10	2.10	10.96	20.91
150	6.67	2.59	9.64	246.96	6.45	2.49	10.25	13.33	4.33	2.29	10.81	18.90	3.78	2.25	9.71	16.62
RMSE (x100) for $\delta_{i1}^{(b)} = 0.8$																
20	4.81	2.80	28.63	122.36	5.31	2.29	29.34	45.33								
30	4.77	2.15	26.47	121.12	4.71	2.18	25.87	59.56	5.53	2.02	29.79	45.78	6.15	2.02	29.89	46.26
50	5.97	2.41	22.32	210.23	5.54	2.11	24.01	44.57	5.68	2.35	26.14	60.99	5.49	2.04	25.92	61.56
80	5.35	2.34	20.07	222.54	6.37	2.08	21.46	42.04	4.69	2.26	22.78	43.93	4.21	2.01	22.38	44.59
150	7.48	2.62	19.88	229.48	7.20	2.37	16.75	23.38	4.19	2.54	20.25	42.30	4.21	2.25	20.80	37.45

Note: Results for estimated parameter based on R = 1000 repetitions. Results for columns 'OLS', 'SUR' and 'VAR' based on `R` package `PSBTGC` (Amann, 2022); see Appendix 2.E. Results for column 'PVAR' based on `R` package `Panelvar` (Sigmund and Ferstl, 2017).

Table 2.14: Monte Carlo evidence heterogeneous panel VAR simulation with common correlate error structure (DGP_4): Bias (x100).

T	N=6				N = 12				N = 21				N = 30			
	OLS	SUR	VAR	PVAR	OLS	SUR	VAR	PVAR	OLS	SUR	VAR	PVAR	OLS	SUR	VAR	PVAR
Bias (x100) for $\delta_{i1}^{(b)} = 0.0$																
20	0.83	0.80	0.86	-40.01	-0.43	-0.51	-0.55	-0.94								
30	-0.46	-0.62	-0.27	-39.92	0.14	0.30	-0.17	1.53	-0.15	-0.21	-0.52	-0.84				
50	0.00	0.09	-0.01	41.77	0.48	0.31	0.25	-1.14	0.12	0.25	0.11	-0.53	-0.18	-0.15	-0.19	-1.08
80	-0.20	-0.17	-0.28	-39.58	0.12	0.08	0.16	-0.59	0.02	0.05	-0.09	-0.80	-0.26	-0.17	-0.22	-0.93
150	0.25	0.25	0.13	-5.47	-0.28	-0.19	-0.24	-0.88	-0.12	-0.08	-0.13	-0.69	0.06	-0.04	0.11	-0.92
Bias (x100) for $\delta_{i1}^{(b)} = 0.2$																
20	-1.74	-1.23	-7.07	-13.59	-2.37	-1.40	-7.64	-13.34								
30	-0.67	-0.24	-6.02	14.55	-1.55	-0.50	-7.07	-14.53	-2.50	-1.16	-7.80	-13.45				
50	-0.47	0.01	-5.49	-104.48	-0.99	-0.50	-6.05	-9.99	-1.67	-0.57	-7.04	-14.64	-2.41	-1.01	-7.59	-13.15
80	0.16	0.40	-5.01	-55.24	-0.44	-0.05	-5.32	-8.51	-1.17	-0.31	-6.55	-10.92	-2.02	-0.94	-7.19	-15.64
150	0.33	0.30	-4.30	61.79	0.14	0.25	-4.38	-10.32	-0.61	-0.11	-5.91	-7.96	-1.20	-0.42	-6.44	-11.05
Bias (x100) for $\delta_{i1}^{(b)} = 0.4$																
20	-2.90	-2.09	-15.78	-45.42	-3.88	-2.01	-16.17	-26.53								
30	-2.00	-1.04	-14.44	-30.59	-2.85	-1.26	-15.03	-29.17	-4.57	-2.05	-16.58	-26.15				
50	-1.19	-0.37	-13.09	15.89	-1.61	-0.43	-13.29	-23.41	-3.07	-1.15	-14.93	-29.49	-4.63	-1.85	-16.54	-26.76
80	-0.58	-0.02	-11.51	-106.64	-0.98	-0.23	-12.19	-20.63	-2.11	-0.39	-13.50	-22.83	-3.38	-1.28	-15.20	-30.62
150	0.14	0.11	-8.18	88.70	0.25	0.38	-8.91	-23.35	-0.84	-0.06	-11.72	-17.85	-2.26	-0.80	-13.34	-23.34
Bias (x100) for $\delta_{i1}^{(b)} = 0.8$																
20	-3.57	-2.28	-33.50	-115.05	-5.36	-2.50	-33.72	-57.51								
30	-2.68	-1.42	-30.77	-103.60	-4.13	-1.51	-30.86	-65.42	-6.54	-2.53	-34.65	-59.11				
50	-1.84	-0.60	-27.28	-92.13	-2.83	-0.63	-27.37	-48.82	-5.02	-1.59	-31.67	-65.84	-7.47	-2.73	-35.44	-58.84
80	-1.38	-0.42	-23.01	-100.21	-1.68	0.04	-24.02	-47.20	-3.31	-0.98	-28.02	-50.31	-5.26	-1.76	-31.77	-66.99
150	-0.06	0.15	-17.74	-1.65	-0.75	-0.13	-18.10	-46.55	-1.06	-0.27	-23.27	-42.32	-3.36	-1.20	-27.87	-50.69

Note: Results for estimated parameter based on R = 1000 repetitions. Results for columns 'OLS', 'SUR' and 'VAR' based on `R` package `PSBTGC` (Amann, 2022); see Appendix 2.E. Results for column 'PVAR' based on `R` package `Panelvar` (Sigmund and Ferstl, 2017).

Table 2.15: Monte Carlo evidence heterogeneous panel VAR simulation with common correlated effects error structure (DGP_4): RMSE (x100).

T	N=6				N = 12				N = 21				N = 30			
	OLS	SUR	VAR	PVAR	OLS	SUR	VAR	PVAR	OLS	SUR	VAR	PVAR	OLS	SUR	VAR	PVAR
RMSE (x100) for $\delta_{i1}^{(b)} = 0.0$																
20	6.91	6.85	5.88	313.60	6.35	6.10	4.83	9.20								
30	6.12	6.01	5.46	370.66	5.81	5.48	4.59	8.07	4.77	4.72	3.74	5.49				
50	4.39	4.36	3.91	313.09	4.08	3.78	3.42	5.85	3.31	3.12	2.76	3.22	3.39	3.06	2.60	3.55
80	3.71	3.61	3.24	178.42	3.15	2.89	2.54	4.39	2.48	2.19	1.99	2.78	2.45	1.97	1.97	2.63
150	2.52	2.42	2.11	152.35	2.00	1.83	1.62	3.10	1.63	1.43	1.39	1.99	1.69	1.43	1.42	1.91
RMSE (x100) for $\delta_{i1}^{(b)} = 0.2$																
20	2.61	2.28	7.07	239.82	2.74	2.06	7.64	13.39								
30	2.40	2.14	6.09	218.42	2.76	2.20	7.08	14.70	2.83	1.75	7.80	13.45				
50	2.77	2.44	5.80	271.88	2.80	2.19	6.18	10.90	2.74	2.02	7.04	14.64	2.71	1.60	7.59	13.15
80	3.21	2.38	6.12	392.07	3.08	2.32	6.08	11.32	2.98	2.23	6.61	10.95	2.85	1.99	7.20	15.64
150	4.25	2.48	7.46	388.68	3.74	2.40	5.92	13.20	2.85	2.27	6.14	9.15	2.65	2.07	6.49	11.09
RMSE (x100) for $\delta_{i1}^{(b)} = 0.4$																
20	3.25	2.64	15.78	142.33	4.05	2.42	16.17	26.53								
30	2.81	2.38	14.44	223.80	3.40	2.18	15.03	29.17	4.59	2.26	16.58	26.15				
50	2.89	2.50	13.12	254.56	3.11	2.21	13.29	23.51	3.56	2.11	14.93	29.49	4.66	2.08	16.54	26.76
80	3.16	2.33	11.83	387.20	3.23	2.37	12.26	21.47	3.20	2.21	13.50	22.83	3.74	2.08	15.20	30.62
150	3.82	2.25	10.24	356.54	3.88	2.45	10.16	24.50	2.91	2.29	11.75	18.41	3.33	2.22	13.34	23.34
RMSE (x100) for $\delta_{i1}^{(b)} = 0.8$																
20	3.85	2.73	33.50	189.38	5.44	2.75	33.72	57.51								
30	3.40	2.54	30.77	334.44	4.60	2.48	30.86	65.42	6.55	2.69	34.65	59.11				
50	3.32	2.37	27.28	300.32	3.69	2.40	27.37	48.82	5.20	2.26	31.67	65.84	7.47	2.82	35.44	58.84
80	3.43	2.54	23.20	296.18	3.41	2.43	24.02	48.16	3.90	2.35	28.02	50.31	5.42	2.37	31.77	66.99
150	4.05	2.33	20.05	384.57	3.45	2.54	19.31	47.22	2.87	2.27	23.27	42.55	3.91	2.32	27.87	50.69

Note: Results for estimated parameter based on R = 1000 repetitions. Results for columns 'OLS', 'SUR' and 'VAR' based on R package `PSBTGC` (Amann, 2022); see Appendix 2.E. Results for column 'PVAR' based on R package `Panelvar` (Sigmund and Ferstl, 2017).

2.D Results of empirical analysis

Table 2.16: Threshold specification for (T)VAR(i)

Country	Specification	Thr. variable	Nobs. (high, low) thr.
Australia	VAR(1,1)		
Austria	VAR(3,1)		
Belgium	VAR(2,1)		
Canada	TVAR(2,2)	$x_{t-1} = 90$	(5,78)
Czech Republic	VAR(2,1)		
Denmark	VAR(2,1)		
Estonia	VAR(4,1)		
Finland	VAR(4,1)		
France	TVAR(2,2)	$x_{t-1} = 90$	(53,30)
Germany	VAR(2,1)		
Greece	VAR(1,1)		
Hungary	VAR(2,1)		
Ireland	TVAR(4,4)	$x_{t-1} = 90$	(51,19)
Italy	VAR(2,1)		
Japan	VAR(3,1)		
Latvia	VAR(4,4)		
Netherlands	VAR(2,1)		
Norway	VAR(2,1)		
Portugal	TVAR(3,4)	$x_{t-1} = 90$	(41,30)
Spain	TVAR(2,2)	$x_{t-1} = 90$	(39,34)
Sweden	VAR(4,1)		
United Kingdom	TVAR(2,3)	$x_{t-1} = 90$	(63,20)
United States	TVAR(2,2)	$x_{t-1} = 90$	(48,35)

Note: Results based on R package PSBTGC (Amann, 2022).

Source: Authors' calculation based on quarterly OECD data; see Appendix 2.A.

Table 2.17: Optimal lag length selection based on SIC

Country	GDP growth	Debt-to-GDP ratio
Australia	1	1
Austria	4	1
Belgium	2	2
Canada	2	2
Denmark	2	1
Finland	4	1
France	2	2
Germany	2	1
Greece	4	1
Ireland	1	3
Italy	2	1
Japan	3	2
Netherlands	3	1
Norway	4	1
Portugal	3	4
Spain	2	2
Sweden	4	1
United Kingdom	2	3
United States	2	3

Note: Lag length selection based on the Schwarz/Bayesian information criterion using the R package `vars` and include a quarterly dummy as well as a deterministic trend and constant for the debt-to-GDP ratio and the GDP growth rate respectively. Results based on `R` package `PSBTGC` (Amann, 2022).
Source: Authors' calculation based on quarterly OECD data; see Appendix 2.A.

Table 2.18: Wald test statistics linear model

	Debt -> Growth												Growth -> Debt											
Country	VAR(1)			VAR(2)			VAR(4)			VAR(i)			VAR(1)			VAR(2)			VAR(4)			VAR(i)		
Australia	7.93	(-)	*	15.19	(-)	**	13.98	(-)		8.57	(-)	**	0.57	(+)		1.00	(-)		8.00	(-)		0.78	(+)	
Austria	0.65	(+)		12.96	(+)	*	9.61	(-)		0.00	(-)		4.62	(-)		11.49	(-)	*	31.96	(-)	**	13.07	(-)	
Belgium	0.02	(-)		0.41	(+)		1.43	(-)		0.04	(+)		3.89	(-)		27.22	(-)	***	84.71	(-)	***	6.79	(-)	
Canada	3.82	(+)		4.00	(+)		8.73	(+)		6.54	(+)		38.13	(-)	***	34.20	(-)	***	45.02	(-)	***	27.72	(-)	***
Denmark	10.01	(+)	*	3.41	(+)		21.36	(+)		4.16	(+)		13.71	(-)	**	15.57	(-)	**	12.40	(-)		16.15	(-)	**
Finland	1.41	(+)		5.79	(+)		5.11	(-)		0.91	(-)		12.01	(-)	**	50.90	(-)	***	82.46	(-)	***	35.07	(-)	***
France	0.01	(+)		0.10	(+)		5.54	(+)		0.45	(+)		32.32	(-)	***	20.81	(-)	**	51.04	(-)	***	23.81	(-)	**
Germany	8.78	(+)		8.11	(+)		8.42	(+)		3.73	(+)		10.78	(-)	**	4.50	(-)		47.31	(-)	**	8.20	(-)	
Greece	0.68	(+)		1.89	(+)		6.80	(+)		0.70	(+)		25.72	(-)	***	39.45	(-)	***	38.48	(-)	***	36.53	(-)	**
Ireland	1.53	(+)		13.95	(+)		12.95	(+)		8.09	(+)		57.87	(-)	***	47.33	(-)	***	0.98	(-)		1.38	(-)	
Italy	1.38	(+)		1.61	(+)		2.99	(+)		1.51	(+)		13.56	(-)	**	23.18	(-)	***	91.30	(-)	***	20.47	(-)	**
Japan	1.15	(+)		1.69	(+)		5.48	(+)		1.03	(+)		3.48	(-)		3.30	(-)		16.21	(-)		4.61	(-)	
Netherlands	0.02	(+)		7.02	(+)		10.26	(-)		0.75	(-)		14.73	(-)	**	15.98	(-)	**	10.69	(-)		14.09	(-)	
Norway	3.76	(-)		2.77	(+)		1.70	(+)		1.21	(-)		0.01	(-)		0.25	(+)		11.67	(-)		13.23	(-)	
Portugal	0.01	(-)		41.69	(+)	***	42.64	(-)	***	37.54	(-)	**	26.70	(-)	***	23.48	(-)	***	17.18	(-)		16.16	(-)	
Spain	9.29	(+)	*	5.09	(+)		3.79	(+)		6.54	(+)		97.37	(-)	***	101.99	(-)	***	73.18	(-)	***	105.09	(-)	***
Sweden	0.80	(-)		4.86	(+)		7.04	(+)		1.53	(+)		13.09	(-)	**	16.24	(-)	**	15.58	(-)		21.07	(-)	*
United Kingdom	1.84	(+)		10.04	(+)		5.14	(-)		8.84	(-)		73.16	(-)	***	50.07	(-)	***	30.60	(-)	**	15.46	(-)	*
United States	0.55	(+)		0.29	(-)		2.89	(+)		0.80	(+)		28.24	(-)	***	18.78	(-)	**	22.73	(-)	*	14.41	(-)	*

Note: Test statistics are based on a VAR(1), VAR(2), as well as the model selection proposed by the SIC in Table 2.3 as well as country-level specification described in Table 2.17 and are denoted VAR(4) and VAR(i) respectively. All simulations based on 500 repetitions, T* = 3xT.
Sign of causal relationship reported in parenthesis. Significant results at the 1, 5 and 10 percent level are denoted ***, ** and * respectively.
Results based on `R` package `PSBTGC` (Amann, 2022).
Source: Authors' calculation based on quarterly OECD data; see Appendix 2.A.

Table 2.19: Wald test statistics for debt-threshold hypothesis (Reinhart and Rogoff, 2010)

	Debt -> Growth												Growth -> Debt											
Country	TVAR(1)			TVAR(2)			TVAR(4)			(T)VAR(i)			TVAR(1)			TVAR(2)			TVAR(4)			(T)VAR(i)		
Australia										11.01	(-)	**										0.29	(+)	
Austria										0.00	(+)											10.45	(-)	
Belgium										0.00	(+)											6.97	(-)	
Canada	4.11	(-)		55.97	(+)	***				18.26	(-)		54.73	(+)	***	77.58	(+)					48.20	(+)	***
Canada	4.11	(+)		55.97	(+)	***				18.26	(+)		54.73	(-)	***	77.58	(-)					48.20	(-)	***
Denmark										3.74	(+)											16.04	(-)	**
Finland										1.84	(-)											23.36	(-)	**
France	5.10	(+)		3.46	(+)		16.72	(+)		15.33	(+)		22.23	(-)		18.32	(-)					34.17	(-)	**
France	5.10	(+)		3.46	(+)		16.72	(+)		15.33	(+)		22.23	(-)		18.32	(-)					34.17	(-)	**
Germany										2.83	(+)											9.04	(-)	*
Greece										0.40	(+)											31.30	(-)	**
Ireland	0.27	(-)		2.07	(-)		21.69	(-)		17.45	(-)		66.46	(-)	***	51.74	(-)	***	14.05	(-)		7.50	(-)	
Ireland	0.27	(+)		2.07	(+)		21.69	(+)		17.45	(+)		66.46	(-)	***	51.74	(-)	***	14.05	(-)		7.50	(-)	
Italy										0.15	(+)											20.64	(-)	***
Japan										1.38	(+)											4.99	(-)	
Netherlands										1.06	(-)											13.59	(-)	
Norway										1.27	(-)											15.64	(-)	
Portugal	4.05	(+)		17.32	(+)	**	17.57	(+)		31.71	(+)		18.32	(-)	***	35.83	(-)	***	50.16	(-)	***	87.49	(-)	***
Portugal	4.05	(+)		17.32	(+)	**	17.57	(+)		31.71	(-)		18.32	(-)	***	35.83	(-)	***	50.16	(-)	***	87.49	(-)	***
Spain	4.45	(+)		1.65	(+)		2.81	(+)		11.14	(+)		66.74	(+)	***				31.29	(-)	**	120.50	(+)	***
Spain	4.45	(-)		1.65	(-)		2.81	(+)		11.14	(-)		66.74	(-)	***				31.29	(-)	**	120.50	(-)	***
Sweden										1.75	(+)											27.30	(-)	**
United Kingdom	0.20	(+)		14.84	(+)	*	11.71	(+)		11.98	(+)		52.84	(-)	***	25.20	(-)	**	12.27	(-)		16.01	(-)	
United Kingdom	0.20	(+)		14.84	(-)	*	11.71	(+)		11.98	(+)		52.84	(+)	***	25.20	(+)	**	12.27	(+)		16.01	(+)	
United States	0.41	(+)		4.83	(+)		13.51	(+)		1.24	(+)		22.46	(+)	*	19.35	(+)		15.24	(+)		11.53	(-)	
United States	0.41	(-)		4.83	(-)		13.51	(-)		1.24	(+)		22.46	(-)	*	19.35	(-)		15.24	(-)		11.53	(-)	

Note: Test statistics are based on a TVAR(1), TVAR(2), as well as the model selection proposed by the SIC in Table 2.3 as well as country-level specification described in Table 2.17 and are denoted TVAR(4) and (T)VAR(i) respectively. All simulations based on 500 repetitions, T* = 3xT. Results based on `R` package `PSBTGC` (Amann, 2022). Sign of causal relationship reported in parenthesis. Significant results at the 1, 5 and 10 percent level are denoted ***, ** and * respectively.

Table 2.20: Estimated instabilities.

	SBVAR	TVAR	SBTVAR			SBTVARc	
Country	Break	Th.	Break	Th.low	Th.high	Break	Th
Australia	2012 Q2	31.88	2010 Q4	30.48	45.97	2001 Q4	44.35
Austria	2011 Q2	68.26	2012 Q3	68.35	81.69	2007 Q3	73.09
Belgium	2008 Q4	101.24	2010 Q3	96.89	106.47	2004 Q3	111.91
Canada	2009 Q1	95.70	2010 Q3	92.14	105.77	2002 Q3	108.69
Denmark	2009 Q1	56.07	2011 Q1	38.56	45.27	2007 Q2	50.79
Finland	2012 Q2	41.26	2012 Q2	40.90	60.97	2005 Q2	46.98
France	2009 Q2	69.36	2002 Q3	71.39	108.66		
Germany	2012 Q3	78.01	2011 Q1	59.23	68.29	2006 Q3	66.71
Greece	2013 Q3	103.22	2006 Q4	106.58	178.09	2005 Q4	105.99
Ireland	2012 Q1	115.32	2011 Q1	29.46	78.93	2006 Q2	47.20
Italy	2013 Q2	103.24	2010 Q2	102.16	116.29	2002 Q2	115.07
Japan	2008 Q4	232.39	2010 Q4	113.85	219.57		
Netherlands	2010 Q2	72.74	2010 Q4	50.92	65.38	2004 Q2	67.06
Norway	2005 Q1	49.42	2011 Q1	5.15	33.78	2004 Q2	44.76
Portugal	2011 Q2	57.46	2005 Q4	65.64	140.36		
Spain	2013 Q1	103.66				2011 Q2	103.79
Sweden	2008 Q1	70.90	2011 Q1	50.56	50.07	2004 Q3	67.33
United Kingdom	2008 Q4	41.57	2010 Q1	66.84	67.33	2008 Q1	63.79
United States	2007 Q3	69.53	2009 Q2	73.01	74.99	2011 Q 3	65.88

Note: Estimated instabilities based on maximum likelihood estimation with regime-dependent variances (HML) according to the model specification in subsection 2.3.3. In case of missing values only insufficient observations were present in at least of one of the regimes. Results based on `R` package `PSBTGC` (Amann, 2022).
Source: Authors' calculation based on quarterly OECD data; see Appendix 2.A.

Table 2.21: Model selection under BWald.

	H0: VAR	
Country	Ha: SBVAR	Ha: TVAR
Australia	**1.1563**	1.1438
Austria	**1.2485**	1.2342
Belgium	**1.2006**	1.1998
Canada	0.9835	0.9673
Denmark	0.9071	0.9288
Finland	0.3672	0.9749
France	1.1102	**1.126**
Germany	0.055	0.3086
Greece	1.1827	**1.1914**
Ireland	0.5339	0.8791
Italy	**1.0268**	1.0138
Japan	0.9179	0.9199
Netherlands	1.0843	**1.104**
Norway	**1.1313**	1.1257
Portugal	0.9955	**1.1098**
Spain	1.1697	**1.1855**
Sweden	0.7002	0.7407
United Kingdom	0.8013	**1.0068**
United States	1.048	**1.0661**

Note: Selection of competing models identified subsection 2.3.3 Selectio rule: if BWald >1, in choose model under alternative (highlighted in bold). Results based on `R` package `PSBTGC` (Amann, 2022).
Source: Authors' calculation based on quarterly OECD data; see Appendix 2.A.

Table 2.22: Wald test statistics endogenous model selection.

			Debt -> Growth			Growth -> Debt		
Country	Model	Regime	Wald	Sign	Sig	Wald	Sign	Sig
Australia	SBVAR	above	2.66	(+)		4.99	(-)	
		below	1.97	(+)		6.30	(-)	
Austria	SBVAR	above	13.83	(+)		11.60	(-)	***
		below	12.79	(+)		17.20	(-)	***
Belgium	SBVAR	above	2.62	(+)		66.83	(-)	***
	SBVAR	below	5.61	(+)		51.68	(-)	***
Canada	VAR	linear	3.58	(+)		15.16	(-)	***
Denmark	VAR	linear	3.60	(+)		19.63	(-)	***
Finland	VAR	linear	2.25	(+)		29.11	(-)	***
France	TVAR	above	3.61	(+)		37.46	(-)	
		below	0.43	(-)		21.33	(-)	
Germany	VAR	linear	6.80	(+)		21.26	(-)	***
Greece	TVAR	above	12.39	(+)	**	26.38	(-)	
		below	16.82	(-)	**	21.58	(+)	
Ireland	VAR	linear	10.99	(+)		27.39	(-)	
Italy	SBVAR	above	2.96	(-)		44.57	(+)	***
		below	0.43	(+)		88.38	(-)	***
Japan	VAR	linear	2.35	(+)		3.56	(-)	
Netherlands	TVAR	above	14.35	(+)		11.32	(-)	
		below	8.43	(-)		5.62	(-)	
Norway	SBVAR	above	3.59	(-)		12.85	(+)	
		below	2.52	(-)		11.21	(+)	
Portugal	TVAR	above	25.01	(-)	**	30.72	(-)	***
		below	28.41	(-)	**	36.32	(-)	***
Spain	TVAR	above	5.42	(+)		97.12	(+)	***
		below	2.55	(+)		105.73	(-)	***
Sweden	VAR	linear	0.77	(+)		13.01	(-)	***
United Kingdom	TVAR	above	28.71	(-)	**	42.47	(-)	*
		below	31.32	(+)	**	37.44	(+)	*
United States	TVAR	above	5.45	(-)		26.03	(-)	***
		below	5.30	(+)		21.84	(-)	***

Note: Test statistics are based on a TVAR(1), TVAR(2), as well as the model selection proposed by the SIC in Table 2.3 as well as country-level specification described in Table 2.17 and are denoted TVAR(4) and (T)VAR(i) respectively. All simulations based on 500 repetitions, $T^* = 3 \times T$ for the hypothesis of no Granger causality from past debt-to-GDP ratios on GDP growth and vice versa. Sign of causal relationship reported in parenthesis. Significant results at the 1, 5 and 10 percent level are denoted ***, ** and * respectively. Results based on R package `PSBTGC` (Amann, 2022).
Source: Authors' calculation based on quarterly OECD data; see Appendix 2.A.

2.E The PSBTGC package

The method described in this piece is made available via the R package PSBTGC (Amann, 2022) and can be downloaded from the package web page. All simulations and empirical applications presented in this paper are integrated into the current version of the package. For more information and documentation on the routine, please see the package web page https://amannj.github.io/resources/PSBTGC/.
This section illustrates how the routines in PSBTGC relate to other available R packages.

2.E.1 The data

For expositional purposes, we resort to the Cecchetti[15] data set (Cecchetti et al., 2011) contained in the PSBTGC package, out of which we only extract data for the UK and the US.

```
# Load PSBTGC package and dependencies
library(PSBTGC)
# Transform data
dat <- Cecchetti %>%
  select(year, country, gr, gd) %>%
  filter(country %in% c('GB', 'US')) %>%
  group_by(country) %>%
  mutate(l1gr = lag.series(gr, l = 1),
         l1gd = lag.series(gd, l = 1)) %>%
  ungroup()
# Summary statistics
summary(dat[,-c(1,2)])
```

```
>>        gr                gd               l1gr              l1gd
>>  Min.   :-5.787   Min.   :0.4218   Min.   :-2.368   Min.   :0.4218
>>  1st Qu.: 0.898   1st Qu.:0.5376   1st Qu.: 1.447   1st Qu.:0.5372
>>  Median : 2.316   Median :0.5989   Median : 2.403   Median :0.5978
>>  Mean   : 1.732   Mean   :0.6080   Mean   : 1.980   Mean   :0.6001
>>  3rd Qu.: 3.046   3rd Qu.:0.6714   3rd Qu.: 3.046   3rd Qu.:0.6594
```

[15] See https://www.bis.org/publ/work352.htm.}

```
>>  Max.   : 6.421   Max.   :0.8816   Max.   : 6.421   Max.   :0.7927
>>                                    NA's   :2        NA's   :2
```

2.E.2 Equation-wise OLS

In its simplest form, function `PGranger_SUR()` with argument `method = 'OLS'` is a simple function that runs OLS equation by equation for all cross-sectional units:

```
SUR <- PGranger_SUR(dat,
                    dep.var = 'gr',
                    indep.var = c('l1gr', 'l1gd'),
                    id.var = 'country',
                    time.var = 'year',
                    method ='OLS')
SUR$Coefficient.estimates[,-7]
```

```
>>    i Variable Coefficient.Estimates Std..Error  t.statistic      p.value
>> 1 GB       gr             -0.0229971  3.1390480 -0.007326138 0.9942105638
>> 2 GB     l1gr              0.7977132  0.1967992  4.053437697 0.0004066411
>> 3 GB     l1gd              0.5633346  5.8626171  0.096089277 0.9241864902
>> 4 US       gr              2.2628053  2.9425766  0.768987722 0.4488302136
>> 5 US     l1gr              0.4434497  0.2096402  2.115289036 0.0441445752
>> 6 US     l1gd             -2.0555528  4.4827082 -0.458551553 0.6503663213
```

```
SUR$Estimation.statistics
```

```
>>    i R.squared adj..R.squared     RMSE              AIC              BIC
>> 1 GB 0.4061371     0.36045535 1.709536 444.653646467896 463.337025502555
>> 2 US 0.1483247     0.08281127 2.152368
```

We can obtain the same results by running R's standard OLS function `lm` for each country separately (shown only for the case of Great Britain to save space).

```
lmGB <- lm(gr ~ l1gr + l1gd, subset(dat, dat$country == 'GB'))
summary(lmGB)
```

```
>>
>> Call:
>> lm(formula = gr ~ l1gr + l1gd, data = subset(dat, dat$country ==
>>     "GB"))
>>
>> Residuals:
>>     Min      1Q  Median      3Q     Max
>> -5.6343 -0.4879  0.1146  1.0272  2.0801
>>
>> Coefficients:
>>             Estimate Std. Error t value Pr(>|t|)
>> (Intercept)  -0.0230     3.1391  -0.007 0.994211
>> l1gr          0.7977     0.1968   4.053 0.000407 ***
>> l1gd          0.5633     5.8626   0.096 0.924186
>> ---
>> Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
>>
>> Residual standard error: 1.71 on 26 degrees of freedom
>>   (1 observation deleted due to missingness)
>> Multiple R-squared:  0.4061, Adjusted R-squared:  0.3605
>> F-statistic: 8.891 on 2 and 26 DF,  p-value: 0.001143
```

Function `PGranger_SUR()` applies the same estimation equations to all cross-sectional units. This restriction is changed in `PGranger2_SUR()`, which estimates SUR that allows for different estimation equations across countries. We compare the results from `PGranger2_SUR()` with argument `method = 'OLS'` with R's standard OLS function `lm` for each country separately. Please note that in this case, `PGranger2_SUR()` requires argument `Equations`, which contains the estimation equation for each country and is allowed to vary across countries. At the time of writing, it is furthermore necessary to input an intercept term manually.

```
# Define data set including intercept term
lmGB <- lm(gr ~ l1gr + l1gd, subset(dat, dat$country == 'GB'))
dat.PG2 <- dat
dat.PG2$Intercept <- 1
# Define equations and run model
Equations <- list('GB' = as.formula(gr ~ Intercept + l1gr + l1gd),
                  'US' = as.formula(gr ~ Intercept + l1gr))
SUR <- PGranger2_SUR(dat.PG2,
```

```
                      Equations = Equations,
                      id.var = 'country',
                      time.var = 'year',
                      method ='OLS')
SUR$Coefficient.estimates
```

```
>>   id Coefficient  Estimates Std..Error      t.value p.value Sig.
>> 1 GB   Intercept -0.0229971  3.1390480 -0.007326138   0.994
>> 2 GB        l1gr  0.7977132  0.1967992  4.053437697   0.000  ***
>> 3 GB        l1gd  0.5633346  5.8626171  0.096089277   0.924
>> 4 US   Intercept  0.9377812  0.5477324  1.712115521   0.098    *
>> 5 US        l1gr  0.4330724  0.2053446  2.109003603   0.044   **
```

As before, the results are identical to running OLS on a subset of the data using individually specified regression equations.

```
summary(lm(gr ~ l1gr + l1gd, subset(dat, dat$country == 'GB')))
```

```
>>
>> Call:
>> lm(formula = gr ~ l1gr + l1gd, data = subset(dat, dat$country ==
>> "GB"))
>>
>> Residuals:
>> Min 1Q Median 3Q Max
>> -5.6343 -0.4879 0.1146 1.0272 2.0801
>>
>> Coefficients:
>> Estimate Std. Error t value Pr(>|t|)
>> (Intercept) -0.0230 3.1391 -0.007 0.994211
>> l1gr 0.7977 0.1968 4.053 0.000407 ***
>> l1gd 0.5633 5.8626 0.096 0.924186
>> ---
>> Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
>>
>> Residual standard error: 1.71 on 26 degrees of freedom
>> (1 observation deleted due to missingness)
>> Multiple R-squared: 0.4061, Adjusted R-squared: 0.3605
>> F-statistic: 8.891 on 2 and 26 DF, p-value: 0.001143
```

```
summary(lm(gr ~ l1gr , subset(dat, dat$country == 'US')))
```

```
>>
>> Call:
>> lm(formula = gr ~ l1gr, data = subset(dat, dat$country == "US"))
>>
>> Residuals:
>>     Min      1Q  Median      3Q     Max
>> -5.7126 -0.7860  0.2143  1.1686  4.0609
>>
>> Coefficients:
>>             Estimate Std. Error t value Pr(>|t|)
>> (Intercept)   0.9378     0.5477   1.712   0.0983 .
>> l1gr          0.4331     0.2053   2.109   0.0444 *
>> ---
>> Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
>>
>> Residual standard error: 2.121 on 27 degrees of freedom
>>   (1 observation deleted due to missingness)
>> Multiple R-squared:  0.1414, Adjusted R-squared:  0.1096
>> F-statistic: 4.448 on 1 and 27 DF,  p-value: 0.04436
```

