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Lisbon School of Economics and Management - ISEG

ESSAYS ON MARKET POWER, INEQUALITY AND ANTITRUST REGULATION

Pedro Cavalcanti Gonçalves Ferreira

Orientadora: Profa. Doutora Filomena Maria dos Santos Garcia

Tese especialmente elaborada para obtenção do grau de Doutor em Economia

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2023

Para Nayara e Leonardo

Resumo

Boas práticas de regulação antitruste e de concorrência exigem, como qualquer política econômica, bons modelos e evidências empíricas. Os modelos ajudam a colocar problemas em perspetiva, descobrir mecanismos e produzir previsões sobre a economia. Além de testar as previsões do modelo, a evidência empírica fornece elementos necessários para avaliar a política e superar distorções. Nesse sentido, esta tese é guiada pelo desejo de contribuir para a política de concorrência no Brasil em questões relacionadas às interações entre o poder de mercado das empresas, os mercados de trabalho, a desigualdade e o papel das autoridades antitruste.

Nosso trabalho adota inicialmente uma abordagem macroeconômica e teórica, revelando mecanismos pelos quais o poder de mercado afeta a desigualdade de renda em países em desenvolvimento como o Brasil. Em seguida, a tese se propõe a avaliar como o trabalho do CADE, a autoridade antitruste brasileira, tem sido capaz (ou incapaz) de lidar com as distorções concorrenciais legais (concentração) e ilegais (conluio), nos mercados de fatores (trabalho) e de produtos, e, por consequência, com suas consequências na distribuição de renda.

Nos últimos anos, o papel do antitruste na distribuição de renda tem se destacado. Desde a década de 1980, o markup preço/custo marginal aumentou significativamente em algumas economias desenvolvidas, em especial nos Estados Unidos, juntamente com a percentual do PIB incorporado aos lucros das empresas. Já a participação da renda do trabalho no PIB tem caído. Vários trabalhos empíricos e teóricos (De Loecker et al., 2020; Edmond et al., 2018; Colciago and Mechelli, 2020) sugerem que o agravamento da desigualdade de renda nos Estados Unidos é resultado da falta de concorrência nos mercados. O relativo enfraquecimento da regulação antitruste seria uma das causas desse processo. Baker and Salop (2015), por

exemplo, argumentam que a política antitruste dos EUA deveria ser reformulada para fazer mais, deixando de considerar como orientadores apenas a eficiência econômica ou os excedentes do consumidor. Para os autores o antitruste deveria também priorizar casos que beneficiem a classe média e os desfavorecidos, tomando decisões com critérios que foquem os consumidores mais pobres. No entanto, o poder de mercado e, em particular, a desigualdade e os baixos salários não se limitam aos Estados Unidos e ao mundo desenvolvido. De facto, as evidências sugerem que a falta de concorrência em mercados essenciais em países como o Brasil é um problema ainda mais premente.

Como afirma o Crane (2015), nos países em desenvolvimento, a riqueza e a renda estão mais concentradas no topo; os mercados de ações são pouco desenvolvidos; os mercados de trabalho têm baixos níveis de concorrência; e os sindicatos são, em geral, mais fracos. No Brasil, os 10% mais ricos recebem quase 60% da renda agregada anual do país, enquanto nos Estados Unidos essa proporção é de 45%, e na Suécia, 30% (World Inequality Database[1]). Além disso, o país tem adotado, ao longo dos anos, estratégias de crescimento baseadas em políticas de substituição de importações, com impacto negativo nos níveis de competição dos mercados. Essas políticas geraram certa industrialização e crescimento econômico, mas a ampla presença do governo, planejando ou atuando diretamente nos mercados, distorceu os incentivos, principalmente pelo controle de preços e fomento à formação de oligopólios em setores industriais estratégicos. De acordo com Fiuza (2001), esse conjunto de políticas governamentais teve o papel de estabelecer mercados incontestáveis e tornar as posições de mercado mais rígidas e estáveis.

Em uma lista de países em desenvolvimento, classificados por seus níveis de markups preço/custo médios, produzida por De Loecker and Eeckhout (2020), o Brasil está entre os três primeiros, junto com Peru e Colômbia, com um rácio preço-custo de 1,61. Além disso, esse índice variou muito pouco nas últimas décadas, período em que houve uma reestruturação e fortalecimento do sistema antitruste brasileiro. Da mesma forma, apesar de um declínio no índice de Gini calculado a partir de pesquisas domiciliares na primeira década dos anos 2000, estudos recentes mostram que, quando se dados de renda obtidos em outras fontes de informação (como o im-

[1] https:/ /wid.world/data/

posto de renda, que capta com mais eficiência rendas de capital), a desigualdade permanece estável (De Souza, 2018).

A relação causal entre desigualdade e poder de mercado não deve ser estabelecida de forma anedótica, apenas por meio de fatos estilizados. Portanto, nosso primeiro capítulo (**"Poder de mercado e desigualdade: um modelo para a economia brasileira"**) visa identificar teoricamente os mecanismos pelos quais o poder de mercado afeta a desigualdade em um país em desenvolvimento. Para tanto, primeiro motivamos nosso modelo empiricamente, estimando a associação entre desigualdade e markups (usando uma abordagem Panel VAR com dados dos estados brasileiros). Embora nossos resultados não possam ser considerados evidências causais em sentido estrito (não há estratégia de quase-experimento ou estrutura de variáveis instrumentais), a evidência estatisticamente robusta, relacionando positivamente um choque de markup à desigualdade, orientou o desenvolvimento do nosso modelo.

Em seguida, construímos um modelo dinâmico de equilíbrio geral e o calibramos para reproduzir a economia brasileira. Esse modelo é baseado na estrutura tradicional do Real Business Cycle, mas possui um conjunto de fricções úteis para mimetizar a distribuição da renda e da riqueza brasileira (entre três quantis de renda/população - 50% inferior, 49% médio e 1% superior). Nosso modelo é razoavelmente diferente de outros que tentam simular relações de poder de mercado e desigualdade para a economia dos EUA. Primeiro, é computacionalmente mais leve, pois não há uma estrutura completa de agentes heterogêneos, mas uma versão estendida do modelo tradicional de dois agentes.

Além disso, é mais adequado lidar com a economia brasileira, pois, além da heterogeneidade na participação no mercado de ativos, há também a heterogeneidade na qualificação da mão de obra e na elasticidade da oferta de trabalho. Outra característica essencial do modelo é o comportamento oligopolista (produto) e oligopsonístico (lado do fator) endógeno das empresas. Juntos, essas especificidades permitem reproduzir a resposta do Gini a um choque momentâneo no markup, identificada empiricamente. Além disso, o modelo contribui para política de concorrência ao descrever como o comportamento oligopsonístico afeta o funcionamento do mercado de trabalho e a dinâmica da desigualdade.

As repercussões do poder de mercado sobre a desigualdade de renda e salários, mecanismo destacado no modelo do primeiro capítulo da tese, é um aspeto ainda ausente na literatura e na prática antitruste em países em desenvolvimento como o Brasil. Nosso segundo capítulo (**"Poder de Mercado, Salários e Desigualdade: evidências do Brasil"**) buscou preencher parcialmente essa lacuna. A primeira parte do capítulo permanece em nível agregado/macro, analisando dados entre mercados/setores. Com acesso a uma base que combinada dados de empregador-empregado no Brasil, primeiro caracterizamos a evolução temporal da concentração do mercado de trabalho local (HHI municipal do emprego). Em seguida, construímos um modelo de efeito fixo com variáveis instrumentais para verificar a associação entre concentração do mercado de trabalho local, desigualdade de renda e salários.

Por fim, na última parte do segundo capítulo, estudamos um mercado específico para avaliar o efeito de processos de fusão e aquisição sobre mercado de trabalho (como nosso modelo revela, os salários são um mecanismo fundamental que conecta o poder de mercado à desigualdade). Uma abordagem de diferença-em-diferenças (DiD) foi implementada para verificar se uma transação de fusão e aquisição impactou os ganhos dos trabalhadores no setor bancário brasileiro. Durante décadas, prevaleceu entre a comunidade antitruste a visão de que o mercado de trabalho não era uma preocupação para a política de concorrência porque era inerentemente competitivo. As evidências neste trabalho vão contra essa noção. Os resultados do capítulo mostram que existe uma ligação potencialmente danosa entre poder de mercado e salários, gerando um efeito regressivo sobre a desigualdade de renda.

O último capítulo da tese (**"Comportamento pós-cartel: avaliando os efeitos da política antitruste no mercado brasileiro de combustíveis"**) tem um perfil puramente de avaliação política e foca na capacidade das autoridades antitruste brasileiras de reestabelecer um mercado competitivo no setor de combustíveis. Os gastos com transporte, nos quais os preços dos combustíveis são muito relevantes, representam uma parcela de 18% do orçamento das famílias brasileiras (superior aos gastos com alimentação), segundo a mais recente Pesquisa de

Orçamentos Familiares (POF-IBGE 2017-2018). Portanto, o impacto distributivo dos esquemas de fixação de preços é considerável. Um estudo do CADE (Conselho Administrativo de Defesa Econômica) estimou que um cartel de combustíveis de Brasília, esquema que conseguiu elevar os preços em pouco mais de 8%, causou, em um ano, cerca de US$ 75 milhões em prejuízos aos consumidores.

Os artigos que avaliam os efeitos da ação antitruste sobre esquemas de cartel geralmente buscam apenas quantificar o impacto sobre os preços, com métodos como regressões antes e depois, diferença-em-diferenças ou abordagens de controle sintético. No entanto, essas metodologias têm algumas desvantagens, principalmente, a exigência de estabelecer uma data exógena ou um evento inovador com base em suposições que podem não ser precisas. Além disso não capturam uma dinâmica pós-cartel que considere uma possível reemergência da conduta ilegal.

Para superar essas fragilidades observadas em outros estudos, aplicamos duas metodologias, Structural Break Analysis (Bai and Perron Test) e Markov Switching Regressions, a quatro casos de cartéis identificados no mercado brasileiro de combustíveis (Brasília, Belo Horizonte, São Luís e Londrina), buscando analisar a eficácia das políticas de concorrência. Como teste comparativo entre os procedimentos MSR e Bai Perron, nosso trabalho mostra que o primeiro foi mais sensível às transições entre regimes, sem perder quebras, e exibiu resultados mais precisos. Do ponto de vista da avaliação da política antitruste, nossos achados indicam uma baixa capacidade das autoridades antitruste para extinguir definitivamente práticas de fixação de preços nos mercados-alvo.

Palavras-chave: desigualdade; antitruste; poder do mercado; avaliação de políticas; Brasil.

Abstract

This thesis examines the relationships between firms' market power, labor markets, inequality, and competition regulation in order to inform Brazilian antitrust practice with knowledge and evidence. To do this, we utilized both theoretical and empirical approaches to identify the processes that potentially link the competition policy efficiency to inequality. Our research demonstrates that market power as a whole, and notably in labor markets, can alter not just the economic efficiency but also the distribution of income, which should be a matter of public policy concern (if government utility gives any weights to distribution).

The significance of antitrust in income distribution has been emphasized in recent years, particularly in papers focusing on the US economy and other developing countries. But market power, inequality and low wages seems to be an even bigger problem in countries like Brazil, where wealth and income are more concentrated at the top, where labor markets are less competitive (because of industrial policies that try to create national champion firms), and where unions are generally weaker.

Identifying some of the mechanisms by which market power affects inequality in a developing country is the objective of our first chapter (**"Market Power and Inequality: a model of the Brazilian economy"**). We constructed and calibrated a dynamic general equilibrium model to replicate the Brazilian economy. It was capable of mimicking Gini's response to a markup shock, reproducing the short-run dynamic identified empirically in a pVAR model (implemented with sub-national data from Brazil). The model also showed how oligopsonistic behavior affects the outcomes of the labor market and how that affects the level and dynamics of inequality.

The effects of market power on income inequality and wages were examined in our second chapter (**"Market Power, Wages, and Inequality: evidence from Brazil"**). Using a matched employer-employee database from Brazil, the first section of this chapter characterizes the temporal evolution of the local labor markets concentration (municipality employment HHI) and then, with a fixed-effect model with instrumental variables, estimates the relationship between the local labor market concentration, income inequality, and wages. In the second section, a Difference-in-Difference (DiD) setup was conducted to determine if a merger and acquisition transaction affected the earnings of banking industry employees in Brazil. The chapter's findings suggest that there may be a negative relationship between market power, wages, and inequality.

The final chapter (**"Post-Cartel Behavior: assessing the effects of antitrust policy on Brazilian fuel market"**) evaluates the ability of the Brazilian antitrust authority (CADE) to restore free competition to cartel-affected Brazilian fuel markets. To this end, we applied Structural Break Analysis (Bai and Perron Test) and Markov Switching Regressions to four cases in the Brazilian fuel market (Brasilia, Belo Horizonte, São Luís, and Londrina), identifying market dynamics before and after CADE's anti-collusion actions. Our findings indicate that antitrust authorities have a limited capacity to eradicate price-fixing practices in targeted markets. This has direct implications for CADE's anti-collusion strategy. It is possible that the fines and criminal penalties imposed on scheme participants are insufficient to deter illegal conduct (especially because they come after years of litigation in court). The case of Braslia, on the other hand, appears to be exemplary of how strong preventive measures coupled with structural ones, aimed at market reorganization, are expected to have lasting effects on price-fixing behavior.

Keywords: inequality; antitrust; market power; policy evaluation; Brazil.

Acknowledgements

I am grateful to my advisor, Professor Filomena Garcia, for her guidance and support throughout this journey.

I would like to express my appreciation to my colleague Eduardo de Sá Fortes Leitão Rodrigues for his contribution to the revision process and comments on the thesis chapters.

I am also grateful to the Instituto de Pesquisa Econômica Aplicada (IPEA) for granting me the opportunity to pursue my Ph.D.

Lastly, I want to express my heartfelt gratitude to my entire family, particularly my wife Nayara Rocha and my son Leonardo, for their patience, support, understanding, love, and encouragement during these demanding years.

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1

Introduction

Good antitrust and competition regulation practices require, like any economic policy, good models and empirical evidence. Models help put problems in perspective, uncover mechanisms, and produce predictions about the economy. In addition to testing the model's predictions, empirical evidence provides elements necessary to evaluate policy and overcome distortions. In this sense, this thesis is guided by the desire to contribute to the competition policy in Brazil in matters related to the interactions between firms' market power, labor markets, inequality, and the role of antitrust authorities.

Our work initially adopts a macroeconomic and theoretical approach, revealing mechanisms by which market power affects income inequality in developing countries like Brazil. Progressively, instead, the thesis ends up assuming an applied market-specific framework to assess how CADE's, the Brazilian antitrust authority, work has been able (or unable) to address legal (concentration) and illegal (collusion) market power distortions in factor (labor) and product markets, and their consequences on the income distribution.

In recent years, the role of antitrust in income distribution has been highlighted. Since the 1980s, the price/marginal cost markup in the United States economy has increased significantly, along with the firm profit share and labor share. Several empirical and theoretical papers (De Loecker et al., 2020; Edmond et al., 2018; Colciago and Mechelli, 2020) suggest that the worsening of income inequality in the United States is a result of the lack of competition in the economy and that weak antitrust regulation is one of the causes. Baker and Salop (2015), for instance, argue that US antitrust policy should be reformulated to do more than account for efficiencies or consumer surplus, prioritizing cases that benefit the middle class and the disadvantaged, and designing antitrust remedies with a focus on the poorest consumers. However, market power and, in particular, inequality and low wages are not limited to the United States and the developed world. In fact, the evidence suggests that the lack of competition in essential markets in countries such as Brazil is an even more pressing problem.

As Crane (2015) states, in developing countries, wealth and income are more concentrated on the top; equity markets are undeveloped; labor markets have low levels of competition; and unions are, in general, weaker. In Brazil, the wealthiest 10% receive almost 60% of the country's annual aggregate income, while in the United States, this proportion is 45%, and in Sweden, 30% (World Inequality Database[1]). In addition, the country has adopted growth strategies based on import substitution policies, with a negative impact on the markets' competition levels. This policy generated some industrialization and economic growth, but the broad government presence, planning or directly acting on markets, distorted incentives, mainly by controlling prices and fostering the establishment of oligopolies in strategic industrial sectors. According to Fiuza (2001), this set of governmental policies played a role in establishing uncontested markets and making market positions more rigid and stable.

In a list of developing countries, ranked by price/cost markups, produced by De Loecker and Eeckhout (2020), Brazil is in the top three, along with Peru and Colombia, with a ratio of 1.61. Moreover, this value had no variation in recent decades, a period in which there was a restructuring and strengthening of Brazil's antitrust system. Similarly, despite a decline in the Gini index computed from household surveys in the first decade of the 2000s, recent studies show that when income from other sources (such as income tax) is included, inequality remains stable (De Souza, 2018).

The causal relationship between inequality and market power should not be established anecdo-

[1] https://wid.world/data/

tally, only through stylized facts. Therefore, our first chapter (**"Market Power and Inequality: a model of the Brazilian economy"**) aims to theoretically identify the mechanisms by which market power affects inequality in a developing country. For this purpose, we first motivated our model empirically, estimating the association between inequality and markups (using a Panel VAR approach with data from Brazilian states). Although our results can not be considered causal evidence in the strict sense (there is no quasi-experiment strategy or instrumental variable framework), the statistically robust evidence, positively relating a markup shock to inequality, guided our model's development.

Next, we built a dynamic general equilibrium model and calibrated it to reproduce the Brazilian economy. This model is based on the traditional Real Business Cycle framework, but it has a set of frictions useful for matching Brazilian wealth and income distribution (between three income/population quantiles - bottom 50%, middle 49%, and top 1%). Our model is reasonably different from others trying to simulate market power and inequality relationships for the US economy. First, it is computationally lighter, as there is no complete heterogeneous agent framework but an extended version of the traditional Two-Agent Model.

Moreover, it is more appropriate to deal with the Brazilian economy as, beyond asset market participation, there is also labor skills and supply elasticity heterogeneity. Another essential model feature is the firm's endogenous oligopolistic (product) and oligopsonistic (factor-side) behavior. Together, these features can illustrate the markup shock response of Gini, identified empirically. Also, this model's most important contribution to competition policy may be that it shows how oligopolistic behavior affects the way the labor market works and the inequality dynamics.

The repercussions of market power on income inequality and wages, a mechanism highlighted in the model from the thesis's first chapter, is an aspect that is still absent in the literature and antitrust practice in developing countries like Brazil. Our second chapter (**"Market Power, Wages and Inequality: evidence from Brazil"**) aimed to fill this gap partially. The first part of the chapter remains at an aggregate/macro level, analyzing cross-markets/sector data. With access to a matched employer-employee base from Brazil, we first characterized the temporal evolution of the local labor markets concentration (Municipality HHI). Then, we build a fixed-effect model with instrumental variables to verify the association between the local labor market concentration, income inequality, and wages.

Finally, in the second chapter's last part, we went deeper into a specific market to evaluate Brazilian merger review processes' effect on the affected market mean wages (our model reveals that wages are one fundamental mechanism for market power/inequality relationship). A Difference-in-Difference (DiD) setup was implemented to verify whether a merger and acquisition transaction impacted the workers' earnings in the Brazilian banking sector. For decades, there was a prevailing view among the antitrust community that the labor market was not a concern for competition policy because it was inherently competitive. The evidence in this work goes against this notion. The results of the chapter show that there is a potentially harmful link between market power and wages, which has a regressive effect on income inequality.

The last thesis chapter (**"Post-Cartel Behavior: assessing the effects of antitrust policy on Brazilian fuel market"**) has a pure policy evaluation profile and focuses on the ability of the Brazilian antitrust authorities to establish a competitive market in the Brazilian fuel sector. Expenses on transportation, in which fuel prices are very relevant, represent a share of 18% of Brazilian households' budget (higher than spending on food), according to the most recent Family Budget Survey (POF-IBGE 2017-2018). Therefore, the distributive impact of price-fixing schemes is considerable. A study from CADE (Conselho Administrativo de Defesa Econômica, Brazilian antitrust authority) estimated that Brasília's fuel cartel, a scheme that managed to raise prices by just over 8%, caused, in one year, about US$75 million in losses to consumers.

Papers assessing the antitrust effects of cartel cases usually take the form of a quantifying approach, measuring the impact on prices with methods like before-and-after dummy regressions, difference-in-difference, or synthetic control designs. However, these approaches have some downsides (notably, the requirement of establishing an exogenous date or breakthrough event based on assumptions that may not be accurate).

To overcome this weakness, we applied Structural Break Analysis (Bai and Perron Test) and Markov Switching Regressions to four cases in the Brazilian fuel market (Brasilia, Belo Horizonte, São Luís, and Londrina) to analyze the effectiveness of competition policies. As a comparative test between MSR and Bai Perron procedures, our work shows that the former was more sensitive to transitions between regimes, without missing breaks, and exhibited precise results. From the point of view of antitrust policy evaluation, our findings indicate a low capacity of the antitrust authorities to extinguish price-fixing practices in targeted markets.

2

Market Power and Inequality: a model of the Brazilian economy

2.1 Introduction

This chapter is an initial attempt to draw lines regarding the interplay between market concentration and income inequality in the Brazilian economy. Our goal is to uncover some of the mechanisms by which markups and barriers to entry influence macroeconomic aggregates and, consequently, indicators such as the share of the income appropriated by the richest and the country's Gini index.

For this purpose, we first conduct an empirical estimation as a motivation procedure. We study the response of Gini and the top 1% share of income to a markup shock, using a PVAR approach estimated with data from Brazilian states. Consistent with the recent empirical literature, we find that the markup shock is positively related to inequality. Moreover, that result is robust to changes in the model specification or different Cholesky orderings.

Second, we build a dynamic general equilibrium model and calibrate it to reproduce the Brazilian economy. To keep it as simple as possible, revealing only the critical characteristics needed for our goal, this model is initially based on a traditional Real Business Cycle framework, first developed by Kydland and Prescott (1982). Nevertheless, it has a set of frictions useful for matching some Brazilian economy characteristics, especially the wealth and income distribution through three quantiles of the country's population (bottom 50%, middle 49%, and top 1%) and the firm's behavior.

These characteristics give our model reasonably different aspects from those we saw in previous papers, making it computationally lighter and more appropriate to deal with the Brazilian economy. As for the critical factors in our model, we can highlight the presence of three representative agents, heterogeneity in asset market participation and labor skills (a way to overcome the two agents models' difficulty in reproducing the income distribution), and a more complex supply side, featuring oligopolistic in the goods market and oligopsonistic behavior in the labor market. Together, these features can illustrate some of the possible effects of TFP and markup shocks through income distribution.

In accordance with our empirical findings, a temporary increase in the firm's markup is manifestly regressive, transferring income from the bottom to the top of the distribution. However, its effects on economic growth may be positive in the short term as a result of increased investment in the formation of new businesses. As a result of the countercyclical behavior of the markup, disturbances in the TFP also reduce impact inequality. Notable is the way in which labor supply elasticity influences the behavior of income distribution between poor and middle-class households. Households with a less elastic supply suffer more from oligopsonistic power, but they benefit more when increased TFP-driven economic growth tightens market competition.

The remainder of the chapter is organized as follows: Section 2 discusses the relationship between antitrust policy and inequality; Section 3 provides a more thorough revision of the literature relevant to our modeling proposal; Section 4 gives details about the PVAR estimation and results; In Sections 5 and 6, the model and equilibrium conditions, and steady-state are explained. In Section 7, some of the model's static properties are looked at. In Section 8, the effects of TFP and markup shocks on the economy as a whole and on distribution are discussed. In Section 8, some policy discussions and the conclusion are given.

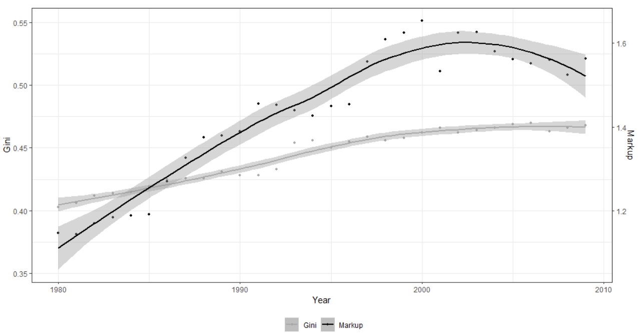
2.2 Antitrust and Inequality

Should antitrust policy consider the implications of competition for income and wealth distribution? Traditionally, antitrust analysis takes the efficiency principle as its standard, which assumes, to a large extent, that distribution is not an issue — allowing, for example, the use of total welfare as the reference index (the sum of consumers' and producers' surpluses). Under this view, agencies should clear mergers that increase market power if there is evidence of efficiency gains capable of counteracting price increases. When considering distributive effects, more complex and generalized analysis is required. Even the policy of prioritizing consumer surplus in antitrust assessments (a practice adopted by a significant number of authorities regarding distribution concerns) is insufficient.

In principle, it is necessary to recognize that consumers and producers are heterogeneous between markets and intra-markets. The effects of increased market power are asymmetric between different types of agents. For instance, Baker and Salop (2015) recognize that the "continued application of consumer welfare standards likely leads to less inequality than a reliance on aggregate welfare considerations". However, they argue that antitrust policy should go further and prioritize cases that benefit the middle class and the less fortunate, as well as design antitrust remedies that target poorer consumers.

Therefore, pursuing the income and wealth inequality agenda can result in a substantial deviation from the antitrust authorities' typical procedures and practices. Nonetheless, practitioners and academics have repeatedly advocated for the incorporation of inequality into competition policy. Recent macroeconomic trends, particularly in the United States, have encouraged the filing of such claims. Estimates indicate an increase in the price/marginal cost markup since the 1980s, with a slight deepening after the Great Recession, along with an upward trend in profit share, a decrease in labor share, and a subsequent rise in income inequality indices (De Loecker et al., 2020; Edmond et al., 2018; Colciago and Mechelli, 2020). Figure 2.1 reports trends in markups and the Gini index from 1980 to 2010. These trends are correlated with the changes observed in American antitrust policy during the 1980s, a period characterized by a substantial deregulation process, indicating a non-negligible effect of competition policy on inequality.

Figure 2.1: US - Estimated Markup and Gini index trends (1980-2009)



Source: FRED/U.S.; US Census Bureau; De Loecker et al. (2020)

Stiglitz (2017) gives a clear and simple mechanism to explain the link between inequality and competition:

> *Market power has, of course, distributive effects as well. The monopolist's monopoly rents come at the expense of consumers': as monopolies raise their prices, their profits increase while the well-being of consumers and workers decreases. An increase in market power is associated with an increase in inequality (p. 4).*

Despite its reasonable intuition, there is no broad consensus about the connection between competition, antitrust policy, and inequality. Furthermore, the argument against it is also very straightforward. In countries with developed financial systems, the shareholding is distributed through the income classes. Furthermore, labor market protection, like labor unions, can generate a rent-sharing process between workers and owners. Therefore, the gains from monopoly rents are not necessarily going to those at the top, minimizing market power's effect on inequality. This kind of reasoning is followed by Crane (2015), for example.

The impact of market power and the lack of competition on income and wealth inequality is an ongoing debate. However, a growing body of work in the economic literature highlights that there may exist sufficient transmission mechanisms to justify the concerns about the relationship between market regulation, antitrust interventions (or lack thereof), and inequality. One of the inaugural papers was Comanor and Smiley (1975), recently extended by Ennis and Kim (2017). The latter calibrates the seminal model for several OECD countries and shows that the wealth of the top 10 percent of households rises by 10 to 24 percent in the presence of market power. So a fiercer competition policy could reduce inequality.

Dierx et al. (2017) opened a field of work with dynamic stochastic equilibrium models, capable of connecting, with some coherence, the micro and macroeconomic impacts of antitrust interventions. In their study, the authors explored a DSGE model for Europe (QUEST model) to access the distributional response to shocks on firms' markups (across different households, savers/non-liquidity-constrained, and hand-to-mouth/liquidity-constrained). The micro-side approach, connected with antitrust agencies' performance, was possible by defining the shock level with estimates for direct and indirect (deterrent) effects of competition policy interventions. The model simulations show some significant redistributive results, with liquidity-constrained households increasing their consumption more. "This supports the view that competition policy interventions, by lowering prices and — as studied in other work, also by increasing the quality and variety of products — are particularly beneficial for the poorest in the society" (p.182).

Finally, still in a general equilibrium framework but enriched with an oligopolistic and endogenous market structure and heterogeneous agents, Colciago and Mechelli (2020) could simulate markups and inequality trends similar to those found in the US post-1980 data. To build this scenario, the authors imposed a transition between models' steady states, increasing entry barriers for firms and resulting in incumbents' market power gains.

Understanding the relationship between competition and inequality also benefits from empirical papers, although this type of application is not yet widespread due to the difficulties in estimating market power and dealing with endogeneity. Yet, in this literature, it is worth highlighting, among others, the papers of Gans et al. (2019); De Loecker et al. (2020) and Han and Pyun (2021).

To check the "distributed shareholding" argument, Gans et al. (2019) calculated the relative distribution of consumption and corporate equity ownership for the United States and found that ownership is more skewed than consumption. Consequently, increased markups should increase inequality. Moreover, they were able to show that corporate equity has become more skewed during the past decades. De Loecker et al. (2020) relies on firms' cost minimization assumptions to construct estimates for markups in the US, from 1950 to 2016, and shows that the upward trend started in 1980 is significantly related to the fall of the labor share and the rise of profit participation in the national income. Based on these markup insights, Han and Pyun (2021) discovered that an increase in the wedge between marginal cost and prices was positively associated with rising income inequality for 20 countries between 1975 and 2011, and that higher markups benefit the top 1% of income earners the most.Moreover, they show that labor market protection mitigates this positive relationship between market power and rising inequality.

So far, we have presented theoretical and empirical reasons for discussing inequality issues in antitrust policies and routines. However, we must recognize that the actual effect of greater market power on the levels of inequality in developed countries is in dispute. Especially if one considers that equity ownership is more or less distributed among the middle classes, the same could occur with abnormal profits from companies' margins. Instead, the developing world is a significantly different environment. In countries like Brazil, wealth is more concentrated on the top, equity markets are likely undeveloped, the labor market is not very fluid, and there is evidence of oligopolistic behavior — with conglomerate and vertically integrated enterprises, exclusive legal privileges for incumbents, and a collection of formerly or still state-owned enterprises (and their respective exclusive markets). Crane (2015) recognizes this specificity:

> *The answer might very well be different for developing countries than for more developed ones. There is a strong a priori argument that the introduction of competition laws—prohibitions on monopolistic conduct and agreements—in developing countries can have progressive wealth redistribution effects (p. 1178).*

In the case of Brazil, during the second half of the 20th century, the strong presence of the

Federal Government as a leader in a development policy, planning or directly acting on markets, distorted incentives, mostly by controlling prices and encouraging the formation of oligopolies in strategic industrial sectors:

> *By reinforcing entry and exit barriers, this set of governmental policies played a role in establishing uncontested markets; weakened entrepreneurship; and made market positions more rigid and stable. Therefore, the governmental policy was not only reckless but opposite to competition(Fiuza, 2001, p. 04, free translation).*

Price control was a widespread reality during the 1970s (mainly to cope with high inflation) and, according to Frischtak (1980), served as an official business cartel coordinated by the Federal Government, based on sector-specific agreements among the most representative firms in the market. Government activity was extremely harmful because it promoted a process of concentration in the leading firms, discouraging small firms; defined the market leader by signaling the establishment of tacit agreements; and undermined the work of antitrust authorities, making any prosecution of cartels unnecessary and unfeasible because they were primarily organized by the government itself.

Of course, it is impossible to make any causal statement without an empirical strategy for this purpose, which is greatly hampered by the absence of frequent and reliable data series. Nonetheless, while European countries and the United States experienced a great leveling of income distribution in the second half of the twentieth century, Brazil has seen, at best, stability at high levels of inequality, despite the existence of periodic falls or sharp rises. As De Souza (2018) points out, the military dictatorship, a period of great state intervention in the economy, coincides with an increase in income concentration at the top. The top 1% share of income, which had reached 17–19% on the eve of the military coup, increased steadily until 1971 – when it marked 26%, the highest share since the 1940s. Even with the democratization, in the second half of the 1980s, Brazil started the new century as one of the world's most unequal countries and got a famous nickname in the 1990s: Belíndia[1].

[1] This expression reflected that the most inferior part of the population's income levels was very close to the misery experienced by the poorest in India. However, on the other hand, the top of the Brazilian income

This reality has not changed much since then, as shown in table 2.1. Estimates based on tax data and national income tables, produced for 2015 by the World Inequality Database (WID), indicate that, in purchasing power parity US$, the bottom 50% in Brazil has an income 38% higher than the poorest in India. Nevertheless, Brazil's poor earn only 27% of the income registered at the lowest tail of the Portuguese distribution. On the other hand, the richest Brazilians make higher income than the richest Belgians. This shows how far apart the top and bottom of Brazil's income distribution is.

Table 2.1: Average incomes, Brazil and selected countries (2015, US$ PPP)

Income groups	**Brazil**	**Portugal**	**Belgium**	**US**	**India**
Bottom 50%	$4,059.62	$14,671.21	$25,304.13	$20,301.10	$2,927.00
Middle 50%	$25,329.93	$53,954.85	$85,073.95	$105,074.87	$13,197.00
Top 1%	$544,456.49	$439,269.36	$500,753.94	$1,405,440.8	$212,976.00

Source: World Inequality Database (based on Brazilian national income accounts and tax data).

Any aggregate indicator that compares countries, with specificities beyond the observed data, should be viewed with a critical eye. However, the global markup estimates produced by De Loecker and Eeckhout (2020), based on a similar framework used for the US, give evidence that, among developing countries, Brazilian firms are listed as the ones with the highest market power (table 2.2). They are the second, behind only Peru, with a margin of 60% between price and marginal cost. If we analyze this indicator in detail, we will see that there are ups and downs within the covered period (1980-2016). Still, the final result is marked by the stability of high values, with a negligible variation of -0.01.

distribution had an income similar to that observed in developed European countries, like Belgium.

Table 2.2: Countries ranked by estimated Markup

Rank	Country	Markup (2016)	Change (1980-2016)
1	Peru	1.64	-0.04
2	Brazil	1.61	-0.01
3	Colombia	1.56	+0.41
4	Mexico	1.55	+0.21
5	Indonesia	1.53	+0.26
6	Philippines	1.50	-0.77
7	Venezuela	1.47	-0.46
8	Argentina	1.45	+0.64
9	Thailand	1.44	+0.21
10	China	1.40	-0.49
11	Chile	1.37	-2.24
12	South Africa	1.34	+0.14
13	Malaysia	1.33	+0.03
14	India	1.32	+0.34
15	Taiwan	1.23	-0.15
16	Pakistan	1.17	-0.01
17	Turkey	1.16	-0.32

Source: De Loecker and Eeckhout (2020).

Since democratization, Brazil has seen an attempt to build policies focused on redistribution. As an example, the country created universal health and social security systems. However, as often, antitrust authorities have neglected wealth and income issues. These two stylized facts, Brazil's presence among the world's unequal countries and those with the highest market concentration, suggest that the antitrust policy could be reformulated in order to account for the interaction between income distribution and competition policy should, either by a normative view based on social justice principles or because the intensification of income inequalities has repercussions on the country's institutional quality and, consequently, in the conditions available for economic development.

2.3 Related literature

This work is related to the broad economic literature on the determinants of income inequality. The inequality phenomenon is multifactorial and dependent on each country's specific institutional context, as shown by Nolan et al. (2019). The authors highlight, for example, changes in globalization, technological progress, the role of financial markets, and the composition of the population (in terms of age and family structure, and changing patterns of household labor force participation). They list distribution policies as well. Other authors point out the lack of good institutions (Acemoglu and Robinson, 2015), lobbying and government corruption (Gupta et al., 2015; Gilens and Page, 2014), and intergenerational persistence (De Nardi, 2004). In the Brazilian context, since the 1970s, there has been a polarized debate about the role of education versus structural issues like segregation/discrimination/limited access to production factors and the need for redistributive policies (Gandra, 2005; Barros and Mendonça, 1995).

Instead, as far as we know, our work is the first to look closely at market power as a source of inequality in an underdeveloped country through a general equilibrium model that, as we shall see, combines labor skills heterogeneity (what may be considered an education-related process), structural features (like limited access to production factors) and oligopolistic behavior, both in goods and labor markets.

For our purposes, we first rely on general equilibrium models that incorporate several levels of heterogeneity between agents. The DSGE models were initially marked by a representative agent's presence, which obviously denied the possibility of any assessment regarding income inequality. However, this framework has recently been revised to make it possible to incorporate some heterogeneity, either by liquidity-constrained or credit-constrained households (as in Galí et al., 2007 and Iacoviello, 2005, respectively). This trend was further deepened by authors who added to the basic models a complete distribution of heterogeneous agents, much in line with Aiyagari (1994) and Krusell and Smith (1998). These models are commonly known as Heterogeneous Agents New Keynesian (HANK) models (e.g., Kaplan et al., 2018).