2.E.3 SURE with two-step FGLS

SURE with homogeneous equations

One can obtain SURE estimates in R in different ways. For one, function `PGranger_SUR()` with option `method = 'SUR'` corresponds to the procedure in Kónya (2006) and Puente-Ajovín and Sanso-Navarro (2015), which assumes identical lag lengths for all countries. This is demonstrated below for the sample data set and a simple VAR(1).

```
SUR1 <- PGranger_SUR(dat,
                     dep.var = 'gr',
                     indep.var = c('l1gr', 'l1gd'),
                     id.var = 'country',
                     time.var = 'year',
                     method ='SUR')
SUR1$Coefficient.estimates[,-7]
```

```
>>    i Variable Coefficient  Estimates Std.Error      t.stat Sig.
>> 1 GB       gr   Intercept  0.8604758 2.6580823  0.32372051
>> 2 GB     l1gr        l1gr  0.6181728 0.1748799  3.53484263   **
>> 3 GB     l1gd        l1gd -0.3619740 4.9422204 -0.07324117
>> 4 US       gr   Intercept  0.7758007 2.4976138  0.31061675
>> 5 US     l1gr        l1gr  0.3062617 0.1869983  1.63777854
>> 6 US     l1gd        l1gd  0.6071935 3.7800097  0.16063280
```

Conventional SUR estimates can also be performed with alternative R packages. For example, we can verify these results by comparing them with the output obtained using the R packages `systemfit` (Henningsen and Hamann, 2008):

```
# Load systemfit and dependencies
library(systemfit)
library(plm)
# systemfit is a bit picky with id names
X <- dat[, c('year', 'country', 'gr', 'l1gr', 'l1gd')]
X$country <- ifelse(X$country == 'GB', 'G',
                    ifelse(X$country == 'Canada', 'C',
                           ifelse(X$country == 'US', 'U',NA)))
colnames(X) <- c('year','country','y','a','b')
sf <- systemfit(y ~ a +b,
                method = 'SUR',
                data= plm.data(X, c("country","year")))
summary(sf)
```

```
>>
>> systemfit results
>> method: SUR
>>
>>         N DF     SSR detRCov   OLS-R2 McElroy-R2
>> system 58 52 202.609  8.5715 0.247865   0.187912
>>
>>    N DF     SSR     MSE    RMSE       R2   Adj R2
>> G 29 26  78.891 3.03427 1.74192 0.383428 0.336000
>> U 29 26 123.718 4.75837 2.18137 0.125219 0.057928
>>
>> The covariance matrix of the residuals used for estimation
>>         G       U
>> G 2.92251 2.03853
```

```
>> U 2.03853 4.63269
>>
>> The covariance matrix of the residuals
>>         G       U
>> G 3.03427 2.42212
>> U 2.42212 4.75837
>>
>> The correlations of the residuals
>>          G        U
>> G 1.000000 0.637441
>> U 0.637441 1.000000
>>
>>
>> SUR estimates for 'G' (equation 1)
>> Model Formula: G_y ~ G_a + G_b
>> <environment: 0x000001b154a6e598>
>>
>>              Estimate Std. Error  t value  Pr(>|t|)
>> (Intercept)  0.860476   2.658082  0.32372 0.7487393
>> a            0.618173   0.174880  3.53484 0.0015524 **
>> b           -0.361974   4.942220 -0.07324 0.9421748
>> ---
>> Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
>>
>> Residual standard error: 1.741915 on 26 degrees of freedom
>> Number of observations: 29 Degrees of Freedom: 26
>> SSR: 78.890998 MSE: 3.034269 Root MSE: 1.741915
>> Multiple R-Squared: 0.383428 Adjusted R-Squared: 0.336
>>
>>
>> SUR estimates for 'U' (equation 2)
>> Model Formula: U_y ~ U_a + U_b
>> <environment: 0x000001b154a6e598>
>>
>>             Estimate Std. Error t value Pr(>|t|)
>> (Intercept) 0.775801   2.497614 0.31062  0.75857
>> a           0.306262   0.186998 1.63778  0.11352
>> b           0.607194   3.780010 0.16063  0.87362
>>
>> Residual standard error: 2.181369 on 26 degrees of freedom
>> Number of observations: 29 Degrees of Freedom: 26
>> SSR: 123.717663 MSE: 4.758372 Root MSE: 2.181369
>> Multiple R-Squared: 0.125219 Adjusted R-Squared: 0.057928
```

The same results can also be obtained using `PGranger2_SUR()` with option `method`

= 'SUR', as long as the list of equations is identical for all countries, and generates identical results to PGranger_SUR() or systemfit().

```
# Setting up equations for SUR
Equations <- list('GB' = as.formula(gr ~ Intercept + l1gr + l1gd),
                  'US' = as.formula(gr ~ Intercept + l1gr + l1gd))
SUR2 <- PGranger2_SUR(dat.PG2,
                      Equations = Equations,
                      id.var = 'country',
                      time.var = 'year',
                      method ='SUR',
                      for.BS = FALSE)
SUR2$Coefficient.estimates
```

```
>>   id Coefficient  Estimates Std..Error     t.value p.value Sig.
>> 1 GB   Intercept  0.8604758  2.6580823  0.32372051   0.751
>> 2 GB        l1gr  0.6181728  0.1748799  3.53484263   0.004   **
>> 3 GB        l1gd -0.3619740  4.9422204 -0.07324117   0.943
>> 4 US   Intercept  0.7758007  2.4976138  0.31061675   0.761
>> 5 US        l1gr  0.3062617  0.1869983  1.63777854   0.125
>> 6 US        l1gd  0.6071935  3.7800097  0.16063280   0.875
```

SURE with heterogeneous equations

Lastly, PGranger2_SUR() allows for varying estimation equations which can easily be specified in the Equations input argument:

```
# Setting up equations for SUR
Equations <- list('GB' = as.formula(gr ~ Intercept + l1gr + l1gd),
                  'US' = as.formula(gr ~ Intercept + l1gr ))
# Run model
SUR2 <- PGranger2_SUR(dat.PG2,
                      Equations = Equations,
                      id.var = 'country',
                      time.var = 'year',
                      method ='SUR', for.BS = FALSE)
SUR2$Coefficient.estimates
```

```
>>   id Coefficient  Estimates Std..Error     t.value p.value Sig.
>> 1 GB   Intercept  0.8351611  2.5716186  0.32476085   0.751
>> 2 GB         l1gr  0.6081488  0.1707867  3.56086783   0.003   **
>> 3 GB         l1gd -0.2769850  4.7720810 -0.05804281   0.955
>> 4 US   Intercept  1.1821788  0.5231771  2.25961500   0.041   **
>> 5 US         l1gr  0.3012489  0.1834792  1.64186903   0.125
```

CHAPTER

THREE

SWITCHING IT UP: THE EFFECT OF ENERGY PRICE REFORMS IN OMAN

Abstract For the most part, the public debate on fossil fuel energy subsidies has been governed by two arguments. From the position of the profit-maximising firm, the economic rationale has gravitated toward the issue of cost competitiveness. Reducing emissions requires a cutback of energy consumption which, when operating through the pricing mechanism, drives up the cost of inputs; increases in fossil fuel prices may harm competitiveness. On the other hand, the environmental argument stresses the importance of cost transparency and externalities. However, a body of research has also emerged which introduces a second layer to the argument of cost-competitiveness by emphasising that an increase in energy prices may not necessarily be detrimental to economic performance. This study provides novel evidence of this insight by examining the effect of a change in fossil fuel subsidies on the manufacturing industry of an oil-rich Middle Eastern economy. Using a novel firm-level micro data set on Omani manufacturing enterprises, our work shows that fossil fuel energy factor price increase productivity, efficiency, and notable business upgrading. The findings in this paper indicate that subsidy reforms may not only be used to achieve environmental goals but may also drive upgrading and modernisation processes of firms that can ultimately improve economic performance.

Key words: Productivity, Fossil fuels, Electricity, Oman
JEL classifications: L60, O13, O14, Q40, Q55

3.1 Introduction

It is a widely established economic fundamental that the cost of production inputs is directly linked to the performance and competitiveness of a business. As a consequence, any associated stakeholder may be worried that an increase in input prices may result in a loss of competitiveness of the economic entity in question. This is as true for the business owner as it is for any other party with an invested interest in the economic success of said venture. The argument is particularly salient when cost-competitiveness is one of the central market propositions of the entity of concern. It should therefore be of little surprise that the intervention in energy prices has been a common practice, especially in industrialising countries where access to competitive energy is central to a country's industrial development. And so is the fear of the adverse effects of policy interventions targeting a reduction of greenhouse gas emissions through changes in the price setting mechanism.[1] Consequently, policymakers perceive a clear trade-off between economic performance and environmental sustainability: *An intervention through an increase in fossil fuel prices increases input costs and reduces a firm's competitiveness.*

On the flip side of this argument, energy price interventions through subsidies can also lead to several unintended and undesired economic consequences and costs, all of which go beyond the effects of pollution or destruction of natural habitats, to name a few. For example, low energy prices may encourage excessive consumption and inhibit energy efficiency. Low energy prices may discourage investment in more energy-efficient capital, ultimately hurting a firm's competitiveness. Furthermore, it may foster the utilisation and research in technologies that would not be undertaken in a more cost-transparent environment (Ley et al., 2016).

Much of the economic research on the subject suggests that environmental protection comes at an additional cost to firms which may adversely affect their global competitiveness (Palmer et al., 1995). Porter (1991), as well as Porter and Van der Linde (1995), challenged this notion arguing that environmental regulation may, in fact, improve competitiveness by stimulating innovation capacities. While different versions of this argument – also referred to as the Porter Hypothesis – have been evaluated in the theoretical and empirical literature,

[1] What is more, climate change mitigation in emerging and developing countries might pose a challenge from a development perspective if it requires more costly, low-carbon energy sources (Jakob and Steckel, 2014).

findings remain far from conclusive. While positive aggregate macroeconomic effects on both national budgets as well as global emissions have been reported in support of abolishing extensive fossil fuel subsidies,[2] Dechezleprêtre and Sato (2017) point out that environmental regulation can hamper firm competitiveness when regulation costs outweigh the benefits of innovation and modernisation.[3]

This paper contributes to the growing body of literature that assesses the effect of changes in fossil fuel subsidies on firms in manufacturing. To the best of our knowledge, this study is the first application of the Omani Annual Industrial Survey (AIS) in the energy economics literature and provides a firm-level account of a change in the energy subsidy policy of the Omani government on manufacturing firms in the period 2012-2017. Our findings suggest that increasing fossil fuel energy prices may not necessarily bring negative economic consequences. On the contrary, we find that an increase in fuel prices drives up productivity and efficiency of Omani manufacturing firms, which is brought about implicitly through the modernisation and upgrading of a firm's production processes and implementation of digital technology and ICT equipment. We also find strong evidence of an inter-energy substitution effect between fossil fuels and electricity. This is the consequence of a shift towards more modern and efficient production capacities and the fragmentation of the Omani electricity grid, which creates competition between fossil fuel sources and electricity.

[2]For example, Coady et al. (2017) evaluate global fossil fuel subsidies and estimate that eliminating subsidies by 2013 would have resulted in a notable reduction of global carbon emissions (by 21 per cent), and fossil fuel air pollution deaths (55 per cent), while raising revenue and social welfare by 4 per cent and 2.2 per cent of global GDP, respectively. This is also acknowledged by Li et al. (2017) who find that removing petroleum and gas subsidies in Malaysia would improve economic efficiency, increase GDP and reduce budget deficits while also inflicting higher prices on households. Similar results are also found in Wang et al. (2018), who show that environmental taxes may positively affect aggregate GDP. What is more, Heine and Black (2018) emphasise the positive effect of well-designed environmental taxation, particularly in developing countries which, among other effects, are expected to increase domestic revenues and generate overall positive welfare. Providing a review of global energy subsidies, Sovacool (2017) argues that most subsidies fail to offer net benefits to society and instead inflict costs through government deficits as well as energy fuel shortages and have negative environmental impacts.

[3]This paper focuses on the effect of changes of firms' energy input prices. We acknowledge that fossil fuel subsidy reforms may have very different impacts on households (Coste et al., 2018; Rentschler et al., 2017). For example, Acharya and Sadath (2017) analyse the welfare impact of an energy subsidy reform in India between 1970 to 2015 and find that an increase in the general price level through a subsidy reform will reduce both real income as well as energy consumption. The authors point out the implicit welfare implications of a change in such a subsidy. Heterogeneous welfare effects of carbon taxation have also been observed by Okonkwo (2020) for the case of South African households.

The remainder of this article is structured as follows: section 3.2 provides a review of the ongoing academic debate on the effect of environmental policies before section 3.3 takes a closer look at the institutional underpinnings of the Omani economy. We introduce the Omani Annual Industrial Survey (AIS) data as well as our empirical strategy in section 3.4 and 3.5 respectively before providing a discussion of our findings in section 3.6, and a conclusion in section 3.7.

3.2 Literature

Traditional economics models postulate that the cost of production inputs directly affects the price of outputs and, thereby, the performance and competitiveness of a business. As firms are already maximising profits given production technologies and consumers' preferences, increasing the price of a firm's input would reduce the profit-maximising output level. However, this class of models is ill-suited to study the impact of energy prices on the firm's production. First, the model does not incorporate any externalities from production. This was first described in Hicks (1932) and resonated in different economic fields such as welfare economics (Binswanger et al., 1978) or the energy literature (Ley et al., 2016). Using fossil fuels as an energy input brings the problem of CO_2 emissions that are not adequately represented in the pricing mechanism. This externality manifests in two dimensions: Low fossil fuel prices lead to an over-extensive use of that particular energy source, as would otherwise be the case under cost transparency. The second shortfall is that these low costs will also carry over to incentivize the development of new fuel-based technologies that would have otherwise been considered too costly under a more cost-transparent scenario (Carraro and Siniscalco, 1994). This adverse effect only becomes larger when fossil fuel prices are lowered artificially, e.g. through fossil fuel subsidies. Consequently, one can argue that changes in policy design itself can play an essential role in addressing the issue of climate change if targeted policy interventions (or the abandonment of fuel-promoting policies) can create an environment. Then, profit-seeking interests incentivise private enterprises to switch to less energy-consuming and cleaner energy sources (Carraro and Siniscalco, 1994; Ley et al., 2016).

Among the most prominent proponents of this argument are Porter and Van der Linde (Porter, 1991; Porter and Van der Linde, 1995). They put forward the idea that well-designed regulation may, in fact, not only reduce emissions but

also bring about improvements in production and overall efficiency that compensate for the initially occurring costs of the policy interventions. This notion - popularised as the *Porter Hypothesis* in the literature - has remained subject to an active academic debate. While Palmer et al. (1995) criticise the *Porter Hypothesis* for being incompatible with the fundamental idea of profit maximization as firms would naturally be expected not to forgo profit-maximising opportunities, no conclusion on its validity has been reached in neither the theoretical nor empirical literature (Ambec et al., 2013).[4]

Different explanations for this ambiguity are offered in the literature: For one, the argument on profit maximisation is challenged by a growing literature (Anderson and Newell, 2004; DeCanio, 1993; DeCanio and Watkins, 1998; Nauclér and Enkvist, 2009; Poterba and Summers, 1995) which indicates that, even among advanced economies, bounded rationality and market imperfections may be the reason behind the slow adoption of energy-efficient technologies, even despite their positive net present value of investment (Pye and McKane, 2000; Worrell et al., 2003). The pattern of sub-optimal investment decisions concerning technology adoption is more pronounced in less advanced economies (Bloom et al., 2013; Ryan, 2018). At the same time, a degree of time dependence in the firms' coping mechanisms in response to high energy costs has been identified in the literature (Gamtessa and Olani, 2018). In the short run, firms seem to respond to high(er) energy costs by substituting into the more energy-efficient *existing* forms of capital with the possible effect of capital under-utilisation and decreases in energy consumption (Berndt and Fuss, 1986; Berndt et al., 1990; Berndt and Wood, 1984), while higher energy costs are found to induce investments in new capital (Newell et al., 2006; Steinbuks and Neuhoff, 2014) and innovations under a longer investment horizon (Newell et al., 1999, 2006; Popp, 2001; Wing, 2008). In other words, sub-optimal long-run investment decisions may remain optimal under a shorter time horizon.

The lack of consensus on the *Porter Hypothesis* in the literature may fur-

[4]Exemplarily, while Dechezleprêtre and Sato (2017) point out that environmental regulation can hamper firm competitiveness when regulation costs outweigh the benefits of innovation and modernisation, a growing body of work is lending support to the notion that energy prices are positively related to green innovation. On the other hand, theoretical work by Mohr (2002), Mohr and Saha (2008) as well as Schmutzler (2001) show that changes in the energy pricing mechanism (through regulation and/or taxation) can indeed trigger green innovations, and empirical works find a positive relationship between energy prices and green innovation activities; see among others Popp (2002), Van Leeuwen and Mohnen (2013), Aghion et al. (2016) or Ley et al. (2016) as well as Rubashkina et al. (2015) for an empirical analysis on the European manufacturing sector.

thermore be explained by (i) the absence of clarity on its actual proposition of which three common versions[5] have been identified as well as (ii) the heterogeneous dimension of the frameworks employed by researchers in their efforts to analyse the arguments of the *Porter Hypothesis*[6]. Particularly empirical research has gone into great detail in its efforts to analyse variations of the hypothesis along multiple dimensions.[7]

A further complication for the literature that assesses the link between energy prices and performance outcomes at the firm level is the existence of different possible response measures (Coste et al., 2018; Rentschler et al., 2017): A firm that comes under cost pressure as a result of an increase of fossil fuel prices may (i) pass on the newly occurring costs; (ii) absorb them; (iii) substitute across alternative types of energy that have become relatively cheaper as a result of the intervention; or (iv) increase their energy efficiency, e.g. by reducing overall energy consumption while maintaining pre-reform output levels through innovation. The overall observed effect of such a policy intervention depends mainly on which of the four channels dominates. What is more, the task of identifying quantitative measures to analyse these coping mechanisms has been proven to be very difficult to come by. For example, Cantore et al. (2016) shows that the link between energy prices and performance can be explained through the relationship between efficiency and productivity. Yet, their study does not analyse the effect of changes in the input price structure on firm-level outcomes. It is a noteworthy observation that the issue of assessing input price structures poses a challenge in the field: While the majority of micro-level studies on energy and plants' performance are focused around the use of electricity[8] relatively little research on the substitutability between different energy

[5] Jaffe and Palmer (1997) distinguish between the 'weak', 'strong' and 'narrow' version of the *Porter Hypothesis*: The 'weak' version states that well-designed environmental regulation may spur innovation. In contrast, the strong version poses that this innovation increases firm competitiveness. The 'narrow' version, in turn, puts forward the argument that flexible market-based regulatory policies are preferred to more prescriptive forms of regulation as they provide firms with greater incentives to innovate (Ambec et al., 2013).

[6] For the latter see Cohen and Tubb (2018) on a meta-analysis of the literature.

[7] For example, one strand of the literature investigates the impact of environmental policies and trade patterns and finds that such interventions stimulate competitiveness in the exporting market while the transmission channel for the importing market is more ambiguous; see (Cantore and Cheng, 2018; Costantini and Crespi, 2008; Costantini and Mazzanti, 2012). Another study supporting the strong version of the *Porter Hypothesis* is Murty and Kumar (2003), which indicate that wastewater regulations increase firm-level productivity for developing countries.

[8] See among others Abeberese (2017); Allcott et al. (2016); Fried and Lagakos (2019); Kassem (2018); Marin and Vona (2021).

sources on the firm-level has been done to this day.[9] Rentschler and Kornejew (2018) belongs to the few studies that consider the impact of a variety of energy sources on firm-level performance by looking at a cross-section of small- and micro-firms in the Indonesian manufacturing sector. The authors find that energy prices have a slight adverse effect on long-run firm competitiveness and that firms utilise fuel substitution, energy efficiency, and cost pass-on in response to an energy price increase. Calì et al. (2022), on the other hand, focus on medium- to large-sized manufacturing firms in two emerging economies, Mexico and Indonesia. The authors find that while surges in electricity price harm plants' performance, increases in fuel prices lead to higher productivity and profits for manufacturing plants.

Our paper provides several contributions to the existing literature. First, it belongs to the very few papers identifying the different cost-coping mechanisms available to a firm, out of which we find strong support for the *substitution* as well as *efficiency* channel.[10] Moreover, our findings suggest that increasing fossil fuel energy prices may not necessarily bring about negative economic consequences. On the contrary, we find that the subsidy strategy change has increased the productivity and efficiency of Omani manufacturing firms. Furthermore, while Calì et al. (2022) find that an increase in fuel prices results in higher productivity and profits by providing incentives to invest in more efficient and productive capital equipment, our findings suggest that positive firm-level outcomes are primarily driven by investment in Information and Communication Technology (ICT). The impact of such technologies has been reported in the literature before (OECD, 2010; Schulte et al., 2016, among others). The link between efficiency and ICT investment is also confirmed in the production economics literature, which reports positive net effects arising from the implementation of Smart Manufacturing Systems (SMS) on productivity, integration, transparency, flexibility and profitability (Kamble et al., 2020; Ndubisi et al., 2019, among others).

Furthermore, as we can link firm-level input price dynamics to changes at the institutional level, this study provides an extensive empirical account of an energy policy intervention by the Omani government, which corresponds to the setting described in the theoretical work of Xepapadeas and de Zeeuw (1999).

[9] See for example Chang (1994) as well as the meta-analyses by Koetse et al. (2008) and Stern (2012), respectively.

[10] As we will discuss in section 3.6, we also observe empirical evidence that indicates the lack of a cost pass-through of energy prices, which be expected given the institutional context.

There, environmental policy (emissions tax) triggers upgrading to equipment with higher average productivity (and is less polluting). However, the policies described in their paper are not net-positive for firms because the trade-off between environmental outcomes and firm productivity is hampered *vis-à-vis* a scenario where environmentally friendly technology had no productivity effect.

Consequently, this paper addresses challenges faced by much of the earlier empirical work on the *Porter Hypothesis* by providing an empirical account of multiple firm-level cost-coping mechanisms, which have primarily been unattainable in earlier work. In addition, it links them to existing bodies of research on the impact of ICT and SMS systems on firm-level outcomes. The policy interventions of the Omani government allow us to analyse the effect of prices of energy sources on productivity, performance and efficiency. We argue that the case of Oman may serve as an interesting reference point for future market interventions in oil-rich Middle Eastern economies in the wake of global climate change concerns. This is of particular relevance as, while related input price effects have been recently encountered for Indonesia and Mexico (Calì et al., 2022), little is known about the broader transmission channels of changes in the energy input mix. Understanding the commonalities and roots of country-specific differences in response to environmental policy actions will be highly relevant for designing and implementing well-targeted environmental policies in the wake of a climate-aware future.

3.3 Institutional context

3.3.1 The role of fossil fuel

Oman's economy is heavily dependent on its oil and gas sectors. In 2009 alone, these two sectors accounted for roughly 78 per cent of the total government revenue and approximately 65 per cent of the country's total exports, with around 41 per cent of its gross domestic product (GDP) (Al-Badi et al., 2011b; Ministry of National Economy, 2010). One of the unique features of the Omani energy sector is its reliance on domestic natural gas for electricity production, as natural gas accounts for around 95 per cent of fuel used for power generation. In contrast, diesel accounts for the remaining fraction (APSR, 2012, 2015; EIA, 2019). Given the abundance of this energy source, the Omani government has followed a strategy to capitalise on its position to subsidise both the gener-

ation and distribution of power from fossil fuels (APSR, 2015). The government of Oman provides a large subsidy to the electricity sector, which amounts to the difference between the average cost of production (financial cost) and the actual tariff (Malik and Al-Zubeidi, 2006).[11]

3.3.2 The Omani electricity grid

The abundance of cheap fossil-fuel power is not the only feature of the Omani energy market. Although most of the population has access to electricity, there are still rural and remote areas that are not electrified. As the population of Oman is geographically scattered, the power systems in Oman are also not interconnected. More precisely, Oman's electricity sector consists of multiple and separate networks: (i) the Main Interconnected System (MIS), which covers most parts of the North and is the larger of the two main networks; (ii) the Salalah system covers Salalah and surrounding areas in the Dhofar region (Southern part of Oman) also referred to as Dhofar power system (DPS); (iii) and lastly, the remaining scattered rural areas of Oman referred to as the Rural Areas Electricity Company (RAEC) (Albadi, 2017; Coyle, 2017).[12] Whereas the first two use gas almost exclusively, the RAEC generates electrical power mainly from diesel generators.

3.3.3 Policy interventions in the energy market

Over the past years, the stagnating oil price and increasing domestic demand for energy have received growing attention from policymakers in Oman and have hit the country, which has been relying on oil revenue to sustain its treasury budget and development agendas, hard.[13]

[11]The Omani electricity tariff structure differs across the various parts of the economy, e.g. domestic, commercial, industrial, agriculture and fisheries, hotels and tourist establishments etc., and can further vary depending on actual usage, as is the case for the domestic tariff, or be subject to price changes depending on the season and overall energy demand (such as the commercial and or/ industrial tariffs); see (Malik and Al-Zubeidi, 2006; MHEW, 2002). Most recently, Oman has put in place a more cost-based tariff structure which, however, does not fall into the sample period of our data and is therefore not explored in further depth.

[12]The two remaining components of the Omani electricity grid are the main oil company in Oman (PDO), which has its own dedicated system as well as other companies that produce power for their own needs such as Oman Mining company, Oman Cement Company, Sohar Refinery, Sohar Aluminum Company, Ministry of Defense, Occidental of Oman, etc. (Albadi, 2017).

[13]For example, in 2014 the oil price fell to a low of 40 USD a barrel which is below the break-even price of crude oil production in most oil producing countries; for Oman, the break-even

Based on these developments, the Omani government recognised the need to reform its decade-old subsidy system in 2015 through a reduction of the fossil fuel subsidies in the country. Pre-reform fossil fuel subsidies in Oman can be translated as government assistance to industries, the private sector and households to guarantee affordable energy and access to energy. The efforts of the Omani government at providing fossil fuel subsidies stalled in 2014, when the net oil revenue of the government had been halved. Since 2015, Oman has been following in the footsteps of other oil-producing countries, which have reformed fossil fuel subsidies lately amid a slump in oil prices.[14] Subsidies on fossil fuels, including petrol and diesel, are estimated to have cost Oman from 1,134 million US$ in 2014 to 54 million in 2017; see Table 3.1. Recognising the benefits of liberalising its fuel prices, in 2015, Oman made a substantive effort to deregulate the prices of diesel and petrol gradually.[15]

Table 3.1: Energy subsidies in Oman

Year	Fossil fuel subsidy (US$, million)	Electricity subsidy (US$, million)
2014	2,948	767
2015	1,136	1,170
2016	446	724
2017	54	988

Source: APSR (2018); CBO (2017); MoF (2018).

Energy subsidy reforms often go beyond simple changes in the pricing mechanism. They typically include a comprehensive reform package with measures designed to facilitate firms' and households' adoption and mitigation processes. The same has been true for the case of Oman, where various programs and initiatives were put in place. For example, the *Industrial Innovation Assistance Program* (IIAP, 2020) was established to help Omani industrial SMEs to improve

oil price for 2014 was estimated at roughly 90 USD/barrel (IMF, 2019).

[14]Since the electricity sector in Oman is heavily subsidised, the cost of energy provision for the government may vary greatly depending on the form of the initial energy source used for the production of electricity. For example, even though electricity supplied by the (diesel-powered) RAEC network accounts for less than 3 per cent of the total electricity supplied in Oman, its subsidies represent about 22 per cent of the subsidies of the gas-powered MIS network (Albadi, 2017). This situation may put additional fiscal pressure on the Omani government due to low global oil prices.

[15]The Annual Report of the Central Bank of Oman (CBO, 2017, pp. 8) states the 2016 budget endeavours to focus on measures to rationalise government expenditures and initiate reforms in subsidies (inc. removal of fuel subsidies). The Oman State budget for 2015 was announced in the *Royal Decree 2015/1*, and the budget for 2016 was announced in the *Royal Decree 2016/2*.

their efficiency and sustainability. Further initiatives which endorse ICT activities through investment support of the digital economy and promote the roll-out and adoption of emerging technologies such as Artificial Intelligence, the Internet of Things and SMS were put in place (MOCIIP, 2019; Oman Digital Strategy, 2019). Therefore, we acknowledge that the observed effects are understood in the context of the institutional underpinnings and policy interventions and cannot be reduced to price adjustment processes alone.

Our empirical analysis in the next section will show that the policy change of the Omani government has led to an increase in the average fossil fuel prices relative to the price of electricity. This allows us to analyse how the Omani government's strategic changes in fossil fuel subsidies have affected the energy configuration of the Omani manufacturing industries. This, by no means, implies that the manufacturing industry will have become 'greener'[16] as a consequence of this policy change. Our empirical analysis indicates (a) a gain in efficiency in the production process as a result of the price increase and (b) a substitution effect from fuel to gas which is the result of the fragmentation of the Omani electricity grid. Before we discuss our identification strategy and empirical findings, we provide a detailed account of the AIS data in the next section.

3.4 Data

Our primary data source is the Omani Annual Industrial Survey (AIS). This data provides us with panel data of establishments in the manufacturing sector and licensed by the Ministry of Commerce and Industry of the Sultanate of Oman; see Appendix 3.B for a detailed description of the data set and questionnaire. The AIS data provides information on roughly 3,600 manufacturing

[16] Zivin et al. (2014) points out that electrification by itself does not necessarily imply lower overall emissions but depends on the means through which electricity is generated. As a matter of fact, gas as a production means for electricity has experienced somewhat of a paradigm shift in the public eye, as noted by Costello (2017). It is now seen as a much less desired bridging technology towards renewable energy sources. The author also points out that existing natural gas technology for electricity production is currently well-known and still more inexpensive than alternatives such as renewable energy, which typically lacks the necessary infrastructure. However, Omani policymakers have acknowledged the high reliance on a finite source of energy, i.e., natural gas (Al-Yahyai and Charabi, 2015). Although the contribution of renewable energy to the Omani electricity supply mix is minimal, several published studies have emphasised the potential of solar and wind resources in Oman (AER-Oman, 2008; Al-Badi et al., 2011a, 2009b, 2011b; Sultan et al., 2010).

establishments surveyed between 2012 and 2017 and follows the International Standard Industrial Classification (ISIC) at the IV-digit level.

3.4.1 Construction of data series

Energy unit prices. Given AIS' information on purchasing price and quantity of electricity, gasoline, diesel, kerosene, LPG, gas as well as lubricants, we calculate respective unit prices as

$$up_{it}^{m} := \frac{value_{it}^{m}}{quantity_{it}^{m}} \tag{3.4.1}$$

where *value* denotes the purchased value (in real LCU) and *quantity* denotes the purchased quantity of $m = \{\textbf{EL}ectricity,\ \textbf{F}ossil\ \textbf{F}uels\}$, i.e. fuels and electricity, for firm i and period t, respectively. We convert all nominal LCU series employing the Omani manufacturing PPI (ISIC II-digits; 2012 = 100) series published in the industrial strategy report on the manufacturing sector (AIS, 2018).

The calculation in Equation 3.4.1 is straight-forward for electricity. To obtain a comparable fossil fuel aggregate, we combine all different types of fossil fuels to express their unit prices in kilowatt-hour (kWh) equivalents as follows. First, we drop lubricants from the group of fossil fuels as they are typically not used as the main means to generate energy from non-renewable sources and are only needed as a by-product in this process. Secondly, we convert each fossil fuel source into their respective kWh equivalents; see Table 3.19 for more information on the conversion standards. For successful conversion of all fossil fuel energy sources, we require the purchased value and the corresponding quantity and unit of measurement. Since the unit of measurement variable has been reported less consistently across firms in AIS, we resort to a simple algorithm described in Appendix 3.C to improve data coverage. Based on the kWh equivalents, we then calculate a price index for fossil fuel (up^{FF}) based on the sum of all $n = \{gasoline,\ diesel,\ kerosene,\ LPG,\ gas\}$ fossil fuel types which we weight by their respective quantities

$$up_{it}^{FF} = \sum_{n} w_{it}^{n} \times up_{it}^{n}, \quad w_{it}^{n} = \frac{q_{it}^{n}}{\sum_{n} q_{it}^{n}} \tag{3.4.2}$$

where q_n denotes the quantity (in kWh equivalents) of fossil fuel type n for firm i and period t, and w_{it}^n corresponds to its fixed-quantity weight, respectively.

Measures of performance, productivity, efficiency and substitution. To measure the capital stock of the surveyed Omani manufacturing firms, we rely on capital stock data available through AIS. There, firms report stocks, purchases, and liquidations and depreciation of buildings, machinery, transport, and ICT equipment over time. In this, we resort to total factor productivity. However, here we are limited by the scope of what is available in the data and thus resort to a simple identification based on a standard Cobb-Douglas production function approach. Additional performance variables analysed are labour productivity[17] and, as we are interested in learning about the effect of energy price changes on the relative profitability *across* establishment, the gross profit margin[18]. Finally, to assess the turnover and energy efficiency, we look at changes in the composition of capital goods (through sales and purchases), where we further differentiate between machinery and ICT goods. This is important as ICT-classified equipment allows us to learn about any type of capital modernisation and production upgrading. This is because the ICT category classifies devices using electronic controls and components forming part of these devices.[19] Lastly, for substitution and energy efficiency measures, we resort to quantity measures of electricity and fossil fuels (in units of kWh-equivalents) relative to value added. We provide summary statistics on a selected group of key variables in Table 3.2.

[17] We measure labour productivity as the ratio of value added (as well as gross output) over employment per business.

[18] We follow the standard definition of gross profit margin calculations which is based on the ratio of gross profits over total revenues.

[19] See AIS survey questionnaire: '***7.4 ICT Equipment:*** *The ICT equipment consists of devices using electronic controls and also the electronic components forming part of these devices.* ***7.5 Machinery and other equipment:*** *This item includes all industrial machinery and other equipment such as construction machinery, cranes, forklift equipment and the like; power generating machinery; metalworking machinery; mining machinery; office machinery, equipment and furniture; and similar machinery and equipment and major renovations and alterations to these. Includes also plants/machinery for treatment of waste.*'

Table 3.2: Summary statistics selected variables AIS

Variable Name (*abbreviation*)	Mean	SD	Min	Max	Nobs.
Unit prices by energy type					
Fossil fuels (FF)	-4.12	0.19	-4.91	-3.40	971
Electricity (EL)	-3.86	0.29	-5.00	-3.16	3518
Performance and profitability					
Value added/Employment (LP^{va})	9.39	1.01	3.87	14.43	3677
Output/Employment (LP^{go})	10.25	1.02	5.19	15.07	3685
Total Factor Productivity (TFP)	8.19	1.53	5.00	11.00	1881
Gross profit margin (GPM)	-0.62	0.70	-5.00	0.00	2796
Efficiency and turnover					
Machinery sales and purchases ($Machinery$)	10.53	2.39	3.09	18.99	1920
ICT sales and purchases (ICT)	8.41	2.08	3.14	15.65	973
Fossil fuels quantity, kWh equivalents (qFF)	13.71	2.10	4.64	18.97	1097
Electricity quantity (qEL)	13.21	1.91	6.04	23.79	3605

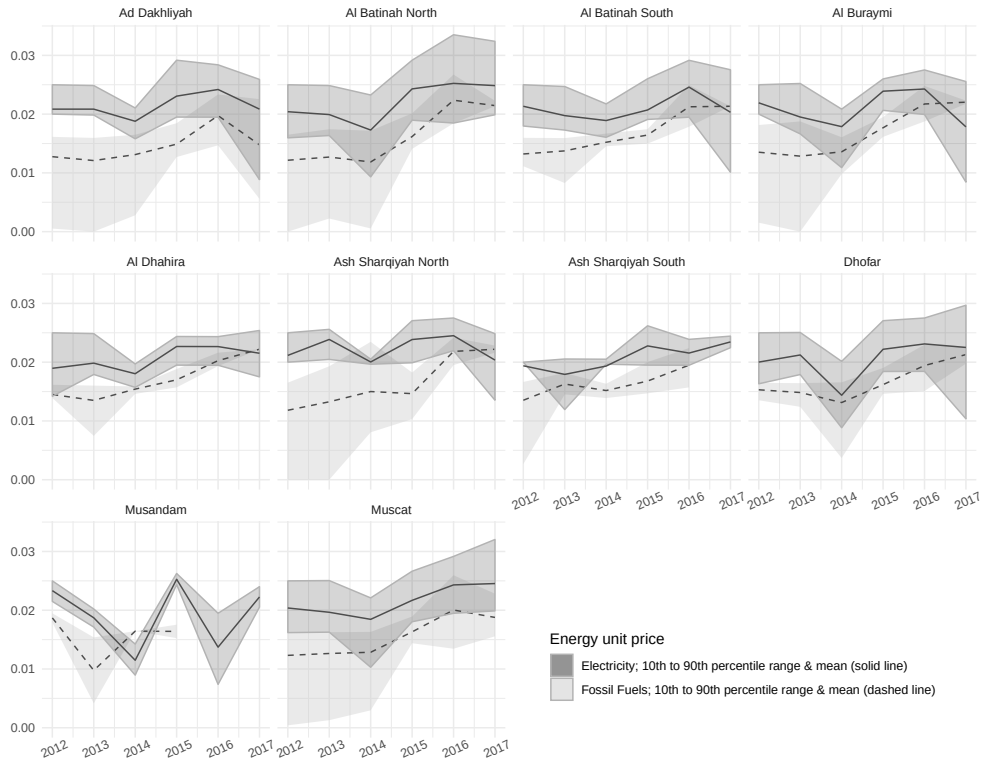
Note: All values in logs. Listed indicators correspond to dependent variables in Equation 3.5.1.

3.4.2 Exploratory evidence

Variation across regions and over time

We provide a visualisation of the two energy unit price series in Figure 3.1, which illustrates a price hike in fossil fuels, particularly after 2015, accompanied by a notably lower degree of firm-level price variation. Furthermore, the unit price differential between fossil fuels and electricity has decreased across all regions over the sampled period. Lastly, the intervals in Figure 3.1 indicate the 10^{th} and 90^{th} percentile of recorded energy unit prices by energy source and suggest more considerable variation in certain provinces for fuel and electricity. Even though electricity rates themselves are fixed for given periods, volatility in our sample, as shown in the aggregated indices in Figure 3.1 may arise as a result of the Omani electricity tariff structure, which has different rates, i.e. peak/off-peak and high-demand periods types of enterprise etc. (Albadi, 2017). Much of the variability observed in Figure 3.1 may also result from changes in the sample composition. If firms only enter during high-/ low-price spells, this will also show in Figure 3.1. In Table 3.3, we summarise the within-region variances of both fuel types. In line with Figure 3.1, we find a considerably lower degree of variation in electricity unit prices compared to their fossil counterparts.

Figure 3.1: Unit prices by energy type and region Omani manufacturing firms



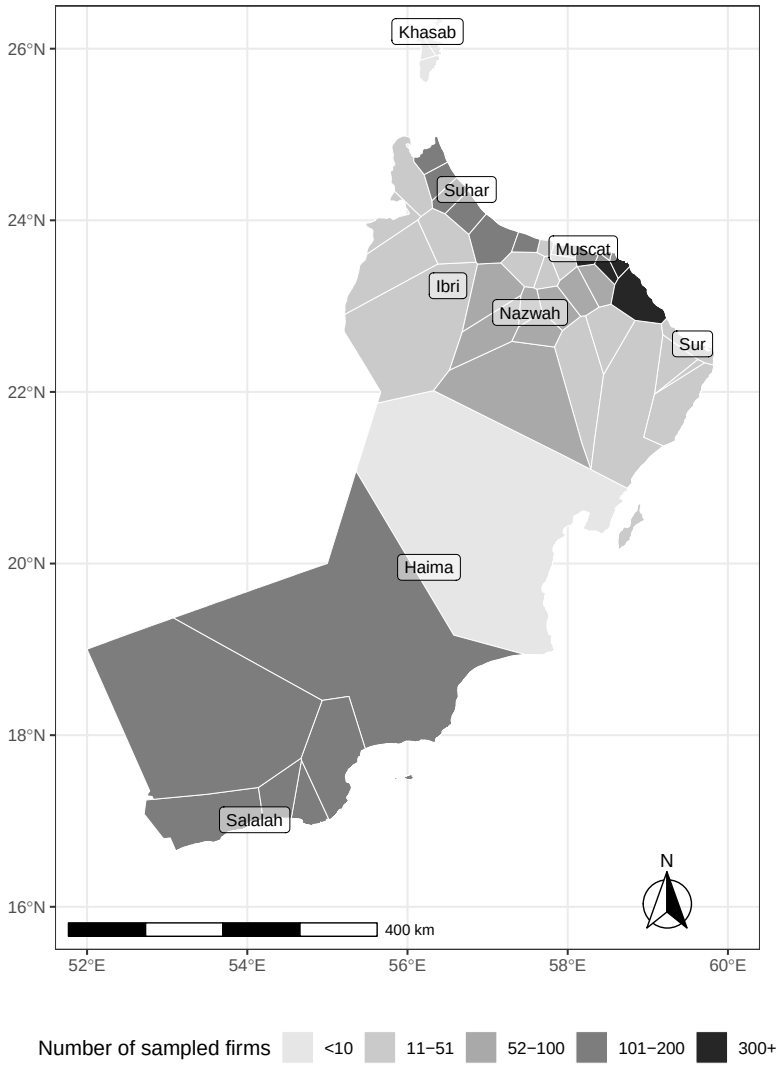
Note: Figure does not include *Al Westa* region which does not provide any firm level data for the second half of the sample period. Unit prices by energy type calculated as described in Equation 3.4.2 and based on self-reported data by firms in AIS data. Intervals indicate the 10th and 90th percentile of recorded energy unit prices by energy source.

Table 3.3: Within-region variance of Omani manufacturing firms

Region	Fossil fuels (FF)	Electricity (EL)
Ad Dakhliyah	0.069	0.025
Al Batinah North	0.087	0.038
Al Batinah South	0.064	0.028
Al Buraymi	0.088	0.028
Al Dhahira	0.063	0.026
Al Wusta	0.087	0.022
Ash Sharqiyah North	0.043	0.077
Ash Sharqiyah South	0.030	0.018
Dhofar	0.124	0.027
Musandam	0.098	0.008
Muscat	0.080	0.041

Note: Within-region variance of energy unit prices, in logs.

Figure 3.2: Spatial distribution of firms across Omani regions



Note: Number of sampled firms for which both electricity and fossil fuel unit prices are available for at least one year. Unit prices by energy type calculated as described in Equation 3.4.2 and based on self-reported data by firms in OASIS data.

In Figure 3.2 we provide a regional representation of the firms in our sample.[20] This illustrates that the large majority of the firms are located in the north/north-eastern part of the country, which is part of the MIS grid, and with a particular concentration in and around Muscat, where electricity is provided by the *Muscat Electricity Distribution Company* (Al-Badi et al., 2009a). Unfortunately, no further information on the actual location of the firms in the Dhofar

[20]To this end we only count firms for which we recover an energy unit price index as calculated in Equation 3.4.2 for at least one year.

Table 3.4: Sample Attrition AIS

Years sampled	Nobs.	Nobs./(full sample)
6 yrs. (all years)	1,740	47%
5 yrs.	2,600	71%
4 yrs.	2,980	81%
3 yrs.	3,334	90%
2 yrs.	3,518	95%
1 yr. (full sample)	3,690	100%

Note: Maximal numbers of observations (nobs.) conditional on years sampled. All years: 2012-2017; full sample: all firms.

region is available that would allow for more exact discrimination of the power grid allocation to either the RAEC or DPS. Unfortunately, AIS does not allow us to infer how much gas was used by firms to produce electricity in-house. However, we argue it is safe to assume that this practice correlates with location and grid access. We account for these region-specific variations through our empirical models, which we present in the next section.

Panel data attrition

One crucial consideration with firm-level studies such as this concerns the economic survival of the unit of inquiry.[21] Put differently, firm-level attrition may pose a serious concern for the validity of our findings, as one would expect unsuccessful firms to be more likely to go out of business than successful ones during our sample period. While we have no reason to doubt the representative nature of the AIS data, Table 3.4 does reveal considerable variation in the firm-level coverage of sampled years. We will accommodate this observation by providing results of a quasi-hard-balanced sub-sample of our data to illustrate that our results are not driven by the effect of firms' entries and exits.

3.5 Empirical strategy

The primary purpose of this paper is to assess to what extent differences in the price movements of fossil fuel and electricity can explain variations in

[21] As this is a within-firm analysis, we acknowledge that the observed effects are conditional on firms' survival. Since the information provided in AIS is insufficient to determine whether a firm has exited the survey due to bankruptcy, we cannot offer a more detailed discussion on the extensive margin.

firm-level outcomes along the delineating factors of performance, productivity, turnover, and efficiency. We build our analysis on a simple empirical model of the form:

$$y_{it} = \beta_i \mathbf{x}_{it} + \varepsilon_{it}, \tag{3.5.1}$$

where $\mathbf{x}_{it} = [up_{it}^{FF}, up_{it}^{EL}]'$ denote the unit prices for fossil fuels (FF) and electricity (EL) for firm i and period t, respectively. Our setup aims to capture differences in firm-level outcomes that result from changes in the price differential of fossil fuel and energy prices. Such effects could either result from firms improving productivity and competitiveness - thereby providing further insights on how the *Porter Hypothesis* is related to changes in energy input prices or the degree of substitutability between energy sources. The set of firm-level outcome variables is denoted y_{it} and includes measures of performance, profitability, turnover, and efficiency as listed in Table 3.2.

There are a few conceptual challenges to address when attempting to analyse the effect of firm responses to changes in the energy price mix in Equation 3.5.1. The most straightforward case is endogeneity due to a reverse-causal relationship between firm-level outcomes and energy prices and could introduce either downward or upward bias in Equation 3.5.1. For example, product- or firm-specific demand shocks could lead to an increase (decrease) in economic productivity, which could change energy prices and plant-level economic outcome indicators such as sales, productivity, investment etc. A further channel for reverse causality dynamics may result from technological shocks or local infrastructure development, affecting energy costs and performance. Exemplarily, the effect of improvement in local infrastructure may be most strongly felt by firms producing non-durables. We account for these issues following Calì et al. (2022) by assuming the following structure of the error term:

$$\varepsilon_{it} = \alpha_i + D_{st} + \tau_r + e_{it}, \tag{3.5.2}$$

where α_i denotes firm-specific intercepts while we introduce ISIC II-digit industry-year dummies through the term (D_{st}) to capture market conditions variations and technology-related development patterns that are intrinsic to the respec-

tive manufacturing industry. We include regional time trends (τ_r) to capture the differences in long-run development dynamics in local governance, infrastructure, development, innovation, etc., all of which similarly affect the plans in the same region.

The fixed effects approach employed here has been widely used in this and related literature and is reported to perform reliably well to address endogeneity, and particularly the selection of unobservables (Mundlak, 1978; Wooldridge, 2005). We also rule out further causes of endogeneity that result from autocorrelation and heteroscedasticity. We combat these issues by providing clustered standard errors that are robust to heteroscedasticity and autocorrelation in the disturbances across all model specifications unless stated otherwise. We provide a battery of related tests, all of which are considered in subsection 3.6.2, where we discuss the robustness of our findings.

3.5.1 Instrumental variable design

While time-invariant unobservables will be accounted for by including firm-level fixed effects (α_i), the issue of time-varying unobserved variation at the plant level remains an issue. There are many possibilities for how plant-level unobserved and time-varying variation could arise. For example, a newly assigned plant CEO could improve its performance and energy prices through their exceptional management skills and potential personal or political proximity to local energy distributors or authorities. Consequently, and in the spirit of Calì et al. (2022), we propose an instrumental variable configuration of plant-level energy prices of the form:

$$up_{it}^{m} = \alpha_0 + \alpha_1 inst_{it}^{m} + D_{st} + \tau_r + \eta_{it}, \tag{3.5.3}$$

for $m = \{FF, EL\}$ where α_0, D_{st} and τ_r have the same interpretation as in Equation 3.5.2. For the validity of the instruments ($inst_{it}^{m}$), we require that it affects a plant's performance only through its direct impact on plant-level prices. For this purpose, we exploit the data to isolate the geographical markup on energy prices resulting from the cost of distributing energy to a particular province. We recover this effect in the following way: First, for an arbitrary firm i and each of the two energy unit price indices up_{it}^{m}, we calculate the average unit prices paid by all other plants (i') in the same region r in a given year as:

$$\bar{up}_{it}^{m} = \sum_{i \in r, i \neq i'}^{N_{rt}} \frac{up_{it}^{m}}{(N_{rt} - 1)}, \tag{3.5.4}$$

where N_{rt} denotes the number of plants populating the same region as firm i, $i \in r$ in period t. Next, we divide this quantity by the country-wide average of the same year to minimise the potential correlation of plant-level outcomes with aggregate shocks affecting each energy type as

$$inst_{it}^{m} = \frac{\bar{up}_{it}^{m}}{\bar{up}_{t}^{m}}, \quad \bar{up}_{t}^{m} = \sum_{i}^{N} \frac{up_{it}^{m}}{N}, \tag{3.5.5}$$

where $\bar{up}_{t}^{m}$ is the average, national unit price of energy source m in year t. This instrument can be interpreted as a proxy for the cost of energy distribution to a particular region and is correlated with plant-level energy prices while mitigating the effect of idiosyncratic firm-level dynamics through the exclusion of the firms in question to calculate regional averages in Equation 3.5.4. For the remainder of this paper, we refer to this setup as the Fixed Effects Instrumental Variable (FE-IV) estimation.

3.5.2 Unobserved dependencies between plants

One of the central limitations of the fixed effects setup in Equation 3.5.2 is that it assumes homogeneity in the effects of the common unobserved effects across regions and firms. Given the complex structure of the Omani energy distribution network as described, this may be too strict of an assumption. Additionally, one can make a clear case that common factors such as global economic conditions or political developments could influence energy prices and firm-level outcomes. This would pose a challenge for the specification proposed in the previous paragraphs as fuel and electricity prices of all (other) firms in the same region/industry are subject to the same omitted common disruptions. The issue of (spatial) cross-sectional dependence in a micro-econometric panel data set like AIS is highly warranted as complex patterns of mutual dependence between cross-sectional units are very likely.

We address this issue in two ways. On the one hand, we re-estimate Equation 3.5.1 by employing Driscoll and Kraay (1998) standard errors that are ro-

bust to a moving-average autocorrelation structure as well as cross-sectional dependence. On the other hand, we adopt a (multi-factor) error structure of Equation 3.5.1 of the form:

$$\varepsilon_{it} = \alpha_i + \gamma_i \mathbf{f}_t + e_{it}, \tag{3.5.6}$$

where α_i again is the firm-specific intercept wile $\mathbf{f}_t$ denotes a set of common unobserved factors with heterogeneous firm-level factor loadings (γ_i). The specification in equations 3.5.1 and 3.5.6 corresponds to the Common Correlated Effects (CCE) estimator by Pesaran (2006) and controls for all time-varying and region-specific unobservable factors which can be a combination of 'strong' factors, that describe global but also country-level shocks such as a change in global market and trading conditions, but also 'weak' factors that capture local spillovers (Chudik et al., 2011). The model accounts for all time-varying and region-specific unobserved factors while simultaneously allowing these factors to correlate with our model's observable components. This is done by augmenting the regression equation with cross-sectional averages as:

$$y_{it} = \beta_i' \mathbf{x}_{it} + \delta_i \bar{\mathbf{x}}_{it} + \eta_i \bar{y}_{it} + u_{it}, \tag{3.5.7}$$

with $\bar{x}_{it}^m = \frac{1}{N}\sum_{i=1}^{N} x_{it}^m,\ m = \{FF, EL\};\ \bar{y}_{it} = \frac{1}{N}\sum_{i=1}^{N} y_{it}$ (Pesaran, 2006).[22] For the remainder of this paper, we refer to this setup as the Common Correlated Effects (CCE) estimation.

For both model specifications described in Equation 3.5.2 and 3.5.6 we further introduce the homogeneity assumption which imposes homogeneous coefficients, i.e. $\beta_i = \beta,\ \forall\, i \in N$. The two complementing estimation strategies provide the framework for discussing the results in the next sections.

[22] We want to point out that the CCE setup imposes more stringent requirements on the data coverage of our sample compared to the FF-IV specification because - as a consequence of the cross-sectional augmentation in Equation 3.5.7 - having many firms with fewer than three observations over time can bring down the degrees of freedom in our regression framework dramatically. To account for this shortcoming, we adjust our data sample to contain only firms for which at least four years' worth of observations are available. What is more, for firms where the algorithm described in Appendix 3.C fails to provide unit price indices for either energy source, we re-populate the missing cells through industry-year-region averages. Through this approach, we recover roughly the same number of observations for the CCE estimations as for the FE-IVs.

3.6 Results

The discussion of the main results in this section is based on the instrumental variable approach and the CCE estimator. This is followed by a discussion on the robustness of our results. Before moving on to discuss the main findings of our empirical analysis, we want to point to the set of complementary tables of the naive Fixed Effects model with clustered standard errors as well as the Driscoll and Kraay (1998) estimator in Table 3.8 and 3.9, respectively.

Several concerns may challenge the robustness of our IV estimation. For one, standard errors on IV estimates are likely to be larger than that of OLS estimates. They can be much larger if the excluded instrumental variable is only weakly correlated with the endogenous regressors. Moreover, estimates based on weak instruments are biased in the same direction as their OLS counterparts and may not be consistent (Chao and Swanson, 2005). We illustrate that our empirical setup does not suffer from these problems. The first stage regression results shown in Table 3.18 demonstrate that the instruments predict plant-level energy prices for both energy sources. In either case, no energy cost pass-through on prices can be observed. This is as expected. Increases in fossil fuel prices may increase the electricity demand (as electricity may require fuel for its generation and could lead to higher costs being passed on to electricity prices. However, this is an unlikely scenario in the context of Oman, given its pre-set energy prices. Conversely, a pass-through to higher fossil fuel prices would not be expected for similar reasons. As a second check for our instrumental variable setup, we follow the recommendations in (Chao and Swanson, 2005) and provide test statistics as part of the regression tables to illustrate that none of our models suffer from weak identification.

The following section presents the FE-IV results alongside the CCE estimations. A general observation across our estimated models is that the IV estimator has amplified the fuel coefficients compared to OLS. This indicates that endogeneity in our framework is biasing the estimation towards zero. At the same time, the similarity between the results obtained through the FE-IV and CCE estimators further indicates that much of the unobserved effects appear to influence Omani manufacturing firms relatively homogeneously.

3.6.1 Main results

Performance effect. As seen in Table 3.5, an increase in fossil fuel prices leads to a rise in all the competitive indicators. The results hold for different forms of labour and total factor productivity across model specifications. For example, a ten per cent increase in fuel prices leads to a rise in labour productivity between 4.1 per cent to 4.6 per cent for the FE-IV (3.4 per cent to 3.8 per cent for the CCE). We find a somewhat higher result for our measure of total factor productivity.

The results for electricity are all found to be both negative and insignificant. These findings are by and large in line with findings in the literature which report a negative impact of electricity price hikes on performance (Abeberese, 2017; Marin and Vona, 2021). The lack of statistical significance of electricity price increases is confirmed by Calì et al. (2022). While the authors identify technology diffusion mechanics behind these results in their study, our analysis indicates that the effect can be mainly explained through modernisation and technology upgrading on the firm's level.

Table 3.5: Performance indicators Omani manufacturing firms

	Dependent variables:							
	LP^{va}		LP^{go}		TFP		GPM	
	(FE-IV)	(CCE)	(FE-IV)	(CCE)	(FE-IV)	(CCE)	(FE-IV)	(CCE)
Fossil Fuels	0.46*	0.34***	0.41**	0.38***	1.01*	0.96***	0.28*	0.28***
	(0.25)	(0.05)	(0.19)	(0.11)	(0.57)	(0.34)	(0.14)	(0.04)
Electricity	−0.55	-0.28	−0.41	-0.34	−0.45	-0.65	−0.11	-0.03
	(0.38)	(0.36)	(0.27)	(0.34)	(0.77)	(1.00)	(0.39)	(0.02)
Industry-time effects	Yes	.	Yes	.	Yes	.	Yes	.
Regional-time effects	Yes	.	Yes	.	Yes	.	Yes	.
Plant fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Common correlated effects	.	Yes	.	Yes	.	Yes	.	Yes
Observations	924	864	927	864	494	432	720	648
R^2	0.29	0.47	0.33	0.60	0.41	0.64	0.31	0.71
Weak identification test	106.14	.	107.40	.	51.72	.	65.10	.
CD test	.	0.01	.	0.01	.	0.02	.	0.01

Note: *p<0.1; **p<0.05; ***p<0.01. FE-IVs: Clustered standard errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005). Cross-sectional dependence (CD) test reports the Pesaran (2015) test where under the null hypothesis the residuals or the variable are weakly cross sectional dependent.

Upgrading effect. Similar to other advances in the literature, we identify a difference in the effects across energy sources in production: Whereas electricity price increases have no significant impact on a firm's performance, increases in fuel prices lead to an increase in performance indicators. Our rationale for this effect is as follows: For one, old technologies are more likely to be powered by fuel engines. In contrast, new technology is much more likely to be running on electricity.[23] On the other hand, increases in fossil fuel prices might also lead business managers to the decision to upgrade their production processes. This could either be done by replacing machinery or modernisation of the production process. As AIS provides detailed information on the sales and purchase behaviour of Omani manufacturing enterprises, Table 3.6 allows us to shed some light on this issue. Our results indicate that an increase in fossil fuel prices leads to a reduction in the sales and purchases of machinery. At first glance, this finding may be at odds with basic intuition. However, when considered in the context of the remaining results of Table 3.6, we argue that the results paint a straightforward story of capital upgrading. We report a positive effect on ICT-related investment, primarily consisting of electronic control devices, in response to a fossil fuel increase. In this sense, both results can be understood as a trade-off: Instead of simply replacing industrial machinery, Omani manufacturing industries invest in upgrading and integrating production processes. This trade-off could be due to financial constraints of the business, where upgrading production processes seems to offer the most attainable goal to retain competitiveness after an increase in input prices.

In a similar vein, the increase in gas-based energy prices may also force Omani enterprises to become more efficient in the implementation of their production processes by increasing the degree of computerisation and automation. More specifically, manufacturing firms in Oman may be forced to implement more efficient energy management systems that improve productivity through predictive maintenance. In this sense, the factor price differential of energy input prices forces Omani manufacturing firms to overcome technology-related lock-in effects recognised by the *Porter Hypothesis* literature. (Ley et al., 2016). The transmission channel of technology upgrading and its effect on productivity has also been recognised in the literature. For example, newly created ICTs offer improvement potential to management structures and the communication and processing of information, laying the groundwork for more efficient

[23] See for example Malek (2005) for a discussion on boiler technology.

work processes (Fouquet and Hippe, 2019). At the same time, computerisation allows production processes to be optimised to facilitate the creation of more efficient use of inputs. In this sense, TFP increases are not merely a reflection of marginal productivity gains passed on by efficient utilisation of energy inputs, but ICT also plays a fundamental role in the upgrading and modernisation of production processes as a whole.[24] The effect of productivity increases as a response to more stringent environmental policy is an established finding in the literature (Albrizio et al., 2014; Arlinghaus, 2015; Calì et al., 2022; Enevoldsen and Andersen, 2008; Rentschler and Kornejew, 2018, among others), and findings in the production economics literature confirm positive net effects arising from the implementation of smart manufacturing systems (SMS) in the areas of productivity, integration, transparency, flexibility and profitability (Kamble et al., 2020; Ndubisi et al., 2019, among others).

An important dimension is added to this point when considering the fragmentation of the Omani electricity grid we elaborated on. While particularly more modern firms are much more likely to already have new and electricity-power production machines, there is the possibility that the electricity generated to power their production processes was produced on-site. For this, there are two possible explanations: First, the electricity grid's fragmentation and dependence on a small/regional provider may create the need for a higher degree of energy independence. Secondly, on-site energy production may also result from purely cost-related considerations. Given the immense variability of fossil fuel prices, particularly in the earlier sample periods as Figure 3.1 illustrated, it may be more economical for a particular business to generate electricity in-house. If, in response to the increase in fossil fuel prices, a firm decided to source its electricity from the primary grid, it would need fewer machinery (as in fuel-powered generators) and more electronic control equipment for a successful switch.

Substitution effect. Our last building block is that of the substitution effect between electric and fossil fuels. As outlined before, we recognise this as the third and complementing transmission channel, which allows us to understand to what degree price interventions are capable of affecting the energy consump-

[24] See the talk by Shailendra Mugdal, Senior Vice President of BIO Intelligence Service at the side event *'ICTs and Climate Change: finding Solutions'* at the international climate negotiations in Poznań, Poland (COP 14) on the role of ICT.

tion input mix of manufacturing businesses in Oman. In this sense, analysing the trade-off between fuel and electricity empirically tests the substitutability or complementarity across both energy sources for the Omani manufacturing industry. Our results suggest a high degree of substitutability between the quantities of electricity and fossil fuel consumed by firms in response to increased fossil fuel prices. While a ten per cent increase in fuel prices for the FE-IV leads to a 4.7 per cent rise in the quantity of electricity, it also leads to a significant reduction of 3.6 per cent of fuels (in kWh equivalents). A similar effect cannot be observed for an electricity price hike. While we find an increase in the consumption of fuels, we find no statistical support for a substitution effect between both energy sources in this case. Interestingly enough, the substitution in energy quantities due to a fuel price hike does not translate one-to-one into efficiency gains: As illustrated in Table 3.6, we find a highly significant effect for fossil fuel but not for electricity efficiency.

Table 3.6: Turnover and efficiency indicators Omani manufacturing firms

	Dependent variables:											
	Machinery		*ICT*		*qEL/VA*		*qFF/VA*		*qEL*		*qFF*	
	(FE-IV)	(CCE)	(FE-IV)	(CCE)	(FE-IV)	(CCE)	(FE-IV)	(CCE)	(FE-IV)	(CCE)	(FE-IV)	(CCE)
Fossil Fuels	−1.40*	−1.81***	1.94*	1.82***	0.17	0.21	−0.90***	−0.48**	0.47*	0.89**	−0.36*	−0.24*
	(0.75)	(0.58)	(1.10)	(0.46)	(0.32)	(0.46)	(0.30)	(0.23)	(0.25)	(0.37)	(0.21)	(0.14)
Electricity	−0.72	−0.86	0.73	0.29	0.84	1.23	1.01**	0.79	0.80	0.43	0.60*	0.09
	(2.12)	(2.46)	(2.73)	(1.30)	(0.58)	(0.99)	(0.41)	(0.58)	(0.58)	(0.92)	(0.32)	(0.63)
Industry-time effects	Yes	.	Yes	.	Yes	.	Yes	.	Yes	.	Yes	.
Regional-time effects	Yes	.	Yes	.	Yes	.	Yes	.	Yes	.	Yes	.
Plant fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Common correlated effects	No	Yes	No	Yes	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Observations	430	430	209	241	740	715	758	745	761	745	761	745
R^2	0.23	0.62	0.44	0.78	0.25	0.745	0.23	0.68	0.20	0.58	0.20	0.34
Weak identification test	85.80	.	31.35	.	103.84	.	106.14	.	107.40	.	107.40	.
CD test	.	0.16	.	1.60	.	-0.94	.	0.94	.	0.01	.	0.21

Note: *p<0.1; **p<0.05; ***p<0.01. FE-IVs: Clustered standard errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005). Cross-sectional dependence (CD) test reports the Pesaran (2015) test where under the null hypothesis the residuals or the variable are weakly cross sectional dependent.

3.6.2 Robustness

In the subsequent section, we discuss the statistical and economic robustness of our findings. While the former is dedicated to deviations of the baseline modelling framework, the latter explores changes in the size and direction of the effects based on sub-sample analyses. As far as the economic robustness analysis is concerned, we now touch on three particular issues. One is the negative effect of fuel prices on investments on machinery turnover, which may seem at odds with the positive impact on performance which we put into perspective by considering a sub-set of typically fuel-powered manufacturing industries. The second robustness check illustrates that the effect of sample attrition has no far-reaching consequences on our results. Lastly, we demonstrate that the dynamics we have identified and described so far can be closely linked to the period after the change in the fossil fuel policy.