However, all these works aimed to study income inequality as a mechanism affecting macroeconomic policies and aggregates. Oppositely, a normative concern about inequality, regarding how these policies impact the functional distribution of income, has gained relevance only after Piketty's work about the concentration at the top. In this last line of studies, we can highlight papers from Hohberger et al. (2020), which examined optimal conventional and unconventional monetary policies in the presence of agents with no access to the financial system; Gornemann et al. (2016), who analyzed the same type of policies but in a complete heterogeneous framework, and Bayer et al. (2020), who estimated a HANK model and showed that a set of macroeconomic shocks, including markup shock, have significantly contributed to the evolution of US wealth and income inequality.

Our model's supply side is inspired as well by a series of papers that incorporate oligopolistic market structures with endogenous entry, as in Bilbiie et al. (2012), Jaimovich and Floetotto (2008), and Etro and Colciago (2010). In these papers, the degree of market power, synthesized by markups between price and marginal cost, depends not only on the usual parameter of substitutability between goods (as in traditional monopoly competition) but also on the number of firms present in the economy. The number of firms, in turn, is defined by a dynamic rule of zero profit, in which the time equilibrium condition determines that the companies' sunk cost, or entry cost, must be equal to the expected profit, endogenizing the process of entry and exit of these firms. Thus, the markup level is determined endogenously as its possible impact on the economy's income distribution.

Still, on the supply side, we incorporate insights from two studies that modeled the Brazilian economy's income inequality. Areosa and Areosa (2016) introduced different skilled agents in the production function, with one of them having limited access to the financial system, and examined the optimal monetary policy in the presence of inequality. Ferreira and Guimarães (2018) explored the same type of heterogeneity in the production function in a model of educational and savings choice with heterogeneous agents. Their model appeared to fit the data on income and wealth inequalities well, explaining existing inequality patterns in Brazil.

Finally, several studies, mentioned earlier in this chapter, modeled the mechanisms by which market power affects individual income distribution. Two of them, closely related to ours, deserve a detailed analysis of their specificities. The first, Dierx et al. (2017), did something relatively simple but quite ingenious. As we explained earlier, the authors used a complete model for the European Union (Quest model) and simulated the possible impacts of the European competition agency's work on the distribution of consumption among the agents/ individuals.

Quest is a New Keynesian model with two regions and an open economy. Its supply side has a production function with different labor types (skilled and non-skilled), similar to what we propose in our model, but the market structure differs since firms face monopolistic competition. Therefore, the level of competition is exogenously determined by the inverse of the elasticity of substitution between the goods varieties, limiting the model possibilities in two ways. First, there is a muting effect on the mechanisms that transfer productivity shocks to income distribution since changes in firms' profits are not reflected in firms' entry and exit decisions and, consequently, in markups. Second, as we will see later, this mechanism can amplify or reduce the effects of economic growth on inequality, depending on the markup's cyclicality. Besides, the markup's level shocks also lose a propagation mechanism when they do not not affect investment decisions in new firms.

On the consumption side, the Quest model features two representative agents: households that are liquidity constrained and consume their disposable income and households that are non-liquidity constrained (so-called Ricardian) and have full access to financial markets. There is, therefore, a new limitation when studying the distribution of income. Models with two agents tend to overestimate inequality when there is a high proportion of hand-to-mouth agents. At the same time, the opposite happens when there are too many Ricardian agents in the economy. In the appendix, a comparison between different models makes this point clearer.

The modeling strategy in Colciago and Mechelli (2020) is also quite clever in bringing together the households' heterogeneity observed in Aiyagari (1994) with the oligopolistic market structures presented in Jaimovich and Floetotto (2008), and Etro and Colciago (2010), achieving a representation of the economy with the complete distribution of income and endogenous structure of competition between firms. Thus, overcoming two of the main limitations previously pointed out in the Quest model. There is, however, an evident trade-off between the heterogeneity supported by the model and its solution's complexity and computational burden. This computational burden leads the authors to simplify supply-side and asset markets. Therefore, their production function uses only labor, without capital, since the agents had no capital choice, investing only in firms' shareholding. Besides, the Aiyagari type of model rules out aggregate uncertainty, limiting the analysis possibilities. Therefore, the authors only studied transitions between steady-states, excluding, for example, the analysis of short-term economic cycles.

That said, the novelty in our model is the choice of an intermediate path, with an economy populated by three representative agents. This option keeps the model's computational requirements at a low level, opening doors to a broader asset market (which includes capital and government bonds, in addition to the firms' shares), while at the same time achieving an income distribution that is reasonably adherent to the reality of a developing economy, such as Brazil (the features of each of the agents aim to reproduce patterns observed in three ranges of the Brazilian distribution: Bottom 50%, middle 49%, and top 1%).

As a final note, we must cite the work by Alpanda and Zubairy (2021). This author built a New Keynesian model with strategic interactions between firms in both labor and goods markets. Although the paper worked only with a representative agent and did not explore the income distribution repercussions, it brings light to market power's role in labor share. Based on the paper, another important part of our chapter is the endogenous response of labor market power to changes in the number of firms and what that means for how resources are shared in the economy.

2.4 Market Power and Inequality: empirical evidence

2.4.1 Markup

To obtain some empirical motivation for our modeling purposes, we estimate, through a GMM PVAR approach, the response of two inequality indices (Gini and the income share of the top 1%) to a markup shock. Nevertheless, before reporting our econometric specification and results, we must clarify what should be understood by markup.

Through this chapter, the markup will always be referenced as the ratio between price and marginal cost:

$$u = \frac{P}{MC} \tag{2.1}$$

For estimation, we will rely upon the insights from De Loecker et al. (2020), who focused on the firms' cost-minimization problem for variable inputs to derive markup estimates. Following Nekarda and Ramey (2020), consider the process of choosing variable input V_i, $i = 1, \ldots, N$ to minimize

$$\mathrm{C} = \sum_{i=1,\ldots,N} (W_i \cdot V_i) \tag{2.2}$$

subject to

$$\bar{Y} = F\left(V_1, V_2, \ldots, V_N\right) \tag{2.3}$$

W_i is the factor price, V_i is the variable input, Y is the output, and $F(...)$ is the production function. λ is the Lagrange multiplier, and so the first-order condition for V_i can be written as:

$$W_i = \lambda \cdot \frac{\partial Y}{\partial V_i} \tag{2.4}$$

Given the Envelope Theorem, the Lagrange Multiplier λ could be seen as the marginal cost. Substituting equation 2.4 into equation 2.1, we get the markup:

$$u = \frac{\alpha}{S_{V_i}} \tag{2.5}$$

where

$$S_{V_i} = \frac{W_i \cdot V_i}{P \cdot Y} \tag{2.6}$$

and

$$\alpha = \frac{\partial Y}{\partial V_i} \cdot \frac{V_i}{Y} \tag{2.7}$$

S_{V_i} is V_i's factor share of revenue. While α is the output elasticity with respect to V_i. The markup can be estimated as the ratio between the output elasticity with respect to a variable factor and the input's revenue share. If we assume that the production function is of Cobb-Douglas type, the elasticity can be considered constant and we can recover a proxy of the markup using only the inverse of the factor's share:

$$\hat{u} = \frac{1}{S_{V_i}} \tag{2.8}$$

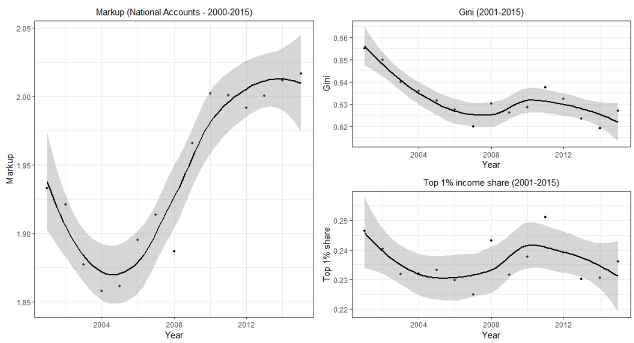
Given all the data limitations, we chose to estimate the Brazilian state's markup using regional accounts tables. Therefore, our approach's variable factors are intermediate goods and service consumption (IC), and the gross output (GO) in a year is the revenue. The inverse of factor share is given by:

$$S_{V_i}^{-1} = \frac{GO}{IC} \tag{2.9}$$

For an initial visual analysis, we estimated Brazil's aggregate markup using the national accounts data from 2000 to 2015 (provided by Brazil's statistical office, IBGE). Then, as a measure of inequality, we took two indexes from the World Inequality Database for the same period. The first one is the income Gini index calculated by combining household survey data with income tax information. The second uses the same database to calculate the share of income received by the wealthiest 1% of the population.

The trends of both the estimated markup and the inequality indicators can be seen in the figure 2.2. As in the previous chart from the United States, there is some comovement between the estimated market power and the inequality, although it is not so straightforward because the trends change over the period. This evidence, however, encourages us to assess the direction of this relationship with a more robust econometric procedure.

Figure 2.2: Brazil - Estimated Markup (national accounts) and Inequality trends (2000-2015)



Source: IBGE - National Accounts; World Inequality Database

2.4.2 Data and Estimation

Similar to the approach in Da Silva (2020), we estimate a Panel VAR model with data at the Brazilian state level. The choice for this level of evaluation is justified by the lack of a long time series for the Country, which would make the hypothesis tests unfeasible. Therefore, we opted to use a panel data with 27 cross-sectional units and 22 points in time, summing 594 observations. Figure A.1 (Appendix A) gives some taste about the Markup and Gini trends in Brazilian states. It is necessary to say that we should not take the absolute values of markup in its face value, but only its tendency, as we did not discount the output elasticity with respect to the variable factor (which is not observable since we neither estimate a production function).

Our estimation is based on annual data, with observations from 1993 to 2014 (the Brazilian states' panel is relatively balanced with few missing values). As previously stated, we retrieve a markup proxy in each Brazilian state through the regional accounts, released by the Brazilian statistical office (IBGE). Therefore, the inverse of the variable input's share is formed by the ratio between the total income (gross output) and the economy's intermediate consumption in a given state. The variables regarding income distribution are two, Gini and Income Share of Top 1%, both calculated by the Institute for Applied Economic Research (IPEA), a governmental think tank. It is worth emphasizing that, unlike the WID's indexes, IPEA only considered data from the IBGE's household surveys and, therefore, bringing together all the questions related to the underestimation of income at the top of the distribution, as discussed in Medeiros et al. (2015). Besides, we adopted as a third variable, the unemployment series for each of the Brazilian subnational units, made available also by IPEA based on IBGE's household survey data (the PVAR specification with three variables will be a test of robustness and stability for the bivariate estimation results).

All variables were tested for the existence of a unit root (in level and first differences, with and without trend), with the tests obeying specifications focused on panel data (Levin-Lin-Chu, Harris- Tzavalis, Im-Pesaran-Shin, and Fisher (ADF) methods). The proxy for the markup, the Top 1% Income Share, and unemployment did not have a unit root in any of the cases, being, therefore, stationary in the period under analysis. However, the unit root was found in level without trend for the Gini index, restoring stationarity when we took the first difference. For this reason, we chose to incorporate Gini in the first difference in our estimation.

Finally, we employed a GMM approach of Abrigo and Love (2016) to our data, which is consistent in cases with small T. A Forward Orthogonal Deviation (FOD) transformation eliminated the individual fixed effects. In our bivariate specification, to achieve identification, we assume that markups do not respond contemporaneously to an inequality shock within the year. This assumption is justified because markup should affect real income, and this income is the component of the Gini index and the Top 1% Share, so it is expected that this change in real income will affect inequality on impact, but not otherwise. Formally, after a PVAR estimation, we use a lower triangular matrix to recover orthogonalized disturbances (Cholesky decomposition), which will give our orthogonal impulse-response function.

Before proceeding to the results, two final notes are required. First, the optimal lag-order selection in the base specification of the PVAR and the other robustness tests (first order in all cases) was based on minimizing multiple criteria (Bayesian information criterion, MBIC, Akaike information criterion, MAIC, Quinn information criterion, MQIC). Also, the choice of lag length and the instruments' validation was made employing the Hansen's J overidentification test, in which the null hypothesis of validity of the instruments was not rejected.

2.4.3 Results

The base model results for the impulse-response function of the inequality with respect to a one standard deviation shock in the markup can be inspected in figure 2.3. They are in line with what is assumed from the trend analysis in the United States and Brazil cases and with what we recovered from the empirical evidence in the literature (Han and Pyun, 2021). Overall, the increase in the economy's markup (which can occur both due to changes in consumer demand functions and reduced competition in markets) has significantly regressive effects, increasing the concentration of income measured by the Gini index and the Top 1% Share. However, these effects occur after the second period because, on impact, the result appears to be zero or slightly progressive (the bands of the 95% confidence interval do not allow us to state with certainty the proper signal). Finally, it appears that the effect is short-lived, not exceeding the seventh year after the shock.

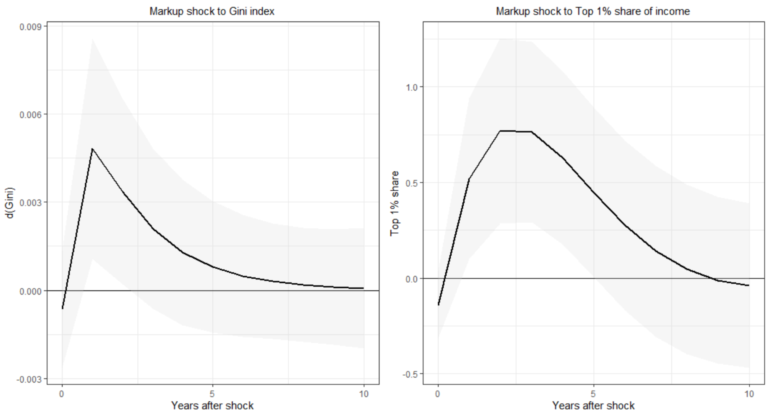


Figure 2.3: Orthogonal impulse-response functions from two variate specification (Cholesky ordering: Markup - Inequality). GMM Panel VAR (N=27, T=22) with standard errors from simulation (1000 repetitions) - 90% confidence interval.

Using another Cholesky ordering (Inequality – Markup) as a robustness check does not alter the results, despite the zero effect on impact being more evident. In response to a markup shock, the Gini index and Top 1% share still increase, revealing market power's regressiveness. Figure A.2 (Appendix A) presents these results.

To end the empirical motivation, a second model was estimated, this time with a third variable, unemployment, capable of capturing possible repercussions of the macroeconomic environment both in the markup and inequality indicators. The new results, available in the figure 2.4 are again in line with the basic model and reaffirm the initial findings on the negative effect of markup on Brazilian states' income distribution. This evidence gives us confidence regarding the interconnection between market concentration and inequality in developing countries like Brazil and leads to the second part of this work. Next, we will model some of the possible transmission channels in this relationship.

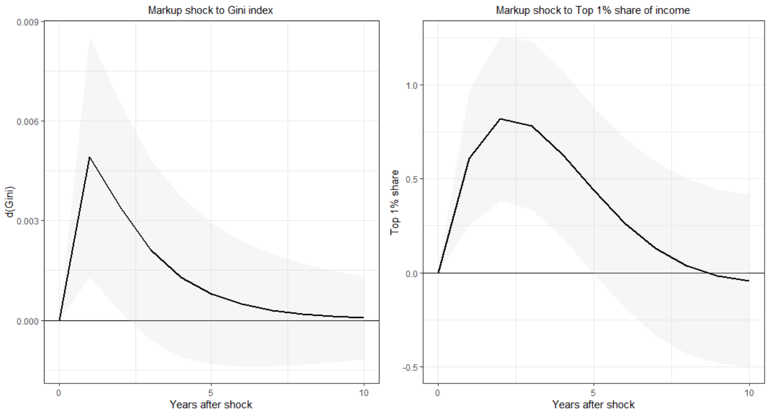


Figure 2.4: Orthogonal impulse-response functions from three variate specification (Cholesky ordering: Markup - Unemployment - Inequality). GMM Panel VAR (N=27, T=22) with standard errors from simulation (1000 repetitions) - 90% confidence interval.

2.5 General Equilibrium Model

In order to study the impact of market power on economic aggregates and, more importantly, on inequality, we propose a framework considerably simpler than heterogeneous agent models but with some capacity to reproduce aspects of the income distribution in a developing country like Brazil. The simplification of household heterogeneity allows exploring more complexity on the supply side and also permits adding aggregate uncertainty with a range of short-term shocks. In summary, one of the most important aspects of our model is the presence of three representative households with heterogeneity in asset market participation (capital, bonds, and firm shareholding), labor skills, and labor supply elasticities. Moreover, in our setting, firms face oligopolistic/oligopsonistic competition on goods and labor markets with an endogenous entry-exit decision. This environment was later calibrated to match Brazilian economic characteristics, especially the wealth and income distribution through three quantiles of the country's population (bottom 50%, middle 49%, and top 1%).

2.5.1 Demand side

Households

Our economy is populated by three types of infinitely lived households $i \in \{c, o, h\}$, with no population growth. The first one, $c - agent$, represents Capitalists or the top 1%. The $o - agent$ are constrained optimizers or middle 49%. Finally, $h - agent$ are poor/hand-to-mouth households or the bottom 50%. Throughout the chapter, all these terms will be used interchangeably, referring to the type of agent. The parameters ω^o and ω^h are the proportion of constrained optimizers and hand-to-mouth households, respectively. The number of capitalists is given by $1 - \omega^o - \omega^h$.

Households i derives utility or desutility from consumption, c_t^i, and labor, l_t^i, obeing a preference function with the same specification for all of them:

$$E_0 \sum_{t=0}^{\infty} \beta^t \left(\frac{(c_t^i)^{1-\chi^c}}{1-\chi^c} - \theta^i \frac{(l_t^i)^{1+\chi^l}}{1+\chi^l} \right) \tag{2.10}$$

β is a common discount factor, χ^c is the relative risk aversion, and χ^l is the inverse of the Frisch elasticity of labor supply (notice that these two last parameters are the same for the three types of agents). Finally, θ^i is an agent-specific parameter that weights the disutility of working and allows obtaining a steady-state equilibrium with calibrated working hours. The types of agents differ in their optimizing behavior as they face different limitations when accessing the asset market, resulting in specific budget constraint profiles.

Therefore, the top 1% households, who own the firms and have savings in the form of government bonds, solve the expected utility maximization problem subject to the following budget constraint:

$$c_t^c + b_t^c + \upsilon N_{et}^c = r_{t-1} b_{t-1}^c + w_t^c l_t^c + \pi_t N_t^c - \frac{1 - \Omega^\circ \omega^\circ - \Omega^h \omega^h}{1 - \omega^\circ - \omega^h} t_t \tag{2.11}$$

and to the following law of motion for the number of firms:

$$N_t^c = (1-\delta^{\upsilon})N_{t-1}^c + N_{et}^c \tag{2.12}$$

The left side of the budget represents current spending, given that c_t^c represents consumption, b_t^c represents resources used to purchase new government bonds, and υN_{et}^c represents total investment in new firms. The υ is a key variable in the model representing the firm entry cost. It will define the optimum number of active companies in the economy. N_{et}^c is the capitalists' per capita number of new firms entering in a period. On the income side, $r_{t-1}b_{t-1}^c$ is the resource obtained from bond holdings, and $w_t^c l_t^c$ and $\pi_t N_t^c$ are labor income and total profits received by capitalist households, respectively. While N_t is the number of active firms, in per capita terms, and π is the profit collected by each one of them. Finally, t_t is a lump-sum tax given to the government at a given period (the term before t_t is a device guaranteeing tax neutrality, allowing each agent to pay, per capita, an amount proportional to their total income).

Now, let's look with more attention to the number-of-firms law of motion. Recall that the number of firms that enter in period t is given by N_{et}^c. As we can see in equation 2.12, each period, a fraction δ^{υ} of firms exits exogenously. In the same equation, it is worth noticing that, as in Jaimovich and Floetotto (2008), there is no time to build – a new firm starts producing in the same period.

Deriving the first-order conditions for top 1% households, we obtain the inverse of consumption´s marginal utility:

$$U^c{}_t = (c_t^c)^{\chi^c} \tag{2.13}$$

A Frisch labor supply function relating labor and consumption choices:

$$w_t^c = U_t^c \theta^c \left(l_t^c\right)^{\chi'} \tag{2.14}$$

Capitalists' bond/saving decision:

$$1 = r_t \, \beta \, E_t \left\{ \frac{U^c{}_t}{U^c{}_{t+1}} \right\} \tag{2.15}$$

As well as shareholding decision:

$$\upsilon_t = \pi_t + \beta \, (1 - \delta^{\upsilon}) \, E_t \left\{ \frac{U^c{}_t}{U^c{}_{t+1}} \, \upsilon_{t+1} \right\} \tag{2.16}$$

The last turns out to be a dynamic zero profit condition since the owners will invest in new firms until the left-hand side, the entry cost υ (this parameter will be pinned down when calibrating the model to achieve the desired markup level), equals the right-hand side, the sum of discounted expected profits.

As previously stated, 49% households have limited access to financial markets. Similar to Cantore and Freund (2021), middle-class agents' bond choices are subject to a cost. These households are penalized when their bond holdings deviate from steady-state levels. The per-period budget constraint for constrained optimizers reads:

$$c^o_t + i^{\circ}_t + b^{\circ}_t + \frac{\varphi^{\circ}}{2} \frac{(b^{\circ}_t - b^o_{ss})^2}{c^o_{ss}} = r^k_t k^o_{t-1} + r_{t-1} b^o_{t-1} + w^o_t l^o_t + f_t - \Omega^{\circ} t_t \tag{2.17}$$

The adjustment cost takes a simple quadratic form, and the weight for the bond friction is given by φ^o. The costs´ wealth effects are neutralized by a lump-sum transfer f_t. Furthermore, the budget is identical to that of capitalist agents in every way except for the presence of physical capital investment, represented by i_t, and rental income (in place of income from ownership and spending in intermediate-goods firms). With a standard specification found in the DSGE literature, these households also pay adjustment costs when choosing to change their investment level, given the following law of motion for capital:

$$k^o_t = (1 - \delta) k^o_{t-1} + \left[1 - \frac{\kappa_I}{2} \left(\frac{i^{\circ}_t}{i^{\circ}_{t-1}} - 1 \right)^2 \right] i^{\circ}_t \tag{2.18}$$

Deriving the first-order conditions for these households, we get an Euler equation for bonds:

$$bc = r_t \, \beta \, E_t \left\{ \frac{U^o{}_t}{U^o{}_{t+1}} \right\} \tag{2.19}$$

Where bc_t is the optimizers' bond adjustment cost:

$$bc_t = 1 + \frac{\lambda^o \, (b^o{}_t - b^o_{ss})}{c^o_{ss}} \tag{2.20}$$

We also get the FOC for capital, with the associated Tobin´s q:

$$1 = \beta \, E_t \left\{ \frac{\frac{U^o{}_t}{U^o{}_{t+1}} \, [r_{t+1} + (1-\delta) \, q_{t+1}]}{q_t} \right\} \tag{2.21}$$

And, finally, the FOC for the investment decision:

$$\begin{aligned} 1 = q_t & \left[1 - \tfrac{\kappa_I}{2} \left(\tfrac{i^o{}_t}{i^o{}_{t-1}} - 1 \right)^2 - \kappa_I \left(\tfrac{i^o{}_t}{i^o{}_{t-1}} \right) \left(\tfrac{i^o{}_t}{i^o{}_{t-1}} - 1 \right) \right] \\ & + \beta \, \kappa_I \, E_t \left\{ \tfrac{U^o{}_t}{U^o{}_{t+1}} \, q_{t+1} \left(\tfrac{i^o{}_{t+1}}{i^o{}_t} - 1 \right) \left(\tfrac{i^o{}_{t+1}}{i^o{}_t} \right)^2 \right\} \end{aligned} \tag{2.22}$$

The Frisch labor supply and the inverse of marginal utility are similar to those for the top 1% agents:

$$U^o{}_t = (c^o_t)^{\chi^o} \tag{2.23}$$

$$w^o_t = U^o_t \theta^o \, (l^o_t)^{\chi'} \tag{2.24}$$

The Bottom 50% class are hand-to-mouth or non-Ricardian households. This type of agent has no access to financial markets and does not decide on savings, consuming all their income. Thus, given their utility function and budget constraint, the only choice is between work/leisure levels, based on the relationship between consumption marginal utility/labor marginal disutility and wages (Frisch labor supply function). Those decision equations are expressed below.

Budget constraint:

$$c_t^h = w_t^h l_t^h - \Omega^h t_t \tag{2.25}$$

Labor supply:

$$w_t^h = U_t^h \theta^h \left(l_t^h\right)^{\chi'} \tag{2.26}$$

Government

Closing our model's demand side, there is the government consumption. Public expenditure, g_t, is financed by raising lump-sum taxes t_t and issuing debt:

$$b_t = g_t + r_{t-1}\, b_{t-1} - t_t \tag{2.27}$$

However, to rule out an explosive debt path, it must follow a tax evolution given by a fiscal rule that reads:

$$\tilde{t}_t = \rho_t\, \tilde{t}_{t-1} + \rho_{tg} \tilde{g}_t \, + \rho_{tb}\, \tilde{b}_t \tag{2.28}$$

Where the variables with tildes indicate deviations from the steady-state share of income.

2.5.2 Supply side

Our model's supply side is an extension of original works from Jaimovich and Floetotto (2008), and Alpanda and Zubairy (2021). Basically, the economy is defined by a continuum summing one of sectors, indexed by ι. In each sector and period, there is a finite number N_t of intermediate firms that produce a differentiated good, indexed by $j \in \{1; 2; ...N\}$. Thus, each intermediate-good firm possesses oligopoly power in the goods market and, as we will show, oligopsony power in labor markets. It is worth highlighting that the entry and exit of intermediate producers into the sectors are endogenous, obeying a dynamic “zero-profit” condition established by capitalists’ decision with respect to the investment in new firms.

Goods market

The final good of the economy, which is consumed and invested in by households, is produced by perfectly competitive firms that aggregate the sector-specific goods, $y_t(\iota)$, into the aggregate good, y_t, using a constant-returns-to-scale production function:

$$y_t = \left(\int_0^1 y_t(\iota)^{\frac{\phi-1}{\phi}} d\iota \right)^{\frac{\phi}{\phi-1}} \tag{2.29}$$

Where ϕ is the elasticity of substitution between the sectoral goods. Solving the final good firms´ optimization problem, the demand function for sectoral goods reads:

$$y_t(\iota) = \left(\frac{p_t(\iota)}{p_t} \right)^{-\phi} y_t, \tag{2.30}$$

Where $p_t(\iota)$ is the sector ι price index and the aggregate price (p_t) is given by:

$$p_t = \left(\int_0^1 p_t(\iota)^{1-\phi} d\iota \right)^{\frac{1}{1-\phi}}. \tag{2.31}$$

Furthermore, departing from the traditional monopolistic competition with Dixit and Stiglitz (1977) formulation, the sectoral goods, $y_t(\iota)$, are now the result of the the transformation of firm-specific goods, $y_t(\iota; j)$, using the following CES function:

$$y_t(\iota) = N_t^{-\frac{1}{\tau-1}} \left(\sum_{j=1}^{N_t} y_t(\iota, j)^{\frac{\tau-1}{\tau}} \right)^{\frac{\tau}{\tau-1}} \tag{2.32}$$

Here, the elasticity of substitution between goods is given by the parameter τ. Notice that the number of firms may vary across periods. For tractability, there is a first right-hand side term that rules out the possible variety effect on the CES function formulation. At the aggregate, imposing symmetric equilibrium, this combination of measure one sectors and N firms will guarantee that $y_t(\iota, j) = y_t(\iota) = y_t$ for all ι and j. The same will apply to the desegregation process in the labor market in the next section.

With this CES function, we can obtain a demand curve for firms' goods as follows:

$$y_t(\iota, j) = \left(\frac{p_t(\iota, j)}{p_t(\iota)}\right)^{-\tau} \frac{y_t(\iota)}{N_t} \tag{2.33}$$

And $p_(\iota)$ is a price index as:

$$p_t(\iota) = N_t^{\frac{1}{\tau-1}} \left(\sum_{j=1}^{N_t} p_t(\iota, j)^{1-\tau}\right)^{\frac{1}{1-\tau}} \tag{2.34}$$

Including 27 into 30, the demand function for each firm j in each sector ι is given by:

$$y_t(\iota, j) = \left(\frac{p_t(\iota, j)}{p_t(\iota)}\right)^{-\tau} \left(\frac{p_t(\iota)}{p_t}\right)^{-\phi} \frac{y_t}{N_t} \tag{2.35}$$

The above demand function formulation results in a price elasticity faced by a single firm which is a function of the number of firms within the sector and, taking symmetry assumption, has the following specification:

$$\eta_{y(\iota,j)p(j,\iota)}\left(N_t\right) = \tau - \frac{1}{N_t}\left(\tau - \phi\right) \tag{2.36}$$

Labor market

At the middle and bottom of the income distribution (Constrained Optimizers and Hand-to-Mouth), the homogeneous labor supplied by each household is differentiated by competitive intermediaries into sector and firm labor services.

The first step, the disaggregation of homogeneous labor into sector labor $l_t^i(\iota)$, for each household i, respect the following function:

$$L_t^i = \left[\int_0^1 L_t^i(\iota)^{\frac{\phi_w^i + 1}{\phi_w^i}} d\iota \right]^{\frac{\phi_w^i}{\phi_w^i + 1}} \tag{2.37}$$

Notice that the elasticity of substitution, ϕ_w^i, is specific to the household. Considering the competitive behavior among the labor intermediaries, we obtain a sector labor supply function for sector ι as:

$$L_t^i(\iota) = \left(\frac{w_t^i(\iota)}{w_t^i} \right)^{\phi_w^i} L_t^i \tag{2.38}$$

In this setting, $w_t^i(\iota)$ is the wage index in each sector for each household. In addition, there is a household-specific aggregate wage index, w_t^i, linked to the sector-specific wage by:

$$w_t^i = \left(\int_0^1 w_t^i(\iota)^{1+\phi_w^i} d\iota \right)^{\frac{1}{1+\phi_w^i}} \tag{2.39}$$

Similar as in Alpanda and Zubairy (2021), there is a second disaggregation, from sector-household-specific labor, $L_t^i(\iota)$, to firm-household-specific, $L_t^i(\iota, j)$, following another CES function:

$$L_t^i(\iota) = N_t^{\frac{1}{\tau_w^i + 1}} \left(\sum_{j=1}^{N_t} L_t^i(\iota, j)^{\frac{\tau_w^i + 1}{\tau_w^i}} \right)^{\frac{\tau_w^i}{\tau_w^i + 1}} \tag{2.40}$$

Once again, it is worth noticing that the elasticity of substitution, τ_w^i, is specific to the household i (Bottom 50% and Middle 49%). The first right-hand side term, $N_t^{\frac{1}{\tau_w^i + 1}}$, rules out the variety effect on this CES function. Symmetric equilibrium assumption gives that $N_t L_t^i(\iota, j) = L_t^i(\iota)$ $= L_t^i$ for all ι and j

From 36 and given perfect competition, we achieve a supply curve faced by firm j, in sector ι, for household i's labor:

$$L_t^i(\iota, j) = \left(\frac{w_t^i(\iota, j)}{w_t^i(\iota)}\right)^{\tau_w^i} \frac{L_t^i(\iota)}{N_t} \tag{2.41}$$

It can now be shown that the sector wage index is given by:

$$w_t^i(\iota) = N_t^{-\frac{1}{1+\tau_w^i}} \left(\sum_{j=1}^{N_t} w_t^i(\iota, j)^{1+\tau_w^i}\right)^{\frac{1}{1+\tau_w^i}} \tag{2.42}$$

Finally, combining firm and sector functions for each household, we get a supply function faced by the firm as:

$$L_t^i(\iota, j) = \left(\frac{w_t^i(\iota, j)}{w_t^i(\iota)}\right)^{\tau_t^i} \left(\frac{w_t(\iota)}{w_t}\right)^{\phi_w^i} \frac{L_t^i}{N_t} \tag{2.43}$$

The supply function formulation yields the following wage elasticity for each household i (Bottom 50% and Middle 49%) that reads:

$$\eta^i_{L(\iota,j)w(j,\iota)}(N_t) = \tau_w^i - \frac{1}{N_t}\left(\tau_w^i - \phi_w^i\right) \tag{2.44}$$

Firms decision

As shown before, the model has, in each sector, at each period, a number N of intermediate firms selling goods with some market power, in an oligopolistic kind of market structure, and buying input with oligopsonistic behavior. Thus, one crucial assumption that gives tractability to the model is that the number of firms is the same in the goods and labor markets.

The firm j in sector ι faces a production frontier determined by the following Cobb–Douglas function:

$$y_t(\iota, j) = A_t k_{t-1}(\iota, j)^{\alpha} L_t^c(\iota, j)^{\alpha^c} L_t^o(\iota, j)^{\alpha^o} L_t^h(\iota, j)^{\alpha^h} \tag{2.45}$$

As in Areosa and Areosa (2016), there is heterogeneity in labor since each of the three agents/households offers differentiated labor services (L_t^c, L_t^o, L_t^h), with specific output elasticities (α^c, α^o, α^h). Despite this, the production exhibits constant returns to scale because the following constraint was imposed: $1-\alpha = \alpha^c + \alpha^o + \alpha^h$.

The oligopolistic intermediate-good firms choose price, output, wages, labor, and capital, aiming to maximize profits, given by:

$$\begin{aligned} \pi_t(\iota, j) = p_t(\iota, j) y_t(\iota, j) - w_t^c(\iota, j) l_t^c(\iota, j) - w_t^o(\iota, j) l_t^o(\iota, j) \\ - w_t^h(\iota, j) l_t^h(\iota, j) - r_t^k k_{t-1}(\iota, j) \end{aligned} \tag{2.46}$$

The optimization problem above is static and is subject to demand and supply functions (equations 2.32 and 2.39) and sectorial/aggregate wages indexes. Firms solve the maximization problem while taking their competitors' decisions in the same industry and economic aggregates as givens (treating their competitors as parameters).

The first order conditions for this problem, imposing symmetry, bring the economy's price and marginal markup, $\mu_t(N_t)$:

$$\mu_t(N_t) = \frac{\tau - \frac{1}{N_t}\,(\tau - \phi)}{\tau - \frac{1}{N_t}\,(\tau - \phi) - 1} \tag{2.47}$$

Wages markdowns, $md_t^i(N_t)$, for each type of household (except capitalists, whose labor market is perfectly competitive):

$$md_t^i(N_t) = \frac{\tau_w^i - \frac{1}{N_t}\,(\tau_w^i - \phi_w^i)}{\tau_w^i - \frac{1}{N_t}\,(\tau_w^i - \phi_w^i) + 1} \tag{2.48}$$

And the economy's factor prices (wages and capital rental rate):

$$w^i{}_t = md^i{}_t \, \frac{y_t \, \alpha^i}{l^i{}_t} \, \frac{1}{\mu_t} \tag{2.49}$$

$$w^c{}_t = \frac{y_t\,\alpha^c}{l^c{}_t}\,\frac{1}{\mu_t} \tag{2.50}$$

$$r^k{}_t = \frac{y_t\,\alpha}{k_{t-1}}\,\frac{1}{\mu_t} \tag{2.51}$$

Both the economy's markup and the markdowns for poor and middle-class households depend on the number of firms, which is endogenous since N is a function of profits and capitalists' behavior (given by the shareholding equation). Furthermore, firms' market power and profits are a combination of the effects of markdowns and markups, both dampening the value of wages for poor and middle-class households (each one with different elasticities resulting from labor supply function and so with specific markdowns). These are fundamental features of the model, with an impact on inequality levels and their dynamics.

2.5.3 Aggregation and market clearing

Since the model has some heterogeneity between households, to obtain the economy's aggregates, it is necessary to transform per capita variables, weighting them by the population parameters ω^o and ω^h, as follows:

Consumption:

$$c_t = c_t^o\,\omega^o + c_t^h\,\omega^h + c_t^c\,\left(1 - \omega^o - \omega^h\right) \tag{2.52}$$

Physical capital:

$$k_t = k_t^o \omega^o \tag{2.53}$$

Number of firms:

$$N_t = N_t^c\left(1 - \omega^o - \omega^h\right) \tag{2.54}$$

Constrained optmizers aggregate labor:

$$L^o{}_t = l^o_t\, \omega^o \tag{2.55}$$

Hand-to-Moth´s aggregate labor:

$$L^h{}_t = l^h_t\, \omega^h \tag{2.56}$$

Capitalists´ aggregate labor:

$$L^c{}_t = l^c_t\, \left(1 - \omega^o - \omega^h\right) \tag{2.57}$$

Government bonds:

$$b_t = b^o_t\, \omega^o + b^c_t\, \left(1 - \omega^o - \omega^h\right) \tag{2.58}$$

Physical capital investment:

$$i_t = i^o_t\, \omega^o \tag{2.59}$$

Investment in new firms:

$$Ne_t = Ne^c_t\, \left(1 - \omega^o - \omega^h\right) \tag{2.60}$$

Finally, at equilibrium, the market clearing conditions given below must hold:

$$y_t = c_t + i_t + \upsilon_t\, Ne_t + g_t \tag{2.61}$$

2.6 Steady state, calibration and comparative analysis

Our steady-state model equations are, in most parts, very similar to other dynamic general equilibrium frameworks found in the economic literature. However, in this section, it is important to emphasize how, in the static equilibrium, we pinned down the circular relationship between entry cost/markup/number of firms/profits. For this equilibrium to be found, it was necessary to calibrate a specific exogenous value for the markup (alternatively, we could have chosen the number of firms). As we have seen, the steady-state markup is given by:

$$\mu_{ss}(N_{ss}) = \frac{\tau - \frac{1}{N_{ss}}\left(\tau - \phi\right)}{\tau - \frac{1}{N_{ss}}\left(\tau - \phi\right) - 1} \tag{2.62}$$

The inverse function gives the number of firms N, since τ and ϕ are parameters to be calibrated. The profit per firm obeys the following formulation:

$$\pi_{ss} = \frac{y_{ss} - r_{ss}\,k_{ss} - {w^o}_{ss}\,{l^o}_{ss} - {w^h}_{ss}\,{l^h}_{ss} - {w^c}_{ss}\,{l^c}_{ss}}{N_{ss}} \tag{2.63}$$

Assume that we have the stationary values of the aggregated variables (output, capital, and working hours). We will then recover a value for π since the markup and the markdown, which also depend on N, determine the economy's factor prices. Thus, finally, we will obtain the fixed cost of entry, because, in the steady-state, it is given by a combination of parameters plus the value of the individual firms' profit:

$$\upsilon_{ss} = \pi_{ss} + \beta(1 - \delta^{\upsilon})\upsilon_{ss} \tag{2.64}$$

It is worth noting that our equilibrium concept involves the calibration/normalization of a unit output, which allows comparisons between the base model and its alternative versions. The values for different agents' worked hours were also exogenously set, taking as a reference the average number of hours in each income distribution range in the household survey carried out annually by the Brazilian statistical office (IBGE). This survey is from 2015, the last year with inequality indices estimates provided by the World Inequality Database (WID). All values for parameters calibration are in table 2.3.

Table 2.3: Base model - parameters values

Parameter	Value	Description
α	0.30	Capital share
α^o	0.46	Middle 49% labor share
α^h	0.16	Bottom 50% labor share
α^c	0.08	Top 1% labor share
β	0.97	Discount Factor
δ	0.025	Depreciation
δ^v	0.036	Firms exit
ω^o	0.49	Middle 49% share
ω^h	0.50	Bottom 50% share
τ	16	Elasticity of substitution
τ_w^o	25	Elasticity of substitution, Labor Middle 49%
τ_w^h	5	Elasticity of substitution, Labor Bottom 50%
ϕ	1	Elasticity of substitution
ϕ_w^o	1	Elasticity of substitution, Labor Middle 49%
ϕ_w^h	1	Elasticity of substitution, Labor Bottom 50%
χ^l	2	Frisch elasticity of labor supply
χ^c	2	Intertemporal elasticity
κ^i	5	Investment cost
λ^o	0.25	Bond cost, Middle 49%
y_{ss}	1	Output at steady state
A_{ss}	2.9435	TFP at steady state
l_{ss}^o	0.3483	Middle 49% worked hours
l_{ss}^c	0.3766	Top 1% worked hours
l_{ss}^h	0.3033	Bottom 50% worked hours
μ_{ss}	1.08	Markup at steady state
v_{ss}	0.3122	Entry cost
g_{ss}	0.32	Public spending at steady state

Parameterization was defined regarding a quarterly calibration [2]. Some of the parameters were taken from Brazilian and international literature related to DSGE models, including α (capital share, De Carvalho and Valli, 2011), δ (depreciation, Cavalcanti and Vereda, 2011), χ_l (Frisch elasticity of labor supply, Alpanda and Zubairy, 2021) χ_c (Intertemporal elasticity, benchmark), κ^i (Investment cost, benchmark) and λ^o (Bond cost, Cantore and Freund, 2021). Others were guided by official Brazilian data sources, such as hours worked, previously cited – l_{ss}^o (Middle 49%), l_{ss}^c (Top 1%) and l_{ss}^h (Bottom 50 %). In addition, β (Discount Factor) was pinned down considering the implicit interest rate on the Brazilian public debt (in 2015, according to the Brazilian independent fiscal institution – IFI). The same data were used to set the share of government spending, g_{ss}. Finally, δ^v (firms exit) was established by taking into account the

[2] Tests of indeterminacy and stability of the equilibrium of the model were conducted for plausible ranges of the main parameters.

IBGE's firm demography indicators (2015).

Second, there is the parameterization of elasticities, both in the goods and labor markets. Regarding the elasticities between sectors, we chose a safe path and, very similar to what did Jaimovich and Floetotto (2008), we set it to 1, although there is literature (Nechio and Hobijn, 2017), which points to an upper limit of up to 5 in the case of the goods market. Still, in the goods market, the literature is not consensual on the elasticity between firms in the same sector, with calibrations ranging from 3 to 20 (Alpanda and Zubairy, 2021). Therefore, we opted for a conservative path, choosing a value of 16 for τ.

Regarding τ_w^o and τ_w^h, there is not a great diversity of studies that try to estimate the wage elasticity of labor supply in Brazil. For this reason, our primary reference was Tucker (2017), which estimated elasticities in an interval between 15 and 75 for the Brazilian formal labor market. In theory, this formal market encompasses both workers in the middle of the distribution and those at the top. Therefore, we adopted values closer to the lower range when calibrating τ_w^o (25). On the other hand, most of the lower class members do not access the formal labor market. So, given the lack of empirical evidence, we assumed that their labor supply is more inelastic than the others, an option justified by the possible frictions that raise firms' market power (e.g., informational difficulties and restricted access to markets given mobility restrictions). Due to the degree of uncertainty in these parameters, we will further test the results through a sensitivity analysis.

Our last parameter of concern, the economy-wide markup (u), also faces a high degree of uncertainty because there are numerous difficulties in its empirical estimation. Previously, we have shown the figures from De Loecker and Eeckhout (2020). They estimated a markup of 1.60 for Brazil. In our calibration, however, we opted for a considerably lower value (1.08). Here are some of the reasons. First, some facts lead us to believe that De Loecker and Eeckhout's markup is overestimated, although the relative positions in the rankings of developing countries may be somewhat accurate. In their paper, the authors use data from the Worldscope dataset. As they notice, companies tend to be large and mainly publicly traded (and probably could have more market power) in this database. Therefore, genuine concerns may exist about the representativeness of the sample. In addition, they calculated the markup for various countries using the variable input elasticities estimated for the United States in De Loecker and Eeckhout (2020). It would not be absurd to imagine the possibility of obtaining lower elasticities if they had estimated production functions for Brazil. Equation 3.4 reveals that this would lead to lower markups.

To conclude this point regarding markups, it is relevant to emphasize that, in our model, market power is a combination of markups and wage markdowns, something not considered in De Loecker and Eeckhout (2020) empirical specification since they assumed that firms take input price as given. To observe the repercussions of these modeling differences, consider the bottom 50% case. The markup level of 1.08 combined with a wage markdown of 0.81 will result in a margin between labor marginal product and wages paid by firms of around 25%, in the steady state. Despite these considerations, the markup calibration will also be subject to sensitivity analysis.

For comparison purposes, after calibrating the base model and a set of alternative versions (details in appendix A.1), we simulated the steady states and collected the outputs related to the income distribution (Gini index and income shares from the bottom 50% and the top 1%). This exercise was carried out to assess the ability of each model to reproduce the income inequality observed in Brazilian data (World Inequality Database estimates for the year 2015). The results are in table 2.4.

Table 2.4: Models´ steady states outputs for key income inequality indices

Key indicators	Data (WID)	Baseline	Monopolistic	No skills	Two agents*
50% share (2015)	12.5%	10.4%	14.4%	37.2%	15.7%
1% share (2015)	23.6%	25.6%	18.7%	8.9%	84.3%
Gini (2015)	0.62	0.51	0.44	0.16	0.74

**Hand-to-Mouth (90%), Capitalists (10%).*

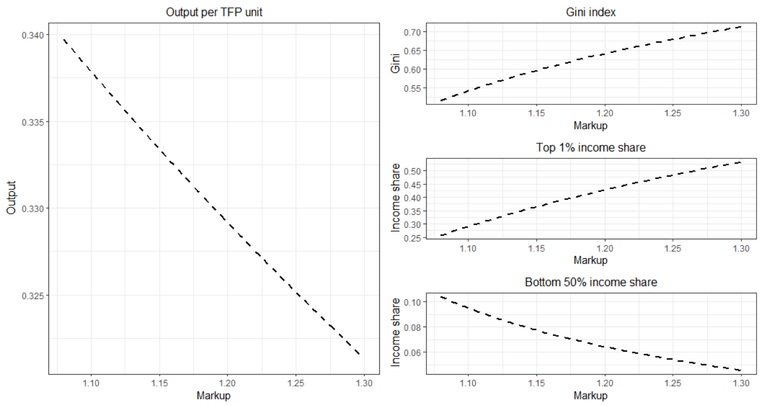
The first point to stand out is that models with only two agents and without heterogeneity in the workforce (the last two columns) have poor performance when the objective is to mimic income distribution. With a substantial share of non-Ricardian households, the two-agent model overestimates inequality (the inverse would occur if we increased the number of agents with access to the asset market). At the same time, the absence of labor skills generates a more equitable distribution than the one found in the data. Adding a third middle-class agent improves the model's performance concerning the indicators of the income distribution. Even so, the assumption that the labor markets have a monopolistic competition structure (a device quite common in models with stickiness in wages) dampens the results with respect to WID estimates (although it does not invalidate this modeling option, since the outcomes are not so discrepant, and with different calibration could achieve better results).

Our original model obtains the best approximation to the data considering the proposed calibration, despite a lower Gini index. However, we must consider that the model does not account for inequalities within the income groups, which would increase the index. Furthermore, we must recognize a trade-off between supply-side/asset market complexity and household distribution accuracy. Heterogeneous models, depicting the complete distribution, should give better results with respect to the Gini index, but with simplified assumptions about the market structure, asset options, and economy-wide shocks. Concerning the limitations, we think that our proposed model offers a good alternative, with good results in terms of distribution and big improvements in terms of supply-side and overall uncertainty.

2.7 Comparative static analysis

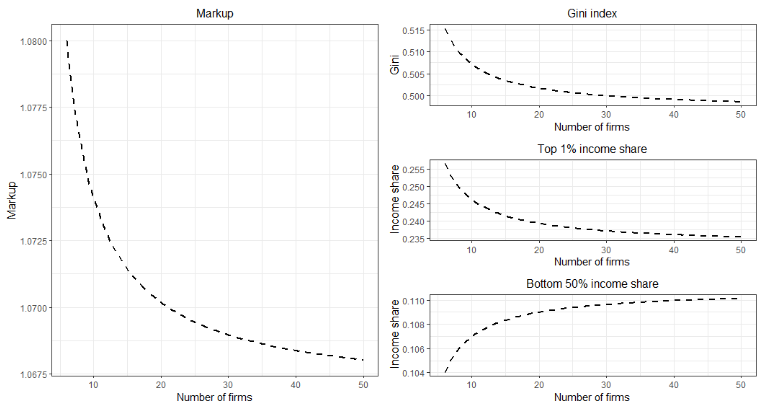
In this section, before getting to the model's dynamics, we are interested in how changes in some of the calibration parameters impact the macro aggregates outcomes observed in the steady-state (output and inequality indices). Our primary focus is on two types of parameters about which there is no agreement in the literature: markup (u) and within-sector household-specific labor elasticity of substitution (τ_w^h and τ_w^o). Additionally, we also considered how calibrating the number of firms would change the model's steady-state results, and there are two main reasons for this exercise. First, in practice, when dealing with an antitrust case, markup values are unobservable to authorities. However, by defining a market, these authorities can notice the number of competing firms. So, it is relevant to observe, in a comparative static way, how changes in this market feature are capable of impacting inequality, for example. Also, as we'll see, our second reason has to do with the fact that the number of firms has a more complex and nonlinear effect on the results, while the results of the markup exercise are very clear.

Figure 2.5: Sensibility to markup (u_{ss})



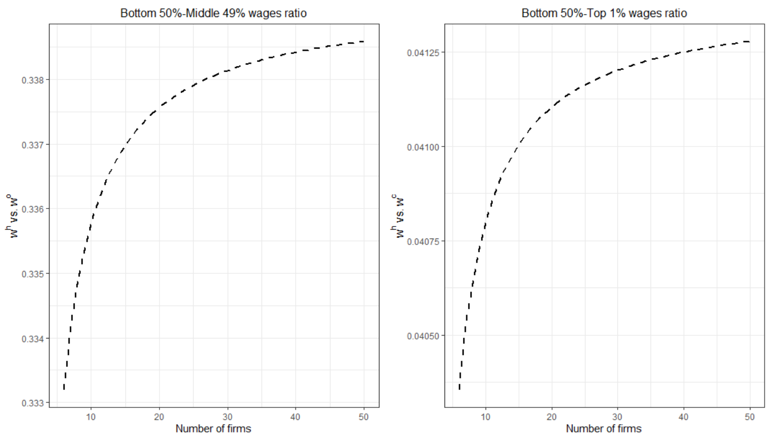
The sensibility analysis for markup is shown in figure 2.5. We gradually increased the parameter value until it reached 1.30, deviating from our benchmark calibrated markup ($u_{ss} = 1.08$). As we can see, increasing markup while holding all other parameters constant reduces steady state economy-wide productivity (because y_{ss} was normalized, the efficiency is measured by output per TFP unit: $\frac{y_{ss}}{A_{ss}}$). It is worth noticing that this relationship does not diverge with respect to the traditional economic literature results, since basic models also predict inefficient outcomes. Markup's effect on output is markedly linear, and more or less the same occurs with inequality indeces. Instead, the impact on the Gini index and inequality, in general, is more dramatic. In our initial configuration, the Gini value was about 0.50. If we consider a markup of up to 1.30, the Gini's result goes up to 0.70. This is an extremely high value, but it is not far from the WID estimate (0.62). However, for our model specification, the upper bound of this sensibility exercise begins to seem unrealistic when we observe the income share values. The 1.30 markup translates to an above 50% income share for the top and a below 5% share for the bottom of the distribution. Therefore, this is one more reason for not calibrating the model with De Loecker and Eeckhout (2020) values. Their markup will give implausible results for the economy's income distribution.

Figure 2.6: Sensibility to number of firms (N_{ss})



In what concerns the impact of the sector-specific number of intermediate goods firms (figure 2.6), we observe a downward-sloping nonlinear effect on markups. When we vary the steady state number of competitors (N_{ss}), price markup changes quickly when N is low but stabilizes when it exceeds 50 firms. Recovering equation 2.62, we see that as N approaches infinity, the markup approaches that of monopolistic competition ($u_{ss} = \frac{\tau}{\tau-1}$). Market concentration affects the Gini index and income shares through effects on markups and wage markdowns, so there is another source of nonlinear effects in the income distribution side. As the wage markdown works like a markup multiplier, we have one important policy implication from the model's comparative static results. When competition agencies analyzes horizontal mergers, regarding distribution issues, they should be more worried when a merger reduces the number of competitors from, say, six to five than when it reduces the number of firms in the market from twelve to eleven.

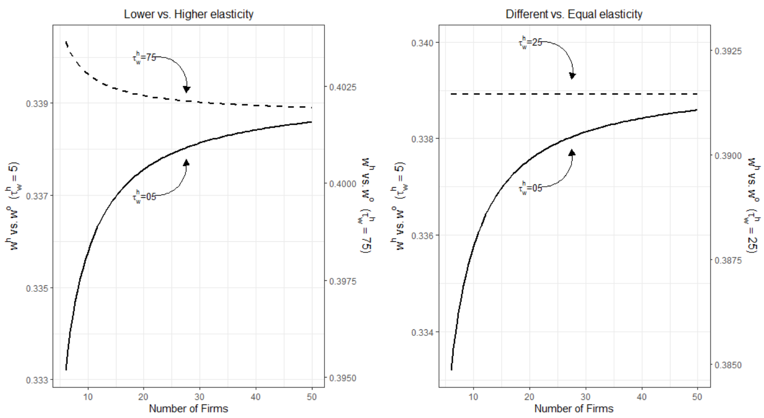
Figure 2.7: Wage ratios sensibility to number of firms



Results in figure 2.7 highlight the impact of wage markdown's channel on the distribution between the income quantiles. Moreover, it reveals the role of labor supply heterogeneity (differences in τ_w). The first panel shows how the ratio between the bottom and middle classes wages varies as we change the market concentration (number of intermediate firms). The second panel gives the same analysis but for the ratio bottom 50%/Top 1% wages. It is worth looking in detail at the first panel. As the market competition grows, the bottom 50% wages react strongly than middle-class agent's wage, increasing the ratio. This movement is explained by labor supply heterogeneity. Given that $\tau_w^o > \tau_w^h$ (constrained optimizers' labor elasticity parameter is more prominent than hand-to-mouth's), the labor market for middle-class agent is closer to perfect competition, being less affected by changes in the number of firms. In contrast, poor households face an immense oligopsony power in the labor market, which is alleviated when more firms enter the economy.

This feature is another relevant outcome from our model, with possible policy repercussions. In the presence of oligopolistic labor markets and heterogeneity, market power affects income distribution not only through markups and profits (in a top-bottom direction) but also through wages. Further, with our calibration (where labor supply is more elastic for the middle 49%), there is an extended distribution effect from middle to bottom households. As competition grows, the gains are more significant for poor households, although there are widespread benefits for workers in general. The model's interaction between competition, labor market outcomes, and inequality puts into question the antitrust enforcement focus, which gives extra weight to consumer prices due to the belief that labor markets are, in general, competitive. A growing literature supports these findings on the impacts of monopsony/oligopsony power on antitrust policy (e.g., Azar et al., 2020 and Marinescu and Hovenkamp, 2019).

Figure 2.8: Elasticity heterogeneity (τ_w^i) - impact on wage ratios



We can take a step further to see how the choice of elasticity parameters impacts the model's outputs. Figure 2.8 shows that the changes in the hand-to-mouth/constrained optimizers ratio are directly results from our calibration of the τ_w^o and τ_w^h values. In panel one, the dashed line indicates this ratio when the bottom 50% elasticity is greater than that of the middle 49%. Therefore, we see a reversal of the result, with a distributive effect more favorable to the middle classes when competition is increased. In the next panel, where the dashed line illustrates the case when there is no heterogeneity in elasticities, the effect of increased competition on the ratio of wages is null. Our assumption about the lower elasticity parameter for poor households is based on several features of the Brazilian labor market — great levels of informality, for example, supposedly give firms much bargaining power. However, because of the relevance of this parameter for the model's outcomes, we recognize that there exists an open path to future work aiming to estimate elasticities differences between income distribution classes. Moreover, the reliance on these elasticity parameters also leads us to conduct a sensitivity test in the session that presents the model's dynamic behavior.

2.8 Model dynamics

An important advantage of the model with limited heterogeneity (three agents) is the ease of studying the dynamics of economic aggregates, that is, how the main outcomes respond to unexpected shocks. In this section, we will initially focus on two types of shock. The first one addresses our initial focus related to the results of empirical motivation. It is an exogenous shock that changes the markup level. The following equation describes the dynamic evolution of this shock, which follows an AR(1) process:

$$\mu_t = (1 - \rho_\mu)\ \mu_{ss} + \rho_\mu\, \mu_{t-1} + \epsilon_{\mu_t} \tag{2.65}$$

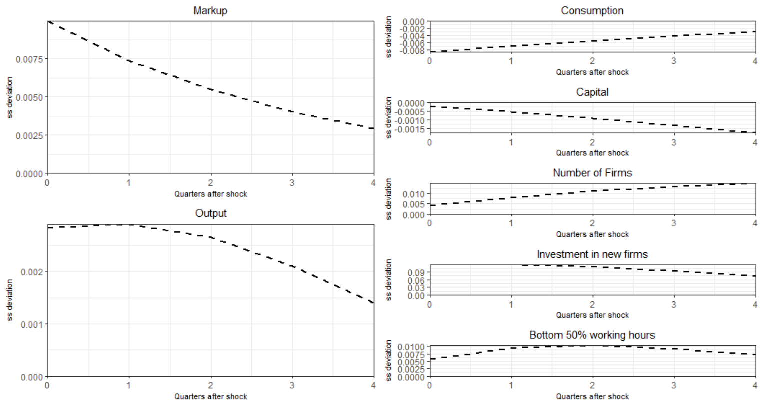
Where μ_{ss} is the markup on steady-state, ρ_μ is the persistence parameter, and ϵ_μ is the unexpected shock, calibrated to be equivalent to 1% variation in markup level. The second shock that we will study is related to short-term growth driven by productivity gains. It is, therefore, a disturbance in the TFP, with dynamics similar to markup:

$$A_t = (1 - \rho_a)\ A_{ss} + \rho_a\, A_{t-1} + \epsilon_{at} \tag{2.66}$$

The TFP disturbance was also calibrated to be equivalent to a 1% rise. Both the markup and TFP persistence parameters were set at 0.75.

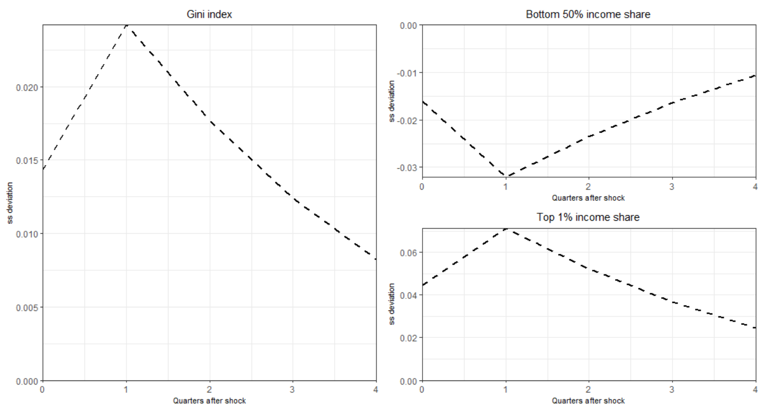
When verifying the macroeconomic aggregates' response to markup, in figure 2.9, a marked difference between the dynamic (general equilibrium) and static (partial equilibrium) analysis emerges. Here, the effect of an unexpected increase in markup is richer and counterintuitive: output increases, even without any efficiency gain. This phenomenon is due, in particular, to the dynamic response of the three agents. The income effect leads poor households to increase the number of working hours, in this way, compensating for the decrease in consumption (the marginal utility of consumption grows and mitigates the higher labor disutility). At the same time, the higher profit per firm leads the top 1% of households to invest more in creating new companies, alleviating the economic decline due to reduced physical capital expenditure.

Figure 2.9: Markup shock - Aggregates impulse-response functions



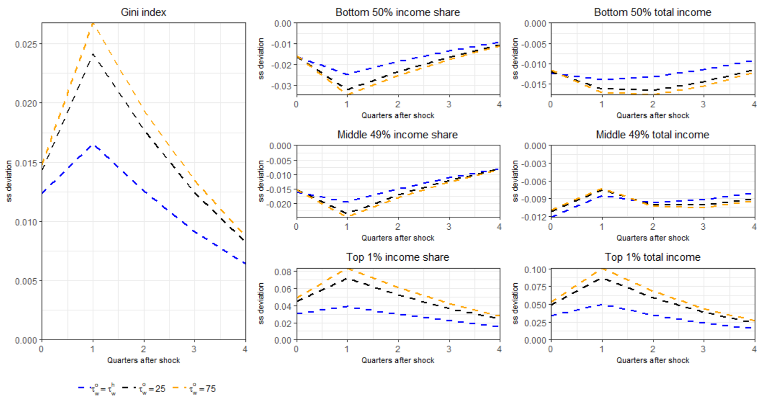
This short-run behavior favors much of the arguments used to justify long-lasting Brazilian growth and industrial policies that stimulated the firm's market power (e.g., state price-wages interventions during the military dictatorship). However, note that, in our model, permanent markup increases are inefficient in the long run, as we could see in the previous session analysis. In addition, the markup impact on inequality is relevant. The dynamics for the inequality indicators are shown in figure 2.10. Thus, it is possible to verify that, similar to the PVAR impulse response functions (IRF) estimated in the empirical motivation section, the markup perturbation has a positive effect on the Gini index. The increasing inequality reflects the income redistribution among the different agents, as the top 1% share increases while there is a reduction in income going to the poorest households.

Figure 2.10: Markup shock - Inequality impulse-response functions



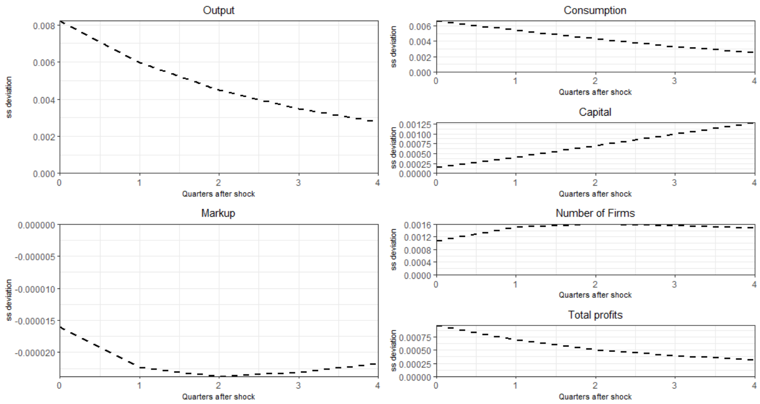
But how does the inequality's response to a markup perturbation change with labor supply heterogeneity? The dynamic sensibility test is plotted in figure 2.11. The blue line depicts the IRF from the model calibrated with $\tau_w^o = \tau_w^h = 5$. The black and orange ones represent, respectively, calibrations with $\tau_w^o = 25$ and $\tau_w^o = 75$, while τ_w^h remains in 5. In this calibration exercise, the labor supply heterogeneity between poor and middle-income households (remember that the labor market for the top 1% is perfectly competitive) seems to have no relevant developments in the general dynamics of inequality. In all scenarios, markup shock has a consistent regressive effect.

Figure 2.11: Markup shock - Impulse-response functions from different τ_w^o



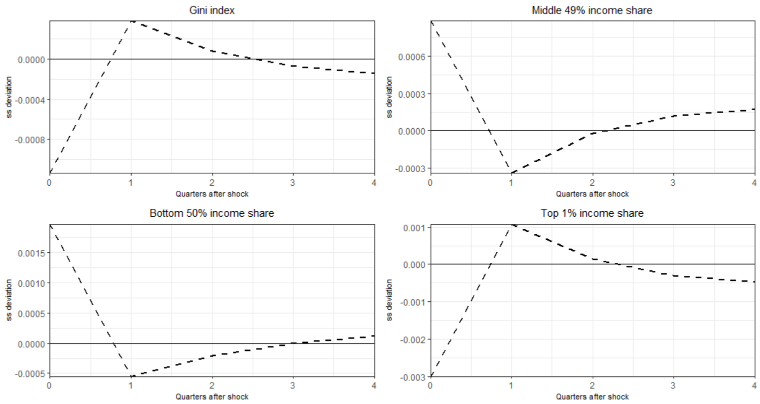
However, there are evident changes in the response levels. When elasticities are low and equal, the markdown channel dampens inequality growth. The mechanism is the following: as a result of greater profitability, the investment in new firms grows and, if, on the one hand, the higher markup redistributes income towards the top, on the other, the oligopsonistic effect diminishes due to the greater number of competitors in the labor market. As we increase the elasticity of one of the groups, this effect becomes less important (this group is closer to the competitive labor market and less influenced by the number of firms), allowing more significant expansion of Gini and other indicators.

Figure 2.12: TFP shock - Aggregates impulse-response functions



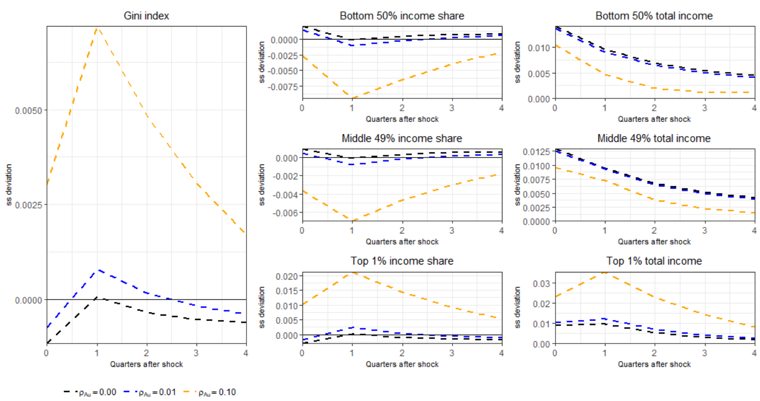
When the disturbance in the model steady state is due to an unexpected increase in productivity, results are consistent with the literature on stochastic dynamic equilibrium. The economy's production increases, as well as consumption and investment in physical capital. Nevertheless, our variable of interest is the markup due to its fundamental role in the distribution of income. As in Jaimovich and Floetotto (2008) and Etro and Colciago (2010), in response to the TFP shock, our model has a countercyclical markup. With greater marginal production, our dynamic zero profit condition (equation 2.16) pushes the creation of new firms to equalize profit and entry cost. This results in a lower markup. Thus, at the same time, two channels play a role, on impact, reducing inequality. The lower markup redistributes income from the top to the middle and poor classes. Similarly, the tightening of competition in the labor market also provides an increase in wage markdowns. Additionally, note that, on impact, the gain of the poorest agent is more significant due to the heterogeneity in labor supply seen in the previous session. This would be in line, for example, with a stylized fact observed in the Brazilian economy between 2006 and 2014. During this period of tightened labor market, a singular reduction in the Gini of labor income occurred, with the bottom households seeing a higher labor income increase (with some middle class "compression").

Figure 2.13: TFP shock - Inequality impulse-response functions



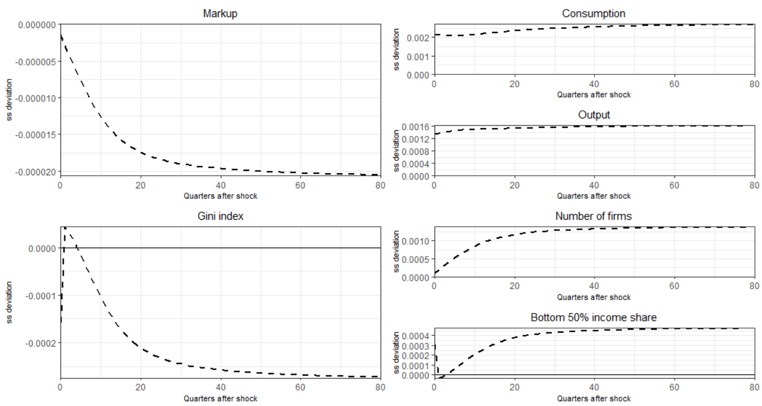
The sign of markup's business cyclical behavior is a matter in dispute in the macroeconomic field (e.g., Nekarda and Ramey, 2020). From the market competition point of view, markup may be pro or counter-cyclical depending on the type of reaction to a productivity gain. On the one hand, the response could be extensive when higher profitability allows firms' entry, resulting in fiercer competition. On the other hand, the reaction can assume an intensive characteristic due to entry barriers, with larger and more productive companies acquiring more market shares and market power. To emphasize the importance of markup cyclicality on inequality behavior, we ran another sensibility test in which TFP shock was positively correlated with markup shock ($\rho_{a\mu}$ parameter).

Figure 2.14: TFP shock correlated with Markup shock (ρ_{Au})



As we can deduce from figure 2.14, especially from the panel that represents the Gini's impulse response function, the direction of the markup's response determines the model's outputs regarding inequality. First, when the shock of the markup is in no way related to the unexpected gains in the TFP, there is a process of income deconcentration in the short run. Then, as we incorporate a correlation between the disturbances, the inequality-reducing behavior is progressively dampened until it reverses into an increase in Gini and top household income share. Some models that address the determinants of mergers and acquisitions waves point to a positive correlation between these processes and the business cycle (e.g., Lambrecht, 2004), which, in theory, would allow the increase of the firm's market power during the period of economic expansion (ϵ_a correlated with ϵ_μ). Based on our modeling exercise, this means that the strength of the antitrust policy when this type of merger wave happens during times of economic growth may have an effect on how the resulting income is shared.

Figure 2.15: Permanent shock on preferences (market growth)



So far, we have studied the short-run effects of supply-side disturbances. As a final analysis, we focus briefly on the impact of the demand-side on market power and inequality. A robust prediction of oligopoly theory is that larger markets are more competitive and have lower price-cost markups (e.g., Campbell and Hopenhayn, 2005). To verify that this assumption applies to this chapter's model, we simulate an unexpected and permanent shock to consumer preferences (a proxy for market growth). As a result, if we look at figure 2.15, we can see that, in line with the IO literature, a greater demand for goods and services increases the number of firms and reduces markup. In addition, there are gains in productivity, generating greater output. Finally, a novelty of our model is that greater demand also positively affects income distribution, reducing, in the long run, although not on impact, the Gini index and increasing the share of the poorest households in the economy's income.

2.9 Conclusion

The antitrust policy has been increasingly criticized for the lack of more general analyses, which consider the effects of its policies beyond the narrowed markets defined in mergers or abuse of dominance investigations. The impacts of market concentration on inequality are among the blind spots pointed out by researchers and practitioners and reveal that authorities should go beyond a homogeneous consumer surplus view. The channels that link inequality and market power need to be investigated deeply, especially in developing countries, where the ownership of oligopolistic firms is considerably more concentrated at the top of the income distribution (Medeiros and Castro, 2018) and the the levels of informality in the labor market and the low unionization rate make it plausible that firms retain a higher monopolistic power. With this in mind, this chapter proposed a general equilibrium model, with an endogenous firm's entry mechanism, to assess how market power, both in the goods and labor markets, affects the dynamics of income inequality.

Our modeling exercise has some relevant implications for antitrust policy. Throughout this chapter, we present some of them, such as the non-linear impact of concentration on market power and inequality. As a result, concern about the effects of mergers should grow exponentially as the number of competitors decreases. Another point worth mentioning is the relationship between cyclical markup behavior and inequality. Countercyclical markups reduce inequality and vice versa, and the direct implication of this for the authorities' work is the need to closely monitor waves of mergers and acquisitions triggered by economic booms. Nevertheless, perhaps the most outstanding contribution of this work in terms of competition policy is the emphasis on the repercussions of oligopsonistic behavior in the labor market. An antitrust policy concerned with inequality must look carefully at the sectorial dynamics of labor supply. As we have seen, it is crucial for the functional redistribution of income, both top to bottom and middle to bottom. Hence, being pro-poor if the bottom agents are less sensitive to changes in firm-sector specific wages and otherwise.

Evidently, this work has some limitations that should stimulate further research. Greater attention is needed to the relationship between market power, informality, and the minimum wage regarding the dynamics of inequality. The minimum wage could introduce discontinuities in the model, functioning as a lower bound for more privileged classes and as a ceiling for the bottom of the distribution. We also do not advance issues related to firms' heterogeneity, which could condition some of the conclusions drawn from our framework. Finally, from an empirical point of view, it would be essential to better understand the labor supply behavior throughout the income distribution, estimating the differences in elasticities with robust empirical strategies.

3

Market Power, Wages and Inequality: evidence from Brazil

3.1 Introduction

A growing body of evidence shows that firms' market power is fundamental to explaining macroeconomic phenomena such as the decline in labor share and the growth of inequality. The predominant approach favors product market analysis. A recent example of this view, with significant impact, is the work of De Loecker et al. (2020), which derived a method to estimate markups through the cost minimization assumption. However, although relatively less mainstream, there is also relevant literature that addresses the issue of market power and its impacts on workers through the lens of Oligopsony/Monopsony models.

Markups (firms' ability to set prices in the product market) and markdowns (firms' ability to set wages in the labor market) are both components of what is commonly referred to as "Market Power", as demonstrated Tortarolo and Zarate (2020), Ferreira (2021) and Alpanda and Zubairy (2021). They cannot be disentangled without modeling assumptions, and competition indices on both sides (product and factor markets) can be considered a good proxy for the general market power notion. However, in most countries, particularly in Brazil, a comprehensive approach to market power, including the factor/labor side and its implications for labor market outcomes (wages, inequality), remains largely absent among academics and practitioners. As an exception, we can mention some recent works, such as those of Felix (2021), and Guanziroli (2021), as well as a few articles in the specialized press. Therefore, the main goal of this chapter is to help fill in some of the gaps in research about the Brazilian labor market and give evidence that can help guide the country's antitrust policy.

With this agnostic approach that looks at the labor side as a source or proxy for market power, our work first shows how the concentration of the local labor market (at the Brazilian municipality level) has changed over time. For this purpose, we use a rich formal labor data source from Brazil, the RAIS (Annual Social Information Report). It is an administrative database with records of each formal employment relationship, matching employees and employers in a very disaggregated way (at least at the municipality level). The data allows us to calculate the level of local labor market concentration — the employment-weighted municipality's average of the Herfindahl-Hirschman Index (HHI) from each industry.