Multicollinearity of independent variables. As seen in Table 3.7, the main empirical findings are robust to two typologies of multicollinearity tests, i.e., cross-correlation across variables and the Variance Inflation Factor (VIF) between explanatory variables across all model specifications. The cross-correlation between our fossil fuel and electricity unit price indices is roughly 0.25. The objective of the VIF analysis is to identify variables that inflate the variance of the estimated coefficients, which is a typical sign of multicollinearity (Cantore and Cheng, 2018; Costantini and Mazzanti, 2012). Table 3.7 indicates that the VIF scores lie well within the acceptable benchmark (between 1 and 10) as discussed in Hamilton (2012); Qi et al. (2014) thereby lending support to the notion that there is no need to remove any explanatory variable because of multicollinearity.

Lagged endogenous variables. As noted in Ambec et al. (2013) a large proportion of studies on the *Porter Hypothesis* have not accounted for the potentially dynamic nature of the relationship between more stringent environmental policies and innovations and productivity.[25] We take this argument into account by also testing a lagged version of Equation 3.5.1:

[25] Ambec et al. (2013, p. 11): *'Most previous studies of the determinants of productivity have regressed productivity at time 0 on proxies of the stringency of environmental regulation at time 0 as well, which does not allow time for the innovation process to occur.'*

$$y_{it} = \beta_i' \mathbf{x}_{it-1} + \psi_{it}$$

Apart from addressing the conceptual issue of the dynamic relationship between interventions and firm-level outcomes, this specification also allows us to address the issue of the simultaneity bias: By lagging the suspected endogenous covariates by one period, it is reasonable to assume that the economic outcome variables of interests in period t affect energy unit prices in $t-1$. We report the result for the FE-IV regression with lagged explanatory variables in Table 3.11 as well as the fixed effects counterparts with Driscoll and Kraay (1998) standard errors in Table 3.10 both of which provide support to the main findings that magnitude and signs contained in Table 3.5 and 3.6 are robust.

Fuel-intensive industries. The negative effect of fuel prices on investments on machinery turnover appears to be at odds with the positive impact on performance. However, this negative effect might be driven by machinery typically fuel powered, such as construction machinery, cranes, forklift equipment, and mining machinery. We check for this effect by identifying manufacturing industries characterised by high use of such machinery. We identify these industries by calculating their average electricity and fuel share (in total energy consumption) over time and region and across manufacturing industries following the IV-digit ISIC classification. We then select industries with an above-average share of fossil fuel in total energy consumption as the fuel-intensive sub-sample. The results in Table 3.12 and Table 3.13 confirm that the previously identified effects are particularly pronounced for the sample of fuel-intensive industries. In this sense, the negative coefficient on machinery in Table 3.13 column (1) suggests that fuel price hikes generate the strongest effects in the industries that are most exposed to this particular kind of input which is a highly plausible result.

Quasi hard-balanced sub-sample. For this piece's final empirical inquiry, we perform the same set of statistical tests on a sub-sample of our data. Unlike previous robustness checks, which tried to characterise a set of industries that are particularly strongly affected by changes in the input price structure, we now illustrate that our findings are not the result of particular entry or exit dynamics of Omani manufacturing firms.

As discussed in the opening paragraphs of this piece and illustrated in Table 3.4, our sample suffers from attrition over the years. Even though the attrition rate is not particularly high from year to year, a strict hard-balancing of our sample would lead to a loss of somewhat over 50 per cent of our observations. Based on our identification strategy, this loss in total observations would render inference based on our instrumental variable approach as not too reliable. To account for this while still retaining the objective of accounting for sample fluctuations, we quasi-hard-balance our sample by requiring a firm to have at least four (consecutive) years' worth of observations to be considered for this particular sub-sample analysis. This way, we retain around 80per cent of all possible data points of our full sample[26] while also guaranteeing that we observe at least one year after/during the change in the fossil fuel price intervention per enterprise.

As seen in Table 3.14 the results of the main study remain intact or become even stronger. The only exception is the result for TFP in Table 3.14 (3) which has now become borderline insignificant. On the other hand, electricity price hikes are now also found to affect (output-based) labour productivity adversely; see Table 3.14 (2). We also confirm the main results based on the balanced sub-sample for efficiency and turnover as shown in Table 3.15: None of the coefficients has switched signs, and all results remain close to the main results in terms of their economic significance.

Post-intervention period. In the previous discussion, we have repeatedly linked the change in the fossil fuel policy strategy of the Omani government with firm-level productivity, upgrading and substitution dynamics. Even though our empirical strategy is not set up to incorporate an event study-like design or allows us to invoke a natural experiment-style analysis, we provide evidence in Table 3.16 and Table 3.17 that the previously discussed dynamics are particularly pronounced after 2014, i.e. when the change in the fuel policy occurred. We tend to see an even stronger postulated effect when comparing the results of the post-intervention period with that of the main findings. We also identify significantly negative effects for all but one performance measure in response to an electricity price increase (Table 3.16). As far as the turnover and efficiency indicators are concerned, they are perfectly in line with the previously observed

[26]We concede that, as a result of missing values in the reported queries, the actual sample size per model can be significantly lower.

dynamics and appear, in terms of magnitude, to be somewhat bigger as well.

3.7 Conclusion

Energy is one of the most critical inputs in a production process. It has immediate effects on a firm's competitiveness and carries notable strategic policy implications. Consequently, government-led price interventions in the energy market can have far-reaching consequences on the competitiveness of a particular industry. This is especially true for industries of countries that enter the global market on the cost-competitiveness front and have a natural advantage due to the abundance of relevant resources. At the same time, the public debate on fossil fuel energy subsidies has primarily been governed by two arguments. The economic line of argument has gravitated towards the rationale of cost-competitiveness: The reduction of emissions requires a cutback on energy consumption which increases the use of inputs and growth; increases in fossil fuel prices, therefore, harm competitiveness. On the other hand, the environmental argument stressed the importance of cost transparency and the effect of externalities. However, a new and growing body of research has also emerged, which adds a second layer to the economic argument and shows that reducing energy consumption may not necessarily be detrimental to growth.

This paper addresses challenges faced by much of the earlier empirical work on the *Porter Hypothesis* by providing an empirical account of multiple firm-level cost-coping mechanisms which have primarily been unattainable to explore in earlier work, and linking them to existing bodies of research on the impact of ICT and SMS systems on firm-level outcomes. The policy interventions of the Omani government further allow us to analyse the effect of prices of energy sources on productivity, performance and efficiency. We argue that the case of Oman may serve as an interesting reference point for future market interventions in oil-rich Middle Eastern economies in the wake of global climate change concerns. Using a novel firm-level micro data set on Omani manufacturing enterprises, our work shows that increases in fossil fuel energy input prices lead to productivity, efficiency, and substantial business upgrading. At the same time, we fail to report similar effects for an increase in electricity input prices. We identify an implicit transmission channel that operates through the modernisation and upgrading of a firm's production processes through implementing digital technology and ICT equipment. Moreover, we also find a

strong substitution effect from fossil fuels to electricity through a unit price increase in fuels but not the other way around. Therefore, our results indicate that subsidy reforms may not only be used to achieve environmental goals but may also drive upgrading and modernisation processes of firms that can ultimately improve economic performance.

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Appendix Chapter 3

3.A Complementary tables

Table 3.7: Multicollinearity

Variance Inflation Factors		
	Fossil Fuels	Electricity
LP^{va}	1.559	2.123
LP^{go}	1.552	2.115
TFP	1.816	2.401
GPM	1.578	2.337
$Machinery$	1.675	2.980
ICT	1.763	3.006
qEL/VA	1.566	2.130
qFF/VA	1.559	2.123
qEL	1.552	2.115
qFF	1.561	2.237
Pearson Correlation		
	Fossil Fuels	Electricity
Fossil Fuels	1	
Electricity	0.248	1

Note: VIF scores excluding fixed effects. Acceptable VIF benchmark between 1 and 10 (Hamilton, 2012; Qi et al., 2014).

Table 3.8: Fixed Effects Estimation

	Dependent variables:									
	LP^{va}	LP^{go}	TFP	GPM	$Machinery$	ICT	qEL/VA	qFF/VA	qEL	qFF
Fossil Fuel	0.20	0.17*	0.83***	0.27**	-0.19	2.38***	0.18	-0.73***	0.44**	-0.47**
	(0.13)	(0.10)	(0.31)	(0.13)	(0.60)	(0.70)	(0.20)	(0.23)	(0.19)	(0.21)
Electricity	-0.36	-0.15	-0.63	-0.23	0.34	3.32**	-0.55	0.63	-0.86*	0.23
	(0.27)	(0.18)	(0.52)	(0.24)	(1.35)	(1.30)	(0.51)	(0.41)	(0.49)	(0.35)
Industry-time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional-time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plant fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	929	932	495	725	539	288	912	929	932	932
R^2	0.297	0.348	0.413	0.316	0.252	0.432	0.276	0.240	0.250	0.202

Note: *p<0.1; **p<0.05; ***p<0.01. Clustered standard errors in parentheses.

Table 3.9: Fixed Effects Estimation Driscoll and Kraay (1998) Standard Errors

	Dependent variables:									
	LP^{va}	LP^{go}	TFP	GPM	$Machinery$	ICT	qEL/VA	qFF/VA	qEL	qFF
Fossil Fuel	0.20**	0.17*	0.83**	0.27**	-0.19	2.38***	0.18	-0.73**	0.44*	-0.47**
	(0.06)	(0.07)	(0.21)	(0.08)	(0.24)	(0.52)	(0.20)	(0.19)	(0.18)	(0.17)
Electricity	-0.36**	-0.15	-0.63*	-0.23*	0.34	3.32***	-0.55	0.63*	-0.86	0.23
	(0.14)	(0.11)	(0.29)	(0.10)	(0.84)	(0.50)	(0.53)	(0.27)	(0.54)	(0.33)
Industry-time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional-time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plant fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	929	932	495	725	539	288	912	929	932	932

Note: *p<0.1; **p<0.05; ***p<0.01. Driscoll and Kraay (1998) standard errors in parentheses.

Table 3.10: Lagged Fixed Effects Estimation Driscoll and Kraay (1998) Standard Errors

	Dependent variables:									
	LP^{va}	LP^{go}	TFP	GPM	$Machinery$	ICT	qEL/VA	qFF/VA	qEL	qFF
Fossil Fuel$_{(t-1)}$	0.18*	0.14	0.85**	0.22*	-0.39	2.41***	0.32	-0.70**	0.56**	-0.47*
	(0.08)	(0.09)	(0.24)	(0.09)	(0.23)	(0.49)	(0.23)	(0.23)	(0.21)	(0.21)
Electricity$_{(t-1)}$	-0.34**	-0.14	-0.75*	-0.13	0.54	3.29***	-0.54	0.47*	-0.79	0.13
	(0.10)	(0.07)	(0.29)	(0.11)	(0.84)	(0.51)	(0.55)	(0.22)	(0.54)	(0.23)
Industry-time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional-time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plant fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	768	770	415	620	472	261	753	768	770	770

Note: *p<0.1; **p<0.05; ***p<0.01. Driscoll and Kraay (1998) standard errors in parentheses.

Table 3.11: Lagged Fixed Effects IV Estimation

	Dependent variables:									
	LP^{va}	LP^{go}	TFP	GPM	$Machinery$	ICT	qEL/VA	qFF/VA	qEL	qFF
Fossil Fuel$_{(t-1)}$	0.47*	0.42**	0.95	0.25*	-1.44*	1.98*	0.15	-0.91***	0.48*	-0.34
	(0.25)	(0.20)	(0.60)	(0.15)	(0.74)	(1.16)	(0.32)	(0.31)	(0.25)	(0.22)
Electricity$_{(t-1)}$	-0.54	-0.41	-0.40	-0.08	-0.24	1.34	0.89	0.89**	0.88	0.52
	(0.39)	(0.27)	(0.77)	(0.40)	(1.96)	(2.42)	(0.59)	(0.41)	(0.60)	(0.34)
Industry-time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional-time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plant fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	746	749	375	544	426	209	730	746	749	749
R^2	0.232	0.245	0.412	0.319	0.233	0.420	0.253	0.221	0.205	0.193
Weak identification test	104.73	105.98	51.25	64.20	85.10	31.35	102.42	104.73	105.98	105.98

Note: *p<0.1; **p<0.05; ***p<0.01. Clustered standard errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).

Table 3.12: Instrumental Variables fuel-intensive sub-sample: Performance Indicators

	Dependent variable:			
	LP^{va}	LP^{go}	TFP	GPM
Fossil Fuels	0.49**	0.42**	1.07*	0.30**
	(0.24)	(0.18)	(0.55)	(0.14)
Electricity	−0.56	−0.41	−0.63	−0.11
	(0.37)	(0.26)	(0.79)	(0.39)
Plant fixed effects	Yes	Yes	Yes	Yes
Industry-time effects	Yes	Yes	Yes	Yes
Regional-time effects	Yes	Yes	Yes	Yes
Observations	895	898	473	696
R^2	0.29	0.34	0.42	0.30
Weak identification test	88.20	42.84	98.12	157.34

Note: *p<0.1; **p<0.05; ***p<0.01.
FE-IVs: Clustered standard errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).

Table 3.13: Instrumental variables fuel-intensive sub-sample: Turnover and Efficiency Indicators

	Dependent variable:					
	$Machinery$	ICT	qEL/VA	qFF/VA	qEL	qFF
Fossil Fuels	−1.30*	2.54**	−0.11	−0.94***	0.47*	−0.35*
	(0.76)	(1.05)	(0.34)	(0.29)	(0.25)	(0.21)
Electricity	−0.81	0.79	1.18*	1.01**	0.76	0.57*
	(2.14)	(2.68)	(0.67)	(0.40)	(0.58)	(0.32)
Plant fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-time effects	Yes	Yes	Yes	Yes	Yes	Yes
Regional-time effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	531	285	895	895	898	898
R^2	0.21	0.48	0.24	0.23	0.20	0.19
Weak identification test	125.15	67.68	138.46	107.23	76.52	129.55

Note: *p<0.1; **p<0.05; ***p<0.01. FE-IVs: Clustered standard errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).

Table 3.14: Instrumental Variables quasi hard-balanced sub-sample: Performance Indicators

	Dependent variable:			
	LP^{va}	LP^{go}	TFP	GPM
Fossil Fuels	0.43*	0.38**	0.82	0.32**
	(0.25)	(0.19)	(0.58)	(0.15)
Electricity	−0.61	−0.48*	−0.33	−0.08
	(0.39)	(0.28)	(0.80)	(0.42)
Plant fixed effects	Yes	Yes	Yes	Yes
Industry-time effects	Yes	Yes	Yes	Yes
Regional-time effects	Yes	Yes	Yes	Yes
Observations	863	866	463	669
R^2	0.23	0.25	0.41	0.32
Weak identification test	65.82	73.34	64.34	102.40

Note: *p<0.1; **p<0.05; ***p<0.01.
FE-IVs: Clustered standard errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).

Table 3.15: Instrumental variables quasi hard-balanced subsample: Turnover and Efficiency Indicators

	Dependent variable:					
	$Machinery$	ICT	qEL/VA	qFF/VA	qEL	qFF
Fossil Fuels	−1.32*	2.04*	0.01	−0.90***	0.50*	−0.39*
	(0.76)	(1.14)	(0.35)	(0.31)	(0.26)	(0.23)
Electricity	−1.58	0.63	1.61**	0.92**	1.25*	0.55*
	(2.45)	(2.74)	(0.76)	(0.42)	(0.71)	(0.32)
Plant fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-time effects	Yes	Yes	Yes	Yes	Yes	Yes
Regional-time effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	529	284	863	863	866	866
R^2	0.23	0.44	0.22	0.22	0.19	0.19
Weak identification test	105.59	102.44	81.43	101.71	127.95	98.23

Note: *p<0.1; **p<0.05; ***p<0.01. FE-IVs: Clustered standard errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).

Table 3.16: Instrumental Variables post-2014 sub-sample: Performance Indicators

	Dependent variable:			
	LP^{va}	LP^{go}	TFP	GPM
Fossil Fuels	0.82*	0.72***	1.29*	0.48*
	(0.42)	(0.28)	(0.68)	(0.28)
Electricity	−0.87*	−0.60*	−2.17***	−0.41
	(0.47)	(0.35)	(0.78)	(0.52)
Plant fixed effects	Yes	Yes	Yes	Yes
Industry-time effects	Yes	Yes	Yes	Yes
Regional-time effects	Yes	Yes	Yes	Yes
Observations	526	527	292	423
R^2	0.35	0.37	0.46	0.27
Weak identification test	58.76	116.97	97.58	70.03

Note: *p<0.1; **p<0.05; ***p<0.01.
FE-IVs: Clustered standard errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).

Table 3.17: Instrumental variables post-2014 sub-sample: Turnover and Efficiency Indicators

	Dependent variable:					
	$Machinery$	ICT	qEL/VA	qFF/VA	qEL	qFF
Fossil Fuels	−1.44*	3.48*	−0.46	−1.33**	0.54*	−0.35*
	(0.85)	(2.08)	(0.59)	(0.53)	(0.32)	(0.20)
Electricity	−0.89	0.47	1.34	1.45***	0.57	0.70*
	(3.65)	(3.20)	(0.89)	(0.51)	(0.76)	(0.42)
Plant fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-time effects	Yes	Yes	Yes	Yes	Yes	Yes
Regional-time effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	318	178	526	526	527	527
R^2	0.26	0.40	0.25	0.24	0.24	0.12
Weak identification test	87.35	45.67	104.36	101.30	71.11	104.17

Note: *p<0.1; **p<0.05; ***p<0.01. FE-IVs: Clustered standard errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).

Table 3.18: Fixed Effects IV: First stage regression

	Dependent variable:	
	Fossil fuels	Eletricity
IV Fossil Fuels	0.353**	0.016
	(0.149)	(0.089)
IV Eletricity	0.479	0.372***
	(0.307)	(0.183)
Plant fixed effects	Yes	Yes
Industry-time effects	Yes	Yes
Regional-time effects	Yes	Yes
Observations	927	927
R^2	0.412	0.632
F Statistic	4.427	10.828

Note: *p<0.1; **p<0.05; ***p<0.01.
Clustered standard errors in parentheses.

3.B The Omani Annual Industrial Statistical Survey

3.B.1 Overview

The Omani Annual Industrial Statistical Survey (OAISS) was designed in accordance with UNIDO (2019) and surveys small, medium and large business engaged with manufacturing activities which are licensed with the Ministry of Commerce and Industry of the Sultanate of Oman. The sampling framework to conduct the survey is based on the commercial registry which is updated on an annual basis and maintained by the Ministry. The commercial registry contains information of active establishments by economic sectors; establishments not licensed were not covered. Micro establishments employing less than ten persons engaged are also not included in the survey.[27] Estimates for non-responding establishments were made based on their activity and employment size.

Data collection was undertaken by a trained team of enumerators and supervisors in the ministry. The survey questionnaires were disseminated to sam-

[27]The Ministry is advised to increase the coverage of the survey in the future to include these establishments as well as the small ones in order to have a global picture of the whole manufacturing sector (UNIDO, 2019).

pling units by field staff of the ministry along with information and guidance on the filling in of the questionnaire; see 3.B.2 for more information and limitations of industrial surveys and questionnaires). Respondents were guided to provide data from their financial accounting records. Completed questionnaires were edited and coded by staff in the Ministry and entered into the Industrial Statistics System. Entered data were checked for accuracy and consistency using broad parameters including technical and operation ratios in the field of industrial statistics.[28] A control table comprising these parameters was regularly generated and analyzed by the UNIDO Statistical Expert. A consistency check using broad parameters was applied to a subset of production variables which are essential to international statistics reporting practices which does, however, not include all variables of the Industrial Statistical Survey.

3.B.2 The Questionnaire

Purpose The Omani Annual Industrial Statistical Survey (OAISS) is aimed at producing basic statistics for the manufacturing sector in Oman. It provides information about the composition and structure of the organized manufacturing sector. More specifically, the survey is used to (i) collect statistics on the operating characteristics and structure of manufacturing establishments operating in Oman; (ii) assess the contribution of the manufacturing sector in the overall economy; (iii) provide several statistics[29] (iv) obtain benchmark data for computing national accounts estimates by the Ministry of National Economy and (v) obtain benchmark data for the construction of Input-Output tables.

Unit of inquiry The statistical unit used for the census was an establishment defined as an economic unit, which engages under a single ownership or control in one, or predominantly one kind of economic activity at a single physical location. However, for certain firms which could not provide separate data on an establishment basis, the enterprise was used as the unit of inquiry.

[28] As of 2019, the minimum wage in the Sultanate of Oman remains at 325 Omani Rial. During the data entry stage, if the average compensation per employee of an establishment is detected to be lower than 325 Omani Rial, the labor compensation section of that establishment will be rejected by the Industrial Statistical System and reviewed again by the enumerator.

[29] With the purpose of (iii.1) monitoring the performance of the manufacturing sector with current plans and strategies adopted by the Ministry; (iii.2) designing industrial development plans, policies and programs; (iii.3) carrying out studies on new industrial projects and (iii.4) analyzing the performance of the different industrial sub-sectors and establishments.

Design The questionnaire was designed according to 'The International Recommendations for Industrial Statistics' of the United Nations and took into consideration the requirements of the Ministry of Commerce and Industry (MOCI). The questionnaire was drafted by the UNIDO expert and finalized after consultation with the Ministry. It was later translated into Arabic by the Ministry of Commerce and Industry.

Limitations of the statistics presented Although field staff was briefed extensively, it was found that the data still had certain inconsistencies which likely stem from insufficient understanding of the terms and concepts applied. As far as possible, these inconsistencies have been corrected in the editing stage. However, there is a tendency among establishments to hide information relating mainly to output and to over-report on inputs. There is a belief that information supplied would be transmitted to the income tax authorities. The regular conduct of such an exercise would help in dissipating this fear and at the same time improve the quality and reliability of the information supplied. Furthermore, a number of establishments are engaged in several equally important but dissimilar activities. Because of non-availability of separate records, these establishments have been classified according to the activity with the highest output. Figures relating to a particular ISIC group might, therefore, include data for other secondary activities as well. In addition, many establishments could not provide detailed information for certain items such as: (i) consumption of electricity, water and fuels separately; (ii) purchase of raw materials and sales from own production by main product (iii) value and/or stocks of stocks as well as value of fixed assets by type. These and other missing data had to be estimated. Therefore, attention should be given when using figures at a more granular level; e.g., components of gross output, detailed intermediate consumption, electricity, water and fuels consumed, etc. Lastly, in the absence of adequate details on products, it has not been possible to produce reliable tables by detailed product group for sales and purchases by the establishments. These tables are presented on the broad section level of the Central Product Classification (CPC).

3.C Construction of unit price series

In this section we describe a simple algorithm to evaluate and correct potential issues related to the missing or wrongfully reported fossil fuel units in

Table 3.19: Fossil fuel unit conversion

		Unit conversion		
Fossil fuel	Unit measure (um)	um/kg	um/m^3	um/kWh
Gasoline	liter	0.75	0.001	10.00
Diesel	liter	0.85	0.001	9.10
Kerosene	liter	0.80	0.001	10.35
LPG	liter	0.51	0.270	6.90
Gas	cubic meter (m^3)	0.51	0.270	8.80

our data. The following steps were taken

1. For all fossil fuels, only consider the sub-sample of the data which correctly lists the quantity, and value as well as unit of a particular purchased fossil fuel type.

2. Based on this sub-sample, calculate the trimmed median and median absolute deviation (MAD) of the unit price ($value/quantity$) for each fossil fuel type.

3. For the sample where the unit was not recorded (i.e. is missing) with the correct unit of measurement but price and quantity were stated, evaluate if the unit price lies within $+/-\ x\ \times MAD$ of the previously calculated sub-sample with $x = 1$.

    - If so, enforce the correct unit of measurement of the sub-sample with previously missing quantities.
    - If not, check if for observations with an incorrect unit of measurement, a conversion of the units (Table 3.19) would bring the unit price within the range of acceptance:
        - If so, units were converted.
        - If not, observations were dropped.

CHAPTER

FOUR

WHAT ARE THE EFFECTS OF ENERGY PRICE INCREASES ON FIRM-LEVEL OUTCOMES? A SURVEY OF THE FIELD AND NEW EVIDENCE FROM CHILE

Abstract This paper contributes to the microdata firm-level literature on the productivity-energy price nexus in three ways. First, it provides a systematic literature review of the most recent advancements in the field. It highlights strong focus on advanced economies and also notes a lack of extensive work on the heterogeneous effects of these price hikes, particularly different energy types. Secondly, the paper introduces the Chilean Annual National Industrial Survey data to expand the coverage of firm-level surveys dedicated to analysing energy prices on firm performance. Following the literature's modelling convention, our results show that environmental regulations increase Chilean firms' competitiveness. Furthermore, we find further support for a wider range of firm-level coping strategies previously identified in the literature for the case of Chile. Finally, the paper expands on the importance of disaggregated analyses by highlighting changes in the response mechanisms across multiple firm-level characteristics and energy types.

Key words: Productivity, Fossil fuels, Electricity, Chile
JEL classifications: L60, O13, O14, Q40, Q55

4.1 Introduction

Transitioning to net-zero emissions will entail a structural transformation beyond the climate policy toolbox. Horizontal policies that foster competition, reduce distortions in factor and product markets, and encourage innovation. Firm growth will ease the transition to net zero. However, while decarbonisation will require massive upgrading of firm capabilities most climate policies do not adequately address the micro-level market failures preventing firms from adopting green management practices or technology.

For now, empirical work has focused on a relatively small selection of countries. However, these efforts have already unveiled notably heterogeneous effects energy price surges have on firm performance. These are, among others, dependant on the type of energy inputs subject to a price hike (Amann et al., 2021; Calì et al., 2022) and hint at various degrees of substitutability between distinct production inputs conditional on firm characteristics (Brucal and Dechezleprêtre, 2021, among others). At the same time, assessing the effects of input price variation on firm-level outcomes has posed a broad challenge to the field, most notably because of the rather extensive requirements this puts on the data. Because of this, empirical work on this topic has remained relatively scarce, and, most damningly, little is known about the firm-level effect of green transitioning in developing countries (Burke et al., 2016).

This paper adds to the field along three lines. First, it provides a systematic literature review of the most recent advancements in the field and highlights notable research focus on advanced economies. It also notes a lack of extensive work on the heterogeneous effects of these price hikes, particularly different energy types. Secondly, the paper introduces the Chilean Annual National Industrial Survey data to expand the coverage of firm-level surveys dedicated to analysing energy prices on firm performance. Our results show that environmental regulations will increase Chilean firms' competitiveness. Furthermore, we confirm various other firm-level coping strategies previously identified in the literature for the case of Chile. Our results suggest fossil fuel price increases are not detrimental to firm performance, a pattern consistent with findings in other countries. We also find that electricity price increases lead to reduced production and workforce reduction. On the other hand, fossil fuel hikes result in increased capital investment, which manifests itself in improved productivity, thereby supporting the *strong* version of the Porter Hypothesis. However, these

productivity increases do not translate into higher profitability (at least not in the short-run), indicating that such price hikes are absorbed by firms, at least to a considerable extent. Given the current data limitations, it remains unclear to what degree Chilean manufacturing firms can pass some of the energy price increases on to their customers. Also, it remains a question for future research on how energy price hikes manifest themselves in firm exits. Finally, the paper expands on the importance of disaggregated analyses by highlighting changes in the response mechanisms across multiple firm-level characteristics and energy types. Most importantly, the heterogeneity analysis in this paper indicates that the strong version of the Porter Hypothesis can only be observed for large firms (with 50 employees or above) and that small firms are more negatively affected by surges in energy prices.

The structure of this paper is as follows: In section 4.2, we provide an overview of the firm-level micro literature on the firm-level energy-productivity nexus before discussing the institutional setting of the Chilean energy market overlapping with our sample period in section 4.3. We then introduce the *Estructura de la industria manufacturera* (INE, 2015) in section 4.4, after which we turn to our empirical strategy and the results in section 4.5 and 4.6, respectively, before concluding in section 4.7.

4.2 Literature

According to mainstream economic beliefs, the costs of production inputs, such as energy, labour, and capital, directly affect the price of outputs. High prices, in turn, impact marked demand and sales and, consequently, the performance and competitiveness of firms. As firms maximise profits while taking production technologies and consumers' preferences as given, increasing firms' input prices will consequently reduce their profit-maximising output level. In other words, any external price intervention will interfere with firms' maximisation strategy and negatively affect production output.

Governments may therefore use energy price interventions to facilitate the economic competitiveness of specific sectors, for example, to promote energy-intensive and export-focused industries. Historically, substantial post-tax energy subsidies (Coady et al., 2017) have been observed alongside gradual increases in the labour tax burden over time (Bachas et al., 2022). However, this notable degree of government intervention affects the free choices of firms

and their optimal allocations of inputs: Artificially lowered energy prices are matched with a high tax burden on labour, thereby intervening in the optimisation process of the firm. Therefore, any energy price reform which aims to restore cost transparency across input factors may restore market powers and increase the relative price of energy vis-a-vis other input factors, such as labour and has the potential to result in more market-efficient outcomes.

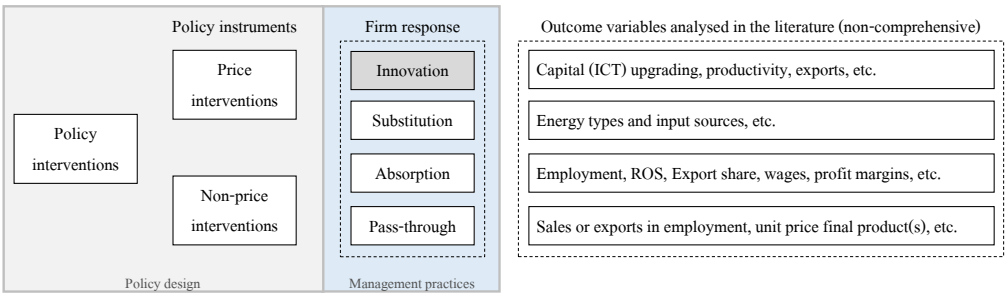
Furthermore, when analysing the impact of energy prices on firms' production, it is a crucial observation that market prices may, in fact, not necessarily incorporate the total societal cost of energy production and utilisation. The reason for this lack of cost transparency lies in the existence of externalities that may distort the accurate price of energy-related production inputs (Ley et al., 2016). This argument gives rise to the view that price interventions should accurately reflect the socio-economic costs associated with the production process. Governments may also be inclined to actively steer their energy subsidy strategies to differentiate between various types of energy (e.g., green vs fossil fuel sources) in alignment with their climate agenda (Monasterolo and Raberto, 2019).

In addition to distorting the market allocation process, energy price interventions through subsidies can lead to several unintended and undesired economic consequences and costs. For example, low energy prices may encourage excessive consumption and inhibit energy efficiency. Keeping energy prices low may thus discourage investment in more energy-efficient capital which ultimately hurts a firm's competitiveness and may furthermore foster the utilisation of and research in technologies that would not be undertaken in a more cost-transparent environment (Ley et al., 2016).

This argument gives rise to the view that some energy price interventions may be used to facilitate the economic competitiveness of certain aspects of the economy. Among the most prominent proponents of this argument are Porter and Van der Linde (Porter, 1991; Porter and Van der Linde, 1995). They put forward the idea that government intervention may spur innovation while potentially also increasing firms' productivity. This coping strategy, where firms *innovate* by adopting new and more efficient technologies and ripe economic benefit, is often referred to as the *Porter Hypothesis*.[1]

[1]The literature loosely distinguishes between three prominent forms of the Porter Hypothesis. Under the *"weak"* form of the Porter Hypothesis, environmental regulations spur innovations, while under the *"strong"* version of the Porter Hypothesis, environmental regulations

Figure 4.1: Firm-level coping framework



Note: Porter-type innovation hypothesis highlighted in grey.
Source: Author's elaboration based on Ambec et al. (2013); Coste et al. (2018); Dechezleprêtre and Sato (2017); Rentschler et al. (2017).

Firms may respond in multiple ways to such interventionist measures. As illustrated by Coste et al. (2018); Rentschler et al. (2017), in addition to *innovation*, firms may pursue other coping strategies such as *passing costs on* to customers and other firms, cost *absorption* as well as adjustments to the production inputs, i.e. *substitution*. The subsequent literature review follows the framework introduced in Figure 4.1 and organises the pieces of literature reviewed along the four outlined coping strategies. This literature review summarises the most relevant papers, including their approach, findings and conclusions.[2] For all four coping strategies, a summary of the main findings is offered at the beginning of each section.