The results of this first step show that, in general, there was a decrease in the level of concentration and market power in Brazilian local labor markets between 2000 and 2018. Although a significant part of the territory is still affected by high levels of market concentration (HHI close to 10,000), the distribution of HHI in 2018 had a more uniform profile, with less mass at the extremely high values. Both extensive and intensive changes characterized this reduction in market power. There were decreasing patterns in the distribution mode for the average HHI — due to the increase in the number of establishments in each industry — and for the share of those industries with the highest concentration levels (top 10% HHI).

In a second step, we estimated the impacts of variations in market concentration (employment HHI) on levels of local income inequality and average wages. As the model developed by Ferreira (2021) predicts, market power tends to have a non-linear regressive effect on economic inequality. Wage losses from firm market power, along with unequal distribution of pure profits and labor elasticities heterogeneity, are relevant mechanisms — either through a reduction in the proportion of the marginal product received by workers (wage markdown) or through a lower employment level caused by the markup. The model predictions were empirically tested by regressing the log of inequality levels (municipal Gini indices) first, and in a second moment, the log of average wages (in each labor market, municipality-industry pair) on the respective market concentration indices, control variables, and a set of fixed effects.

Identifying "causal" effects of the HHI on inequality and wages requires more than controlling for unobserved heterogeneity (by fixed effects). They are equilibrium outcomes. So there are endogeneity/simultaneity problems between inequality (Gini indices), wages, and market structure (HHI) (in practical terms, regression errors are correlated with regressors). Therefore, this chapter's main specifications took an instrumental variables approach as a complementary empirical strategy — quasi-exogenous variations are obtained with an HHI leave-one-out mean between municipalities, in the Gini regression, and with a Bartik instrument, in the case of wages.

Our results indicate a robust and economically relevant association between inequality, wages, and market structure/market power. Instrumental variable specification found a log-log coefficient (HHI local elasticity of Gini) of 0.036, which means that a municipality in the high concentration group (HHI above 7,500) has a level of inequality approximately 3% higher than municipalities with moderate HHI (2,500). The theory states that wage variation is one of the mechanisms causing this relationship between inequality and market concentration. In our preferred specification, wage-HHI elasticity is about -0.1. Both regressions are robust to different sample manipulations and changes in regression models.

Although we have found a robust relationship between labor market concentration, wages, and inequality, interpreting this evidence as causal is still controversial in the Industrial Organization literature. pointed out by Berry et al. (2019), many factors may impact both concentration and market outcomes, and our set of fixed effects and instrumental variables may not rule out all the sources of bias in the regression models. That is why this chapter has a third step, where causality between concentration and wages is established through a quasi-experimental research design.

Coping with the tradition of the empirical industrial organization literature, we study a specific market, the banking market, and assess the impact of a merger operation on workers' wages. Our empirical strategy (differences-in-differences - DiD - approach) allows us to consider the merger of two Brazilian banks as an exogenous shock on affected local markets, and, therefore, obtain treatment effects estimates.

Our results reveal that, after the M&A, the higher market power in the labor and product markets had a negative effect on the merging firms' mean wages. At the merging firms level, the wage reduction in the treated group ranged from 2% to 3% until four years before the transaction. These values are relatively consistent when controlling for composition effects and heterogeneity among control and treated municipalities. On the other hand, the merging operation does not have an overall sensitive impact on the local banking sector level, except in places with a high concentration level prior to the transaction.

Our work is related to several others concerned with the repercussions of labor market power. Aside from the previously mentioned papers, it is worth noting Arnold (2019) and Prager and Schmitt (2021), which used DiD approaches in the merger context; Rinz (2020) and Abel et al. (2018), which studied local concentration in the US and UK labor markets, respectively; and, finally, Naidu et al. (2018), and Marinescu and Hovenkamp (2019), which discussed how antitrust authorities should deal with anti-competitive labor market conduct. Nevertheless, this work contributes to several antitrust/monopsony literature dimensions.

This research conducted an in-depth investigation of the labor market power in a developing country. As far as we know, our work is the first to evaluate the extension of local concentration in Brazilian formal labor markets and to illustrate its evolution over the last decades. From 2000 to 2018, the Brazilian economy experienced a volatile macroeconomic scenario, with an economic boom between 2006 and 2014 followed by a burst. The market power analysis reveals a markedly anti-cyclical movement during the boom and a moderate one during the crisis.

Additionally, we went through the effects of market concentration on wages and, further, tried to measure the magnitude of the HHI variation's influence on income inequalities. As in Rinz (2020) and others, the literature usually analyzes only aspects related to wage inequality. When accounting for the whole income distribution (with the Gini index), our estimates also brought in the impacts of pure profits generated from firms' market power. From a methodological point of view, our article has another peculiarity. Following Rodríguez-Castelán et al. (2020), its identification strategy explores instrumental variables of the type Bartik/shift-share. This kind of instrument uses as a source of exogenous variations (with respect to local markets) changes in the HHI that are caused by national trends in a certain industry.

Finally, the last part of this research raises concerns about antitrust policy's impact on labor markets. Investigations assessing the effects of antitrust policy on the labor market are still scarce. This is our motivation to take a closer look at a merger process released with restrictions by the Brazilian competition authority. The existence of a merger control agreement indicates relevant competition concerns. To what extent do these concerns extend to the labor market? Our empirical investigation can provide some answers to improve the merger review process.

The rest of the chapter proceeds as follows. Section 2 presents theoretical insights that motivate our research. Next, we show some data aspects and Brazilian local labor market concentration evolution. Section 4 presents empirical strategy and estimates of the concentration effect on inequality and wages. Section 5 presents the empirical framework, data, and findings from our differences-in-differences estimation. Then, section 6 concludes.

3.2 Theoretical motivation

We begin this section by recovering De Loecker et al. (2020) derivation of firms' markup from the cost minimization problem. This approach has been broadly adopted by recent literature about the effects of market power. An optimizing firm chooses its variable input quantity (labor, material) V_i, $i = 1, \ldots, N$ to minimize

$$\mathrm{C} = \sum_{i=1,\ldots,N} (W_i \cdot V_i) \tag{3.1}$$

subject to a production technology:

$$\bar{Y} = F(V_1, V_2, \ldots, V_N) \tag{3.2}$$

W_i is the factor price (wage), V_i is the variable input (labor, in our case), Y is output, and $F(...)$ the production function. λ is the Lagrange multiplier, and so the first-order condition for V_i can be written as:

$$W_i = \lambda \cdot \frac{\partial Y}{\partial V_i} \tag{3.3}$$

Given the Envelope Theorem, the Lagrange multiplier λ should be seen as the marginal cost. Using the fact that marginal cost can be expressed as the ratio between prices and markups ($\lambda = \frac{P}{u}$), we can derive a simple formula for the markup:

$$u = \frac{\alpha_i}{S_{V_i}} \tag{3.4}$$

Where S_{V_i} is V_i's factor share of revenue, while α_i is the elasticity of output with respect to V_i. Given its relative simplicity, this ratio estimator has been largely used in the literature (IO, Macro, and Trade) to estimate aggregated or sectoral markups. The researcher "only" needs to estimate the production function, recover the factor elasticity, and compute the factor share from the data.

However, a relevant detail is sometimes ignored. This approach has a fundamental assumption of perfect competition in factor markets. If imperfections give buying power to firms, the minimization problem requires a new formulation, and the markup cannot be disentangled from the factor/wage markdown. As Tortarolo and Zarate (2020) highlighted, the first-order condition of this problem with respect to any variable input is now:

$$W_i \left(1 + \frac{1}{\epsilon_i}\right) = \lambda \cdot \frac{\partial Y}{\partial V_i} \tag{3.5}$$

Where ϵ_i is the elasticity of factor/labor supply and the term between parentheses is the inverse of the markdown, md_i. So, the initial markup formula now represents a market power index, given by the ratio between markup and markdown:

$$mkp = \frac{u}{md_i} = \frac{\alpha_i}{S_{V_i}} \tag{3.6}$$

.

This result shows the importance of factor-side imperfections as a source of market power. Therefore, labor market power is also a channel to consider when researchers evaluate market power's consequences on economic efficiency and distribution. In several contexts, it is very imprecise to talk only about markups. Comprehensively, it is better to refer to the notion of market power, even when using only one side, product or factor markets, estimate or index.

Our empirical work uses the Herfindahl-Hirschman Index (HHI) of employment, a measure of labor market concentration, as a proxy for market power to study its impacts on inequality and wages. This approach can be theoretically substantiated by recovering the model by Ferreira (2021). It is a dynamic stochastic model with heterogeneous agents. For details of its formulation, we refer the reader to the paper. Here, we consider only the supply side and the firms' problems, which results in an inverse relationship between wages and the number of competitors.

The economy´s final good is produced by perfectly competitive firms, which aggregates the sector-specific goods, $y_t(\iota)$, to the aggregate good, y_t, using a technology as below:

$$y_t = \left(\int_0^1 y_t(\iota)^{\frac{\phi-1}{\phi}} d\iota\right)^{\frac{\phi}{\phi-1}} \tag{3.7}$$

The elasticity of substitution between the sectoral goods is ϕ. The final good firms´ optimization problem results in a demand function for sectoral goods:

$$y_t(\iota) = \left(\frac{p_t(\iota)}{p_t}\right)^{-\phi} y_t, \tag{3.8}$$

There is a price index for sector ι ($p_t(\iota)$). The aggregate price (p_t) is given by:

$$p_t = \left(\int_0^1 p_t(\iota)^{1-\phi} d\iota\right)^{\frac{1}{1-\phi}}. \tag{3.9}$$

The sectoral goods, $y_t(\iota)$, by its time, are produced by the transformation of firm-specific goods, $y_t(\iota; j)$:

$$y_t(\iota) = N_t^{-\frac{1}{\tau-1}} \left(\sum_{j=1}^{N_t} y_t(\iota, j)^{\frac{\tau-1}{\tau}}\right)^{\frac{\tau}{\tau-1}} \tag{3.10}$$

The parameter τ is the elasticity of substitution between goods. With this CES function, the demand curve for firms' goods assumes a formulation as follows:

$$y_t(\iota, j) = \left(\frac{p_t(\iota, j)}{p_t(\iota)}\right)^{-\tau} \frac{y_t(\iota)}{N_t} \tag{3.11}$$

And $p_(\iota)$ is a price index as:

$$p_t(\iota) = N_t^{\frac{1}{\tau-1}} \left(\sum_{j=1}^{N_t} p_t(\iota, j)^{1-\tau}\right)^{\frac{1}{1-\tau}} \tag{3.12}$$

Finally, including 3.8 into 3.11, we obtain the demand function for each firm j in each sector ι:

$$y_t(\iota, j) = \left(\frac{p_t(\iota, j)}{p_t(\iota)}\right)^{-\tau} \left(\frac{p_t(\iota)}{p_t}\right)^{-\phi} \frac{y_t}{N_t} \tag{3.13}$$

From the demand function formulation, the price residual elasticity faced by every firm is a function of the number of competitors within the sector. Taking the symmetry assumption, the elasticity assumes the following specification:

$$\eta_{y(\iota,j)p(j,\iota)}\left(N_t\right) = \tau - \frac{1}{N_t}\left(\tau - \phi\right) \tag{3.14}$$

In the labor market, the homogeneous labor supplied by each type of household is differentiated by competitive intermediaries-suppliers into the sector and firm labor services. The supply curve faced by firms is derived in a very similar way from above. First, the homogeneous labor for each household is desegregated into sectoral labor $l_t^i(\iota)$, following a CES function:

$$L_t^i = \left[\int_0^1 L_t^i(\iota)^{\frac{\phi_w^i+1}{\phi_w^i}}\, d\iota\right]^{\frac{\phi_w^i}{\phi_w^i+1}} \tag{3.15}$$

In this case, the elasticity of substitution, ϕ_w^i, is specific to the household. Once again, there is a second disaggregation, to firm-household-specific labor, $L_t^i(\iota, j)$:

$$L_t^i(\iota) = N_t^{\frac{1}{\tau_w^i+1}}\left(\sum_{j=1}^{N_t} L_t^i(\iota, j)^{\frac{\tau_w^i+1}{\tau_w^i}}\right)^{\frac{\tau_w^i}{\tau_w^i+1}} \tag{3.16}$$

As above, the elasticity of substitution, τ_w^i, is specific for the type of household i. Finally, combining firm and sector functions for each household, we get a supply function faced by the firm as:

$$L_t^i(\iota, j) = \left(\frac{w_t^i(\iota, j)}{w_t^i(\iota)}\right)^{\tau_t^i}\left(\frac{w_t(\iota)}{w_t}\right)^{\phi_w^i}\frac{L_t^i}{N_t} \tag{3.17}$$

With specific wage elasticities for each type of household, i, that reads:

$$\eta_{L(\iota,j)w(j,\iota)}^i\left(N_t\right) = \tau_w^i - \frac{1}{N_t}\left(\tau_w^i - \phi_w^i\right) \tag{3.18}$$

In each sector, there are N intermediate firms with market power on product and labor sides. For tractability, we imposed that both sides have the same number of firms. The firm j in sector ι faces a Cobb–Douglas production function, and chooses price, output, wages, labor, and capital, aiming to maximize profits, given by:

$$\pi_t(\iota, j) = p_t(\iota, j) y_t(\iota, j) - \textstyle\sum^i w_t^i(\iota, j) l_t^i(\iota, j) - r_t^k k_{t-1}(\iota, j) \tag{3.19}$$

The optimization problem above is subject to demand and supply functions and sectoral/aggregate wages indexes. Imposing symmetry, and considering that firms take as given the decisions of their competitors (treating their rivals as parameters), the first-order conditions for this problem bring the markup, $\mu_t(N_t)$:

$$\mu_t(N_t) = \frac{\tau - \frac{1}{N_t}\,(\tau - \phi)}{\tau - \frac{1}{N_t}\,(\tau - \phi) - 1} \tag{3.20}$$

Wage markdowns for each type of household i:

$$md_t^i(N_t) = \frac{\tau_w^i - \frac{1}{N_t}\,(\tau_w^i - \phi_w^i)}{\tau_w^i - \frac{1}{N_t}\,(\tau_w^i - \phi_w^i) + 1} \tag{3.21}$$

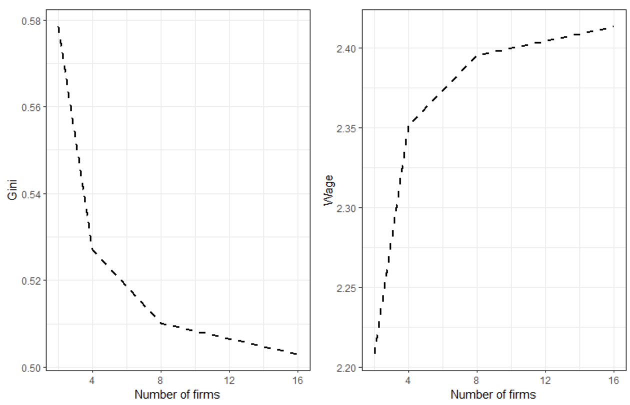
And wages levels:

$$w^i{}_t = md^i{}_t \, \frac{y_t \, \alpha^i}{l^i{}_t} \, \frac{1}{\mu_t} \tag{3.22}$$

Both markup and markdown depend on the number of firms. With symmetric firms, is possible to show that $HHI = \frac{1}{N} \times 10,000$. Since firms' market power and profits are a combination of the effects of markdowns and markups (as highlighted earlier), we established a direct link between the concentration index (HHI), wages, and inequality levels. The expected behavior of inequality and wages can be checked in 3.1, where we plotted the results from the model's deterministic steady states, changing only the number of competitors while keeping constant the same parametrization as in Ferreira (2021).

Therefore, it is possible to observe that the model's specification results in a downward-sloped relationship between the number of competitors (more competitors, lower HHI/concentration) and income inequality measured by the Gini index. Conversely, tightening competition results in higher wages. The model also predicts that the relationships between market concentration, inequality, and wages are non-linear. The effects of concentration are more pronounced when there is already a high level of market power. When the number of competitors tends to infinity, the impact approaches zero.

Figure 3.1: Model simulation - Gini index and wages



Source: Author´s computations

In figure 3.1, only the number of competitors changed, keeping other economy's outputs, in particular the levels of productivity, constant, thereby obtaining pretty clean paths to wages and inequality. There are good reasons to believe that several shocks hit, simultaneously, the competition in the market and the level of labor's marginal productivity ($\frac{y_t \alpha^i}{l_t^i}$), creating a confounding factor for the HHI-inequality-wages relationship. This is undoubtedly one of the sources of bias/endogeneity in the empirical models we deal with and motivates our instrumental variable strategies and our quasi-experimental model, outlined in the following sections.

3.3 Data and labor market concentration

We begin this section with a brief description of the data and some concepts used throughout the chapter. First, our preferred definition of the local labor market is the municipality-industry pair. However, the level of industry aggregation may vary in some sections due to data limitations and changes in the CNAE (National Classification of Economic Activities, Brazilian official sectoral classification) levels and versions adopted by the RAIS database. Whenever it was possible, we took the CNAE subclass level (six digits) at its most recent version (2.0) as our reference.

That was not the case with our more extended data series, used to describe concentration evolution in the labor market (from 2000 to 2018) and estimate inequality models. The industry classification adopted to harmonize the series was the more aggregated and older version of CNAE (1.0 classes/five digits). Initially, CNAE classes were the only desegregation level present in the RAIS database. Six-digit codes (subclasses) were made available only from 2006 onwards. Additionally, the transition to version 2.0 of CNAE took place in the same year. CNAE 2.0 breaks the previous industries into more sectors; therefore, a different, backward, technique would artificially increase the HHI in recent years, even maintaining the same five-digit level (the greater number of sectors results in "smaller" and, consequently, more concentrated markets).

There is a second relevant issue with the "local labor market" definition. The municipal level may not be comprehensive enough to include all worker mobility patterns. Likewise, the industry may not be the crucial element for substitutability among firms. Most papers with US data use commuting zones (CZ) or core-based statistical areas (CBSA) as a geographic basis (Rinz, 2020; Arnold, 2019). Also, some benchmark papers consider the location-occupation pair as another way of describing a local labor market (Lipsius, 2018; Qiu and Sojourner, 2019; Azar et al., 2020). The concept closest to CZ in Brazil is the microregions defined by the national statistical office (IBGE) and adopted by Felix (2021) in her work. Additionally, the RAIS database has information about workers' occupations (CBO codes). Because of these limitations in our definition of the local market (municipality and industry pairs), we used two different ways to think about the labor market in the robustness test for the wage model: microregion-industry pairs and municipality-occupation pairs.

As we stated before, our primary data source is the RAIS (Annual Social Information Report) database. It is an administrative register, mandatory for firms, and used to pay social benefits for formal workers. There is, therefore, a strong incentive for filling in the information correctly. The database started in the mid-1980s, but we opted for a narrower time frame (from 2000 or 2006) due to changes in the variables over the years. Within RAIS, it is possible to retrieve, through the unique identification, in addition to all the worker's links over the years (hiring and termination date for each firm, monthly and average salary, occupation), social data such as age, gender, and level of education. There are also specific data from firms (at holding or establishment level), such as tax identification number, sector and sub-sector of activity, location (municipality and state), in addition to the number of establishments' employees.

With data for each establishment (the number of employees, location, and industry), we calculated the specific Herfindahl-Hirschman Index (HHI) of employment for all "local labor markets" and, through weighted aggregation, its version at the municipality level. As usual, the markets' HHI was obtained by the sum of the squared share (s) of each establishment's employment, following the equation below:

$$HHI_{i,r,t} = (s_1^2 + s_2^2 + s_3^2 + \dots s_n^2) \times 10,000 \tag{3.23}$$

Where i is the sector (CNAE class or subclass), r is the region (in our benchmark specification, the municipality), and n is the number of firms. Therefore, the pair ir is the local labor market. This HHI is the variable of interest on which we regressed average wages in the local labor market. Instead, for the models in which we analyze intra-municipal inequality, measured by the Gini index, the regressor was its weighted and aggregated version that reads:

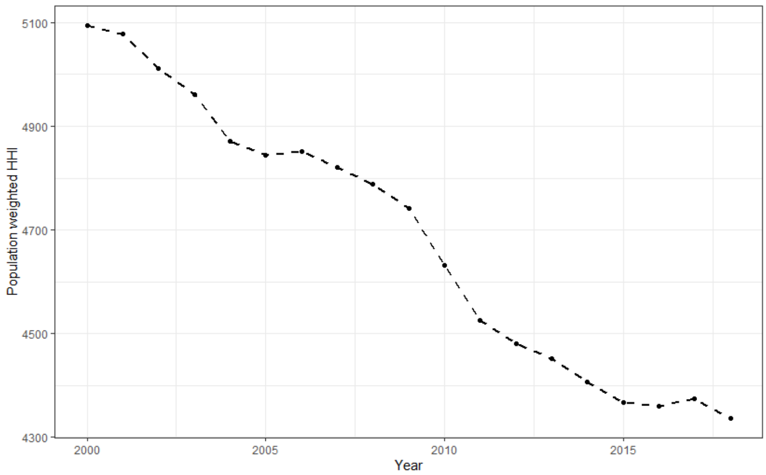
$$HHI_{r,t} = \sum_i Weight_{i,r,t} \times HHI_{i,r,t} \tag{3.24}$$

The $Weight_{i,r,t}$ is calculated by the ratio between the local industry and total municipality employment. From this local HHI, we could also obtain the “national” index of local market concentration by aggregating once again, this time by population weights.

$$HHI_t = \sum_r \frac{Population_{r,t}}{Nat.Population_t} \times HHI_{r,t} \tag{3.25}$$

The last aggregation results are plotted in figure 3.2. The panel shows the evolution of local employment concentration from 2000 to 2018. Between the second half of the 2000s and the first of the 2010s, the Brazilian economy was hit by a positive shock caused, among other reasons, by the commodities boom. However, the economy slowed down in 2014, and, from 2015 onward, the country faced a severe recession. In the literature, there are authors, like Lambrecht (2004), who point to a positive correlation between economic expansion and merger and acquisition activity, which would ultimately lead to an increase in market concentration during economic booms. Instead, considering the stylized facts in 3.2, the concentration trend in the Brazilian local labor market seems to correspond more to the behavior predicted in macroeconomic models outlined by Jaimovich and Floetotto (2008); Etro and Colciago (2010); Ferreira (2021), which predict an inverse relationship between growth and market concentration. Until 2015, the concentration trend was firmly downward during the Brazilian economic boom. In the following years, there was a reversal, although the upward movement was moderate.

Figure 3.2: HHI evolution - Brazilian Municipalities, 2000-2018



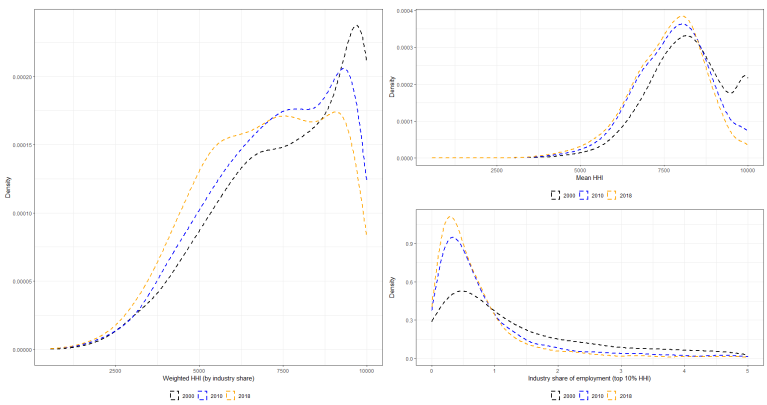
Source: Author's computations (RAIS)

The territorial pattern of labor market concentration and the municipal HHI density can be seen in figure B.1 (appendix B) and in the left panel of 3.3, respectively. The two ways of viewing the same data reinforce the finding that the labor market in Brazilian municipalities has become less concentrated in recent decades. At the end of the series, in 2018, we see fewer red spots on the Brazilian municipalities map, indicating that several cities moved from a very high level of market concentration (HHI between 7500 and 10000) to a high (between 5000 and 7500) or moderate concentration (2500 and 5000). In other words, when compared to 2000, the 2018 municipal HHI has a much more uniform profile in the middle to the right of the distribution in Figure 4, with less mass at extreme values.

Despite deconcentration in the labor market, it is still possible to find regions, especially in Brazil's North and Northeast, where the HHI reaches very high values, especially in smaller locations. Some are conceptually considered in a perfect monopsony condition (HHI 10000), where the only formal employer is the municipal administration. As public employment has a different rationale than the private sector, we left municipalities with only public employment out of the benchmark inequality regression model.

Our municipality's concentration index was obtained from the sum of local industries' HHI, weighted by their shares of total employment. So, changes in the concentration distribution can be caused by extensive variation within industries, i.e., a reduction or elevation in the economy/municipality's mean HHI, or by between variation, with increasing or decreasing patterns in the shares of the most concentrated industries.

Figure 3.3: Densities-HHI evolution - Brazilian Municipalities, 2000-2018

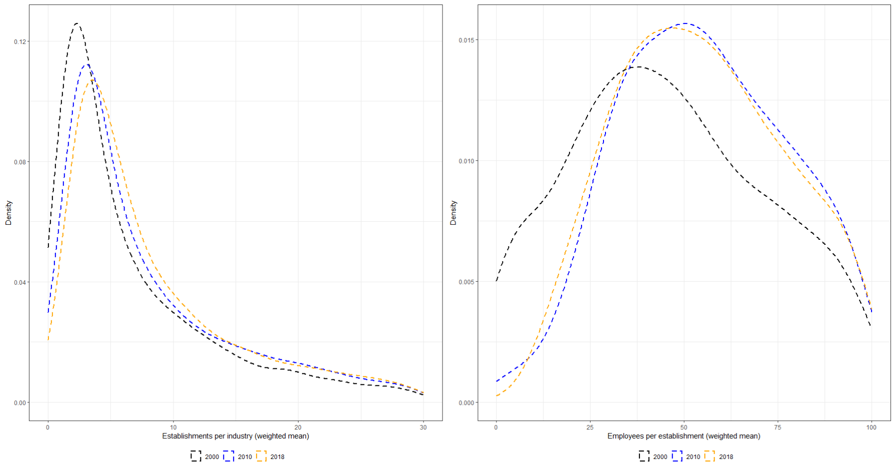


Source: Author's computations (RAIS)

Inspecting figure 3.3, it is worth noticing that the deconcentration trend in local labor markets is due to both types of variation. The panels on the right represent, respectively, the distribution of the average HHI from municipalities' industries and the employment share of those 10% with the highest level of concentration. From 2000 to 2018, we observed a reduction in modal values from both distributions, with tails on the right with less weight. Going further, it is possible to investigate why there was a general reduction in the average HHI of local economic sectors. This question is addressed in figure 3.4.

The sectoral HHI at the municipality level is calculated as the sum of the employment shares of each firm. Therefore, it is influenced by the number of employees in the establishment and the number of competitors operating in a given sector and location. The left panel shows the distribution of the average number of firms in local industries (weighted by sector importance in total municipal employment). The second panel, on the right, shows the distribution of the average number of employees per firm among Brazilian cities.

Figure 3.4: Number of establishments and employees - Brazilian Municipalities, 2000-2018



Source: Author's computations (RAIS)

Brazil experienced accelerated growth in the period under analysis. Consequently, there was an intensive labor market expansion (increasing the average employment level per firm). On the other hand, the same period was marked by a significant entry movement, illustrated by the rise in the modal values from the number of firms' distribution. If there wasn't such extensive growth in entry, which offset the increase in incumbent firms' employment, there would be a greater market concentration due to the economic boom.

Also, between 2000 and 2018, the country witnessed a cycle of advances and setbacks concerning income inequality and workers' wage level. How can these phenomena be partially explained by the market concentration effect? This evidence about this question will be revealed in the next session, in which we outline an empirical strategy for estimating the impacts of HHI variation on inequality (measured by the Gini index) and average sectoral wages.

3.4 Concentration, inequality, and wages

3.4.1 Concentration and inequality

Our first model is a reduced-form fixed-effect regression of inequality (Gini index) on aggregated HHI (and a vector of control variables) at the municipality level. There is a challenge in estimating the causal impact of HHI on inequality: local unobserved heterogeneity may be correlated with the market structure and, at the same time, with local income distribution. As we can see in the map in B.1 (appendix B), most areas with higher levels of HHI are also regions where the levels of inequality and poverty are higher (Brazilian North and Northeast regions). That is why pure correlations or simple regression coefficients can not be considered causation between Gini and HHI. This problem is addressed by the fixed effects approach in our model (region fixed effect). There are also concerns about time socks, which may bias our estimates, and to deal with it, we included the time fixed effect (Two-Way Fixed Effect - TWFE). Our baseline model equation is shown below:

$$Log(Gini)_{r,t} = \beta_1 Log(HHI)_{r,t} + \sum^{q} \beta_q X_{q,r,t} + Region_r + Year_t + \varepsilon_{r,t} \tag{3.26}$$

where r=1,...,N and t= 1,..., T are indexing regions (municipalities, in baseline regressions) and time, respectively. $Log(Gini)_{r,t}$ is the log of municipality level Gini index at time t, and $Log(HHI)_{r,t}$ is the log of aggregated labor market concentration in municipality r in time t. $\sum^{q} \beta_q X_{q,r,t}$ is the vector of time-varying region characteristics. Finally, $Region_r$ and $Year_t$, are (unobserved) region and time fixed effects. Our coefficient of interest is thus β_1, which, given the log-log specification, can be thought of as Gini's elasticity with respect to HHI.

As stated before, our market concentration data were obtained from the RAIS database. The remaining variables (Gini index and controls) were retrieved from the Brazilian population census, conducted by the national statistical office (IBGE). Although we have a very long RAIS series, with data on the labor market since the 1980s, the Gini and other variables at the municipal level are only statistically relevant with the decennial census microdata. For this reason, our inequality model is a panel with observations at only two points in time (2000 and 2010). There are 11,025 observations from 5564 Brazilian municipalities. Despite being a reasonably balanced panel, some municipalities were not present in the series's first year. The 2000 and 2010 models' variables' descriptive statistics are reported in table B.1 (appendix B).

The concept of local labor market and characteristics of the RAIS database, which catches only formal market and public employment, justify including some of the time-varying variables like the proportion of formal work and public employment, the unemployment rate, and level of income per capita. Others, such as level of literacy and rural population, are municipal social characteristics that may not be captured by the fixed effects and could impact the distribution of income and the market structure.

The validity of our fixed effects estimates relies on the assumption that omitted municipality characteristics or time shocks correlated with both inequality and labor market concentration are the only sources of bias. Still, there are remaining concerns about endogeneity because the market structure and income distribution are outcomes of an equilibrium. Therefore, there is a possibility of double causation or simultaneity. Higher inequality levels may favor market entry barriers, for example, and cause market concentration. To address these concerns about simultaneity or remaining omitted variable bias (as the earlier model in Ferreira (2021), points out, shocks that hit, at the same time, marginal product/revenue and the number of competitors can affect the relationship between Gini-HHI), we looked for an HHI quasi-exogenous source of variation that could be adopted as an instrumental variable in our regression model.

In our model, it was employed an *leave-one-out* concentration mean instrument. Similar to Rinz (2020) and Azar et al. (2017), we computed the HHI for each labor market (industry-municipality), in each year, using the average HHI across the other municipalities within the same industry in the same year, as summarized by the equation below:

$$HHI_{i,r,t}^{-m} = \frac{\sum_{r \neq m} HHI_{i,r,t}}{N-1} \tag{3.27}$$

Where m is the specific municipality for which we calculate the HHI; as before, r is the index for municipalities, i for industries, and t for time. Then the instrumented municipality's HHI is obtained by aggregating with industry weights as in equation 3.24. Theoretically, our strategy identifies the effects of HHI on local inequality by using only variation that is locally caused by changes in the national concentration, with the *leave-one-out* concentration means as a proxy of the national variation. As shown next (in the first stage F-test), this instrument is strong enough for our modeling proposes and indicates that, if our assumptions are reasonable, the HHI coefficient is downward biased in non-instrumented regressions.

Table 3.1: Effect of market concentration on Income Inequality

Dependent Variable:	log(Gini)				
Model:	OLS	OW FE	OW FE	TW FE	IV
log(HHI)	0.029***	0.131***	0.022**	0.019**	0.036**
	(0.005)	(0.013)	(0.010)	(0.009)	(0.017)
Controls	No	No	Yes	Yes	Yes
Municipality FE	No	Yes	Yes	Yes	Yes
Year FE	No	No	No	Yes	Yes
IV	No	No	No	No	Yes
Standard-Errors:	Clustered by municipality (id)				
Observations	9,988	9,988	9,988	9,988	9,988
Kleibergen-Paap Wald test (1st stage)	-	-	-	-	654.0

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The benchmark regression models were estimated with a reduced sample, where the top and bottom 1% municipalities in population terms and those cities considered to be in a perfect monopsony situation were excluded. This procedure is justified to avoid results driven by population outliers or municipalities in which labor market variations are determined outside the private sector (where the only formal employer is the government). In any case, these choice impacts were tested in the following robustness checks.

The results from local Gini index regressions on employment HHI are reported in 3.1 (complete regressions tables and IV first stages are in B.2 and B.3), where five models are tabulated. The first four do not adopt the instrumental variable and range from the ordinary least squares, without controls, to the full two-way model with fixed effects for unobserved heterogeneity in municipalities and time shocks. The final specification is our preferred one, in which HHI was instrumented using the leave-one-out method.

The HHI elasticities of the Gini index are between 0.131 and 0.019 and are all statistically significant. Therefore, their signals align with the predictions made by the model that theoretically support our work. We must, however, be careful not to take all these results as a causal effect. Most are reported only as a way of checking the consistency of estimates. The only specification that, given our assumptions, might have some flavor of causality is the last one, with an instrumental variable, which indicates an elasticity of 0.036 (statically significant at 0.05, and the first stage's F statistics indicates a strong instrument).

Table 3.2: Robustness - Effect of HHI on Inequality

Dependent Variable:	log(Inequality)				
Model:	(1)	(2)	(3)	(4)	(5)
log(HHI)	0.071***	0.052***	0.043**	0.119***	0.050**
	(0.023)	(0.019)	(0.021)	(0.038)	(0.022)
Index	gini	gini	gini	theil	top 10% share
Sample	full	non-monopsony	nonoutlier	benchmark	benchmark
Standard-Errors:	Clustered by municipality (id)				
Observations	11,025	9,816	10,609	9,458	9,458
Kleibergen-Paap Wald test	332.3	625.7	329.5	654.0	654.0

All specifications include two-way fixed effect and instrumented variables
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

This elasticity roughly means that a municipality in the high concentration group (HHI above 7,500) has a level of inequality approximately 3% higher than municipalities with moderate HHI (2,500). Nevertheless, what is the economic significance of this finding? A 3% change in the Gini equals the accumulated drop in Brazilian inequality between 2012 and 2015. Before the economic crisis, the index went from 54.1, in 2012, to 52.5, in 2015 (Barbosa et al., 2020). We are not suggesting that labor market concentration variations were the main determinants of the decline in inequality. Instead, these results emphasize some potential for policies that consider market concentration as one of the mechanisms of inequalities. Additionally, it should be taken into account that the Gini calculated with population/survey data tends to underestimate the top incomes, particularly those with a larger share of rents from the firms' profits (Morgan, 2017). Due to the lack of data, one can hardly speculate that, in some ways, this may have weakened the relationship between concentration and inequality.

The robustness of our original estimates was tested using five checking procedures. They are shown in table 3.2. The first three are changes in the regression samples. The first sample is the complete and original one; the second is the benchmark sample but recovering population outliers. The third uses the benchmark sample, adding the municipalities in perfect monopsony (HHI 10,000). The last two tests adopt other inequality indices as dependent variables: the Thail index and the top 10% income share. Qualitatively, the robustness test regressions confirm the benchmark model's findings, giving us confidence about the association between inequality and market power. The HHI elasticities of inequality are always positive and statistically significant. The effect magnitudes vary, instead, being stronger when the dependent variable is the Theil index or when the sample includes all observations.

Up to this point, it has been shown that the Brazilian labor market concentration trended downward in the last two decades, except for the recent economic crisis years. Also, our regression models above reported a positive association between income inequality and employment HHI (market concentration index). This association was robust to different sample manipulations and inequality measures and to our instrumental variable strategy (local HHI instrumented by quasi-exogenous shocks sourced from national HHI changes).

Indeed, the instrumental variable model indicates that the standard fixed-effects regression has a negative bias, reducing the coefficient on the HHI index. The evidence, therefore, points to a recent progressive effect of the market structure on the Brazilian income distribution. The next section adopted estimates that follow similar principles to verify the effects of concentration on workers' wages — one of the possible mechanisms driving the inequality/market structure relationship as suggested by the theoretical models presented earlier.

3.4.2 Concentration and wages

Our second model follows an empirical strategy very similar to the previous one to assess the elasticity of wages with respect to market concentration. However, some fundamental details have changed because data are now collected at the local labor market level (the industry-municipality pair in the preferred specification). Our reduced-form regression with fixed effects adopts a new specification that reads:

$$\begin{aligned} \text{Log(Wage)}_{i,r,t} \quad &= \quad \beta_1 Log(HHI)_{i,r,t} \; + \; \sum\nolimits^q \beta_q X_{q,i,r,t} \\ &+ \text{LaborMarket}_{ir} + Region_r + Industry_i + Year_t + \varepsilon_{i,r,t} \end{aligned} \tag{3.28}$$

Its components are indexed by i for industry, r for region (municipality), and t for time. $Log(Wage)_{i,r,t}$ is the log of local labor market (industry-municipality) mean wage in a given year. $Log(HHI)_{i,r,t}$ is, consequently, the industry-municipality employment concentration index (and our coefficient of interest is also β_1, the HHI elasticity of sector mean wage), while $\sum^q \beta_q X_{q,i,r,t}$ is a vector of control variables (mean values for tenure, gender proportion, education, age; and municipality population). Finally, there is now a broader set of fixed effects controlling for unobserved heterogeneity: $LaborMarket_{ir}$, $Region_r$, $Industry_i$, and $Year_t$. The labor market fixed effect is the interaction between industry and location.

Once again, employment concentration levels were obtained from RAIS, computing the sectoral HHI index. However, the use of data from 2006 onward allowed us to consider a narrower industry classification level, the CNAE's subclasses (six digits). There is no census information for each labor market, so we rely on RAIS to obtain the remaining variables (mean wages and controls). This is, somehow, beneficial to our analysis since we could use a longer and more recent series.

The data spans from 2006 to 2018, forming a panel with 13 time points (12 in the Bartik instrument model). In total, our complete sample has 7,004,694 observations, resulting from yearly data combining about 1,300 sectors and 5,600 locations. Not all municipalities have observations for all industries (the mean number of industries per municipality is 96.79), as, despite being a reasonably balanced panel, not all local labor markets (industry-municipality) are observed in the 13 years of the series. Descriptive statistics for all data series are reported in table B.4 (appendix B).

As stated before, the validity of fixed effects estimates assumes that omitted heterogeneity is the only source of bias. This assumption is probably wrong in our current model because the endogeneity concerns (originated by double causation and omitted variables, given that wages and HHI are market equilibrium outcomes) are even more relevant than previously. So, once again, our preferred estimation procedure adopts a quasi-exogenous source of variation to instrument the local employment HHI. As in Rodríguez-Castelán et al. (2020), our analysis relies on a Bartik (2002) instrument strategy. The local industry HHI for employment in a municipality is instrumented by national changes in the concentration of the same sector. That is, the sector i and municipality r specific HHI instrument is defined as below:

$$IV_{HHI_{i,r,t}} = HHI_{i,r,t=2006} \times g_{i,t} \tag{3.29}$$

Therefore, our instrument is computed through the interaction between two pieces of data: $HHI_{i,r,t=2006}$, which is the concentration in labor market ir (sector-municipality pair) in the first year of the RAIS series (2006); and $g_{i,t}$, the concentration (HHI) growth rate for industry i at national level between 2006 and period t. Using fixed local concentration levels ($HHI_{i,r,t=2006}$), which we can assume as exogenous to the forthcoming years, the instrument rules out specific and unobserved local shocks affecting concentration levels and wages simultaneously (the source of bias). Moreover, the instrument brings back some variation with the supposedly exogenous changes in the local labor market concentration ($g_{i,t}$) driven by national trends (national trade policies, industry-specific incentives, the competitive environment, or the labor market). First stage F tests support the instrument's relevance in predicting local employment HHI.

Our benchmark estimation originates from a nonoutlier sample – removing local markets with zero, top, or bottom 1% mean earnings. At a less aggregated level, this procedure reduces concerns about measurement errors. Differently as before, wage models kept sectors in a perfect monopsony state since it is not unusual to find a local industry with a single operational firm. Moreover, changes from monopsonistic to a more competitive regime (or vice versa) are a valuable source of variation when analyzing the relationship between concentration and earnings. Either way, our model's robustness test measured the impact of these choices.

Table 3.3: Effect of market concentration on wages (mean by CNAE subclass)

Dependent Variable:	log(Wage)				
Model:	OW FE	TW FE	IV	Microrreg. (IV)	Occup. (IV)
log(HHI)	-0.047***	0.000	-0.098***	-0.062***	-0.121***
	(0.0005)	(0.000)	(0.004)	(0.004)	(0.018)
Mun-Ind FE	Yes	Yes	Yes	No	No
Micro-Ind FE	No	No	No	Yes	No
Mun-Ocup FE	No	No	No	No	Yes
Year FE	No	Yes	Yes	Yes	Yes
IV	No	No	Yes	Yes	Yes
Standard-Errors:	Clustered by labor market (id)				
Observations	6,707,076	6,707,076	4,350,929	1,749,515	3,355,276
Kleibergen-Paap Wald test (1st stage)	-	-	10,859.4	9,496.0	1,069.7

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The main results are shown in table 3.3 (complete regressions tables and IV first stages are in B.5 and B.6). Overall, as predicted, there is a negative and statistically significant relationship between concentration and workers' earnings (except for the non-instrumented Two-Way fixed effect regression, which does not reject the null). The result holds and becomes more substantial when estimation relies on the Bartik instrument strategy. In this specification, the preferred one, a reduction of 10% in local labor market concentration leads to an economically and statistically significant increase of approximately 1% in the mean wage. Moreover, the instrumental variables approach indicates that the model which does not control for unobserved local shocks is biased toward zero the true effect of local labor market concentration on earnings. In fact, in the non-instrumented specification with a time-id fixed effect, the coefficient is zero to the third decimal place.

Table 3.4: Robustness - Effect of HHI on wages

Dependent Variable:	log(Wage)			
Model:	(1)	(2)	(3)	(4)
log(HHI)	-0.097***	-0.076***	-0.059***	-0.081***
	(0.005)	(0.005)	(0.005)	(0.005)
Sample	full	non-monopsony	benchmark	benchmark
Standard-Errors:	Clustered by labor market (id)			
Observations	5,084,091	2,645,269	3,402,871	3,402,871
Kleibergen-Paap Wald test (1st stage)	11,518.7	7,735.7	7,448.2	9,134.6

All specifications include two-way fixed effect and instrumented variables
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The positive bias deserves to be explored further. It may be caused by measurement error. However, recovering the model that theoretically underlies this work, it can be speculated that local demand/productivity shocks may play a role, affecting more concentrated sectors, and increasing both the HHI and the wages (through marginal revenue). Unfortunately, there is no firm-level data on production and revenue in the RAIS database. The Brazilian statistical office (IBGE) has this kind of data in its annual sectoral censuses. However, the identified base is confidential, and we could not access it timely [1]. Despite not quantifying the impact of firm productivity on the wage model, it is possible to have confidence in the evidence obtained. As was already said, the main goal of the instrumental variable strategy is to deal with the bias from unobserved local shocks.

The last two columns of table 3.3 present alternative models in which we changed the definition of the local labor market. The first column documents the estimates in which the HHI was calculated at the microregion level (i.e., the labor market considered is the industry-microregion pair). It attempts to use a geographical component closer to what is commonly used in papers with data from the US, the commuting zones (CZ). The second group of estimates changes the sectoral element of the local labor market and considers no longer the industry but the occupation of workers as a reference. Due to the computational burden, the occupation model only relies on a data series from 2015 to 2018.

[1] We formally requested access to the data in January 2020, but the COVID-19 outbreak forced the closure of IBGE's restricted data room.

There are statistically significant numerical differences in both models concerning baseline estimates. Wages are considerably more elastic when the market concentration index considers occupations. In the opposite direction, the estimate for microregions is lower. These variations are in line with expectations as they are computed in smaller and larger markets, respectively. Nevertheless, it is noteworthy that baseline and alternative estimates are qualitatively similar and reaffirm the evidence that market power dampens Brazilian workers' earnings.

In addition to changes in the local labor market concept, our estimates were subjected to four other robustness checks. They are reported in table 3.4. Columns (1) and (2) show the instrumental variable's results on two different samples. The first is the complete sample, recovering outlier observations. In the second, observations from monopsonistic sectors (HHI=10000) were excluded. We changed the regressions specifications in models (3) and (4). (3) has an autoregressive term ($Log(Wage)_{i,r,t-1}$), while in (4) the market structure (HHI) enters in the lagged form ($Log(HHI)_{i,r,t-1}$). Qualitatively, there are no remarkable changes in relation to the preferred regression model.

Finally, our inequality and wages models were tested for non-linearity. It can be seen in table B.7 (appendix B) that the quadratic term is non-significant in all cases, which leads us to conclude that the log-log specification was sufficient to linearize the data. There is, therefore, some divergence to the Ferreira (2021)'s model. The data points out a constant HHI elasticity for the Gini index and wages. In contrast, the theoretical model has a greater degree of non-linearity, with a proportionally more significant increase or decay for inequality and workers' earnings when the number of competitors is lower.

Our empirical strategy found statistically robust evidence about the relationship between market power, inequality, and wages. Nevertheless, before making causal statements, we should consider an open debate about the HHI and its validity as a market power proxy. The HHI is still widely used today, particularly by antitrust authorities, as one of the main ways of measuring market power. The usefulness of the index lies in its simplicity, requiring data only on sales/employment for all firms in a given market. Moreover, there are reasonable theoretical arguments that justify its use, in addition to the model presented in our theoretical motivation (Miller and Sheu, 2021), and it appears to have good predictive power regarding market outcomes, as we see in Autor et al. (2020). Despite this, there are relevant arguments against the direct relationship between the HHI and the firms' market power. A summary of them can be found in Eeckhout (2021). The author asserts that the measured HHI crucially depends on how we define a market and that there is no obvious way to do it. Moreover, like other researchers in Industrial Organization, Eeckhout highlights that concentration is an endogenous outcome with no straightforward instrumental approach.

Aware of the possible limitations of the methodology used so far, the next section will forego the cross-market aggregate analysis, focusing on a specific market, the Brazilian banking market, to assess the effects of a merger and acquisition operation. Adopting a quasi-experimental approach (differences-in-differences - DiD), it is possible, with some assumptions, to treat the merger as an exogenous shock on affected local markets and, therefore, obtain estimates for the impact of growing market power/concentration on workers' earnings.

3.5 Mergers and wages

3.5.1 Institutional setting

This section presents a brief overview of the analyzed merger. Nevertheless, as we relied on restricted data from RAIS, firms' identities and some details will not be disclosed. The transaction involved the merger (through acquisition) of two banking firms with a broad presence in the Brazilian territory. The acquiring bank was one of the major players in the market and held around 20% of the sectoral workforce. On the other hand, the acquired firm had about 5% of total banking industry employment. It is worth noting that the two banks held 75% of the employment share in some smaller cities.

The merger would result in an expected increase of about 200 points in the national employment's HHI ($\Delta HHI = 2 \times s_1 \times s_2$). According to the data from RAIS, this index was close to 1,800 in the year prior merger. Taking as reference the guide for horizontal merger review (CADE, 2016) from the Brazilian antitrust authority (Administrative Council for Economic Defense - CADE), through the lens of the buying power, the banking labor market could be considered moderately concentrated (HHI ranging from 1,500 to 2,500 points). The HHI variation due to the transaction was significant (above 100 points). Concentration indices obtained at the national level somewhat minimize the actual HHI for the local labor markets. If, as in the previous sessions, we compute a sector-municipality level index, it is possible to verify that the Brazilian cities' mean HHI (without population weighting) was, before the merger, 4,800 points. These banking labor market characteristics should have raised significant concerns for the antitrust agency.

However, due to the Brazilian banking sector's competitive scenario, the analysis of the transaction mainly focused on aspects of the retail market. The Brazilian banking sector had low rivalry on the product side, a state deepened by a previous wave of M&A operations that began in the early 2000s. In one decade, the CR4 ratios for deposits and credit markets increased from 51.3% and 52.9% to 70.9% and 71.4%, respectively. The retail HHI rose from 841.5 and 870.9 to 1,386.7 and 1,441.7, according to da Silva (2014).

Given the level of competition in retail banking, CADE approved the acquisition a year after its announcement but imposed an agreement with behavioral remedies. Among other measures, the document prohibited the acquiring bank from carrying out new merger or acquisition transactions in the following years and forced it to implement easy credit portability in cities where concentration levels were high.

The agreement did not deal with labor market concentration issues. As presented in detail below, the impact on local labor markets was quite significant — we found a median ΔHHI of 474, nearly five times the baseline of Cade's horizontal merger guide. The following sections will estimate whether the increased market concentration affected workers' earnings. For this purpose, we will first define an empirical causal inference strategy and then present the results along with a set of robustness checks.

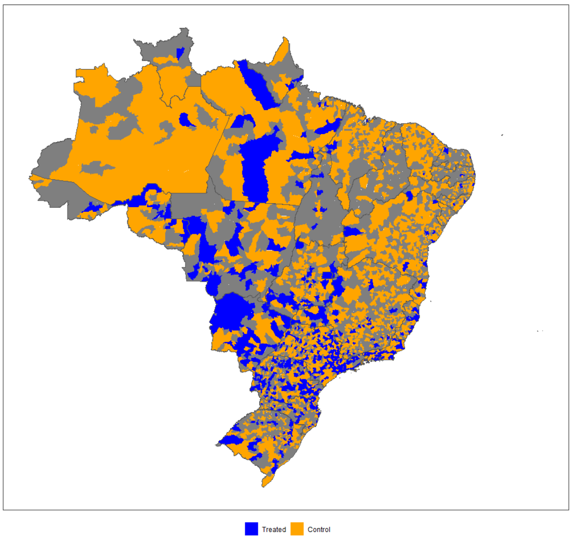
3.5.2 Empirical strategy

Our proposed empirical approach involves estimating the treatment effect of the merger between the two banks through a dynamic differences-in-differences technique (saturated TWFE DiD). This approach takes a set of assumptions about the treated group and its counterfactual (untreated) to obtain a natural quasi-experiment, i.e., a supposed exogenous shock that allows evaluating the effects of the increase in market power in the affected municipalities.

In a cross-section setup, if there is a data set with treated and untreated units, the treatment effect would be, intuitively, obtained by a simple difference-in-means approach. Nevertheless, this naive estimator suffers from selection bias. Treated units possibly differ from untreated units in characteristics that affect the outcome of interest. Some estimators, like regression adjustments or propensity score matching, may overcome these biases, but they require the assumption that observables in the researcher's data set explain the entire selection. In real-world implementations, this assumption hardly holds.

With time-series data, a simple before and after analysis might return our parameter of interest. However, there is an omitted variable problem because the treated unit may be trending through time or have been hit by unobserved shocks between periods. The DID approach, which relies on panel data (cross-sections for two or more periods), addresses both problems simultaneously. The first difference, between the same unit over time, gets rid of selection bias. The comparison between treated and untreated at the same time takes care of unobserved shocks, as long as the parallel trend assumption holds (the treated and untreated group would exhibit the same trend in the absence of treatment).

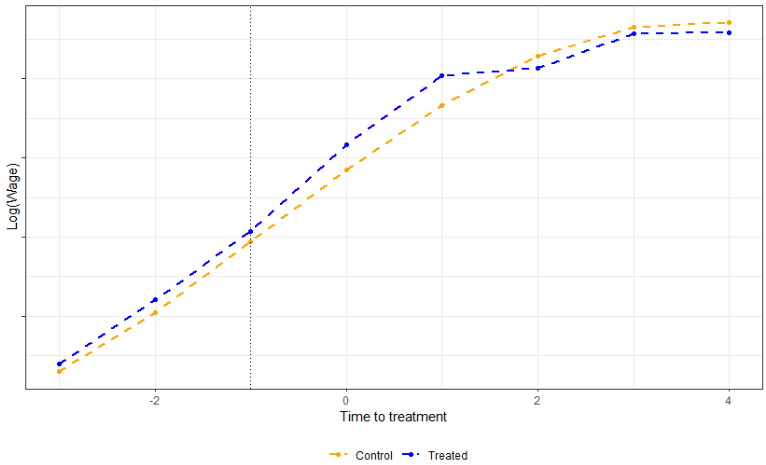
Figure 3.5: Treated and Control municipalities



Source: RAIS

In the empirical implementation, the crucial point is identifying the counterfactual group (untreated), which allows estimating the effect on those treated (treatment effect on treated - ATT). In our setting, we have access to RAIS data about employees, employers, and their links over time, at the municipality level. This dataset profile allows us to consider the merger as an exogenous event. The transaction occurred at the national level, not being affected by purely local market factors. Furthermore, the different local labor markets (the banking sector, six-digit CNAE code, in each municipality) are competitively heterogeneous: there are cities in which the two merging firms overlap; in others, only one operates. So, these conditions create two municipalities groups: the one exogenously affected by the merger (treated), regions where the firms operate simultaneously, and the one where initial competitive conditions remain unchanged (untreated), i.e., only one firm had establishments prior to the merger. The territorial distribution of treated and non-treated municipalities can be seen in the figure 3.5.

Figure 3.6: Treated and Control: mean wage evolution



Source: Author's computations

Our identification assumptions get rid of some possible sources of bias. The first two, related to selection biases and omitted variables (trends and time shocks), are addressed by DiD, as long as the parallel trend holds (at least a conditional trend with a common support assumption). The evolution of workers' mean wage (in merging firms) for treated and untreated groups can be inspected in figure 3.6 (where the vertical dotted line indicates the year prior to the merger announcement), and some descriptive statistics about them are in table 3.5. It is worth noticing that, before the merger announcement (until time -1), there was no evidence that the two groups were trending differently, although there were some non-negligible differences between the municipalities (the treated cities are, in general, larger markets with a bigger GDP; they also have older workers, with a higher percentage of women; however, median wages vary little between treated and untreated). Despite the observational evidence, we tested for pre-trends in two ways: with the inclusion of a linear trend in the saturated TWFE DiD model and through the event study approach (TWFE with leads and lags).

Table 3.5: Diff-in-Diff model - descriptive statistics (merging firms-municipality level, year prior to merger)

	Control (N=1,769)		**Treated** (N=523)	
Variable	Median	Std. Dev.	Median	Std. Dev.
Mean wage (base = control mean wage)	100	14.25	101.25	10.8
HHI	5,000	2,031	2,709	713
ΔHHI (after the merger)	0.00	474.10	321.75	445.43
Employed workers (merging firms)	17	24.35	98	4,618
GDP (municipality)	338,850,178	1,139,461,632	2,334,482,714	32,763,739,882
Female (% of employees)	36.36	23.14	48,48	11.43
Mean tenure (months)	86	50.57	110	33.41
Mean age	30.63	4.11	32.82	2.65
Union rate (% of employees)	66.00	34.90	72.00	22.85
College education (% of employees)	100	11.56	95.34	5.55

Note: statistics for the year prior to the merger operation

Another source of bias is merger-generated efficiencies. In theory, these efficiencies can offset the downward wage pressure by increasing the marginal revenue. Our empirical approach compares the mean wages between groups from the same merging firms. Therefore, it is assumed that efficiency gains would be intracompany distributed, with no different impacts on treated and untreated establishments. However, a specific type of synergy could still bias our estimate. The merger process can lead to a restructuring with cuts in personnel in areas "not directly linked to production" (overhead labor costs) in the merger-affected establishments. If these workers have a higher mean wage, our "treatment effect parameter" could be misleading, downward biasing the impact of market power on wages. To deal with this and other composition issues, we implemented robustness tests in which the estimation relies on stayers wages, i.e., incumbent workers who remained in the sample throughout the entire data series.

Before proceeding to the results, it is necessary to explain why, in this chapter, we have chosen to study the effects of a merger and acquisition transaction in the banking sector. First, the banking industry is considerably concentrated in Brazil, as detailed above. Second, there is a relative territorial dispersion of firms' operations in this sector, as shown in figure 3.5. In other words, we have a relevant number of units (municipalities) treated and untreated which allows greater power for our estimation. Finally, it is a sector with a reasonable level of specialization in its workforce — cross-sector elasticities are not supposed to be sufficient to cancel out the impact of increasing concentration and market power, favoring the treatment effect's identification. The same cannot be said, for example, in the food retail sector, in which a large part of the workforce can migrate to other similar activities. Of course, findings in this specific sector are not general, and empirical research in others markets is needed.

3.5.3 Models and results

Our first set of results has as the DiD outcome (dependent variable) the merging firms' log of mean wages, in each year, at the municipality level ($Log(Wage)_{r,t}$). Three variations of the general differences-in-differences model were implemented. The first one is the simple Two-Way Fixed Effect (TWFE), in which there is only one parameter of interest (δ), linked to the interaction between the dummy of the treatment group and the post-treatment period. The model's specification can be checked in equation 3.30.

$$Log(Wage)_{r,t} = \delta Treat_r \times Post_t + Region_r + Year_t + \varepsilon_{r,t} \tag{3.30}$$

As usual, $Region_r$ and $Year_t$ are the terms that control for units (municipalities) and time fixed effects. There is extensive literature regarding the limitations of the simple TWFE approach. The problems identified by multiple authors (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021) are, in particular, related to settings when there are multiple periods (dynamic effect) and variations in treatment timing (staggered adoption). Concerns about different treatment timing are not an issue in our empirical implementation. However, the merger-effect dynamics (multiple periods) are relevant to us.

Wages, prices, and market shares can be more or less flexible, and the variation in these outcomes' rigidity determines the evolution of wages in our sample periods. We chose to set the period after the merger announcement (not its approval by the antitrust agency) as the treatment "beginning", because, while there are regulations against what is known as gun-jumping, pre-merger coordination between the parties can occur, for example, concerning pricing, without the authority realizing it. On the other hand, hidden unlawful pre-approval changes in employment contracts would be rare, as unions monitor them.

This dynamic may have an identifiable impact on wages: if prices go up, marginal revenue goes up, and, if there is some rent-sharing process, wages can be higher in the treated group, despite the market power; later, with firms' monopsony power, wages can be pressured downward. The simple TWFE estimator would hide this post-merger wage behavior. After the estimation, we get an aggregate value for the multiple periods, as the model concentrates the whole treatment effect and its dynamics on a single parameter. In cases like the one described above, the result would be misleading.

Wooldridge (2021) makes a similar point. For the author, the TWFE model is not necessarily wrong as long as it allows for greater heterogeneity in the treatment effect. This heterogeneity is captured in a saturate specification, including terms for post-treatment periods (dynamic effect) and/or cohorts (staggered adoption). This extended version of the TWFE also accommodates time-invariant or pre-treatment covariates (centered and interacted with the treatment dummy) and linear/non-linear trends. This approach was adopted in the second difference-in-differences specification, presented in the equation 3.31.

$$Log(Wage)_{r,t} = \sum_{k=0}^{4} \delta_k 1[t = k] \times treat_r + Region_r + Year_t + \varepsilon_{r,t} \tag{3.31}$$

Only one change was made from the previous simple TWFE version. The δ parameter was disaggregated into $k \in \{0, 1, 2, 3, 4\}$ parameters (δ_k) for each interaction between the treatment dummy and the post-merger announcement years (there are no cohorts indicators since our setting does not have staggered adoption). The new parameters can identify the treatment effect dynamics. Furthermore, table 3.5 shows non-negligible differences between the two studied groups (treated and untreated municipalities). Because of this, and to make sure that there were no pre-trends, we estimated two other extended TWFE with linear trend terms and covariates, respectively.

Finally, we also implemented an event study style regression [2]. It is an extended TWFE model, but with leads, i.e., the interaction between the treatment's dummy variable and indicators for the years before the merger ($q \in \{-3, -2\}$). The specification is detailed in equation 3.32. The event study approach has two purposes. First, it allows another way to test the pre-trend. Through a joint significance test (Wald test), it is possible to verify the null hypothesis that the coefficients of the leads are equal to zero. Second, it is a good visualization tool for the outcome dynamics.

$$Log(Wage)_{r,t} = \underbrace{\sum_{q=-3}^{-2} \delta_q 1[t = q] \times treat_r}_{Leads} + \underbrace{\sum_{k=0}^{4} \delta_k 1[t = k] \times treat_r}_{Lags} + Region_r + Year_t + \varepsilon_{r,t} \tag{3.32}$$

Nevertheless, it is necessary to point out that, in the event study, the post-merger parameters have as their baseline the period -1 (the year before the announcement). Instead, the extended TWFE compares the after-treatment periods with an aggregation of all previous years. The models' assumptions are, therefore, different. Additionally, the extended TWFE with a linear trend assumes that the pre-trend influences the post-treatment period. In the event study, it is implicitly that any previous trend vanishes after period -1.

Simple and extended TWFE results (including linear trend and covariate models) are described in table 3.6. The estimates from the event study (obtained with and without covariates), in turn, are plotted in the figure 3.7. The aggregate treatment effect estimated in the simple TWFE model, although negative, is very close to zero and therefore not statistically significant. At first glance, this would seem to show that the merger had no effect on the wages of the workers at the firms involved.

[2]A discussion about the construction of event-study models/plots and its identifying assumptions can be found in Freyaldenhoven et al. (2021)

Table 3.6: Differences-in-Differences estimates - merging firms

Dependent Variable:	log(Wage)				
Model:	Simple	No covariates	Trend	Covariates	Small regions
Treat x Post	-0.006				
	(0.004)				
Treat x Year 0		0.021***	0.016***	0.021***	0.030***
		(0.004)	(0.005)	(0.004)	(0.006)
Treat x Year 1		0.026***	0.019**	0.026***	0.038***
		(0.005)	(0.008)	(0.005)	(0.009)
Treat x Year 2		-0.029***	-0.039***	-0.029***	-0.050***
		(0.006)	(0.010)	(0.006)	(0.010)
Treat x Year 3		-0.031***	-0.043***	-0.031***	-0.064***
		(0.006)	(0.012)	(0.006)	(0.011)
Treat x Year 4		-0.022***	-0.037**	-0.023***	-0.058***
		(0.007)	(0.015)	(0.006)	(0.013)
Linear trend			0.002		
			(0.002)		
Controls (centered and interacted)	No	No	No	Yes	No
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	17,595	17,595	17,595	17,514	13,204

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

Nevertheless, it is misleading, as predicted above. The scenario is very different in all the extended TWFE models, which break down the treatment parameters to capture the outcome dynamics. Between the merger announcement and the antitrust authority's approval (from period 0 to 1), there is a positive jump in the mean wages in establishments affected by the merger. This positive effect vanishes immediately after the transaction's clearance when workers' earnings fall between 2% and 3% in regions where market concentration increases (and the effect is persistent until at least period 4).

This wage behavior justifies our choice to consider the merger announcement as the treatment beginning. There is an evident anticipation movement before the approval. How to interpret these up and down phenomena? Our view is that retail market prices reacted immediately to the merger expectation, while adjustments in quantities (of products and production factors) may have been delayed. The temporary increase in workers' pay could be justified by the extra profit the market misalignment brought in (especially in the banking sector, where a big part of pay is tied to performance).

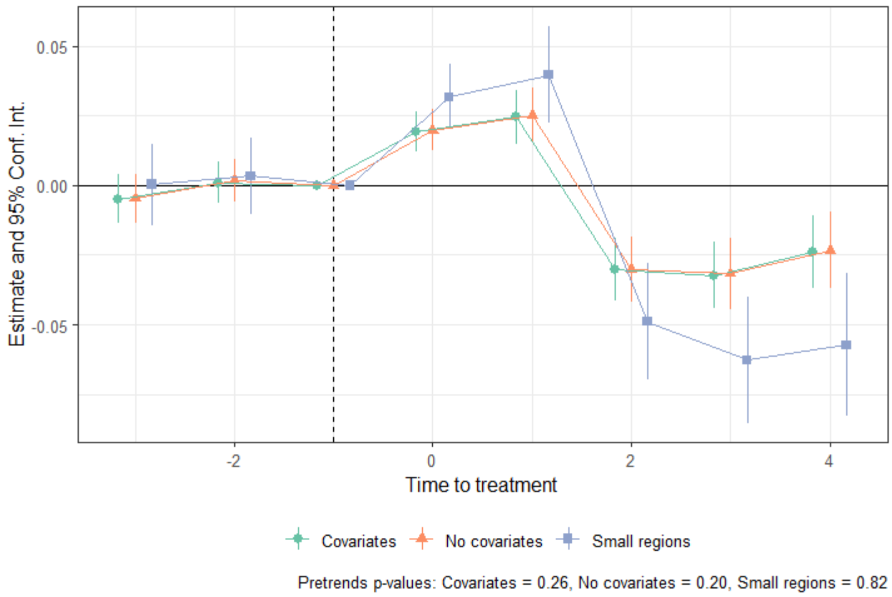


Figure 3.7: Event study estimates - merging firms

Still, in our initial results, in table 3.6, there is no evidence of divergent trends between treated and untreated groups before the merger. The linear trend coefficient is close to zero and statistically insignificant (third column). The Wald test in the event study plot (figure 3.7) points to the same conclusion, with p-values higher than 0.20. The covariates also do not have an apparent influence on estimates. The parallel trend assumption seems to hold unconditionally.

In addition to the baseline TWFE models (simple, with and without covariates and linear trend), estimated in the entire sample of municipalities, new estimates (from the model without covariates and trend) were produced in a new sample, composed only of municipalities with less than 50 workers in merging firms' establishments (small regions model). The new model is a robustness test addressing the potential violations of common support. There are non-negligible differences between treated and untreated municipalities (treated municipalities are, in general, larger markets). Even controlling for covariates, we may be comparing treated units with other untreated units that are not the appropriate counterfactual (common support assumption). In addition, the sample with smaller municipalities is also a test for non-linear behavior. It can potentiate the treatment effect since, in general, smaller markets are already more concentrated (and most models predict a nonlinear effect of market power).

The small region model reproduces the same wage dynamic pattern, with an initial jump followed by a significant drop after the merger's approval, although the effects are more substantial. It is evidence that the baseline estimates do not violate the common support principle. It is also indicative that there exists a non-linear effect of increasing market concentration. The impact is grater in smaller and more concentrated markets (achieves a 5% reduction in workers' wages).

Table 3.7: Differences-in-Differences estimates - merging firms and incumbent workers

Dependent Variable:	log(Wage)			
Model:	No covariates	Trend	Covariates	Small regions
Treat x Year 0	0.011*	0.023***	0.026***	0.022**
	(0.006)	(0.007)	(0.007)	(0.011)
Treat x Year 1	0.009	0.027**	0.032***	0.024*
	(0.007)	(0.011)	(0.010)	(0.013)
Treat x Year 2	-0.017**	0.007	0.010	-0.027*
	(0.008)	(0.014)	(0.014)	(0.014)
Treat x Year 3	-0.036***	-0.006	-0.003	-0.056***
	(0.009)	(0.017)	(0.017)	(0.016)
Treat x Year 4	-0.029***	0.007	0.010	-0.050***
	(0.008)	(0.020)	(0.020)	(0.017)
Linear trend		-0.006*	-0.007**	
		(0.003)	(0.003)	
Controls (centered and interacted)	No	No	Yes	No
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	16,643	16,643	16,149	11,501

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

Our baseline estimates rely on each municipality's average wages as the dependent variable. When running our DiD models, we capture two distinct sources of variation: changes in individuals' earnings and changes in the composition of the establishments' workforce. Therefore, the aggregated outcome is subject to composition effects. As Guanziroli (2021) has shown, the merger and its consequent impact on firms' objective functions would change the proportion of workers from different occupations with different wage levels.

Nevertheless, contrary to Guanziroli (2021), this fact does not lead us to adopt a regression with individual worker data. Our choice is justified, among other reasons, because, when using individual data, we would face a series of other sources of bias, such as, for example, the attrition caused by the merger (endogenous selection). In addition, throughout the entire sample period, some individuals enter and leave the establishments affected by the merger. We would then have an experiment design with variation in treatment timing, but once a unit becomes treated, there is no guarantee that it will remain treated in the next period. In this kind of setup, estimation needs a new set of assumptions that aren't met by our TWFE model.

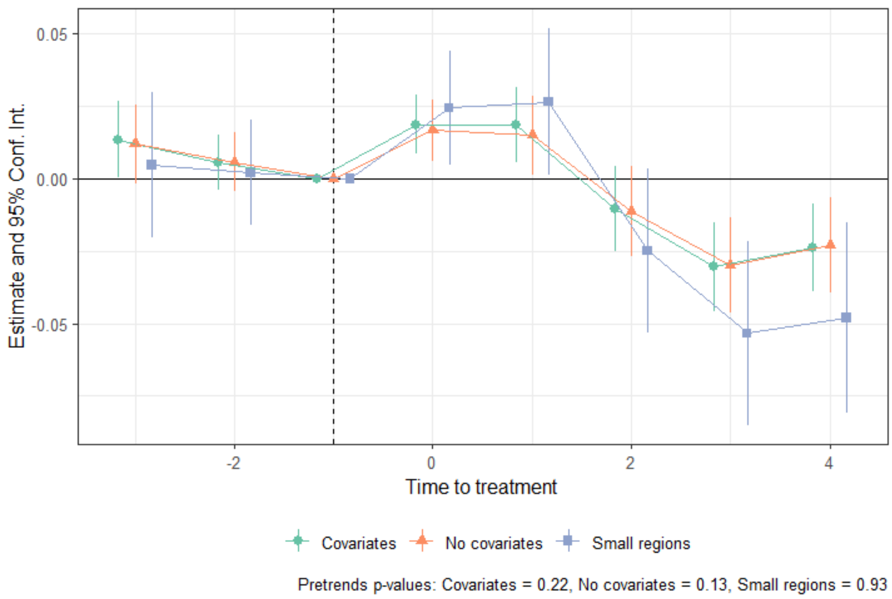


Figure 3.8: Event study estimates - merging firms and incumbent workers

Additionally, and perhaps more relevant for the chapter's proposal, variation in aggregate earnings due to changes in the occupation's proportion is mainly driven by the merger impact. In the end, firms change their optimizing outcomes in response to new levels of buying and product-side power generated by the transaction. Therefore, composition changes caused by market power should not be excluded when evaluating the overall merger effect. However, our benchmark estimation may still be biased by other non-market power composition variations, which are mainly caused by post-merger restructuring. Merging firms may eliminate the redundant workforce, impacting treated establishments/municipality' mean wage.

This issue was addressed by a new set of estimates using "stayers" workers; i.e., mean outcomes were calculated using wages from individuals who remained in their initial establishments during all the sample periods. With this procedure, we eliminate not only the composition effect caused by the firms' restructuring but also the merger-induced one. Therefore, our results should be read as a partial identification approach. Stayers' sample estimates are the lower bound for the treatment effect. At the same time, the benchmark, or entire sample, results are the higher bound.

Table 3.8: Differences-in-Differences estimates - market

Dependent Variable:	log(Wage)			
Model:	No covariates	Trend	Covariates	Small regions
Treat x Year 0	0.031***	0.041***	0.033***	0.048***
	(0.006)	(0.006)	(0.005)	(0.008)
Treat x Year 1	0.014***	0.029***	0.026***	-0.004
	(0.004)	(0.007)	(0.006)	(0.011)
Treat x Year 2	-0.006	0.013	0.008	-0.040***
	(0.005)	(0.009)	(0.008)	(0.015)
Treat x Year 3	-0.013***	0.011	0.004	-0.039**
	(0.005)	(0.011)	(0.010)	(0.018)
Treat x Year 4	-0.014**	0.015	0.008	-0.071***
	(0.006)	(0.013)	(0.011)	(0.023)
Linear trend		-0.005**	-0.004**	0.005*
		(0.002)	(0.002)	(0.003)
Controls (centered and interacted)	No	No	Yes	No
Municipality FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	56,668	56,668	54,883	32,766

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

Stayers' extended TWFE results (baseline, linear trend, covariate, and small regions models) are described in table 3.8. The event study's estimates (with and without covariates, small regions) are plotted in the figure 3.8. In the models with all municipalities, we cannot exclude the possibility of divergent trends for treated and untreated groups, even in the presence of covariates. Thus, it is recommended to consider the results in the specification with a linear trend parameter (second and third columns). They point to the same initial wage jump, followed by an effect close to zero (not statistically significant). In the small-region model, which controls for violations of the common support assumption and captures the merger impact in previously more concentrated regions, the merger effect estimates remain strongly negative, even in the presence of the linear trend. When the results from the initial benchmark and the stayers' model are added together, they show that the merger had, at best, no effect on workers' wages.

However, there is good evidence that workers' wages went down, especially in smaller and more concentrated markets.