4.2.1 Innovation effect

The literature finds robust support for the Porter-type innovation hypothesis where policy-led price interventions lead to innovation and notable business upgrading. In most cases, such upgrading processes result in significant efficiency increases.[3] This process holds almost universally and is neither country- nor sector-specific. At the same time, price interventions do not necessarily

will lead to increases in firm competitiveness. Finally, as the third and final variant, the *"narrow"* version of the Porter hypothesis states that more flexible regulations may be better at providing preferable incentive structures to firms than more prescriptive forms; see Ambec et al. (2013); Cohen and Tubb (2018); Jaffe and Palmer (1997) for a more extensive discussion and meta-analysis of the literature.

[2]See, among others, Arlinghaus (2015); Cohen and Tubb (2018); Dechezleprêtre and Kruse (2018); Dechezleprêtre and Sato (2017); Ellis et al. (2019); Kozluk and Zipperer (2015).

[3]The extent to which this impacts demand elasticities is discussed in subsection 4.2.2.

translate directly to responses in firm performance: The literature overwhelmingly finds little evidence that energy price increases will erode firm performance. Instead, multiple studies indicate that firms' performance may not be affected or improve due to energy price increases. In short, the findings are in line with the *"weak"* form of the Porter Hypothesis, where environmental regulations spur innovations. At the same time, there is less support for the *"strong"* version of the Porter Hypothesis, where environmental regulations will lead to increases in firm competitiveness. Lastly, the literature also identifies managerial quality as a main driver behind firm-level innovation dynamics and emphasises sector-level heterogeneities in firm responses.

Energy price hikes foster firm-level innovation. Using US patent data for the period 1970 to 1994, Popp (2002) measures the effect of energy price increases on firm-level innovation. His findings confirm that energy price increases are a strong driver in explaining firms' proclivity to innovate and suggest that environmental taxation and regulations may be subject to a double-dividend where they reduce pollution and foster the development of new and greener technologies. Similarly, in a survey of 135 Dutch firms, De Groot et al. (2001) reports that firms are more likely to integrate technologies to improve energy efficiency into their business operations, primarily when driven by cost-saving financing incentives. Furthermore, the authors find strong sector- and firm-specific components explaining both investment behaviour and acceptance of policy interventions. Especially firm size, energy intensity and competitive position were critical distinguishing factors in explaining differences in behaviour and attitude towards policy. Confirming the importance of patents as a proxy for innovation, a similarly strong impact of clean patents on innovation is found by Dechezleprêtre et al. (2014). Analysing patent citations, the authors assert that the higher innovation potential of clean technologies creates more significant positive spillovers. The authors also argue that more robust support for green policies might positively affect economic growth and that a redirection of public funds may have a higher innovation potential than dirty technologies.

Focusing on the automotive sector, Aghion et al. (2016) assess the relationship between "clean" (such as electric, hybrid, and hydrogen) and "dirty" (internal combustion engine) innovation in the automotive sector. The authors find that firms are more likely to innovate in clean technologies in response to

fuel price hikes: Increases in fuel prices lead to a decrease in the generation of combustion-engine-type patents while positively affecting the generation of "clean" patents. Furthermore, they illustrate that both innovation history and aggregate spillovers explain and create path dependence in the type of innovation a firm pursues.

There is strong support for the *weak* Porter Hypothesis; evidence for its *strong* version is more ambiguous. Multiple studies have assessed the impact of carbon taxes in the context of the European Emissions Trading Schemes (EU ETS), which have sprung numerous reviews too; see among others Arlinghaus (2015); Dechezleprêtre et al. (2019); Dechezleprêtre and Kruse (2018); Joltreau and Sommerfeld (2019). The surveyed literature does confirm a significant and positive relationship between environmental performance, measured by emissions of pollutants or adoption of environmental standards, and economic indicators. In turn, environmental regulations are fond to consistently produce improved environmental outcomes (such as lower emission rates) while being, on average, not detrimental to the economic performance of firms. These results are confirmed by ample empirical evidence spanning single- as well as cross-country cases: In a micro-level analysis of German manufacturing firms, Flues and Lutz (2015) find indications that preferable taxation treatment for electricity does not translate into improved performance outcomes for firms subject to a lower rate. The turnover, exports, value-added, investment and employment results suggest that lower energy input prices may not necessarily improve economic outcomes. Wagner and Petrick (2014) find that German firms responded to the EU ETS regulation by reducing their carbon intensity and not by reducing the scale of their production. Similar positive results on emission reduction can be observed for France (Wagner et al., 2014), Norway (Klemetsen et al., 2020) and Lithuania (Jaraite-Kažukauske and Maria, 2016). While data limitations did not allow Wagner et al. (2014) to evaluate the impact on output, Klemetsen et al. (2020) report a significant and positive effect on value-added and productivity. Conversely, Jaraite-Kažukauske and Maria (2016) find evidence of modest capital upgrading but fail to report positive impacts on firm-level profitability. Finally, Martin et al. (2014) analyse the impact of a carbon tax on manufacturing plants using production census data for the UK. While the authors report that carbon taxation significantly adversely affected energy intensity and use, it did not negatively affect firm performance,

including gross output or productivity.

Through a pan-European lens, Dechezleprêtre et al. (2018) analyse the effects of the EU ETS across all countries covered by the EU scheme. The authors find support for the Porter hypothesis, where increased investment in carbon-saving technologies resulted in average productivity gains for regulated firms. Similarly, Calel and Dechezleprêtre (2016) find that the EU ETS has increased low-carbon innovation among regulated firms by as much as 10 per cent without crowding out patenting for other technologies. The authors also find that the EU ETS has not affected patenting beyond the set of regulated companies and find more significant innovation potential among chemicals manufacturers, energy companies, and automobile manufacturers.

Broadening the scope to a more heterogeneous group of countries, Ellis et al. (2019) reviews empirical work on the impact of carbon pricing on competitiveness in OECD and G20 countries for the electricity and industrial sectors. The authors summarise that most papers find no statistically significant effects of carbon pricing or energy prices on different dimensions. These include competitiveness, net imports, foreign direct investments, turnover, value-added, employment, profits, productivity, and innovation.

More recently, Hájek et al. (2019) find support for the notion that carbon prices can reduce emissions effectively in a pan-European study (including Sweden, Finland, Denmark, Ireland and Slovenia). In contrast, using regional data between 1998 to 2017 Arcila and Baker (2022) show that the carbon tax policy in the Canadian province of British Columbia did not have the desired effect as CO2 emissions and fossil fuel consumption have risen. Looking at a selection of companies listed on the Shanghai and Shenzhen stock exchanges between 2007 and 2019, Zhang and Kong (2022) show that renewable energy policies promote total factor productivity growth of energy firms. The authors identify efficiency improvements and technological innovation as the main drivers. Furthermore, the authors report some adverse effects of such renewable energy policies on the productivity levels of state-owned, large and high-equity firms.

Measurement issues play a notable role in assessing the strength of the innovation effect. Noting the mixed empirical evidence, Hille and Möbius (2019b) emphasise the importance of controlling for potential endogeneity of environmental regulation as productivity may also affect environmental regulations. For example, firms at the technological frontier may encourage the

implementation of stricter environmental regulations for strategic corporate reasons. Analysing the environmental regulation-productivity nexus across 14 manufacturing sectors of 28 OECD countries between 1995 and 2009, the authors find no support for the *strong* Porter Hypothesis where environmental regulations lead to productivity gains. Instead, their results confirm the *weak* version of the Porter Hypothesis, which positively relates environmental regulations to innovation.

While endogeneity concerns have been acknowledged in the field, evidence on the Porter Hypothesis remains mixed. For example, Rubashkina et al. (2015) analyse nine manufacturing sectors in 17 European economies between 1997 and 2009 and control for potential endogeneity of environmental regulations. Still, they fail to report evidence supporting the strong version of the Porter Hypothesis. Contrary to these findings, Franco and Marin (2017) present evidence in support of the regulation-productivity nexus using manufacturing sector data for eight European economies between 2001-2007.

While Porter-type innovation potential is widely established across developed and developing economies, heterogeneous impacts are observed based on certain firm characteristics. Albrizio et al. (2017) posit that the policy impact of environmental regulation may vary depending on the type of firm. The authors analyse the environmental stringency-productivity nexus using a cross-country panel data set of 17 OECD countries and ten manufacturing sectors over the period 1990-2009, as well as a firm-level data set comprised of 11 OECD countries with 22 manufacturing sectors between 2000 and 2009. The authors find that the most productive firms see a temporary productivity boost in response to environmental regulation, while a slowdown in productivity growth is found for less-productive firms.

Two of the most recent studies look at the French manufacturing sector. Dussaux (2020) explore the effect of energy price changes on output, investment, and employment and further focus on identifying heterogeneous response patterns based on firm size. The author finds a direct causal link between energy price variations on energy use and evidence of reallocation of production and workers from energy-intensive to energy-efficient firms. In the aggregate, the effect of energy price on output and investment is not statistically different from zero. The author also finds notably different effects depending on firm size: Large firms reduce their output by 2.6 per cent while medium-sized

firms do not change their output. Surprisingly, small firms increase their output by 1.4 per cent. Marin and Vona (2021) evaluates the effect of energy price increases on socio-economic performance indicators for French manufacturing firms. The authors report a significant causal link between higher energy prices and lower emissions while noting a small, negative impact on productivity. At the same time, the results hint at a particularly significant environmental trade-off in energy-intensive sectors that are exposed to international competition and larger firms.

Using administrative plant-level data from the US census of manufacturers, Ganapati et al. (2016) quantify the effect of price variation on the investment decisions of roughly 5,000 single-product plants in six US manufacturing industries producing homogenous products. The authors find that surges in energy prices reduce investment expenditures and that the magnitude of the negative effect of energy prices on firms' level of employment and productivity is higher in more energy-intensive sectors.

Similar findings can be observed in the developing world. Golder (2011) finds that the decline in energy intensity of Indian manufacturing post-1992 can be attributed to the better energy efficiency of energy-intensive industries. As the author argues, these may, in turn, be traced in part to hikes in the price of energy paid by manufacturing firms. At the same time, the author further asserts the high potential of further energy improvements given the considerable variation of energy prices across firms, and that particularly older and large(r) firms seem to inhibit low energy efficiency while significant energy efficiency spillover from foreign firms in such industries to local firms is also observed.

Sadath and Acharya (2015) evaluate how price variation affected the investment decisions of a sample of 6,806 manufacturing firms in India between 1993 and 2003. Their findings suggest that higher energy prices caused a decline in the firms' investment expenditure because of an income effect (i.e. the pass-through of increased costs to consumers generates a decrease in final output demand) and the inability to cope with the increased marginal cost of production adequately. Market structure, demand elasticity and the evolution of market willingness to pay are essential factors concerning energy price increases.

Using the Indian Annual Survey of Industries for 2001 to 2008, Abeberese (2017) provides further evidence on Indian manufacturing firms and presents evidence on how electricity prices affect firm-level productivity. Price increases make firms switch to less electricity-intensive production processes, become

less machine-intensive, and reduce output and productivity rates. These findings align with other works isolating the economic impacts of electricity hikes which, compared to fossil fuel increases, are much more detrimental to firms' performance patterns.

Sanchez-Vargas et al. (2019) use a 2002 cross-section on 900 manufacturing firms in Mexico to analyse the link between environmental regulation and productivity. They report strong non-linear patterns between the two variables. The authors also find that this trade-off is substantial for smaller firms and becomes negligible for large enterprises and posit that aggregate Porter Hypothesis-type responses may depend on firm-type characteristics.

Structural and managerial aspects are linked to green innovations of firms, but evidence for developing countries is scarce. Analysing the determinants of eco-innovation, Costa-Campi et al. (2015) identifies innovation drivers for energy efficiency using Spanish firm-level data of around 4,500 establishments between 2008 and 2011. The authors find that tangible investments, i.e., investment in more efficient machinery, sustainable materials and process innovation etc., have a greater effect on energy efficiency than intangible investments. Furthermore, the authors find evidence that firms undertaking organisational innovation and putting a strong emphasis on environmental goals are more likely to increase their energy efficiency and that firm size and foreign market orientation are additional important determinants of eco-innovations.

Blass et al. (2014) argue that management practices play a pivotal role in how firms cope with energy price changes. The authors emphasise the importance of managerial quality in adopting energy efficiency. Based on a sample of around 750 small and medium-sized manufacturing firms in the US, the authors find that top operations managers, which typically possess knowledge of raw materials, production processes, costs, quality controls and techniques for maximising manufacturing efficiency, are more likely to implement changes that result in higher energy efficiency gains and more disruptive technologies than their general management counterparts (owners, CEOs) While empirical evidence on the dissemination of green technology remains scarce, De Haas et al. (2022) identify managerial and financial barriers as a bottleneck to a green transition in emerging economies. The authors use firm-level data in a cross-country study of 22 emerging economies to show that firms that are subject to constraints in environmentally-minded management and credit constraints are

causally linked to lower investment in efficient and less-polluting industries.

Using firm-level data on management practices and energy expenditures from 40 countries and around 2,000 manufacturing firms in central and eastern Europe, Central Asia and the Middle East and North Africa, Schweiger and Stepanov (2018) examine the relationship between generic management practices and energy intensity. In line with the studies mentioned above, the authors record a 23 per cent reduction in fuel intensity as the management score improves from the 25th to the 75th percentile of the distribution when fuel subsidies are low. However, under a high-subsidy regime, the decline in energy intensity amount to only 2 per cent. The authors hint at the existence of a potentially non-linear response pattern of firms and posit that better-managed firms are in a position to be more energy-efficient whenever energy prices are high while also taking advantage of lower prices.

By analysing data following two of China's regional pilot emission trading schemes, Yong et al. (2021) provide empirical evidence on how management quality affects firms' responses to higher energy prices through carbon pricing. Using data on approximately 200 firms, the authors illustrate that well-managed firms are, on average, more productive and positively associated with "green" patenting. Well-managed firms are also found to be significantly more energy efficient under a carbon-pricing regime, while similar results cannot be observed for their less well-managed counterparts.

Despite the relevance of business decisions on climate change, the energy-management nexus in the context of developing countries remains largely unexplored. Recently, Grover and Karplus (2020) exploited a recent wave of World Bank Enterprise Surveys (WBES) data. This recent update features a "Green Economy" module. It provides the authors with a representative cross-section of firms in 31 countries (eight of which belong to either the "low" or "lower-middle" income group) and information on firm-level energy use, type of pollution emissions, and energy conservation practices. The authors report that better management practices are typically associated with higher energy efficiency and that manufacturing firms particularly show a strong relationship between management practices and energy efficiency.

Causal Porter-type innovation mechanics can also be established in the most recent micro-level literature focusing on developing economies. Most empirical evidence in the literature has so far been collected for advanced

economies (Burke et al., 2016). The lack of comparable insights in the context of developing countries is a particular concern as the most substantial demand growth in energy and potential greenhouse gas emissions will come from developing economies and emerging markets (Wolfram et al., 2012). More recently, a sub-strand of the literature has emerged focusing on the joint causal analysis of environmental and economic impacts on the firm level in the context of developing economies.

Brucal and Dechezleprêtre (2021) show that large and energy-intensive sectors in Indonesian manufacturing tend to reduce energy consumption most significantly in response to an energy price hike. Plants react to higher energy prices by updating their capital stock and investing in new and presumably more energy-efficient technology. The authors find that energy use and emissions of surviving plants decline significantly in response to an energy price increase and have no significant effect on output. Brucal and Dechezleprêtre (2021) show that the effects of energy price increases and plant-level performance differ depending on their initial output and energy intensity as large and energy-intensive sectors tend to reduce energy consumption more significantly. This effect extends well beyond innovation strategies as energy-intensive firms are also found to have a higher probability of exiting the market in response to a price hike.

Calì et al. (2021) evaluate the direct and indirect impact of energy prices on firms' economic performance for eleven developing countries from 2002 to 2013. Using World Bank firm-level survey data, the authors find that higher energy prices do not necessarily hamper economic performance. Positive effects on firm profitability are only observed for energy-efficient firms. At the same time, energy prices are not found to have any discernable effects on productivity. The authors also find considerable heterogeneities as energy-intensive medium and large firms have a less positive impact on performance variables from a rise in energy prices than other medium and large firms.

Firm-level responses to energy price increases vary depending on the *energy type.* Using firm-level data of small and micro enterprises from Indonesia for 2013, Rentschler and Kornejew (2018) explore the impact of input price changes of electricity, and various fossil fuels have on firm performance. The authors find that higher prices for all energy types are associated with higher energy efficiency.

Calì et al. (2022) focus on medium- to large-sized manufacturing firms in two emerging economies, Mexico and Indonesia, and find that while surges in electricity price harm plants' performance, increases in fuel prices lead to higher labour and total factor productivity and profits of manufacturing plants. The authors find strong evidence of capital upgrading of firms in response to a fossil fuel price hike where firms respond by upgrading capital and machinery, which, in turn, yields positive productivity outcomes. This effect is particularly pronounced for capital intensive manufacturing sectors.

In the same vein, Amann et al. (2021) analyse the effects of energy price hikes on manufacturing firms in Oman. They show that increases in fossil fuel energy factor prices are causally linked to improvements in productivity and efficiency and lead to notable business upgrading. Such effects are only observed for fossil fuels but not energy price increases for both studies. Electricity hikes are, by and large, found to be more detrimental to firm performance and will be explored more extensively in the next section of this review.

4.2.2 Substitution effect

This section discusses to what extent firms can substitute between various energy sources in response to price hikes by focusing on substitution possibilities between different energy types, such as electricity and various fossil fuel types.[4] Given the particular type of data that is needed for such an analysis, it should come as no surprise that empirical evidence is relatively scarce. However, the overall picture emerging from the literature is that substitution of different energy sources happens extensively. The extent to which energy inputs can serve as a substitute for each other depends on the country and policy context. For example, notable differences in substitutability between fossil fuel sources have been observed, most notably from coal to gas. Furthermore, the possibility of substituting electricity in response to a relative price increase (vis-a-vis constant fuel prices) is more limited, and so are ample insights for developing countries.

The potential for intra-energy substitution finds broad support in the literature. In a meta-study focusing on North America and Europe, Koetse

[4]The most prominent fossil fuel types are coal, diesel, kerosene, petrol, LPG and other types of natural gases and lubricants. This selection may, however, vary depending on the country's context.

et al. (2008) hints at the potential for capital- electricity substitution prospect. The study quantifies this potential to be large for all regions and periods, especially in the long run and in North America, indicating opportunities for the uptake of energy-saving capital through energy price regulations. Similarly, a meta-analysis of 47 studies on inter-fuel subsidies Stern (2012) confirms the existence of substitution possibilities between fossil fuels and electricity and posits that such replacements seem more constrained at the macro level and less constrained at lower levels of aggregation. The author finds strong evidence for the possibility of substitution between electricity and fossil fuels for the manufacturing sector, which features the most extensive elasticities compared to either macro- or industry-level elasticities.

Strong intra-fuel substitution of firms can be observed in response to the EU ETS with an unambiguous decrease in the use of coal. Wagner and Petrick (2014) show that German EU ETS Firms vastly reduced their carbon emissions by switching from high-carbon to low-carbon fuels. Changes in the policy framework caused firms to abate one-fifth of their CO2 emissions between 2007 and 2010. This reduction was achieved predominantly by improving energy efficiency and curbing the consumption of natural gas and petroleum products, but not electricity use. Similarly, Wagner et al. (2014) illustrate that the EU ETS emission certificates led French manufacturing firms to substitute coal and oil with gas. As the authors show, this accounts for nearly 50 per cent of the observed emissions reductions. Again, analysing French manufacturing firms, Dussaux (2020) confirms the finding that higher energy costs lead to energy substitution. The results suggest that firms reduce their energy intensity by decreasing energy use more than the other inputs and reduce their CO2 intensity by increasingly utilising electricity instead of fossil fuels. The authors also find that larger firms typically improve their environmental performance in response to higher energy costs (electricity and fossil fuels). To summarise, results suggest that firms reduce their energy intensity by decreasing energy use more than the other inputs and reduce their CO2 intensity by increasing electricity use relative to fossil fuel use.

In the context of developing countries, the degree to which different fuel types can be replaced by each other remains more unexplored. Rentschler and Kornejew (2018) firms change their energy mix and use less of the more ex-

pensive energy: While electricity can be substituted away by a blend of other energy sources. Still, it plays a minor role in replacing fossil-based energy sources. While LPG and petrol are complementary and diesel and kerosene are the most critical substitutes, petrol is also the least substitutable energy source. Diesel and LPG have the most detrimental impacts on firm competitiveness, and sector-level differences are notable.[5]

Amann et al. (2021) test for the substitutability between electricity and fossil fuels for Omani manufacturing firms. However, the authors do not differentiate between the various fossil fuel types and only evaluate the substitutability of electricity and aggregated fossil fuel measures. They find a high degree of substitutability between the quantities of electricity and fossil fuels consumed by firms in response to increasing fuel prices. Regardless, a similar effect cannot be observed for electricity price increases for which no significant substitution can be reported.

4.2.3 Absorption effect

The literature finds that energy price increases trigger different absorption channels and that firms' proclivity to absorb price hikes varies depending on their characteristics and sectoral attributes. Unsurprisingly, particularly firms in energy-intensive sectors respond most notably to price hikes. While price hikes typically affect financial metrics, they do not necessarily erode business fundamentals. Furthermore, there is no conclusive evidence for firm exit or significant aggregate employment loss in response to energy price increases. However, the evidence does point to potential within-sector labour reallocation, underscoring the importance of implementing sensitive policy measures.

Energy input prices affect financial metrics. In a firm-level study on 560 US firms listed on the NYSE, Narayan and Sharma (2011) find that oil price affects returns of firms differently depending on firm size and their sector: They show that transport and energy sectors experience the most notable decreases in their returns in response to an oil price increase. Furthermore, larger firms are more consistently associated with declining returns in response to an oil price increase than smaller enterprises.

[5]For example, kerosene is found to be relatively important for mining coal, lignite and peat as well as in producing technical products and tobacco, but less relevant for other sectors.

In a study on Japanese manufacturing firms, Fujii et al. (2013) finds heterogeneous effects of different environmental pollutants on various financial metrics such as Return on Assets (ROA), Return on Sales (ROS) and Capital Turnover (CT). The author finds a positive relationship between financial performance and environmental performance based on CO2 emissions; however, reductions in CO2 emissions do not seem to improve capital productivity in the short term.

More recently, Trumpp and Guenther (2017) revisit the issue of firm profitability and environmental policies using an international sample of 2,361 firm-years from 2008 to 2012 from the Asset4 database. The authors report a U-shaped relationship between carbon performance and profitability and stock market performance for manufacturing industries. The authors argue that positive stock market performances can only be expected once a certain level of carbon performance has been achieved.

On the one hand, there is no systematic evidence for job destruction in response to energy price increases. Using German firm-level manufacturing data, Flues and Lutz (2015) analyse the effect of electricity taxation on firm turnover, exports, value-added, investment and employment. The authors fail to find any systematic, statistically significant effects of the electricity tax legislation and argue that the preferential electricity taxation of energy-intensive sectors has not had a positive impact on the economic performance of these firms. This finding is supported by the results in Wagner and Petrick (2014), which find that while German EU ETS Firms vastly reduced their carbon emissions by switching from high-carbon fuels to low-carbon fuels, they did not do so at the expense of employment. Furthermore, their findings suggest that ETS regulations caused an overall increase in total exports between 6 to 18 per cent between phases I and II of the EU ETS. Analysing the collective impact of the EU ETS on carbon emissions and firm performance Wagner et al. (2014) do not find notable effects on employment or emission reallocation strategies by the firms. Additional studies in the European context confirm these results: For example, Martin et al. (2014) illustrate that the carbon tax as part of the Climate Change Levy (CCL) package did not affect either employment or plant exit for the UK. Summarising work on the effectiveness of the EU ETS, Arlinghaus (2015) concludes that while the ETS was successful in reducing emissions, no causal effects on employment or output can be observed.

Dechezleprêtre et al. (2018) analyse the effects of the EU Emissions Trading System (ETS) across all countries covered by the EU ETS. The authors report no adverse effect on firm profit or employment when comparing the economic metrics of regulated and unregulated firms. More specifically, the authors report that the impact of profit depends on firm size. They illustrate that smaller firms saw no statistically significant change in their profits and a statistically significant decrease in their return on assets. The authors argue that this finding is compatible with larger firms being more able to pass through the costs of carbon emissions onto their customers and smaller abatement costs. Lastly, the authors find little evidence for sectoral differences: For one, employment effects across all sectors are positive and insignificant, except for the basic metals and electricity & heat sectors, which both report positive and significant employment effects.

These results are also confirmed by Ellis et al. (2019) which, in a selection of cross-country and advanced economy studies, finds little evidence of negative employment effect in response to carbon prices: Out of the nine reviewed studies[6], only Marin et al. (2018) provide evidence of an adverse employment effect during phase I on the EU ETS scheme. More specifically, Marin et al. (2018) find positive effects during both phases of the ETS for markup and investment and turnover and labour productivity in the second phase. Regarding the impact on employment, the authors find that the first phase of the ETS scheme is found to have modest yet significant employment effects. Conversely, no significant impact on wages nor TFP could be identified.

Further supporting evidence on the energy price-employment notion can be found in Hille and Möbius (2019a). Using a macro-level panel on 33 primary, secondary, and tertiary sectors in 27 OECD countries from 1996 to 2009, the authors find that rising energy prices have no significant effect on net employment across manufacturing sectors. Furthermore, the employment effect becomes positive and significant once all economic sectors are considered.

Looking at a sample of 77 developing economies between 2000 and 2014, Kpodar et al. (2019) find that increases in domestic gasoline and diesel prices adversely affect export growth in the short run. However, this initial negative response subsides within the first two years after the price increase. It is most prevalent in countries with (i) high energy dependencies and (ii) constrained

[6]The studies are either pan-European or target specific, high-income economies such as Germany, Italy, the UK, Canada or France.

access to alternative energy sources such as electricity. These findings align with studies highlighting the potential arising from high inter-energy substitutability.

On the other hand, there is some evidence for within-sector (labour) reallocation. Looking at the French manufacturing sector, Dussaux (2020) observes that an increase in energy prices leads to a shift of production and employment to more energy-efficient firms, with no aggregate employment effect on the industry level overall. Most interestingly, the authors find that small firms and entrants compensated for the employment loss recorded for larger and energy-intensive firms. More specifically, the author finds that an increase in the energy cost does not affect employment of small firms but reduces it by 2.6 per cent for medium-sized firms and by 5.5 per cent for large firms. Furthermore, only large firms reduce their level of investment in response to a higher energy cost.

Marin and Vona (2021) find a modest impact of higher energy prices on employment but no effect on wages. The authors report evidence of capital deepening and job replacements are skill-biased and hint at heterogeneous labour adjustment processes as large establishments report larger (negative) employment effects; negative wage adjustment is more prevalent in small firms. Using a synthetic control analysis between 1998 and 2017, Arcila and Baker (2022) show that the carbon tax policy in the Canadian province of British Columbia is associated with a more stagnant employment growth compared to the synthetic control group.

Dechezleprêtre et al. (2020) analyse the impact of energy prices and environmental stringency on manufacturing employment using manufacturing sector-level data across OECD countries between 2000 and 2014. The authors find a significant and negative effect of energy price increases on manufacturing employment, particularly for energy-intensive sectors. Additionally, energy price hikes are associated with an increase in firm exits. The authors also report substantial evidence pointing at a worker reallocation within manufacturing as firms that remain operational experience a slight increase in overall employment. However, this does not offset the overall negative effect on employment.

Developing economies show similar dynamics and display pronounced sector-level differences. Rentschler and Kornejew (2018) belongs to the few

studies that consider the impact of a variety of energy sources on firm-level profitability by looking at a cross-section of small- and micro-firms in the Indonesian manufacturing sector. The authors find that energy prices have a small yet significant adverse effect on long-run firm competitiveness. While firms attempt to respond to these measures by employing energy substitution, efficiency increases and cost pass-through (as discussed in the respective subsections of this review), the adverse effects of energy price increases cannot be fully mitigated. Most importantly, the authors highlight that higher energy prices are typically associated with higher long-run costs. Although price increases across all energy prices negatively affect competitiveness, the most pronounced effects are found for diesel and LPG. Finally, firm-level response patterns are very different depending on the analysed sector. Consequently, increasing prices of certain energy types are likely to have disproportionately large effects on specific sectors due to their production processes' inherent (technological) characteristics, which determine their ability to implement response measures.

Very noteworthy sector-level differences are also reported by Kumar and Prabhakar (2020) who analyse how exports of eleven energy-intense sectors in India react to changes in energy prices. The authors present evidence suggesting that energy price increases negatively affect trade, asserting that carbon leakage concerns are not necessarily unfounded; however, these effects are also relatively small. Furthermore, their results hint at substantial sector-specific differences: Energy-intensive sectors are affected more severely by energy price hikes and are most notable for the non-ferrous metal and machinery sectors.

Looking at the economic performance of Indonesian manufacturing plants in response to an increase in energy prices, Brucal and Dechezleprêtre (2021) report that energy price increases are causally linked to upticks in plant exit and employment contraction, particularly for energy-intensive and large firms only.[7] The authors also report job reallocation from energy-intensive to energy-efficient firms and sectors. While most sectors did not contract their employment base in response to an energy price increase, significant effects were reported for the Indonesian food and basic metal sector.

Most recently, using World Bank Enterprise Survey data, Calì et al. (2021) analyse firms' economic performance in response to energy price increases for 11 developing countries from 2002 to 2013. The authors do not find statistical

[7] The impact is not significant for less energy-intensive plants and positive for small plants.

evidence that price hikes lead to employment loss.

4.2.4 Pass-through effect

The final transmission channel firms have in response to higher energy prices is to pass them on to the consumer through an increase in unit sales prices. The literature estimates pass-through rates by identifying how unit sales prices respond to energy price variation. Numerous studies identify significant cost pass-through from firms to the consumer indicating firms only bear parts of the carbon costs which are typically either smoothed over the value chain (Lin and Jiang, 2011) to a considerable extent borne by the consumer (Arlinghaus, 2015; Joltreau and Sommerfeld, 2019).

Substantial carbon cost pass-through is found in the empirical literature for the manufacturing sector. Consumers may have to bear the burden of energy price interventions if firms can pass on these costs to the consumer. Additionally, as laid out in Joltreau and Sommerfeld (2019), firms may also be in a position to generate profits on top of abating emissions and depend on the market structure as well as price elasticities of energy (Sijm et al., 2012) and the existence of price rigidities (Fabra and Reguant, 2014). Focusing the analysis on specific manufacturing sectors, numerous studies have found evidence supporting the pass-through mechanism. For example, cost pass-through is reported a national multi-sector setting for Germany (Alexeeva-Talebi, 2010) and isolated sector-level studies (Oberndorfer et al., 2010). Oberndorfer et al. (2010) quantify the cost pass-through certain refinery, glass, chemicals and ceramics commodities in the UK using weekly price data. The authors estimate that up to 50 per cent of energy price increases and up to 76 per cent of diesel price increases are passed through to retail consumers of gasoline over five weeks. Other studies analysing cost pass-through on the commodity level, such as Alexeeva-Talebi (2011); De Bruyn et al. (2010), confirm the existence of this transmission channel for a broad set of advanced economies and illustrate that in particular cases, such as the refineries in Europe, pass-through rates as high as 100 per cent can be observed. Similar results are also found in Ganapati et al. (2016), illustrating that US American firms overwhelmingly passed on much of the energy price increases to final customers. The authors emphasise that the magnitude of cost pass-through in the event of an energy price shock largely depends on the mar-

ket's competitiveness.

One can observe similar dynamics in the context of the firm-level literature surveyed in this report; see, for example, Arlinghaus (2015) for a more extensive discussion. In line with Demailly and Quirion (2008), who predict a notable pass-through of EU ETS costs for the iron and steel manufacturing sectors, researchers have found evidence for notable cost pass-through to the consumer across multiple sectors using different metrics. While Wagner and Petrick (2014) find that German EU ETS Firms vastly reduced their carbon emissions by switching from high-carbon fuels to low-carbon fuels, their findings also suggest that ETS regulations caused an overall increase in total exports between 6 to 18 per cent between phases I and II. As the authors state, given the data availability, it is unclear whether the increase in exports reflects an increase in the volume of shipments, a price increase, or both. This result is in line with Marin et al. (2018), who find significant increases in markups of firms under the EU ETS scheme. Similar results of increased markups and overall profits are found in Makridou et al. (2019) while findings in Dechezleprêtre et al. (2018) again highlight the relevance of firm size: For their sample of four European economies, i.e. France, Netherlands, Norway and the UK, their analysis of revenues is compatible with the notion that larger firms are more able to pass through the costs of carbon emissions onto their customers, and smaller abatement costs.