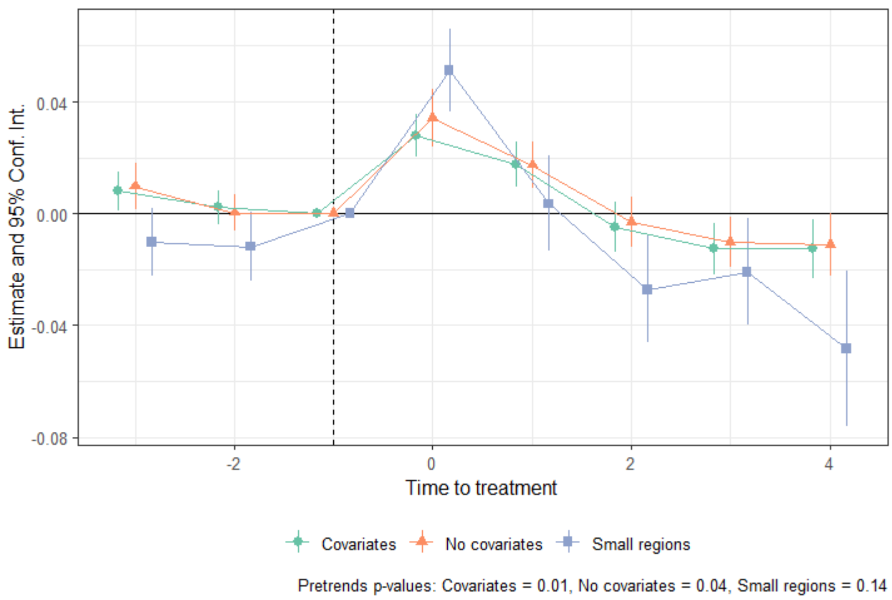


Figure 3.9: Event study estimates - market

So far, DiD models have focused on the merging firms' outcomes. Next, the new series of results analyze the transaction's repercussions on the local baking labor market as a whole, following the same empirical strategy as the baseline estimates. The results are displayed in table 3.8 (extended TWFE) and figure 3.9 (event study). Unlike the firms' model, the banking labor markets' model, in the sample with all municipalities, seems to return to the initial state after the CADE's approval — the models with linear trends indicate null effects after the initial rise. Again, however, in the smaller and more concentrated markets (small regions model), the merger has generated lower aggregate wages, with reductions of around 7% in the last sample period.

The evidence at the market level reinforces the notion that the merger has more negative repercussions in smaller regions with concentrated labor markets - and highlights the need for regionalized merger analysis. At the national level, the two involved banks held 20% (acquirer) and 5% (acquired) of the banking labor force. In theory, and as evidenced by the data, the impact on the market should be small, in general. However, when focusing on specific cities, where competition was already small, it was noticed that the two firms held up to 75% of the workforce. This level of concentration, and its consequent market power, resulted in an estimated 7% lower average wages in the years following merger approval. At this level of analysis, covering the entire banking market, our results are very much in line with the findings of Prager and Schmitt (2021). Their study found that the effects on wages were only significant in markets with previously high HHI levels.

3.6 Conclusion

The effects of market power on wages and, consequently, income inequality, remain absent from the literature and antitrust practice in developing nations, particularly Brazil. The objective of this study was to provide evidence to guide potential modifications to the way antitrust authorities approach labor market issues.

With detailed and identified matched employer-employee data from Brazil, it was initially possible to characterize the temporal evolution of the local labor market concentration (Municipality HHI, an index constructed with the weighted aggregation of the local industries' HHIs). Then, we constructed a fixed-effect model with instrumental variables to examine the relationship between local labor market concentration, income inequality, and wages (one of the mechanisms by which market power may affect inequality). To address possible concerns about the validity of the regression models with the HHI as the independent variable, a quasi-experimental empirical strategy was used to determine if the increase in market power caused by a merger and acquisition operation in Brazilian banking had an effect on workers' wages.

Our findings support the policy recommendation that Brazilian antitrust authorities, concerned with income inequality, should increasingly incorporate labor market data into their analysis procedures. Despite being included in CADE's manuals, this type of study is uncommon in Brazilian MA reviews. Equally uncommon are actions against employment contracts with non-compete clauses or no-poaching agreements between competing firms. Reforming antitrust processes creates a vast area for future research that aims to aid practitioners by focusing on the development of new policies, the creation of labor market tools and models, and the formulation of antitrust remedies. Several articles on this subject have been published, including those by Rose (2019); Sillman (2020); Naidu et al. (2018)

There is also some empirical and academic work to be done on the relationship between market power and wages. This literature continues to rely heavily on evidence derived from reduced form regressions. There is room for works with structural specifications, such as Felix (2021), to estimate a more comprehensive set of parameters and interrelationships between firms' and workers' decisions. This domain is also compatible with the causal inference framework utilized in our research. More post-merger or post-cartel analyses of the responses of wages in various markets will contribute significantly to policy guidance. Finally, we studied market power without effectively separating factor (labor) and product-side wage effects (although using employment HHI as our market power proxy). For the Brazilian case, research papers could be written that disentangle the effects of each market power's component (markups and markdowns). It will need access to identified databases on the labor market, such as RAIS, and to sectoral censuses conducted by the Brazilian statistical office (which contain information on firms' production and revenues).

4

Post-Cartel Behavior: assessing the effects of antitrust policy on Brazilian fuel market

4.1 Introduction

For decades, the Brazilian economy was characterized by oligopolized and heavily regulated markets with some legal cartels, an outcome of the industrial policies adopted before the 1990s (Frischtak, 1980; Considera, 2002). Therefore, the practice of harming free competition seems to be widespread, until today, in several productive sectors. That's why dealing with anti-competitive conduct, especially cartel cases with price-fixing behavior, is, probably, one of the most prominent aspects of the Brazilian antitrust authority's work. Some of the recent episodes have had enormous political repercussions, such as the subway cartel in São Paulo, the bid-rigging in Petrobras' auctions, or the prosecution against JBS, the largest producer of animal protein in the world.

In most cases, these investigations become public after major dawn raids by police or antitrust agents, with extensive media coverage and numerous preventive arrest warrants issued against the scheme's leaders. While the pre-investigation period is widely publicized and scrutinized, the "post-cartel" period is largely overlooked. How successful is the antitrust action? In some ways, the purpose of this chapter is to answer these questions in order to assess the effectiveness of antitrust policies in Brazil.

This chapter relies on structural break analysis (Bai and Perron Test) and Markov Switching Regressions, methods widely used by macroeconomists to study business cycles, but which have recently been adopted by Industrial Organization researchers, although with an emphasis on screening and not on an assessment of the effectiveness of antitrust prosecution. As an auxiliary method and a type of robustness check, we also combined these two approaches with a traditional IO conduct parameter model (Bresnahan, 1989) (which helped us to identify and get some certainty about collusion and "competition" periods).

Commonly, papers studying the antitrust effect on cartel cases take the form of a quantifying approach, trying to measure the impact on prices with methods like before-and-after dummy regressions, difference-in-difference, or synthetic control designs. Some examples are Lucinda and Seixas (2016); Cuiabano (2019); Afonso and Féres (2017). However, despite their importance and robustness, these approaches have some downsides. Before the analysis, researchers should establish one exogenous date or breakthrough event based on assumptions that may not be accurate. After all, they might end up with treatment effects composed of different regimes (collusion, price-wars, or competition), resulting in under or overestimates, as shown in Boswijk et al. (2019).

Unlike the methods adopted in previous work, ours sets the breakpoints endogenously and maps the cartel behavior evolution through time. Therefore, it's possible to obtain evidence about the effectiveness of different measures against cartels implemented in different prosecution phases. Ultimately, these methods would allow us to estimate, with more precision, the actual damage caused by the cartel or the real price impact of the antitrust policy.

We apply the empirical techniques described above to four cartel cases in the Brazilian fuel market (Brasília, Belo Horizonte, São Luís, and Londrina). Expenses on transportation, in which fuel prices are very relevant, represent a share of 18% of Brazilian households' budgets (higher than spending on food), according to the most recent Family Budget Survey (POF-IBGE 2017-2018). Therefore, the distributive impact of price-fixing schemes is considerable[1].

[1] A study from Brazilian antitrust authority (Motta and Resende, 2019) estimated that Brasília's fuel cartel managed to raise prices by just over 8%, and caused, in one year, about US$ 75 million in consumers' losses.

Additionally, price-fixing in the fuel market is undoubtedly responsible for the largest number of complaints and prosecutions against Brazil's noncompetitive practices. Since 2012, the first year after the reform of the antitrust law, the Brazilian authorities have judged 25 cases involving irregularities in fuel retail, with 15 convictions and fines amounting to US$ 100 million. Finally, free and open access data on resale prices, wholesale prices, volumes, margins, and costs of retail fuel stations, provided by ANP (Oil Market Regulatory Agency), allows great flexibility in the econometric approach as well as replications and comparisons between different studies.

This chapter is linked to a small but promising body of work that applies endogenously dating techniques to price-fixing cases, as Boshoff and van Jaarsveld (2019); Crede (2019); Boswijk et al. (2019); Silveira et al. (2019) (the latter employing a Markov regime change approach based on GARCH models). Our main contribution was to examine data not only concerned with screening cartels or estimating damages, but also with establishing links between price series breaks and actual measures against cartels taken by authorities. And for this purpose, the fact that we exploited the traditional IO conduct parameter model to identify collusion regimes is a relevant innovation to the practitioner's toolbox. Additionally, as far as we know, this work is the first to systematically apply both Structural Breaks tests and Markov Switching Regressions to an extended set of cartel cases, which allows comparing the strengths and weaknesses of these approaches. Since these methods were used in a market where collusion is known to be unstable (due to the undifferentiated nature of fuel and the large number of competitors), our work also allows us to test how sensitive to constant regime changes the two methods are to constant regime changes.

As a comparative test between MSR and Bai and Perron procedures, our results show that despite the relative accuracy of structural breaks effectively signaled, the breakpoints test might be less useful when we suspect recurrent collusion or instability in cartel agreements. Since partitions of the sample, with a minimal size to allow econometric estimation, must be defined, it will probably miss some breaks or shorter episodes. In fact, we have observed supposedly missing breaks in our set of cartel cases (in other words, the Bai and Perron tests are somehow more vulnerable to Type II errors, despite no evidence for systematic Type I errors). We also found that when markets suffer from successive periods of collusion followed by price wars or competition phases, it's hard to interpret the results from break estimation without other information sources. Instead, Markov switching techniques seemed to be more sensitive to transitions between regimes without missing breaks and give us more precise results. Although, in some cases, we had to count on methods such as conduct parameters to identify the regimes.

From the point of view of antitrust policy evaluation, our findings seem to indicate a low capacity of the authorities to extinguish price-fixing practices in targeted markets. The collusive behavior in fuel retailing is quite resilient, with strong recidivism or residual collusion after the antitrust intervention. Except for Brasília's cartel case, the data indicates that cartel episodes end less because of the authorities' action and more due to natural changes in the market. At best, we can say that the competition policy is making price-fixing schemes less stable.

The remainder of this chapter is structured as follows. Section 2 provides a theoretical background of post-cartel behavior. Next, Section 3 discusses relevant aspects of the Brazilian fuel market. Section 4 presents our econometric procedures, and Section 5 describes our data and some modeling issues. Finally, Section 6 shows our main results and findings, and Section 7 concludes with some policy implications.

4.2 "Post-Cartel Behavior"

Intuitively, we can speculate on the effects of antitrust action on the market affected by a cartel. The most obvious and a reason for the existence of anti-collusion policy would be the reestablishment of adequate competition levels, which leads to a price reduction and an increase in quantity sold (and the respective benefits: rising economic allocative efficiency and consumers' purchasing power). However, the results would not necessarily be the return to marginal cost prices. They could be different depending on what seemed natural about the way the market was set up: pure competition in markets with low concentration or oligopolistic competition (like

Bertrand, Cournot, and others) in markets with high concentration and barriers to entry.

Instead, there are no guarantees that the competition authority's action will be effective. As a result, the market may remain collusive and prices will be at levels of the pre-intervention phase. Finally, as can be found in the empirical literature, there are indications that, in some cases, the effects are mixed. There is not necessarily a return to competition, not even the evident maintenance of collusive practice. Markets could suffer a hysteresis effect with changes in the pricing strategy, but prices will still be above the competitive level. A whole spectrum of states could explain this new market behavior. As an example, we may face a kind of post-cartel tacit collusion. Further, depending on market conditions, the sequence of events may even allow the cartel to re-establish itself (recidivism).

Although intuitive, "post-cartel" behavior has been scarcely addressed in the theoretical literature. The primary reference is still the work of Harrington (2004), who developed a post-cartel pricing model during litigation. The model considers that firms assess the likelihood of having to compensate for the damage caused by the cartel and act strategically, maintaining, during the litigation phase, prices at levels above those of competition, generating an underestimation in damage value (the estimation of a cartel's damage is, in general, based on but-for prices, which take pre and post-cartel values as reference). Erutku and Hildebrand (2010) studied the fuel market in Quebec, a Canadian province, and gave some empirical validity to Harrington's model. They obtained results that show a bias between competitive prices and those observed when firms act strategically to reduce the damage estimate. According to the authors, this bias diminishes over time as the litigation process comes to an end, but it increases when the authorities file criminal charges.

In addition to Harrington's theoretical approach, on the other hand, there is a vast empirical literature on the effects of anti-cartel policy, but with divergent results, as referenced in Ordóñez-de Haro and Torres (2014). Ordóñez-de-Harro and Torres extensively analyzed a group of actions taken by Spanish competition authorities against cartels in the food industry. They found minimal effects. Prices have dropped slightly in some markets but have gotten higher in others; therefore, they didn't find any evidence of actual gains for consumers. However, it's interesting to note that, according to the authors, there was a change in the pricing strategy, manifested in the notable reduction of price variance. As they suggest, this new strategy was based on prices that were above competitive levels but stable enough to minimize the risks of another raid.

When, after antitrust intervention in the market, prices remain above competitive levels, we can highlight two scenarios, among many others: the re-establishment/continuity of the price-fixing scheme (recidivism), or tacit coordination between firms, facilitated by years of communication and coordination before the discovery of the cartel (residual collusion). The last case is the hypothesis advanced by Crede (2019), which analyzed the pasta market in Spain, France, and Italy. The paper shows that prices in Spain and Italy, countries virtually affected by a cartel, were higher than in France during the collusion and remained higher after the cartel's dismantling. The author argues that possible changes in local industries' structures can hardly explain these differences. González and Moral (2019) suggest a similar scenario in the Spanish fuel market. The authors showed that price levels shortly increased after the competition authority imposed fines on colluding firms and that these increases largely compensated for the penalties. Therefore, it indicates that consumers are likely to bear penalty costs and that this behavior would, in hypothesis, only be possible due to some residual collusion.

The hypothesis of post-cartel tacit collusion, facilitated by the firms' experience in establishing coordination during the collusive phase, has been tested in two recent experiments. Fonseca and Normann (2012) and Chowdhury and Crede (2020) showed that a history of cooperation facilitates tacit collusion by reducing uncertainty about other firms' actions and that the likelihood of this scenario is strongly linked to the success of the previous collusive phases. Crede went further and considered the effects of some antitrust measures on the probability of residual collusion. He showed that changes in the composition of firms operating in the market (rematching), for example, had a substantial effect on reducing post-cartel tacit collusion.

The possibility of re-establishing/continuing the price-fixing scheme (recidivism) after the action of the antitrust authority has received significant attention in antitrust literature, mainly in Europe and North America (as far as we know, recidivism in the Brazilian market is a problem, not addressed yet by literature). Penalties imposed by the authorities (only administrative fines in some jurisdictions or criminal charges in others, like the USA and Brazil) may not be enough to discourage collusive behavior in some markets, especially if collusion benefits are still high and some factors facilitate price-fixing agreements. Some evidence suggests that many companies are repeat offenders in the EU, so antitrust investigations are weak enforcement. For this issue, we can cite Lande and Connor (2011); Connor and Bolotova (2006); Smuda (2014). The authors concluded that both US and European penalties for cartels are not high enough to achieve deterrence. Levenstein et al. (2015), in a sample composed of cases judged in the United States and the European Union between 1961 and 2013, identified a high frequency of cartelization in the chemical, pharmaceutical, construction, and fuel sectors. Among the factors that facilitate collusion, they mentioned the inelasticity of demand, market concentration, barriers to entry, industry history and culture, as well as the presence of producer associations.

On the other hand, sometimes even the announcement of an antitrust investigation has the power to promote changes in market agents' actions. This happened in Canada, as Clark and Houde (2014) has shown. The authors found that prices fell just after the Canadian Competition Bureau announced, in May 2006, an investigation into the retail fuel sector. Yet, as pointed out by González and Moral (2019), they didn't analyze what happened after the case was closed, a kind of evidence that could elucidate essential aspects of the dynamic of post-cartel behavior.

4.3 Brazilian fuel market

The fuel market trajectory in Brazil is a typical example of how decades of strict economic regulation can generate persistent effects on competitive practices. Until the late 1990s, oil production and commercialization were entirely managed by the Brazilian government. There was a legal monopoly of a state-owned company, Petrobras, in upstream and refining. There were controls on prices, margins of sale, and freight in the distribution and resale of automotive fuels. The first attempt to open the market can be traced back to 1996, when prices were set free in distribution and retail in some regions, like the South, Southeast, and Northeast. In 1997, the Petroleum Law legally broke Petrobras' monopoly in upstream and initiated a comprehensive liberalization process through this market. But the process ended only in 2002, when prices were released in all regions and gasoline imports were allowed.

In the period that followed the liberalization, many new wholesale traders and independent stations (resellers without any contractual relationship with fuel suppliers) started to operate. Currently, Brazil's fuel market has 206 wholesale traders and about 41 thousand fuel stations, 40% of which are independent (Agência Nacional de Petróleo, 2016). Despite the growing number of players in the market, there is evidence that free competition still faces cultural and structural barriers. More than a third of the cartel charges filed by CADE in the last decade are related to the fuel market, particularly the retail of gasoline.

The history of official price-fixing practices is a relevant incentive for collusive practices, but other market characteristics reinforce the trend toward anti-competitive behavior. In Brazil, fuel refining is, until today, Petrobras' non-official monopoly. The state-owned company owns 17 of the 18 refineries operating in the country, across the national territory (the only exception is the Manguinhos refinery, privately owned but whose production represents less than 2% of the total). There is also an oligopoly in the distribution chain. The largest group of firms, formed by Petrobras, Ipiranga, and Raízen, accounted for about 72% of the total volume of liquid fuels distributed in Brazil in 2015.

Reselling liquid fuels is the final step in the fuel chain and consists of receiving fuel from the distributors and serving the final consumer through fuel pumps. Stations have been free to set their prices since 2002. In theory, this would be a highly competitive market: it's local and dispersed, with many firms selling a homogeneous product. In practice, however, the number of competitors varies significantly between locations, and there are focal points and homogeneous costs due to the concentration of refining and distribution and the integrated relationship between distributors and branded stations.

Regulatory barriers to entry also characterize the fuel market. A fuel station needs both environmental licenses and authorization from ANP (Brazilian National Agency of Petroleum, Natural Gas, and Biofuels) to operate. In some cities, there is even evidence that the resellers' lobby has influenced municipalities to impose regulatory difficulties on the entry of major competitors, such as stations installed on large commercial surfaces or supermarkets (a well-documented case in charges against the Brasilia cartel, for example). Additionally, competitors' prices are easy to monitor (Brazilian legislation requires easy visualization) and the resellers' unions' presence is notable (often standardizing commercial practices). So, it's very likely the prevalence of collusive conduct in this market.

Lastly, it is worth noting that, until the end of 2016, the government informally imposed a policy on Petrobras that prevented the adjustment of fuel prices based on the variation in international oil prices. We can highlight two relevant consequences for competition in the gasoline market. First, this policy hampered the conditions for importing firms, worsening Petrobras' market power problem. Second, for many years, price adjustments in refining were relatively rare. Considering that the wholesale price is the essential cost for fuel stations, this stable environment may have reduced uncertainty and informational frictions in the market, facilitating the arrangement and monitoring of collusive schemes. Changes in refining price policy may have helped to break down price-fixing behaviors in recent years.

4.4 Econometric strategy

As stated before, the most widely adopted approach to measuring the effects of competition policy in cartel cases relies on an estimation of a before-and-after price dummy regression. The idea is to fit a reduced-form regression model as follows:

$$p_t = \alpha_1 + \alpha_2 dummy_t + \beta x_t + \varepsilon_t \tag{4.1}$$

Where, p_t is the price level at period t, x_t is a vector of demand and cost shifters, and the dummy variable indicates the cartel period, before the antitrust intervention ($dummy_t = 1$ if the observation in time t are included in cartel phase, $dummy_t = 0$ after the action against cartel, given the assumption that the raid ended the scheme). In the policy evaluation process, our attention is on α_2. Suppose the regression model is correctly specified, and the parameter is positive and statistically significant. In that case, we can interpret it as a positive margin over the competitive price level, strong evidence of the cartel's existence, and an argument favoring the change in market behavior after the authority's enforcement. In general, these models are suitable because they don't require much data, and their results are easy to interpret.

When the information available allows, the assessment process can be more robust if the research exploits some cross-sectional data. With the difference-in-difference approach, the econometrician uses panel data from different periods and markets (some that are affected by the cartel, the treated group, and others that aren't, the control group) to estimate treatment effects. This last approach has a negative aspect: researchers should select the control markets exogenously and accept an assumption about the common trend between prices of control and treatment units.

More recently, as in Motta and Resende (2019), another technique has been adopted to endogenize the control group choice or weighting. The Synthetic Control Method consists of a counterfactual construction based on a weighted mean of markets that are not treated: *synthetic controls models optimally choose a set of weights which when applied to a group of corresponding units produce an optimally estimated counterfactual to the unit that received the treatment. This counterfactual, called the synthetic unit, outlines what would have happened to the aggregate treated unit had the treatment never occurred (Cunningham, 2018).*

As was discussed in the introduction, all these models have some critical downsides. First, they assume that the changes between collusive and non-collusive periods occur only as price levels shift (in other words, only in the intercept or in the outcome means). As an example, the regression coefficients in the before-and-after method should not necessarily be the same across different market regimes. Cost pass-through to price can be different, as shown in the collusion literature. Second, the dummy, diff-and-diff, and synthetic controls all assume that the official cartel breakdown date or the legally established collusion period are correct, which can be extremely misleading, especially when dealing with a supposed recurrent cartel, as in the fuel market.

When relying only on official information, our results for overcharges may be overestimated, as they will possibly include price war periods in the collusive phase. Or our impact measure may be understated if we do not account for transition periods (the start or end of a cartel scheme, as Harrington, 2004 argues, is not always a sharp event). Finally, we don't know if the antitrust intervention extinguished the cartel scheme. The breakdown could be only temporary, and the traditional approaches usually ignore the post-cartel dynamics. Even when the researchers look at data in different periods, they, in practice, are groping in the dark since the date choices are still outside the model.

To evaluate the antitrust policy in the Brazilian fuel market (overcoming this bias from misspecifying effective collusion periods and accessing more accurately the actual post-cartel behavior dynamics), we will combine three approaches: tests for multiple structural changes (Bai and Perron tests), Markov Switch Regressions (MRS), and a traditional IO structural model design to estimate the market conduct parameter (structural model).

Our tests still consider the same reduced form model shown above, with demand and cost shifters, but in an extended version of the Data Generation Process (DGP), including dynamic, as an auto-regressive distributed lags form (ARDL). Specifically, in Bai and Perron's approach, for the DGP, there may exist m potential breaks in our data series (producing $m+1$ regimes):

$$p_t = \alpha_j + \sum_{l=1}^{n} \gamma_{jl} p_{t-l} + \sum_{l=0}^{n} \beta_{jl} x_{t-l} + \varepsilon_t \tag{4.2}$$

Taking the regimes $j = 0, \ldots, m$, is worth noting that the intercept, coefficients for autoregressive and for demand/cost shifters variables may vary across them. If the number and dates $(T1, ..., Tm)$ of the regime breakpoints were predetermined, the model could be estimated using the standard least squares approach. But, in our application, the candidate's set of break dates is unknown. To deal with this kind of problem, Bai and Perron (1998) developed an algorithm for a global optimization procedure that identifies the breaks and regression coefficients while minimizing the sums-of-squared residuals as follows:

$$S(\gamma, \beta|\{T\}) = \sum_{j=0}^{m} \left\{ \sum_{t=T_j}^{T_{i+1}-1} p_t - p'_{t-l}\gamma_j - x'_{t-l}\beta_j \right\}^2 \tag{4.3}$$

However, this minimization is possible only over predetermined sample partitions for which the minimal segment (or the minimal percentage of observations between two breaks) is bigger than h, the trimming parameter (so, the suitable h choice depends on the number of observations in the data). After we obtained the number of breaks, the test between the null hypotheses of no breaks against m is done using the standard F-statistic, a framework previously developed in Chow (1960). So, by estimating successive breakpoints in a specific fuel market, we should be able to check the cartel's behavior through time and evaluate if there is any coincidence between regime changes and antitrust actions against the cartel.

With the Markov Searching Regression (MRS) approach, we could pursue the same goal: map the cartel behavior and assess if antitrust enforcement was able to change it. But, instead of defining a minimal segment and letting the model do the work, signaling the regimes, the MRS technique requires the previous definition of a maximum number of states in our data series. Given the possibilities of our sample, the model's computational burden, and our assumption about the possible regimes (collusion and non-collusion), our previous reduced form equation was set in two regimes:

$$p_t = \begin{cases} \alpha + \sum_{l=1}^{n} \gamma_l p_{t-l} + \sum_{l=0}^{n} \beta_l x_{t-l} + \varepsilon_t, & s_t = 1(collusion) \\ \alpha + \sum_{l=1}^{n} \gamma_l p_{t-l} + \sum_{l=0}^{n} \beta_l x_{t-l} + \varepsilon_t, & s_t = 2(non-collusion) \end{cases} \tag{4.4}$$

Now, the pricing behavior is assumed to be dependent on an unobserved discrete state variable S_t, and different DGPs can be estimated for each s_t. If we also assume that ε_t is normally distributed, the parameter values can be found by maximizing a log-likelihood function formed by the normal density function and the one-step ahead probability of being in one of two regimes:

$$l(\alpha,\beta,\gamma,\sigma,\delta) = \sum_{t=1}^{T} \log \left\{ \sum_{m=1}^{S} \frac{1}{\sigma_s} \phi \left(\frac{p_t - \mu_t(m)}{\sigma(m)} \right) \cdot P\left(s_t = m | \Im_{t-1}, \delta\right) \right\} \tag{4.5}$$

Where $S_t = (1,2)$, δ represents the parameters that determines the regime probabilities, ϕ is the standard normal density function and $\Im_{t-1}$ is the set of information in previous period. In order to complete the Markov model, we must add one more assumption: the probability P of being in S_t follows a first-order chain with a transition matrix:

$$\xi = \begin{bmatrix} \xi\left(s_t = 1 | s_{t-1} = 1\right) \xi\left(s_t = 2 | s_{t-1} = 1\right) \\ \xi\left(s_t = 1 | s_{t-1} = 2\right) \xi\left(s_t = 2 | s_{t-1} = 2\right) \end{bmatrix} \tag{4.6}$$

The probability of switching or remaining in one regime is given by ξ. Since the Markov rule implies that the one-step-ahead probabilities depend on previous observations, the log-likelihood function in equation 4.5 must be estimated recursively. Following Hamilton (1989) and Kim (1994), it's possible to obtain parameter estimates and, with them, calculate filtered or smoothed probabilities of being in a regime at a specific time. In our work, we adopted the results from smoothed probabilities to describe cartel behavior evolution through time because these probabilities are established considering all the sample information (previous and further observations).

So far, our empirical strategy must have been able to highlight, with accuracy, a regime change in the observed market. However, the problem of identifying which periods are supposed to be considered collusive or non-collusive has yet to be solved. We could rely on official documents to establish when the cartel was active. This type of exogenous procedure is, instead, precisely what we have tried to avoid. We shouldn't completely ignore the information from investigations, but it must always be contrasted with the model's empirical results. Another option would be to observe the evolution of prices or gross margins (data available for fuel markets). Increases in prices or margins would be a signal that we have entered into a collusive regime. Even though this information is important, it could be misleading because prices and margins are affected by changes in demand and costs (margin data doesn't account for labor or other costs).

That's why, in our chapter, we consider a well-known structural model, with conduct parameter (Bresnahan, 1989), a useful tool. This model has been widely adopted and scrutinized in recent decades. Criticisms about its lack of power for identifying conduct (underestimation of market power), as in Corts (1999) and Salvo (2004), were not dismissed. However, we were not interested in the absolute value of the conduct parameter but in its relative variation between regimes and structural breaks. Therefore, the conduct parameter should be an auxiliary piece of information and a way to check other methods' robustness. Suppose that we identified a structural break in Bai and Perron's test or a regime switch in MRS. Applying the parameter conduct model with dummies for the two periods in our sample will point out a change from collusion to a non-collusion phase if the first period's coefficient is higher than the second one and if the variation is statistically significant. Otherwise, with insignificant variation, our finding should be considered problematic. In the next section, we will show how we set up this structural model and discuss some practical issues related to our data, structural tests, and Markov switching regression.

4.5 Data and modeling issues

Recovering the base model in reduced form, our DGP has the generic specification below:

$$p_{gt} = \alpha + \sum_{l=1} \gamma_l p_{gt-l} + \sum_{l=0} \beta_{1l} p_{wt-l} + \sum_{l=0} \beta_{2l} w_{t-l} + \sum_{l=0} \beta_{3l} p_{et-l} + \sum_{l=0} \beta_{4l} y_{t-l} + \varepsilon_t \qquad (4.7)$$

The core of our dataset was obtained from the Brazilian fuel regulatory agency (ANP). ANP conducts a monthly survey at the fuel station level, which collects and turns into public weighted (by sales) average values for gasoline resale, wholesale, and ethanol prices (p_{gt-l}, p_{wt-l} and p_{et-l}), respectively, our dependent variable, our main cost factor, and a substitute good. Additionally, as a cost factor, we have labor (w_{t-l}). This variable was constructed using data from the register of employed and unemployed workers (from the Ministry of Economy) and represents a variation in labor costs: the sum of wages of newly hired minus the sum of wages of dismissed station employees. Finally, we tested two more variables as demand shifters: the number of vehicles registered in each location and IBC-BR, the Central Bank's economic activity index (y_t). Due to the high degree of collinearity, the data on the vehicle fleet was dropped. All data have monthly observations and were available from 2004 to 2019 (until July, for B. Horizonte and Brasília cases, and November for Londrina and S. Luís). Further, to model the conduct parameter, the vehicle fleet variable was recovered, and we added gasoline sales (ANP), which covers only the period 2004-2018 (except for Brasília, where we have data from 2012 to 2019).

Table 4.1: Variables and data sources

Variable	Source	Details
Gasoline Retail Price p_{gt}	ANP	Weighted (by sales) monthly average prices (BR Real/L)
Gasoline Wholesale Price p_{wt}	ANP	Weighted (by sales) monthly average prices (BR Real/L)
Gasoline sales q_t	ANP	Monthly sales of gasoline by wholesalers (m^3)
Ethanol p_{et}	ANP	Weighted (by sales) monthly average prices (BR Real/L)
Labor Costs w_t	Register of Employed and Unemployed workers, Ministry of Economy)	Sum of wages of hired minus the sum of wages of dismissed stations employees (BR Real)
Economic Activity Index y_t	Brazilian Central Bank	IBC-BR: Monthly economic activity index (base: Jan-2003)
Vehicles fleet v_t	Dentran (Brazilian Traffic Department)	Monthly local vehicle fleet

The literature regarding the structural break tests indicates that the results' accuracy depends mainly on the correct DGP specification (Crede, 2019). Therefore, it's recommended that the model goodness-of-fit and the structure of lags have been previously evaluated in a base period, gathering observations from a competition phase. However, in our case, this means a severe weakness since there is no way to guarantee that we know in advance which observations can be considered in the competition regime. As a second-best option, we chose to adjust the model on a sample partition before what was officially delineated, in the antitrust proceedings, as the cartel period. However, this DGP may contain collusive periods not identified by the authorities, and, therefore, from now on, the results of the structural break test should be analyzed with caution. We should first combine all the information available in our results to state whether the regime change means a transition from non-collusive to collusive periods or otherwise (this highlights the importance of using other methods in our inference processes, such as MRS and conduct parameters).

Table 4.2: Specification of DGPs (data prior to the cartels´ official period)

Variable	Brasília	Belo Horizonte	São Luís	Londrina
	0.009	0.006*	0.009	-0.000674
Constant	(0.008)	(0.003)	(0.005)	(0.010)
	0	0	0	0
	-0.323***	-0.392***	-0.314493***	-0.128795
Gasoline retail price(-1)	(0.103)	(0.091)	(0.086)	(0.125)
	1	1	1	1
	2.137***	1.182***	1.279***	1.115***
Gasoline wholesale price	(0.313)	(0.0888)	(0.417)	(0.398)
	0	0	0	1
	-12.821***		3.552	5.587194
Gasoline wholesale price sqr.	(4.035)		2.933	(7.013)
	0		0	0
	-0.139*	0.068**	-0.042	-0.054
Ethanol	(0.104)	(0.032)	(0.145)	(0.101)
	0	1	0	0
	0.0002	0.00003	0.007***	0.0002
Labor costs	(0.0003)	(0.0002)	(0.002)	(0.000747)
	0	0	4	1
	0.008**	0.0005	-0.007**	-0.002
Economic Activity Index (IBC-BR)	(0.004)	(0.002)	(0.003)	(0.005)
	2	0	1	0
Adjusted R^2	0.585	0.885	0.752	0.588
Observations	47	35	32	35
Period	2004M01 2007M12	2004M01 2006M12	2004M01 2006M12	2004M01 2006M12

Dependent variable: retail gasoline prices (source: ANP). All variables are in first differences and adjusted for seasonality.
*Coefficients with ***, **, and * are significant at level 1, 5, and 10%.*
The third row of each variable indicates the lag that was used in the regression.

The results from fitting a DGP for each of the four markets analyzed are displayed in table 4.2 and the diagnostics tests are in table 4.3. It's worth noting that our lag structure is as parsimonious as possible, only enough to fit the model adequately. Since Bai and Perron's algorithm has a partitioning procedure based on the trimming parameter, the inclusion of many regressors could compromise the convergence proprieties of our estimation. In general, the coefficients show an expected pattern, with some deviations, especially on demand shifters (there are negative signs in Ethanol and IBC-BR coefficients, which might be explained by Rotemberg and Saloner (1986) model of 'price wars' during business cycle booms, considering that we didn't rule out the possibility of including observations from the cartel period). Another relevant aspect of our DGPs is that there is heteroskedasticity, or serial correlation, in São Luís and Londrina. Therefore, we must process their structural break tests with robust standard errors.

Table 4.3: DGPs residual diagnostic tests

Test	Null hypothesis	Brasília		Belo Horizonte		São Luís		Londrina	
		Statistic	*p-value*	Statistic	*p-value*	Statistic	*p-value*	Statistic	*p-value*
Jaque-Berra	Normally distributed	0.39	0.81	0.18	0.91	0.97	0.61	7.3	0.02
Breusch-Pagan-Godfrey	Homoskedasticity	4.32	0.63	9.67	0.28	6.5	0.36	16.5	0.02
Breusch-Godfrey	No serial correlation	3.68	0.15	2.6	0.11	5.1	0.07	2.7	0.25
Ramsey RESET	Correct specification	0.63	0.52	0.84	0.40	1.18	0.24	1.2	0.23

We must state one last observation about structural break tests. There is no predefined ideal trimming parameter. Additionally, Bai and Peron's tests can be conducted by allowing or not allowing different error distributions through the breaks, and neither there nor there is an ideal choice. A robust way to deal with these issues is to estimate our breaks with different trimming parameters and homogeneity and heterogeneity in error distributions to check if the results are consistent.

In the case of Markov Switching Regression, as highlighted by Boshoff and van Jaarsveld (2019), the principal practical aspect is the model sensibility to which variables can vary between regimes (in MRS, not only the coefficients may change but also the error variance). That's why the initial step of our estimation is a model selection based on an information criterion.

As stated before, the variation in our sample doesn't allow more than two regimes, so the decision was only about regime-dependent variables and error variances. As we can see from table 4.4 (the best model is in bold), in the Brasília case, there is regime dependence on intercept, auto-regressive term, wholesale price, ethanol, and error variance. In the Belo Horizonte fuel market, the error variance is neither regime determined nor the labor coefficient. In São Luís, ethanol, IBC-BR, and labor are not dependent. Londrina didn't observe regime variation in error variance, ethanol, or labor variables.

Table 4.4: Model selection by information criterion (Akaike)

Switching parameters	Brasília	B. Horizonte	São Luís	Londrina
C	-	-	-	-
CAR	-	-3.419	-2.086	-2.540
CARW	-2.252	-3.525	-2.114	-2.547
CARWE	-2.261	-3.639	-2.105	-2.540
CARWI	-2.258	-3.604	-2.105	**-2.551**
CARWEI	-	**-3.641**	-	-2.543
CARWL	-2.251	-3.515	-2.105	-2.545
CARWS	-	-3.554	**-2.377**	-
CARWSE	**-2.349**	-3.630	-2.355	-2.493
CARWSI	–	-	-2.366	-2.360
CARWSL	-	-	-2.367	-

Abbreviations: (AR) Gasoline retail(-1), (C) Constant, (W) Gasoline wholesale, (E) Ethanol, (I) IBC-BR, (L) Labor costs, (S) Error variance. All prices in the estimation were deflated using INPC (Brazilian consumer's inflation index)

Finally, for the conduct parameter, it's necessary to define a structural model formulated by a system of demand and pricing equations. For demand, we have a linear specification as follows:

$$q_t = \alpha + \beta_1 p_{gt} + \beta_2 p_{et} + \beta_3 y_t + \beta_4 v_t + \varepsilon_t \tag{4.8}$$

Where, q_t is the monthly quantity of gasoline (in m^3) sold in the market under consideration; p_{gt} and p_{et}, are, as before, gasoline retail and ethanol prices; y_t is the economic activity index; and v_t is the local vehicle fleet.

Then, there is a pricing equation (as usual, formed by marginal cost and marginal revenue equality):

$$p_{gt} = \lambda \frac{q_t}{\beta_1} + \gamma_0 + \gamma_1 p_{wt} + \gamma_2 w_t + \varepsilon_{pt} \tag{4.9}$$

p_{wt} and w_t are cost factors (gasoline wholesale price and labor); β_1 is a coefficient taken from the demand estimation; and λ is the conduct parameter. This parameter nests a set of possible market structures: competition, if equals 0, monopoly or perfect collusion, if equals 1; cournot-nash, if equals $1/N$, where N is the number of firms. Or it may represent an intermediate market power index if the values are different from benchmark models. To identify the regimes/periods highlighted by our approach as collusive or non-collusive, it's necessary to add dummies interacting with the conduct parameters (we found that Markov probabilities are more reliable to distinguish periods, so we use their results to set the dummies). Therefore, the pricing equation takes the form below:

$$p_{gt} = \sum_{p=1}^{n} dummy_{pt} \lambda_p \frac{q_t}{\beta_1} + \gamma_0 + \gamma_1 p_{wt} + \gamma_2 w_t + \varepsilon_{pt} \tag{4.10}$$

The subscript p is the period, n is the number of periods signaled by the Markov approach (in our setting, this n periods can be in two regimes, collusion or non-collusion), and λ_p is the conduct parameter specific for each period. In a specific period, if this parameter is close to 0, it is plausible that we should identify it as a competition phase. Values that differ from 0 may be interpreted as more collusive (keep in mind that this approach underestimates true conduct value and we do not expect values close to 1). To test if the variation between parameters/periods is significant, we applied Wald tests, comparing a period with the previous one. It's worth noting that the system of equations has endogenous variables (q_t and p_{gt}). We need to proceed with our estimation with instrumental variables in a two-step least-squares approach for unbiased coefficients. In our case, the instruments were excluded cost and demand factors and lagged gasoline and ethanol prices.

4.6 Results

This section will provide the main results from our econometric strategy, focusing on describing cartel behavior during antitrust enforcement (or what has been named post-cartel behavior). We will first present a brief historical context for each of the markets analyzed, highlighting some of the authorities' measures. Then we will confront these events with breaks estimated by Bai and Perron's test and with the smoothed probabilities of being in a collusion regime provided by the Markov switching method. Finally, it will be checked if the conduct parameters are sensitive to changes pointed out by our approach. Additional results from our estimations are in the chapter's appendices C.1 and C.2.

4.6.1 Brasília

Despite not being closed yet, the Brasília cartel case is, in many senses, extremely relevant in recent Brazilian antitrust history. First, this cartel managed to raise prices in the wealthiest and most highly educated city and the country's capital. It was operating literally in the neighborhood of the Antitrust Authority's (CADE) headquarters. Moreover, the cartel scheme was, somehow, common knowledge years before the antitrust action. Maybe because of these former aspects, CADE, and other authorities, acted with great strength during and after the Dubai Operation (the initial raid against the cartel's members), imposing preliminary penalties and conditions on firms and individuals that hadn't been seen in fuel markets before.

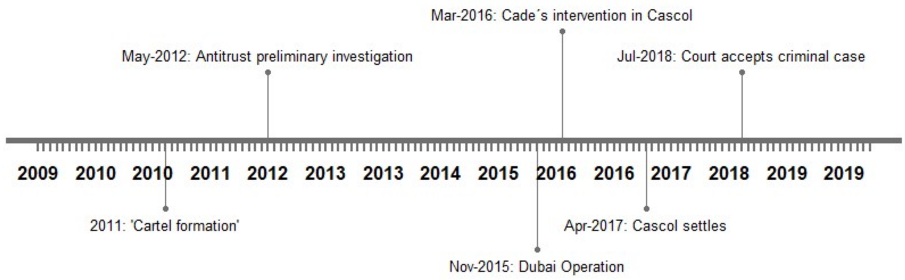


Figure 4.1: Brasilia - timeline

Officially, the cartel investigation began in 2009 after a complaint. In May 2012, the firms received a formal notification about the preliminary proceeding, and this, hypothetically, could be considered the first possible breakdown event. But, apparently, the cartel remained active. In November 2015, CADE and judicial authorities conducted the first phase of the Dubai Operation, which fulfilled dozens of search and seizure warrants, temporary arrests, and coercive bench.

After the operation, Cade continued to screen the market and found that the prices were still above the competitive level. Besides, the evidence gathered in the investigation demonstrated that the company Cascol, the leader in Brasilia's fuel retail market, was also one of the cartel's leaders. That's why, in March 2016, the antitrust authorities imposed a preventive intervention on the Cascol administration, appointing an independent administrator to manage the fuel stations. Also in 2016, in May, there was another raid, Dubai Operation II, which fulfilled more search and seizure warrants. In April 2017, Cade's Administrative Court approved a Termination Commitment Agreement signed with Cascol. Under this agreement, the company paid an amount of US$ 20 million in fees and made a disinvestment commitment, accepting to sell several of its fuel stations. In July 2018, the Brazilian federal court received the criminal charges against cartel members.

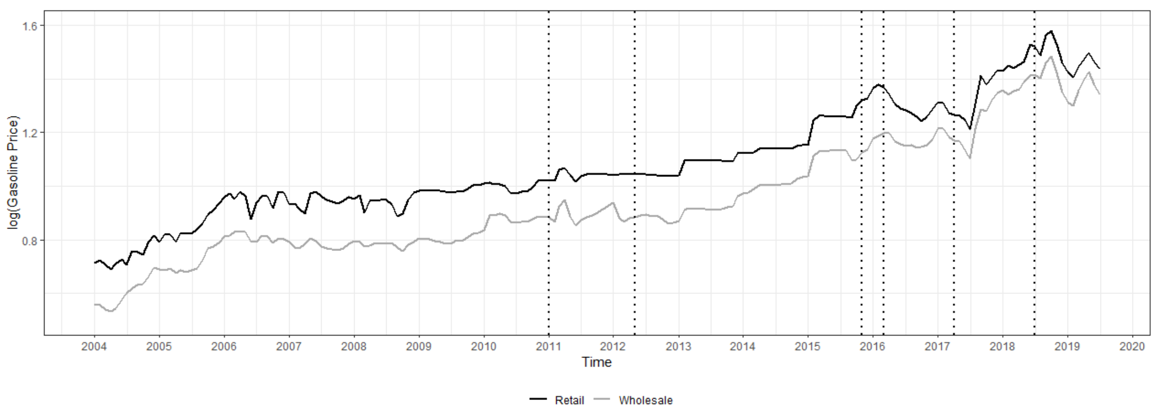


Figure 4.2: Brasília - gasoline price evolution (2004-2019). The dashed lines indicate cartel dates, as shown on the timeline.

When judicial authorities presented criminal charges at court, they considered that the cartel scheme began in early 2011. If we look at the price series (figure 4.2) we found a small price increase around January-April 2011, but it's temporary and follows an increase in wholesale values. In another relevant event date, May 2012 (when firms were informed about the antitrust investigation), prices remained stable, although there was some gasoline cost variation. Therefore, there is no apparent behavior indicating that the cartel was somehow affected. Going further, after the first phase of Dubai Operation, what is possible to retain is that prices were rising and that antitrust action didn't reverse this trend. Unlike, the next three events, especially CADE's intervention, were followed by decreases in prices, although it's not yet possible to disentangle them from wholesale price evolution. So, the price behavior is not informative; that's why we must rely on other approaches, hoping that they will give clear information.

Table 4.5: Brasília - structural breaks dates

h	Homogeneity								
0.10	may/10	aug/11	nov/12	-	-	may/15	may/16	-	feb/18
0.15	nov/10	-	nov/12	-	sep/14	-	mar/16	-	feb/18
h	Heterogeneity								
0.10	may/10	aug/11	nov/12	dec/13	-	may/15	may/16	may/17	mai/18
0.15	nov/10	-	nov/12	-	sep/14	-	mar/16	-	feb/18

Global vs m-breaks Bai and Perron Test (significant at level 5%)

Unfortunately, results from structural break tests are also unclear (table 4.5). The break dates vary greatly with trimming parameters. Even when there exists a match between them, there isn't certainty about what kind of regime (collusion or non-collusion) is operating between breaks. This problem is expected in our approach because the sample comes from a market where the collusion is probably unstable (given the high number of firms). Price-fixing schemes can operate for a long time, but they will be affected by recurrent price wars, and the number of breaks may highlight this. Despite unclear results, it's worth noting that there was no break in early 2011, nor around November 2015 (Dubai Operation). But there is a break around March-May 2016 when CADE appointed an independent administrator to manage Cascol stations.

In figure 4.3, we added another layer to the price evolution graph. The continuous black line indicates the smoothed probabilities of being in a collusion regime (resulting from the estimation of a Markov Switching Regression Model). The dashed black lines are the break dates resulting from Bai and Perron's test (with homogeneity in errors distribution and trimming parameter $h = 10$). It's worth noting that there are some coincidences between breakpoints and probability changes, which give credibility to our results. There is also another relevant aspect. Structural break tests lost several regime changes because they were too short (it identified only the beginning of 2010's apparent price war, and only the endpoint of another one in 2015, as an example). On the other hand, the structural break approach was sensible to behavior changes between 2011 and 2013, which did not affect the regime probabilities.

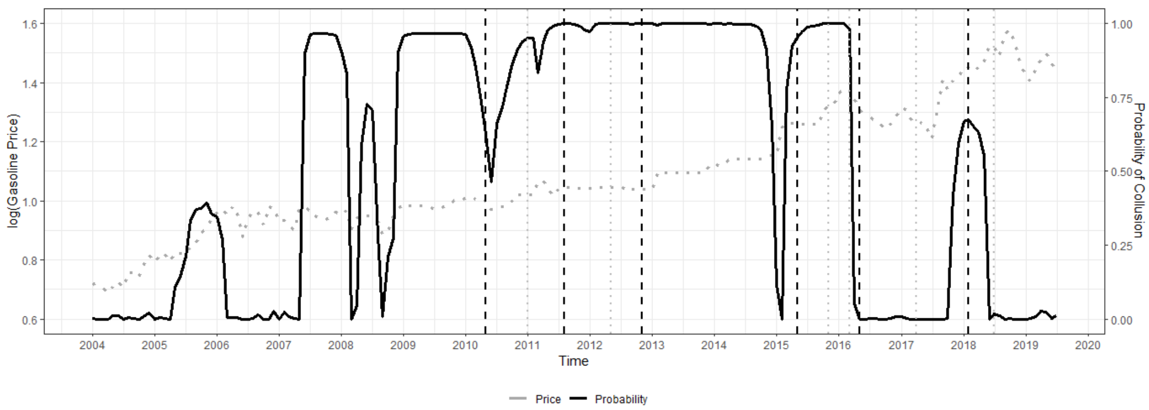


Figure 4.3: Brasília - collusion probabilities and breaks (2004-2019). The gray doted lines indicate the price evolution and official cartel dates, the black dashed lines are the breaks estimated in Bai Perron Test (homogeneity and $h = 10$).

On the cartel's behavior through antitrust litigation, it's possible to retain some points. First and most importantly, the initial raid against cartel members (Operation Dubai I) was ignored by the agents. They were supposedly re-establishing the cartel after a price war at the beginning of 2015 and remained with collusive practices after the antitrust action. But, apparently, measures taken after the Dubai Operation were enough to break up the cartel (and the intervention in the Cascol administration seems to have been the most effective). There was a perturbation in this competitive state around 2018, but at that time, Brazil suffered from a truck drivers' strike and saw fuel shortages, which may have affected the outcomes of the market. Second point: Brasília's cartel probably lasted longer than judicial authorities seem to believe. Our results indicate that it might have begun in 2007, not in 2011, and was relatively persistent until 2016.

Table 4.6: Brasília - conduct parameter

Periods	Parameter	F	p-value
Jan-2012 to Feb-2016	0.089	-	-
After Mar-2016	0.019	65.73	0.000

F-statistics are testing the significance of variation on conduct parameter between periods. Data from quantity for Brasilia market are more precise (include only gasoline sold at local stations), but observations are available only from 2012.

However, how can we be sure about the fact that state probabilities point out a collusive regime? Our approach took the conduct parameter variation to deal with this question. Until 2016, the conduct parameter was estimated at 0.089. It's not that high, but after March 2016, it fell considerably, and this change was highly significant, as can be checked in table 4.6. Until July 2019, the period covered by our sample, antitrust intervention can be considered successful. As previously stated, the paper written by Motta and Resende (2019) supports our findings. They applied a Diff-and-Diff approach using the same ANP database and found a decrease in gasoline prices of about 8% after the cartel breakdown in Brasília.

4.6.2 Belo Horizonte

Belo Horizonte's cartel had a fundamental difference compared with Brasilia's case: the antitrust authority was not a protagonist at the beginning of the investigation, in 2008. Mão Invisível (Invisible Hand) Operation, the inaugural accusation, was conducted by criminal authorities with the support of Brazilian federal police. There wasn't any market intervention, despite the fulfillment of temporary prison warrants. Only in 2017, CADE imposed fines on colluding firms and individuals after they signed a Termination Commitment Agreement.

In this cartel case, we must highlight some important dates: The judicial authorities considered that the cartel period spanned from March 2007 to April 2008; the police's dawn raid against scheme members happened in July 2008, with almost 30 prison warrants; firms and individuals settled with CADE in April 2017, and CADE's Administrative Court judged and condemned the cartel in April 2019. Scheme members were fined amounts that summed US$ 35 million.

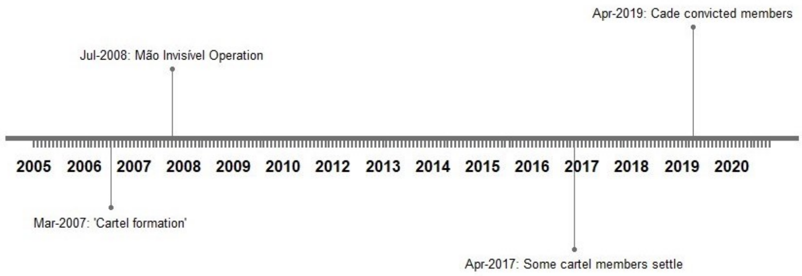


Figure 4.4: Belo Horizonte - timeline

Looking at price series (figure 4.5), we can identify a shift in the neighborhood of the official date of the cartel's beginning, but we are still struggling to disentangle the cartel effect from cost shocks. The police raid, surprisingly, isn't so evident in price evolution. On the other hand, when firms settled with CADE, prices showed a considerable rise pattern, although, once again, the movement is following the cost trend.

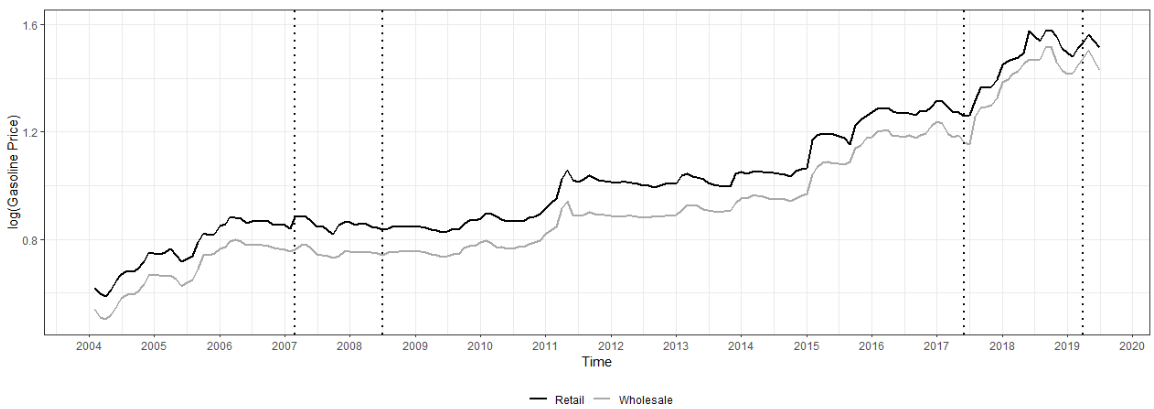


Figure 4.5: Belo Horizonte - gasoline price evolution (2004-2019). The dashed lines indicate the official cartel dates, as shown on the timeline.

Checking table 4.7, the structural break tests are still not so informative, with a significant divergence between the two trimming parameters. However, the relevant information here is that there is no break date that captured the dawn raid's effects or, later, the settlement or the administrative judgment. It's worth noting that November 2010, January-February 2013, and September-December 2015 seem to be signaling changes in Belo Horizonte's fuel market.

Table 4.7: Belo Horizonte - structural breaks dates

h	Homogeneity								
0.10	apr/08	-	nov/10	-	jan/13	jun/14	sep/15	dec/16	apr/18
0.15	-	jan/09	nov/10	-	feb/13	-	dec/15	-	oct/17
h	Heterogeneity								
0.10	apr/08	-	apr/10	aug/11	jan/13	jun/14	sep/15	dec/16	apr/18
0.15	-	jan/09	nov/10	-	feb/13		dec/15	-	oct/17

Global vs m-breaks Bai and Perron Test (significant at level 5%)

As in Brasília's case, the Markov Switching Regression method can provide a better overview of the cartel's behavior through time. Let's observe the data from figure 4.6. Once again, we saw some matching between structural breaks and regime changes in MRS probabilities, but shorter episodes are still missing in Bai and Perron's approach.

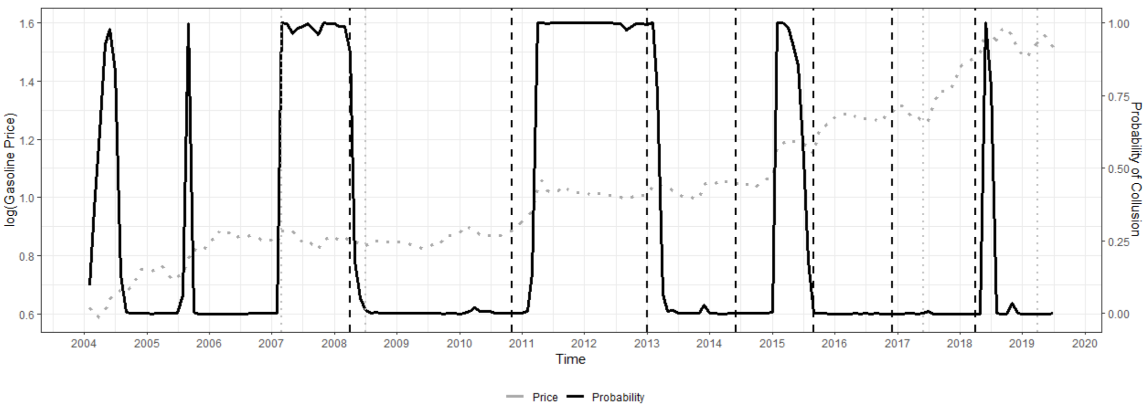


Figure 4.6: Belo Horizonte - collusion probabilities and breaks (2004-2019). The gray doted lines indicate the price evolution and official cartel dates, the black dashed lines are the breaks estimated in Bai Perron Test (homogeneity and $h = 10$).

What is significant about cartel behavior is that we do not know if the dawn raid broke the first cartel appearance in 2008 (it appeared to be naturally fading prior to the police operation). In the absence of antitrust measures, the market evolution apparently offered conditions to re-establish the collusive scheme three years later, at the beginning of 2011. Supposedly, the cartel faded away at the beginning of 2013 and reappeared briefly in 2015. Our findings also indicate that it has not been operating recently, but there isn't a clear relationship between its breakdown and the antitrust authority's direct actions. At best, we might state that the successive measures (like the Termination Commitment Agreement and the 2019 judgment) helped prevent the cartel's resurgence.

Table 4.8: Belo Horizonte - conduct parameter

Periods	Parameter	F	p-value
Jan-2004 to Feb-2007	0.052	-	-
Mar-2007 to Mar-2008	0.088	17.42	0.000
Apr-2008 to Jan-2011	0.033	33.33	0.000
Feb-2011 to May-2013	0.068	46.25	0.000
After Jun-2013	0.017	135.97	0.000

F-statistics are testing the significance of variation on conduct parameter between periods.

Finally, the conduct parameters in table 4.8 reinforce our findings. The values were higher for periods with high probabilities of being in a collusive regime (March 2007 to March 2008 and February 2011 to May 2013) and fell considerably after 2013. According to Wald's tests, all the parameter changes were significant.

4.6.3 São Luís

Like Belo Horizonte's cartel, in São Luís' case, the investigations, initiated in 2011, were held by the criminal authorities (Cronos Operation). The antitrust litigation was opened only in 2014 after CADE received transcripts of telephone wiretaps authorized by the court, as well as other evidence forwarded by criminal authorities. Recorded conversations showed that the owners of stations in São Luís agreed to set higher prices and induced other stations to do the same between February and March 2011. Also, the investigations found market-sharing agreements that were coordinated by the association of fuel retailers.

To proceed with our analysis, it's worth retaining the following essential events/date. For criminal and antitrust litigation, the cartel period was set between February and March of 2011, during which criminal authorities conducted the Cronos Operation (with telephone wiretaps). CADE filed the antitrust case in October 2014, and the administrative judgment occurred in June 2017. Firms and individuals that participated in the cartel were fined US$ 4,2 million approximately. Finally, in September 2018, fuel retailers settled in court and signed a commitment not to exchange information about prices.

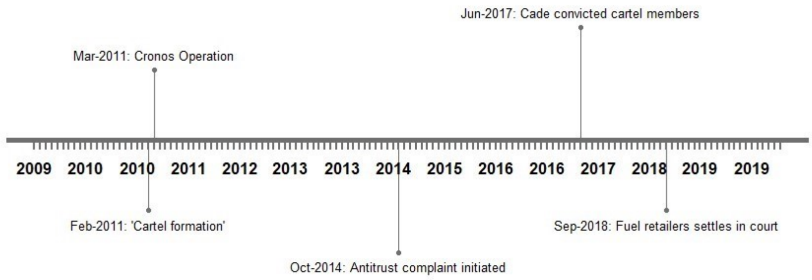


Figure 4.7: São Luís - timeline

If we take the price series in the São Luís fuel market (figure 4.8), it is possible to see a sharp increase during the beginning of 2011. The wholesale price also went up, but its pattern seems to be more moderate. However, this behavior did not appear to be sustainable, possibly because the criminal authorities made the cartel investigation public at the same time. Running through the chain of events, prices got higher after the beginning of the antitrust investigation and decreased following the two last events (CADE's judgment and agreement in court), but we don't yet have enough evidence to connect these movements with antitrust measures.

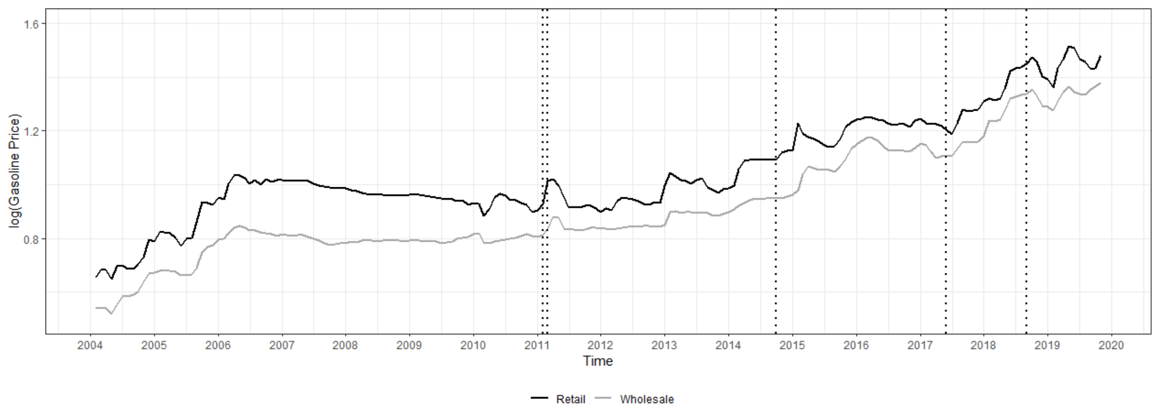


Figure 4.8: São Luís - gasoline price evolution (2004-2019). The dashed lines indicate the official cartel dates, as shown on the timeline.

With nothing but the break dates, our structural change approach (table 4.9) is even more confusing in São Luís' cartel case. However, the high number of breaks and the divergence between the two trimming parameters are strong evidence of the cartel's instability. Relevant events during antitrust litigation seem, instead, to be ignored by break dates. The only and most important exceptions are the dates from the beginning of 2011, which indicates that there were some market changes in the period considered by authorities as the cartel´s phase.

Table 4.9: São Luís - structural breaks dates

h	Homogeneity									
0.10	nov/08	-	mar/10	apr/11	nov/12	dec/13	-	jan/15	feb/16	mar/17
0.15	-	may/09	-	jan/11	sep/12	-	may/14	-	jan/16	-
h	Heterogeneity									
0.10	nov/08	-	mar/10	apr/11	nov/12	dec/13	-	jan/15	feb/16	mar/17
0.15	-	may/09	-	jan/11	sep/12	-	may/14	-	jan/16	-

Global vs m-breaks Bai and Perron Test (significant at level 5%)

Cartel's probability, plotted in figure 4.9, depicts a volatile scenario. The antitrust and criminal authorities were partially right in defining the beginning of the cartel in February or March 2011, but, in fact, the agreement between station owners probably was an attempt to re-establish the scheme that operated previously, between 2006 and 2010. This attempt wasn't totally successful, and there were successive comings and goings in price-fixing behavior. This behavior is a reasonable explanation for the results from the structural breaks method (which, by the way, are less precise but not so divergent with MRS findings).

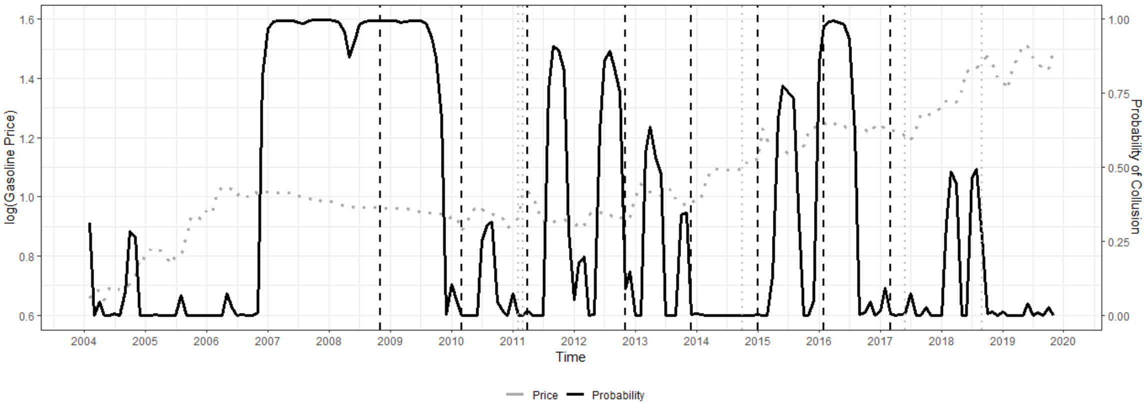


Figure 4.9: São Luís - collusion probabilities and breaks (2004-2019). The gray doted lines indicate the price evolution and official cartel dates, the black dashed lines are the breaks estimated in Bai Perron Test (homogeneity and $h = 10$).

We have evidence that actions against the cartel didn't end collusive behavior in the fuel market. It's relevant to notice that there was residual collusion with very probable cartel episodes until 2016. However, they are very short and less effective than the ones registered before 2010 (authorities' work may have created conditions for this instability, but, unfortunately, our approach is not enough to disentangle its effects from other market changes).

Table 4.10: São Luís - conduct parameter

Periods	Parameter	F	p-value
Jan-2004 to Oct-2006	-0.032	-	-
Nov-2006 to Dec-2009	0.059	63.89	0.000
After Jan-2010	-0.002	8.59	0.003

F-statistics are testing the significance of variation on conduct parameter between periods.

Once more, the conduct parameter method gave some certainty about our analysis, showing that the λ_p was higher between 2006 and 2009 (the cartel's stable period) and significantly decreased before 2010 (the phase with short collusive phases).

4.6.4 Londrina

Londrina's cartel was extensively analyzed by Cuiabano (2019), who found that the scheme managed to raise gasoline prices by 3.6% to 6.6% above the competitive level. Her estimates adopted, as a reference, information from antitrust charges, which indicate that the collusive period spanned from April-May to August 2007. The official scheme "end date" coincides with the Medusa III Operation, a dawn raid executed by Paraná state's police and antitrust authorities. This raid fulfilled 16 search and seizure warrants in stations located in Londrina and neighboring municipalities. A previous police investigation, with telephone wiretaps, showed that collusion started when one of the retailers dropped its price and started a price war between stations in the Londrina region. So, in April-May 2007, fuel retailers initiated conversations to agree on price increases and readjustment dates. Cartel's leaders used the retailer's association to expand their agreement to all associates.

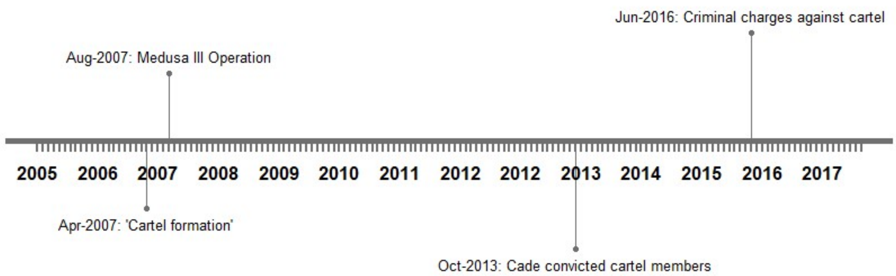


Figure 4.10: Londrina - timeline

In addition to the cartel period and Medusa III operation, we must retain two more relevant events. CADE administrative court convicted the collusive behavior six years later, in October 2013, with penalties summing up to more than US$ 2,5 million. Criminal authorities presented charges against the cartel after nine years, in June 2016. There was one last event. In February 2020, Paraná's court extinguished the case because they exceeded the maximum time for a criminal case, but our data set doesn't include this period.

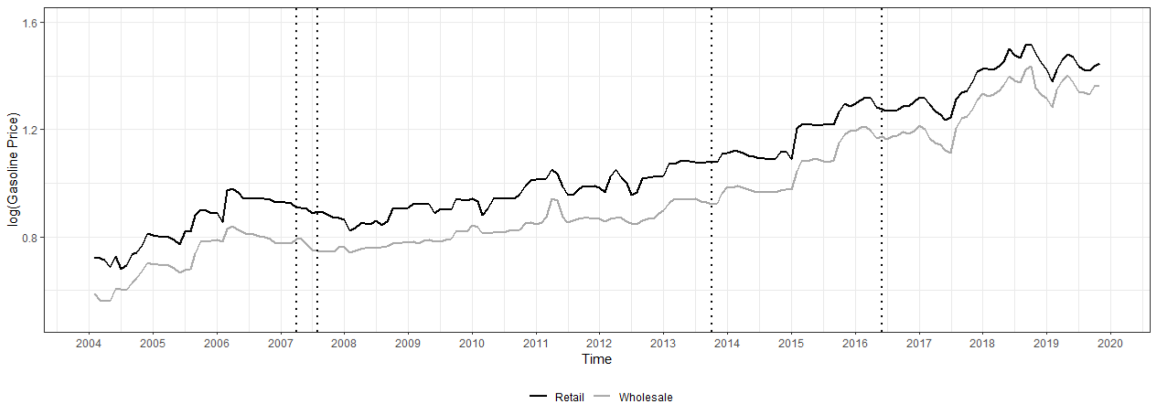


Figure 4.11: Londrina - gasoline price evolution (2004-2019). The dashed lines indicate the official cartel dates, as shown on the timeline.

Taking a naive approach, observing the price series doesn't offer anything more than some clues about the effects of antitrust action, as stated in all previous cases. The structural change method, instead, provides more pieces of information, although it is noisy. There aren't any structural changes in the neighborhood of August 2007 (Medusa III Operation), but there are shifts around October 2013 and June 2016.

Table 4.11: Londrina - structural breaks dates

h				Homogeneity					
0.10	aug/08	dec/09	may/11	aug/12	nov/13	apr/15	aug/16	–	aug/18
0.15	–	jun/09	aug/11	–	jul/13	oct/15	–	dec/17	–
h				Heterogeneity					
0.10	aug/08	dec/09	may/11	aug/12	nov/13	apr/15	aug/16	–	aug/18
0.15	–	jun/09	aug/11	–	jul/13	oct/15	–	dec/17	–

Global vs m-breaks Bai and Perron Test (significant at level 5%)

Adding another layer again to our price evolution graph to show the collusion probabilities and the structural breaks (figure 4.12), what we get is bad news for antitrust authorities. The cartel formation captured in criminal authorities' telephone wiretap was, actually, an attempt to avoid the cartel breakdown caused by the price war registered at the beginning of 2007. It's worth recognizing that the Medusa III Operation may have been the cause of the temporary end of the cartel between 2008 and 2009, but our results show that the collusion behavior was persistent, with some instability, until the end of 2015. The Markov switching probabilities also revealed a relatively mild disturbance after the cartel's conviction at CADE's Administrative Court. However, the collusive scheme kept operating for another two more years, and it's quite likely that the recent period of competition is due to other market conditions not necessarily related to antitrust policy.

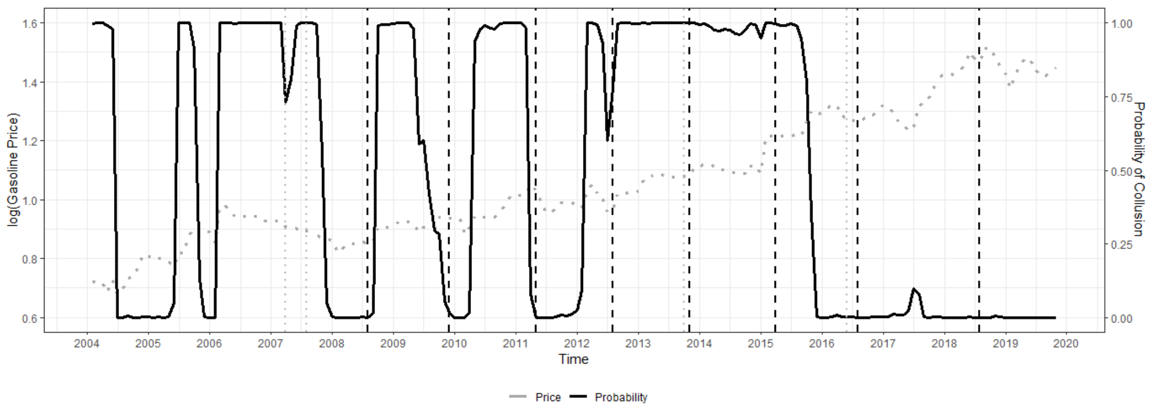


Figure 4.12: Londrina - collusion probabilities and breaks (2004-2019). The gray doted lines indicate the price evolution and official cartel dates, the black dashed lines are the breaks estimated in Bai Perron Test (homogeneity and $h = 10$).

Finally, to test our results' reliability, we ran, once more, the conduct parameter estimation over the regime changes identified by switching regressions. Our findings showed that, between June 2005 and December 2007, λ_p was higher than in any other period. Relying on our parameter estimation, it's also possible to say that there was a competitive phase from January until July 2008. On some levels, this market tendency changed to a more collusive pattern after 2008, a new regime that, with some price wars, was prevalent until the end of 2015. All parameter changes were statistically significant for Londrina's sample.

Table 4.12: Londrina - conduct parameter

Periods	Parameter	F	p-value
Jan-2004 to May-2005	0.072	-	-
Jun-2005 to Dec-2007	0.121	17.08	0.0000
Jan-2008 to Jul-2008	0.023	63.32	0.0000
Aug-2008 to Dec-2015	0.064	16.02	0.0000
After Jan-2016	0.027	49.15	0.0000

F-statistics are testing the significance of variation on conduct parameter between periods.

4.7 Policy aspects and conclusions

Concerns about analyzing antitrust policy effectiveness have been increasingly considered in the empirical literature and the government agents' practice. As Ordóñez-de Haro and Torres (2014) points out, this assessment should be made considering the ability to prevent, identify, and punish anti-competitive behavior. But punishment policies are not so valued if they fail to restore adequate competition levels in target markets, improving consumers' welfare and the social distribution of the wealthy, which is crucial for unequal countries like Brazil.