There is evidence of inter-sectoral linkages and notable dispersion of energy price increases along the value chain. Cost increases may also result in notable knock-on effects across industries and transmittable spillovers through firm-level linkages. Lin and Jiang (2011) demonstrates for the case of China that energy price increases can lead to a contraction of energy-intensive industries while simultaneously raising input prices for other industries, thereby impacting downstream sectors. Naturally, inter-sectoral linkages may be country- and sector-specific and successful adaptation strategies depend on cross-national technology transfer (Javorcik, 2004; Li and Tanna, 2018).

In the same vein, Chan et al. (2017) analyses the effect of aggregate energy costs on export performance while including indirect energy costs. Using panel data for ten manufacturing sectors in 43 countries from 1991 to 2012, the authors find that intermediate factor intensities and trade relationships significantly affect how energy shocks impact export prices as indirect energy costs

are passed through the upstream supply chain. The results show that energy cross-subsidies that raise industry tariffs to support lower household rates could extensively reduce net manufacturing exports across developing and developed economies.

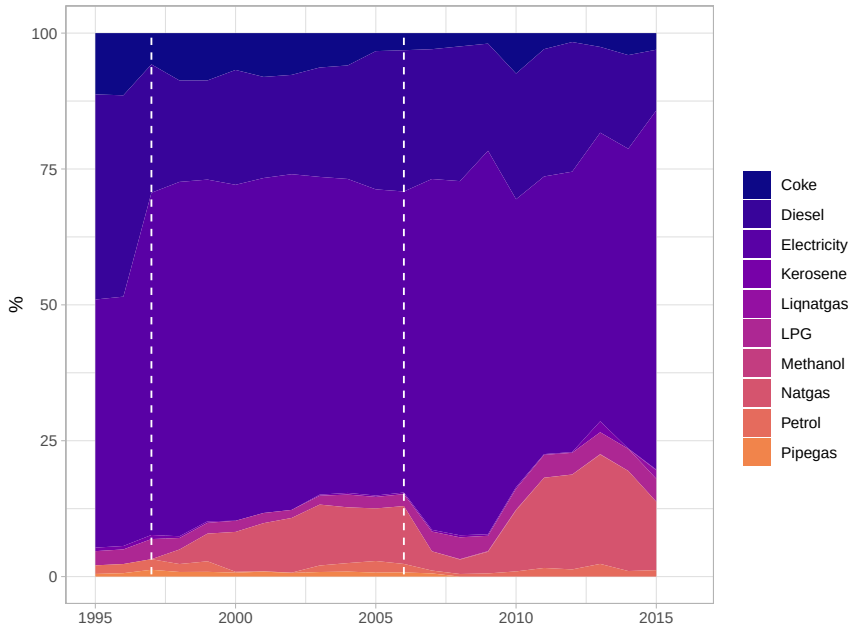
In the context of Indonesia, Rentschler and Kornejew (2018) confirm that higher prices for all energy types are associated with higher long-run sales prices; that is, the firms pass the energy costs to the consumer. Considering a sample of 11 developing countries over the period 2022 to 2013, Calì et al. (2021) do not find significant evidence for an aggregated cost pass-through effect. However, the authors observe a significant pass-through effect for firms experiencing power outages. As the authors conclude, the results show that energy constraints associated with higher prices and other energy features can stimulate economic and innovation strategies to offset increasing energy costs.

4.3 Institutional setting

The energy market in Chile has numerous interesting features. In the paragraph below, we provide a brief history of the energy distribution in Chile and prize stabilisation funds overlapping with our sample period.

Price stabilisation funds. In response to the oil price spikes after the Gulf War (1990-91), Chile launched its Oil Prices Stabilisation Fund (FEPP), under which authorities established a price cap based on the expected evolution of crude oil prices. The fund became operational whenever international prices deviated from the determined reference price. As the fund covered different products, i.e., gas, kerosene, diesel, and LPG, *cross-product substitution has become a long-lasting mechanism for Chilean firms to cope with energy price variations* (Coady et al., 2013). The FEPP is not the only stabilisation fund utilised by Chilean authorities to smooth out energy price variations. In 2005, the Fuel Prices Stabilisation Fund (FEPC) was a temporary measure to respond to the price spike resulting from supply interruption following hurricane Katrina. The mechanism operated similarly to the FEPP but relied on a narrower band (of 5 per cent) around a reference price based on the recent and expected price evolution. Even though this mechanism was initially intended to be used for approximately a year, it was eventually phased out in 2010, with the total fiscal cost of the FEPC over 2006–09 estimated at 0.65 per cent of the 2012 GDP (Vagliasindi,

Figure 4.2: Energy composition over time, Chile



Note: Purchased quantities by energy type. Years 2007 and 2006 highlighted with dashed lines.
Source: Authors' calculations based on INE (2015).

2013).[8]

Energy distribution in Chile. Until the year 1995, which constitutes the starting year of our analysis, coal production was subsidised. With the abolishment of the subsidies and the bankruptcy of the state-owned enterprise ENACAR, domestic coal production fell from 1.7 million tonnes in 1994 to 0.4 tonnes in 1998 (O'Ryan et al., 2004). The *electricity crisis of 1998-99* exposed the weaknesses of the energy sector's governance. In 1998, the country suffered a severe drought, which had a notable impact on the energy generation of the CIS network. This event, in conjunction with disruptions in three natural gas plants, caused blackouts between November 1998 and April 1999, forcing the government to ration supply (Serra, 2022).

The subsequent period of the late 1990s to mid-2000s was characterised by

[8]More recently, the stabilisation fund was replaced by a tax adjustment mechanism. In 2011, Chile introduced the Consumer Protection System for Fuel Excise Taxes (SIPCO). Instead of a fund, this adjustment mechanism relies on a taxation scheme to dampen the transmission of changes in international prices to domestic prices. The tool reduces excise taxes for fuel when global prices jump above a 10 per cent band around a reference price and increases excise taxes when international prices fall below the band (Coady et al., 2013).

the *heavy use of natural gas* in electricity generation and was fueled by the massive natural gas imports from Argentina (Cansino et al., 2018). The gas-heavy energy production episode came to an abrupt halt in 2005 when Argentina began restricting and taxing natural gas exports due to domestic deficits leading to a notable price surge in Chile.[9] For example, the year 2007 saw a reduction of natural gas imports by 51.5 per cent due to Argentina's decision to interrupt its exports of this power source (CNE, 2015). Chilean power generators coped with the natural gas shortage by substituting oil for gas. The entry of oil-fired power plants ensured supply. Between 2005 and 2015, their capacity in the CIS quadrupled, increasing their share from nine per cent to 21 per cent (CNE, 2016). In addition, subsequent years saw the construction of new coal plants to close the wedge between reduced energy supply and growing demand. Coal plants played a crucial role in this endeavour because of their comparably lower average net present cost of electricity generation (Serra, 2022).

In the years after the 1998/99-drought and the natural gas imports interruptions in 2007, Chile intensified the use of fossil fuels as an electricity generation source to attain an independent and reliable power supply. For one, this included reducing the relative weight of hydropower (Cansino et al., 2018). Secondly, the country also diversified its energy mix in terms of energy sources and provision of certain energy types. For example, in 2009 and 2010, Chile imported natural gas, mainly from Algeria, Equatorial Guinea, Egypt and Indonesia. Furthermore, it began constructing large-scale liquefied natural gas terminals (Mundaca et al., 2015).

Technological advancements and growing environmental concerns have led to a stronger focus on green and renewable energy in recent years. By the 2010s, increasing public opposition and stricter environmental requirements halted the construction of coal-fired plants in the 2010s (Serra, 2022), and new policy measures promoting non-conventional renewable energies were put into practice (Cansino et al., 2018; MEFR, 2008). The year 2014 also saw the passing of carbon tax legislation with the *General Tax Reform Bill*[10] (Ministerio de Hacienda, 2014). However, given its inception in 2014, the tax reform falls too

[9] Chile had already suffered a cut in natural gas supplies from Argentina in 2002 due to labour strikes (Cansino et al., 2018).

[10] The tax reform bill (Ley 20.780) contains the following three green taxes: (i) a tax on CO2 emissions from stationary sources with boilers and turbines (sum over 50MW); (ii) a tax on local contaminants also on stationary sources with boilers and turbines (PM, SO2 and NOX); (iii) a tax on the first sale of new cars considering the expected NOx emissions over their lifetime (Pizarro, 2021).

close to the end of the sample period to merit an empirical analysis. This task will be left for future research with a more updated firm-level data set.[11]

Until 2017, Chile had four main electricity grids: the Northern Interconnected System (SING), the Central Interconnected System (SIC), the Aysen System (SEA), and the Magallanes System (SEM), with the former to being integrated into the National Electricity System (SEN). Within these grids, numerous state-owned and private energy producers have been in charge of energy generation (Serra, 2022).

4.4 Data

4.4.1 The Chilean manufacturing data base

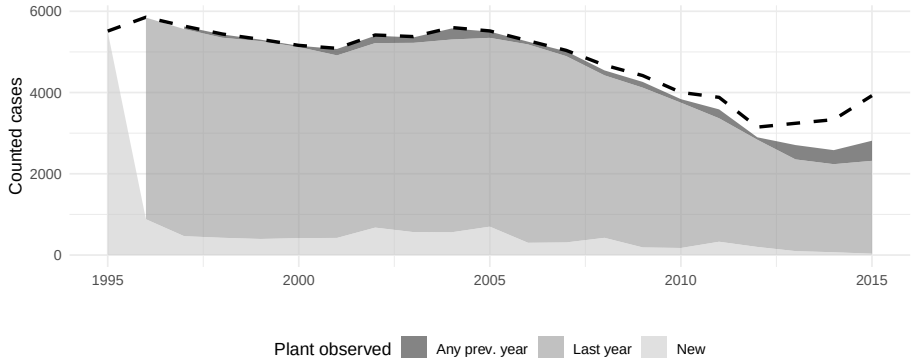
The *Annual National Industrial Survey* (*Estructura de la industria manufacturera*), ANIS from now on, is made available by the *National Statistical Institute* (INE, 2015). Raw ANIS data is available for the period 1995-2019. From 1995 to 2007, ANIS offers a harmonised panel data set (referred to as the *combined data* as opposed to the *raw data* which refers to the separate annual waves of ANIS) that identifies the same manufacturing firm with a unique ID linking plants over time. After that, the continuity of the time-independent identifier is abolished.[12]

While comprehensive, this combined data set does suffer from a lack of regional and manufacturing sector-level identifiers. To recover information about the regions and the ISIC 4-digit codes for each plant, we resort to the raw data version of ANIS, which contains both desired variables. We match plants in the combined and raw data through their characteristics. We apply the same matching techniques to expand the panel structure in the post-2007 period. This procedure yields a final data set containing a total of 100,803 observations on 12,169 plants with an average of 4,200 unique firms observed across survey years. We provide a more extensive discussion on matching Chilean manufacturing firms

[11] Although the implementation of the carbon tax started in January 2017, at the time of writing, the implementation of the policy remains an ongoing process; see https://www.worldbank.org/en/results/2021/02/15/market-instruments-for-climate-change-mitigation, also https://www.thepmr.org/system/files/documents/Existingpercent20Carbonpercent20Taxpercent20tablepercent20Marchpercent202015.pdf and https://projects.worldbank.org/en/projects-operations/project-detail/P130378. Last access: September 2022.

[12] For reference, see https://www.ine.cl/docs/default-source/estructura-de-la-industria/manuales/encuesta-nacional-industrial-anual/documentos/instructivo-de-uso-de-base-de-datos-enia-2019.pdf?sfvrsn=a45724f6_4. Last access: September 2022.

Figure 4.3: Distinct plants over time, Chile



Note: Dashed line: number of unique plants reported in raw data.
Source: Authors' calculations based on INE (2015).

across the two versions of ANIS in Appendix 4.D. Figure 4.3 illustrates that our matching algorithm produces a consistent sample of firms over the entire sample horizon and a low attrition rate.

Secondly, the combined data also exhibits some irregularities in reporting energy and fossil fuel data. We address the issue of potentially misreported energy and fossil fuel information employing a simple cleaning algorithm that identifies and corrects inter-temporal changes in reported quantities and unit prices of pre-defined magnitudes. We provide a more extensive discussion on the cleaning algorithm in Appendix 4.E.

4.4.2 Variables of interest

Energy price series. The central premise of this paper is to analyse to what extent (policy-driven) changes in energy-input prices affect firm-level performance. We pay particular attention to the potential degree of substitutability between energy input sources, namely electricity and fossil fuels. For this, information on purchasing price and quantity of electricity as well as fossil fuels are required. We calculate respective unit prices as:

$$up_{it}^{m} := \frac{value_{it}^{m}}{quantity_{it}^{m}}, \tag{4.4.1}$$

where *value* denotes the purchased value (in real LCU) and *quantity* denotes the purchased quantity of $m = \{\mathbf{EL}ectricity,\ \mathbf{F}ossil\ \mathbf{F}uels\}$, for firm i and period

t, respectively. While the calculation is straightforward in the case of electricity, the fossil fuel aggregate is obtained by calculating kWh-equivalent quantities of all $n = \{gasoline,\ diesel,\ kerosene, \dots\}$ fossil fuel types according to the conversion rates provided in Table 4.1. We calculate the fossil fuel unity price as follows:

$$up_{it}^{FF} = \sum_{n} w_{it}^{n} \times up_{it}^{n}, \quad w_{it}^{n} = \frac{\tilde{q}_{it}^{n}}{\sum_{n} \tilde{q}_{it}^{n}}, \tag{4.4.2}$$

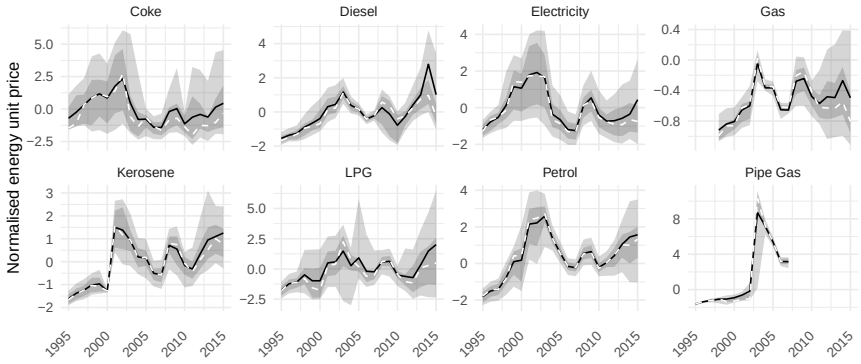
where $\tilde{q}_{it}^{n}$ denotes the quantity (in kWh equivalents) of fossil fuel type n for firm i and period t, and w_{it}^{n} corresponds to its fixed-quantity weight, respectively.

Impact variables. The paper aims to analyse the effect of energy price variations along various economic firm-level metrics. Following the previously established categorisation of firm-level coping strategies in section 4.2, we compartmentalise the firm-level outcome variables along the four main firm-level coping strategies. More precisely, we distinguish between the *innovation - competitiveness* channel as well as the *substitution* and *absorption* channel.

Concerning the *innovation/competitive* channel, we evaluate the strong and weak form of the Porter Hypothesis by assessing the effects energy price increases have on innovation and productivity, respectively. For Total Factor Productivity (TFP), we implement the value-added production function with a Cobb Douglas production following Ackerberg et al. (2015). To evaluate the *substitution* channel, we separately assess the effects of energy price hikes on the consumed quantities of various energy sources, i.e., electricity, coke, diesel, petrol, LPG and gas. Lastly, to analyse the *absorption* channel, we differentiate between effects on the workforce, such as employment or wages, as well as monetary business metrics, such as profit margins, Return on Sales etc.[13] We provide a comprehensive list of all analysed variables in Table 4.5. We deflate all monetary series using ISIC II-digit industry-level following Haraguchi and Amann (2022).

[13]Our analysis is constrained by limitations arising from the data. For example, since there is no quantity-related information on sales, some productivity or pass-on indicators cannot be recovered from the data.

Figure 4.4: Harmonised energy unit prices by energy type over time, Chile



Note: Harmonised energy unit prices. ISIC II-digit industry-level deflators following Haraguchi and Amann (2022). Dashed line: median. Shaded area: $10^{th}/90^{th}$ (light); $25^{th}/75^{th}$ (darker) percentile.
Source: Authors' calculations based on INE (2015).

4.4.3 Exploratory evidence

This section highlights a selection of aggregate firm-level dynamics of the energy price variations observed by Chilean manufacturing firms.

Stylised fact 1: Chilean manufacturing firms overwhelmingly consume electricity. Firms rarely utilise all types of energy regularly. As illustrated in Table 4.4, more than 95 per cent of Chilean manufacturing firms demand electricity. At the same time, the share and contribution of the different fossil fuel types vary extensively. For example, the contribution of natural gas has increased from low single-digit numbers in the mid-1990s to 20 per cent in 2012.[14]

Stylised fact 2: Electricity prices were relatively higher in earlier periods while fossil fuel prices are more volatile. Figure 4.4 plots harmonised energy prices by energy type as defined in Equation 4.4.1 over time. We observe a notable increase in the variability and the mean unit price of the different fossil fuels in the early 2000s. After that, lower variability and a more stagnant increase in energy price walks can be observed until around 2010.

By plotting the aggregated electricity and fossil fuel index, Figure 4.8 illus-

[14] Table 4.4 also indicates a notable drop in the number of firms utilising electricity in the last sample year (from 87 per cent in 2014 to 71 per cent in 2015). Consequently, we conduct the empirical analysis with and without the final sample year to ensure the robustness of our findings.

trate that electricity and fossil fuel prices have experienced a different progression in price increases over time. While electricity prices experienced a more rapid surge around the early 2000s, they remained more stable thereafter. Furthermore, aggregated fossil fuel prices remain much more volatile throughout the sampled period. In Figure 4.6, we break down the separate energy unit price series for electricity and fossil fuels by ISIC Rev II-digit sector. The results indicate more substantial between-industry variation in fossil fuel prices than electricity. This is expected as manufacturing sectors require different fossil fuel types for different purposes in the production process. Their use may range from being a primary energy source, a primary production component or an intermediate or auxiliary commodity, depending on the sector and type of production process. A common trend observed across all industries remains that the relative price difference between electricity and fossil fuels has unequivocally closed throughout the sample period: In relative terms, electricity is cheaper than fossil fuels in 2015 than in 2005.

Stylised fact 3: Energy price changes are experienced by all operating firms. In Figure 4.5 we decompose the electricity and fossil fuel price index using the Melitz and Polanec (2015) as well as Mean Divisa decomposition; see Appendix 4.F for more information on deposition techniques. As the results show, energy price changes are experienced by all operating firms and are generally not more pronounced for incumbent or exiting firms. Decomposition analysis of energy trends shows no dominant effect or strong inter-temporal correlation between energy prices. Figure 4.7 illustrates that price increases in one energy type do not significantly correlate with future or past price changes in the other energy type for surviving firms.

Stylised fact 4: Non-linearities in firm-level response patterns. In Figure 4.9 we use pooled quantile regressions to identify potential non-linearities in firm-level response patterns. These correlations indicate that firm-level responses to price increases may depend on the price level. For example, firms might be more wasteful in their energy use at lower energy price levels and retain less efficient production processes because of low opportunity costs. Our explanatory evidence suggests stronger (negative) correlations between competitiveness indicators and electricity price increases at lower energy price levels, while fossil fuel responses are less variable.

4.5 Empirical strategy

4.5.1 The baseline model

The baseline model of our analysis is similar to the setup in Amann et al. (2021); Calì et al. (2022) and estimates the following equation:

$$y_{it} = \boldsymbol{\beta}_i \mathbf{x}_{it} + \varepsilon_{it}, \tag{4.5.1}$$

where $\mathbf{x}_{it} = \left[up_{it}^{FF}, up_{it}^{EL}\right]'$ denote the unit prices for fossil fuels (FF) and electricity (EL) for firm i and period t, respectively.

To account for endogeneity, we use a combination of industry/region-year dummy interactions to capture variations in regional and industry-specific market conditions and technology-related developments. More specifically, we assume the following error term structure for Equation 4.5.1:

$$\varepsilon_{it} = \alpha_i + D_{st} + \tau_{rt} + e_{it} \tag{4.5.2}$$

where α_i denotes firm-specific intercepts. We introduce industry-year interactions (D_{st}) to capture industry-specific market conditions and technology-related developments. We also include region-year interactions (τ_r) to capture the evolution of region-specific long-run development dynamics in local governance, infrastructure, development, innovation, etc. Following literature conventions, we also rule out further causes of endogeneity that result from autocorrelation or heteroscedasticity by employing clustered standard errors.

4.5.2 Instrumental variable design

The spatial/leave-one-out instrument. [15] While time-invariant unobservables will be accounted for by including firm-level fixed effects (α_i), the issue of time-varying unobserved variation at the plant level remains an issue. There are many possibilities for how plant-level unobserved and time-varying variation could arise. For example, a newly assigned plant CEO could improve its

[15] We follow this instrumental variable design as it has been extensively used in the micro firm-level literature on this topic, particularly Amann et al. (2021); Calì et al. (2022); see Table 4.6 for a summary.

performance and energy prices through their exceptional management skills and potential personal or political proximity to local energy distributors or authorities. Consequently, the instrumental for plant-level energy prices is given by:

$$up_{it}^{m} = \alpha_0 + \alpha_1 inst_{it}^{m} + D_{st} + \tau_{rt} + \eta_{it}, \tag{4.5.3}$$

for $m = \{FF, EL\}$. The terms α_0, D_{st} and τ_{rt} have the same interpretation as in Equation 4.5.2. For the validity of the instruments ($inst_{it}^{m}$), we require that it affects a plant's performance only through its direct impact on plant-level prices. For this purpose, we exploit the data to isolate the geographical markup on energy prices resulting from the cost of distributing energy to a particular province. We recover this effect in the following way: First, for an arbitrary firm i and each of the two energy unit price indices up_{it}^{m}, we calculate the average unit prices paid by all other plants (i') in the same region r in a given year. Spatial instruments of this form have been criticised in the literature for not satisfying the exclusion restriction (Betz et al., 2018). Exemplarily, we have to rule out that the price paid by the other i' firm only affects firm i's performance via its energy prices and not directly through i's outcome variable(s). Similarly, we have to rule out that firm performance of all (i') firms does not affect firm i's performance. Both these assumptions may be difficult to justify, particularly in a narrowly defined manufacturing sector. We address this issue by only considering energy prices for all i' firms that are *not* located in the same ISIC II-digit sector s as firm i. Consequently, the instrumented energy unit prices are calculated as:

$$\bar{up}_{it}^{m} = \sum_{i \in r, i \neq i', s_i \neq s_{i'}}^{N_{rts}} \frac{up_{it}^{m}}{(N_{rt} - 1)}, \tag{4.5.4}$$

where N_{rts} denotes the number of plants in period t populating the same region as firm i, $i \in r$ that are not engaged in the same manufacturing activity, i.e. the same ISIC II-digit sector s, $s_i \neq s_{i'}$. Finally, we divide this quantity by the country-wide average of the same year to minimise the potential correlation of plant-level outcomes with aggregate shocks affecting each energy type as:

$$inst_{it}^{m} = \frac{\bar{up}_{it}^{m}}{\bar{up}_{t}^{m}}, \quad \bar{up}_{t}^{m} = \sum_{i}^{N} \frac{up_{it}^{m}}{N}, \tag{4.5.5}$$

where $\bar{up}_{t}^{m}$ is the average, national unit price of energy source m in year t. This instrument can be interpreted as a proxy for the cost of energy distribution to a particular region and is correlated with plant-level energy prices while mitigating the effect of idiosyncratic firm-level dynamics through the exclusion of the firms in question to calculate regional averages in Equation 4.5.4.

We provide the first-stage regression results shown in Table 4.7. The results confirm that the instruments predict plant-level energy prices for both energy sources. To this end, the first-stage results are similar to Amann et al. (2021); Calì et al. (2022). We report weak identification test statistics for all IV regressions following Chao and Swanson (2005).

4.5.3 Heterogeneities

To analyse heterogeneities in firm-level response patterns, we adjust Equation 4.5.1 employing a dummy identifier of the form:

$$y_{it} = (\boldsymbol{\beta}_i \mathbf{x}_{it})\, I_{(z_{it} \in Z)} + \varepsilon_{it}, \tag{4.5.6}$$

where $I(\cdot)$ is an indicator function which takes the value one if condition Z is met for a particular observation in our data z_{it}.

To this end, our results in their current forms extend to a disaggregated analysis based on *firm size*[16] and *energy intensity*[17] of the respective manufacturing firms; see tables 4.2 and 4.3 for frequency tabulations of the two firm characteristics. More extensive disaggregation and analysis of nonlinear effects hinted

[16] We classify according to commonly used size-related cut-offs for large (more than 50 employees), medium (between 20 and 49) and small (less than 20), which roughly splits our sample into three equally-proportioned blocks; see Table 4.2.

[17] We do so by classifying the manufacturing sectors as either energy-intensive or non-intensive following the sector classification of EIA (2022); see Table 4.3 for a frequency table conditional on energy intensity. We also test an energy-sector specification based on our manufacturing data by sorting the respective manufacturing sectors based on their average energy intensity. To this end, the energy-intensity classification obtained in this way is very similar in terms of sector identification. It leads to qualitatively similar results to the one obtained using the EIA classification. We only present results following the EIA classification.

at in *Stylised fact 4* remain subject to ongoing research.

4.6 Results

4.6.1 Main results

Table 4.8 confirms that fossil fuel increases leads to business upgrading for machinery and vehicles, while it has no discernible effect on ICT upgrading. Overall, fossil fuel hikes are found to increase the balance of net assets and lend support to the *weak* form of the Porter Hypothesis. In turn, we cannot find similar support for the notion that energy price increases may lead to capital upgrading for electricity price hikes. These results align with other studies differentiating firm-level coping strategies by energy type and underscore the importance of a disaggregated analysis.

The importance of differentiating electricity and fossil fuel hikes is further underscored in Table 4.9 and provides consistent evidence that electricity price increases are detrimental to firms' productivity. However, this is not the case for fossil fuel surges, which are found to improve output productivity and TFP. To this end, our results provide empirical evidence in favour of the *strong* version of the Porter Hypothesis.

Quantifying the *substitution effect*, Table 4.10 reveals that electricity and fossil fuel increases lead to a significant reduction in consumed quantities of electricity. The quantified effects for fossil fuel are smaller than for the case of Oman (Amann et al., 2021). In contrast, the observed electricity savings effect in response to EL hikes is significant and similar in magnitude to the Oman case. We also observe similar responses across all energy types in response to a surge in fossil fuel hikes, which are significant and positive. These results seem somehow at odds with the Oman study. However, closer inspection reveals that these results may stem from inter-fuel substitution at a more granular level which we cannot disentangle at the current level of analysis with aggregated fossil fuel price indices. We can find evidence in support of this notion in Rentschler and Kornejew (2018), who provide some interesting benchmark figures. Our estimated fossil fuel elasticities (of 0.17) correspond to similar quantity elasticities across the various fuel types analysed in Rentschler and Kornejew (2018, Table 4). Moreover, we find price-quantity elasticities for electricity, petrol, diesel

and LPG, which are comparable in magnitude and direction. Lastly, Rentschler and Kornejew (2018, Table 4) indicates notable inter-fuel substitutability where price increases in a particular fuel type correspond to positive quantity responses across various other fuel types. Consequently, results in Table 4.10 seem plausible, yet a more detailed analysis of substitution elasticity by energy type may shed further light on the substitutability of sources.

In terms of *absorption* dynamics Table 4.11 reveals that fossil fuel increases positively affect wages but not employment. Furthermore, the wage effect is only observed for the all-worker aggregate, not the production worker subsample. This result may indicate some worker reallocation (Brucal and Dechezleprêtre, 2021; Dechezleprêtre et al., 2020). More than that, it may indicate higher remuneration for potentially high-skilled non-production workers. On the other hand, electricity hikes seem to result in manufacturing firms shedding employment, particularly production workers.

Table 4.12 confirms that energy price increases uniformly negatively affect firms' profit margins. However, the transmission channels again vary by energy type. While fossil fuel increases result in a cost increase (presumably via higher wages), electricity surges seem to affect profit margins through a reduction in sales (and overall production output). The results for Return on Sales are consistent with the previous findings on sales costs and asset evolution: Investment in machinery and higher wages increase production costs with no effects on sales, reducing firms' profit margins.

To conclude, electricity price increases lead to reduced production and workforce reduction. On the other hand, fossil fuel hikes result in increased capital investment, manifesting in improved productivity, thereby supporting the *strong* version of the Porter Hypothesis. However, these productivity increases do not translate into higher profitability (at least not in the short-run), indicating that such price hikes are absorbed by firms, at least to a considerable extent. Given the current data limitations, it remains unclear to what degree Chilean manufacturing firms can pass some of the energy price increases on to their customers. Also, it remains a question for future research on how energy price hikes manifest themselves in firm exits.

4.6.2 Heterogeneities

Firm size. We perform a sub-sample analysis conditioning on firm size. We classify according to commonly used size-related cut-offs for large (more than 50 employees), medium (between 20 and 49) and small (less than 20), which roughly splits our sample into three equally-proportioned blocks; see Table 4.2.

Evaluating heterogeneities concerning the Porter hypothesis, Table 4.13 illustrates that medium and large firms primarily drive the upgrading effects described in subsection 4.6.1. Particularly for small firms, no capital upgrading effect can be observed. Most interestingly, the more disaggregate analysis indicates that large firms seem to upgrade machinery *and* ICT equipment, two results confirmed by earlier studies on this question in different country contexts (Amann et al., 2021; Calì et al., 2022). Table 4.14 indicates that productivity increases through capital upgrading in response to fossil fuel price hikes are only observed for medium and, most notably, large firms. Furthermore, the adverse productivity effects observed for electricity price hikes are experienced by small and medium-sized firms. These results indicate that the impacts of energy price increases are unequally balanced across firms based on their characteristics and that only large firms are in the position to reap the benefits of productivity effects that are driven by energy price increases. Particularly for smaller firms, energy price increases are particularly detrimental. Therefore, it stands to reason that the Porter Hypothesis may be less of a universal mechanism and more the function of past business performance. Table 4.15 indicates that intra-fuel elasticities do not vary notably depending on firm size. However, larger firms appear more likely to substitute electricity for fossil fuels in response to electricity hikes. This finding may partly explain why surges in electricity prices do not adversely affect large firms' business metrics as they do for small(er) enterprises. Finally, when assessing the absorption capacities of energy price surges by size, Table 4.16 indicates that particularly small firms see the most detrimental outcomes on their business metrics in response to energy price hikes, particularly for electricity. [18]

Energy-intensive sectors. We furthermore evaluate the impact of energy price variations conditional on energy consumption. We do so by classifying the manufacturing sectors as either energy-intensive or non-intensive follow-

[18] The effect of energy price on employment or wages does not vary extensively conditional on firm size, and the results are therefore omitted for brevity.

ing the sector classification of EIA (2022); see Table 4.3 for a frequency table conditional on energy intensity.

Capital upgrading is more prevalent for energy-intensive sectors; however, evidence for the weak Porter Hypothesis exists for energy-intensive and non-intensive sectors in response to fossil fuel surges (Table 4.17). Further evidence in support of the strong version of the Porter Hypothesis is again observed, most extensively, for the energy-intensive subsectors (Table 4.18). Our results also indicate that intra-fuel substitution is more prevalent for energy-intensive sectors (Table 4.19) and that energy-intensive sector firms are found to absorb energy price hikes more extensively than non-energy-intensive firms; see Table 4.20 and Table 4.21, respectively.

4.7 Conclusion

This paper contributes to the microdata firm-level literature and the energy-productivity price nexus in three ways.

First, it provides a systematic literature review of the most recent advancements in the field. It discusses firms' four main coping mechanisms in response to energy price hikes. Furthermore, it highlights a notable concentration of work on advanced Western economies as well as the lack of extensive work on the heterogeneous effects of these price hikes and, in particular, by differentiating between energy types. Secondly, the paper extends previous work on this question to the Chilean Annual National Industrial Survey data. Our results indicate that electricity price increases lead to reduced production and workforce reduction. On the other hand, fossil fuel hikes result in increased capital investment, manifesting in improved productivity, thereby supporting the *strong* version of the Porter Hypothesis. However, these productivity increases do not translate into higher profitability (at least not in the short-run), indicating that such price hikes are absorbed by firms, at least to a considerable extent.

Moreover, the heterogeneity analysis in this paper indicates that the strong version of the Porter Hypothesis can only be observed for large firms (with 50 employees or above) and that small firms are more negatively affected by surges in energy prices. Moreover, more energy-intensive firms are also found to experience more extensive Porter-type innovations, and their business metrics and employment are typically more affected by energy price hikes than firms in non-energy-intensive sectors.