Usually, these policies' success has been measured through methodologies focused on estimating the impact of antitrust enforcement on prices. From this point of view, the empirical results have been relatively mixed. It's not uncommon cases where the outcome of litigation against price-fixing schemes is well below expectations. The resultant heterogeneity might be explained by a whole spectrum of possible post-cartel behaviors: during antitrust litigation, markets can face a hysteresis effect, changing pricing strategies to avoid high penalties; they can also observe a kind of tacit collusion; or, if conditions are favorable to conspiracy, the cartel may be re-established. That's why studying the market dynamics after the authorities' raid against a specific cartel is crucial for IO and competition policy literature. For this purpose, models that try to define the evolution of the affected market endogenously are fundamental, including to attest the credibility of the results obtained with the most traditional approaches.

As shown before, when relying only on official documents to define dates and events that will impact their estimation process directly, researchers are severely exposed to the possibility of ending up with misleading results, as correctly argued Boshoff and van Jaarsveld (2019) and Boswijk et al. (2019). That's why, in this chapter, we left aside the concern of just measuring the effect on prices and adopted a broad overview through market evolution under antitrust litigation, using techniques such as structural break tests and Markov switching regressions to analyze whether there was any observable consequence of the action of the Brazilian antitrust authorities in the retail gasoline market. In addition to its relevance to competition policy in general, this work also contributed to applied research on the sense that, as far as we know, it's the first to systematically compare two endogenously cartel dating approaches and, further, to propose combining them with the IO model of conduct parameters to help identify collusion regimes and, somehow, check the reliability of the results from structural breaks and switching regression approaches.

Regarding the methodological issues, our findings showed that Markov regressions were more robust for the purpose of scrutinizing the evolution of price-fixing schemes in fuel markets. This is so because its results are more sensible, more straightforward, and less likely to miss shorter regime changes. The structural break test's significant problem is its need for a minimum size between break intervals, which strongly depends on our sample's number of observations. Since we have theoretical and empirical reasons to suspect that collusion in fuel markets is, although common, unstable, Bai and Perron's analysis ends up being too sensitive to trimming parameter choice. When the minimum size between breaks is decreased, the test obtains many break dates, and it's hard to interpret without previous knowledge. With larger trimming parameters, the results have better convergence properties but fail to identify some episodes. We ran a simple simulation comparing methodologies' performance in two regime series with low and high persistence to illustrate this point. Results can be checked in the appendix C.3.

On the other hand, it can be argued that only two regimes (collusive or non-collusive) in the fuel market samples are artificially defined, which would be the downside of the MRS.

Unfortunately, the type of data generated in this market doesn't have enough variations to estimate three or more regimes (collusion, competition, and price war, for example). In the end, we must recognize that sometimes there is a trade-off, and maybe the best option is to combine the two techniques, even if one of them is playing the role of a reliability test.

On the competition policy aspects, this work presented some extremely relevant findings. If our models are reasonably accurate, the current policy's ability to restore the expected levels of competition in the cartel-hit market is seriously in doubt. In three out of four cases studied (Belo Horizonte, São Luís, and Londrina), antitrust action had, at most, restricted effects. There is evidence that dawn raids sometimes temporarily disrupt collusion agreements but do not extinguish them if market conditions remain the same. Also, the impact of fines imposed on scheme participants is mitigated when established after years of litigation (and, in Brazil, they are always questioned in court). Brasília's case, however, seems paradigmatic of how strong preventive measures combined with structural ones, aimed at market reorganization, are supposed to have lasting effects against price-fixing behavior.

This lesson goes in the same direction advanced by one of the experiments mentioned earlier in this chapter (Chowdhury and Crede, 2020). A history of price coordination reduces uncertainty between agents involved in collusion and strengthens the cartel's capacities, even allowing it to take a tacit form. So, it's often necessary for the antitrust authorities to act breaking down entry barriers and adding new players to the market (a rematching, as happened when CADE imposed on Cascol the disinvestment in some of its fuel stations). Market forces are more effective in deterring cartel formation. The recent changes in Petrobras pricing policy, that took place in 2016, are one example, as they may be one of the reasons for the contemporaneous lower prevalence of collusion. Unfortunately, our empirical design does not sufficiently control this hypothesis to make any causal relations, which should be an issue for further research.

5

Conclusion

The relationship between market power and inequality is reasonably intuitive. As stated by Stiglitz (2017), there is a (supposedly) clear and simple mechanism to explain why market concentration impacts income distribution regressively and, therefore, why antitrust should care about these issues: "the monopolist's monopoly rents come at the expense of consumers; as monopolies raise their prices, their profits increase while the well-being of consumers and workers decreases; an increase in market power is associated with an increase in inequality". Nevertheless, in the real economy, the answer is more complex. Despite some evidence favoring the regressive effect, the impact of the lack of competition on inequality is an open debate. In developed countries, the firm's shareholding is distributed, to some degree, through the income classes, and labor unions generate a rent-sharing process between workers and owners. So, all gains from monopoly rents are not necessarily going to those at the top of the distribution. While market power can harm the poor, the middle and working classes may have some benefits, and the net effect on income distribution may vary between several countries.

In contrast, developing countries like Brazil have income and wealth even more concentrated at the top. They also adopted development policies that weakened market competition (import substitution). Additionally, in general, workers' unions are less widespread and have less power in labor markets (to a large extent, they represent occupations already at the top of the distribution, such as public employees). Finally, Brazilian competition policy has a recent implementation (during the 1990s) and, therefore, antitrust authorities may not be strong enough to face markets' distortions effectively. These facts lead us to predict a more directly deleterious relationship between market power and income inequality in developing countries.

The purpose of this thesis was to evaluate, both theoretically and empirically, these assumptions regarding the interaction between market power and inequality in Brazil. In its theoretical stage, our work presented a dynamic general equilibrium model in which workers and consumers were heterogeneous in their access to the asset market and in their skills and labor supply elasticities. This model also featured endogenous firms' market power in goods and factors (labor) markets. The model's dynamic behavior showed that an unexpected shock in the firm's markup (higher market power) is regressive, transferring income from the bottom to the top of the distribution. Its effects on economic growth may be positive in the short term, though, due to the increased investment in new companies. On the other hand, economic growth, generated by the TFP increase, reduces inequality due to the countercyclical markups.

An exciting aspect of the modeling we have proposed is that labor supply heterogeneity has a relevant impact on income distribution between poor and middle-class households. Households with less elastic supply have lower markdowns and suffer more from oligopsonistic power. Instead, they benefit more when the economic growth generated by the increased TFP tightens market competition. Another relevant finding has implications for antitrust policy. Our model presented, as an output, a non-linear relationship between concentration and inequality. Given that, antitrust authorities may look carefully at mergers when the number of competitors or the HHI is already small, as the regressive markup-inequality path is exponential. Finally, our theoretical exercise highlighted the repercussions of oligopsonistic behavior on labor markets.

The thesis's second chapter was mainly empirical. As predicted by our model, initially, it revealed that a proxy for firms' market power (local employment HHI) positively correlates with Brasilian municipalities' Gini index (regressive impact on income distribution) and dampens local labor markets' average wages. The fixed-effect IV regressions of Gini on HHI found that municipalities in the high concentration group (HHI above 7,500) have a level of inequality approximately 3% higher than municipalities with moderate HHI (2,500). At the municipality-industry level (local labor market), with Bartik IV, we estimated that an increase of 10% in concentration leads to an economically and statistically significant reduction of approximately 1% in the mean wage.

In the second chapter's last part, quasi-experimental design results went in the same direction. Our differences-in-differences approach evaluated wages' response to a merger of two firms in the Brazilian banking sector and found a reduction of between 2% and 6% in wages over the three periods following transaction approval. These values partially hold, especially in the previously highly concentrated market, when we adopt strategies to control labor market composition effects. If there are concerns about income inequality, our results may motivate Brazilian antitrust authorities to reform their processes, increasingly incorporating labor market analysis into their reviews

Our third chapter also suggests a reappraisal of antitrust authority processes. Our empirical results indicate its inability to effectively combat collusion in some sectors. The chapter proposed to evaluate the actions against cartels in four regional fuel markets in which CADE, the Brazilian antitrust authority, has opened lawsuits related to price-fixing infractions. We chose to study fuel markets because expenses on transportation represent a share of 18% of Brazilian households' budget (higher than spending on food), and this sector is, undoubtedly, responsible for the most significant number of complaints and prosecutions related to collusion practices in the country.

Our findings are pretty relevant. Antitrust action had disappointing effects in most studied cases (Belo Horizonte, São Luís, and Londrina). The evidence shows that dawn raids sometimes temporarily disrupt collusion agreements but do not extinguish them if market conditions remain unchanged. Nevertheless, Brasilia's cartel is one emblematic case, which may be a helpful example for future actions. In this case, CADE took preventative measures, combined with structural ones, aimed at market reorganization. According to our Markov Regressions model, antitrust prosecutions seem to have lasting effects on price-fixing behavior in this specific cartel. Brasilia's lesson goes in the same direction advanced by the experiments in Chowdhury and Crede (2020). A history of price coordination reduces uncertainty between agents involved in collusion and strengthens the cartel's capacity to restore the scheme. So, fines are not necessarily sufficient, and the antitrust authority may have to impose measures to break down entry barriers, thus adding new players into the market.

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Appendix A

Chapter 2

A.1 Alternative models

As shown in the Chapter 2, our model proposes adding some frictions to the classic general equilibrium framework in order to reveal possible mechanisms through which market power, on both goods and inputs (labor) side, affects general macroeconomic indicators and, in particular, the functional distribution of income generated by the economy (with focus on developing countries like Brazil).

Among these modeling options, it is worth highlighting the existence of three types of agents, with heterogeneity in their access to the asset market and their marginal contribution to the firms' output. In addition, two of the existing agents in this economy face an oligopsonistic structure in the input (labor) market. Thus, to deepen our analysis regarding these frictions' impact on outcomes, we established three alternative versions of the base model, with specific changes related to each of the characteristics under scrutiny.

In the first alternative specification, households still have labor skills heterogeneity, so there is no change in the intermediate-goods firms' production function. However, now these same firms do not have market power in the input-labor market anymore. Each type of household (capitalists, constrained optimizers, and hand-to-mouth) supplies differentiated labor (j_l) to type-specific unions that bundle this labor and sell it to firms. These union aggregate differentiated labor using a Dixit and Stiglitz (1977) function below:

$$l_t^i = \left(\int_0^1 l_t^i(j_l^i)^{\frac{\phi_w^i - 1}{\phi_w^i}} dj_l^i \right)^{\frac{\phi_w^i}{\phi_w^i - 1}} \tag{A.1}$$

Where ϕ_w^i is the elasticity of substitution among the differentiated workers from each type of household. The latter is a standard New Keynesian model's way to add workers market power, but since we do not have wage stickiness in our model, the solution for household problem giver a simple labor supply function with a wedge:

$$w_t^i = \frac{\phi_w^i}{\phi_w^i - 1} U_t^i \theta^i l_t^{i\chi^l} \tag{A.2}$$

The second alternative model maintains the same monopolistic competition structure in the inputs (labor) market but lost all heterogeneity in the workforce. There are no different substitution elasticities in this new formulation, as all types of households provide differentiated workers for the same aggregator union. In addition, there are no productivity differentials in the production function; the following specification replaces the former:

$$y_t(\iota, j) = A_t k_{t-1}(\iota, j)^\alpha L_t(\iota, j)^{1-\alpha} \tag{A.3}$$

The aggregation of labor provided by the households (Top 1%, Middle 49% and Bottom 50%) also needs to assume a new aspect:

$$L_t = l_t^o \, \omega^o + l_t^h \, \omega^h + l_t^c \left(1 - \omega^o - \omega^h\right) \tag{A.4}$$

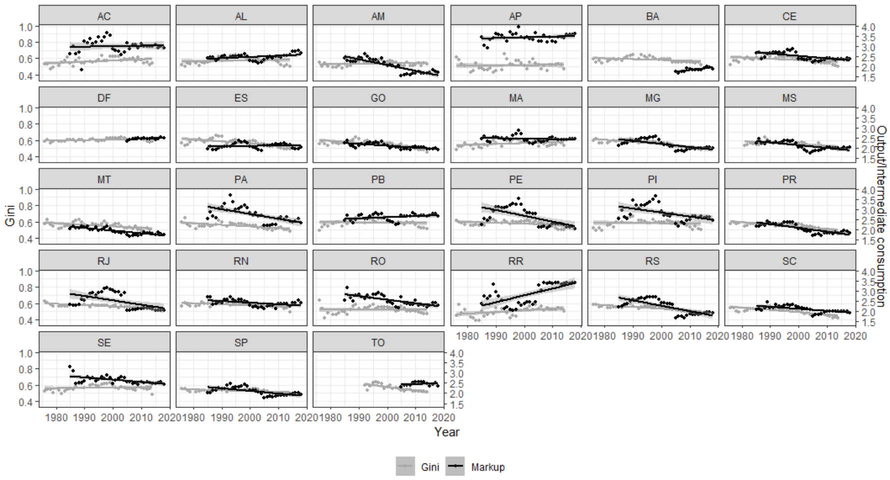
The third and last modification aims to assess the impacts of the number of agents in the model. With this objective in mind, we rescued the base model and made only one significant change, reducing the number of households. Only the two standard agents from TANK models remained in our framework: hand-to-mouth household, which does not have access to the asset market, and Ricardians household, the one that smooth consumption through savings and investment instruments. Therefore, the problem of non-Ricardian households (equivalent to the bottom 50% of the previous model) remains the same. However, there is now a specific budget constraint for the new Ricardian agent, which incorporates all the economy's assets; bonds, physical capital, and the companies' ownership (and, consequently, investment in new capital, new good, and in the creation of new firms):

$$c_t^R + b_t^R + \upsilon N_{et}^R + i_t^R = r_{t-1} b_{t-1}^R + w_t^R l_t^R + \pi_t N_t^R + r_t^k k_{t-1}^R - t_t \tag{A.5}$$

It is worth noticing that this last model still has oligopsonistic competition in the labor market, but it is faced only by hand-to-mouth agents since Ricardian households are in most aspects similar to the base model's capitalists, and so supply work hours in a perfectly competitive structure.

A.2 Sub-national Trends

Figure A.1: Brazilian states - Estimated Markup (regional accounts) and Inequality trends (2000-2015)



Source: IBGE - Regional Accounts; IPEA

A.3 PVAR: additional result

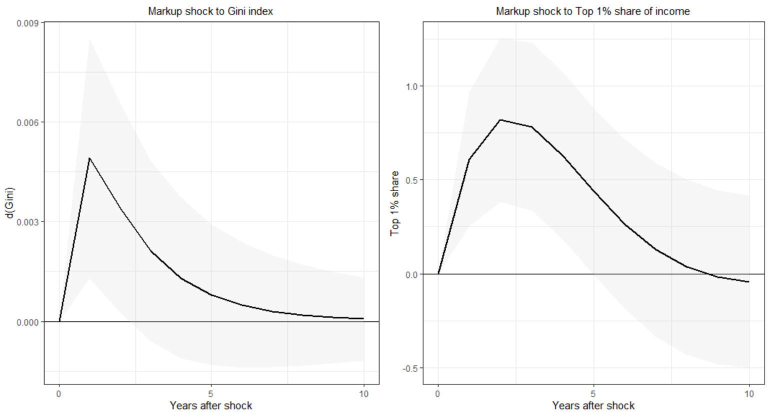


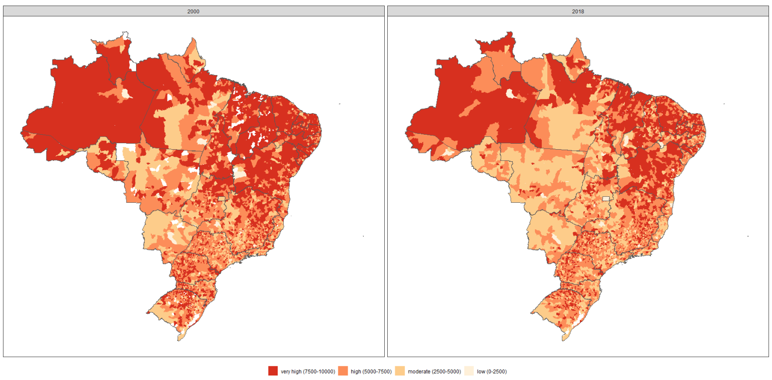
Figure A.2: Orthogonal impulse-response functions from two variate specification (Cholesky ordering: Inequality - Markup). GMM Panel VAR (N=27, T=22) with standard errors from simulation (1000 repetitions) - 90% confidence interval.

Appendix B

Chapter 3

B.1 HHI evolution

Figure B.1: Map - HHI evolution - Brazilian Municipalities, 2000-2018



Source: Author's computations

B.2 Concentration and inequality

Table B.1: HHI and Gini model - descriptive statistics (municipality level)

2000 (N=5,461)				
Variable	Mean	Std. Dev.	Min.	Max.
Gini	54	0.68	30	87
HHI	7,633	1,961	1,416	10,000
Formal employment share (%)	23.64	16.21	0.00	78.24
Public sector share (%)	6.51	4.26	0.00	44.22
Income (pc, BRL)	349.05	188.30	77.52	1759.76
Unemployment rate (%)	11.09	6.16	0.00	59.17
Illiteracy rate (%)	21.66	12.38	0.91	59.95
Population	30,973	187,389	795	10,437,203
Rural population share (%)	40.85	23.32	0.00	100.00
2010 (N=5,564)				
Variable	Mean	Std. Dev.	Min.	Max.
Gini	49	0.6	28	80
HHI	7,288	1,879	1,062	10,000
Formal employment share (%)	30.25	18.05	0.90	83.21
Public sector share (%)	6.61	4.34	0.00	42.08
Income (pc, BRL)	498.88	240.98	112.20	2,043.74
Unemployment (%)	6.74	3.82	0.00	41.93
Illiteracy rate (%)	16.15	9.83	0.95	44.40
Population	34,283	203,130	805	11,253,503
Rural population share (%)	36.16	22.03	0.00	95.82

Table B.2: Effect of market concentration on Income Inequality - regression table

Dependent Variable:	log(Gini)	
Model:	TW FE	IV
log(HHI)	0.019**	0.036**
	(0.009)	(0.017)
Formal employment share (%)	-0.004***	-0.004***
	(0.0003)	(0.0003)
Public sector share (%)	-0.001**	-0.001**
	(0.0005)	(0.0005)
Income (pc, BRL)	0.0006***	0.0006***
	(0.00002)	(0.00002)
Unemployment rate (%)	0.003***	0.003***
	(0.0003)	(0.0003)
Illiteracy rate (%)	-0.009***	-0.009***
	(0.0006)	(0.0006)
Rural population share (%)	0.236***	0.237***
	(0.029)	(0.030)
Municipality FE	Yes	Yes
Year FE	Yes	Yes
IV	No	Yes
Standard-Errors:		
Clustered by municipality (id)		
Observations	9,458	9,458

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table B.3: Effect of market concentration on Income Inequality - IV first stage

Dependent Variable:	log(HHI)
log(IV)	0.959***
	(0.037)
Formal employment share (%)	-0.001***
	(0.0004)
Public sector share (%)	0.0008
	(0.0008)
Income (pc, BRL)	-0.00009***
	(0.00003)
Unemployment rate (%)	-0.0002
	(0.0006)
Illiteracy rate (%)	0.002***
	(0.0009)
Rural population share (%)	-0.051
	(0.044)

Kleibergen-Paap Wald test (1st stage): 654.0
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

B.3 Concentration and wages

Table B.4: HHI and Wages model - descriptive statistics (labor market level)

Variable	Mean	Std. Dev.	Min.	Max.
Mean wage (BRL)	1,596	1,205	0.00	99,582
HHI	7,157	3,298	2	10,000
Number of industries (per mun.)	96.79	126.81	1	1,178
Population (mun. level)	35,369	208,635	786	12,176,866
Mean tenure (months)	38.23	37.02	0.00	596.9
Mean age	34.31	7.54	0.00	108
Female (% of employees)	39.68	35.53	0.00	100
College education (% of employees)	11.73	22.36	0.00	100

Observations: 7,004,694 (yearly, from 2006 to 2018)

Table B.5: Effect of market concentration on wages - regression table

Dependent Variable:	log(Wage)			
Model:	TW FE	IV	Microrreg. (IV)	Occup. (IV)
log(HHI)	0.000	-0.094***	-0.062***	-0.121***
	(0.0004)	(0.005)	(0.004)	(0.018)
Mean tenure (months)	0.0006***	0.0008***	0.0009***	0.001***
	(0.000009)	(0.00001)	(0.00002)	(0.00003)
Mean age	0.005***	0.005***	0.005***	0.006***
	(0.00003)	(0.00005)	(0.00009)	(0.00007)
Female (% of employees)	-0.126***	-0.136***	-0.167***	-0.122***
	(0.0008)	(0.001)	(0.002)	(0.002)
College education (% of employees)	0.041***	0.046***	0.060***	0.045***
	(0.0002)	(0.0003)	(0.0006)	(0.0004)
Population	-0.0000002***	-0.0000002***	-0.0000001***	
	(0.00000001)	(0.00000001)	(0.000000007)	
Mun-Ind FE	Yes	Yes	No	No
Micro-Ind FE	No	No	Yes	No
Mun-Ocup FE	No	No	No	Yes
Year FE	Yes	Yes	Yes	Yes
IV	No	Yes	Yes	Yes
Standard-Errors:	Clustered by labor market (id)			
Observations	6,707,076	4,350,929	1,749,515	3,355,276

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table B.6: Effect of market concentration on wages - IV first stage

Dependent Variable:	log(HHI)
log(IV)	0.089***
	(0.0009)
Mean tenure (months)	0.001***
	(0.00001)
Mean age	0.0001*
	(0.00007)
Female (% of employees)	-0.019***
	(0.002)
College education (% of employees)	0.0006
	(0.0005)
Population	-0.0000004***
	(0.00000003)

Kleibergen-Paap Wald test (1st stage): 10,859.4
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table B.7: Gini and Wages - quadratic models

Dependent Variable: Model:	log(Gini) (1)	log(Wage) (2)
log(HHI)	1.539497	-0.186***
	(0.964976)	(0.068)
log(HHI)^2	-0.085853	0.005
	(0.055103)	(0.004)
Standard-Errors:	Clustered (id)	
Observations	9,458	3,924,409
KP Wald test (1st stage) log(HHI)	5,382.2	342.0
KP Wald test (1st stage) log(HHI)^2	5,341.0	356.7

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Appendix C

Chapter 4

C.1 Markov Switching transition matrices

Table C.1: Brasília - transition matrix

	Competition	Collusion
Competition	0.932	0.068
Collusion	0.065	0.935
Expected durations	14.831	15.267

Table C.2: Belo Horizonte - transition matrix

	Competition	Collusion
Competition	0.955	0.045
Collusion	0.127	0.873
Expected durations	22.374	7.857

Table C.3: São Luís - transition matrix

	Competition	Collusion
Competition	0.93	0.07
Collusion	0.149	0.851
Expected durations	14.279	6.722

Table C.4: Londrina - transition matrix

	Competition	Collusion
Competition	0.93	0.07
Collusion	0.071	0.929
Expected durations	14.378	14.051

C.2 Structural model (conduct parameter) outputs

Table C.5: Brasília - Structural model (conduct parameter)

Demand Eq.		Pricing Eq.	
Constant	0.9708	Constant	1.529656***
	(2.097)		(0.378)
Gasoline Retail Price	-3.281***	Gasoline Wholesale Price	0.9002863***
	(0.1494)		(0.051)
Etanol Price	2.248***	Labor Costs	-0.0003763***
	(0.273)		(0.0001)
Cars	0.000036***	λ_p: Jan-2012/Feb-2016	0.089*
	(0.000003)		(0.052)
Economic Activity Index	0.068***	λ_p: After Mar-2016	0.018
	(0.0090)		(0.048)
R^2	0.881	R^2	0.898
Observations	78	Observations	78
Period	2012M01 2019M07	Period	2012M01 2019M07

*Dependent variables: sales and gasoline retail prices, respectively (source: ANP). Instruments: excluded cost and demand factors and lagged gasoline and ethanol prices Coefficients with ***, **, and * are significant at level 1, 5, and 10%.*

Table C.6: Belo Horizonte - Structural model (conduct parameter)

Demand Eq.		Pricing Eq.	
Constant	7.548***	Constant	0.297***
	(0.969)		(0.088)
Gasoline Retail Price	-2.83***	Gasoline Wholesale Price	1.001***
	(0.176)		(0.020)
Etanol Price	1.95***	Labor Costs	0.0001
	(0.205)		(0.00008)
Cars	0.00000046***	λ_p: Jan-2004/Feb-2007	0.052**
	(0.00000012)		(0.021)
Economic Activity Index	0.032***	λ_p: Mar-2007/Mar-2008	0.088***
	(0.005)		(0.021)
		λ_p: Apr-2008/Jan-2011	0.033**
			(0.016)
		λ_p: Feb-2011/May-2013	0.067***
			(0.013)
		λ_p: After Jun-2013	0.017
			(0.013)
R^2	0.875	R^2	0.968
Observations	179	Observations	180
Period	2004M01 2018M12	Period	2004M01 2018M12

*Dependent variables: sales and gasoline retail prices, respectively (source: ANP). Instruments: excluded cost and demand factors and lagged gasoline and ethanol prices Coefficients with ***, **, and * are significant at level 1, 5, and 10%.*

Table C.7: São Luís - Structural model (conduct parameter)

Demand Eq.		Pricing Eq.	
Constant	-0.612***	Constant	-1.061 ***
	(0.158)		(0.033)
Gasoline Retail Price	-0.415***	Gasoline Wholesale Price	1.419***
	(0.021)		(0.039)
Etanol Price	0.542***	Labor Costs	0.001
	(0.022)		(0.0007)
Cars	0.000003***	λ_p: Jan-2004/Oct-2006	-0.032
	(0.0000001)		(0.033)
Economic Activity Index	0.011***	λ_p: Nov-2006/Dec-2009	0.059*
	(0.0008)		(0.033)
		λ_p: After Jan-2010	-0.002
			(0.015)
R^2	0.980	R^2	0.951
Observations	180	Observations	191
Period	2004M01 2018M12	Period	2004M01 2018M12

*Dependent variables: sales and gasoline retail prices, respectively (source: ANP). Instruments: excluded cost and demand factors and lagged gasoline and ethanol prices Coefficients with ***, **, and * are significant at level 1, 5, and 10%.*

Table C.8: Londrina - Structural model (conduct parameter)

Demand Eq.		Pricing Eq.	
Constant	2.34***	Constant	0.217
	(0.240)		(0.210)
Gasoline Retail Price	-0.362***	Gasoline Wholesale Price	1.030***
	(0.043)		(0.035)
Etanol Price	0.221***	Labor Costs	0.0006
	(0.032)		(0.0004)
Cars	0.0000008***	λ_p: Jan-2004/May-2005	0.072**
	(0.0000001)		(0.036)
Economic Activity Index	-0.004***	λ_p: Jun-2005/Dec-2007	0.121***
	(0.001)		(0.035)
		λ_p: Jan-2008/Jul-2008	0.023
			(0.040)
		λ_p: Aug-2008/Dec-2015	0.064*
			(0.033)
		λ_p: After Jan-2016	0.027
			(0.030)
R^2	0.692	R^2	0.941
Observations	170	Observations	180
Period	2004M01 2018M12	Period	2004M01 2018M12

*Dependent variables: sales and gasoline retail prices, respectively (source: ANP). Instruments: excluded cost and demand factors and lagged gasoline and ethanol prices Coefficients with ***, **, and * are significant at level 1, 5, and 10%.*

C.3 Simple simulation: MRS and structural changes

Aiming to demonstrate the relationship between the Markov Switching Regressions technique and structural break tests, we performed a simple simulation exercise. Suppose that, in some markets, retail prices follow a markovian DGP, with two regimes and the following deterministic formulations:

$$p_t = \begin{cases} 0.2 + 1.1c_t - 0.05d_t, & s_t = 1(collusion) \\ 0.0 + 1.1c_t + 0.05d_t, & s_t = 2(non-collusion) \end{cases} \tag{C.1}$$

Where c_t represents the costs and d_t the demand shifters, and S_t is the discrete state variable that defines the regime. The first equation represents a collusive regime ($s_t = 1$), in which there is an increase in the price level (intercept) and, as in Rotemberg and Saloner model, there is an anti-cyclical response to demand. The second is defined as a kind of oligopolistic market regime ($s_t = 2$) where agents establish a small markup on costs. In our simulation study, Markov transition matrices have two possible profiles, with high or low persistence regimes, as shown in table C.9.

Table C.9: Simulation - transition matrices

High Persistence

	Competition	Collusion
Competition	0.99	0.01
Collusion	0.05	0.95

Low Persistence

	Competition	Collusion
Competition	0.95	0.05
Collusion	0.15	0.85

To simulate our market, also consider that the cost and demand vectors have their own DGPs, characterized by an AR (1) process, with *idd* shocks (ϵ_t and v_t), and a specific intercept:

$$\begin{cases} c_t = 1.0 + 0.8c_{t-1} + \epsilon_t, \\ d_t = 0.2 + 0.9d_{t-1} + v_t \end{cases} \tag{C.2}$$

This basic model generated two price series with 300 observations each, as can be seen in figures C.1 and C.2. We can obtain the filtered and smoothed probabilities shown in C.3 and C.4 by estimating a model of markovian regression.

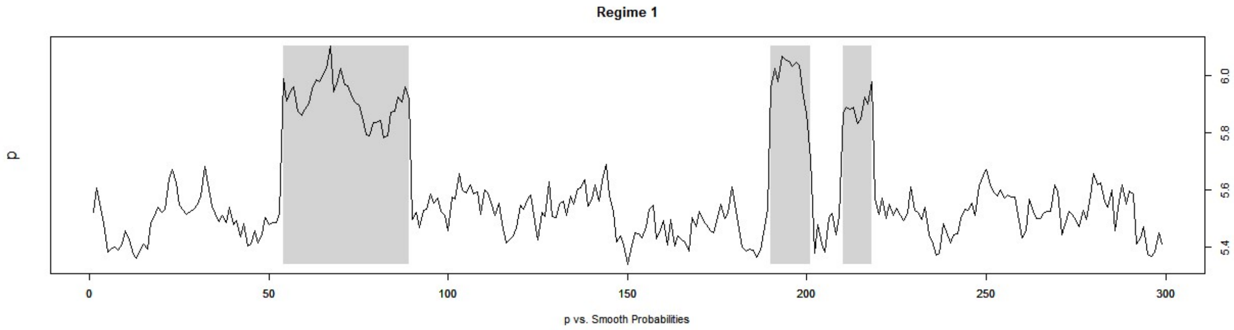


Figure C.1: Simulation - simulated price series with two high persistence regimes. The gray shaded area indicates the collusion regime.

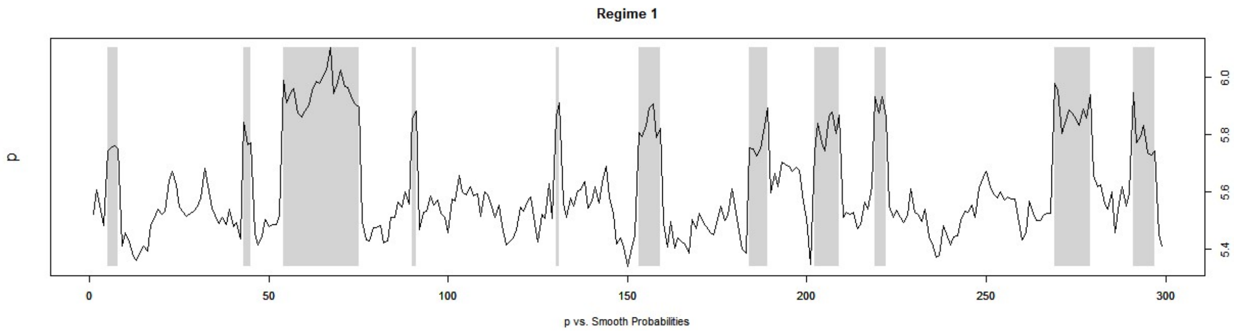


Figure C.2: Simulation - simulated price series with two low persistence regimes. The gray shaded area indicates the collusion regime.

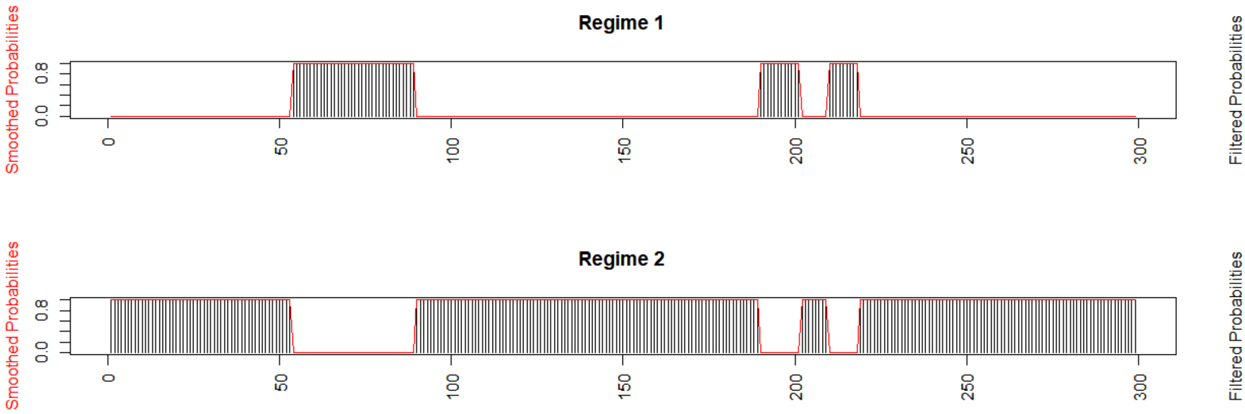


Figure C.3: Simulation - smoothed and filtered probabilities for high persistence regimes. Regime 1 is the collusive one.

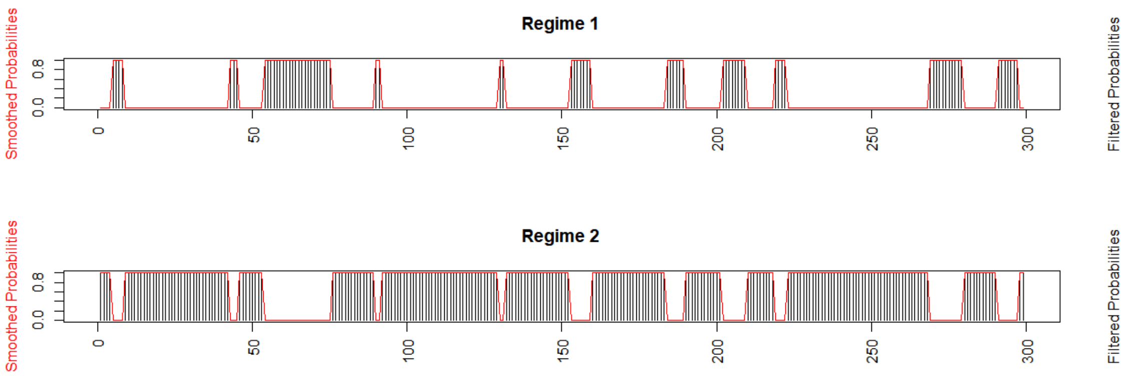


Figure C.4: Simulation - smoothed and filtered probabilities for low persistence regimes. Regime 1 is the collusive one.

It's worth noting that the series with low persistence (a DGP more similar to our set of cartel cases in the fuel market) presents several short periods of collusion (11 episodes of a cartel, with 22 structural breaks), followed by others of competition. On the other hand, in series with high persistence, there are fewer regime changes (only three periods of a cartel and six breaks) and more observations between them. So, let's check how Bai and Perron's test performed when estimating structural breaks for these two series, allowing for changes in the crucial trimming parameter (h). Our estimation's primary results, the observations where Bai and Perron's test signaled a structural break, can be seen in table C.10.

Table C.10: Simulation - Breaks identified by Bai and Perron's test (observations)

h	High Persistence	Low Persistence
0.15	45, 89, 174, 218	53, 100, 183
0.10	53, 89, 189, 218	42, 75, 152, 183, 222, 268
0.05	53, 89, 187, 201, 218	53, 75, 152, 166, 183, 209, 223, 268, 282
0.02	6, 53, 65, 89, 99, 144, 189, 201, 209, 218, 227, 237	8, 42, 47, 53, 75, 89, 94, 129, 134, 152, 159, 183, 189, 201, 209, 217, 222, 268, 279, 286, 291

We did not perform a Monte Carlo simulation with hundreds of simulated samples because our goal was only to illustrate conclusions about the downsides of structural break tests. However, even with this simple exercise, we could show that structural break test performance is not satisfactory when there are recurrent regime changes in the market. In our series of low persistence regimes, with a trimming parameter of 0.15, the estimation found only 3 out of 22 breaks. When the minimum number of observations between sample partitions is reduced, the test shows better results, signaling more correct breaks. However, this improvement in type II error reduction came at the expense of type I error robustness. It's worth noting that with $h = 0.02$, the test returns a significant number of false breaks (a phenomenon explained by the terrible convergence proprieties when there are only six observations between intervals).