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Appendix Chapter 4

4.A Supplementary tables

Table 4.1: Fossil fuel unit conversion

		Unit conversion		
Fossil fuel	Unit measure (um)	um/kg	um/m^3	um/kWh
Coal	kg	1	850	7.00
Diesel	litre	0.85	0.001	9.10
Gas	cubic meter (m^3)	0.51	0.270	8.80
Gasoline	litre	0.75	0.001	10.00
Kerosene	litre	0.80	0.001	10.35
LPG	litre	0.51	0.270	6.90

Source: Authors' calculations based on EIA (2022).

Table 4.2: Firm size by category

	N	%
Responses		
Large (>=50)	32252	32.26
Medium (>=20, <=49)	32391	32.40
Small (<=19)	35235	35.24
Non-responses		
NA	96	0.10

Note: Total responses: 99974 observations of manufacturing firms in Chile between 1995-2015 is the sum of columns 'N'. Column '%' sum up to 100% (with rounding errors).

Table 4.3: Energy intensity

	N	%
Responses		
Energy-intensive	64379	64.4
Non-intensive	35595	35.6

Note: Total responses: 99974 observations of manufacturing firms in Chile between 1995-2015 is the sum of columns 'N'. Column '%' sum up to 100% (with rounding errors).
Energy-intensity classification following EIA (2022); last visit September 2022.

Table 4.4: Data availability by year and energy type, Chile

	Share non-missing observations							
	Electricity	Coke	Diesel	Gas	Kerosene	LPG	Petrol	Pipe Gas
1995	99	3	34	0	6	32	33	6
1996	98	3	35	0	6	32	33	6
1997	98	3	38	0	5	34	33	7
1998	98	3	40	4	4	32	34	6
1999	97	3	40	6	5	33	28	5
2000	99	3	41	7	0	33	1	5
2001	98	2	42	7	0	33	3	5
2002	99	3	41	8	0	33	3	5
2003	99	2	45	9	3	34	27	4
2004	99	2	49	9	3	34	32	5
2005	99	2	48	9	3	35	30	5
2006	99	1	52	10	3	35	29	5
2007	99	1	53	9	3	35	28	4
2008	99	1	54	16	3	35	33	0
2009	99	1	52	19	3	33	32	0
2010	98	1	51	19	3	33	31	0
2011	97	2	49	20	3	32	29	0
2012	95	1	50	20	3	33	26	0
2013	93	1	52	16	2	25	23	0
2014	87	1	53	15	2	24	21	0
2015	71	1	49	16	1	23	18	0

Note: Rounded shares.

Source: Authors' calculations based on INE (2015).

Table 4.5: Descriptive statistics

Variable						Responses	
Abreviation	Variable description	Mean	Med.	LQ	UQ	N	%
Independen variables							
EL	Electricity unit price; see Equation 4.4.1	0.1	0.1	0.1	0.1	93868	97.17
FF	Fossil fuel unit price; see Equation 4.4.2	2.2	1.8	0.9	3.0	69582	72.03
Asset Investment (weak Porter Hypothesis)							
Machinery/output	Investment in fixed assets, machinery and equipment/gross value of net production	0.1	0.0	0.0	0.0	51278	53.08
ICT/output	Investment in fixed assets, software and ICT equipment/gross value of net production	0.0	0.0	0.0	0.0	2492	2.58
Vehicles/output	Investment in fixed assets, vehicles/gross value of net production	0.0	0.0	0.0	0.0	22782	23.58
Asset balance/output	Net balance of fixed assets at the end of the period/gross value of net production	0.1	0.0	0.0	0.1	61082	63.23
Productivity (strong Porter Hypothesis)							
Output/worker	Gross value of net production/average total contract workers	68599.8	23556.5	13481.7	48014.4	95883	99.26
Value-added/worker	Net value-added/average total contract workers	24098.2	8082.3	4409.2	15412.2	95892	99.27
TFP	Total Factor Productivity following Ackerberg et al. (2015) with Cobb Douglas production function	9.6	9.7	8.8	10.5	16189	16.76
Substitution							
Qnt. Electricity	Quantity of electricity consumed	10972.0	84.0	27.0	414.0	94724	98.06
Qnt. Coke	Quantity of coke consumed	5670.3	307.0	30.0	3037.5	2034	2.11
Qnt. Diesel	Quantity of diesel consumed	690.2	26.0	8.0	91.0	44022	45.57
Qnt. Petrol	Quantity of petrol consumed	0.4	0.4	0.2	0.5	24388	25.25
Qnt. LPG	Quantity of LPG consumed	155.2	5.0	2.0	18.0	31612	32.72
Qnt. Pipe gas	Quantity of pipe gas consumed	83686.9	6388.0	1855.0	25257.0	3727	3.86
Qnt. Natural gas	Quantity of natural gas consumed	7298234.7	28265.0	4360.0	217499.0	8907	9.22
Absorption: Workforce							
Wages all workers	Average remuneration total contract workers	6218.2	4857.2	3126.4	7632.8	95863	99.24
Wages production workers	Average remuneration workers associated with the industrial process	4060.8	2877.8	1155.5	5044.2	60558	62.69

Table 4.5: Descriptive statistics *(continued)*

Abreviation	Variable description	Mean	Med.	LQ	UQ	N	%
Emp. all workers	Average total contract workers	68.8	26.0	15.0	62.0	95992	99.37
Emp. production workers	Average contract workers associated with the industrial process	51.8	19.0	10.0	46.0	95166	98.52
Absorption: Business metrics							
Profit margin	(Total net income from the sale of products and work performed / Total net cost of goods received and work performed under contract - 1)	0.3	0.4	0.2	0.5	96381	99.77
Sales/output	Total net income from the sale of products and work performed/gross value of net production	1.0	1.0	1.0	1.0	96375	99.77
Costs/output	Total net cost of goods received and work performed under contract/gross value of net production	0.6	0.6	0.5	0.8	96393	99.79
Return on Sales	Total net income from the sale of products and work performed - Total net cost of goods received and work performed under contract - Net balance of fixed assets at the end of the period	10168.5	13684.2	-20558.8	129285.1	93748	97.05

Note: LQ/UP: Lower and upper quartile. Top/bottowm 0.1 per cent trimmed. ISIC II-digit industry-level defators following Haraguchi and Amann (2022)
Source: Authors' calculations based on INE (2015).

Table 4.6: Overview related literature

	Amann et al. (2021)
Data	• Oman; Annual Industrial Survey; 3600 mnf. firms (2012-2017).
Method	• FE-IV (leave-one-out/spatial instrument). • CCE (non-IV).
Variables	• *Dependent:* value-added/employment; output/employment; TFP; gross profit margin; q^m; machinery and ICT sales and purchases • *Independent:* up^m, various sector- & region-controls.
Finding	• Fossil fuel energy factor prices lead to improvements in productivity as well as efficiency and notable business upgrading. • Positive firm-level outcomes are largely driven by investment in Information and Communication Technology (ICT). • Effects more pronounced for fuel-intensive industries.
	Brucal and Dechezleprêtre (2021)
Data	• Indonesia; mnf. industry; covering all medium and large enterprises (1980-2015).
Method	• FE-IV - Fixed-weight Energy Price Index (Sato et al., 2019): $$(\dots) = \sum_j w^j_{i0} ln(p^j_{kt}),$$ where w^j_{i0} is the share of energy sorce j in firm i in $t = 0$, p^j_{kt} is *province* k*'s median* price of source j where i operates in t (also see Marin and Vona, 2021). • Do not differentiate between energy types; $up_{it} = \sum_j w^j_{it} up^j_{it}$.
Variables	• *Dependent:* energy use; CO2 emissions; output; employment; energy and CO2 intensity; energy/worker; capital and capital intensity; purchases/sales of land, buildings, machinery, vehicles. • *Independent:* energy price (firm-level average energy price across energy sources weighted by firm-level consumption-share by energy type), various sector- & region-controls.
Finding	• Energy price increases reduce energy use and CO2 emissions • Effect varies across sectors, more pronounced for energy-intensive sectors • Price surges increase probability of firm exits.

Table 4.6 continued from previous page

	Calì et al. (2022)
Data	• Indonesia; Statistik Industri: mnf. firms with 20+ employees (1998-2015). • Mexico; Encuesta Anual de la Industria Manufacturera; mfn. firms (2009-2015).
Method	• FE-IV (leave-one-out/spatial instrument).
Variables	• *Dependent:* TFP; profitability; value-added/employment; value-added/KWh; energy efficiency; machine turnover • *Independent:* up^m, various sector- & region-controls.
Finding	• Fuel price hikes result in higher productivity and profits of manufacturing plants. • Inefficient fuel-powered capital is replaced with more productive electricity-powered equipment. • Machinery turnover is less responsive to fuel price hikes for firms operating "frontier" technologies (categorised as electricity-intensive plants, as well as foreign-owned and exporting plants).
	Calì et al. (2021)
Data	• World Bank's Enterprise Surveys for 11 countries between the years 2002 and 2013
Method	• FE model with time effects and country-specific and foreign-owned trends. • Energy intensity is interacted with country-sector specific energy prices. • Test if more intensive firms are more responsive to energy price changes. • Use time-invariant measure of energy intensity and measure sector-country level (rather than firm-level) energy prices to reduce degree to which unobserved time-varying firm-level shocks might correlate with energy prices and performance.
Variables	• *Dependent:* Total employment; sales/(total employment); value-added/(total employment); returns on sales; export share. • *Independent:* Energy price index, interacted with energy export share; firm size; ownership; energy-outage dummy; R&D dummy. The Energy price index (EP) given by $$EP_{cst} = log\left(\sum_j \theta_{jcs,1995} \times p_{jct}\right)$$ where $\theta_{jcs,1995}$ is the share of energy source j (e.g. crude oil, natural gas, electricity, etc.) over total energy use of sector s in country c in year 1995 and p_{jct} is the real price of energy source j in country c and year t.

Table 4.6 continued from previous page

Finding	• Fails to reject hypothesis that higher energy prices are positively related to better economic performance. • Energy intensity negatively affects performance. • Size and R&D activities facilitate response of price hikes. • In response to energy price increases firms "pass-on", "absorption" and undergo Porter-style "innovation", the latter especially among large firms and firms engaged in R&D.
	Marin and Vona (2021)
Data	• France; mnf. industry (1997-2015).
Method	• FE-IV - Shift-share instrument: $$(\dots) = \sum_j w_{i0}^j ln(p_t^j),$$ where w_{i0}^j is the share of energy sorce j in firm i in $t = 0$, p_t^j is *national average*price of source j where i operates in t (also see Brucal and Dechezleprêtre, 2021). • Do not differentiate between energy types; $up_{it} = \sum_j w_{it}^j up_{it}^j$.
Variables	• *Dependent:* energy consumption; CO2 emissions; employment; annual wage; employment share by occupation group. • *Independent:* energy price (in KWh, firm-level average energy price across energy sources weighted by firm-level consumption-share by energy type), initial capital stock of firm j in $t = 0$, various sector- & region-controls.
Finding	• Energy price increases reduce energy use and CO2 emissions as well as employment (socioeconomic trade-off). • Heterogeneous wage effect (wage increase for technician,s decrease for low-skilled workers). • Price surges have the largest effect is for large (multi-) establishment in energy-intensive and trade-exposed sectors. • Modest effect on productivity, energy price increases drive up demand for capital.

Table 4.6 continued from previous page

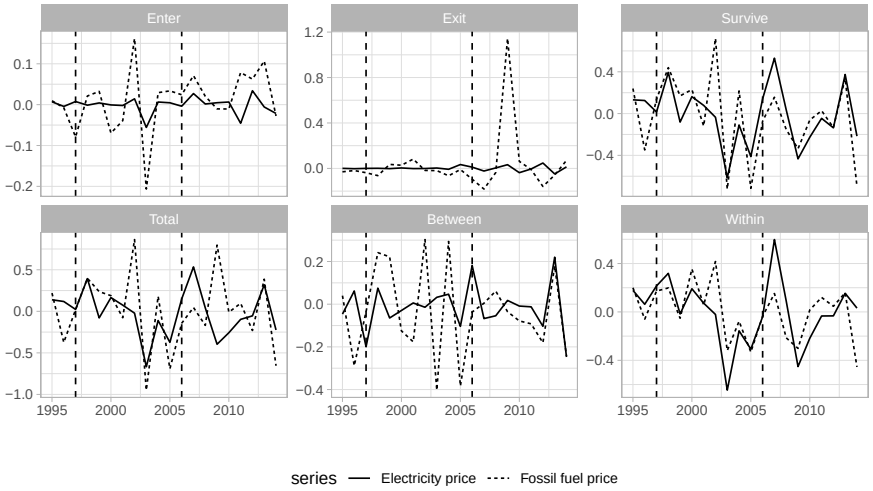
	Rentschler and Kornejew (2018)
Data	• Indonesia; 41,402 small and micro mining and manufacturing firms (2013).
Method	• Pooled/cross-section.
Variables	• *Dependent:* cost share • *Independent:* prices of electricity, petrol, diesel, kerosene, LP, sector- & region-controls.
Finding	• Higher energy prices are shown to have a small adverse effect on firms' long-run competitiveness. • Fuel substitution, energy efficiency, and cost pass-on are used by firms to adapt to higher prices.

Abbreviations: up^m: unit price of energy type m; q^m: physical quantity of energy type m; $m = \{Electricity, Fossil\ Fuel\}$; $j = \{electricity, naturalgas, petrol, \dots\}$.
KWh: Kilowatt hours; TFP: Total Factor Productivity; FE: Fixed effects; IV: Instrumental Variable.
Source: Author's elaboration.

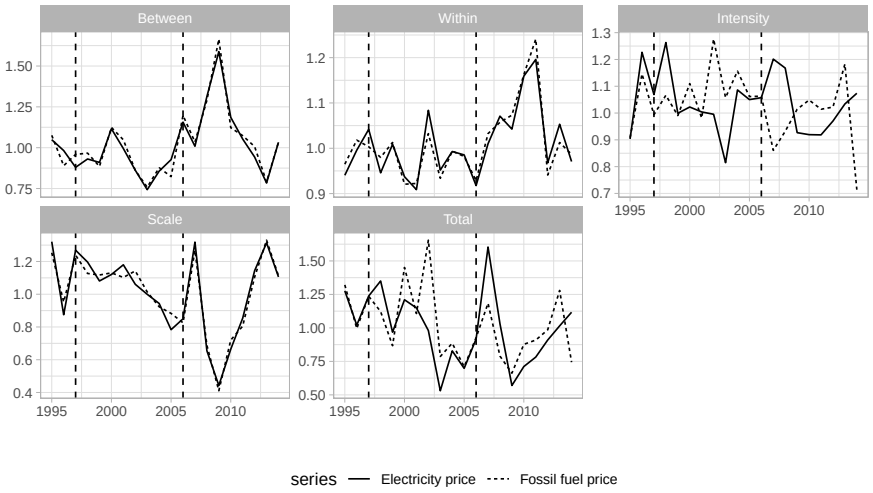
4.B Supplementary figures

Figure 4.5: Decomposition of energy price indices

(a) Melitz and Polanec (2015) decomposition

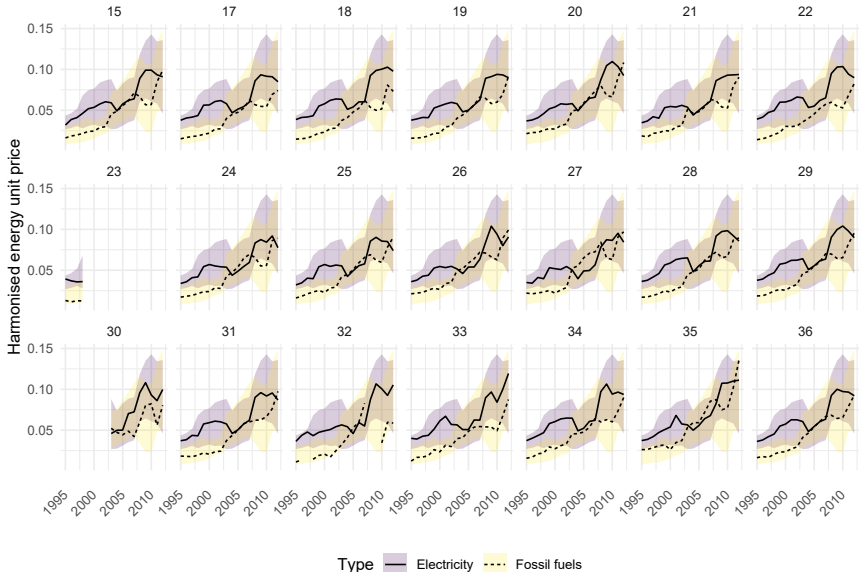


(b) Mean Divisa Index decomposition



Note: Years 1997, 2006 highlighted with dashed lines. Figure 4.5a decomposes total changes in electricity and fossil fuel prices following Melitz and Polanec (2015). The decomposition disaggregates overall trends into (i) changes for firms that enter the market from one period to the next; (ii) changes for firms that exit the market from one period to the next; (iii) changes for firms that remain (survive) in the market in both periods. These changes are further decomposed into: (iii.a) between changes measure the changes in the market shares of all surviving firms between both periods; (iii.b) within changes measure the average change witnessed across all surviving firms between both periods. Figure 4.5b decomposes total changes in electricity and fossil fuel prices using the Mean Divisa Index into the following components: (i) scale effect, which measures the changes in the overall size of the economy; (ii) intensity effect, which measures the energy intensity of productions of a firm; (iii) between changes, which measure the reallocation between sectors; (iv) within changes, which measure the reallocation between firms within a given sector. See appendix 4.F for more information on deposition techniques.
Source: Authors' calculations based on INE (2015).

Figure 4.6: Energy unit prices by energy type and ISIC sector over time, Chile

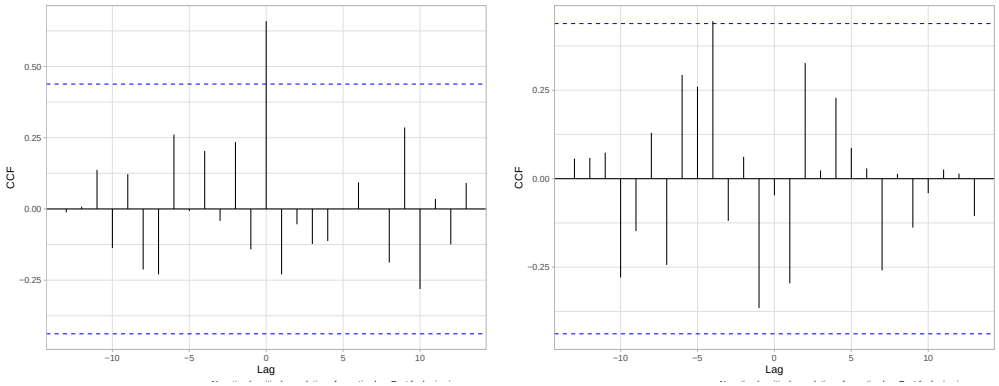


Note: Harmonised energy unit prices by energy type and ISIC Rev. 3. II-digit industry-level deflators following Haraguchi and Amann (2022). Dashed line: median. Shaded area: 5^{th}-95^{th} percentile.
Source: Authors' calculations based on INE (2015).

Figure 4.7: Cross correlation of energy price indices

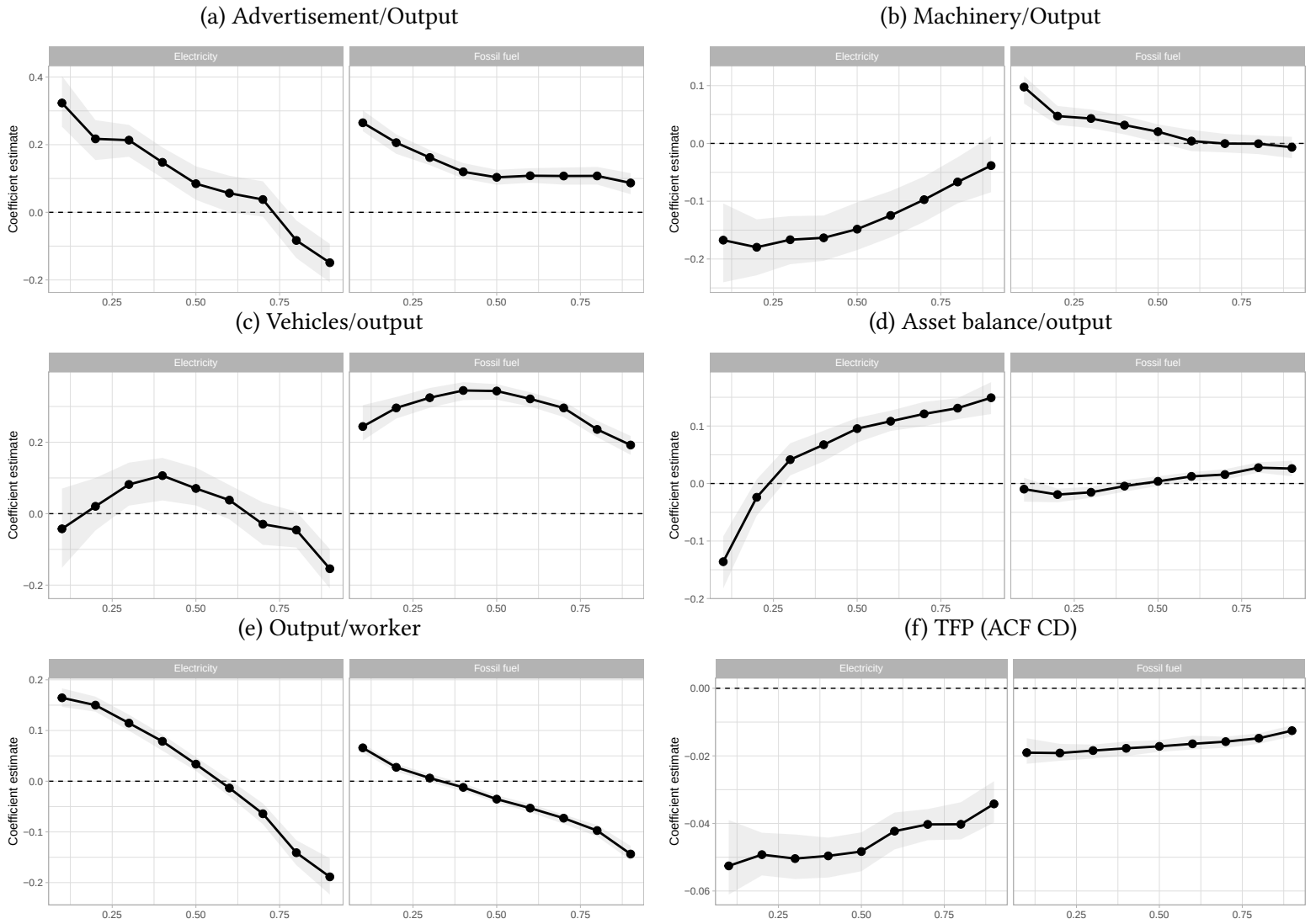
(a) Surviving firms

(b) Intensity effect



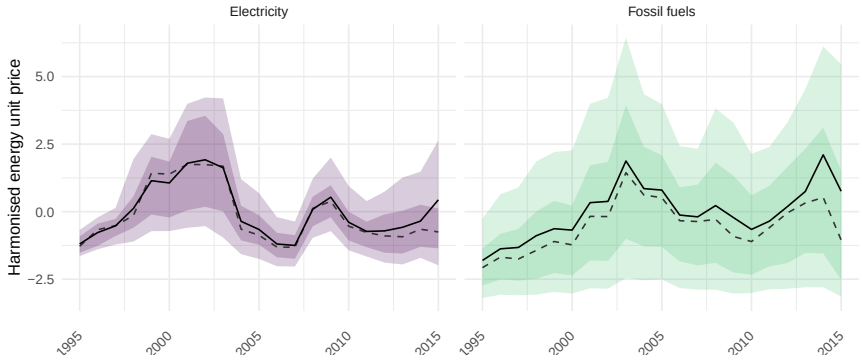
Note: Negative (positive) correlation of negative lag: Past fuel price increases (decrease) correlated with decreases (increases) in energy prices.
Source: Authors' calculations based on INE (2015).

Figure 4.9: Non-linear correlations



Note: Pooled quantile regression model. Quantiles on x-axis.
Source: Authors' calculations based on INE (2015).

Figure 4.8: Harmonised energy unit prices by energy type over time, Chile



Note: ISIC II-digit industry-level deflators following Haraguchi and Amann (2022). Dashed line: median. Shaded area: $10^{th}/90^{th}$ (light); $25^{th}/75^{th}$ (darker) percentile.
Source: Authors' calculations based on INE (2015).

4.C Empirical results

4.C.1 First stage

Table 4.7: First Stage IV

Model:	EL (1)	FF (2)
Variables		
EL IV	0.889***	-0.034
	(0.025)	(0.026)
FF IV	-0.115***	0.809***
	(0.037)	(0.045)
Fixed-effects		
Plant	Yes	Yes
Industry-Year	Yes	Yes
Region-Year	Yes	Yes
Fit statistics		
Adjusted R^2	0.60316	0.77699
Within Adjusted R^2	0.05239	0.01972
Observations	48,868	48,868

Clustered (Plant % Year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

4.C.2 Main results

Table 4.8: Baseline results: Asset Investment (weak PH) - IV

Model:	Machinery/output (1)	ICT/output (2)	Vehicles/output (3)	Asset balance/output (4)
Variables				
EL	-0.007	-0.012	0.026	-0.022
	(0.025)	(0.097)	(0.034)	(0.023)
FF	0.029*	0.084	0.039*	0.031***
	(0.014)	(0.048)	(0.021)	(0.008)
Fixed-effects				
Plant	Yes	Yes	Yes	Yes
Industry-Year	Yes	Yes	Yes	Yes
Region-Year	Yes	Yes	Yes	Yes
Fit statistics				
Adjusted R^2	0.34141	0.33433	0.55690	0.67130
Within Adjusted R^2	0.00132	0.00552	0.00149	0.01476
Observations	39,922	2,143	18,719	63,920
Weak id. test	114.9	38.9	79.9	110.9

Clustered (Plant & Year) standard-errors in parentheses..
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.*

Table 4.9: Baseline results: Productivity (strong PH) - IV

Model:	Output/worker (1)	Value-added/worker (2)	TFP (3)
Variables			
EL	-0.017*	-0.018*	-0.006*
	(0.009)	(0.009)	(0.003)
FF	0.007**	0.004	0.006**
	(0.003)	(0.005)	(0.002)
Fixed-effects			
Plant	Yes	Yes	Yes
Industry-Year	Yes	Yes	Yes
Region-Year	Yes	Yes	Yes
Fit statistics			
Adjusted R^2	0.86420	0.70161	0.80943
Within Adjusted R^2	0.00296	0.00221	0.00236
Observations	67,982	65,502	13,470
Weak id. test	73.5	92.6	83.7

Clustered (Plant & Year) standard-errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.*

Table 4.10: Baseline results: Substitution - IV

Model:	Qnt. Electricity (1)	Qnt. Coke (2)	Qnt. Diesel (3)	Qnt. Petrol (4)	Qnt. LPG (5)	Qnt. Pipe gas (6)	Qnt. Natural gas (7)
Variables							
EL	-0.623***	-0.401*	0.031	0.033*	0.077*	0.216*	-0.137
	(0.183)	(0.202)	(0.040)	(0.018)	(0.039)	(0.103)	(0.081)
FF	0.171***	0.104*	0.493***	0.125***	0.496***	-0.197***	0.549***
	(0.014)	(0.050)	(0.043)	(0.012)	(0.059)	(0.061)	(0.101)
Fixed-effects							
Plant	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics							
Adjusted R^2	0.83264	0.86092	0.71762	0.88940	0.69267	0.83596	0.76687
Within Adjusted R^2	0.05607	0.07970	0.07288	0.08538	0.08445	0.05737	0.06037
Observations	68,242	2,002	42,925	23,994	31,197	2,483	8,716
Weak id. test	93.9	89.4	119.5	90.7	95.2	81.2	85.5

Clustered (Plant & Year) standard-errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.*

Table 4.11: Baseline results: Absorption (workers) - IV

Model:	Wages all workers (1)	Wages prod. workers (2)	Emp. all workers (3)	Emp. prod. workers (4)
Variables				
EL	0.002	-0.012	-0.027***	-0.036***
	(0.007)	(0.015)	(0.008)	(0.011)
FF	0.005**	0.003	0.004	0.005
	(0.002)	(0.006)	(0.004)	(0.005)
Fixed-effects				
Plant	Yes	Yes	Yes	Yes
Industry-Year	Yes	Yes	Yes	Yes
Region-Year	Yes	Yes	Yes	Yes
Fit statistics				
Adjusted R^2	0.84395	0.69907	0.89582	0.87418
Within Adjusted R^2	0.00124	0.00820	0.00163	0.00176
Observations	67,967	45,782	68,004	67,575
Weak id. test	93.7	102.6	37.1	75.6

Clustered (Plant & Year) standard-errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.*

Table 4.12: Baseline results: Absorption (business metrics) - IV

Model:	Profit margin (1)	Sales/output (2)	Costs/output (3)	Return on Sales (4)
Variables				
EL	-0.016*	-0.003*	0.003	-0.044*
	(0.008)	(0.002)	(0.005)	(0.023)
FF	-0.010**	0.0004	0.005*	-0.017*
	(0.004)	(0.0009)	(0.002)	(0.010)
Fixed-effects				
Plant	Yes	Yes	Yes	Yes
Industry-Year	Yes	Yes	Yes	Yes
Region-Year	Yes	Yes	Yes	Yes
Fit statistics				
Adjusted R^2	0.39673	0.22942	0.47037	0.77731
Within Adjusted R^2	0.00055	0.00025	0.00043	0.00102
Observations	64,959	68,189	68,228	35,097
Weak id. test	72.4	99.1	88.5	95.4

Clustered (Plant & Year) standard-errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.*

4.C.3 Heterogeneities

Table 4.13: Firm-size: Asset Investment (weak PH) - IV

Model:	Machinery/output (1)	ICT/output (2)	Vehicles/output (3)	Asset balance/output (4)
Variables				
EL × firm = large	0.003	-0.068	0.002	-0.046
	(0.035)	(0.139)	(0.040)	(0.043)
EL × firm = medium	-0.065	-0.050	0.045	-0.027
	(0.041)	(0.286)	(0.068)	(0.025)
EL × firm = small	0.059	-1.17	0.047	0.004
	(0.062)	(0.723)	(0.070)	(0.022)
FF × firm = large	0.042***	0.096*	0.039	0.040***
	(0.014)	(0.047)	(0.025)	(0.011)
FF × firm = medium	0.033**	0.211	0.051	0.032**
	(0.014)	(0.154)	(0.035)	(0.013)
FF × firm = small	0.015	-0.318	0.059	0.005
	(0.038)	(0.513)	(0.050)	(0.017)
Fixed-effects				
Plant	Yes	Yes	Yes	Yes
Industry-Year	Yes	Yes	Yes	Yes
Region-Year	Yes	Yes	Yes	Yes
Fit statistics				
Adjusted R^2	0.34405	0.34796	0.56102	0.67259
Within Adjusted R^2	0.00478	0.02571	0.01060	0.01862
Observations	39,901	2,142	18,709	63,920
Weak id. test	59.4	102.0	69.8	110.1

Clustered (Plant & Year) standard-errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.*

Table 4.14: Firm-size: Productivity (strong PH) - IV

Model:	Output/worker (1)	Value-added/worker (2)	TFP (3)
Variables			
EL × firm = large	-0.013	0.010	-0.002
	(0.013)	(0.016)	(0.002)
EL × firm = medium	-0.032**	-0.036**	-0.003
	(0.013)	(0.014)	(0.003)
EL × firm = small	-0.030**	-0.054**	-0.008
	(0.012)	(0.020)	(0.005)
FF × firm = large	0.016**	0.007	0.01*
	(0.006)	(0.008)	(0.007)
FF × firm = medium	0.012**	0.007	0.002
	(0.005)	(0.007)	(0.002)
FF × firm = small	-0.004	0.004	-0.003
	(0.007)	(0.010)	(0.002)
Fixed-effects			
Plant	Yes	Yes	Yes
Industry-Year	Yes	Yes	Yes
Region-Year	Yes	Yes	Yes
Fit statistics			
Adjusted R^2	0.86847	0.70482	0.93917
Within Adjusted R^2	0.03438	0.01270	0.68158
Observations	67,940	65,462	13,470
Weak id. test	114.9	38.9	79.9

Clustered (Plant & Year) standard-errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.*

Table 4.15: Firm-size: Substitution - IV

Model:	Qnt. Electricity (1)	Qnt. Coke (2)	Qnt. Diesel (3)	Qnt. Petrol (4)	Qnt. LPG (5)	Qnt. Pipe gas (6)	Qnt. Natural gas (7)
Variables							
EL × firm = large	-0.652***	-0.637**	-0.103*	0.057***	0.062	0.324***	-0.187
	(0.210)	(0.284)	(0.053)	(0.020)	(0.048)	(0.096)	(0.117)
EL × firm = medium	-0.579***	-0.164	0.023	0.028	0.105**	0.108	-0.112
	(0.180)	(0.188)	(0.038)	(0.022)	(0.049)	(0.114)	(0.133)
EL × firm = small	-0.609***	0.037	0.068	0.008	0.138**	0.133	0.003
	(0.139)	(0.254)	(0.052)	(0.015)	(0.055)	(0.151)	(0.159)
FF × firm = large	0.154***	0.087	0.468***	0.089***	0.535***	-0.193**	0.452***
	(0.015)	(0.064)	(0.038)	(0.011)	(0.072)	(0.072)	(0.108)
FF × firm = medium	0.174***	0.069	0.496***	0.143***	0.467***	-0.205**	0.652***
	(0.023)	(0.062)	(0.054)	(0.013)	(0.071)	(0.080)	(0.113)
FF × firm = small	0.223***	0.185*	0.517***	0.171***	0.372***	-0.024	0.777***
	(0.020)	(0.095)	(0.059)	(0.013)	(0.067)	(0.155)	(0.163)
Fixed-effects							
Plant	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics							
Adjusted R^2	0.83646	0.86870	0.72221	0.89120	0.69667	0.85225	0.77217
Within Adjusted R^2	0.07717	0.13117	0.08655	0.10048	0.09632	0.15094	0.08260
Observations	68,200	2,002	42,895	23,985	31,180	2,483	8,708
Weak id. test	110.9	81.9	73.8	116.3	73.5	92.6	83.7

Clustered (Plant & Year) standard-errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.*

Table 4.16: Firm-size: Absorption (business metrics) - IV

Model:	Profit margin (1)	Sales/output (2)	Costs/output (3)	Return on Sales (4)
Variables				
EL × firm = large	0.013	-0.005	-0.006	0.004
	(0.014)	(0.005)	(0.008)	(0.035)
EL × firm = medium	-0.020	-0.014**	0.001	-0.022
	(0.012)	(0.006)	(0.007)	(0.029)
EL × firm = small	-0.053***	-0.010**	0.017**	-0.075**
	(0.016)	(0.004)	(0.006)	(0.032)
FF × firm = large	-0.024***	0.001	0.010**	-0.008
	(0.006)	(0.005)	(0.003)	(0.017)
FF × firm = medium	-0.007	0.002	0.005*	-0.011
	(0.006)	(0.003)	(0.002)	(0.016)
FF × firm = small	-0.015*	0.0004	0.005*	-0.019
	(0.008)	(0.006)	(0.002)	(0.023)
Fixed-effects				
Plant	Yes	Yes	Yes	Yes
Industry-Year	Yes	Yes	Yes	Yes
Region-Year	Yes	Yes	Yes	Yes
Fit statistics				
Adjusted R^2	0.39854	0.53877	0.47258	0.78191
Within Adjusted R^2	0.00351	0.00998	0.00452	0.02166
Observations	64,920	64,705	68,186	35,097
Weak id. test	43.7	111.5	55.9	38.6

Clustered (Plant & Year) standard-errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.*

Table 4.17: Energy-intensity: Asset Investment (weak PH) - IV

Model:	Machinery/output (1)	ICT/output (2)	Vehicles/output (3)	Asset balance/output (4)
Variables				
EL × sector = Intensive	-0.008	-0.024	0.004	-0.034
	(0.028)	(0.135)	(0.036)	(0.023)
EL × sector = Non-intensive	-0.010	-0.118	0.089*	-0.004
	(0.036)	(0.231)	(0.046)	(0.029)
FF × sector = Intensive	0.037**	0.092	0.048**	0.037***
	(0.015)	(0.058)	(0.023)	(0.009)
FF × sector = Non-intensive	0.017*	0.126	0.021	0.028***
	(0.006)	(0.106)	(0.035)	(0.013)
Fixed-effects				
Plant	Yes	Yes	Yes	Yes
Industry-Year	Yes	Yes	Yes	Yes
Region-Year	Yes	Yes	Yes	Yes
Fit statistics				
Adjusted R^2	0.34368	0.34480	0.55931	0.67234
Within Adjusted R^2	0.00476	0.02116	0.00693	0.01789
Observations	39,922	2,143	18,719	63,920
Weak id. test	114.9	38.9	79.9	110.9

Clustered (Plant & Year) standard-errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.*

Table 4.18: Energy-intensity: Productivity (strong PH) - IV

Model:	Output/worker (1)	Value-added/worker (2)	TFP (3)
Variables			
EL × sector = Intensive	-0.015*	-0.011	-0.003*
	(0.009)	(0.010)	(0.002)
EL × sector = Non-intensive	-0.022*	-0.036**	-0.005
	(0.011)	(0.015)	(0.003)
FF × sector = Intensive	0.009*	0.010	0.005***
	(0.004)	(0.007)	(0.0008)
FF × sector = Non-intensive	0.008*	-0.010	0.003
	(0.005)	(0.007)	(0.002)
Fixed-effects			
Plant	Yes	Yes	Yes
Industry-Year	Yes	Yes	Yes
Region-Year	Yes	Yes	Yes
Fit statistics			
Adjusted R^2	0.86491	0.70226	0.93817
Within Adjusted R^2	0.00815	0.00440	0.67632
Observations	67,982	65,502	13,470
Weak id. test	73.5	92.6	83.7

Clustered (Plant & Year) standard-errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.*

Table 4.19: Energy-intensity: Substitution - IV

Model:	Qnt. Electricity (1)	Qnt. Coke (2)	Qnt. Diesel (3)	Qnt. Petrol (4)	Qnt. LPG (5)	Qnt. Pipe gas (6)	Qnt. Natural gas (7)
Variables							
EL × sector = Intensive	-0.624***	-0.587**	0.007	0.036*	0.085*	0.226**	-0.194**
	(0.181)	(0.273)	(0.037)	(0.020)	(0.045)	(0.094)	(0.085)
EL × sector = Non-intensive	-0.628***	-0.141	0.102	0.032	0.074	0.260**	-0.026
	(0.182)	(0.223)	(0.062)	(0.019)	(0.058)	(0.102)	(0.171)
FF × sector = Intensive	0.172***	0.114*	0.514***	0.118***	0.491***	-0.174**	0.535***
	(0.015)	(0.062)	(0.046)	(0.013)	(0.065)	(0.065)	(0.103)
FF × sector = Non-intensive	0.171***	0.005	0.402***	0.140***	0.499***	-0.192	0.591***
	(0.020)	(0.078)	(0.048)	(0.013)	(0.088)	(0.113)	(0.128)
Fixed-effects							
Plant	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics							
Adjusted R^2	0.83413	0.86756	0.71889	0.89028	0.69529	0.85160	0.77084
Within Adjusted R^2	0.06447	0.12366	0.07704	0.09266	0.09225	0.14721	0.07635
Observations	68,242	2,002	42,925	23,994	31,197	2,483	8,716
Weak id. test	93.9	89.4	119.5	90.7	95.2	81.2	85.5

Clustered (Plant & Year) standard-errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.*

Table 4.20: Energy-intensity: Absorption (workers) - IV

Model:	Wages all workers (1)	Wages production workers (2)	Emp. all workers (3)	Emp. production workers (4)
Variables				
EL × sector = Intensive	0.004	-0.032	-0.050***	-0.063***
	(0.007)	(0.023)	(0.009)	(0.012)
EL × sector = Non-intensive	-0.004	0.037*	-0.016*	-0.025**
	(0.007)	(0.019)	(0.010)	(0.013)
FF × sector = Intensive	0.008***	-0.002	0.006	0.008
	(0.003)	(0.008)	(0.005)	(0.006)
FF × sector = Non-intensive	-0.003	0.003	-0.0008	-0.005
	(0.003)	(0.009)	(0.006)	(0.007)
Fixed-effects				
Plant	Yes	Yes	Yes	Yes
Industry-Year	Yes	Yes	Yes	Yes
Region-Year	Yes	Yes	Yes	Yes
Fit statistics				
Adjusted R^2	0.84452	0.70472	0.89703	0.87583
Within Adjusted R^2	0.00484	0.02682	0.01328	0.01485
Observations	67,967	45,782	68,004	67,575
Weak id. test	93.7	102.6	37.1	75.6

Clustered (Plant & Year) standard-errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.*

Table 4.21: Energy-intensity: Absorption (business metrics) - IV

Model:	Profit margin (1)	Sales/output (2)	Costs/output (3)	Return on Sales (4)
Variables				
EL × sector = Intensive	-0.033**	-0.023***	0.006	-0.085**
	(0.014)	(0.007)	(0.009)	(0.032)
EL × sector = Non-intensive	-0.009	-0.004	0.002	-0.025
	(0.010)	(0.004)	(0.004)	(0.024)
FF × sector = Intensive	-0.014*	0.005	0.010**	-0.019
	(0.007)	(0.006)	(0.004)	(0.019)
FF × sector = Non-intensive	-0.009	-0.0003	0.003	-0.017
	(0.005)	(0.003)	(0.003)	(0.011)
Fixed-effects				
Plant	Yes	Yes	Yes	Yes
Industry-Year	Yes	Yes	Yes	Yes
Region-Year	Yes	Yes	Yes	Yes
Fit statistics				
Adjusted R^2	0.39798	0.53916	0.47225	0.77828
Within Adjusted R^2	0.00262	0.01106	0.00398	0.00538
Observations	64,959	64,742	68,228	35,097
Weak id. test	47.1	70.2	70.2	66.4

Clustered (Plant & Year) standard-errors in parentheses.
Weak identification test statistic for instrumental variables following Chao and Swanson (2005).
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.*

4.D Firm-level matching algorithm

The Annual National Industrial Survey (ENIA, Estructura de la industria manufacturera) is made available by the National Statistical Institute (INE) in two distinct forms: (1) A combined data set that identifies the same manufacturing firm with a unique ID over time but fails to provide information on the location of firms or their ISIC 4-digit classification code. While this information is available in the corresponding raw data files of ENIA, these data do not use a firm-level identifier which differs (a) from that of the combined data set and (b) varies over time: Between 20995 to 2007, a constant 5-digit ID is provided in the data while after that, the firm-level identifier change in structure. To extract information of the regions as well as the ISIC 4-digit identification of the respective firms which are present in the raw data but not the combined data sets made available by INE, we use firm-level characteristics to match firms between the "combined" and raw data sets.

The proposed algorithm achieves a successful match are of $> 95\%$ by employing the following steps:

- For every year, check if the following characteristics produce an *exact match* between the combined and raw data sets, respectively:
  - Year of survey.
  - 3-digit ISIC sector.
  - Share foreign/domestic ownership.
  - Firm size. There exists some degree of ambiguity between the combined and raw firm data when classifying firm sizes which requires the re-classify size of 8 groups in the raw data to 4 groups in the harmonised data. This reclassification is 1-to-1 for all but 1 out of the 8 groups identified in the raw data; see Table 4.22. If a firm is in category 6 in the raw data, it can be matched to *either* code 3 or 4 in the combined data.

Table 4.22: Firm-size classification raw and combined ANIS data sets, Chile

Raw code	Raw cut-offs	Combined code	Combined cut-offs
0	0 <= *emp.* >= 04	0	< 10 employed
1	05 <= *emp.* >= 09	0	< 10 employed
2	10 <= *emp.* >= 19	1	10-19 employed
3	20 <= *emp.* >= 49	2	20-49 employed
4	50 <= *emp.* >= 99	3	50-249 employed
5	100 <= *emp.* >= 199	3	.
6	200 <= *emp.* >= 499	3.5	.
7	500 <= *emp.* >= 999	4	250+ employed
8	emp. >=1000	4	250+ employed

- Furthermore, we enforce a fuzzy match for the value and amount of electricity consumed by firms across both data sets as follows:
  - Quantity electricity purchased (+/- 5 percent).
  - Value electricity purchased (+/- 10 percent)
- Because of the fuzzy nature of the match, no exclusive 1-to-1 match outcome is to be expected. Consequently, in case multiple IDs can be matched based on the above match criteria, we identify the best match by:
  - The longest number of matched years between two IDs;
  - The lowest average absolute difference between the reported quantity (value) of electricity purchased that falls within an acceptable range between two IDs;
  - In other words, we pick the matched firm that is closest to the reported values in the combined data set as long as the difference does not exceed 5 (10) per cent of the reported quantity (value) of electricity purchased, respectively.
- This way we can uniquely identify 90 per cent of all firms in the combined data set as described in Table 4.23.

Table 4.23: Firm matches across raw and combined ANIS data sets, Chile

Matches	Cases	Per cent
Unique ID match	10,538	90.3
2 IDs matched	1,038	8.9
3 IDs matched	84	0.7
Unique ID match after 2nd-round	11,156	95.6

- Finally, while the outlined algorithm matches around 90% of firm IDs uniquely, the nature of the algorithm is such that the same matched firm may satisfy the "closest proximity" requirement of the previous chat. This is the case for around 9.5% of all recorded cases. Given these, we re-iterate the previous step by evaluating the closest proximity *conditional* on the proximity of the other non-unique matches of the same firm IDs. Through this final step, the match rate between the two data sets is at 95.6%; see last line in Table 4.23.

4.E Energy unit-price adjustment algorithm

The energy and electricity cleaning algorithm described in this section is designed to identify and eliminate artificial jumps in the raw energy data that are the potential result of data imputation or reporting issues; Consider the example where quantities of an unspecified liquid were reported in tonnes in periods t_0 t_1 and t_2, respectively, but the unit of measurement in period t_1 would be wrongfully given in m^3. In such a case, the conversion of m^3 in t_1 to tonnes would lead to an inter-temporal change of reported quantities and unit prices (assuming a not notable change in purchased volumes over the three years) of a factor close to the conversion factor between tonnes and cubic meters. The algorithm we propose identifies jumps of these magnitudes and overwrite the (assumed to be wrongfully reported) quantity information if the corrected measures would not constitute a minimum/maximum in the series. In our example, this means that the quantity measure in t_1 will be adjusted, if the adjusted value would not be smaller (larger) than $min(t_0, t_2)$ ($max(t_0, t_2)$), respectively.

More specifically, the cleaning algorithm applied to the respective energy series takes the following steps:

1. For each firm, look at previous/next period and observe change of a price sequence up_t, $t = 1, ...T$, at an arbitrary point in time t_0, referred to as $\Delta up_{t_0 \pm 1} = up_{t0}/up_{t_0 \pm 1}$.

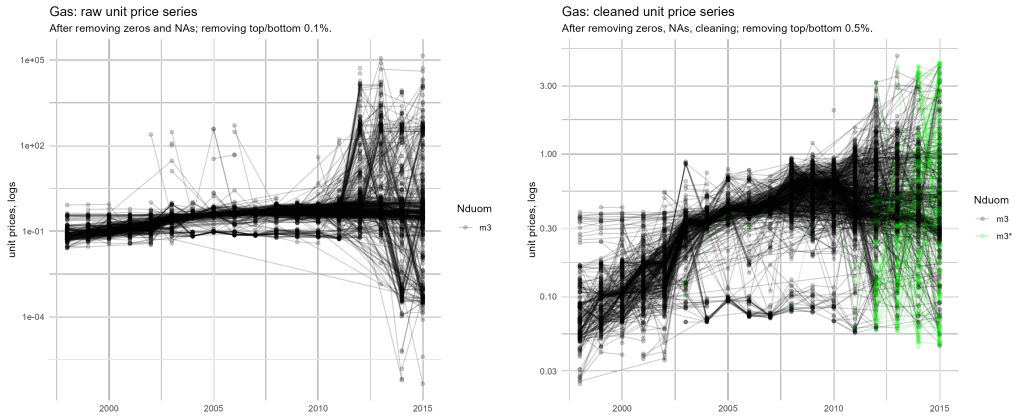
2. If $\Delta up_{t_0 \pm 1}$ is of order $100, 1000, \ldots$, correct up_t by factor of this magnitude, called $\widetilde{up}_{t0}$.

3. Check if $\widetilde{up}_{t0}$ is smaller (bigger) than min($up_{t \in T, t \neq t_0}$) (max($up_{t \in T, t \neq t_0}$):

   - If it is: replace $\widetilde{up}_{t0}$ with missing.
   - If not: Replace up_{t0} with $\widetilde{up}_{t0}$and highlight measurement unit with asterisk.

Figures 4.10, 4.11 and 4.12 compare the raw vs cleaned energy unit prices. Any adjusted series are labelled with an asterisk.

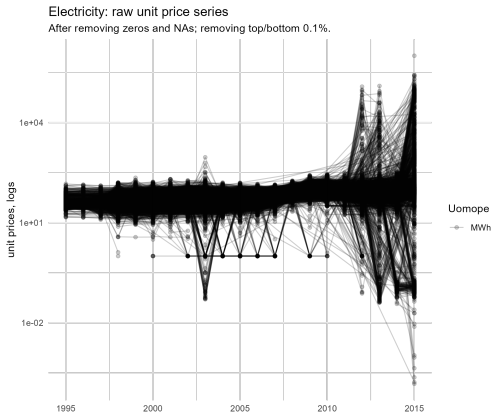
Figure 4.10: Raw vs revised energy unit prices



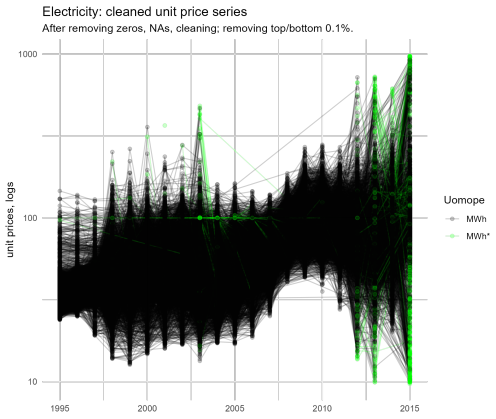
(a) Gas raw unit prices

(b) Gas cleaned unit prices

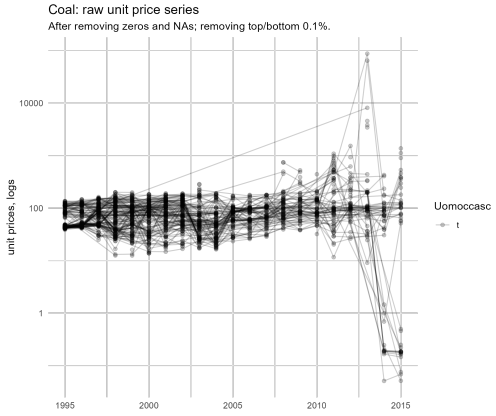
Figure 4.11: Raw vs revised energy unit prices (continued)



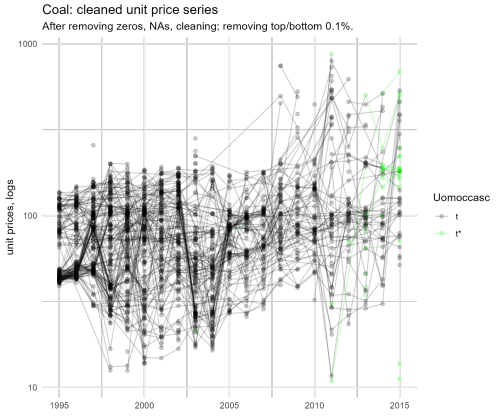
(a) Electricity raw unit prices



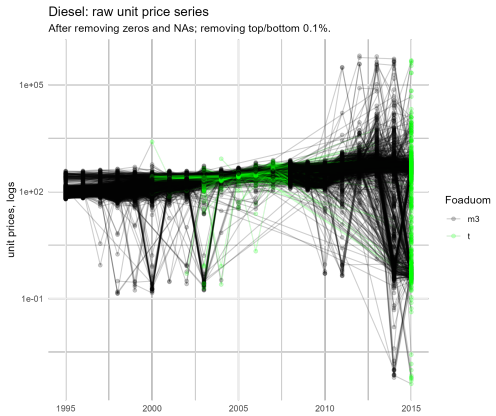
(b) Electricity cleaned unit prices



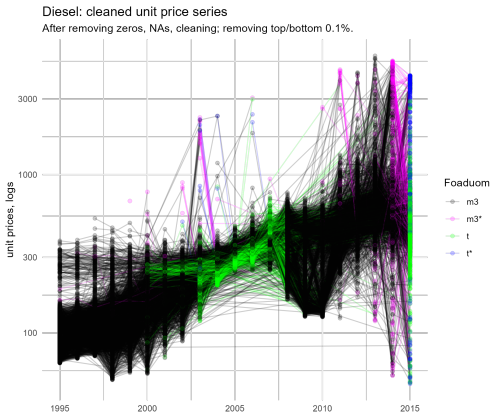
(c) Coal raw unit prices



(d) Coal cleaned unit prices

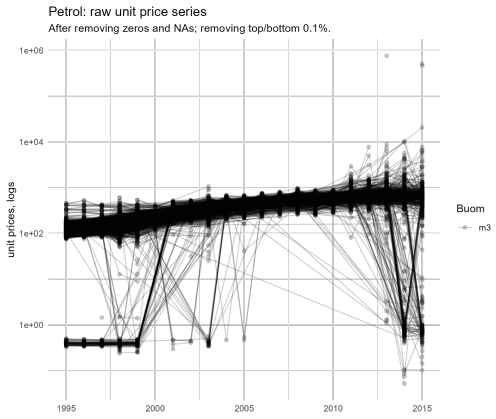


(e) Diesel raw unit prices

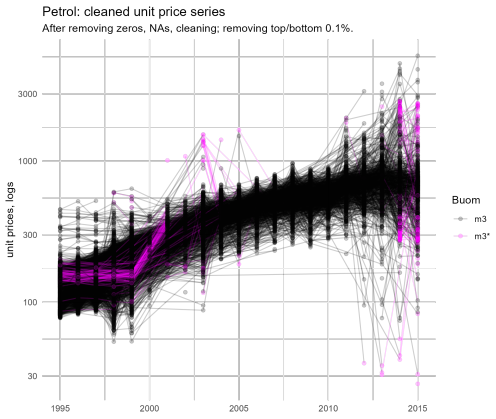


(f) Diesel cleaned unit prices

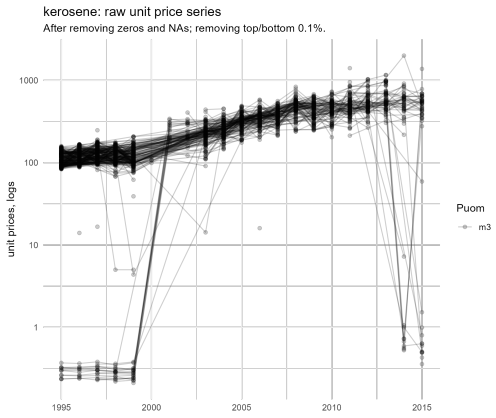
Figure 4.12: Raw vs revised energy unit prices (continued)



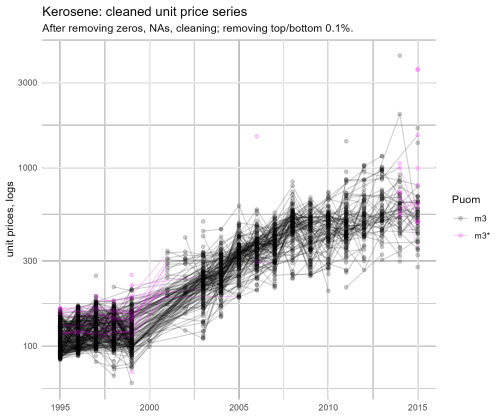
(a) Petrol raw unit prices



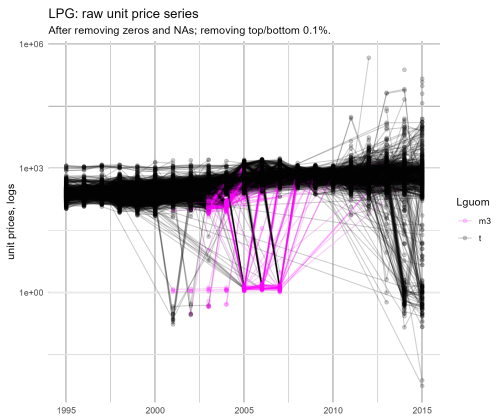
(b) Petrol cleaned unit prices



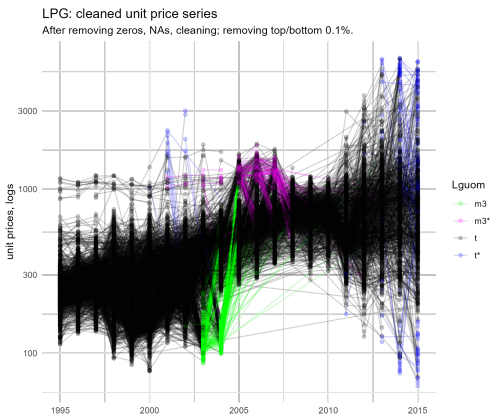
(c) Kerosene raw unit prices



(d) Kerosene cleaned unit prices



(e) LPG raw unit prices



(f) LPG cleaned unit prices

4.F Decomposition of energy price indices

The Melitz and Polanec (2015) decomposition. The Melitz and Polanec (2015) decomposition expresses the firm-level variable of interest ϕ for firm i, $i = 1, \ldots, N$, in period t such that the aggregate measure of interest Φ can be expressed as a weighted average of the form

$$\Phi_t = \sum_i^N s_{it}\phi_{it},$$

where $\sum_i^N s_{it} = 1$ and $s_{it} \geq 0$. To disaggregate the changes in aggregate outcomes between two periods, i.e. $t = 1$ and $t = 2$, we define:

$$\begin{aligned} \text{Surviving firms: } & s_{it} = 0 \text{ and } s_{it+1} > 0; \\ \text{Exiting firms: } & s_{it} > 0 \text{ and } s_{it+1} = 0. \end{aligned}$$

Furthermore, we denote the aggregate market share of all firm i in group G such that $i \in G \in N$ as:

$$s_{Gt} = \sum_{i \in G} s_{it},$$

and the aggregate firm-level outcome of that particular group as:

$$\Phi_{Gt} = \sum_{i \in G} \frac{s_{it}}{s_{Gt}}\phi_{it}.$$

We can then express the aggregate measure of interest Phi_t for two consecutive periods as the sum of surviving S, exiting X and entering E firms as:

$$\begin{aligned} \Phi_t &= s_{St}\Phi_{St} + s_{Xt}\Phi_{Xt} = \Phi_{St} + s_{Xt}\left(\Phi_{Xt} - \Phi_{St}\right) \\ \Phi_{t+1} &= s_{St+1}\Phi_{St+1} + s_{Et+1}\Phi_{Et+1} = \Phi_{Et+1} + s_{Et+1}\left(\Phi_{Et+1} - \Phi_{St+1}\right). \end{aligned}$$

The difference in the aggregate levels between both periods can then be calculated as stated below, where we decompose the changes in the aggregate between two consecutive periods in terms of the contributions of surviving, entering and exiting firms:

$$\Delta\Phi = \underbrace{\Phi_{St+1} - \Phi_{St}}_{\text{Surviving firms}} + \underbrace{s_{Et+1}\left(\Phi_{Et+1} - \Phi_{St+1}\right)}_{\text{Entering firms}} + \underbrace{s_{Xt}\left(\Phi_{S1} - \Phi_{X1}\right)}_{\text{Exiting firms}}.$$

Furthermore, using the Olley and Pakes (1996)[19] decomposition, we can further decompose the surviving firms' contributions into the unweighted mean changes of all surviving firms, $\Delta\bar{\phi}_S$ and the covariance of the market share and individual effect of the respective surviving firms, $\Delta cov_s(s_{it}, \phi_{it})$:

$$\Delta\Phi = \underbrace{\underbrace{\Delta\bar{\phi}_S}_{\text{(1) Within}} + \underbrace{\Delta cov_s(s_{it}, \phi_{it})}_{\text{(2) Between}}}_{\text{Surviving firms}} + \underbrace{s_{Et+1}\left(\Phi_{Et+1} - \Phi_{St+1}\right)}_{\text{(3) Entry}} + \underbrace{s_{Xt}\left(\Phi_{S1} - \Phi_{X1}\right)}_{\text{(4) Exit}}.$$

In words, we decompose the aggregate effect into (1) changes in the unweighted mean across all surviving firms; (2) changes in the covariance between market share and firm effect, and the effect experienced by (3) incumbent and (4) exiting firms. For the purpose of our analysis, we define $\phi_{it} = up_{it}^m$ as the energy unit-price of energy type $m = \{electricity, fossilfuels\}$ for firm i and year t as defined in Equation 3.4.1, respectively.

[19] Olley and Pakes (1996) decompose the aggregate measure Φ_t as the individual firm-level observations ϕ_{it} weighted by s_{it}:

$$\Phi_t = \sum_i^N s_{it}\phi_{it}$$

into the components:

$$\Phi_t = \frac{1}{n_t}\sum_{i=1}^{n_t}\phi_{it} + \sum_i\left(s_{it} - \frac{1}{n_t}\right) \times \left(\phi_{it} - \frac{1}{n_t}\sum_{i=1}^{n_t}\phi_{it}\right).$$

We can define $\bar{\phi}_t = \frac{1}{n_t}\sum_{i=1}^{n_t}\phi_{it}$ as the unweighted firm-level mean and $\bar{s}_t = \frac{1}{n_t}$ as the average market share to simply the above expression:

$$\Phi_t = \bar{\phi}_t + \underbrace{\sum_i\left(s_{it} - \bar{s}_t\right)\left(\phi_{it} - \bar{\phi}_t\right)}_{cov(s_{it},\phi_{it})}.$$

Consequently, the changes in the aggregate variable Φ_t between two consecutive periods are given by the difference in the unweighted mean $\Delta\bar{\phi}$ and changes in the covariance $\Delta cov(s_{it}, \phi_{it})$:

$$\Delta\Phi = \Delta\bar{\phi} + \Delta cov(s_{it}, \phi_{it}).$$

The Logarithmic Mean Divisa Index (LMDI) decomposition. Following Ang and Liu (2001), define the aggregate variable Z consisting of individual terms Z_i which, in turn, can further be decomposed into separate elements $z_{i1}z_{i2}\dots z_{in}$ such that:

$$Z = \sum_i Z_i = \sum_i z_{i1}z_{i2}\dots z_{in}.$$

The aggregate changes of Z between periods ℓ and t can be decomposed into n separate components, corresponding to each of the particular elements of $z_{i1}z_{i2}\dots z_{in}$:[20]

$$\mathcal{D} = \frac{Z_t}{Z_l} = \mathcal{D}_{z1}\mathcal{D}_{z2}\dots\mathcal{D}_{zn}.$$

For the energy price definition, we disaggregate the change in energy prizes E into four separate components

$$E = \sum_i E_i = \sum_i \underbrace{X}_{\text{Scale}} \underbrace{\frac{X_s}{X}}_{\text{Between}} \overbrace{\frac{X_i}{X_s}}^{\text{Within}} \underbrace{\frac{E_i}{X_i}}_{\text{Intensity}},$$

where subscripts i and s denote the firm and sector, respectively, and X corresponds to the value-added, which are deflated using manufacturing industry-level deflators following Haraguchi and Amann (2022).[21]

Applying Equation 4.F.1 in our context, the common weight by the decomposed components is a function of firm-level energy use and (aggregate) energy consumption:

$$w_{i|\ell\to t} = \frac{(E_{it} - E_{i\ell})/(lnE_{it} - lnE_{i\ell})}{(E_t - E_\ell)/(lnE_t - lnE_\ell)}.$$

[20]The decomposition can be expressed in either a multiplicative or additive form. We focus on the multiplicative form here as only this form is consistent in aggregation, i.e., one can consistently aggregate it to higher levels, which is relevant in the context of energy decomposition applications as these may correspond to separate energy types, sectors etc. The general formula for the contribution of the k^{th} factor z_n, where $k = 1,\dots,n$, is given by:

$$\mathcal{D}_{zk} = exp\left\{\sum_i \frac{(Z_{it} - Z_{i\ell})/(lnZ_{it} - lnZ_{i\ell})}{(Z_t - Z_\ell)/(lnZ_t - lnZ_\ell)} \times ln\left(\frac{z_{ikt}}{z_{ik\ell}}\right)\right\}. \tag{4.F.1}$$

[21]We resort to value-added instead of gross output to account for potential double-counting through the use of intermediates.

In the context of our decomposition, this corresponds to (i) scale effect, which measures the changes in the overall size of the economy; (ii) intensity effect, which measures the energy intensity of productions of a firm; (iii) between changes, which measure the reallocation between sectors; (iv) within changes, which measure the reallocation between firms within a given sector.